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Within US Trade and the Long Shadow of the American Secession

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Abstract

Using data from the US commodity flow surveys, we show that the historical Union-Confederacy border lowers contemporaneous trade between US states by about 16 percent relative to trade flows within the former alliances. Amongst one million placebos, there is no other constellation of state grouping that would yield a larger border effect. The finding is robust over different econometric models, treatment of the rest of the world, available survey waves, or levels of aggregation. Including contemporaneous controls, such as network, institutional or demographic variables, and Heckscher-Ohlin or Linder terms, lowers the estimate only slightly. Historical variables, such as the incidence of slavery, do not explain the effect away. Adding US states unaffected by the Civil War, we argue that the friction is not merely reflecting unmeasured North-South differences. Finally, the estimated border effect is larger for differentiated than for homogeneous goods, stressing the potential role for cultural factors and trust.

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Keywords: American Secession, border effect, intranational trade, gravity, US state levels.

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150 years after Confederate troops attacked Fort Sumter in South Carolina, a recent US-wide survey by the Pew Research Center summarizes the findings as: ‘‘*The Civil War at 150: Still Relevant, Still Divisive*”.[1] The poll reports that 56 percent of Americans believe that the Civil War is still relevant to politics and public life today. And that 4 in 10 Southerners sympathize with the Confederacy. But does the long defunct border between the Confederation and the Union still affect *economic* relations between US states that belonged to different alliances today? Is the former border still relevant, still divisive? This paper sheds light on this question using bilateral trade flows between states.

The Civil war has cost 620,000 American lives, more than any other military conflict. Golden and Lewis (1975) document that it has retarded the economic development of the whole nation and of the South in particular. And, as the Pew poll shows, the nation is still divided along the lines of the former alliances over whether the war was fought over moral issues – slavery – or over economic policy. Yet, long before the war, the Southern and the Northern economies differed: The South 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.[2] Its infant manufacturing industries were protected by import tariffs against European competition.

The North-South divide is very visible in contemporaneous state-level data. On average, the South is still poorer, more rural, more agricultural, less educated, more religious, and has different political views. The economic gap may have 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, continues to beset the country. A special

[1]Pew Research Centre for the People and the Press, “Civil War at 150: Still Relevant, Still Divisive”, Apri 8, 2011; available at http://pewresearch.org/pubs/1958/.

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

sense of Southern identity continues to mark a cultural divide within the US.

This paper contributes to a growing literature on the long-shadow of history for economic transactions (Nitsch and Wolf, 2009; Falck et al., 2010; Head et al., 2010). It shows that the former border still constitutes a discontinuity in the economic geography of the United States. The modern literature has identified cultural differences across countries as impediments of international trade, but typically not within the same country. Estimates of various border effects abound in the literature and there are well-tested empirical methods to measure their trade-inhibiting force. The more challenging question in this paper is: Can the estimated border effect be interpreted as a *genuine* Union-vs-Confederation effect?

We proceed in three steps. First, employing the theory-consistent (but parsimonious) gravity model of Anderson and van Wincoop (2003), henceforth AvW, 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 between 22 and 16 percent. In comparison, the Canada-US border restricts trade by 155 to 165 percent (AvW). Nitsch and Wolf (2011) find that the former border between East and West Germany restricts trade by about 26 to 30 percent in 2004. Running a million placebos, we show that no other border between random groups of (old) US states yields a stronger trade-reducing effect. The result is robust to employing alternative methodologies, using different waves of the Commodity Flow Survey (1997, 2002, 2007), drawing on sectoral rather than aggregate bilateral trade data, or measuring transportation costs differently (travel time instead of sheer geographical distance). Including the rest of the world, or different treatment of states, whose allegiance to either 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 original AvW model to account for observable differences between the South and the North. The controls are meant to capture migrant, ethnic, or religious networks. While these variables matter empirically, they do not reduce the estimated border effect. We account for cultural differences expressed by different colonial relations across states, for different patterns of urbanization, and for additional geographical variables. We include variables that relate to the institutional setup of states, or that measure differences in the judicial system. We control for differences in endowment proportions, 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 model.

Third, we acknowledge that the North-South border, marked by the Secession, is likely not to be exogenous. Engerman and Sokoloff (2000 and 2005) suggest that it is related to endowment differences between Northern and Southern states in cropland, or in the size and structure of agricultural production. The emergence of the border may have to do with historical ethnic patterns, historical educational achievements of the population, or institutional differences as captured by the historical incidence of malaria as in Acemoglu et al. (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 contemporaneous 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 increases. 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 18 percent), which can now be attributed more plausibly 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[3] 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. Wei (1996) and Hillberry (1999) do not find that tariffs, quotas, exchange rate variability, transaction costs, and regulatory differences can explain the border effect. Recent studies illustrate that the impact of borders also extends to the sub-national level, implying that additional reasons for high local trade levels must exist. Examples are Wolf (1997 and 2000), Hillberry and Hummels (2003), Combes et al. (2005), Buch and Toubal (2009), and Nitsch and Wolf (2009).

The remainder of the paper is structured as follows. Section I. provides de-

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

tails of the empirical strategy. Section II. describes the benchmark 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*

Our empirical strategy follows Anderson and van Wincoop (2003) and the subsequent research. Based on a multi-country framework of the Krugman (1980) constant elasticity of substitution model with iceberg trade costs, the literature stresses that the consistent estimation of bilateral barriers requires to take *multilateral trade resistance* into account. In this paper, we use the nonlinear least squares (NLS) model suggested by AvW (2003) to estimate the border effect. We also employ an approach that uses state specific fixed effects to control for multilateral resistance. Finally, we implement the idea of Baier and Bergstrand (2009) to linearize the model by help of a first order expansion of the multilateral resistance terms. In all setups, we proxy trade costs by geographical distance and the historical border between the former alliances of states in the Union and the Confederacy.

