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The U.S.-Canada Border Effect: Smaller Than Previously Thought and Becoming Smaller

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16. Abstract

We revisit the effect of the U.S.-Canada national border on trade, considering to what extent the border reduces trade when observable economic factors are controlled. A reexamination of the data yields estimates of the border effect that are 50% higher than previously expected (Feenstra, 2004); however, the nonlinearity of the estimation and distance measure reduce the effect by 65%. We therefore conclude that the border effect in 1993 had a factor of 4.10. This figure is 15% lower than the effect proposed by previous research. We also calculate the border effect for subsequent years and find that this effect steadily decreased to a low of 3.21 in 2007. Interestingly, a traditional linear methodology cannot be used to identify this decline clearly.

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Abstract

We revisit the effect of the U.S.-Canada national border on trade, considering to what extent the border reduces trade when observable economic factors are controlled. A reexamination of the data yields estimates of the border effect that are 50% higher than previously expected (Feenstra, 2004); however, the nonlinearity of the estimation and distance measure reduce the effect by 65%. We therefore conclude that the border effect in 1993 had a factor of 4.10. This figure is 15% lower than the effect proposed by previous research. We also calculate the border effect for subsequent years and find that this effect steadily decreased to a low of 3.21 in 2007. Interestingly, a traditional linear methodology cannot be used to identify this decline clearly.

Chapter 1 Introduction

Since McCallum's seminal study (1995) of regional trade patterns across the United States-Canadian border, the issue of border effects has attracted widespread attention. McCallum used a basic gravity model to determine that in 1988, inter-provincial trade was greater than province-state trade by a factor of 22. Anderson and van Wincoop (2003) and Feenstra (2002) later corrected the biases affecting McCallum's estimate and found a border effect for 1993 with a factor of 4.6-4.7 (see summary discussion of Feenstra, 2004, Chapter 5). We provide new insights on the U.S.-Canada national border effect by 1) examining the data construction and estimation methods to revise the estimate for 1993 and 2) extending the analysis to subsequent years.

We begin by examining the sensitivity of border effects to data construction and estimation design using 1993 data. Our re-estimation includes improvements in 1) sample selection, 2) trade data construction, 3) estimation methodology, 4) model specifications, and 5) bilateral distance measures. Our findings contrast with the majority of the literature based on the dataset generated by Anderson and van Wincoop (2003). We confirm the possibility of a downward bias in those results, due to issues with Anderson and van Wincoop's (2003) trade data that were initially raised by Balistreri and Hillberry (2007). We find that the sample selection procedure used in the estimates by Anderson and van Wincoop (2003) and Feenstra (2002) creates downward bias. Appropriate sample selection and data construction procedures increase the border effect by approximately 50% to a factor of 7.11—a higher figure than was previously expected (Feenstra, 2004).

However, the estimation methodology, model specification, and bilateral distance measures help to decrease the border effect. Instead of using ordinary least square (OLS) together with log-linear transformation—as in the majority of previous studies—we use the nonlinear estimators recommended by Santos Silva and Tenreyro (2006). Santos Silva and Tenreyro (2006) found that the OLS estimators for gravity equations with log-linear transformation yielded biased estimates if there were zero trade observations or if the errors were heteroskedastic, whereas the nonlinear estimators were econometrically consistent under weaker assumptions. The use of a nonlinear estimation methodology decreases the border effect to a factor of 5.24. We subsequently include nonlinear terms for distance and modify the measurement methods for distance using driving mileage and time, rather than the traditional great-circle distance measure. These three modifications all help to decrease the estimated border effect.

When the five factors are used, we find a border effect with a factor of 4.10. We conclude that the border effect is much lower than was originally estimated by McCallum (1995); in fact, it is 15% lower than the reduced factor that is currently widely accepted (Feenstra, 2004).

We then examine the evolution of the border effect by estimating the effects in subsequent years for which data are available—1997, 2002, and 2007. We find that the border effect continuously decreased during the observation period. In 2007, the estimated border effect has a factor of 3.21, which means that the border effect decreased by 22% over 14 years. The decrease is empirically meaningful because it indicates that increased security concerns do not seem to be significant enough to offset the decreasing trend.

Interestingly, the conventional OLS estimator used after the log-linear transformation does not indicate that the use of nonlinear terms, the distance measurement method, or the decline in the border effect affects the results. We suggest that this finding was observed primarily because log-linear models put too much weight on trade within large states or provinces. In other words, a decline in trade costs will stimulate trade among small states and provinces once observable factors are controlled for.

In Chapter 2, we review the relevant literature. Chapter 3 explains our five methodological improvements. Chapter 4 presents the resulting estimation of the border effects. In Chapter 5, we provide concluding remarks.

Chapter 2 Literature Review

The effect of the U.S.-Canada border on trade has attracted widespread attention in both trade literature (McCallum, 1995; Helliwell, 1998; Helliwell and Verdier, 2001; Feenstra, 2002; Anderson and van Wincoop, 2003; Balistreri and Hillberry, 2007; Hillberry and Hummels, 2008; Baier and Bergstrand, 2009; Yi, 2010) and research on foreign trade policies (Brown and Anderson, 2002; Taylor et al., 2004). McCallum (1995), controlling for the distance and size of the economies in question using a basic gravity equation for trade, found that inter-provincial trade was greater than province-state trade in 1988 by a factor of 22. Helliwell (1998) conducted a follow-up study and confirmed that the border effect was persistent in subsequent years.

Anderson and van Wincoop (2003) proposed methodological improvements to the gravity equation used by McCallum (1995). They showed that the traditional gravity equation, which simply includes the GDPs of exporters and importers, omits what they called multilateral resistance terms. These terms capture the decrease in trade caused by the availability of competitive alternative choices. Theoretically, these terms can be accounted for using the price indexes for the exporter and importer. Applying this approach to the Armington trade model (Armington, 1969; Anderson, 1979), the researchers identified a border effect with a factor of 4.6. Baier and Bergstrand (2009) analyzed why the estimates with and without multilateral resistance terms are drastically different and concluded that the multilateral resistance terms influence the border effect estimates because they are log-linearly approximated by GDP and trade cost variables. However, Balistreri and Hillberry (2007) criticized the multilateral resistance terms and proposed an alternative means of calculating the terms based on computational general equilibrium modeling. Feenstra (2002, 2004) recommended using a more robust approach: including a full set of importer and exporter dummies (see also Hummels, 1999). Feenstra (2002) showed that using the dummy variable approach made it possible to correctly estimate the average border effects on trade from the U.S. to Canada and from Canada to the U.S.

Regardless of the methods used to account for these multilateral resistance terms, all of the abovementioned studies employed log-linear specifications for the gravity equation.[1] However, in a different context, Santos Silva and Tenreyro (2006) criticized this approach for two reasons. First, observations with zero trade flow cannot be included in the analysis because it is not possible to take the log of zero. The percentage of zero trade observations is approximately 5-10% in the U.S.-Canada trade data. Although this figure is much lower than the corresponding figure for the world trade data (40-60%; Santos Silva and Tenreyro, 2011), simply omitting these zero trade observations obviously generates bias. Second, log-linear transformation makes an error of the regression correlated with covariates, except for a case with special structure of the error. In fact, Monte Carlo experiments by Santos Silva and Tenreyro (2006, 2011) showed that all of the following estimators are severely biased: a log-linear specification that drops zero observations, a log-linear specification that adds unity to the observations, Tobit, and a sample selection model. These researchers later examined the techniques that do not require log-transformation: normal quasi-maximum likelihood estimation (a.k.a. non-linear least squares or NLS), Poisson quasi-maximum likelihood estimation (PQML), and gamma quasi-maximum likelihood estimation (GQML). These estimates are consistent under the same set of assumptions, but although their efficiency depends on the variance of the error, PQML performs best in that setting.

