Is the International Border Effect Larger than the Domestic Border Effect? Evidence from US Trade

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Abstract

Many studies have found that international borders represent large barriers to trade. But how do international borders compare to domestic border barriers? We investigate international and domestic border barriers in a unified framework. We consider a data set of exports from individual US states to foreign countries and combine it with trade flows between and within US states. After controlling for distance and country size, we estimate that relative to state-to-state trade, crossing an individual US state's domestic border appears to entail a *larger* trade barrier than crossing the international US border. Due to the absence of governmental impediments to trade within the United States, this result is surprising. We interpret it as highlighting the concentration of economic activity and trade flows at the local level. (JEL codes: F10, F15)

Keywords: International border, intranational home bias, domestic border, gravity, trade costs, distance

1 Introduction

In a seminal article, McCallum (1995) found that Canadian provinces trade up to 22 times more with each other than with US states. This astounding result, also known as the *international border effect*, has led to a large literature on the trade impediments associated with international borders. More recently, Anderson and van Wincoop (2003) revisited the US–Canadian border effect with new micro-founded estimates. Although they are able to reduce the border effect considerably, there is widespread consensus that the international border remains a large impediment to trade.[1]

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[1] Anderson and van Wincoop (2004) report 74% as an estimate of representative international trade costs for industrialized countries (expressed as a tariff equivalent). About two-thirds of these costs can be attributed to border-related trade barriers such as tariffs and non-tariff barriers. The remainder represents transportation costs. While McCallum (1995) compares trade between Canadian provinces and US states to inter-provincial trade, Anderson and van Wincoop (2003) add inter-state trade data.

A parallel, smaller literature has documented that border effects also exist within a country, known as the *domestic border effect* or intranational home bias. For example, Wolf (2000) finds that trade within individual US states is significantly larger than trade between US states even after he controls for economic size, distance, and a number of additional determinants. Similarly, despite the absence of formal international trade barriers associated with the single market, Nitsch (2000) finds that domestic trade within the average European Union country is about 10 times larger than trade with another EU country.[2]

It is important to understand the nature of domestic and international trade barriers since they might impede the integration of markets and have negative welfare consequences. Accurately identifying the magnitudes of border effects at the domestic and international levels is a necessary step for assessing their economic significance. The contribution of this article is to merge the two strands of literature about border effects into a unified framework. We construct a data set that includes three tiers of US trade flows: (i) trade within individual US states, e.g., Minnesota–Minnesota; (ii) trade between US states, e.g., Minnesota–Texas; and (iii) trade between US states and foreign countries, e.g., Minnesota–Canada.[3]

We use gravity theory to estimate the relative size of the domestic and international border effects. As is typical in the literature, the domestic border effect indicates how much a US state trades with itself relative to state-to-state trade, while the international border effect indicates how much a US state trades with foreign countries relative to state-to-state trade. After controlling for distance and economic size, we find that relative to state-to-state trade, crossing an individual US state's domestic border entails a *larger* trade barrier than crossing the international US border. Put differently, although trading internationally is of course more costly in total than trading intranationally, our results indicate that the estimated marginal increase in trade barriers when leaving the domestic state is relatively larger than the increase associated with leaving the United States.

What are the economic reasons behind the large domestic border effect? International trade economists traditionally emphasize trade barriers

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[2] An earlier study by Wei (1996) finds similar results for OECD countries. Nikolaus Wolf (2009) finds sizeable domestic border barriers in the historical context for Germany in the late 19th and early 20th centuries. Chen (2004) documents significant intra-European Union border effects at the industry level.

[3] Other papers, such as Hillberry and Hummels (2008), have used geographically more finely disaggregated US trade data. However, these data and the related papers pertain only to the question of the domestic border effect. They are silent on the international border effect. Our innovation is in combining US domestic and international trade data for the first time.

associated with international borders such as tariffs, bureaucratic hurdles, and informational barriers. Although beginning with Wolf (2000) and Nitsch (2000) the empirical literature has also demonstrated that borders within a country are associated with a significant trade-impeding effect, it is much harder to think of administrative and informational barriers that coincide with state borders within the same country. Instead, one plausible explanation is related to work by Hillberry and Hummels (2008). Based on ZIP-code-level domestic US trade flows, they document that trade within the United States is heavily concentrated at the local level. In particular, trade within a single ZIP code is on average three times higher than trade with partners outside the ZIP code. This concentration might be due to the prevalence of trade in intermediate goods at the local level, arguably as a result of supply chain optimization as companies seek to minimize transportation costs and suppliers co-locate with final goods producers. This high concentration of trade at the local level implies large domestic border barrier estimates. In that interpretation, the estimated domestic border effect does not reflect state-border barriers per se but rather local agglomeration effects. But of course, the fact that firms cluster in areas as small as a single ZIP code might be indicative in itself of trade costs associated with relatively short distances. As we discuss in Section 5, other reasons for the strong local concentration of trade include informational and search costs, for example in the form of business, social, and immigration networks, increasing returns at the local level as well as location-specific tastes.

Given the large literature on border effects, it can arguably be seen as a logical extension to estimate international and domestic border effects in a joint framework so that they can be directly compared. In fact, research by Fally, Paillacar, and Terra (2010) is related to our work. As part of a study examining wage differences across Brazilian states, they estimate a gravity equation in which bilateral trade flows are explained by a set of trade cost variables that include both domestic and international border effects. Consistent with our results for the United States, their estimates imply that the average Brazilian state border has a relatively *larger* negative impact on bilateral trade flows than the international border.[4]

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4 Given the three sets of trade flows and two dummy variables reflecting border effects, it is necessary to decide which set of trade flows to use as the base or omitted category. In our paper the base is trade between US states, while Fally, Paillacar, and Terra (2010) use trade within Brazilian states as the base. Thus, we generate a positive estimate for the ownstate border effect and a negative estimate for the international border effect, while Fally, Paillacar, and Terra (2010) generate negative estimates for both border effects. In other words, relative to state-to-state trade, we find that within-state trade is relatively higher and international trade is relatively lower. For Fally, Paillacar, and Terra (2010), relative to within-state trade, both state-to-state trade and international trade are lower. In the first column of their Table 2, they report an estimate of −2.594 for their internal border dummy and an estimate of −4.326 for their international border dummy in a

On the other hand, results using Chinese trade data indicate that in a number of instances the domestic (i.e., provincial) border tends to have a relatively *smaller* negative effect on trade flows than the international border. For example, Poncet (2003) finds that the international border effect exceeds the domestic border effect for 1987 and 1992 (but not for 1997). Similarly, the results by De Sousa and Poncet (2011) indicate that the international border effect exceeds the domestic border effect for the years 1995, 1999, 2002, 2005, and 2007.[5] In contrast, Hering and Poncet (2010) find that the domestic border effect exceeds the international border effect for 1997.

