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A Re-Examination of the Border Effect

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ABSTRACT

This paper reexamines the evidence on the border effect, the finding that the border drives a wedge between domestic and foreign prices. We argue that the border effect can be inflated by the volatility and persistence of the nominal exchange rate and by the cross-country heterogeneity in the distribution of within-country price differentials. We develop a simple framework to separate the border effect from these confounding factors. Using price data from Engel and Rogers (1996) and Parsley and Wei (2001), we show that after controlling for the confounding factors the border effect between the U.S. and Canada and the U.S. and Japan is negligible.

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1. Introduction

One of the salient findings in empirical international economics is that an administrative border between geographical regions reduces the volume of trade and leads to a price discrepancy across these regions. This regularity has been called the border effect and has been documented in numerous studies. In an influential set of papers, Engel and Rogers (1996, 2000, 2001) find that even after controlling for distance, there is a substantial difference in prices across the U.S.-Canadian border. Parsley and Wei (2001) perform a similar exercise on U.S.-Japanese data and find a large border effect. Using data on quantities, McCallum (1995) finds that intranational trade flows are, ceteris paribus, 22 times larger than international trade flows. In a similar vein, *provincial* borders in Canada (Helliwell and Verdier, 2001) and *state* borders in the U.S.A. (Wolf, 2000) account for a significant fraction of the decreased trade flows across provinces and states relative to trade flows within states and provinces. Furthermore, Ceglowski (2003) finds that *provincial* borders in Canada account for a significant fraction of the discrepancy of prices across provinces. This has important welfare implications and Obstfeld and Rogoff (2000) include the border effect in their list of the major puzzles in international economics.

The presence of a border effect in and of itself is not surprising. However, the magnitude of the estimated border effect is surprisingly – many would say unbelievably – large. The border between the U.S. and Canada, after controlling for distance and other characteristics, is equivalent to 75,000 miles (Table 3, p. 1117, Engel and Rogers (1996)). Parsley and Wei (2001) find the border between the U.S. and Japan is equivalent to 43,000 trillion miles (note that the distance to the Moon is a mere 238,900 miles). Likewise, there are few reasons to believe that impediments to trade between states or provinces are large enough to affect trade flows.[1] Finding a significant border effect where it should not be found, and finding coefficients that are orders of magnitude larger than one can plausibly defend, raises doubts about the validity of the empirical methodology used to isolate the border effect.

The key innovation of Engel and Rogers (1996) was to use city-level price information to estimate the quantitative importance of an international border on price differentials. Their insight was that *n*-city prices could be transformed into $n(n-1)/2$ city-pair combinations, so that

[1] For example, there are explicit legal norms (e.g., U.S. constitution) prohibiting limitation of intranational trade.

the border effect could be measured as the difference observed in the sample of cross-border pairs relative to the within-country pairs. One difficulty with this approach, however, is that significant differences in the distribution of within-country prices across countries can confound the border effect. We call this the *country heterogeneity effect.* We show below that in the U.S.-Canada case, correcting for differences in the within-country volatility of price differentials alone eliminates the border effect.

The second important difference between cross-border and within-country pairs is that the exchange rate affects only the cross-border pairs. This has two implications for cross-border pairs relative to within-country pairs. First, because nominal exchange rates are highly volatile, cross-border pairs are automatically more volatile relative to within-country pairs (the *exchange rate effect*). Second, movements in the nominal exchange rate are highly persistent. Holding the variance of the innovations to the nominal exchange rate constant, the more persistent the shocks, the higher the variance of the real exchange rate (the *persistence effect*). Hence, the border effect is confounded with the volatility and persistence of nominal exchange rates. We are not the first to identify this issue. For example, Engel and Rogers (2001) discuss possible ways of correcting for the exchange rate effect when more than two countries are analyzed. Our contribution is to introduce a unified framework to decompose and estimate the border effect, correcting for these possible biases in the border coefficient, even in the two-country case.

We use price data from Engel and Rogers (1996) and Parsley and Wei (2001) to estimate the border effect for U.S./Canadian and U.S./Japanese cities, respectively. In contrast to previous studies, we show that the border effect is economically and statistically negligible after accounting for factors that confound the border effect. In the U.S.-Canada sample, correction for cross-country heterogeneity alone reduces the border effect from over 71,000 km to 47 km. In the case of the U.S. and Japan, the role of exchange rate volatility is more important. After correcting for the exchange rate effect and the persistence effect, we find that the half-life of price differentials between U.S.-U.S. city pairs and between U.S.-Japan city pairs is statistically and economically indistinguishable.

The structure of the paper is as follows. In section 2, we motivate the econometric specification that is conventionally used to estimate the border effect. In Section 3, we discuss potential problems in using the standard econometric specification to estimate the border effect. Specifically, we argue that *country heterogeneity, persistence* and *nominal exchange rate* *effects* are likely to upwardly bias estimates of the border effect. Then we present a simple method to control for these biases. In Section 4, we use U.S./Canada and U.S./Japan price data sets analyzed in previous studies to re-estimate the border effect after correcting for the biases we identify. We conclude in Section 5.

2. Trade Costs and Dispersion of Prices

In this section we derive the estimating equation used in much of the literature on border effects in goods prices. We begin with the law of one price (LOOP) implied by arbitrage in goods markets. Specifically, the no-arbitrage condition implies that identical goods should sell for the same price in different locations, after adjusting for trade costs and differences in currency denomination.[2] To fix ideas, define P_{it}^k as the price of good *k* in location *i* at time *t* and S_t as the exchange rate that converts prices from location *j*'s currency to location *i*'s currency. T_{ij}^k is the (iceberg) cost of trade between locations *i* and *j* for per unit of good *k*, and $Q_{ijt}^k = P_{it}^k / S_t P_{jt}^k$ is the real exchange rate. Note that the specification of the trade cost here is very general and includes the cost of shipping (which could be proportional to distance), tariffs, and any other administrative barrier that might impede the flow of goods. Then, the law of one price states that

$$\frac{1}{T_{ij}^k} \leq Q_{ijt}^k \leq T_{ij}^k , \tag{1}$$

that is, the price differential between two locations cannot exceed the cost of trade between two locations.[3] Equivalently, condition (1) can be stated as $-t_{ij}^k \leq q_{ijt}^k \leq t_{ij}^k$, where (here and henceforth) small letters denote logs of the respective variables. Observe that q_{ijt}^k are constrained to the interval $[-t_{ij}^k, t_{ij}^k]$, which is called the band of inaction, and $\sigma(q_{ijt}^k)$, the time series standard deviation of q_{ijt}^k, is a function of the trade cost t_{ij}^k.

[2] Anderson and van Wincoop (2004) and Coleman (2005) discuss possible pitfalls of this approach to measuring trade costs.

[3] We assume that the trade costs are symmetric going from *i* to *j* and *j* to *i*. This need not be the case, and the empirical tests below will not impose symmetry.

To pin down the connection between the real exchange rate and trade costs, assume that q_{ijt}^k is uniformly distributed in $[-t_{ij}^k, t_{ij}^k]$[4] and the cost of trade has the specific form $T_{ij}^k = \exp\left(const + \beta_1 \ln d_{ij} + \beta_2 Border_{ij} + \phi_k + \alpha_i + \alpha_j + \varepsilon_{ij}^k\right)$, where d_{ij} is the distance between locations *i* and *j*, $Border_{ij}$ is a dummy variable equal to one if locations are separated by a border and zero otherwise, α_i is the cost of trade specific to location *i*, ϕ_k is the cost of trade specific to good *k*, ε_{ij} is a random time-invariant component in the cost of trade between locations *i* and *j*. Here distance proxies for shipping costs, costs of acquiring information, etc. Then, the standard deviation of the real exchange rate is proportional to the log of the trade costs:

$$\sigma(q_{ijt}^k) \propto t_{ij}^k = const + \beta_1 \ln d_{ij} + \beta_2 Border_{ij} + \phi_k + \alpha_i + \alpha_j + \varepsilon_{ij}^k . \qquad (2)$$

The distance equivalent of the border—that is, the border effect—is equal to $\exp(\beta_2 / \beta_1)$.[5] Note that this is a cross-sectional regression because $\sigma(q_{ijt}^k)$ collapses time series observations of q_{ijt}^k into a single number. Importantly, $\sigma(q_{ijt}^k)$ is scale invariant (that is, P_{it}^k, P_{jt}^k, S_t can be in arbitrary measurement units and $E(q_{ijt}^k)$ does not have to be zero) and thus $\sigma(q_{ijt}^k)$ can be used for price indices which are *normalized* averages of prices for different goods.[6]

Engel and Rogers (1996, 2000, 2001) and the subsequent literature use the standard deviation of *changes* in the real exchange rate as the dependant variable in (2). Taking the difference of the real exchange rate helps reduce the persistence of the real exchange. We

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[4] We follow the literature in making this assumption, however, it is not at all obvious that the real exchange rate would generally exhibit such a distribution. See Coleman (2005) for a discussion of the behavior of the real exchange rate with storage and capacity constraints on transport.

[5] Parsley and Wei (2001) suggest an alternative measure of the border effect: $\bar{d} \times \exp(\beta_2 / \beta_1 - 1)$ where $\bar{d}$ is the average distance between cities. Since this measure is a monotonic transformation of $\exp(\beta_2 / \beta_1)$, our qualitative conclusions do not change if we use this alternative measure.

[6] Under stronger assumptions, one can infer trade costs directly from price differentials. Specifically, if $P_{it}^k = T_{ij}^k S_t P_{jt}^k$ holds, then the deviation of real exchange rate from unity reveals the size of the trade cost, that is, $q_{ij,t}^k = t_{ij}^k$. Thus, to estimate the border effect on price differentials, it is enough to consider a specification like: $\left|q_{ij,t}^k\right| = t_{ij}^k = const + \phi_k + \alpha_i + \alpha_j + \beta_1 \ln d_{ij} + \beta_2 Border_{ij} + \varepsilon_{ijt}^k$. This avenue is pursued by Crucini, Telmer and Zachariadis (2000), Parsley and Wei (2002) and others. Alternatively, Obstfeld and Taylor (1997) use TAR models to estimate directly inaction bounds t_{ij}^k from time series of $q_{ij,t}^k$. They regress the estimates of t_{ij}^k on variables like distance, nominal exchange rate, etc. These two approaches can be used only for prices of identical goods across locations and cannot be applied to price indices because the real exchange rate is not scale-invariant.

follow the literature and use specification (2) to estimate the border effect. In the tests below we use the standard deviation of both level and changes in the real exchange rate.

3. Potential Problems in Isolating the Border Effect

3.1 Country heterogeneity effect

The first problem that arises in estimating (2) stems from possible differences in within-country price dispersion that become confounded with the border effect. For concreteness, suppose that the two countries are Canada and the U.S. and cities are the relevant geographical units. We denote pairs of cities by *UU* (*US-US*), *UC* (*US-Canada*) and *CC* (*Canada-Canada*). The border effect in (2) measures by how much a *ceteris paribus* transition from an intra-national city pair (*UU* or *CC*) to an international city pair (*UC*) raises the average volatility of the price differential. Intuitively, the border effect should reflect factors that are specific to *UC* pairs, and should not include factors that stem from differences between *UU* and *CC* pairs. To put this in the language of the program evaluation/treatment effects literature, the border coefficient should pick up the effect associated with the treatment of crossing the border. If, in the absence of the border treatment, city-pairs differ for other (non-border related) reasons (e.g. more competition or more variety could yield greater price dispersion across US city pairs relative to Canadian city pairs), one should condition on this heterogeneity to isolate the effect of the treatment. Smith (2004) discusses the consequences of ignoring such heterogeneity in the benchmark group.