[1] Anderson and van Wincoop (2003) used nonlinear least squares because they imposed a nonlinear constraint based on the multilateral resistance terms, but their gravity equation was log-transformed.

In addition to influencing model design and estimation methodology, the data construction process can affect estimates of border effects. Most recent studies of the U.S.-Canada border effect have used the dataset constructed by Anderson and van Wincoop (2003). We believe there are at least three areas in which this dataset could be improved. First, Anderson and van Wincoop (2003) restricted their attention to 30 U.S. states and 10 Canadian provinces. More strikingly, these authors chose the 30 states whose shares of the trade with Canadian provinces are the highest. This selection process generates downward bias in the estimation.

In addition, Anderson and van Wincoop (2003) merged Canadian data with intra-U.S. trade data (Commodity Flow Survey, CFS) in a process that has been criticized by Balistreri and Hillberry (2007). Anderson and van Wincoop (2003) scaled down the trade values in CFS using a single factor to adjust for the differences in the industry coverage and the construction of the original data. Balistreri and Hillberry (2007) suggested that the use of a single factor is not appropriate for adjustment because wholesale shipments are more sensitive to geographic trade friction (Hillberry and Hummels, 2003). Balistreri and Hillberry (2007) concluded that Anderson and van Wincoop may have overcorrected for the presence of wholesale trade in inter-state trade flows.[2] They then showed that the border effect was underestimated by Anderson and van Wincoop (2003). Overall, Balistreri and Hillberry (2007) concluded that the border effect factor fell approximately between six and seven.

Finally, the dataset was constructed only for 1993. The question of the border effect in 1993 is an important one, but what occurred in later years is also critical given the potential

[2] Another issue with the data construction procedure is a treatment of intermediate goods trade. Yi (2010) examined the possibility that the vertical linkage of the production process might amplify the national border effect. This researcher used a structural approach to calculate the border effect and obtained approximately same result (an 80% reduction in trade).

effects of political efforts to reduce the cost of border crossing (Woudsma, 1999; Brown and Anderson, 2002; Bradbury, 2010). In fact, one of the most critical questions is the evolution of the border effect over time. Other than Helliwell (1998), who compares the figures for 1988-1996, researchers have not investigated the evolution of the border effect. Although free trade agreements have removed tariffs, traders must still pay additional costs if they wish to trade across the border. Customs administration after the free trade agreement (FTA) has been intended to achieve two conflicting goals: to reduce the remaining trade costs and to ensure security checks. After the September 11 attacks in 2001, security concerns intensified. Nevertheless, the governments of the U.S. and Canada have continuously attempted to improve the border crossing infrastructure to encourage no-risk traffic. For example, they introduced the Free and Secure Trade (FAST) program in 2005 to reduce customs processing time for certified drivers carrying goods from certified shippers and certified importers. Trade between the U.S. and Canada increased at an average annual rate of 8.9% in constant dollars between 1994 and 2000 (Bureau of Transportation Statistics, 2001) and the total trade value in 2002 was still higher than that of 1997, despite the decreases in 2001 and 2002 (Bureau of Transportation Statistics, 2006).

The measurement of trade distance also affects the outcomes. In constructing a distance table, we must define the origins and destinations and choose both a measurement method and an estimation methodology for intra-state trade distance. The definition of intra-state trade distance remains controversial in the literature (Wolf, 2000; Helliwell and Verdier, 2001; Hillberry and Hummels, 2003). However, we sidestep this problem by omitting home-state (home-province) trade from the sample, as in other studies of the U.S.-Canada national border effect. Province and state capitols are usually employed as the origins and destinations, and the distance between them is typically measured using the great-circle distance method. Exceptions are Hillberry and Hummels (2003), Hillberry and Hummels (2008), and Ishise and Matsuo (2012). These papers employed the actual shipping distance or the average distance based on the CFS instead. Unfortunately, the corresponding international data are not available.

Another concern related to trade distance is the nonlinearity of the distance effects in the trade model. Traditionally, the natural log of distance is included as a regressor in gravity regressions; however, recent papers include additional terms to capture the nonlinearity of the effects. For example, Eaton and Kortum (2002) employed dummy variables dividing certain distances, Hillberry and Hummels (2008) employed a nonparametric function, and Wolf (2009) included additional terms for distances above quartiles. In the context of inter-state trade in the U.S., Ishise and Matsuo (2012) found that the terms capturing kinks at quartiles are statistically significant.

Chapter 3 Data and Estimation Methodology

3.1 Interregional Trade Data

We constructed two datasets for inter-regional trade for 1993, 1997, 2002, and 2007. The first inter-regional trade dataset (Dataset A) was generated using a method explained in Helliwell (1998) and Anderson and van Wincoop (2003). We combined three datasets: province merchandise trade data from the Input-Output Division of Statics Canada (IO), province-state merchandise trade data from the International Trade Division of Statistics Canada (IT), and inter-state commodity flows from the CFS by the U.S. Census Bureau.

Of these three, the IO data are considered to be the most appropriate for measuring trade flow (McCallum, 1995). The IO data measure trade between the true origin and the final destination by netting out intermediate shipments. However, the data are available only for inter-provincial trade and by province, not for particular province-state combinations. In contrast, the IT data reflect province-state trade by industry but may not indicate the true origins and final destinations of the shipments. Based on the strengths and weaknesses of the two data sources, Helliwell (1998) and Anderson and van Wincoop (2003) employed the IO data for inter-provincial trade and made adjustments for state-province trade. For each of the 28 industrial categories, we calculated the shares of the international trade by province that were associated with the individual states using the IT data. These figures were applied to the total international trade by industry based on the IO data.

The CFS data were used for inter-state trade flows, but they also required adjustments to make them consistent with the IO data. The CFS data were based on surveys of shipments that originated from representative establishments. These data are different from the IO data and the IT data in two ways. First, the survey covers only manufacturing, mining (excluding oil and gas), wholesale, and select retail establishments. Second, the survey counts every intermediate shipment separately and does not record origin and final destination. Taking these differences into account, Anderson and van Wincoop (2003) scaled down the CFS data by 48%, the ratio of the total domestic merchandise trade to the total domestic shipments from the CFS dataset.

Dataset A was generated using the same adjustment that Anderson and van Wincoop (2003) used. For 1993, we obtained relatively similar trade data.[3] As seen from table A.1 in the appendix, the correlation of the trade flow figures for the two datasets is 0.99.

Our second inter-regional trade dataset (Dataset B) took into account inconsistencies in industry coverage. The inter-state trade data were taken from the CFS. We adjusted for industry coverage in the inter-provincial and province-state trade data to make them more consistent with the CFS data. As previously described, the CFS data were generated from a survey and cover only specific type of industries. Thus, we employed the industries included in the CFS dataset when we generated the inter-provincial and state-province trade data; agricultural products, oil and gas, and retail sales were excluded. Following Balistreri and Hillberry (2007), we did not adjust the CFS data to merge them with the other data. Supplementary to their points regarding wholesale shipment, we have additional evidence that the non-adjusted CFS data provide more reasonable estimates of trade flow than the adjusted data. The CFS data from 1997, 2002, and 2007 contain international shipment statistics, and they are available for total shipment from the aggregate U.S. states to the rest of the world.[4] When we compared the value of the international shipments according to the CFS and the balance of payments data for the industry included in the

[3] Minor differences may originate from revisions to the Canadian inter-provincial input-output table, merchandise trade data, and rounding errors.