AvW (2003) show that the CES demand system with symmetric trade costs can be written as

$$\ln z_{ij} = \beta_0 + \beta_1 \text{Border}_{ij} + \beta_2 \ln \text{Dist}_{ij} + \boldsymbol{\gamma X_{ij}} - \ln P_i^{1-\sigma} - \ln P_j^{1-\sigma} + \epsilon_{ij}, \quad (1)$$

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

and $z_{ij} \equiv x_{ij} / (Y_i Y_j)$ is the value of bilateral exports x_{ij} between state i and state j relative to the product of the states' GDPs, Y_i and Y_j. β_0 is a constant across state pairs, $\beta_1 = -\alpha(\sigma - 1)$ and $\beta_2 = -\rho(\sigma - 1)$, where $\sigma > 1$ is the elasticity of

substitution. $\text{Border}_{ij} = (1 - \delta_{ij})$ 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. $\ln \text{Dist}_{ij}$ is the log of geographical distance between states. $\boldsymbol{X_{ij}}$ denotes a vector of additional controls. AvW set $\boldsymbol{\gamma} = \mathbf{0}$ and focus on a parsimonious gravity model. In our exercise, we switch $\boldsymbol{\gamma}$ on and off and work with various vectors $\mathbf{X}_{ij}$. θ_k is the share of income of state k in world income; ϵ_{ij} is the standard error term.

The complication with estimating that model is that the multilateral resistance terms $\ln P_i^{1-\sigma}$ and $\ln P_j^{1-\sigma}$ depend on estimates of $\hat{\beta}_1$ and $\hat{\beta}_2$ in a non-linear fashion. AvW (2003) propose estimating their gravity model by means of an iterative procedure that minimizes the sum of squared residuals in (1), while simultaneously using (2) to obtain values for the multilateral resistance terms.

Baier and Bergstrand (2009) propose to apply a first order linear expansion to (2), to solve for the multilateral price indices, and substitute the solution into (1). The ensuing log linear equation can be estimated by means of simple OLS. In the present case, this amounts to including multilateral resistance (MR) terms for distance (MRDist_{ij}) and the border (MRBorder_{ij}) into the equation.

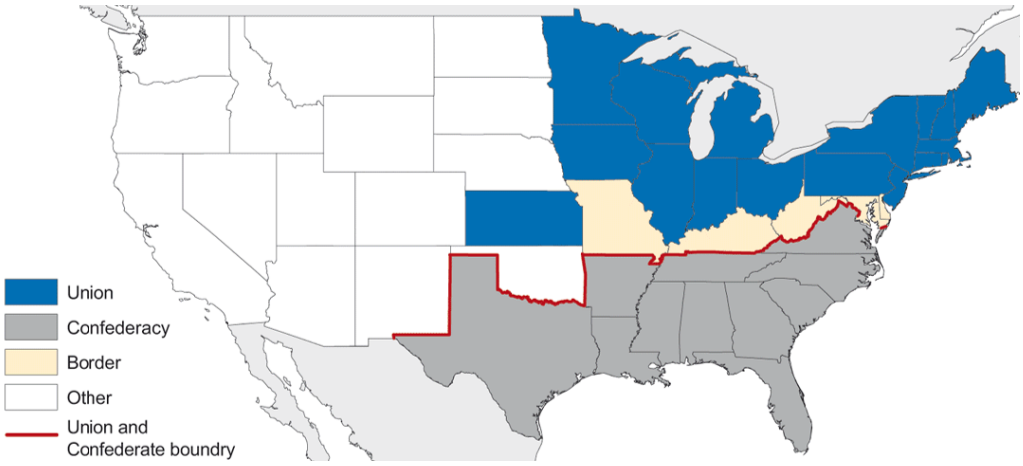
Finally, we follow a large strand of literature (Hummels, 1999; AvW, 2003; Feenstra, 2004; Redding and Venables, 2004) and apply origin and destination fixed effects in an OLS gravity regression. The fixed effects capture *all* time-invariant origin and destination specific determinants, such as multilateral resistance terms, but also geographical characteristics and historical or cultural facts. In that sense, this model is more general than the two other ones, since it accounts for any country-level unobserved heterogeneity.

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 Surveys* (CFS) collected by the Bureau of Transportation Statistics. The CFS tracks shipments in net selling values in millions of dollars. The CFS covers 200,000 (100,000; 50,000; 100,000)

representative US firms for 1993 (1997; 2002; 2007). The literature is concerned about the low number of firms surveyed in the waves after 1993, see Erlebaum et al. (2006). For this reason, existing studies have usually focused on the 1993 wave which represents about 25 percent of registered US firms; we follow in this tradition. 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 as in Wei (1996) as the quarter of the distance between a state and its closest neighbor.[4]

FIGURE 1
Union versus Confederate States



Our primary sample consists of 28 US states divided into two groups that originate from the split caused by the Secession (as shown in Figure 1). The **South** comprises 11 states, while the **North** consists of 17 states, as listed in Table 1. Five states (Delaware, Kentucky, Maryland, Missouri and West Virginia) are excluded from the benchmark sample since soldiers from these states fought on both sides of the Civil War and the allegiance to either group of states is unclear. Still today, these five states do not belong to the (fuzzily defined) "deep South".[5] Somewhat abusing terminology, we call these 5 states *border*

[4]This is the common practice in the literature, see also AvW (2003), Feenstra (2004), as well as Baier and Bergstrand (2009).

[5]Reed and Reed (1997) define the "deep South" as an area roughly coextensive with the old

states. We conduct sensitivity analysis with respect to the choice of excluding those states.