In practice, there is heterogeneity across cities as well as countries. As we show below, it turns out to be important how the dispersion for each city pair is decomposed into a city and country contribution. To simplify exposition, we omit for now the error term and controls for distance and commodities. For all pairs, the volatility of the real exchange rate is described by:

$$\sigma_{ij} = \beta UC_{ij} + \gamma_U UU_{ij} + \gamma_C CC_{ij} + \sum_{s=1}^{N} \alpha_s D_s \,, \qquad (3)$$

where *UC*, *UU*, *CC* are dummy variables for *UC*, *UU*, and *CC* city pairs, D_s is a city dummy equal to one if $s=i$ or $s=j$ and zero otherwise and N is the number of cities. Without loss of generality, suppose that 1,…,k cities are in Canada. The average volatility for types of city pairs is then given by:

US-Canada: $$\bar{\sigma}_{UC} = \beta + \bar{\alpha}_U + \bar{\alpha}_C \,, \qquad (4)$$

US-US: $\bar{\sigma}_{UU} = \gamma_U + 2\bar{\alpha}_U$, (5)

Canada-Canada: $\bar{\sigma}_{CC} = \gamma_C + 2\bar{\alpha}_C$, (6)

where $\bar{\alpha}_C = \frac{1}{k}\sum_{s=1}^{k}\alpha_s$ is the average city effect for Canadian cities and $\bar{\alpha}_U = \frac{1}{N-k}\sum_{s=k+1}^{N}\alpha_s$ is the average city effect for US cities. The coefficients $\beta, \gamma_U, \gamma_C$ measure the contribution to volatility of being a US-Canada pair, a US-US pair and a Canada-Canada respectively. For example, if $\gamma_U < 0$—i.e., the country component of volatility net of the average U.S. city effect is negative—the volatility of the real exchange rate for US-US pairs is less than the sum of the volatilities of US cities. Coefficients $\beta, \gamma_U, \gamma_C$ can take negative and positive values.

Unfortunately, while (3) provides an exact decomposition of variance in theory, we cannot estimate (3) in practice because the *UU* and *CC* dummies are collinear with the set of city dummies and the *UC* dummy.[7] Specifically,

$$CC_{ij} = -\tfrac{1}{2}UC_{ij} + \tfrac{1}{2}\sum_{s=1}^{k} D_s, \quad (7)$$

$$UU_{ij} = -\tfrac{1}{2}UC_{ij} + \tfrac{1}{2}\sum_{s=k+1}^{N} D_s. \quad (8)$$

One possible identification strategy is to follow Engel and Rogers (1996). ER effectively substitute (7) and (8) into (3) to eliminate CC_{ij} and UU_{ij} so that (see Appendix for derivations):

$$\begin{aligned}\sigma_{ij} &= \beta UC_{ij} + \gamma_U UU_{ij} + \gamma_C CC_{ij} + \sum_{s=1}^{N}\alpha_s D_s \\ &= [\beta - \tfrac{1}{2}(\gamma_U + \gamma_C)]UC_{ij} + \sum_{s=k+1}^{N}\left(\tfrac{1}{2}\gamma_U + \alpha_s\right)D_s + \sum_{s=1}^{k}\left(\tfrac{1}{2}\gamma_C + \alpha_s\right)D_s. \quad (9)\end{aligned}$$

The country heterogeneity is now absorbed in the city-fixed effects. The coefficients on the border dummy in the ER specification measures the increase in the volatility relative to the *average* volatility for intracountry pairs. The implicit assumption is that $\gamma_U = \gamma_C$, i.e., country-specific contributions to volatility are identical. The problem, as we show below, is that in the U.S-Canada case, in particular, country effects are quite different. When this happens, using the average intranational volatility as the benchmark can be confusing because $[\beta - \tfrac{1}{2}(\gamma_U + \gamma_C)]$ can be greater than zero (i.e. one finds a border effect) even when $\gamma_C < \beta < \gamma_U$. In other words,

[7] This means that $\{\beta, \gamma_U, \gamma_C\}$ and $\{\bar{\alpha}_U, \bar{\alpha}_C\}$ are not identified separately. For example, an alternative set of parameter values is $\{\tilde{\beta}, \tilde{\gamma}_U, \tilde{\gamma}_C, \tilde{\bar{\alpha}}_C, \tilde{\bar{\alpha}}_U\}$ where $\tilde{\beta} = \beta + \bar{\alpha}_U + \bar{\alpha}_C$, $\tilde{\gamma}_U = \gamma_U + 2\bar{\alpha}_U$, $\tilde{\gamma}_C = \gamma_C + 2\bar{\alpha}_C$, $\tilde{\bar{\alpha}}_C = 0$ and $\tilde{\bar{\alpha}}_U = 0$.

one finds a border effect even when from the U.S. perspective crossing the border effectively *reduces* the variance of the real exchange rate. In the absence of a structural model, we do not know if the arithmetic average, or any other combination of within-country variances, is the appropriate benchmark for evaluating the effect of the border.

We propose an alternative decomposition into country and city effects. One can rearrange terms in (3) as follows (see Appendix for derivations):

$$\begin{aligned}\sigma_{ij} &= \beta UC_{ij} + \gamma_U UU_{ij} + \gamma_C CC_{ij} + \sum_{s=1}^{N} \alpha_s D_s = \\ &= (\beta + \bar{\alpha}_C + \bar{\alpha}_U) UC_{ij} + (\gamma_U + 2\bar{\alpha}_U) UU_{ij} + (\gamma_C + 2\bar{\alpha}_C) CC_{ij} + \sum_{s=1}^{k} \tilde{\alpha}_s D_s + \sum_{s=k+1}^{N} \hat{\alpha}_s D_s = \\ &= b_{UC} UC_{ij} + b_{CC} CC_{ij} + b_{UU} UU_{ij} + \sum_{s=1}^{k} \tilde{\alpha}_s D_s + \sum_{s=k+1}^{N} \hat{\alpha}_s D_s \, , \qquad (10)\end{aligned}$$

where $\hat{\alpha}_s = \alpha_s - \bar{\alpha}_U$ if *s* is a US city (i.e., deviation of the city effect from the national mean), $\tilde{\alpha}_s = \alpha_s - \bar{\alpha}_C$ if *s* is a Canadian city. Because $\sum_{s=1}^{k} \tilde{\alpha}_s = \sum_{s=k+1}^{N} \hat{\alpha}_s = 0$, dummies CC_{ij} and UU_{ij} are not collinear with other right-hand side variables.[8] The implicit assumption for identification of the border effect is that the *average* city effect is the same across countries. Without loss of generality the average city effect is normalized to zero.

The first advantage of our approach is that b_{UC}, b_{UU}, and b_{CC}—the coefficients on *UC*, *UU* and *CC*— now measure the average volatility for US-Canada, US-US, and Canada-Canada city pairs so that the estimated coefficients are directly related to the objects of interest in equations (4)-(6). Therefore, the differences $b_{UC} - b_{CC}$ and $b_{UC} - b_{UU}$ give meaningful estimates of increases in the volatility of the real exchange rate when one goes from an intra-national city pair to an international city pair. The second advantage is that we can separately estimate the border effect for each country provided $\bar{\alpha}_U = \bar{\alpha}_C$: that is, we can estimate $b_{UC} - b_{CC} = \beta - \gamma_C$ and $b_{UC} - b_{UU} = \beta - \gamma_U$. Because rearrangement in (10) and (9) is purely *algebraic*, (10) and (9) have the same explanatory power (e.g., the same R^2) and one can back out coefficients in specification (10) from specification (9) and vice versa.[9]

To get further intuition, abstract from city-specific effects by setting $\alpha_s = 0$ for all *s*. In the ER specification (9), the border effect is equal to $\bar{\sigma}_{UC} - \frac{1}{2}(\bar{\sigma}_{UU} + \bar{\sigma}_{CC})$. The border coefficient captures the variance of cross-border pairs net of the variance of within-country

[8] Under this restriction, the addition of the *CC* dummy does not require that we drop an arbitrary city dummy.
[9] One can also rearrange (3) so that the right-hand side variables are border dummy, country and city dummies. This rearrangement yields a specification similar to (9).

pairs. When intracountry variances are similar (in the extreme case $\bar{\sigma}_{UU} = \bar{\sigma}_{CC}$) the border coefficient tells us exactly what intuitively we understand it should – it reflects the variation of cross-border pairs that exceeds the variance of within-country pairs. However, suppose that there is no difference between the variances of U.S. and cross-border pairs (and therefore no border effect from the U.S. perspective) and that the within-country variance in Canada is smaller than in the U.S. Setting $\bar{\sigma}_{UC} = \bar{\sigma}_{UU} > \bar{\sigma}_{CC}$ shows that the border coefficient in (9) mixes the border effect with differences in the average volatility of price differentials for intranational pairs across countries. Intuitively, the international price differentials reflect both the effect of crossing the border – the object of interest – and the effect of trading with cities located in a country with greater (or lesser) price dispersion.[10]

The implications are easy to grasp. First, treating *UU* and *CC* pairs as a homogenous group when they are not can produce misleading results. Second, to identify correctly the border effect, one has to control for within-country differences by introducing country-specific dummies as in (10).[11] Third, if the border effect measured by the coefficient on the *UC* dummy is between the estimates of the coefficients on the *UU* and *CC* dummies, one cannot claim to have found a significant border effect. Hence, the border dummy *UC* must be statistically (and economically) greater than the coefficients on the *UU* and *CC* dummies. Of course, if *UU* is the omitted category, then the *Border* coefficient must be statistically greater than zero and greater than the coefficient on the *CC* dummy. The upshot is that the researcher needs to augment (2) with the *CC* dummy as follows:

$$\sigma(q_{ij,t}^{k}) = const + \phi_k + \alpha_i + \alpha_j + \beta_1 \ln d_{ij} + \beta_2 Border_{ij} + \beta_3 CC_{ij} + \varepsilon_{ij}^{k}, \qquad (11)$$

where *Border* is the dummy variable for US-Canada pairs, city-specific effects are constrained as in (10), β_2 measures the increase in the dispersion in the transition from a US-US pair to US-Canada pair and $\beta_2 - \beta_3$ measures the increase for the transition from a Canada-Canada pair to a US-Canada pair holding everything else constant. In general, the benchmark (untreated) group is heterogeneous (i.e., $\beta_3 \neq 0$) and, therefore, the researcher should compute

[10] If we had powers to conduct a controlled experiment, we would take two cities in the same country, draw a border between them and measure how the border changes the volatility of the price differential. In reality, one has to rely on specifications like (9) or (10) to compute the counterfactual.

[11] If it were clear why price-differentials systematically differ across cities and across countries, one could condition on the precise factors that explain those differences. In the absence of such a theory, however, a combination of city- and country-dummies must be used to proxy for those effects.

the border effect relative to the US-US and Canada-Canada benchmark. We suggest using a *conservative* estimate of the border effect:

$$BE = \min\left\{\exp\left(\beta_2 / \beta_1\right), \exp\left(\left[\beta_2 - \beta_3\right] / \beta_1\right)\right\}, \quad (12)$$

so that the border effect measures the minimal increase in the transition from an intranational to an international pairs of locations.[12]

We show below that there is a significant difference between within-country volatility measures for the Canada and the U.S., and failing to account for that difference dramatically increases the estimated border coefficient.

3.2 The persistence effect

In the case we considered above, arbitrage is assumed to be instantaneous, that is, the arbitrager can reallocate any quantity of any good within an arbitrarily small time period. Furthermore, the implicit but critical assumption in deriving (2) is that the analyzed real exchange rates have the same persistence and volatility of innovations. In practice, it is far more likely that adjustment costs prevent instantaneous arbitrage and that there is gradual convergence of the real exchange rate back to and perhaps within the inaction band. Because of this gradual adjustment, Taylor and Taylor (2004) observe that it is important to know how much the real exchange rate deviates from its steady state and how quickly it reverts to equilibrium. Put differently, it is interesting to divide the variance of the real exchange rate into innovation variance and propagation. The relevant objects are then the size of the shocks to the real exchange rate and the speed at which those shocks are dissipated.