The article is organized as follows. In Section 2, we carefully examine the general equilibrium theory of trade with trade barriers to derive our empirical estimation framework. In Section 3, we describe the data set which we use in Section 4 to estimate international and domestic border effects. In Section 5, we discuss a number of potential explanations for our empirical results. Section 6 concludes.

2 Gravity Theory and the Estimation Framework

2.1 Gravity theory

Gravity equations can be derived from a variety of trade models, such as the gravity framework with multilateral resistance by Anderson and van Wincoop (2003), the Ricardian trade model by Eaton and Kortum (2002), Chaney's (2008) extension of the Melitz (2003) heterogeneous firms model as well as the heterogeneous firms model by Melitz and Ottaviano (2008) with a linear demand system.[6] To obtain results that are easily comparable to the previous literature on border effects, we adopt the widely used gravity framework by Anderson and van Wincoop (2003). Our results, however, could also be generated with the other frameworks.

Anderson and van Wincoop's (2003) parsimonious model rests on the Armington assumption that countries produce differentiated goods and

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log-linear regression with exporter and importer fixed effects and controls for distance and other bilateral trade costs. Their border estimates are directly comparable to ours due to the Frisch-Waugh theorem. Their estimates imply that trade within Brazilian state is on average 13.4 times larger than trade between Brazilian states [exp(2.594) = 13.4], whereas trade between Brazilian states is only 5.7 times larger than trade with foreign countries [exp(4.326−2.594) = 5.7]. In that sense, their results also imply that the domestic border appears to entail a larger trade barrier than the international border.

[5] It is unclear though whether the differences between the domestic and international border effect point estimates are statistically significant, especially for the earlier years. Similar to the previous footnote, the coefficients have to be transformed appropriately to make them directly comparable to ours.

[6] See Chen and Novy (2011) for an overview.

trade is driven by consumers' love of variety. They derive the following
gravity equation for exports x_{ij} from region i to region j:

$$x_{ij} = \frac{y_i y_j}{y^W} \left(\frac{t_{ij}}{\Pi_i P_j} \right)^{1-\sigma}, \qquad (1)$$

where y_i and y_j denote output of regions i and j, y^W denotes world output,
t_{ij} is the bilateral trade cost factor (one plus the tariff equivalent), Π_i is the
outward multilateral resistance term, and P_j is the inward multilateral
resistance term. The parameter $\sigma > 1$ is the elasticity of substitution.
The bilateral trade costs t_{ij} capture a variety of trade frictions such as
transportation costs, tariffs, and bureaucratic barriers, and they also
include the border barriers.

2.2 The estimation framework

We follow McCallum (1995) and other authors by hypothesizing that
trade costs t_{ij} are a log-linear function of geographic distance, $dist_{ij}$, and
a border dummy, $INTERNATIONAL_{ij}$, which takes on the value 1 when-
ever regions i and j are located in different countries. In addition, we
hypothesize that domestic trade costs within a region's own territory
might be systematically different from bilateral trade costs. We therefore
include an ownstate dummy variable, $OWNSTATE_{ij}$, that takes on the
value 1 for $i = j$. Our trade cost function can thus be expressed as

$$\ln(t_{ij}) = \tilde{\beta}\, INTERNATIONAL_{ij} + \tilde{\gamma}\, OWNSTATE_{ij} + \tilde{\delta}\ln(dist_{ij}), \quad (2)$$

where $\tilde{\beta}$ and $\tilde{\gamma}$ reflect the international and the ownstate (i.e., domestic)
border effects, respectively, and $\tilde{\delta}$ is the elasticity of trade costs with
respect to distance.

The trade cost function (2) nests the trade cost functions used by Wolf
(2000), Hillberry and Hummels (2003), and Anderson and van Wincoop
(2003). Wolf (2000) and Hillberry and Hummels (2003) only consider
trade flows within the US so that an international border effect cannot
be estimated. This corresponds to $\tilde{\beta} = 0$ in Equation (2). Anderson and
van Wincoop (2003) follow McCallum's (1995) specification that does not
allow for a domestic border effect ($\tilde{\gamma} = 0$).

We log-linearize Equation (1) so that we obtain

$$\ln(x_{ij}) = \ln(y_i) + \ln(y_j) - \ln(y^W) + (1-\sigma)\ln(t_{ij}) + (\sigma-1)\ln(\Pi_i P_j). \quad (3)$$

Substituting the trade cost function (2) yields the following estimating
equation:

$$\begin{aligned}
\ln(x_{ij}) = {}& \ln(y_i) + \ln(y_j) + \beta\, INTERNATIONAL_{ij} + \gamma\, OWNSTATE_{ij} \\
& + \delta \ln(dist_{ij}) + (\sigma-1)\ln(\Pi_i P_j) + \alpha + \varepsilon_{ij},
\end{aligned} \qquad (4)$$

where $\beta = (1-\sigma)\,\tilde{\beta}$, $\gamma = (1-\sigma)\,\tilde{\gamma}$, and $\delta = (1-\sigma)\,\tilde{\delta}$ and where the logarithm of world output is captured by the constant α and where we add a white-noise error term ε_{ij}.

2.3 Border effects in theory

The empirical literature typically finds that international borders impede trade. This corresponds to $\beta < 0$ in estimating Equation (4). Trading within a state is typically associated with higher trade flows, corresponding to $\gamma > 0$. We first examine whether gravity theory allows us to predict whether the international border effect β is larger or smaller in absolute value than the domestic border effect γ, i.e., whether $|\beta| \gtrless |\gamma|$.

As we explain below in more detail, our data set comprises three tiers of trade flows:

(i)   ownstate trade: trade flows within a US state, for example within Minnesota, such that $OWNSTATE_{ij} = 1$ and $INTERNATIONAL_{ij} = 0$,
(ii) national trade: trade flows between two US states, for example from Minnesota to Texas, such that $OWNSTATE_{ij} = INTERNATIONAL_{ij} = 0$, and
(iii) international trade: trade flows from a US state to a foreign country, for example from Minnesota to Canada, such that $OWNSTATE_{ij} = 0$ and $INTERNATIONAL_{ij} = 1$.