[4] The CFS data include shipments to the major trade partners, whereas the U.S. balance of payments data for industry are available only for the countries overall.

CFS (table A.2), we found that these two numbers were reasonably close for all of the years considered.

For both datasets, we used the 48 contiguous U.S. states and 10 Canadian provinces. This is another way in which our data differ from those of Anderson and van Wincoop (2003). Anderson and van Wincoop (2003) employed 10 Canadian provinces but only 30 U.S. states as samples.[5] Given that the Canadian territories have an economic structure that is considerably different from that of other areas, we followed Anderson and van Wincoop (2003) in excluding the Canadian territories. Similarly, we also followed these researchers in excluding Alaska, Hawaii, the District of Columbia, and the U.S. territories because their economic and transportation structures are considerably different. However, we felt that selecting the top 30 U.S. states with regard to U.S.-Canada trade may generate downward-biased estimates.

3.2 Measurement of Bilateral Distance

We constructed several measures of distance. First, we employed the conventional great-circle distance method, using the distances between the capitols of states and provinces ("GC Cap"). This distance measure is employed in many papers in the field (e.g., Disdier and Head, 2008). Next, we altered the means of measuring minimum driving mileage and time. The great-circle distance may be significantly different for combinations in which network connectivity is low (i.e., those that require longer network distance) or combinations for which network quality is low (i.e., those that require longer driving time). Given that a large proportion of freight traffic employs trucks or multimodal transportation that includes trucks, we felt that the minimum driving distance or time was the most reasonable measure. The use of the minimum driving

[5] Anderson and van Wincoop (2003) included an aggregation of the remaining 20 states plus the District of Columbia in constructing their multilateral resistance terms, but trade flow data were not used in the estimation.

distance is widely accepted, as in the CFS data and the papers that employed network distance. In such cases, road network distance as measured on a map is the sole measure. However, we felt that driving time may be a better reflection of bilateral transportation costs. Transportation costs generally include travel time and financial costs rather than travel distance and costs. As reported in table A.3, travel distance ("Mile Cap") is highly correlated with travel time ("Time Cap"); however, it may be significantly different in certain cases. Lastly, we used primary cities instead of capitols as the origins and destinations ("Mile Pri" and "Time Pri"). Because the capital is not always the economic center of a state, we felt that using primary cities provided an advantage: it is a better way to determine the actual average distance than the use of capitals. We must admit, however, that shipment activities can be spread out within an area; thus, even a primary city may not perfectly represent the origin or destination.

Bilateral trade distance and time were calculated using ArcGIS, a Geographic Information Systems (GIS) software. We used a Network Analyst extension of ArcGIS and street network data provided by ESRI, the provider of the ArcGIS software. The street network data indicated road length, road class, and speed limits for all major roads in the U.S. and Canada and identified one-way roads. One shortcoming of the data was the lack of information regarding congestion. Thus, the minimum travel time that we have estimated may not be the exact actual travel time for the route.

We began by estimating the minimum travel times for the routes between capitals and primary cities based on the road network data by ESRI. Our outputs were the minimum travel time and route distance calculated using the GIS software for inter-state and inter-province trade.

For cross-border trade, 76 major land-border crossing points with custom codes were used.[6] We estimated the minimum travel time between capitals or primary cities and these custom points and then determined the origin, destination, and custom point combinations with the minimum travel time.

A weakness of this method of estimating transportation costs is that it excludes rail, water, and air transportation, the distances of which may be significantly different from the road network distances identified here. The estimated trade values for ocean state-province combinations may be more biased because the effective trade distance is shorter or due to slower vessel operation speeds. However, we feel that this bias is limited because the proportion of the trade value associated with this type of transport is low. As we indicated earlier, in 2007, trade by air and ship made up less than 10% of the total trade, and trade by rail comprised less than 20%.[7]

3.3 Estimation Model and Methodology

As discussed by Feenstra (2004), various trade models suggest that

$$y_{e,i} = \text{factor}_e \times \text{factor}_i \times \text{trade cost}_{e,i} \quad , \qquad (3.1)$$

where $y_{e,i}$ indicates the value of the trade flow from exporter e to importer i.[8]

[6] We feel that minor roads and custom points without custom codes are not appropriate for handling freight traffic. These 76 major customs points process 85-94 % of total overland northern border trade in 1997, 2002, and 2007. Thus, ignoring minor crossings does not affect our estimation.

[7] According to the North American Transborder Freight Data obtained from the Bureau of Transportation Statistics (http://www.bts.gov/programs/international/transborder/)

[8] As shown by Hillberry and Hummels (2008), an important aspect of the CFS data is the distinction that this dataset makes between the extensive margin (who trades) and the intensive margin (how much each of them trades). We use the total values in comparison to the previous estimates. Channey (2008) showed that a gravity equation, such as (1), holds even if the model explicitly distinguishes between extensive and intensive margins.

By taking the natural logs of both sides, designating the GDP as the “factor" and determining the trade cost based on the great circle distance and the border indicator, we can obtain the estimation equation used by McCallum (1995). However, Anderson and van Wincoop (2003) objected that simply using the GDP causes one to ignore the multilateral resistance terms. Anderson and van Wincoop (2003), Balistreri and Hillberry (2007), and Baier and Bergstrand (2009) used the Armington model to generate what they called “multilateral resistance" terms. A question that arises from their approach is how to correctly construct the terms (see Balistreri and Hillberry, 2007).

We used a more robust approach by including a full set of dummy variables instead. To control the multilateral resistance terms, Feenstra (2002) used

$$\ln y_{e,i} = \gamma_e + \varphi_i + \alpha \text{ border}_{e,i} + \beta \ln \text{gcdist}_{e,i} + \varepsilon_{e,i} \tag{3.2}$$

where γ_e and φ_i are exporter and importer dummies, “border” is an indicator that takes a value of one if e and i are from different countries, gcdist is the great-circle distance between e and i, β and δ are the coefficients to be estimated, and $\varepsilon_{e,i}$ captures the unmeasured portion of the trade costs or the measurement errors for trade.

We included several additional terms to reflect the nonlinearity of the log distance in the gravity equation. Traditionally, physical distance was captured by a single variable, the log of the bilateral great-circle distance (Feenstra, 2004). This measure is convenient because its regression coefficient indicates the distance elasticity of the trade flow. However, an increasing number of studies have suggested the possibility of nonlinearity in distance with respect to trade flow (Disdier and Head, 2008). We followed Wolf (2009) and included three-stepwise distance indicators for every quartile break (ln dist(q)$_{e,i}$) along with a dummy variable for adjacency of areas (“adj$_{e,i}$").

Traditionally, the gravity equation was estimated after the log-transformation (see Feenstra, 2004). However, Santos Silva and Tenreyro (2006) criticized this practice as a source of bias. Instead, they recommended estimating the level-exponential specification

$$y_{e,i} = \exp\left(\gamma_e + \varphi_i + \alpha \text{border}_{e,i} + \delta \text{adj}_{e,i} + \sum_q \beta_q \ln \text{dist}\ (\text{q})_{e,i}\right) + u_{e,i} \qquad (3.3)$$

using quasi-maximum likelihood estimation.