Measures of volatility such as $\sigma(q_{ij,t}^k)$ cannot reveal this information because they combine the persistence of $q_{ij,t}^k$ and the volatility of the innovations to $q_{ij,t}^k$ into a single number. In fact, $\sigma(q_{ij,t}^k)$ increases in the volatility and persistence of innovations.[13] Thus, real

[12] It is true that in reporting the minimum of the two values in (12) we pick the smallest border coefficient and the alternative estimate of the border may be dramatically larger. However, without a structural model, it is impossible to reconcile two different estimates of the same border. Therefore, we report the more conservative estimate.

[13] To see the point, consider the following example where the real exchange rate follows the process $q_t = \alpha q_{t-1} + \varepsilon_t$ where the innovations are $\varepsilon_t \sim iid(0,\sigma^2)$ and α measures the persistence of the real exchange rate. Provided $\alpha < 1$, the variance of the respective real exchange rates is $\sigma^2(q_t) = \sigma^2 / (1-\alpha^2)$. Hence, an increase in the persistence of the series increases the variance of the real exchange rate. See Appendix for derivations for AR(*l*) processes.

exchange rates with the same band [-*t*,*t*] can have different variances because some of the real exchange rates are more persistent or have more volatile innovations than other real exchange rates. To illustrate the point, consider two real exchange rates which have the same band [-*t*,*t*] and all other characteristics but have different persistence. If a more persistent real exchange rate happens to be associated with international pairs of locations, then a greater variance of the real exchange rate would be attributed to the border when in fact there is no genuine border effect in going from one country to another. Hence, it is important to control for differences in the persistence of real exchange rates.[14]

Persistence in the real exchange rate is interesting in itself as a measure of market integration. Empirical work suggests that those factors that one might expect to result in a breakdown of the law of one price also contribute to higher persistence in real exchange rate differentials. For example, Parsley and Wei (1996), Ceglowski (2003), Imbs et al (2003) and others find that distance increases the persistence of real exchange rates. Crucini and Shintani (2002) find that the persistence of the real exchange rate is systematically different across intra- and international city pairs.

In summary, the border coefficient in equations (2) and (4) reflects the extent to which the variance of international price differentials differs from the variance of intra-national price differentials. The variances are a function of the persistence in the price differentials as well as the size of the shocks to the price differentials (the innovations). Thus, it will be important to disentangle these effects both within and across countries to understand the sources of the border effect. In what follows, we use the half-life (*HL*) of an innovation to the real exchange rate as a measure of persistence and the standard deviations of innovations to the real exchange rate as a measure of the size of the shocks to the real exchange rate.[15]

3.3 The nominal exchange rate effect

A final problem with interpreting the border effect based on differences in the volatility of real exchange rates is that the behavior of international price pairs is confounded with

[14] Engel and Rogers (1996) examine the variance of *differences* in the real exchange rate. If the real exchange rate were to follow a random walk, this would eliminate the persistence effect. However, as we show below, differencing does not fully purge the persistence and exchange rate effects.

[15] The largest root of the companion matrix associated with lag polynomial in (16) is another possible measure of persistence. The main drawback of this alternative measure is that it ignores short run dynamics captured by other roots. This can be particularly important when there is a root close to the largest root. See Kilian and Zha (2002) for a discussion of the half-life as a measure of persistence. We also considered interquantile range of innovations to the real exchange rate but qualitative conclusions do not change.

nominal exchange rate volatility. In effect, intracountry city pairs have a fixed exchange rate while intercountry city pairs face floating exchange rates. Since volatile nominal exchange rate affects only international city pairs, the volatility of the real exchange rate induced by changes in the nominal exchange rate is fully attributed to the border effect. Moreover, the nominal exchange rate is not only volatile, it is also highly persistent (e.g., Meese and Rogoff 1983), which further increases the volatility of the real exchange rate for international city pairs. Hence, there is an overlap between the persistence effect and the nominal exchange rate effect. To identify the border effect, one has to control for the nominal exchange rate when comparing intra- and international city pairs.

Engel and Rogers (2001) use multiple countries to separate the border effect from the volatility of nominal exchange rates.[16] We suggest an alternative procedure that is applicable to two-country cases. Specifically, we condition the log ratio of prices on the nominal exchange rate and consider variation in the log ratio of prices that is unrelated to movements in the nominal exchange rate. Our econometric specification is:

$$p_{ijt}^{k} = \sum\nolimits_{m=1}^{l} \psi_{m,ijk} p_{ij,t-m}^{k} + \sum\nolimits_{m=0}^{l} \varphi_{m,ijk} s_{t-m} + \omega_{ijt}^{k}, \qquad (13)$$

where $s_t = \ln(S_t)$, $p_{ijt}^{k} = \ln(P_{it}^{k} / P_{jt}^{k})$, ω_{ijt}^{k} is an innovation in p_{ijt}^{k} that is uncorrelated with changes in the nominal exchange rate. Note that we include the current value of the nominal exchange rate in the right-hand side of (13), which may introduce simultaneity bias in the estimates of the coefficients. On the other hand, if the current value is not included in the list of regressors, ω_{ijt}^{k} includes variation that stems from innovations in the nominal exchange rate and, hence, using the standard deviation of ω_{ijt}^{k} should bias the estimate of the border effect upwards. In our applications we find that in either case the qualitative results are similar, which is not surprising given stickiness of prices. Of course, for intranational city pairs (13) reduces to $p_{ijt}^{k} = \sum\nolimits_{m=1}^{l} \psi_{m,ijk} p_{ij,t-m}^{k} + \omega_{ijt}^{k}$.

Note that if cities are small relative to the economy, each city takes the exchange rate as given, i.e., there is no feedback from discrepancy in prices between any two cities to the exchange rate. Indeed, it is unlikely that price discrepancy for any good in any city pair

[16] Specifically, Engel and Rogers include $\sigma(s_{ijt})$ as a regressor in (2). Note that this does not fix the persistence and country heterogeneity effects and, hence, these authors still find a significant border effect.

influences bilateral trade in such a way so as to affect the exchange rate between two nations.[17,18]

Importantly, in (13) we control for the persistence and size of innovations in the nominal exchange rate. Specifically, $\sigma(\omega_{ijt}^k)$ measures the size of innovations to price differentials and coefficients $\{\psi_{1,ijk},...,\psi_{l,ijk}\}$ capture the dynamics of the price differentials holding the nominal exchange rate constant. Thus, $\sigma(\omega_{ijt}^k)$ is the counterfactual volatility of innovations to the price differential for international pairs that would have been if observed exchange rate were fixed. In a similar way, we can construct the counterfactual persistence of price differentials for international pairs. To do this, we hit (13) with a unit shock ω_{ijt}^k and use the estimated $\{\psi_{1,ijk},...,\psi_{l,ijk}\}$ to find the impulse response of p_{ijt}^k to the shock. Because ω_{ijt}^k is orthogonal to s_t by construction and there is not feedback from p_{ijt}^k to s_t, the impulse response shows the dynamics of p_{ijt}^k when the exchange rate is held constant. Then we use the impulse response to find the half-life, which is the last period when the impulse response is greater than 0.5 in absolute value. Now we can compare the constructed counterfactuals for international pairs with actual counterparts for intranational pairs because the actual data and counterfactuals operate under the same fixed exchange rate regime.

Analogously to (11), we can now estimate the effect of the border on the persistence of the real exchange rate and the volatility of innovations to the real exchange rate from

$$\widehat{HL}_{ij}^k = \beta_0 + \beta_1 \ln d_{ij} + \beta_2 Border_{ij} + \beta_3 CC_{ij} + \phi_k + \alpha_i + \alpha_j + \xi_{ij}^k , \tag{14}$$

$$\hat{\sigma}(\omega_{ijt}^k) = \beta_0 + \beta_1 \ln d_{ij} + \beta_2 Border_{ij} + \beta_3 CC_{ij} + \phi_k + \alpha_i + \alpha_j + \zeta_{ij}^k , \tag{15}$$

where $\widehat{HL}_{ij}^k$ and $\hat{\sigma}(\omega_{ijt}^k)$ are estimated according to the procedure we describe above.[19] Note that in (15) there is no bias from the persistence effect because ω_{ijt}^k are uncorrelated innovations

[17] Of course, this argument holds only if cities are small relative to the country. For example, this would not hold if we consider Prague of the Czech Republic or Vienna of Austria. Here we assume implicitly that the share of variation in prices at the city level due to macroeconomic shocks is small. In our data, the share is tiny (about 5-10%) and thus macroeconomic shocks are relatively unimportant and our quantitative and qualitative conclusions should not be sensitive to the relaxing this assumption.

[18] We test if the assumption of no dynamic feedback holds in the data and we cannot reject the null that lags of p_{ijt}^k are jointly zero in a regression analogous to (13) with nominal exchange rate as the dependent variable.

to real exchange rate. Thus, this approach solves all the problems we have identified above. It controls for the volatility of the nominal exchange rate for international city pairs in the two-country case. It separates the persistence and volatility of innovations in price differentials.

To illustrate the importance of the nominal exchange rate volatility, we also compute half-lives and variance of innovations from:

$$q_{ijt}^{k} = \sum\nolimits_{m=1}^{l} \gamma_{m,ijk} q_{ij,t-m}^{k} + \eta_{ijt}^{k}, \tag{16}$$

where we do not control for the nominal exchange rate effect. By re-estimating (14) and (15) with $\widehat{HL}_{ij}^{k}$ and $\hat{\sigma}(\eta_{ijt}^{k})$ obtained from (16) and comparing the obtained results with the results in the original (14) and (15), we can assess the effect of the nominal exchange rate on the estimates of the border effect.

Observe that, in contrast to other studies, we do not restrict intercepts and slopes in (13) or (16) to be the same across *i*, *j*, and *k* because imposing the same $\{\psi_1,...,\psi_l\}$ for all *i*, *j*, and *k* can produce misleading results if the underlying adjustment processes are heterogeneous (Pesaran and Smith 1995). For example, Imbs et al (2005) show that this restriction biases upwards estimated half-lives of the real exchange rate.

3.4 Discussion

In summary, we have identified three problems with using price dispersion to estimate the border effect. First, to identify the border effect one has to control for cross-country heterogeneity in the distribution of prices within countries. Second, the volatility of the nominal exchange rate will in and of itself bias the border coefficient upward since it only affects cross-border city pairs. Finally, equation (2) can be justified by instantaneous arbitrage while the adjustment of real exchange rate is likely to be gradual. Thus, in estimating the border effect one has to control for the persistence of the real exchange rate.

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[19] Note that we use *estimates* of half-lives and $\sigma(\omega_{ijt}^{k})$ as right-hand side variables in (14) and (15). This may be especially important for half-lives as the estimates of slopes in (16) are biased in finite samples. If the error in estimates of HL is classical, this does not affect the estimates of parameters in (14) and (15). To verify the importance of the bias for our conclusions, we use bootstrap as in Kilian (1998) and Inoue and Kilian (2002) to correct for the finite sample bias in the estimates of HL even for integrated series. Specifically, we resample residuals from the model (13) combined with $s_{ijt} = s_{ij,t} + \varepsilon_{ij,t}$ (for example, Meese and Rogoff (1983) motivate this process for the nominal exchange rate) and then use this model to construct bootstrap replications of the data. This approach is similar to Kilian (1999). The qualitative results do not change for bias-corrected estimates of the half-lives.

To address these three issues, we suggest that the persistence of real exchange rates and the volatility of innovations to real exchange rates should be considered separately as in specifications (14) and (15) even in the two-country case. In particular, specification (15) measures the effect of the border on the size of innovations to price differentials after controlling for the nominal exchange rate and country heterogeneity effects. Likewise, specification (14) measures the effect of the border on the speed of adjustment in prices after controlling for the country heterogeneity, persistence and nominal exchange rate effects. This approach provides a unified framework to control/correct for the identified biases (country heterogeneity, persistence, nominal exchange rate) and to investigate how the border affects volatility and persistence of price differentials.