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		

Table 2 shows averages and standard deviations (for the year of 1993) of the variables used in this study. Southern states have on average substantially larger shares of Afro-Americans (22.9 versus 7.4 percent); the share of Christians is higher while the share of Jewish citizens is smaller (0.8 versus 2.1 percent). The percentage share of urban population is lower in South than in North (65.6 versus 72.9). Historically (as 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 percent lower than the av-

cotton belt from eastern North Carolina through South Carolina west into East Texas, with extensions north and south along the Mississippi.

erage 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 the 1992 presidential election, 64 percent of Southern states voted Republican while only 12 of Northern states did.[6]

II. The Effect of the Former Union-Confederation Border

A. Benchmark Results

Estimating equation (1) allows assessing the *average* impact of the border on cross-border North-South trade flows relative to within region flows. Table 3 provides our benchmark results for the year of 1993. Estimates of the AvW (2003) NLS model are shown in column (1). In line with the gravity literature, the estimated 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 about 19.6 percent ($e^{-0.218} - 1$) in 1993. That estimate is statistically significant at the 1 percent level. This is the same as to say that "within" trade is by the factor 1.24 ($e^{0.218}$) bigger than "between" trade. The associated ad valorem tariff equivalent of the border is 2.5 to 11.5 percent, depending on the choice of elasticity of substitution.[7] 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. AvW (2003) find that cross-border trade for the Canada-US case is about 80.8 percent lower than within trade.[8] This amounts to a tariff equivalent of 20 to 128 percent. Results by Nitsch and Wolf (2011) suggest that the former East-West border within

[6]North-South differences are also clearly visible when looking at pairs of states. Table A-1 in the Web Appendix differentiates between the sample of *all* pairs ($N = 768$) and the sample of cross-border pairs (states from different sides of the historical border; $N = 364$).

[7]Broda et al. (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.

[8]Table 2 in AvW, two-country model: $e^{-1.65} - 1$.

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	Share (%) of blacks in population.
Jewish Share	2.105	2.339	0.809	1.285	Share (%) of Jewish in population.
Christian Share	86.882	3.059	91.636	3.139	Share (%) of Christian in population.
Other Religion Share	1.131	0.786	0.919	0.416	Share (%) of people with other religion.
No Religion Share	7.647	1.998	5.000	1.673	Share (%) of people with no religion.
Urban Share	72.853	16.095	65.655	12.098	Share (%) of urban 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.
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, age 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 / Total Output	-4.515	0.687	-4.159	0.427	Agri. over total output, mio US $.
ln Manufacturing / Total Output	-1.615	0.250	-1.661	0.364	Manuf. over total output, mio 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, age 15-44.
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 else.
Republican	0.118	0.332	0.636	0.505	1 if republican, 1992 pres. election, 0 else.
Judiciary Election	1.824	0.883	1.182	0.405	1 if judiciary is elected, 0 else.

Notes: Data sources as in Table A-1 (Web Appendix).

Germany reduces cross-border trade by about 20.5 percent relative to within-

region trade.[9]

In column (2), we estimate the model using origin and destination fixed effects, which account for all unobserved importer and exporter characteristics. Our model explains 87 percent of the variation in trade patterns. Under fixed effects, cross-border trade is on average 14.5 percent 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 AvW (2003) estimation in the Canada-US case. In column (3), we use two indicator variables to measure within-group trade relative to cross-border trade separately for the North and the South. 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 interesting as we expect to find a positive 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. However, that result is not robust; see column (6).[10]

In column (4), we estimate a "multicountry" model. We consider trade between US states, between 20 OECD countries[11] and exports from individual US states to OECD countries[12] into the fixed effects model of column (2). We use OECD trade, distance and GDP data provided by AvW and US state exports to

[9]Table 2a in Nitsch and Wolf (2011), pooled OLS in 2004: $e^{-0.229} - 1$.

[10]This result vanishes when counting the border states into the South (Table A-6 of the Web Appendix), or when adding Western states to the analysis (Table 9).

[11]These include Canada, Australia, Japan, New Zealand, Austria, Belgium-Luxembourg, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom.

[12]We focus on exports from US states to the OECD as import data of individual US states from OECD states (and vice versa) are not available.

OECD countries from Robert Feenstra's webpage.[13] Column (4) reports that the distance parameter remains close to -1, while the border reduces North-South trade within the US by 16.9 percent. Sample size increases to 1,776 observations, while the explanation power of our model increases only slightly to 87.7 percent.[14]

TABLE 3
Basic Border Effect Results

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Year of Data:	1993						
Data:	Aggregated						Commodity
Specification:	AvW NLS	Fixed Effects		FE Multi	OLS with MR Terms		Chen (2004) FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Border Dummy$_{ij}$	-0.218***	-0.157***		-0.185***	-0.157***		-0.090***
	(0.04)	(0.03)		(0.04)	(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***	-0.993***	-1.055***	-1.039***	-0.978***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)
Fixed Effects							
Importer	n.a.	YES	YES	YES	-	-	-
Exporter	n.a.	YES	YES	YES	-	-	-
Importer×Commodity	n.a.	-	-	-	-	-	YES
Exporter×Commodity	n.a.	-	-	-	-	-	YES
Multilateral Resistance	n.a.	-	-	-	YES	YES	-
Observations	768	768	768	1,776	768	768	13,303
Adjusted R^2	n.a.	0.874	0.874	0.877	0.751	0.759	0.636

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. States in sample as in Table 1. District of Columbia is excluded. In column (4), we adapt a multicountry fixed effects approach and add exports of individual US states to 20 OECD countries and between OECD trade. *** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

Next, we estimate equation (1) by including MR terms into the gravity estimation as suggested by Baier and Bergstrand (2009). Columns (5) and (6) show

[13] http://cid.econ.ucdavis.edu/

[14] When we also consider Western states of the US (as described in Section IV.*B.*) in the multicountry sample, we still find a negative and significant North-South border effect of 8.1 percent, while sample size increases to 3,517 observations.

that the adjusted explanation power of the estimation slightly falls to 75 percent, while the border estimate remains very close compared to the fixed effects estimation. The border impeding trade effect between the North and the South persists with a magnitude of 15 percent. In column (6), 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 the final 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 (7). We find that the border reduced North-South trade by about 8.6 percent.