The second tier is thus the omitted category in Equation (4), implying that the ownstate border effect is estimated relative to the benchmark of trade between US states. We choose this benchmark to obtain coefficients which are directly comparable to those in the literature (Nitsch, 2000; Wolf, 2000). Therefore, the sign and magnitude of the ownstate border effect can be gauged by comparing trade costs t_{ii} within a typical US state i to bilateral trade costs t_{ij} with another US state j. We draw this comparison by considering their ratio t_{ii}/t_{ij}. Equation (1) for ownstate trade x_{ii} and bilateral trade x_{ij} and Equation (2) for t_{ii} and t_{ij} imply that this ratio is given by

$$\frac{t_{ii}}{t_{ij}} = \left(\frac{x_{ij}y_i}{x_{ii}y_j}\right)^{\frac{1}{\sigma-1}}\frac{P_i}{P_j} = \frac{\exp(\tilde{\gamma})(dist_{ii})^{\tilde{\delta}}}{(dist_{ij})^{\tilde{\delta}}}.$$

Using $\tilde{\gamma} = \gamma/(1-\sigma)$ and $\tilde{\delta} = \delta/(1-\sigma)$ this can be rewritten as

$$\exp(\gamma) = \frac{x_{ii}\,y_j}{x_{ij}\,y_i}\left(\frac{P_j}{P_i}\right)^{\sigma-1}\left(\frac{dist_{ij}}{dist_{ii}}\right)^{\delta}. \tag{5}$$

As a simple example, first assume the symmetric case where $y_i = y_j$, $P_i = P_j$, and $dist_{ii} = dist_{ij}$. A positive ownstate effect $\gamma > 0$ would follow only if $x_{ii}/x_{ij} > 1$. Now assume the more representative case where bilateral distance $dist_{ij}$ exceeds domestic distance $dist_{ii}$. Given that the distance elasticity of trade is negative ($\delta < 0$), an even bigger ratio x_{ii}/x_{ij} would be required to ensure $\gamma > 0$. More generally, we conclude that given the distance element of trade costs as well as the output and multilateral resistance variables, the sign and magnitude of the domestic border effect parameter γ will primarily depend on the extent of domestic trade x_{ii} relative to bilateral trade x_{ij}.

As in the literature, we also use the benchmark of trade between US states for estimating the international border effect. To gauge its sign and magnitude, we compare bilateral trade costs t_{ik} between a typical US state i and a typical foreign country k to trade costs t_{ij} between two US states. Their ratio is given by

$$\frac{t_{ik}}{t_{ij}} = \left(\frac{x_{ij}}{x_{ik}} \frac{y_k}{y_j}\right)^{\frac{1}{\sigma-1}} \frac{P_k}{P_j} = \frac{\exp(\tilde{\beta})(dist_{ik})^{\tilde{\delta}}}{(dist_{ij})^{\tilde{\delta}}},$$

or

$$\exp(\beta) = \frac{x_{ik}}{x_{ij}} \frac{y_j}{y_k} \left(\frac{P_j}{P_k}\right)^{\sigma-1} \left(\frac{dist_{ij}}{dist_{ik}}\right)^{\delta}. \tag{6}$$

As before, assume the simple symmetric case where $y_k = y_j$, $P_k = P_j$, and $dist_{ik} = dist_{ij}$. A negative international border effect $\beta < 0$ would follow only if $x_{ik}/x_{ij} < 1$. In the more common case where international distance $dist_{ik}$ (say, between Minnesota and Japan) exceeds inter-state distance $dist_{ij}$ (say, between Minnesota and Texas), an even smaller ratio x_{ik}/x_{ij} would be required to ensure $\beta < 0$. Given distances as well as the output and multilateral resistance variables, the international border effect parameter β will therefore mainly depend on the extent of international trade x_{ik} relative to inter-state trade x_{ij}.

Thus, Equations (5) and (6) can in principle yield either sign for γ and β. The fact that most empirical studies find $\gamma > 0$ or $\beta < 0$ is consistent with but by no means implied by gravity theory. Neither does gravity theory make a prediction about the absolute magnitudes of β and γ. *A priori* we therefore cannot infer whether $|\beta| \gtrless |\gamma|$.[7]

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[7]   The conclusion that β and γ are not bounded by theory would also go through if we relaxed the symmetry assumption for the output and multilateral resistance variables.

3 Data

To obtain comparable results, we use the same data sets as Wolf (2000) and Anderson and van Wincoop (2003) for domestic trade flows within the United States. The novelty of our approach is to combine these domestic trade flows with international trade flows from individual US states to the 50 largest US export destinations. Thus, our data set comprises, for instance, trade flows within Minnesota, exports from Minnesota to Texas as well as exports from Minnesota to Canada.[8] We take data quality seriously, and below we describe in detail the data sources, potential concerns, and how we address these concerns.

3.1 Domestic exports: Commodity Flow Survey

For our measures of the shipments of goods within and across US states, we use aggregate trade data from the Commodity Flow Survey, which is a joint effort of the Bureau of Transportation Statistics and the Bureau of the Census. We use survey results from 1993, 1997, and 2002. The survey covers the origin and destination of shipments of manufacturing, mining, wholesale trade, and selected retail establishments. The survey excludes shipments in the following sectors: services, crude petroleum and natural gas extraction, farm, forestry, fishery, construction, government, and most retail. Shipments from foreign establishments are also excluded; import shipments are excluded until they reach a domestic shipper. US export (i.e., trans-border) shipments are also excluded.

3.2 International exports: Origin of Movement

Our data on exports by US states to foreign destinations are from the Origin of Movement series.[9] These data are compiled by the Foreign Trade Division of the US Bureau of the Census. The data in this series identify the state from which an export begins its journey to a foreign country. However, we would like to know the state in which the export was produced. Below we provide details on the Origin of Movement series and its suitability as a measure of the origin of production.[10]

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[8] There are similarities and differences between the data sets used in Anderson and van Wincoop (2003) and our work. As noted, we both combine domestic and international trade flows. For example, we both use state-to-state trade flows (48 states in our case and 30 states in Anderson and van Wincoop) as well as trade flows that cross international borders. The key difference is that our data set additionally includes intra-state flows. As a result, we are able to estimate both state and international border effects, while Anderson and van Wincoop focus on the latter only.

[9] Other studies that have used the Origin of Movement series include Smith (1999), Coughlin and Wall (2003), Coughlin (2004), and Cassey (2011).

[10] The highlighted details as well as much additional information can be found in Cassey (2009).

Beginning in 1987, the Origin of Movement series provides the current-year export sales, or free-alongside-ship (f.a.s.) costs if not sold, for 54 'states' to 242 foreign destinations. These export sales are for merchandize sales only and do not include services exports. The 54 'states' include the 50 US states plus the District of Columbia, Puerto Rico, US Virgin Islands, and unknown. Following Wolf (2000), we use the 48 contiguous US states. Rather than all 242 destinations, we use the 50 leading export destinations for US exports for 2005.[11] We use the annual data from 1993, 1997, and 2002 for total merchandize exports.[12]

Concerns about using the Origin of Movement series to identify the location of production are especially pertinent for agricultural and mining exports.[13] Cassey (2009) has examined the issue of the coincidence of the state origin of movement and the state of production for manufactured goods.[14] The reason for restricting the focus to manufacturing is that the best source for location-based data on export production, 'Exports from Manufacturing Establishments,' covers only manufacturing.[15]

Cassey's key finding relevant to our analysis is that, overall, the Origin of Movement data is of sufficient quality to be used as the origin of the production of exports. Nonetheless, the data for specific states may not be of sufficient quality as the origin of production. These states are Alaska, Arkansas, Delaware, Florida, Hawaii, New Mexico, South Dakota, Texas, Vermont, and Wyoming. He recommends the removal of Alaska and Hawaii in particular. As we use the 48 contiguous US states, our data set is consistent with this recommendation. The next two candidates for removal would be Delaware and Vermont. Cassey further highlights that the consolidation of export shipments might systematically affect the Origin of Movement estimates (relative to the origin of production). Specifically, consolidation tends to bias upward the estimates for Florida and Texas and to bias downward the estimates for Arkansas

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[11] Alphabetically, the countries are Argentina, Australia, Austria, Belgium, Brazil, Canada, Chile, China, Colombia, Costa Rica, Denmark, Dominican Republic, Ecuador, Egypt, El Salvador, Finland, France, Germany, Guatemala, Honduras, Hong Kong, India, Indonesia, Ireland, Israel, Italy, Japan, Kuwait, Malaysia, Mexico, Netherlands, New Zealand, Norway, Panama, Peru, Philippines, Russia, Saudi Arabia, Singapore, South Africa, South Korea, Spain, Sweden, Switzerland, Taiwan, Thailand, Turkey, United Arab Emirates, UK, and Venezuela.