4 A Re-estimation of the Border Effect for Canada-U.S. and Japan-U.S. Pairs

In this section we apply our method to estimate the border effect on the volatility and speed of convergence of price differentials. To contrast the results with previous findings, we use the data collected by Engel and Rogers (1996) and Parsley and Wei (2001) who find large border effects for U.S./Canada and U.S./Japan respectively.

4.1 U.S.-Canada

We start with the U.S./Canada case. ER's data covers 14 categories of goods, nine Canadian cities and 14 U.S. cities. The time span for each city pair varies from 1978-1992 at minimum to 1976-1995 at maximum.[20] Because for some cities the price data are released bimonthly, ER focus on differences over two-month intervals and their dependent variable is $\sigma(\Delta_2 q_{ijt}^k)$ where $\Delta_2 x_t = x_t - x_{t-2}$. Panel A of Table 1 presents descriptive statistics of $\sigma(\Delta_2 q_{ijt}^k)$ and $\sigma(q_{ij,t}^k)$ for *UU*, *UC*, and *CC* city pairs.[21] Examination of the table immediately indicates that, on average, *CC* pairs are considerably less volatile than *UU* pairs and treating these two groups as homogenous may be inappropriate. For example, $\sigma(\Delta_2 q_{ijt}^k)$, which is the focus of ER's analysis, equals 0.031 for *UU* pairs and 0.016 for *CC* pairs.

[20] Detailed discussion of the data can be found in Engel and Rogers (1996). The data are available at Charles Engel's website: http://www.ssc.wisc.edu/~cengel/Data/Border/BorderData.htm.

[21] Panel unit root tests (Appendix Table A2) reject the null of unit root for real exchange rates in *UU*, *CC*, *UC* pairs so that $\sigma(q_{ijt}^k)$ is a well-defined measure of volatility.

Because distance is an independent variable in the border effect regressions, we also illustrate the distribution of $\sigma(\Delta_2 q_{ijt}^k)$ for each type of city pair, conditional on distance (Figure 1).[22] We depict the distribution using a box plot, which shows the median (the bar in the middle of the rectangle), 25th and 75th percentiles (the base and top of the rectangle) as well as the range of the data (the length of the line). Note that the standard deviation conditional on distance need not be positive. Several features of the data are immediately apparent. First, the medians of the *UU* pairs and *UC* pairs are similar, while the median of the CC pairs is lower. Second, *UU* pairs have much greater dispersion relative to *CC* pairs, in fact, greater than *UC* pairs. The figure suggests that the border effect arises from differences between *CC* and *UC* pairs.

In contrast to $\sigma(\Delta_2 q_{ijt}^k)$, the difference in $\sigma(q_{ij,t}^k)$ between *UU* and *UC* pairs is large while the difference is less apparent in $\sigma(\Delta_2 q_{ijt}^k)$ (see the second row of Table 1). A possible explanation is that q_{ijt}^k for *UC* pairs is more persistent than q_{ijt}^k for intranational pairs and, because differencing reduces persistence, the volatility of $\Delta_2 q_{ijt}^k$ is inhibited relative to the volatility of q_{ijt}^k.[23] We explore the implications of this below.

Table 2 follows ER's procedure for estimating the border effect (see Table 3 in Engel and Rogers (1996)). In the first column of panel A we estimate equation (2) with ER's dependent variable $\sigma(\Delta_2 q_{ijt}^k)$. The estimation is repeated in the second panel (first column) using $\sigma(q_{ijt}^k)$ as the dependent variable. To preserve space and keep the analysis focused, in what follows we report only results for regressions pooling across goods.[24] We find that that the coefficients on the border dummy and distance are all positive. Specifically, in the first panel the estimates are β_1=1.076 and β_2=12.026 so that the border effect is $\exp(\beta_2 / \beta_1) = 71{,}438$ km, consistent with ER's estimates.[25]

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[22] Since the dispersion is conditional on distance, it may take negative values.

[23] Engel and Rogers (footnote 8, p. 1116, 1996) note that the dispersion of price differentials is different for the U.S. and Canada. They, however, do not elaborate on possible implications of such a difference.

[24] Results are similar when regressions are estimated for separate good categories and alternative measures of price differentials such as the relative exchange rate.

[25] Our point estimates of β_2, β_1 are close to ER's. However, because the border effect is $BE = \exp(\beta_2 / \beta_1)$, even a minor variation in estimates of β_2, β_1 can result in large changes in the estimates of the border effect. In any case, the order of magnitude is the same.

The effect of within-country differences (the country heterogeneity effect) on the estimated border effect can be seen by adding the *CC* dummy to the regression (2) using the identification strategy in equation (10). Our conjecture is that by pooling *UU* and *CC* pairs, the researcher overstates the border effect. To confirm this intuition, we estimate (11) and report the results in the second column. We find that the key coefficient β_2 in (2) is sensitive to the inclusion of the *CC* dummy while β_1 remains essentially unchanged. In panel A, using ER's dependent variable, the coefficient β_2 for the pooled regression remains statistically significant but drops from 12.026 to 4.148 thus making the border effect fall from 71,438 km to 47 km. In panel B, inclusion of the *CC* dummy causes the border coefficient to fall from 503 billion km to 100 million km.

As we argue in Section 3.1, the difference in the estimate of the border effect is driven by the country heterogeneity effect. In particular, under the ER specification differences in the volatility of real exchange rate for *UU* and *CC* pairs are absorbed by the city dummies. This conjecture is confirmed by inspection of coefficients on the city dummies in Table 3. Canadian cities (the shaded rows) are systematically negative while the coefficients on city dummies for U.S. cities are systematically positive. Once we control for the differences across countries (specification (11); Table 3, column 2), the volatility of *UC* and *CC* pairs is measured by the coefficient on the *Border* and *CC* dummy and the coefficients on city dummies do not have any pattern. We should note that there is no economic theory to help us distinguish between the ER specification and the one we propose. We believe that our specification is the most sensible approach given the nature of the country-specific variation in the data, but at a minimum, our results suggest that the border coefficient is highly sensitive to the assumption one makes about the source of variation and the relevant benchmark for measuring the border effect.

The enormous border effect that remains in $\sigma(q_{ijt}^k)$ (i.e. in levels rather than differences) is consistent with the fact that price differentials between international pairs include the nominal exchange rate, which is highly persistent and volatile. Since half-lives are directly related to the variance of the series, persistence of the nominal exchange rate translates into high volatility of price differentials for international city pairs. In contrast, variance of changes in real exchange rate $\sigma(\Delta_2 q_{ijt}^k)$ is inhibited for persistent series and, hence, the border effect measured on the basis of $\sigma(\Delta_2 q_{ijt}^k)$ is smaller than that based on $\sigma(q_{ijt}^k)$.

To assess this conjecture, we estimate (16) with l=6 to generate estimates of half-lives and $\sigma(\eta_{ijt}^{k})$.[26] Note that in (16) we do not control for the volatility of the nominal exchange rate and we want to verify that the half-life is significantly larger for international city pairs than for intranational city pairs. The average estimated half-lives and standard deviations of innovations in the real exchange rate are reported in Appendix Table A1. Consistent with the studies that analyze prices or disaggregated price indices (e.g., Parsley and Wei 1996, Crucini and Shintani 2002, Ceglowski 2003) or allow heterogeneity in the dynamics of real exchange rate across pairs of locations (Imbs et al 2005), the average half-lives are 20, 23 and 34 months for *UU*, *CC* and *UC* pairs respectively.[27] [28] The volatility of innovations is comparable for *UU* and *UC* pairs. Specifically, the mean of $\sigma(\eta_{ijt}^{k})$ equals 0.031 and 0.028 for *UU* and *UC* pairs respectively. Innovations for *CC* pairs are only half as volatile as those for *UU* pairs. Hence, in terms of $\sigma(\eta_{ijt}^{k})$ and half-lives, *UU* and *CC* pairs are different and controlling for this heterogeneity will be important for the estimates of the border effect.

To estimate the border effect for the volatility and persistence of innovations to real exchange rate, we use the estimated $\sigma(\eta_{ijt}^{k})$ (columns 1 and 2, Panel A, Table 4) and half-lives (columns 1 and 2, Panel B, Table 4) as the dependent variables in (15) and (14). Estimates of the coefficients on log distance between cities suggest that, *ceteris paribus*, larger distance is associated with increased persistence and volatility of innovations. As we expected, controlling for intranational heterogeneity (by adding a *CC* dummy; column 2) reduces the size of the border effect. In particular, the border effect falls from 14.5 million km to 120 km and from 3,117 km to 1,005 km for $\sigma(\eta_{ijt}^{k})$ and half-lives respectively.

The conclusions change dramatically once we control for the volatility of the nominal exchange rate as in (13) with l=6. Means of the estimated volatility and half-lives of innovations in the price ratio p_{ijt}^{k} are reported in Appendix Table A1. This can be seen by comparing panels A and B of Figure 2. Panel A shows that the distribution of half-lives of *UC*

[26] We choose l=6 to keep our results comparable to ER's and to control for seasonal variation. We include monthly dummies in (16) to control for seasonal factors.
[27] Other studies (e.g., Parsley and Wei 1996) can have different estimates of half-lives because they use alternative ways to compute the half-lives (e.g., analyze only the largest root of the lag polynomial).
[28] For U.S./Canada case, half-lives greater than 10 years are coded as missing (less than 2% of the sample). The conclusions we reach here and in the rest of the paper do not change when we explicitly control for censoring of half-lives using truncated regressions.

pairs is shifted to the right of intracountry pairs. After controlling for the exchange rate, Panel B indicates that the *UC* pairs are now almost indistinguishable from intracountry pairs. Indeed, the average half-life of innovations for *UC* pairs falls from 34 months, when we do not control for volatility and persistence of the nominal exchange rate, to 23 months, which is comparable to the persistence we observe in intranational pairs. The volatility of innovations, $\sigma(\omega_{ijt}^{k})$, decreases from 0.031 to 0.029 thus remaining slightly above the volatility of innovations for *UU* pairs.

Using these fixed-exchange-rate counterfactuals, we estimate the border effect on the volatility of innovations to the real exchange rate (as in (15)) and the persistence of the real exchange rate (as in (14)). The results are reported in column 3 (no control for country heterogeneity effect) and column 4 (control for country heterogeneity effect) of Table 4. Distance continues to be associated with increased persistence and volatility of innovations in the price ratio as the coefficients on log distance are positive and statistically significant in all cases. The implied border effect greatly diminishes relative to the case when we do not control for the nominal exchange rate in the real exchange rate for international city pairs. For example, the border effect on the half-life decreases from 1,005 km (column 2, panel B) to 1 (column 4, panel B), where in the latter case we control for the volatility of the nominal exchange rate and differences in within-country heterogeneity. Likewise, the border effect on the volatility of innovations further falls from 120 km (column 2, panel A) to zero km (column 4, panel A) with the coefficient on the *Border* dummy changing the sign to negative. Again, note the importance of the country heterogeneity effect by comparing columns 3 and 4 in Table 4.