For notational simplicity, let j be a subscript for e,i combinations, and let $\mathbf{x}_j$ be the set of explanatory variables (including dummies). As emphasized by Wooldridge (2010), if the conditional expectation of the error is zero,

$$\text{E}[u_j \mid \mathbf{x}_j] = 0 \qquad (3.4)$$

then NLS, PQML, and GQML are all consistent as long as the other standard technical conditions of the estimations are satisfied (See also Gourieroux et al., 1984).

The difference of the estimators appears in the efficiency of the estimation. Suppose that the conditional variance is in the following form:

$$\text{Var}[y_j \mid \mathbf{x}_j] = \sigma^2 (\text{E}[y_j \mid \mathbf{x}_j])^{\zeta} \qquad (3.5)$$

If $\zeta = 0$ (i.e., if the conditional variance is constant), then NLS is efficient in the class of all quasi-maximum likelihood estimators (Gourieroux et al., 1984; Wooldridge, 2010). Similarly, if

$\zeta = 1$, PQML is efficient; if $\zeta = 2$, GQML is efficient. As discussed by Santos Silva and Tenreyro (2006), the difference in the efficiency of the estimators is analogous to the weighted least squares (WLS) in the linear estimations. WLS achieves efficiency in the estimation by putting greater weight on more reliable observations, as expressed by variance. In this nonlinear estimation model, the NLS treats all the observations equally, PQML puts proportionally more weight on observations with large trade, and GQML puts even more weight on observations with large trade. In a context of world trade, applying more weight to large trade may be justifiable due to higher accuracy of data from large industrialized countries. In the context of the U.S.-Canada trade, however, there is no a priori reason for how to put weights. Instead, a selection of estimation method is better to be based on the nature of the empirical data.

We conducted a test proposed by Wooldridge (1991), which is essentially considering a model of variance:

$$\mathrm{E}[u_j^2 \mid \mathbf{x}_j] = \delta_0 + \delta_1 \mathrm{E}[y_j \mid \mathbf{X}] + \delta_2 (\mathrm{E}[y_j \mid \mathbf{X}])^2 \tag{3.6}$$

and examining the coefficients of δ_0, δ_1 and δ_2 in the corresponding estimation equation 3.6.[9]

If $\zeta = 0$ and we employ the NLS for the estimation, then equation (6) implies $\delta_1 = \delta_2 = 0$. An F-test of the joint hypothesis (testing zeros of these two terms) examines the efficiency assumption of the NLS. If we reject the null hypothesis of $\delta_1 = \delta_2 = 0$, then the NLS is not likely to be efficient. Similarly, after estimating the model by the PQML, we conducted an F-test of $\delta_0 = \delta_2 = 0$; after estimating the model by the GQML, we conducted an F-test of $\delta_0 = \delta_1 = 0$. Roughly

[9] In the actual computation, we employ modified versions for the robustness purpose. First, we employ a robust version of the test explained in Wooldridge (1991) and Wooldridge (2010). Second, we include powers of -1 and 3, adding to 0, 1 and 2, in the regression equations. Nevertheless, these modifications make no difference in our results.

speaking, the null hypothesis of the test is the specification that the conditional variance is correct. A large F-statistics (i.e., a small p-value) indicates that the specification of variance is not appropriate. In other words, a small F-statistics (a large p-value) supports the estimator.

Chapter 4 Results

Here we present our regression results. First, we show the re-examination results for the 1993 data, and second, we present the results for the following years.

4.1 The Effect of Data Construction Process

First, we examined the sensitivity of border effects to the data construction process and model selection using 1993 data. In column (1) of table 4.1, we began by replicating the result obtained by Feenstra (2002) using the dataset constructed by Anderson and van Wincoop (2003).[10] As is well-known, the border coefficient is approximately -1.55, suggesting that the intra-country trade is 4.70 (= exp(1.55)) times larger than the cross-border trade.[11]

The remaining columns display results based on our dataset to examine validity of our data and effects of sample selection. The distance used in table 4.1 is a great-circle distance between capitols, which is the same as Feenstra (2002). Column (2) uses observations based on the same rule with Dataset A. Specifically, the number of states is limited to 30 states, and observations with a trade flow smaller than 1 million U.S. dollars are eliminated.[12] We obtained almost identical results with column (1): Intra-country trade is larger than cross-border trade by a factor of 4.66. Although they are not exactly the same, we consider our data to reasonably replicate that of Feenstra (2002). Column (3) is the result using the same data without the 1 million dollar threshold. Including minor non-zero trade increases the border factor to 5.28. When we relieve the state restriction of 30 states and include all 48 contiguous states, the border

[10] We obtained the data from James Anderson's webpage.
[11] When we calculate the border effect factor, we take an exponential of the coefficient without rounding. Thus the exponential of coefficients in the table may not match our estimated border effects. The standard error of the border effect is calculated based on the delta method.
[12] This threshold was not actually included in the final dataset of Anderson and van Wincoop (2003). We nevertheless realize that our Dataset A include far more minor trade flows than theirs (see table A1). We suspect that rounding errors during the merging process of IO data and IT data generate this inconsistency. For eliminating these minor trade flows, we employ this threshold in column (2).

effect becomes even stronger, increasing to 5.79 (column (4)). In summary, including minor trade flows into observation increases the estimated border effect by 23 % to 5.79.

Table 4.1 Data Examination

	(1)	(2)	(3)	(4)	(5)
Year	1993	1993	1993	1993	1993
Data	AvW	Data A	Data A	Data A	Data B
Area	$30+10$	$30+10$	$30+10$	$48+10$	$48+10$
Threshold	–	$>$ 1MUSD	>0	>0	>0
Distance	GC Cap	GC Cap	GC Cap	GC Cap	GC Cap
Specification	Log-lin	Log-lin	Log-lin	Log-lin	Log-lin
Method	OLS	OLS	OLS	OLS	OLS
Border	-1.55^{**}	-1.54^{**}	-1.66^{**}	-1.76^{**}	-1.96^{**}
	(.066)	(.059)	(.074)	(.070)	(.077)
ln dist	-1.25^{**}	-1.21^{**}	-1.28^{**}	-1.30^{**}	-1.28^{**}
	(.025)	(.036)	(.041)	(.026)	(.026)
Obs.	1,511	1,455	1,515	3,111	3,104
Implied effect	4.70	4.66	5.28	5.79	7.11
	(.310)	(.274)	(.390)	(.407)	(.546)

Notes: Heteroskedasticity robust standard errors are in parentheses. ** indicates 1% significance, and * indicates 5% significance in the Wald-tests that the coefficients are zero. All the estimations control a full set of exporter and importer dummies.

The effect of data construction process speculated by Balistreri and Hillberry (2007) is examined in column (5). We examined all 48 contiguous states plus 10 provinces from Dataset B (data with industrial adjustment) using the same estimation model as columns (1) to (4). From a comparison between columns (4) and (5), we find an additional increase of the border effect factor to 7.11. As described earlier, this dataset takes into account consistency in industrial coverage among three data sources. Specifically, agricultural products, oil, and gas are excluded from Dataset A because they are not included in the CFS.