[12] We have also tried the data for manufacturing only (as opposed to total merchandise). The two series are very highly correlated (99%). The regression results are almost identical, and we therefore do not report them.

[13] See http://www.trade.gov/td/industry/otea/state/technote.html.

[14] For the initial work on this issue, see Coughlin and Mandelbaum (1991) as well as Cronovich and Gazel (1999).

[15] The data in the 'Exports from Manufacturing Establishments' are available at http://www.census.gov/mcd/exports/ but does not contain destination information, so it cannot be used for the current research project.

and New Mexico. As a robustness check, we drop these states from the sample (see Section 4.3).

3.3 Adjustments for data comparability

Our simultaneous use of the intra-state and inter-state shipments data from the Commodity Flow Survey and the merchandize international trade data from the Origin of Movement series requires an adjustment to increase the comparability of these data sets. As in Anderson and van Wincoop (2003), such an adjustment arises because of three important differences between the data sources, the net effect of which is to increase the commodity flow estimates relative to the international trade flow estimates. First, the merchandize international trade data capture a shipment from the source to the port of exit just once, whereas the commodity flow data likely captures a good in a shipment more than once, recorded in more than one shipment. For example, a good may be shipped from a plant to a warehouse and, later, to a retailer. In this case, the value of the good will be counted twice. But if the good had been exported, its value would have been counted just once as it was shipped from the source to the port of exit. Second, goods destined for foreign countries, when they are shipped to a port of exit, are included in domestic shipments. Third, the coverage of sectors differs between the data sources. The Commodity Flow Survey includes shipments of manufactured goods, but it excludes agriculture and part of mining. Meanwhile, the merchandize trade data include all goods.

Identical to Anderson and van Wincoop (2003), we scale down the data in the Commodity Flow Survey by the ratio of total domestic merchandize trade to total domestic shipments from the Commodity Flow Survey. Total domestic merchandize trade is approximated by gross output in the goods-producing sectors (i.e., agriculture, mining, and manufacturing) minus international merchandize exports.[16] This calculation yields an adjustment factor of 0.495 for 1993, 0.508 for 1997, and 0.430 for 2002.[17] Similar to Anderson and van Wincoop (2003), our adjustment to the commodity flow data does not solve all the measurement problems, but it is the best feasible option.

3.4 Other data

The rest of the data used in our estimations can be characterized as either well-known or straightforward. For individual US states, we use state

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[16] See Helliwell (1997, 1998) and Wei (1996).

[17] The difference between our adjustment factor for 1993 and that of Anderson and van Wincoop, 0.495 versus 0.517, is due to data revision.

gross domestic product data from the US Bureau of Economic Analysis. For foreign countries, we use data on gross domestic product taken from the IMF World Economic Outlook Database (October 2007 edition).

We use the standard great circle distance formula to cover inter-state and international distances between capital cities in kilometers. As intra-state distance, we use the distance between the two largest cities in a state. As alternatives for intra-state distance, we also try the measure used by Wolf (2000) that weights the distance between a state's two largest cities by their population, as well as the measure suggested by Nitsch (2000) that is based on land area. Finally, we also use a distance measure that is related to actual shipping distances, based on data for individual shipments used by Hillberry and Hummels (2003), see Section 4.3 for details.

4 Empirical Results

We form a sample that is balanced over the years 1993, 1997, and 2002. This yields 1801 trade observations per cross-section within the US.[18] Adding 50 foreign countries as export destinations increases the number of trade observations by 2338 so that our sample includes 4139 observations per cross-section, or 12 417 in total.[19] Recall that due to the data quality concerns as well as for consistency reasons, Alaska, Hawaii, and Washington, DC were dropped, so we use the 48 contiguous US states.

First, we show that our data exhibit a substantial domestic border effect, as established by Wolf (2000). In separate regressions, we also show that the data exhibit a significant international border effect, as established by McCallum (1995). Second, we combine the domestic US trade data with the international observations. This allows us to estimate the domestic and international border effects jointly and to directly compare their magnitudes. Finally, we carry out a number of robustness checks.

4.1 Domestic and international border effects estimated separately

In Columns 1 and 2 of Table 1, we show results that replicate the intranational home bias. For comparison with Wolf (2000) who uses a sample

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[18] The maximum possible number of US observations would be $48 \times 48 = 2304$. The 503 missing observations are due to the fact that a number of Commodity Flow Survey estimates did not meet publication standards because of high sampling variability or poor response quality.

[19] The maximum possible number of international observations would be $48 \times 50 = 2400$. Sixty-two observations are missing mainly because exports to Malaysia were generally not reported in 1993. Only 18 of the observations not included in our sample are most likely zeros (as opposed to missing).

Table 1 Domestic and international border effects, estimated separately

Sample	United States only			United States and 50 countries	
Year	1993	1993, 1997, 2002	1993, 1997, 2002	1993	1993, 1997, 2002
	(1)	(2)	(3)	(4)	(5)
$ln(y_i)$			0.92**	1.29**	1.22**
			(0.02)	(0.02)	(0.02)
$ln(y_j)$			0.91**	0.83**	0.83**
			(0.02)	(0.01)	(0.01)
$ln(dist_{ij})$	−1.08**	−1.07**	−0.94**	−0.86**	−0.85**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
$OWNSTATE_{ij}$	1.46**	1.48**	1.76**		
	(0.20)	(0.19)	(0.19)		
$INTERNATIONAL_{ij}$				−1.19**	−1.04**
				(0.06)	(0.05)
National trade (reference group)	Yes	Yes	Yes	Yes	Yes
Ownstate trade	Yes	Yes	Yes	No	No
International trade	No	No	No	Yes	Yes
Observations	1801	5403	5403	4091	12 273
Clusters	−	1801	1801	−	4091
Fixed effects	Yes	Yes	No	No	No
Random effects	No	No	Yes	No	Yes
R-squared	0.90	0.90	0.82	0.79	0.78