Engel and Rogers (1996) argue that the large border effect cannot be completely explained by the volatility of the nominal exchange rate. They find that the border effect remains large when price differentials are measured by the relative exchange rate measured as $RER_{ijt}^{k} = \left((P_{it}^{k} / P_{it}) \big/ (P_{jt}^{k} / P_{jt}) \right)$ where P_{it}^{k} is the price of good *k* in city *i* at time *t*, P_{it} is the Consumer Price Index for city *i* at time *t* (descriptive statistics are in Appendix Table A3). Clearly, *RER* does not inflate dispersion for international pairs as the real exchange rate does. We reproduce their large estimate of the border effect for $\sigma(\Delta_2 RER_{ijt}^{k})$ in column 1, Panel A, Table 5. We show, however, that these findings are also affected by the country heterogeneity effect. Indeed, when the *CC* dummy is included (column 2, Panel B, Table 5), the estimate of the border effect shrinks from 845 km to zero.[29]

These results suggest that after controlling for the country heterogeneity effect (by including the *CC* dummy) differences between the cross-border and within-country pairs are largely driven by exchange rate movements. The results are consistent with Mussa (1986) who argues that movements in the real exchange rate are driven by the nominal exchange rate since prices are sticky. The country heterogeneity effect explains most of the border effect when the dependent variable is the variance of the change in the real exchange rate. When the dependent variable is the variance of the real exchange rate, nominal exchange rates are the key source of shocks that cause cross-border pairs to exhibit a "border effect" and the persistence of nominal exchange rates is the source of persistent cross-border price differentials. After removing the exchange rate effect, international price differentials behave similar to intranational price differentials. To the extent a border exists, the border is the nominal exchange rate.

4.2 U.S.-Japan

Now we repeat the exercise for the U.S./Japan sample of Parsley and Wei (2001; henceforth PW) who collect quarterly information on prices of 27 traded goods (e.g., toothpaste, coffee, jeans) in 48 Japanese and 48 U.S. cities over 1976:Q1-1997:Q4. We limit their sample to 10 U.S. and 14 Japanese cities, the largest in terms of population.[30] Panel B of Table 1 reports mean values of $\sigma(\Delta q_{ijt}^{k})$ and $\sigma(q_{ijt}^{k})$ by types of city pairs and Figure 3 presents a box plot for $\sigma(\Delta q_{ijt}^{k})$.[31] As in the U.S./Canada case, U.S.-U.S. (*UU*) pairs are different from Japan-Japan (*JJ*) pairs. For instance, $\sigma(\Delta q_{ijt}^{k})$ is 0.153 and 0.080 for *UU* and *JJ* pairs, respectively. Thus, treating intranational city pairs as homogenous may lead to spuriously large estimates of the

[29] Likewise, controlling for country heterogeneity eliminates the border effect in the volatility of innovations in and persistence of the relative exchange rate.

[30] U.S. cities are Denver CO, Indianapolis IN, New Orleans LA, St. Louis MO, Omaha NE, New York NY, Columbus OH, EL Paso TX, Ft. Worth TX, Houston TX. Japanese cities are Sapporo, Sendai, Chiba, Yokohama, Niigata, Nagoya, Kyoto, Osaka, Kobe, Okayama, Hiroshima, Fukuoka, Kumamoto, Kagoshima. The thresholds are 300,000 and 500,000 in population for U.S. and Japanese cities respectively. More details can be found in Parsley and Wei (2001). The data are available at David Parsley's website: http://mba.vanderbilt.edu/david.parsley/Research.htm. The price data analyzed in PW and ER are from different sources and thus not comparable. PW use price data collected the American Chamber of Commerce, while ER use price data collected by Bureau of Labor Statistics.

[31] As in the U.S./Canada case, panel unit root tests reject the presence of a stochastic trend in the real exchange rate (Table A2) so that the variance of real exchange rate is well-defined.

border effects especially for $\sigma(q_{ijt}^k)$. Also note a tremendous increase in $\sigma(q_{ijt}^k)$ relative to $\sigma(\Delta q_{ijt}^k)$ for U.S.-Japan (*UJ*) pairs, which is consistent with real exchange rate being persistent.

Following the previous analysis of the U.S./Canada case, we estimate equation (2) using $\sigma(\Delta q_{ijt}^k)$ and $\sigma(q_{ijt}^k)$ as the dependent variables and show the results in Table 6, Panel A and B. The border effect implied by the estimates is of a truly cosmic size, which is consistent with PW's estimates (Table 1, p. 96): the border is analogous to 43.2 million km for $\sigma(\Delta q_{ijt}^k)$ and more than 1 trillion km for $\sigma(q_{ijt}^k)$.[32] In contrast to the U.S./Canada case, controlling for the country heterogeneity effect (column (2)) reduces the estimate of the border effect but does not eliminate it. For instance, the border effect falls to 29,080 km for $\sigma(\Delta q_{ijt}^k)$. Despite this reduction, it is hardly conceivable that the border can have such a profound effect on the persistence and volatility of price differentials. The volatility of the nominal exchange rate is a more likely culprit in the U.S./Japan case because the volatility of USD/Yen exchange rate is much larger than the volatility of exchange rate for the U.S. and Canada. In fact, differences in the border effect for $\sigma(\Delta q_{ijt}^k)$ and $\sigma(q_{ijt}^k)$ already indicate that the border is likely to be confounded with the volatility and persistence of the nominal exchange rate.

To verify this hypothesis, we examine the volatility and persistence of innovations in the real exchange rate. Specifically, we estimate (16) with l=4 and report the means of the estimated half-lives and $\sigma(\eta_{ijt}^k)$ in Appendix Table A1.[33] Half-lives for international city pairs are considerably larger than half-lives for intranational pairs. The differences are particularly striking for *UU* (2.2 quarters) and *UJ* (14.5 quarters) pairs with *JJ* (5.5 quarters) pairs in between of these two extremes (see Panel A, Figure 4).[34] Volatility of innovations is larger for international pairs (0.195) than for intranational pairs (0.120 for *UU* and 0.061 for *JJ*).

[32] PW report a different estimate because they use polynomial in distance while we use log of distance. Also, we analyze only a sub-sample of PW's cities.

[33] We choose l=4 to control for seasonal variation (the data are quarterly). For the same reason we also include seasonal dummies.

[34] The average half-life for intranational pairs deserves a comment. Observing 4.2 half-life for *JJ* pairs is generally consistent with finding half-lives of or under one year (see discussion above). On the other hand, the half-life of 1.5 quarters for *UU* pairs appears somewhat low. Parsley and Wei (1996) use the similar data and find that the half-life for tradable goods is in 4-6 quarter range. Parsley and Wei, however, use approximate half-lives based on the largest root of the lag polynomial. We were able to reproduce their results when we use the approximate half-life. In contrast, the half-life based on impulse response function takes into account all eigenroots and, consequently, can lead to different estimates of the half-lives if the short run dynamics is important. This is exactly

After regressing half-lives and $\sigma(\eta_{ijt}^{k})$ on log distance, border dummy and other controls, we find border effects equivalent to galactic distances. Even after controlling for the country heterogeneity effect, the border effect is greater than 1 trillion km for half-lives (column 2, Panel B, Table 7) and the volatility of innovations $\sigma(\eta_{ijt}^{k})$ (column 2, Panel A, Table 7). Hence, $\sigma(q_{ijt}^{k})$ is significantly more volatile because the half-life is significantly longer and innovations are significantly large for international pairs than for intranational pairs. Again it is hardly plausible that border induces such impediments to elimination of price differentials and the volatility of the nominal exchange rate is a more likely explanation for finding enormous border effects.

Indeed, when we control for the volatility of the nominal exchange rate as in (13),[35] the average fixed-exchange-rate counterfactual for the half-life for international pairs equals 4.1 quarters which is less than the average half-life of *JJ* pairs (Appendix Table A1; also compare panels A and B in Figure 4). Likewise, the counterfactual volatility of innovations is 0.146 which is comparable to the volatility of innovations in the price ratio for *UU* pairs (Appendix Table A1). This translates into a modest border effect in the volatility of ω_{ijt}^{k} (315 km) and no border effect in the half-lives (column 4, Panels A and B respectively, Table 7). Note that the coefficient on the *Border* dummy is not statistically significant in both the half-life and the $\sigma(\omega_{ijt}^{k})$ regressions while the distance continues to significantly contribute to explaining the variation in half-lives and $\sigma(\omega_{ijt}^{k})$. Also note the importance of controlling for country heterogeneity effect, i.e., the border effect is larger in column 3 (no control for the country heterogeneity effect) than in column 4 (control for the country heterogeneity effect).

In summary, if the U.S. and Japan had a fixed exchange rate, the half-life of price differentials in *UJ* and *UU* city pairs conditional on distance and other controls would be statistically equal. The same applies to the volatility of innovations in price differentials. As in the U.S./Canada case, we conclude that the border effect is confounded with other factors, notably the volatility of the nominal exchange rate, so that conventional methods to assess the

what happens in our case. For many *UU* city pairs second largest eigenroot is close to the largest eigenroot in absolute value and it tends to be of a different sign thus dampening the deviation from steady state more quickly than the largest eigenroot alone might suggest. Like in U.S./Canada case, half-lives greater than 20 years are coded as missing (approximately 2% of the sample).

[35] We continue using l=4 for the U.S./Japan case.

border effect provide overstated estimates because of the country heterogeneity, persistence, and nominal exchange rate biases with nominal exchange rate effect being particularly important.

5. Concluding remarks

The border effects estimated from price data are often implausibly large. Moreover, significant border effects are found where they should not be found. We take these facts as indication that the commonly applied methodology makes assumptions that do not hold in the data. We show that the border effect can be confounded with factors that are unrelated to costs of crossing the border. Specifically, we identify three effects—country heterogeneity, persistence and nominal exchange rate effects—that can inflate the border effect so that the distance equivalent of the border can be of cosmic sizes. To address these problems, we develop a simple framework that separates the border effect from these confounding factors even in two-country cases.

We apply our approach to price data collected by Engel and Rogers (1996) for a sample of U.S. and Canadian cities and price data collected by Parsley and Wei (2001) for a sample of U.S. and Japanese cities. In sharp contrast to Engel and Rogers (1996), Parsley and Wei (2001) and other studies, we find no evidence of the border effect after controlling for factors confounding the border effect, i.e., cross-border price differentials behave in statistically and economically not distinguishable way from within-country price differentials. In the sample of U.S. and Canadian data, the border effect disappears once country-heterogeneity is taken into account. In the case of the U.S. and Japan, where exchange rate movements are larger, the barrier of the border is the exchange rate and the border itself has no significant effect on the dispersion of price differentials across the two countries.