This adjustment may affect the estimation of the border effect twofold. One is disproportional change in the pairwise trade volume, depending on the mix of trade products. The other is relative decrease in inter-province and state-province trade compared to inter-state trade. Although the effect of the first aspect is ambiguous, the second aspect is likely to increase the estimated border effects. When Balistreri and Hillberry (2007) criticized Anderson and van Wincoop's (2003) data, they argued that the adjusted inter-state trade data Anderson and van Wincoop (2003) used was smaller than what they should have used, and therefore Anderson and van Wincoop (2003) underestimated the border effect. Our focus is not on the adjustment of wholesale trade; however, our industrial mix adjustment does the same effect on the border effect estimation. We consider that the inter-provincial and state-province trade data Anderson and van Wincoop (2003) used is too large to be comparable to inter-state trade data they use. As Balistreri and Hillberry (2007) suggested, relative increase in inter-state trade suggests a stronger border effect.

From the comparison of the results from Datasets A and B, the data construction process of Anderson and van Wincoop (2003) is likely to lead to underestimation of the border effect. Including minor trade data increases the border factor to 5.79 and including both minor trade and adding industrial adjustment boosts the factor to 7.11, which is a 51% increase compared to the baseline (column (5) compared to column (1)).

4.2 Sensitivity to Estimation Model and Estimation Methodology

We now turn our models to the level-exponential specification based on the methodologies recommended by Santos Silva and Tenreyro (2006). As emphasized by Santos Silva and Tenreyro (2006), the log-linear specification is highly likely to be biased. Instead, we compare level-exponential specification with three different estimators: the NLS, PQML, and GQML.

Table 4.2 Comparing Estimation Methodologies

	(1)	(2)	(3)	(4)	(5)	(6)
Year	1993	1993	1993	1993	1993	1993
Data	Data B	Data B	Data B	Data B	Data B	Data B
Area	$48 + 10$	$48 + 10$	$48 + 10$	$48 + 10$	$48 + 10$	$48 + 10$
Threshold	–	–	–	–	–	–
Distance	GC Cap	GC Cap	GC Cap	Time Pri	Time Pri	Time Pri
Specification	Lev-exp	Lev-exp	Lev-exp	Lev-exp	Lev-exp	Lev-exp
Method	NLS	PQML	GQML	GQML	PQML	NLS
Border	-1.62^{**}	-1.66^{**}	-1.70^{**}	-1.26^{**}	-1.41^{**}	-1.80^{**}
	(.152)	(.097)	(.073)	(.136)	(.099)	(.084)
Adj.				$.833^{**}$	$.806^{**}$	$.933^{**}$
				(.063)	(.051)	(.077)
ln dist	-1.06^{**}	-1.03^{**}	-1.43^{**}	$-.255^{**}$	$-.345^{**}$	$-.660^{**}$
	(.044)	(.025)	(.033)	(.046)	(.052)	(.064)
ln dist1				$-.133^{**}$	$-.119^{**}$	$-.131^{**}$
				(.021)	(.017)	(.022)
ln dist2				$-.067^{**}$	$-.072^{**}$	$-.101^{**}$
				(.019)	(.013)	(.014)
ln dist3				.013	.020	$-.028$
				(.019)	(.016)	(.016)
Spec. $F^{\dagger}$				235.3	711.8	1354.2
(p-value)				(0.00)	(0.00)	(0.00)
Var. $F^{\ddagger}$	5.75	.020	17.9	29.7	.000	11.35
(p-value)	(.219)	(1.00)	(.001)	(0.00)	(1.00)	(.023)
Implied effect	5.07	5.24	5.48	3.52	4.10	6.05
	(.770)	(.511)	(.399)	(.480)	(.407)	(.509)

Notes: Numbers of observations are 3,306. Heteroskedasticity robust standard errors are in parentheses. ** indicates 1% significance, and * indicates 5% significance in the Wald-tests that the coefficients are zero. All the estimations control a full set of exporter and importer dummies.
† F-statistics testing that the additional terms (adding to the border dummy and log of distance) are jointly zero.
‡ F-statistics of the robust version of the conditional variance test (Wooldridge, 1991). The null is, roughly speaking, the specification of the conditional variance is correct.

Generally, the level-exponential specification reduces the estimated border effect. Columns (1) to (3) of table 4.2 present the results of a full-sample from Dataset B using level-exponential specifications with the NLS, PQML, and GQML. In contrast to the OLS, these level-exponential models examine 3,306 full-samples (58 times 57), including combinations with zero trade flow. Compared to the border effect factor of 7.11 in the OLS counterpart (column (5) in table 4.1), the border effect decreases to 5.07, 5.24, and 5.48.

When using the level-exponential specification, selection of the estimation methodology is critical. The null hypothesis of the conditional variance test that the variance is correctly specified is not rejected for NLS and PQML, while it is rejected for GQML. In other words, the conditional variance tests are supportive for PQML and NLS estimations but not for GQML.

Columns (4) to (6) compare the three estimations by taking nonlinearities in distance measures into account. We concentrate here on the conditional variance tests, and the interpretations of the coefficients and other statistics are detailed in the next subsection. As shown by the conditional variance tests, NLS and GQML are rejected at 95%-level. Conditional variance tests generally suggest PQML is the most appropriate estimation methodology, and this result is true for the following specifications. Hence, we only show the results obtained by PQML along with the results of the conditional variance test in the rest of the presentations.

4.3 Sensitivity to the Various Aspects of Distance

Mismeasurement of distance and nonlinearity in distance factors lead to an overestimate of the border effect factor. Table 4.3 compares a series of results using the same data (Dataset B), samples (48 states and 10 provinces) and estimation model (level-exponential specification with PQML estimator).

Table 4.3 Nonlinearity in Distance and Distance Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year	1993	1993	1993	1993	1993	1993	1993
Data	Data B	Data B	Data B	Data B	Data B	Data B	Data B
Area	48 + 10	48 + 10	48 + 10	48 + 10	48 + 10	48 + 10	48 + 10
Threshold	–	–	–	–	–	–	–
Distance	GC Cap	GC Cap	GC Cap	GC Cap	Mile Cap	Mile Pri	Time Pri
Specification	Lev-exp	Lev-exp	Lev-exp	Lev-exp	Lev-exp	Lev-exp	Lev-exp
Method	PQML	PQML	PQML	PQML	PQML	PQML	PQML
Border	-1.66^{**}	-1.53^{**}	-1.51^{**}	-1.50^{**}	-1.48^{**}	-1.42^{**}	-1.41^{**}
	(.097)	(.098)	(.099)	(.099)	(.093)	(.099)	(.099)
Adj.		$.617^{**}$	$.632^{**}$	$.641^{**}$	$.597^{**}$	$.807^{**}$	$.806^{**}$
		(.050)	(.053)	(.053)	(.047)	(.051)	(.051)
ln dist	-1.03^{**}	$-.744^{**}$	$-.707^{**}$	$-.655^{**}$	$-.716^{**}$	$-.332^{**}$	$-.345^{**}$
	(.025)	(.028)	(.036)	(.048)	(.044)	(.050)	(.052)
ln dist1				$-.0198^{**}$	$-.0176^{**}$	$-.0482^{**}$	$-.119^{**}$
				(.006)	(.006)	(.007)	(.017)
ln dist2			$-.0104$	$-.0167^{**}$	$-.0096$	$-.0376^{**}$	$-.072^{**}$
			(.006)	(.005)	(.006)	(.006)	(.013)
ln dist3				$.0188^{**}$	$-.0198^{**}$	$-.0003$	.020
				(.007)	(.007)	(.008)	(.016)
Spec. F†	–	–	732.8	872.6	980.6	711.5	711.8
(p-value)			(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Var. F‡	.020	.007	.000	.000	.000	.000	.000
(p-value)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
Implied effect	5.24	4.60	4.55	4.48	4.39	4.13	4.10
	(.511)	(.450)	(.451)	(.441)	(.410)	(.411)	(.407)

Notes: Numbers of observations are 3,306. Heteroskedasticity robust standard errors are in parentheses. ** indicates 1% significance, and * indicates 5% significance in the Wald-tests that the coefficients are zero. All the estimations control a full set of exporter and importer dummies.
† F-statistics testing that the additional terms (adding to the border dummy and log of distance) are jointly zero.
‡ F-statistics of the robust version of the conditional variance test (Wooldridge, 1991). The null is, roughly speaking, the specification of the conditional variance is correct.