Following AvW (2003), we use the model and the parameter estimates to simulate the ratio of trade with border barriers (BB) to the counterfactual level of trade under borderless trade (NB).
footnoteDetailed results are in Table A-4 in the Web Appendix. Multilateral resistance increases by 6 percent for states of the South, but only by 1 percent for those of the North. Moreover, South-South trade rises by 12 percent due to the border, while North-North trade increases by only 3 percent.

B. Placebo Estimations

Is there something special about trade across the former Union-Confederation border as opposed to trade across other hypothetical borders? To deal with this question, we randomly assign 11 out of the 28 'old' US states to a hypothetical "South" and the remainder to a hypothetical "North".[15] Based on regression (2)

[15]The number of potential "South" subsamples and hence of state groups is huge: 21,474,180. Estimating all possible border effects between these groups of states is computationally ex-

of Table 3, we run a million placebo regressions. We find a negative and significant (at the 10% level) border effect in 13.4 percent of the cases. In 56 cases the border effect is slightly larger than the 15.7 percent found in our benchmark case. The largest effect we find is 1.6 percentage-points larger than our original effect, but the standard error is so large that one cannot reject the hypothesis that the effect is identical to the 15.7 benchmark result. In all 56 cases, the "South" consists predominantly of New England and the Great Lakes States.

FIGURE 2
Placebo Estimations. Frequency and Average Size of Significant Border Effects in Different State Groupings

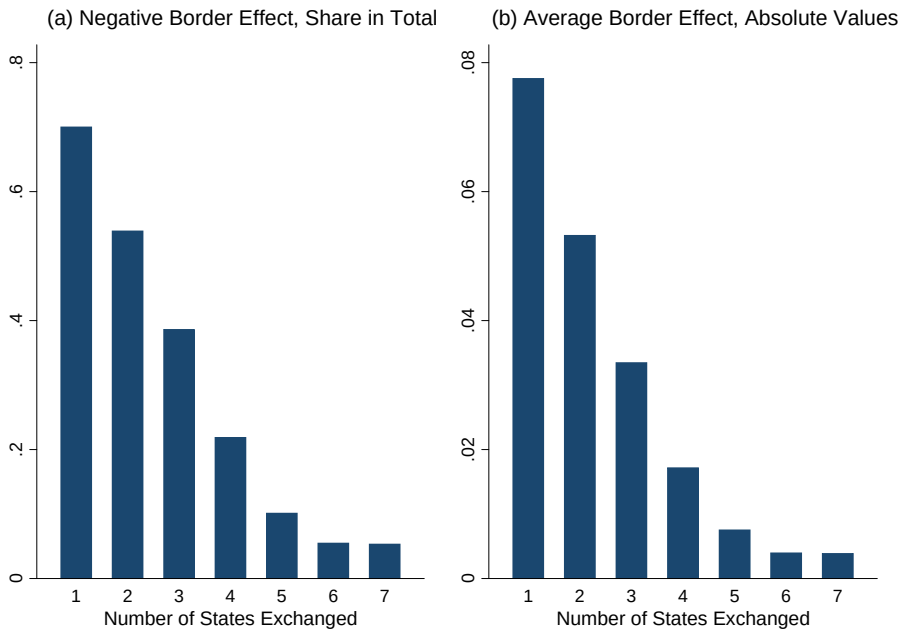


Figure 2 compares the hypothetical South to the "true" sample by counting the number of misallocated states (put into the "wrong" group). Diagram (a) depicts that about 70 % of all samples, where one state was misallocated, yield a negative and statistically significant border effect. If 2 states are misallocated that share drops to 55 %; if more than 5 states are put into the "wrong" group

tremely costly. A single regression takes about one second. Computation time then amounts to 249 days.

the share falls to below 10%. Diagram (b) displays the absolute value of the average border effect found in different subsamples. If one state is allocated to the "wrong" group, the average border effect is slightly below 0.08 (as compared to 0.16 in the "correct" grouping). The average effect falls quickly as more states are misallocated and is below 0.007 if 5 or more states are exchanged.[16]

C. Sensitivity Analysis

Table 4 summarizes border effect estimates obtained from using the 1997, 2002 or 2007 waves of the Commodity Flow Survey rather than the more reliable 1993 data. Across the non-linear AvW procedure, the fixed-effects model, the Baier and Bergstrand (2009) approach, and the the commodity-level regression, we find negative border effects that are all statistically significant at the 1% level. Interestingly, there is no evidence that the border effect shrinks over time. Comparison across time is hindered by different sampling across waves. The former border reduces trade by between 9 and 18 percent, with the average effect clustering around at about 14 percent.

The use of geographical distance as a measure of transportation costs has been criticized by Head and Mayer (2002). Since 71 to 75 percent of shipments in the US are transported by truck (Department of Transportation), we use actual travel time from Google maps as an alternative measure of transportation costs. Ozimek and Miles (2011) provide a tool to retrieve these data. We find that the use of travel time reduces the estimated border effect in the preferred 1993 sample from 15 to 11 percent, thereby confirming the hypothesis that geographical distance inflates the estimated border effects. However, across waves, the effect remains negative and statistically significant.[17]

[16] In further placebo exercises, we investigate border effects between coastal and interior states as well as between Eastern and Western states *in the whole US*. Results are contained in Table A-2 of the Web Appendix. We do not find a border effect between coastal and interior states. There is no border effect neither at a hypothetical East-West border (approximately drawn at the 90° longitude line).

[17] The 1997 wave with its 50,000 sampled firms is an exception. Detailed results are found in Table A-5 of the Web Appendix.

TABLE 4
Sensitivity Across Different Survey Waves

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

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, effects on log distance and MR terms are not reported. Robust standard errors reported in parenthesis. n.a. means not applicable. Table A3 in the Web Appendix contains full results. AvW NLS denotes the Anderson and van Wincoop (2003) Nonlinear Least Squares Method. 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 fixed effects following Chen (2004). 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.