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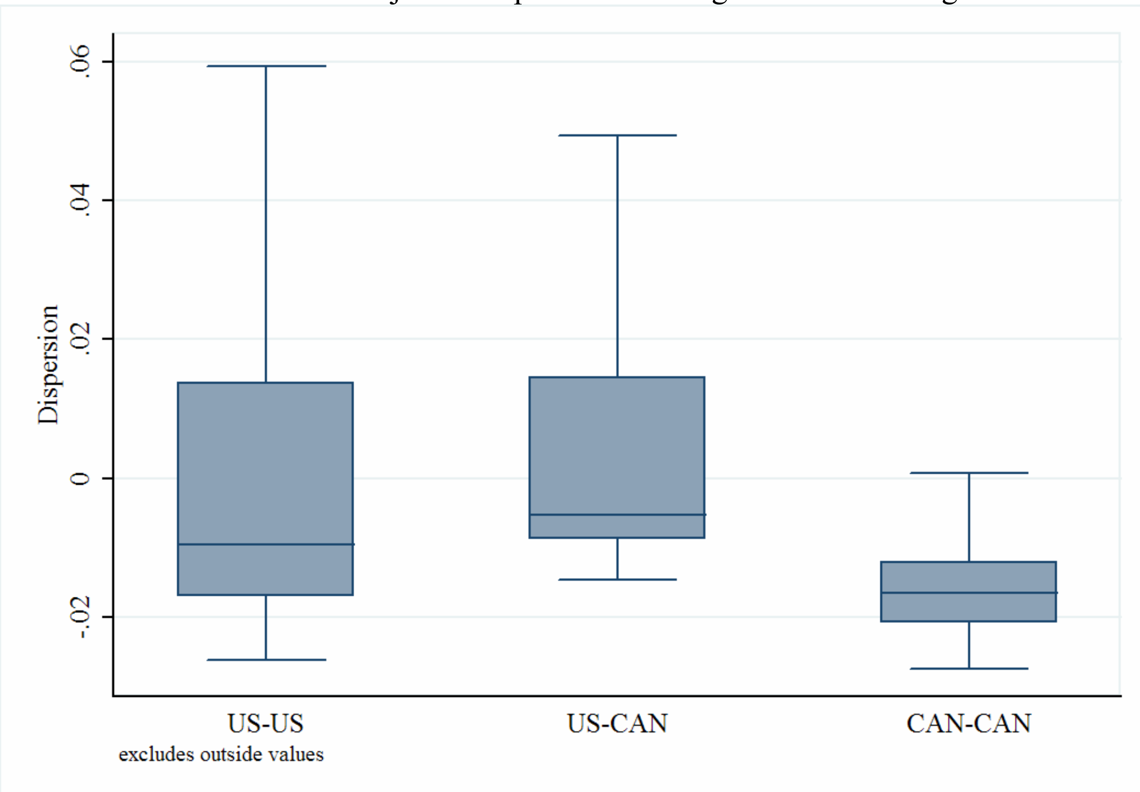
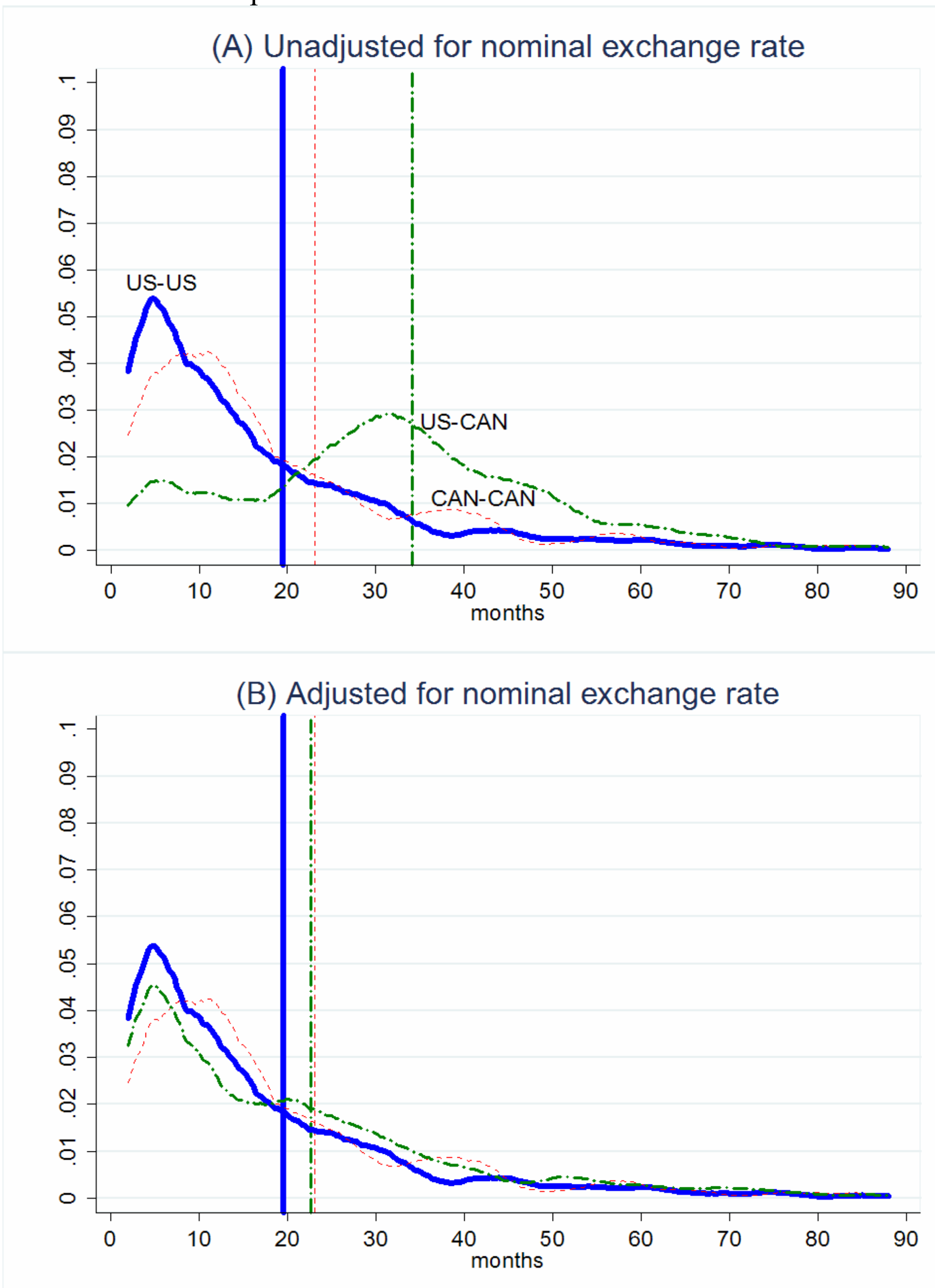
Figure 1. Distribution of distance-adjusted dispersion of changes in real exchange rate. U.S.-Canada.



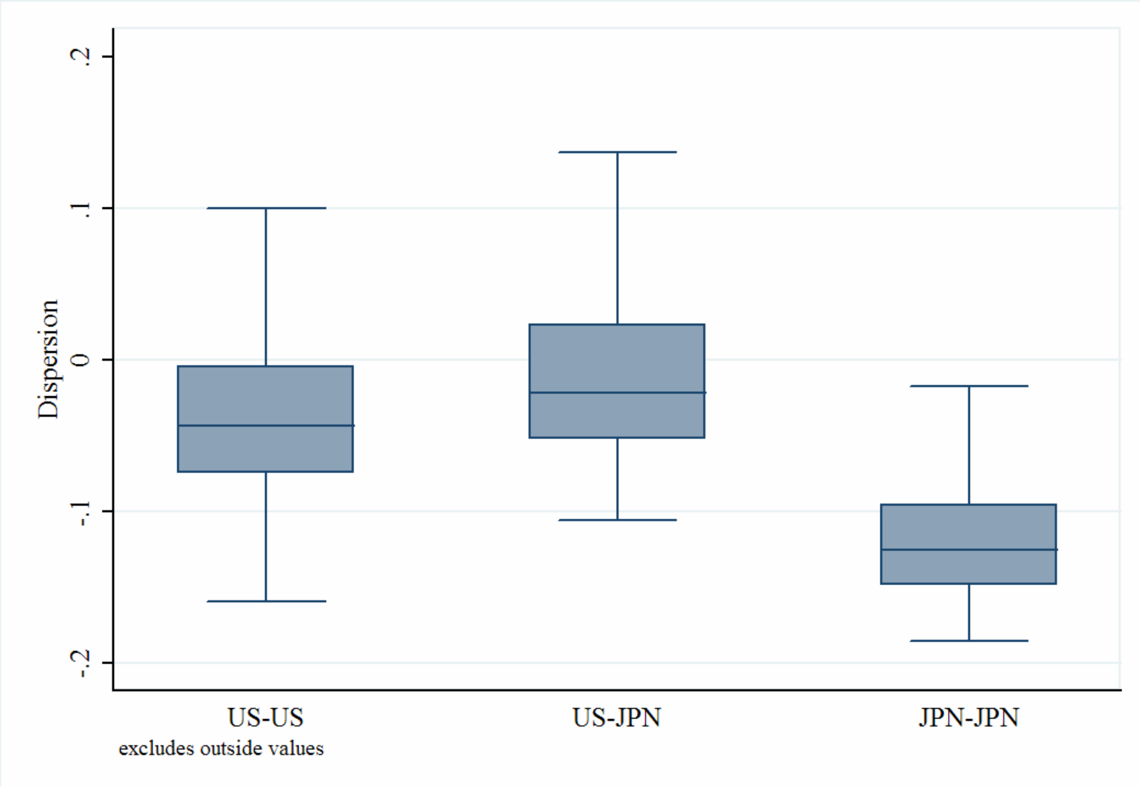
Note: Distance-adjusted dispersion is computed as the residual in the regression of dispersion on log distance between cities.

Figure 2. Estimated half-lives of price differentials. U.S.-Canada.



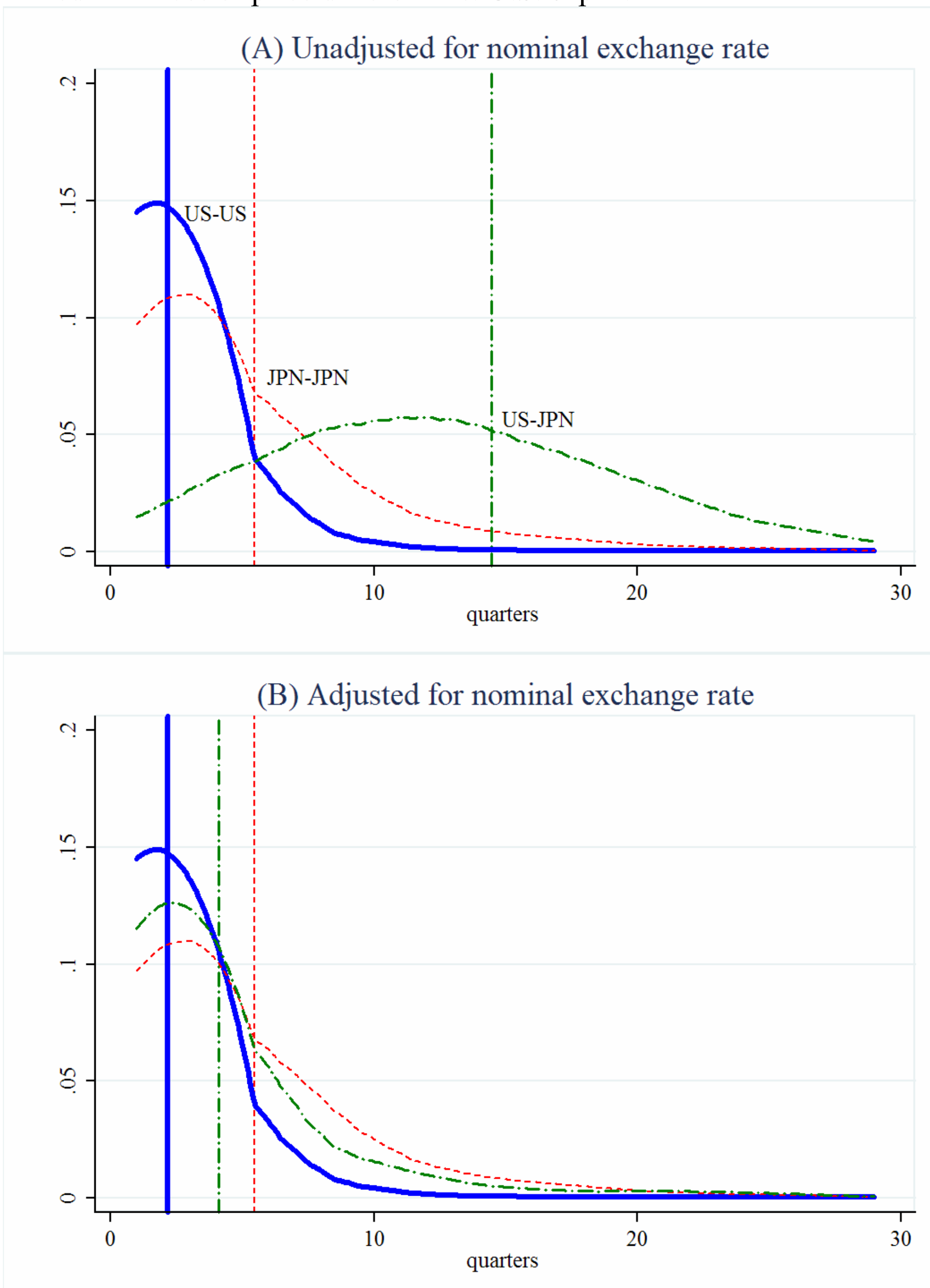
Note: The figure plot kernel densities of estimated half-lives. Bandwidth is 0.2. Solid line, broken, and dash/dot lines in panels A and B correspond to *UU*, *CC*, and *UC* pairs respectively. Vertical lines show the means for the respective group of city pairs. In panel A, half-lives are computed from the estimates of $\{\psi_{1,ijk},\ldots,\psi_{l,ijk}\}$ in (16). In panels B, half-lives are computed from the estimates $\{\psi_{1,ijk},\ldots,\psi_{l,ijk}\}$ in (13). Number of lags to remove short term dynamics in (13) and (16) is 6.