A portion of the border effect is explained by a larger trade between adjacent states and provinces. We begin by examining the adjacency factor using a great-circle distance between capitols as a distance measure. Column (1) displays the baseline result without any adjacency or stepwise distance variables, which is the same as column (2) of table 4.2. The estimated border effect factor is 5.24, as we discussed earlier. When an adjacency dummy variable is included (column (2)), the adjacency dummy variable is positive and significant, which suggests that neighboring states and provinces trade more. Including an adjacency dummy variable decreases the border effect factor to 4.60. A part of the border effect factor may simply come from the fact that not many province-state combinations are adjacent each other.

The non-linear effect of distance is another source of overestimation. Following Wolf (2009), we introduced three stepwise breaks for log of distance indicated as ln dist1, 2, and 3. When we include a break in the median (column (3)), the term is negative but not statistically significant. A joint hypothesis that the adjacency dummy and ln dist2 are both zero is strongly rejected, primarily due to the adjacency dummy. If we include all of the three quartile breaks (column (4)) they are all negative and significant until the third quarter, which means trade intensity decays more rapidly than log of distance. However, the third quartile indicates a positive sign, and basically offsets the effect of the second quartile. We suspect this is an indication of low-performance of the great-circle distance as a measurement of bilateral trade costs. With additional distance quartile breaks, the adjacency dummy variable becomes larger in magnitude, and the border effect factor becomes smaller, decreasing to 4.48. Therefore, another part of the border effect factor seems to originate from non-linear effects of distance.

Last, we find that the inaccurate measurement methods of distance may lead to an overestimation of the border effect factor. So far, we have followed the traditional great-circle distance between capitols. However, capitols are not always the economic center, and more importantly the great-circle distance is not the true transportation cost between them. Although great-circle distance between capitols is a good approximation of transportation cost when two regions are distant, the effects of mismeasurement become serious when two regions are close, as in our case. Columns (5) to (7) examine the model using network distance and driving time of the minimum travel time route. As we explained in the data section, they are not generalized transportation costs because of two reasons. One is that shipment activities may be located sparsely within an area, such that even a primary city is not a perfect measure of the origin, and the other is that the estimated route or time does not reflect congestion in the real world. The network distance is, however, more appropriate than great-circle distance because it reflects transportation network connectivity. Travel time is even more appropriate than network distance because it takes into account the quality of road networks.

The border effect factor decreases to 4.39 when we consider the network connectivity (i.e., measure by network distance), as presented in column (5). If we switch the origins and destinations to primary cities, the effect decreases to 4.13 (column (6)), and it decreases to 4.10 if we take into account the quality of the network connection by travel time (column (6)). In summary, the border effect is much smaller than what was originally estimated by McCallum (1995) and even 15% smaller than the previous estimation (Feenstra, 2004) after all.[13]

4.4 Evolution of the Border Effect over Time

The border effect seems to be smaller than previously estimated for 1993, and our analysis also shows that the border effect has consistently decreased over time. The implied border effect factor is 4.10 in 1993, but it becomes 3.54 in 1997, 3.30 in 2002, and 3.21 in 2007 (columns (1) to (4) in table 4.4). Specifically, the border effect factor has decreased by 21% over the 14-year period. We cannot find negative effects from the heightened security concern from 2001, which may have decreased freight traffic in 2002. This effect may be observed because

[13] Nonlinearity in distance has the effects in estimating the border effect by the log-linear model. Detailed results are displayed in Appendix Table A4.

trade friction reduction between 1997 and 2000 is large enough to offset the negative shock by the September 11 attacks.

Table 4.4 Changes over Time

	(1)	(2)	(3)	(4)
Year	1993	1997	2002	2007
Data	Data B	Data B	Data B	Data B
Area	48 + 10	48 + 10	48 + 10	48 + 10
Threshold	–	–	–	–
Distance	Time Pri	Time Pri	Time Pri	Time Pri
Specification	Lev-exp	Lev-exp	Lev-exp	Lev-exp
Method	PQML	PQML	PQML	PQML
Border	-1.41^{**}	-1.26^{**}	-1.20^{**}	-1.17^{**}
	(.099)	(.087)	(.083)	(.075)
Adj.	$.806^{**}$	$.753^{**}$	$.717^{**}$	$.751^{**}$
	(.051)	(.049)	(.052)	(.047)
ln dist	$-.345^{**}$	$-.378^{**}$	$-.415^{**}$	$-.423^{**}$
	(.052)	(.044)	(.053)	(.045)
ln dist1	$-.119^{**}$	$-.086^{**}$	$-.097^{**}$	$-.090^{**}$
	(.017)	(.014)	(.018)	(.017)
ln dist2	$-.072^{**}$	$-.065^{**}$	$-.075^{**}$	$-.051^{**}$
	(.013)	(.012)	(.016)	(.014)
ln dist3	.020	.019	.025	.024
	(.016)	(.014)	(.018)	(.017)
Spec. F†	711.8	660.3	627.4	643.8
(p-value)	(0.00)	(0.00)	(0.00)	(0.00)
Var. F‡	.000	.000	.000	.000
(p-value)	(1.00)	(1.00)	(1.00)	(1.00)
Implied effect	4.10	3.54	3.30	3.21
	(.407)	(.307)	(.273)	(.243)

Notes: Numbers of observations are 3,306. Heteroskedasticity robust standard errors are in parentheses. ** indicates 1% significance, and * indicates 5% significance in the Wald-tests that the coefficients are zero. All the estimations control a full set of exporter and importer dummies.
† F-statistics testing that the additional terms (adding to the border dummy and log of distance) are jointly zero.
‡ F-statistics of the robust version of the conditional variance test (Wooldridge, 1991). The null is, roughly speaking, the specification of the conditional variance is correct.

One of the potential factors that may reduce the border crossing cost is the introduction of the FAST program. The policy may contribute to the cost reduction in the period between 2002 and 2007; however, the fraction of the FAST freight traffic is minor at customs locations other than Michigan-Ontario crossings. The fraction of the FAST trucks in land-border crossings in the U.S.-Canada trade is 44% in the Michigan-Ontario crossings, but only 23% in Buffalo, New York and 5% in Blaine, Washington (The Border Policy Research Institute, 2009).

Our model construction is still supported by the data for the following years. The PQML estimation still appears to work because null hypothesis for conditional variance is not rejected. Additionally, adjacency and the first two quartile distance terms are consistently in expected signs and statistically significant. Namely, adjacent states and provinces trade more than others and distance effect becomes stronger when the distance becomes longer.