To make sure that our treatment of border states (i.e., states whose allegiance was unclear and that are therefore excluded from our benchmark sample), does not bias our results, we assign them alternatively to the South or to the North. The border states were slave states, but officially never seceded, so it is coun-

terfactual to include them into the South. We find that the assignment of those border states does not matter qualitatively for our findings. Estimated effects are slightly lower than when border states are excluded altogether.[18]

D. Estimates by Sector

Finally, we also run regressions sector-by-sector. Table 5 provides summary results, suppressing other coefficients except the one on the border dummy.[19] The estimated border effect is $\hat{\beta}_1 = -\alpha\,\widehat{(\sigma - 1)}$, thereby confounding the elasticity of substitution and the trade-cost increasing effect of the border. It is therefore not surprising, that the low-σ agricultural sector features a high but only moderately robust estimate, while the low-σ mining sector does not display any border effect. There is no border effect in the machinery sector, neither. This is presumably due to North-South differences in comparative advantage that the simple AvW model does not capture. The border effect is most pronounced in the chemical and manufacturing sectors, where the degree of product differentiation is high (hence, σ low). This finding supports the view that the former border reflects a cultural divide that makes transactions more difficult and that this is most pronounced in highly differentiated industries.

III. Accounting for Observed Contemporaneous Heterogeneity

A. Benchmark Results

In this section we investigate, 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 the variables are not bilateral in nature, we bilateralize them by either taking the absolute difference of variables in state i and state j, denoted by the operator

[18] Detailed results are found in Table A-6 of the Web Appendix.
[19] Detailed results are found in Table A-6 of the Web Appendix.

TABLE 5
Sectoral Results (fixed-effects estimation)

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Sector	Agriculture	Mining	Chemical	Machinery	Manufacturing
			PANEL A: 1993		
	(A1)	(A2)	(A3)	(A4)	(A5)
Border Dummy$_{ij}$	-0.309***	0.022	-0.227***	-0.022	-0.068
	(0.08)	(0.23)	(0.08)	(0.07)	(0.06)
Observations	4,815	1,336	3,078	4,324	11,990
Adjusted R^2	0.746	0.757	0.671	0.657	0.757
			PANEL B: 1997		
	(B1)	(B2)	(B3)	(B4)	(B5)
Border Dummy$_{ij}$	-0.244***	-0.303*	-0.095	-0.072	-0.200***
	(0.08)	(0.17)	(0.06)	(0.06)	(0.05)
Observations	5,490	2,655	3,215	3,455	7,620
Adjusted R^2	0.783	0.774	0.755	0.733	0.803
			PANEL C: 2002		
	(C1)	(C)	(C3)	(C4)	(C5)
Border Dummy$_{ij}$	-0.176*	-0.210	-0.216***	0.005	-0.271***
	(0.09)	(0.36)	(0.08)	(0.07)	(0.06)
Observations	4,470	1,629	2,820	3,205	7,080
Adjusted R^2	0.773	0.762	0.722	0.695	0.774
			PANEL D: 2007		
	(D1)	(D2)	(D3)	(D4)	(D5)
Border Dummy$_{ij}$	-0.308***	-0.101	-0.302***	-0.020	-0.277***
	(0.07)	(0.16)	(0.06)	(0.07)	(0.05)
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, fixed effects and effects on log distance not reported. Robust standard errors reported in parenthesis. Table A-7 in the Web Appendix contains full results. Commodities pooled into sectors as listed in Table A-11 and A-12 in the Web Appendix. States in sample as in Table 1. District of Columbia excluded.*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

Δ, or by using the product of variables in state i and state j, denoted by the operator $\times$. The product of variables relates to *network* effects between pairs,

while the Δ operator focuses on the *difference* between state pairs.[20] Table 6 reports results for our benchmark year 1993. All estimations include origin and destination fixed effects.

Column (1) of Table 6 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. In our sample, and in accordance with the literature, adjacency increases bilateral trade by about 45 percent. Due to the omission of border states from our baseline estimations, adjacency correlates negatively with 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.[21]

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, 2007). The literature reasons that common culture and tastes increase trade flows as they facilitate contracts and instill trust; they also make it more likely that states produce and consume similar goods. Migration and networks might bias the border effect estimate upwards as they increase trade but are negatively associated with the border. 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*; (ii) the product of the share of Afro-Americans in total state population 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 the *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

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

[21] Clearly, not including adjacency biases all coefficients.

TABLE 6
Contemporaneous Controls, 1993 (fixed-effects estimation)

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

	(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)
$\times$Black Share$_{ij}$		0.000**	0.001***	0.001***	0.001**	0.001**
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\times$Jewish Share$_{ij}$		-0.009**	-0.008*	-0.006	-0.006	-0.006
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\times$Christian Share$_{ij}$		0.001	0.002	0.001	0.002	0.002
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\times$Other Religion Share$_{ij}$		0.051**	0.053**	0.051**	0.041	0.041
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
$\times$No Religion Share$_{ij}$		0.011**	0.011**	0.011**	0.009*	0.009*
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\times$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)
Observations	768	768	768	768	768	768
Adjusted R^2	0.884	0.909	0.909	0.911	0.911	0.911

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

in the bilateral migration stock indicates an increase in trade by 22 percent in column (2).[22] If we include network controls, the border still turns out to reduce bilateral trade by 11.7 percent.[23]

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; Hillberry 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), may have lasting effects on bilateral trade.[24] We construct an indicator variable that takes value one if a pair of states had a common colonizer (Britain, France or Spain) and zero otherwise. We find that a common colonial past increases bilateral trade by about 19 percent. Yet, while most of those network variables matter statistically, they do not reduce the estimated border effect. If at all, they leave it slightly higher.