Figure 3. Distribution of distance-adjusted dispersion of changes in real exchange rate. U.S.-Japan.



Note: Distance-adjusted dispersion is computed as the residual in the regression of dispersion on log distance between cities.

Figure 4. Estimated half-lives of price differentials. U.S.-Japan.



Note: The figure plot kernel densities of estimated half-lives. Bandwidth is 0.2. Solid line, broken, and dash/dot lines in panels A and B correspond to *UU*, *JJ*, and *UJ* pairs respectively. Vertical lines show the means for the respective group of city pairs. In panel A, half-lives are computed from the estimates of $\{\psi_{1,ijk},\ldots,\psi_{l,ijk}\}$ in (16). In panel B, half-lives are computed from the estimates $\{\psi_{1,ijk},\ldots,\psi_{l,ijk}\}$ in (13). Number of lags to remove short term dynamics in (13) and (16) is 4.

Table 1. Descriptive statistics for U.S./Canada and U.S./Japan.

Panel A: U.S./Canada				
	US-US (1)	US-CAN (2)	CAN-CAN (3)	All (4)
Real exchange rate, q_{ijt}^{k}				
Standard deviation of changes in real exchange rate, $\sigma(\Delta_2 q_{ijt}^{k})$	0.031	0.037	0.016	0.032
Standard deviation of real exchange rate, $\sigma(q_{ijt}^{k})$	0.067	0.103	0.040	0.082
Log(distance)	7.17	7.53	7.21	7.35
N	1140	1764	504	3408

Panel B: U.S./Japan				
	US-US (1)	US-JPN (2)	JPN-JPN (3)	All (4)
Real exchange rate, q_{ijt}^{k}				
Standard deviation of changes in real exchange rate, $\sigma(\Delta q_{ijt}^{k})$	0.153	0.211	0.080	0.158
Standard deviation of real exchange rate, $\sigma(q_{ijt}^{k})$	0.159	0.648	0.103	0.389
Log(distance)	7.08	9.26	6.07	7.86
N	1215	3780	2457	7452

Note: the table reports mean values of the presented variables. Average is taken over city pairs. See text for the definitions of the variables. Different price indices are used for U.S. in U.S./Canada and U.S./Japan cases. $\sigma(x)$ is time series standard deviation of variable *x*.

Table 2. Estimates of the border effect. Real exchange rate. U.S./Canada.

	No control for country heterogeneity effect	Control for country heterogeneity effect
	(1)	(2)
Panel A: Dependent variable is standard deviation of changes in real exchange rate, $\sigma(\Delta_2 q)$		
Ln(*dist*)	1.076***	1.076***
	(0.269)	(0.269)
Border	12.026***	4.148***
	(0.363)	(0.393)
CC		-15.756***
		(0.529)
R^2	0.78	0.78
Border effect (km)	**71,438**	**47**
Panel B: Dependent variable is standard deviation of real exchange rate, $\sigma(q)$		
Ln(*dist*)	1.749**	1.749**
	(0.856)	(0.856)
Border	47.127***	32.217***
	(1.152)	(1.249)
CC		-29.821***
		(1.681)
R^2	0.50	0.50
Border effect (km)	**503 bln**	**100 mln**

Note: Coefficients in panels A and B are multiplied by 1000. *CC* is the dummy variable for Canada-Canada city pairs, *Border* is the dummy variable for US-Canada pairs (*UC* dummy). Omitted category is US-US city pairs. Robust standard errors are in parentheses. *, **, and *** are significant at 10%, 5%, and 1% respectively. The benchmark specification is (11). Border effect is computed according to (12). City and good category dummies are included but not reported.

Table 3. Border effect and country/city dummies. U.S.-Canada.

	No control for country heterogeneity effect	Control for country heterogeneity effect
	(1)	(2)
Ln(dist)	1.076***	1.076***
	(0.269)	(0.269)
Border	12.026***	4.148***
	(0.363)	(0.393)
CC		-15.756***
		(0.529)
Baltimore	5.329***	2.246***
	(0.606)	(0.596)
Boston	5.264***	2.181***
	(0.598)	(0.587)
Calgary	-4.135***	0.660
	(0.563)	(0.539)
Chicago	3.975***	0.892
	(0.559)	(0.554)
Dallas	4.620***	1.538**
	(0.629)	(0.615)
Detroit	1.309**	-1.773***
	(0.564)	(0.558)
Edmonton	-4.854***	-0.059
	(0.564)	(0.540)
Houston	4.329***	1.246**
	(0.631)	(0.618)
Los Angeles	-0.637	-3.719***
	(0.581)	(0.576)
Montreal	-5.259***	-0.463
	(0.564)	(0.540)
Miami	3.838***	0.755
	(0.608)	(0.598)
New York	-1.468***	-4.550***
	(0.564)	(0.559)
Ottawa	-5.428***	-0.633
	(0.566)	(0.543)
Philadelphia	3.556***	0.473
	(0.567)	(0.562)
Pittsburgh	2.135***	-0.948
	(0.632)	(0.618)
Quebec	-5.297***	-0.501
	(0.559)	(0.535)
Regina	-2.596***	2.199***
	(0.559)	(0.535)
San Francisco	1.870***	-1.212**
	(0.584)	(0.579)
St. Louis	5.767***	2.684***
	(0.601)	(0.591)
Toronto	-5.170***	-0.374
	(0.569)	(0.546)
Vancouver	-5.528***	-0.733
	(0.572)	(0.549)
Winnipeg	-4.891***	-0.096
	(0.558)	(0.534)
Washington, D.C.	3.271***	0.188
	(0.604)	(0.594)
Constant	18.337***	24.502***
	(1.954)	(1.959)

Note: Dependent variable is the standard deviation of changes in the real exchange rate, $\sigma(\Delta_2 q)$. *CC* is the dummy variable for Canada-Canada city pairs, *Border* is the dummy variable for US-Canada pairs (*UC* dummy). Omitted category is US-US city pairs. Coefficients are multiplied by 1000. Robust standard errors are in parentheses. *, **, and *** are significant at 10%, 5%, and 1% respectively. Columns 1 and 2 correspond to Table 2, Panel A. See the note for Table 2 and the text for further details.

Table 4. Estimates of the border effect. Real exchange rate. U.S./Canada.

	No control for the nominal exchange rate		Control for the nominal exchange rate	
	No control for country heterogeneity effect	Control for country heterogeneity effect	No control for country heterogeneity effect	Control for country heterogeneity effect
	(1)	(2)	(3)	(4)
Panel A: Dependent variable is volatility of innovations				
Ln(*dist*)	0.604***	0.604***	0.664***	0.664***
	(0.222)	(0.222)	(0.227)	(0.227)
Border	9.978***	2.892***	4.719***	-2.387***
	(0.298)	(0.323)	(0.305)	(0.331)
CC		-14.172***		-14.212***
		(0.435)		(0.445)
R^2	0.76	0.76	0.75	0.75
Border effect (km)	**14.5 mln**	**120**	**1,220**	**0**
Panel B: Dependent variable is half-life				
Ln(*dist*)	1.572***	1.572***	1.196*	1.196*
	(0.560)	(0.560)	(0.612)	(0.612)
Border	12.646***	14.426***	1.904**	3.626***
	(0.746)	(0.814)	(0.834)	(0.904)
CC		3.560***		3.444***
		(1.096)		(1.192)
R^2	0.35	0.35	0.24	0.24
Border effect (km)	**3,117**	**1,005**	**5**	**1**

Note: Coefficients in panel A are multiplied by 1000. *CC* is the dummy variable for Canada-Canada city pairs, *Border* is the dummy variable for US-Canada pairs (*UC* dummy). Omitted category is US-US city pairs. Robust standard errors are in parentheses. *, **, and *** are significant at 10%, 5%, and 1% respectively. The benchmark specification is (11). Border effect is computed according to (12). In Panel A volatility of innovations is the st. dev. of residuals in (16) (columns 1 and 2) and in (13) (columns 3 and 4). Half-lives in Panel B are computed from (16) (columns 1 and 2) and (13) (columns 3 and 4). Number of lags to remove short term dynamics in (13) and (16) is 6 (bimonthly data). City and good category dummies are included but not reported.

Table 5. Estimates of the border effect. Relative exchange rate. U.S./Canada.

	No control for country heterogeneity effect (1)	Control for country heterogeneity effect (2)
Panel A: Dependent variable is standard deviation of changes in relative exchange rate, $\sigma(\Delta_2 RER_{ijt}^k)$		
Ln(*dist*)	1.044***	1.044***
	(0.266)	(0.266)
Border	7.036***	-1.161***
	(0.357)	(0.388)
CC		-16.393***
		(0.522)
R^2	0.78	0.78
Border effect (km)	**845**	**0**
Panel B: Dependent variable is standard deviation of relative exchange rate, $\sigma(RER_{ijt}^k)$		
Ln(*dist*)	1.903**	1.903**
	(0.863)	(0.863)
Border	26.067***	11.100***
	(1.161)	(1.260)
CC		-29.934***
		(1.696)
R^2	0.45	0.45
Border effect (km)	**888,994**	**341**

Note: Relative exchange rate is defined as $RER_{ijt}^k = \left((P_{it}^k / P_{it}) \big/ (P_{jt}^k / P_{jt})\right)$ where P_{it}^k is the price of good *k* in city *i* at time *t*, P_{it} is the Consumer Price Index for city *i* at time *t*. Coefficients in panels A and B are multiplied by 1000. *CC* is the dummy variable for Canada-Canada city pairs, *Border* is the dummy variable for US-Canada pairs (*UC* dummy). Omitted category is US-US city pairs. Robust standard errors are in parentheses. *, **, and *** are significant at 10%, 5%, and 1% respectively. The benchmark specification is (11). Border effect is computed according to (12). City and good category dummies are included but not reported.

Table 6. Estimates of the border effect. Real exchange rate. U.S./Japan.

	No control for country heterogeneity effect	Control for country heterogeneity effect
	(1)	(2)
Panel A: Dependent variable is standard deviation of changes in real exchange rate, $\sigma(\Delta q)$		
Ln(*dist*)	4.651***	4.651***
	(1.114)	(1.114)
Border	81.777***	47.802***
	(3.168)	(2.829)
JJ		-67.952***
		(1.904)
R^2	0.83	0.83
Border effect (km)	**43.2 mln**	**29,080**

Panel B: Dependent variable is standard deviation of real exchange rate, $\sigma(q)$		
Ln(*dist*)	13.812***	13.812***
	(2.423)	(2.423)
Border	480.929***	459.764***
	(6.897)	(6.161)
JJ		-42.330***
		(4.160)
R^2	0.89	0.89
Border effect (km)	**>1 trln**	**>1 trln**

Note: Coefficients in panels A-B are multiplied by 1000. *JJ* is the dummy variable for Japan-Japan city pairs, *Border* is the dummy variable for US-Japan pairs (*UJ* dummy). Omitted category is US-US city pairs. Robust standard errors are in parentheses. *, **, and *** are significant at 10%, 5%, and 1% respectively. The benchmark specification is (11). Border effect is computed according to (12). City and good category dummies are included but not reported.