Notes. The dependent variable is $ln(x_{ij})$. OLS estimation. Robust standard errors are reported in parentheses, clustered around country pairs ij in Columns 2, 3, and 5. Exporter and importer fixed effects in Columns 1 and 2, time-varying in Column 2; random effects in Columns 3 and 5. Constants and year dummies are not reported. ** Significant at 1% level.

for 1993, in Column 1 we only use data for that year. In Column 2, we add the data for 1997 and 2002. Like Hillberry and Hummels (2003), we use (year-specific) exporter and importer fixed effects so that the output regressors drop out. Our point estimates in Columns 1 and 2 (1.46 and 1.48) are virtually identical to Wolf's baseline coefficient of 1.48 for the ownstate indicator variable. The interpretation of this coefficient is that given distance and economic size, ownstate trade is 4.4 times higher than state-to-state trade [exp(1.48) = 4.4]. As we will use random effects in subsequent tables (see below), we also run a random effects specification that

corresponds to the fixed effects specification in Column 2. The results are reported in Column 3. Output regressors are now included. The ownstate coefficient is slightly higher (1.76 compared to 1.48 in column 2) but we cannot reject the hypothesis that the ownstate coefficient is equal to 1.48 (p-value $= 0.15$).

Hillberry and Hummels (2003) reduce the ownstate coefficient by about a third when excluding wholesale shipments from the Commodity Flow Survey data. The reason is that wholesale shipments are predominantly local so that their removal disproportionately reduces the extent of own-state trade.[20] However, Nitsch (2000) reports higher home bias coefficients by comparing trade within European Union countries to trade between EU countries. He finds home bias coefficients in the range of 1.8–2.9.

In Columns 4 and 5, we do not consider ownstate trade but rather focus on the international border effect. These regressions use the sample of 50 foreign countries. In Column 4, we estimate an international border coefficient of -1.19 for the year 1993, implying that after we control for distance and economic size, exports from US states to foreign countries are about 70% lower than exports to other US states [$\exp(-1.19) = 0.30$]. This coefficient is somewhat lower in magnitude than the estimate of -1.65 obtained by Anderson and van Wincoop (2003) with trade data between US states and Canadian provinces. When we pool the data over the years 1993, 1997, and 2002 in Column 5, the border effect is estimated at -1.04. Estimation in that column is carried out with random effects since fixed effects at the country level would be collinear with the international border dummy variable.

Overall, we conclude that we obtain estimates for domestic and international border effects in Table 1 that are broadly consistent with the literature.

4.2 Is the international border effect larger than the domestic border effect?

In Table 2, we turn to estimating the domestic and international border effects jointly so that their magnitudes are directly comparable. For this purpose, we simultaneously use domestic and international trade flows, while continuing to use inter-state trade as the reference group as in Table 1. When we pool the data over the years 1993, 1997, and 2002 in

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[20] We do not have access to the private-use coding of wholesale shipments and thus cannot replicate their finding with our data. However, our main results in Table 2 on the relative size of the domestic and international border effects is qualitatively robust to a reduction by a third in the ownstate coefficient magnitudes. Hillberry and Hummels (2003) further reduce the ownstate coefficient by using an alternative distance measure that is based on actual shipping distances. We refer to Section 4.3 where we employ such a measure, but our main result is unchanged.

Table 2 Domestic and international border effects, estimated jointly

Sample	United States and 50 countries	
Year	1993	1993, 1997, 2002
	(1)	(2)
$ln(y_i)$	1.28**	1.21**
	(0.02)	(0.02)
$ln(y_j)$	0.82**	0.82**
	(0.01)	(0.01)
$ln(dist_{ij})$	−0.83**	−0.82**
	(0.03)	(0.03)
$OWNSTATE_{ij}$	2.04**	2.05**
	(0.20)	(0.20)
$INTERNATIONAL_{ij}$	−1.24**	−1.10**
	(0.06)	(0.05)
$|OWNSTATE_{ij}|=|INTERNATIONAL_{ij}|$	[0.00]	[0.00]
National trade (reference group)	Yes	Yes
Ownstate trade	Yes	Yes
International trade	Yes	Yes
Observations	4139	12 417
Clusters	–	4139
Random effects	No	Yes
R^2	0.79	0.79

Notes. The dependent variable is $ln(x_{ij})$. OLS estimation. Robust standard errors are reported in parentheses, clustered around country pairs ij in Column 2. Random effects in Column 2. Constants and year dummies are not reported. **Significant at 1% level. The numbers in brackets report p-values for the test $|OWNSTATE_{ij}|=|INTERNATIONAL_{ij}|$.

Column 2, we add random effects instead of country fixed effects. The reason is again that country fixed effects would be perfectly collinear with the ownstate and international dummy variables. Exporter and importer fixed effects would also be impractical because of collinearity with the international border dummy.[21]

Columns 1 and 2 show that the ownstate coefficients are estimated at 2.04 and 2.05, while the international coefficients are estimated at −1.24 and −1.10. The hypothesis that the two border coefficients in each column are equal in absolute magnitude is clearly rejected (p-value = 0.00). Thus, a key finding in Table 2 is that the domestic border effect is larger in

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[21] The collinearity arises because the foreign countries in our data set are only importers but never exporters.

absolute magnitude than the international border effect. That is, relative to inter-state trade, crossing an individual US state's domestic border is estimated to entail a *larger* trade barrier than crossing the international US border.

Another observation is that the joint estimation in Table 2 yields somewhat different estimates of the domestic border effect. The coefficient on $OWNSTATE_{ij}$ is 1.48 when estimated separately with fixed effects (see Table 1, Column 2) and 1.76 when estimated separately with random effects (see Table 1, Column 3), and 2.05 when estimated jointly (see Table 2, Column 2).[22] Note that the distance coefficient in those columns changes from -1.07 and -0.94, respectively, to -0.82, and the latter value is close to the distance coefficients in Columns 4 and 5 of Table 1. Likewise, the income elasticities are also similar to those estimated in Columns 4 and 5 of Table 1.

4.3 Robustness

Various authors, such as Helliwell and Verdier (2001) and Head and Mayer (2009), have pointed out that the estimation of border effects is sensitive to how distance is measured. For example, if the relevant economic distance within a US state is much shorter than indicated by conventional measures—perhaps because economic activity is highly concentrated in two nearby cities—then it might no longer be surprising if a state trades considerably more within its boundaries than with partners further away. To address this concern we employ three alternative distance measures that have been suggested in the literature.