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 by the US Department of Labor. In theory, differences in labor market institutions could increase bilateral trade, because differential legislation acts as a source of comparative advantage as in Cunat and Melitz (2009). In our analysis, we find that institutional differences tend to reduce trade (albeit statistical precision of estimates is low).

[22] A similar effect has been identified by Combes et al. (2005) for trade within France.
[23] $100 \times (exp(-0.124) - 1)$.
[24] See, for instance, Head et al. (2010).

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 (Clinton against 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.

In column (4), we include controls for the difference in relative factor endowments of states, thereby accounting for the Heckscher-Ohlin trade theory. Omitting differences in 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. 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[25]; (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 variables on the difference in the capital-labor ratio and the difference in relative skill endowment bear the right sign. Differences in the availability of cropland reduce bilateral trade, 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 11.5 percent.[26]

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

[26]$100 \times (exp(-0.122) - 1)$.

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 border effect remains negative and significant.

Finally, following the literature on the Linder effect, we include the difference in the log of per capita income as in Thursby and Thursby (1987), Bergstrand (1989), and Hallak(2010). The hypothesis is that states with dissimilar GDP per capita should have differing 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 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. We have also experimented with direct measures of inequality (Gini coefficients), but without success.

Column (6) represents our most comprehensive and preferred model. The border effect is about 12.2 percent.[27] It explains more than 91 percent of the variation in bilateral trade flows, 85 percent of which are attributable to included variables and controls.[28]

B. Sensitivity Analysis

Table 7 summarizes sensitivity results pertaining to the comprehensive model in column (6) of Table 6.[29] Panel A deploys the FE approach. Our baseline border effect of -0.130 is reported in column (A1). We find a negative and significant

[27] $100 \times (exp(-0.130) - 1)$.

[28] 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. In additional regressions, we include differences in export openness to the rest of the World (ROW) ($|exports_{iROW}/Y_i - exports_{jROW}/Y_j|$). This does not change any results.

[29] Details are relegated to Table A-8 in the Web Appendix.

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 50,000 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×commodity and destination×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.[30]

IV. Accounting for Historical Determinants

A. Benchmark Results

The economic literature on the emergence of armed conflicts shows that strong bilateral trade links decrease the probability that two countries go to war, while multilateral openness increases the odds of conflict (Martin et al., 2008). If determinants of bilateral trade are persistent over time, the border could not be considered exogenous in the statistical sense. 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 thus the incidence of the border). Moreover, Eichengreen and Irwin (1998) suggest that history might affect contemporaneous trade flows through persistent effects on institutions.

According to Engerman and Sokoloff (2000 and 2005), dissimilarities in agri-

[30]When we work with sectoral data 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 A-9 in the Web Appendix reports detailed results.

TABLE 7
Controls, Alternative Samples and Models: Summary Results

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

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 variables of Table 6 column (6) as additional controls. Full results are reported in Table A-8 in the Web Appendix. *** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

cultural land use, driven by soil endowments and climate, led to the South adopting slavery and, more broadly, to the emergence of conflicting economic interests between the North and the South, and ultimately, to the Secession. The different economic models may have long-lasting effects on inequality within states, which may, in turn, be relevant for today's level of economic transactions (Linder effect). It may also have persistent effects on institutions, which affect contemporaneous bilateral trade. The historical settlement structure may have induced networks along cultural lines that survived over time.[31] Absolute differ-

[31]The analysis relates to the literature on the long-term impact of factor endowments and

ences in historical variables are positively correlated to the border, so that their omission may bias the estimated border effect away from zero.

To account for these possibilities, Table 8 includes historical differences in (i) cropland; (ii) average farms size; (iii) population density; and (iv) illiteracy rates of the non-slave population.[32] In column (1) to (3), we find that none of these variables matter statistically, except for historical farm size differences which are significant at the 10 percent level. Including farm size increases rather then decreases the border coefficient to -0.230. This is surprising as historical farm size differences correlate positively with the border.

One would expect the legacy of slavery to partly capture the border barrier in column (4). However, we find that differences in slave shares in 1860 exert no impact on bilateral trade patterns and do not explain away the border barrier.[33] Interestingly, the inclusion of the absolute difference in shares of free blacks in 1860 exerts a positive and significant effect on contemporaneous trade.

In addition, similarities in culture due to similar settlement structures in US states before the war could have induced social and 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 German or British settlers, but the impact of German networks vanishes with the inclusion of further variables in subsequent columns.

According to Acemoglu et al. (2002), historical climatic differences measured by the incidence of malaria, may have affected the characteristics and

institutions (Acemoglu et al., 2002; Nunn, 2009; Galor et al., 2009).

[32]Additionally, all models include our additional contemporaneous controls from Table 6 column (6) and importer as well as exporter fixed effects.

[33]If we use the difference in the share of slaves in 1840, when there were 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.

TABLE 8

Contemporaneous and Historical Controls, 1993 (fixed-effects estimation)

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

	(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 6 column (6) included	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
Historical Controls							
Δ ln 1860 Cropland$_{ij}$	-0.023						-0.032
	(0.02)						(0.02)
Δ ln 1860 Farm Size$_{ij}$	0.138*						0.117
	(0.08)						(0.09)
Δ ln 1860 Population Density$_{ij}$		0.022					0.017
		(0.02)					(0.02)
Δ 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

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 variables as of column (6), Table 6 as additional controls. *** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

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 affect contemporaneous trade flows through its lasting effect on institutions. 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 20.9 percent,[34] even when we include variables capturing the historical determinants of the Secession.[35]

B. Including the West

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 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 states that were not affected 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.[36] The border dummy is unity for states that found themselves on opposite sides of the Civil War and zero for all other pairs of states. Adding the West adds a control set of state pairs that are characterized by their absence of a past shaped by the Civil War.

[34] $100 \times (exp(-0.235) - 1)$.

[35] We have also experimented with direct measures for the historical transportation system (differences or networks of railroad miles per 100 square miles of land area after the Civil War in 1870). The result is robust to the inclusion of the historical transportation system.