Table 4.5 Summary of the Estimated Border Effects

	Estimation method	Distance measure	Nonlinear terms	Year 1993	1997	2002	2007
(1)	OLS	GC Cap	no	7.11	7.40	6.92	8.10
				(.546)	(.411)	(.388)	(.552)
(2)	OLS	Time Pri	yes	6.93	7.28	6.82	8.02
				(.594)	(.478)	(.451)	(.603)
(3)	PQML	GC Cap	no	5.24	4.43	4.14	3.98
				(.511)	(.375)	(.324)	(.296)
(4)	PQML	Time Pri	yes	4.10	3.54	3.30	3.21
				(.407)	(.307)	(.273)	(.243)

Notes: Heteroskedasticity robust standard errors are in parentheses. Each cell reports the implied border effect from different estimations.

Interestingly, the traditional OLS specifications cannot detect this decreasing trend. Table 4.5 compares the transition in the estimated border effects using the log-linear model (OLS) and level-exponential model (PQML) with and without distance measurement change. Row (1) presents the transition in the border effect estimated by the log-linear model with the OLS estimator, using a simple great-circle between capitols as a measurement of distance. Row (2) also employs the log-linear model with the OLS estimator but uses our measurement of distance (i.e., distance is measured by travel time between primary cities, and nonlinear terms of distances and the adjacency dummy are included). Row (3) displays the results estimated by the level-exponential model with the PQML estimator, using a simple great-circle distance between capitols as a measurement of distance. The results in row (4) are the same as those presented in table 4.4.[14]

Neither difference in distance measure nor observation year makes a significant difference in the border effect estimates in the log-linear models (Rows (1) and (2)). The update in distance measurement decreases the estimate slightly, but the difference is insignificant. Moreover, the estimated border effect does not decrease over the 14 year period no matter how we measure the distance.

In contrast, the level-exponential models with the PQML estimator detect the decline in the border effect, even without updates in distance measurement. Mechanically, OLS is closer to GQML than the other estimates in putting larger weights on larger observations (Santos Silva and Tenreyro, 2006). Thus, the difference suggests rapid growth in cross-border trades between minor state-province combinations, while the growth in cross-border trades between large state-province combinations is not remarkable when economic and distance factors are controlled. We hypothesize that the result originates from a decline in fixed costs in cross-border trade. Learning administrative processes and finding new trade partners used to be costly for companies located

[14] Detailed results for Rows (1) to (3) are shown in Appendix Tables A5-A7.

in minor states or provinces. However, these fixed costs have been decreasing over the last two decades because of government efforts to simplify administrative processing and enhanced information availability through the internet. The decline in the fixed costs enabled these potential exporters, who previously could not export goods, to begin exporting. As a result, controlling for economic factors, it has become easier to trade between minor state-province combinations.

Chapter 5 Conclusions and Future Directions

The border effect that we estimated for 1993 is 4.10, which is much smaller than what McCallum (1995) estimated and even smaller than what Anderson and van Wincoop (2003) and Feenstra (2002) estimated. The border effect is then continuously decreasing over time, eventually to 3.21 in 2007.

In the previous research, the border effect was underestimated by 23% by ignoring minor trade flows. Joining U.S. data and Canadian data leads to additional underestimation in the previous estimates. Our first conclusion is that the revision of data leads to approximately 50% higher border effects than were previously estimated.

The estimated border effect decreases considerably when we switch the model to the level-exponential model that Santos Silva and Tenreyro (2006) recommended. Due to the existence of zero trade flow combinations, the OLS log-linear model is definitely biased. The quasi-maximum likelihood estimators with level-exponential specification are preferred because they are more accurate estimates under weaker assumptions. The choice of estimator is crucial in applying level-exponential specification, and PQML is strongly supported in our data. The estimated border effect decreases to 5.24, which is approximately 30% smaller than what is estimated by the OLS log-linear model.

Distance measurement also affects the estimation of the border effect. As Wolf (2000) stated, adjacent states and provinces typically trade more, and ignoring the effect will lead to overestimation of the border effect. Additionally, the effect of distance is not linear. When two trading partners are further apart, the distance elasticity becomes higher. This property suggests that the border effect previously estimated may contain the non-linear effects of distance. Last, the measurement method of distance is also important. Capitols, which have been used for previous specifications, are often not the economic center of the states or provinces. Also, the great-circle distance is not the best measurement for transportation costs between origins and destinations. When we consider the road network performance (i.e., measure travel time instead of the great-circle distance), the border effect becomes even lower. Combining all three aspects, the estimated border effect for 1993 becomes as low as 4.10.

The border effect is continuously decreasing over time, and it decreased to 3.21 in 2007. The trend should be welcomed by all the related organizations that have made efforts to reduce trade costs between the U.S. and Canada. However, the border factor of 3.21 means that the border still reduces two-thirds of the trade. It would be interesting to evaluate effectiveness of the trade-cost reduction policies, such as the FAST program.

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Appendix

Appendix Tables

Table A.1 Descriptive Statistics of Trade Values

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year	1993	1993	1993	1993	1993	1997	2002	2007
Data	AvW	Data A	Data A	Data A	Data B	Data B	Data B	Data B
Area	$30+10$	$30+10$	$30+10$	$48+10$	$48+10$	$48+10$	$48+10$	$48+10$
Threshold	–	$>1M$	>0	–	–	–	–	–
Mean	962.7	931.2	917.1	572.2	1075.7	1270.2	1530.5	2089.2
S.D.	1914.5	1855.0	1844.4	1364.8	2535.3	2943.4	3550.8	4779.1
Obs. of 0	49	68	45	195	202	230	274	197
Obs.	1,511	1,492	1,552	3,306	3,306	3,306	3,306	3,306
				Correlations				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1)	–	.999	.968	.968	.972	.941	.926	.924
(2)	.985	–	–	–	.962	.938	.918	.914
(3)	.983	–	–	–	.962	.939	.919	.915
(4)	.988	–	–	–	.968	.938	.922	.920
(5)	.973	.984	.988	.988	–	.959	.947	.951
(6)	.941	.958	.958	.954	.962	–	.952	.951
(7)	.927	.953	.945	.940	.946	.956	–	.953
(8)	.920	.938	.937	.931	.934	.947	.953	–

Correlations are pairwise correlation among the trade data sets. The upper triangle shows correlation in levels. The lower triangle shows the the correlation after taking logs (and hence excluding observations with zero trade values).

Table A.2 U.S. Total Exports in Balance of Payments Data and the CFS

Year	1997	2002	2007
BoP	582.5	588.2	958.2
CFS	577.8	595.5	873.4
BoP/CFS	1.008	.988	1.097

Notes: BoP is the total exports of U.S. to the rest of the world in the Balance of Payments data. Industry included are Coals & related fuels (X110); Nonagri ex fuels (X12); Selected bldg matls, except metals (X13); Capital goods (X2); Automotive (X3); Consumer goods (X4). CFS is the total values of international shipments in CFS data. The units are billions of the current U.S. dollars.