Column 1 of Table 3 uses the alternative measure for ownstate distance proposed by Wolf (2000). This measure weights the distance between a state's two largest cities by their population. It thus better reflects heavy concentration of economic activity in relatively small areas. For example, most economic activity in Utah is concentrated around Salt Lake City such that the conventional great circle distance measure could easily overstate actual shipping distances. As expected, on average this alternative measure results in shorter ownstate distances (109 km versus 179 km) so that it reduces the domestic border effect compared to Table 2. In particular, the coefficient on $OWNSTATE_{ij}$ declines from 2.05 (Table 2,

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22 Given that the estimates from the different tables (in particular, 1.48 from Column 2 of Table 1 and 2.05 from Column 2 of Table 2) stem from separate regressions, it is of course not possible to carry out a direct test of whether they are statistically different from each other. But although the point estimate of 2.05 is significantly different from the value 1.48 and the point estimate of 1.48 is significantly different from the value 2.05, it is possible to find an intermediate value, say, 1.76 as in Column 3 of Table 1, from which neither 1.48 in Column 2 of Table 1 nor 2.05 in Column 2 of Table 2 are significantly different.

Table 3 Robustness checks

Sample	United States and 50 countries			United States and 50 countries		United States and 50 countries			
Years: 1993, 1997, 2002				Distance > 200 m.	Fewer states	Adjacency	Language	Currency	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$ln(y_i)$	1.21**	1.21**	1.21**	1.22**	1.21**	1.21**	1.21**	1.21**	1.21**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
$ln(y_j)$	0.81**	0.82**	0.79**	0.82**	0.81**	0.81**	0.80**	0.81**	0.79**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$ln(dist_{ij})$: Wolf (2000)	−0.80**								
	(0.03)								
$ln(dist_{ij})$: Nitsch (2000)		−0.84**							
		(0.03)							
$ln(dist_{ij})$: Actual shipping distance			−0.76**						
			(0.02)						
$ln(dist_{ij})$				−0.81**	−0.78**	−0.65**	−0.86**	−0.82**	−0.70**
				(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
$adjacency_{ij}$						1.11**			1.01**
						(0.07)			(0.07)
$language_{ij}$							0.88**		0.83**
							(0.06)		(0.06)
$currency_{ij}$								−0.10	−0.12
								(0.09)	(0.08)
$OWNSTATE_{ij}$	1.64**	2.23**	1.50**	2.08**	2.03**	1.48**	1.96**	2.05**	1.44**
	(0.22)	(0.17)	(0.20)	(0.20)	(0.22)	(0.19)	(0.20)	(0.20)	(0.19)
$INTERNATIONAL_{ij}$	−1.13**	−1.06**	−0.24**	−1.10**	−1.21**	−1.28**	−0.31**	−1.19**	−0.62**
	(0.06)	(0.05)	(0.07)	(0.06)	(0.06)	(0.06)	(0.07)	(0.10)	(0.10)

Table 3 Continued

Sample	United States and 50 countries			United States and 50 countries		United States and 50 countries			
Years: 1993, 1997, 2002				Distance > 200 m.	Fewer states	Adjacency	Language	Currency	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$|OWNSTATE_{ij}|=|INTERNATIONAL_{ij}|$	[0.02]	[0.00]	[0.00]	[0.00]	[0.00]	[0.30]	[0.00]	[0.00]	[0.00]
National trade (reference group)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ownstate trade	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
International trade	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12 417	12 417	12 417	12 117	10 368	12 417	12 417	12 417	12 417
Clusters	4139	4139	4139	4039	3456	4139	4139	4139	4139
Random effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.78	0.79	0.79	0.78	0.79	0.79	0.80	0.79	0.80

Notes. The dependent variable is $ln(x_{ij})$. OLS estimation. Robust standard errors are reported in parentheses, clustered around country pairs ij. Random effects in all columns. Constants and year dummies are not reported. **Significant at 1% level. The numbers in brackets report p-values for the test $|OWNSTATE_{ij}|=|INTERNATIONAL_{ij}|$. Column 4 drops all pairs of US states that are less than 200 miles apart. Column 5 drops states with inferior data quality (AR, DE, FL, NM, TX, VT). Column 6 adds an adjacency dummy that is 1 if two regions have a land border. Column 7 adds a common currency dummy. Column 8 adds a common currency dummy. Column 9 combines the adjacency, language, and currency dummies.

Column 2) to 1.64 (Table 3, Column 1). Despite the smaller magnitudes of the domestic border effect, it is still significantly different from the absolute value of the international border estimate in Column 1 of Table 3 (the p-value is 0.02). Also note that compared to Column 2 of Table 2, the result for the international border effect in Column 1 of Table 3 is virtually the same (-1.13 compared to -1.10). A similar observation can be made concerning the distance coefficient.

In Column 2 of Table 3, we employ a measure of ownstate distance which is based on land area as in Nitsch (2000). His measure is based on a hypothetical circular economy with three equal-sized cities, one in the center and the other two on opposite sides of the circle. The average internal distance of such an economy, and also other economies with more complex structures, can be approximated by the radius of the circle. In the data, this is computed as $1/\sqrt{\pi} = 0.56$ times the square root of the area in km^2, and on average this results in roughly similar ownstate distances (170 km versus 179 km). Nevertheless, the ownstate dummy estimate increases slightly to 2.23 compared to 2.05 in Column 2 of Table 2.

In Column 3, we employ a third alternative distance measure that is closer to actual shipping distances by ground transportation observed within the United States. Based on private-use Commodity Flow Survey data at the ZIP code level, Hillberry and Hummels (2003, Equation 4 and Table 1) provide a statistical relationship between the distance measure used by Wolf (2000), an ownstate dummy and an adjacency dummy. They estimate the following equation:

$$\ln(actual\,dist_{ij}) = \lambda_1 \ln(Wolf\,dist_{ij}) + \lambda_2 OWNSTATE_{ij} + \lambda_3 adjacency_{ij} + e_{ij} \tag{7}$$

with $\lambda_1 = 0.821$, $\lambda_2 = -0.498$, and $\lambda_3 = -0.404$. We use these coefficients to approximate actual shipping distances within the United States as well as to Canada and Mexico, and we then use them as an explanatory variable. The resulting distances are on average considerably shorter compared to the great circle distances, both within US states (18 km versus 179 km) as well as between US states and to Canada and Mexico (450 km versus 1556 km). The distances to overseas countries are not affected as those routes are not covered by ground transportation. In Column 3 of Table 3, both border coefficients are reduced in magnitude to 1.50 from 2.05 for the ownstate coefficient and to -0.24 from -1.10 for the international coefficient compared to Column 2 of Table 2. But note that the absolute difference between the coefficients remains highly significant.

Results for additional robustness checks are reported in the remaining columns of Table 3. Hillberry and Hummels (2008) document that trade is highly concentrated at the local level and that it consists to some extent of

local wholesale shipments. In Column 4, we provide results for trade between locations that are not within immediate proximity to limit the potential influence of wholesale shipments. In particular, we drop all state-to-state observations that are less than 200 miles apart to check whether they distort the sample. This check removes 100 cross-sections from the panel. Nonetheless, the regression results are virtually the same as in Column 2 of Table 2. We obtain similar results if we also drop all within-state observations less than 200 miles apart (not reported here).