[36] West includes all US states that were not assigned to the North, the South or the border states in Table 1, excluding the District of Columbia, Alaska and Hawaii.

TABLE 9
Additionally Including the West, 1993

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Data:	Aggregated				Commodity
Specification:	Fixed Effects		OLS with MR Terms		Chen (2004) FE
	(1)	(2)	(3)	(4)	(5)
Border Dummy$_{ij}$	-0.073*		-0.119***		-0.198***
	(0.04)		(0.04)		(0.03)
South–South Dummy$_{ij}$		0.240***		0.242***	
		(0.07)		(0.09)	
North–North Dummy$_{ij}$		0.276		0.375***	
		(0.91)		(0.04)	
West–West Dummy$_{ij}$		-0.036		0.232***	
		(0.09)		(0.08)	
ln Distance$_{ij}$	-0.358***	-0.358***	-0.690***	-0.611***	-0.163***
	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)
Additional Controls	YES	YES	YES	YES	YES
Observations	1,739	1,739	1,739	1,739	24,948
Adjusted R^2	0.851	0.852	0.734	0.747	0.631

Notes: Constant, fixed effects, MR terms and controls not reported. Robust standard errors reported in parenthesis. All models include variables as of column (6), Table 6 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 9 reports the results. All models include additional contemporaneous controls.[37] In columns (1), (3) and (5), we find for the fixed effects, the OLS with MR terms, and the pooled commodity FE regression a significant trade impeding effect of the Secession treatment. The effect ranges between 7 and 18 percent. In addition, we again find in column (2) that the South trades more amongst each other while the effect on the North is positive but insignificant. There seems not to be any particular trade effect within Western 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.[38]

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

[38]Results are similar for the other years and can be found in Table A-10 in the Web Appendix.

V. Civil War at 150: Still Relevant, Still Divisive

The former border between the Union and the Confederation is still relevant today: The defunct border represents a trade barrier that lowers trade between US states by on average 7 to 20 percent. In a million placebo estimations, we find supportive evidence that the magnitude of this border effect is unique. The result is robust to using alternative waves of the Commodity Flow Survey, to different econometric methods, or to the inclusion of Western states or the rest of the world. It cannot be substantially attenuated, let alone eliminated, by adding a vast array of contemporaneous and historical variables that correlate both with the border dummy and, potentially, also with bilateral trade.

The great Mississippi novelist and poet William Faulkner famously writes "The past is never dead. It's not even past." (Requiem for a Nun, 1951). This holds true for the Secession that tore the US apart 150 years ago, even when the judgment is based on bilateral trade data and econometric analysis: Trade between the former Confederation and the former Union is about 14.5 percent smaller on average than within the alliance. A number of additional results seem important: First, the effect of the long defunct border on today's trade is not attributable to the legacy of slavery alone. It becomes weaker if not the Secession but the status of slave states is the criterion for belonging to one of the two groups. Second, the border effect is not merely a North-South effect. When the border is redefined to reflect whether two states have been on opposing sides in the Civil War, it remains significantly negative. Third, the trade inhibiting force of the former border has to do with the degree of differentiation of products: the higher, the stronger. This suggests that the channel through which the border still matters may be through cultural affinity or trust.

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WEB APPENDIX to Within US Trade and the Long Shadow of the American Secession

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Web Appendix

TABLE A-1
SUMMARY STATISTICS AND DATA SOURCES, 1993

Unit of Observation: Pairs of States					
Sample	Full		North–South		Data Source
	(N = 768)		(N = 364)		
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 A-2
Placebo Coast-Interior and East-West, 1993

Dependent Variable: $\ln$ bilateral exports between i and j relative to states' GDPs

	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

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. 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. Significance levels as in Table 3.

TABLE A-3
Sensitivity Analysis Various Years

Dependent Variable: $\ln$ bilateral exports between i and j relative to states' GDPs

Data	Aggregated			Commodity
Specification	AvW NLS	FE	OLS with MR Terms	FE Chen (2004)
Year of Data		**1997** (N = 766)		
	(A1)	(A2)	(A3)	(A4)
Border Dummy$_{ij}$	-0.128***	-0.091***	-0.126***	-0.138***
	(0.04)	(0.03)	(0.04)	(0.02)
$\ln$ 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
Year of Data		**2002** (N = 739)		
	(B1)	(B2)	(B3)	(B4)
Border Dummy$_{ij}$	-0.175***	-0.146***	-0.150***	-0.194***
	(0.04)	(0.04)	(0.05)	(0.02)
$\ln$ 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
Year of Data		**2007** (N = 768)		
	(C1)	(C2)	(C3)	(C4)
Border Dummy$_{ij}$	-0.175***	-0.134***	-0.144***	-0.199***
	(0.04)	(0.03)	(0.05)	(0.02)
$\ln$ 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
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. Robust standard errors reported in parenthesis. n.a. means not applicable. 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. Pooling over all commodities in 1997 (2002; 2007), we have in column (4) 11,275 (7,721; 12,772) observations. Significance levels as in Table 3.

TABLE A-4
Counterfactual from AvW NLS, 1993

Average of $\mathbf{P}^{1-\sigma}$		
	North	South
With border barrier (BB)	0.73	0.87
	(0.02)	(0.03)
Borderless trade (NB)	0.72	0.82
	(0.02)	(0.02)
Ratio (BB/NB)	1.01	1.06
	(0.01)	(0.02)

Impact of Border Barriers on Bilateral Trade			
	North–North	South–South	North–South
Ratio BB/NB	1.03	1.12	0.80
	(0.01)	(0.04)	(0.03)
Due to bilateral resistance	1.00	1.00	1.07
	(0.00)	(0.00)	(0.03)
Due to multilateral resistance	1.03	1.12	1.07
	(0.01)	(0.04)	(0.02)

Notes: The upper part of the Table reports the average of $P_i^{1-\sigma}$. For the North the average is taken over the 17 states in the sample, for the South over the 11 states. The lower part of the Table reports the ratio of trade with the estimated border barriers (BB) to that under borderless trade (NB). This ratio is broken down into the impact of border barriers on trade through bilateral resistance and through multilateral resistance.