Table A.3 Descriptive Statistics of Distance Variables

	(1) GC Cap	(2) Mile Cap	(3) Time Cap	(4) Mile Pri	(5) Time Pri
Mean (level)	1858.0	1503.1	26.77	1481.4	26.0
S.D. (level)	1071.5	874.7	17.0	902.3	16.8
Mean (log)	7.32	7.11	3.06	7.08	3.02
S.D. (log)	.708	.709	.725	.746	.760
			Correlations		
	(1)	(2)	(3)	(4)	(5)
(1)	–	.985	.929	.935	.913
(2)	.988	–	.959	.952	.944
(3)	.970	.987	–	.920	.987
(4)	.956	.969	.960	–	.928
(5)	.944	.962	.971	.989	–

Correlations are pairwise correlation among the distance variables. The upper triangle shows correlation in levels. The lower triangle shows the the correlation after taking logs.

Table A.4 Nonlinearity in Distance and Distance Measure (Log-linear model)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year	1993	1993	1993	1993	1993	1993	1993
Data	Data B	Data B	Data B	Data B	Data B	Data B	Data B
Area	$48 + 10$	$48 + 10$	$48 + 10$	$48 + 10$	$48 + 10$	$48 + 10$	$48 + 10$
Threshold	> 0	> 0	> 0	> 0	> 0	> 0	> 0
Distance	GC Cap	GC Cap	GC Cap	GC Cap	Mile Cap	Mile Pri	Time Pri
Specification	Log-lin	Log-lin	Log-lin	Log-lin	Log-lin	Log-lin	Log-lin
Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Border	-1.96^{**}	-1.93^{**}	-1.91^{**}	-1.91^{**}	-1.88^{**}	-1.90^{**}	-1.94^{**}
	(.077)	(.078)	(.078)	(.078)	(.079)	(.080)	(.086)
Adj.		$.580^{**}$	$.690^{**}$	$.781^{**}$	$.765^{**}$	$.877^{**}$	$.862^{**}$
		(.066)	(.066)	(.066)	(.064)	(.063)	(.064)
ln dist	-1.28^{**}	-1.12^{**}	$-.935^{**}$	$-.657^{**}$	$-.748^{**}$	$-.546^{**}$	$-.584^{**}$
	(.026)	(.030)	(.038)	(.056)	(.057)	(.056)	(.057)
ln dist1				$-.0395^{**}$	$-.033^{**}$	$-.0463^{**}$	$-.111^{**}$
				(.007)	(.007)	(.008)	(.019)
ln dist2			$-.038^{**}$	$-.0370^{**}$	$-.037^{**}$	$-.0493^{**}$	$-.108^{**}$
			(.006)	(.006)	(.006)	(.006)	(.012)
ln dist3				$-.0368^{**}$	$-.035^{**}$	$-.0468^{**}$	$-.0549^{**}$
				(.007)	(.008)	(.008)	(.015)
Implied effect	7.11	6.89	6.76	6.76	6.55	6.66	6.93
	(.546)	(.536)	(.528)	(.530)	(.521)	(.535)	(.594)

Notes: Numbers of observations are 3,104. Heteroskedasticity robust standard errors are in parentheses. ** indicates 1% significance, and * indicates 5% significance in the Wald-tests that the coefficients are zero. All the estimations control a full set of exporter and importer dummies.

Table A.5 Changes over Time (Log-linear model 1)

	(1)	(2)	(3)	(4)
Year	1993	1993	1993	1993
Data	Data B	Data B	Data B	Data B
Area	48+10	48+10	48+10	48+10
Threshold	> 0	> 0	> 0	> 0
Distance	GC Cap	GC Cap	GC Cap	GC Cap
Specification	Log-lin	Log-lin	Log-lin	Log-lin
Method	OLS	OLS	OLS	OLS
Border	-1.96^{**}	-2.00^{**}	-1.93^{**}	-2.09^{**}
	(.077)	(.056)	(.056)	(.068)
ln dist	-1.28^{**}	-1.21^{**}	-1.26^{**}	-1.26^{**}
	(.026)	(.024)	(.024)	(.024)
Obs.	3,104	3,076	3,032	3,109
Implied effect	7.11	7.40	6.92	8.10
	(.546)	(.411)	(.388)	(.552)

Notes: Heteroskedasticity robust standard errors are in parentheses. ** indicates 1% significance, and * indicates 5% significance in the Wald-tests that the coefficients are zero. All the estimations control a full set of exporter and importer dummies.

Table A.6 Changes over Time (Log-linear model 2)

	(1)	(2)	(3)	(4)
Year	1993	1997	2002	2007
Data	Data B	Data B	Data B	Data B
Area	$48 + 10$	$48 + 10$	$48 + 10$	$48 + 10$
Threshold	> 0	> 0	> 0	> 0
Distance	Time Pri	Time Pri	Time Pri	Time Pri
Specification	Log-lin	Log-lin	Log-lin	Log-lin
Method	OLS	OLS	OLS	OLS
Border	-1.94^{**}	-1.99^{**}	-1.92^{**}	-2.08^{**}
	(.086)	(.066)	(.066)	(.075)
Adj.	$.862^{**}$	$.714^{**}$	$.721^{**}$	$.674^{**}$
	(.064)	(.063)	(.063)	(.067)
ln dist	$-.584^{**}$	$-.624^{**}$	$-.684^{**}$	$-.782^{**}$
	(.057)	(.056)	(.060)	(.061)
ln dist1	$-.111^{**}$	$-.097^{**}$	$-.107^{**}$	$-.065^{**}$
	(.019)	(.018)	(.020)	(.020)
ln dist2	$-.108^{**}$	$-.090^{**}$	$-.096^{**}$	$-.079^{**}$
	(.012)	(.013)	(.013)	(.013)
ln dist3	$-.0549^{**}$	$-.040^{**}$	$-.014$	.023
	(.015)	(.014)	(.015)	(.014)
Obs.	3,104	3,076	3,032	3,109
Implied effect	6.93	7.28	6.82	8.02
	(.594)	(.478)	(.451)	(.603)

Notes: Heteroskedasticity robust standard errors are in parentheses. ** indicates 1% significance, and * indicates 5% significance in the Wald-tests that the coefficients are zero. All the estimations control a full set of exporter and importer dummies.

Table A.7 Over Time (Level-exponential model 1)

	(1)	(2)	(3)	(4)
Year	1993	1997	2002	2007
Data	Data B	Data B	Data B	Data B
Area	48 + 10	48 + 10	48 + 10	48 + 10
Threshold	–	–	–	–
Distance	GC Cap	GC Cap	GC Cap	GC Cap
Specification	Lev-exp	Lev-exp	Lev-exp	Lev-exp
Method	PQML	PQML	PQML	PQML
Border	-1.66^{**}	-1.49^{**}	-1.42^{**}	-1.38^{**}
	(.097)	(.085)	(.078)	(.074)
ln dist	-1.03^{**}	$-.981^{**}$	-1.04^{**}	-1.02^{**}
	(.025)	(.025)	(.025)	(.025)
Var. F†	.002	.001	.003	.001
(p-value)	(.999)	(1.00)	(1.00)	(1.00)
Implied effect	5.24	4.43	4.14	3.98
	(.511)	(.375)	(.324)	(.296)

Notes: Numbers of observations are 3,306. Heteroskedasticity robust standard errors are in parentheses. ** indicates 1% significance, and * indicates 5% significance in the Wald-tests that the coefficients are zero. All the estimations control a full set of exporter and importer dummies.

† F-statistics of the robust version of the conditional variance test (Wooldridge, 1991). The null is, roughly speaking, the specification of the conditional variance is correct.