As we explain in Section 3.2, Cassey (2009) raises doubts as to whether the Origin of Movement data are sufficiently similar to the actual origin of production in the case of Arkansas, Delaware, Florida, New Mexico, Texas, and Vermont. In Column 5, we drop these six states from our sample. Once again, the regression results are overall quite similar to those in Table 2.

In Column 6, we follow Wolf (2000) by adding an adjacency dummy that takes on the value 1 whenever two states are neighboring (say, Minnesota and Wisconsin).[23] Similar to Wolf (2000), we find that adding an adjacency dummy reduces the ownstate coefficient. Nevertheless, the domestic border effect remains larger in the absolute value than the international border effect. However, in Column 6, we can no longer reject the hypothesis that their absolute values are equal (p-value $= 0.30$).

In Column 7, we control for a common language dummy that takes on the value 1 whenever countries have English as an official language according to the CIA World Factbook. In our sample, these countries are Australia, Canada, Hong Kong, India, Ireland, New Zealand, Singapore, South Africa, and the UK. For all intra-US observations, the common language dummy is also set to take on the value 1. We note that a dummy variable for common legal origin (common law) would be exactly the same in our sample. Thus, it should arguably be interpreted as a broader measure of cultural and political similarity. As typical in the gravity literature, the language dummy is positive and highly significant. Compared to Column 6, its inclusion increases the ownstate coefficient to 1.96, and the international dummy coefficient is considerably reduced in the absolute value to -0.31.

In Column 8, we use a dummy variable for a common currency. It takes on the value 1 whenever one of the foreign countries uses the US dollar as their official currency, or where the local currency is freely exchanged against the US dollar, or where countries tied their currency against the US dollar for at least one of the years of our sample. In our sample these

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[23] All ownstate observations are defined to also count as adjacent observations in our sample.

countries are Argentina, Ecuador, El Salvador, Hong Kong, and Panama, and we also include all intra-US observations.[24] However, the common currency dummy turns out insignificant.

Finally, in Column 9, we combine the three additional trade cost regressors from Columns 6–8. The domestic border effect coefficient follows as 1.44, and the international border effect coefficient stands at −0.62. Statistically their absolute values are strongly different from each other (p-value $= 0.00$). This result shows that once we use a more complete trade cost function that controls for a wider range of trade cost elements, our main finding is corroborated: the domestic border effect appears larger in absolute value than the international border effect.

In Table 4, we carry out a number of additional robustness checks that alter the trade cost function (2). The results in Table 1 are characterized by a larger distance elasticity in absolute value for the domestic border effect regressions than for the international border effect regressions. This suggests that the trade cost function (2), which is log-linear in distance, could be problematic when applied to the pooled sample in Table 2. Instead, it might be more appropriate to use a trade cost function that allows for a larger distance elasticity at relatively short distances (typically associated with domestic border effect regressions) and for a smaller distance elasticity at relatively longer distances (typically associated with international border effect regressions). In Column 1 of Table 4, we adopt such a trade cost function in the form of a double-logarithmic specification for distance.[25] Of course, the distance coefficient now takes on a different value (−5.78 as opposed to a value in the vicinity of −1 as in the previous regressions) but it remains highly significant. The regression retains its explanatory power, yielding an R-squared of 79%. Most importantly, although the coefficient on $OWNSTATE_{ij}$ declines from 2.05 (Table 2, Column 2) to 1.53, it is still larger in absolute value than the $INTERNATIONAL_{ij}$ coefficient. But their difference is no longer statistically significant given the corresponding p-value of 0.27.

For completeness, in Column 2 of Table 4, we consider the opposite case of a trade cost function that implies a smaller distance elasticity at shorter distances. This specification uses the square of logarithmic distance. It results in a larger domestic border effect estimate equal to 2.56 so that the difference to the absolute value of the international border effect estimate becomes significant.

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[24] The source of this information is available at http://www.gocurrency.com/countries/united_states.htm.

[25] If the trade cost function depends on $\tilde{\delta}\ln[\ln(dist_{ij})]$ instead of $\tilde{\delta}\ln(dist_{ij})$ in Equation (2), then the elasticity of trade costs with respect to distance becomes d $\ln(t_{ij})$/d $\ln(dist_{ij}) = \tilde{\delta}/\ln(dist_{ij})$. This elasticity is decreasing in distance.

Table 4 Robustness checks for the functional form of distance

Sample	United States and 50 countries		United States and 50 countries	
Years: 1993, 1997, 2002			Intervals by km	Intervals by obs.
	(1)	(2)	(3)	(4)
$ln(y_i)$	1.21**	1.21**	1.21**	1.21**
	(0.02)	(0.02)	(0.02)	(0.02)
$ln(y_j)$	0.81**	0.82**	0.81**	0.84**
	(0.01)	(0.01)	(0.01)	(0.01)
$ln[ln(dist_{ij})]$	−5.78**			
	(0.24)			
$[ln(dist_{ij})]^2$		−0.05**		
		(0.00)		
$ln(dist_{ij})$: interval 1			−0.07	−0.76**
			(0.06)	(0.06)
$ln(dist_{ij})$: interval 2			−0.20**	−0.80**
			(0.06)	(0.05)
$ln(dist_{ij})$: interval 3			−0.27**	−0.85**
			(0.05)	(0.05)
$ln(dist_{ij})$: interval 4			−0.35**	−0.88**
			(0.05)	(0.04)
$ln(dist_{ij})$: interval 5			−0.37**	−0.78**
			(0.04)	(0.04)
$OWNSTATE_{ij}$	1.53**	2.56**	2.77**	1.96**
	(0.25)	(0.17)	(0.18)	(0.20)
$INTERNATIONAL_{ij}$	−1.25**	−1.08**	−0.93**	−0.80**
	(0.06)	(0.06)	(0.07)	(0.06)
$|OWNSTATE_{ij}|=|INTERNATIONAL_{ij}|$	[0.27]	[0.00]	[0.00]	[0.00]
National trade (reference group)	Yes	Yes	Yes	Yes
Ownstate trade	Yes	Yes	Yes	Yes
International trade	Yes	Yes	Yes	Yes
Observations	12 417	12 417	12 417	12 417
Clusters	4139	4139	4139	4139
Random effects	Yes	Yes	Yes	Yes
R^2	0.79	0.78	0.80	0.81

Notes. The dependent variable is $ln(x_{ij})$. OLS estimation. Robust standard errors are reported in parentheses, clustered around country pairs ij. Random effects in all columns. Constants and year dummies are not reported. **Significant at 1% level. The numbers in brackets report *p*-values for the test $|OWNSTATE_{ij}|=|INTERNATIONAL_{ij}|$. Column 1 uses the logarithm of $ln(dist_{ij})$ as a regressor. Column 2 uses the square of $ln(dist_{ij})$ as a regressor. Column 3 uses five distance intervals delineated by 750 km, 1500 km, 3000 km, and 6000 km (see the text for details). Column 4 uses five distance intervals with an equal number of observations each (see the text for details).