TABLE A-5
Alternative Distance Measure (fixed-effects estimation)

Dependent Variable: $\ln$ bilateral exports between i and j relative to states' GDPs

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

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 A-6
Sensitivity Analysis: Allocation of Border States, 1993

Dependent Variable: $\ln$ bilateral exports between i and j relative to states' GDPs

	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

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. Column (1) to (3) allocate border states (Delaware, Kentucky, Maryland, Missouri, West Virginia) to South as listed in Table 1. North as in Table 1. Column (4) to (6) allocate border states (Delaware, Kentucky, Maryland, Missouri, West Virginia) to North as listed in Table 1. South as in Table 1. District of Columbia excluded. Significance levels as in Table 3.

TABLE A-7
SECTORAL REGRESSIONS (fixed-effects estimation)

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Sector	Agriculture	Mining	Chemical	Machinery	Manufacturing
Year of Data			**1993**		
	(A1)	(A2)	(A3)	(A4)	(A5)
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**		
	(B1)	(B2)	(B3)	(B4)	(B5)
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**		
	(C1)	(C)	(C3)	(C4)	(C5)
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**		
	(D1)	(D2)	(D3)	(D4)	(D5)
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 A-11 and A-12 in the Appendix. Significance levels as in Table 3.

TABLE A-8
Additional Controls, Alternative Samples and Models: Summary Results

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

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
Observations	768	768	13,303	766	766	11,275	739	739	7,721	768	768	12,772
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

Notes: Constant, fixed effects, MR terms and controls not reported. Robust standard errors reported in parenthesis. All models include variables as of column (6), Table 6 as additional controls. Significance levels as in Table 3.

TABLE A-9
Sectoral Regressions Including Controls (fixed-effects estimation)

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Sector	Agriculture	Mining	Chemical	Machinery	Manufacturing
Year of Data			**1993**		
	(A1)	(A2)	(A3)	(A4)	(A5)
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**		
	(B1)	(B2)	(B3)	(B4)	(B5)
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**		
	(C1)	(C2)	(C3)	(C4)	(C5)
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**		
	(D1)	(D2)	(D3)	(D4)	(D5)
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. All models include variables as of column (6), Table 6 as additional controls. Commodities pooled into sectors as listed in Table A-11 and A-12 in the Appendix. Significance levels as in Table 3.

TABLE A-10
Additionally Including the West: Sensitivity

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Data	Aggregated				Commodity
Specification	Fixed Effects		OLS with MR Terms		Chen (2004) FE
Year of Data			**1997** (N = 1,699)		
	(A1)	(A2)	(A3)	(A4)	(A5)
Border Dummy$_{ij}$	-0.081*		-0.224***		-0.146***
	(0.04)		(0.04)		(0.02)
South–South Dummy$_{ij}$		0.110		-0.057	
		(0.08)		(0.08)	
North–North Dummy$_{ij}$		-1.085		-0.099***	
		(1.15)		(0.04)	
West–West Dummy$_{ij}$		0.011		0.192***	
		(0.08)		(0.07)	
ln Distance$_{ij}$	-0.324***	-0.328***	-0.595***	-0.600***	-0.433***
	(0.05)	(0.05)	(0.03)	(0.04)	(0.02)
Additional Controls	YES	YES	YES	YES	YES
Adjusted R^2	0.844	0.844	0.759	0.759	0.826
Year of Data			**2002** (N = 1,649)		
	(B1)	(B2)	(B3)	(B4)	(B5)
Border Dummy$_{ij}$	-0.110**		-0.102**		-0.176***
	(0.05)		(0.05)		(0.03)
South–South Dummy$_{ij}$		0.104		-0.042	
		(0.09)		(0.09)	
North–North Dummy$_{ij}$		0.104		0.008	
		(0.06)		(0.05)	
West–West Dummy$_{ij}$		-0.131		0.089	
		(0.09)		(0.08)	
ln Distance$_{ij}$	-0.391***	-0.395***	-0.741***	-0.729***	-0.399***
	(0.05)	(0.06)	(0.04)	(0.04)	(0.03)
Additional Controls	YES	YES	YES	YES	YES
Adjusted R^2	0.847	0.847	0.732	0.731	0.822
Year of Data			**2007** (N = 1,725)		
	(C1)	(C2)	(C3)	(C4)	(C5)
Border Dummy$_{ij}$	-0.013		-0.048		-0.132***
	(0.04)		(0.05)		(0.02)
South–South Dummy$_{ij}$		0.062		-0.067	
		(0.08)		(0.08)	
North–North Dummy$_{ij}$		0.007		0.043	
		(0.06)		(0.05)	
West–West Dummy$_{ij}$		0.041		0.108	
		(0.09)		(0.08)	
ln Distance$_{ij}$	-0.456***	-0.445***	-0.771***	-0.746***	-0.505***
	(0.05)	(0.05)	(0.04)	(0.04)	(0.02)
Additional Controls	YES	YES	YES	YES	YES
Adjusted R^2	0.857	0.857	0.748	0.748	0.787
Fixed Effects					
Importer	YES	YES	-	-	-
Exporter	YES	YES	-	-	-
Importer×Commodity	-	-	-	-	YES
Exporter×Commodity	-	-	-	-	YES
Multilateral Resistance	-	-	YES	YES	-

Notes: Constant, fixed effects, MR terms and controls not reported. Robust standard errors reported in parenthesis. All models include variables as of column (6), Table 6 available for all states as additional controls. Pooling over all commodities in 1997 (2002; 2007), we have in column (5) 18,185 (12,003; 22,101) observations. Significance levels as in Table 3.

TABLE A-11
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 A-12
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					

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