Table 7. Estimates of the border effect. Real exchange rate. U.S./Japan.

	No control for the nominal exchange rate		Control for the nominal exchange rate	
	No control for country heterogeneity effect (1)	Control for country heterogeneity effect (2)	No control for country heterogeneity effect (3)	Control for country heterogeneity effect (4)
Panel A: Dependent variable is volatility of innovations				
Ln(*dist*)	2.026**	2.026**	2.113**	2.113**
	(1.008)	(1.008)	(0.974)	(0.974)
Border	96.556***	66.378***	42.313***	12.153***
	(2.868)	(2.561)	(2.775)	(2.478)
JJ		-60.355***		-60.320***
		(1.727)		(1.667)
R^2	0.80	0.80	0.68	0.68
Border effect (km)	**>1 trln**	**>1 trln**	**497 mln**	**315**
Panel B: Dependent variable is half-life				
Ln(*dist*)	0.379*	0.379*	0.266*	0.266*
	(0.202)	(0.202)	(0.144)	(0.144)
Border	9.901***	11.622***	-0.241	1.428***
	(0.576)	(0.514)	(0.410)	(0.366)
JJ		3.443***		3.338***
		(0.341)		(0.244)
R^2	0.43	0.43	0.18	0.18
Border effect (km)	**>1 trln**	**>1 trln**	**0**	**0**

Note: Coefficients in panels A are multiplied by 1000. *JJ* is the dummy variable for Japan-Japan city pairs, *Border* is the dummy variable for US-Japan pairs (*UJ* dummy). Omitted category is US-US city pairs. Robust standard errors are in parentheses. *, **, and *** are significant at 10%, 5%, and 1% respectively. The benchmark specification is (11). Border effect is computed according to (12). In Panel A volatility of innovations is the st. dev. of residuals in (16) (columns 1 and 2) and in (13) (columns 3 and 4). Half-lives in Panel B are computed from (16) (columns 1 and 2) and (13) (columns 3 and 4). Number of lags to remove short term dynamics in (16) and (13) is 4 (quarterly data). City and good category dummies are included but not reported.

Table A1. Descriptive statistics.

	(1)	(2)	(3)	(4)
		Panel A: U.S./Canada		
	US-US	US-Canada	Canada-Canada	All
Volatility of innovations				
$\sigma(\eta)$, **no** control for the volatility of nominal exchange rate, eq. (16)	0.028	0.031	0.014	0.028
$\sigma(\omega)$, control for volatility of nominal exchange rate, eq. (13)	0.028	0.029	0.014	0.025
Half-life of price differentials, months				
no control for volatility of nominal ex. rate, eq. (16)	19.8	34.0	23.2	26.6
control for volatility of nominal exchange rate, eq. (13).	19.8	22.6	23.6	19.0
		Panel B: U.S./Japan		
	US-US	US-Japan	Japan-Japan	All
Volatility of innovations				
$\sigma(\eta)$, **no** control for the volatility of nominal exchange rate, eq. (16)	0.120	0.195	0.061	0.138
$\sigma(\omega)$, control for volatility of nominal exchange rate, eq. (13)	0.120	0.146	0.061	0.115
Half-life of price differentials, quarters				
no control for volatility of nominal ex. rate, eq. (16)	2.2	14.5	5.5	9.3
control for volatility of nominal exchange rate, eq. (13).	2.2	4.1	5.5	4.2

Note: Table reports mean values of the presented variables. Half-life is in the periods at which price series are available. U.S./Canada and U.S./Japan data are bimonthly and quarterly, respectively. For U.S./Canada case, half-lives greater than 10 years are coded as missing (less than 2% of the sample). For U.S./Japan case, half-lives greater than 20 years are coded as missing (less than 2% of the sample). $\sigma(x)$ is the time series standard deviation of variable *x*.

Table A2. Unit root tests of price differentials.

Method	U.S.-Canada real exchange rate, *q*		U.S.-Japan real exchange rate, *q*	
	Statistic	p-value	Statistic	p-value
	(1)	(2)	(3)	(4)
Null: Unit root (assumes common unit root process)				
Levin, Lin & Chu t-stat	-11.47	0.00	-49.91	0.00
Breitung t-stat	-6.19	0.00	-6.19	0.00
Null: Unit root (assumes individual unit root process)				
Im, Pesaran and Shin W-stat	-10.4	0.00	-14.045	0.00
ADF - Fisher Chi-square	7076.9	0.00	16581.6	0.00
PP - Fisher Chi-square	13357.0	0.00	52287.4	0.00

Note: tests are for levels with intercept only. Probabilities for Fisher tests are computed using an asymptotic Chi-square distribution. All other tests assume asymptotic normality. Number of lags to remove short term dynamics is 6 for U.S.-Canada case (bimonthly data) and 4 for U.S.-Japan (quarterly data). Bartlett kernel with plug-in Newey-West bandwidth selection is used for PP-Fisher. See Levin, Lin and Chu (2002), Breitung (2000), Im, Pesaran and Shin (2003), Maddala and Wu (1999, ADF-Fisher) and Choi (2001, PP-Fisher) for details.

Table A3. Descriptive statistics for relative exchange rate. U.S.-Canada.

	US-US (1)	US-Canada (2)	Canada-Canada (3)	All (4)
Standard deviation of changes in *relative* exchange rate, $\sigma(\Delta_2 RER_{ijt}^k)$	0.032	0.032	0.016	0.029
Standard deviation of *relative* exchange rate, $\sigma(RER_{ijt}^k)$	0.069	0.080	0.039	0.070
Volatility of innovations, $\sigma(\omega)$, in *relative* exchange rate, eq. (16)	0.028	0.026	0.014	0.025
Half-life of price differentials for *relative* exchange rate, eq. (16), months	14.6	22.4	20.6	19.4

Note: Table reports mean values of the presented variables. Relative exchange rate is defined as $RER_{ijt}^k = \left((P_{it}^k / P_{it})\middle/(P_{jt}^k / P_{jt})\right)$ where P_{it}^k is the price of good *k* in city *i* at time *t*, P_{it} is the Consumer Price Index for city *i* at time *t*. Half-life is in the bimonthly periods. Half-lives greater than 10 years are coded as missing (less than 2% of the sample). $\sigma(x)$ is the time series standard deviation of variable *x*.

Appendix

Decomposition of volatility

To derive (9), substitute (7) and (8) into (3) to eliminate CC_{ij} and UU_{ij}:

$$\begin{aligned}
\sigma_{ij} &= \beta UC_{ij} + \gamma_U UU_{ij} + \gamma_C CC_{ij} + \sum_{s=1}^{N} \alpha_s D_s = \\
&= \beta UC_{ij} + \gamma_U(-\tfrac{1}{2}UC_{ij} + \tfrac{1}{2}\sum_{s=k+1}^{N} D_s) + \gamma_C(-\tfrac{1}{2}UC_{ij} + \tfrac{1}{2}\sum_{s=1}^{k} D_s) + \sum_{s=1}^{N} \alpha_s D_s = \\
&= [\beta - \tfrac{1}{2}(\gamma_U + \gamma_C)]UC_{ij} + \sum_{s=k+1}^{N}(\tfrac{1}{2}\gamma_U + \alpha_s)D_s + \sum_{s=1}^{k}(\tfrac{1}{2}\gamma_C + \alpha_s)D_s = \\
&= [\beta - \tfrac{1}{2}(\gamma_U + \gamma_C)]UC_{ij} + (\tfrac{1}{2}\gamma_U + \bar{\alpha}_U)U_{ij} + (\tfrac{1}{2}\gamma_C + \bar{\alpha}_C)C_{ij} + \sum_{s=k+1}^{N} \hat{\alpha}_s D_s + \sum_{s=1}^{k} \tilde{\alpha}_s D_s \, ,
\end{aligned}$$

where U_{ij} (C_{ij}) is equal to one if city i or j is in the US (Canada) and zero otherwise.

To derive (10), rearrange terms in (3) as follows:

$$\begin{aligned}
\sigma_{ij} &= \beta UC_{ij} + \gamma_U UU_{ij} + \gamma_C CC_{ij} + \sum_{s=1}^{N} \alpha_s D_s = \\
&= \beta UC_{ij} + \gamma_U UU_{ij} + \gamma_C CC_{ij} + \sum_{s=1}^{k} \alpha_s D_s + \sum_{s=k+1}^{N} \alpha_s D_s \\
&= \beta UC_{ij} + \gamma_U UU_{ij} + \gamma_C CC_{ij} + \sum_{s=1}^{k} \tilde{\alpha}_s D_s + \bar{\alpha}_C \left(\sum_{s=1}^{k} D_s\right) + \\
&\qquad + \sum_{s=k+1}^{N} \hat{\alpha}_s D_s + \bar{\alpha}_U \left(\sum_{s=k+1}^{N} D_s\right) = \\
&= \beta UC_{ij} + \gamma_U UU_{ij} + \gamma_C CC_{ij} + \sum_{s=1}^{k} \tilde{\alpha}_s D_s + \bar{\alpha}_C \left(2 \cdot CC_{ij} + UC_{ij}\right) + \\
&\qquad + \sum_{s=k+1}^{N} \hat{\alpha}_s D_s + \bar{\alpha}_U \left(2 \cdot UU_{ij} + UC_{ij}\right) = \\
&= (\beta + \bar{\alpha}_C + \bar{\alpha}_U)UC_{ij} + (\gamma_U + 2\bar{\alpha}_U)UU_{ij} + (\gamma_C + 2\bar{\alpha}_C)CC_{ij} + \sum_{s=1}^{k} \tilde{\alpha}_s D_s + \sum_{s=k+1}^{N} \hat{\alpha}_s D_s = \\
&= b_{UC}UC_{ij} + b_{CC}CC_{ij} + b_{UU}UU_{ij} + \sum_{s=1}^{k} \tilde{\alpha}_s D_s + \sum_{s=k+1}^{N} \hat{\alpha}_s D_s \, .
\end{aligned}$$

Persistence and variance

Consider q_t that follows an AR(l) process. Then the covariance matrix for $\boldsymbol{Q}_t = [q_t, ..., q_{t-l}]'$ is given by $\Gamma_0 \equiv E(\boldsymbol{Q}_t \boldsymbol{Q}_t') = A\Gamma_0 A' + E(\boldsymbol{\varepsilon}_t \boldsymbol{\varepsilon}_t') = A\Gamma_0 A' + \Omega$, where A is the companion matrix for the process. Thus, $vec(\Gamma_0) = (I - A \otimes A)^{-1} vec(\Omega)$ and $\sigma^2(q_t) \equiv \gamma_{1,1} = e_1 vec(\Gamma_0)$. Let us denote $B \equiv (I - A \otimes A)^{-1}, \Sigma \equiv vec(\Omega)$. Then $\sigma^2(q_t) \equiv \gamma_{1,1} = \sum_{i=1}^{l^2} B_{1,i}\Sigma_i = B_{1,1}\sigma_\varepsilon^2$. Observe that $\sigma^2(q_t)$ is strictly increasing in $B_{1,1}$ and σ_ε^2. Eigenvalues of $A \otimes A$ are products of eigenvalues of A (see Magnus and Neudecker, 1999, p. 28-29). Since persistence is directly related to the absolute size of eigenvalues, we conclude that the larger the eigenvalues (i.e., larger B), the larger is the half-life. Hence, holding everything else constant, a series with a long half-life HL has a larger variance.

Analysis of the frequency domain helps to understand why $\sigma(\Delta q_t)$ is attenuated for persistent q_t. Operator Δ is a filter that eliminates or under-weighs low frequency variation. Thus if most variation of the series comes from low frequency portion of the spectrum (high persistence), $\sigma(\Delta q_t)$ should be less than $\sigma(q_t)$ if q_t is persistent. Note that differencing series does no make series have the same persistence unless q_t is a random walk.