Inspired by Eaton and Kortum (2002), in the remaining columns of Table 4, we distinguish between several distance intervals and allow the distance coefficients to vary over these intervals. This approach represents a more flexible trade cost function. As a reference point, we note that the average distance in the domestic border effect regressions in Columns 2 and 3 of Table 1 is 1485 km with a median of 1284 km, and the average distance in the international border effect regression in Column 5 of Table 1 is 5451 km with a median of 3816 km.[26] We allow for five intervals that are supposed to reflect these different ranges. In particular, in Column 3 of Table 4, the first interval captures all bilateral observations with the shortest distances in the sample of up to 750 km. The second interval captures distances between 750 km and 1500 km, the third interval those between 1500 km and 3000 km, the fourth those between 3000 km and 6000 km, and the fifth interval captures all distances above 6000 km.[27]

It turns out that the first individual distance coefficient is not significant, suggesting that at very short distances trade is hardly sensitive to slightly longer routes. In contrast, Hillberry and Hummels (2008) document a highly nonlinear distance effect, with the distance elasticity falling as distance rises. But this effect applies to extremely short distances. For example, Hillberry and Hummels (2008) show that trade within a single US ZIP code is on average three times higher than trade with partners outside the ZIP code. But the average ZIP code has a median radius of only four miles. Likewise, Llano-Verduras, Minondo, and Requena-Silvente (2011) document a similar relationship at very short distances for geographically finely disaggregated Spanish trade data.[28] However, our sample does not focus on such short ranges. In fact, the average distance in the shortest distance interval in our sample is 439 km and thus substantially higher. The most important aspect of Column 3 for our purposes is that the domestic border effect estimate is significantly larger than that for the international border effect in absolute value. The corresponding coefficients are 2.77 and −0.93.

Finally, we allow for five intervals that contain an equal number of observations. These intervals are delineated by the 1166 km, 2589 km, 6323 km, and 9835 km marks. The results are reported in Column 4 of

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[26] As he only considers trade for Canada and the United States, McCallum (1995) compares trade flows over a similar range of distances. Our data set includes US trade with many countries outside North America so that the average distance for international flows is longer.

[27] These intervals capture 1371, 1878, 2148, 1845, and 5175 observations, respectively.

[28] Figure 1 in Hillberry and Hummels (2008) shows that the value of trade drops almost tenfold between 1 and 200 miles, with most of that decline occurring at the first few miles. Llano-Verduras, Minondo, and Requena-Silvente (2011) report sharp reductions in the value of trade for shipments between 25 and 250 km (see their Figures 1 and 2).

Table 4. It remains the case that the $OWNSTATE_{ij}$ dummy is significantly larger in absolute value than the $INTERNATIONAL_{ij}$ dummy. The values are 1.96 and -0.80, respectively.

Overall, we conclude that although the point estimates of the domestic and international border effects can change depending on the distance measure, the distance function, and the subsample, it is a robust feature of the data that the absolute magnitude of the domestic border effect exceeds that of the international border effect. Their difference is highly significant in almost all specifications.

5 Discussion

We discuss a number of potential explanations for our empirical result that the domestic border effect is comparatively large in magnitude. One major explanation is related to work by Hillberry and Hummels (2008). Based on ZIP-code-level domestic US trade flows, they document that trade within the United States is heavily concentrated at the local level: trade within a single ZIP code is on average three times higher than trade with partners outside the ZIP code. As a major reason, they point out the co-location of producers in supply chains to exploit informational spill-overs, to minimize transportation costs, and to facilitate just-in-time production.[29] The local concentration of trade might also be related to external economies of scale in the presence of intermediate goods and associated agglomeration effects (see Rossi-Hansberg, 2005), as well as to hub-and-spoke distribution systems and wholesale shipments (see Hillberry and Hummels, 2003). Such spatial clustering of economic activity can lead to large domestic border barrier estimates, as we find in our results.[30] In that case, the domestic border effect should be interpreted as reflecting the local concentration of economic activity rather than border barriers associated with crossing a state border.

The concentration of trade at the local level is also borne out in other types of data. Using individual transactions data from online auction websites, Hortaçsu, Martínez-Jerez, and Douglas (2009) find that purchases tend to be disproportionately concentrated within a short distance perimeter, with many counterparties based in the same city. Some of these

[29] Historically, competition on US state-to-state transportation routes was heavily restricted by the Interstate Commerce Commission well into the post-World War II era, giving companies an additional incentive to co-locate (see Levinson, 2006).

[30] The concentration of trade at the local level might also be related to firms' slicing up their production chains (multistage production and vertical specialization). Yi (2010) offers an explanation of the border effect using the vertical specialization argument in a Ricardian framework.

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purchases can be explained by their location-specific nature, for example, in the case of opera tickets. But the evidence also suggests that lack of trust and lack of direct contract enforcement in the case of breach may be major reasons behind the same-city bias, which the authors subsume under 'contracting costs.' They also find evidence for culture and local tastes as factors that shape the local concentration of trade. For example, the same-city effect is most pronounced for local interest items such as sports memorabilia (also see Blum and Goldfarb, 2006).

Business networks and immigration patterns might also be related to strong trade flows between relatively close locations. Combes, Lafourcade, and Mayer (2005) report that business and immigrant networks significantly facilitate trade within France. They cite the reduction of information costs and the diffusion of preferences as two main economic mechanisms through which networks may operate. This includes the reduction of search costs associated with matching buyers and sellers (Rauch and Casella, 2003). As an additional facilitating factor for trade, Rauch and Trindade (2002) also mention the possibility of community sanctions that could be imposed among members of an ethnic network. In the context of the border effect in US data, Millimet and Osang (2007) find that incorporating migration flows within the US diminishes the estimated intranational home bias. Business and immigrant networks therefore likely play an important role in explaining the trade-reducing effect of distance.[31]

6 Conclusion

We collect a data set of US exports that combines three types of trade flows: trade within an individual state (Minnesota–Minnesota), trade between US states (Minnesota–Texas), and trade flows from an individual US state to a foreign country (Minnesota–Canada). This data set allows us to jointly estimate the effect on trade of crossing the domestic state border and the effect of crossing the international border.

While we obtain point estimates consistent with those generally found in the literature, we show that the international border effect is in fact *smaller* than the state border effect. That is, while trading internationally is still the most costly in absolute terms, overcoming the first few miles that are associated with leaving the home state appears harder than crossing the international border once the home state has been left. This result is robust

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[31] The impact of ethnic networks on exports from US states has been explored recently by Bandyopadhyay, Coughlin, and Wall (2008). One of their findings is that the inclusion of a common network effect reduces the negative impact of distance on exports.

to alternative distance measures, alternative functional forms for distance, additional trade cost factors and different subsamples. Our article thus sheds new light on the relative size of border effects as typically estimated in gravity applications. In particular, our finding of a relatively strong domestic border effect can be interpreted as reflecting the concentration of economic activity and trade flows at the local level.

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