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# Do Borders Really Slash Trade? A Meta-Analysis

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**Abstract**

National borders reduce trade, but most estimates of the border effect seem puzzlingly large. We show that major methodological innovations of the last decade combine to shrink the border effect to a mere 28% reduction in international trade flows worldwide. The border effect varies across regions: it is large in emerging countries, but close to zero in OECD countries. For the computation we collect 1,271 estimates of the border effect reported in 61 studies, codify 32 aspects of study design that may influence the estimates, and use Bayesian model averaging to take into account model uncertainty. Our results suggest that methods systematically affect the estimated border effects. Especially important is the level of aggregation, measurement of internal and external distance, control for multilateral resistance, and treatment of zero trade flows. We find no evidence of publication bias.

**Keywords:** Bayesian model averaging, bilateral trade, borders, gravity, meta-analysis, publication selection bias

**JEL Codes:** F14, F15

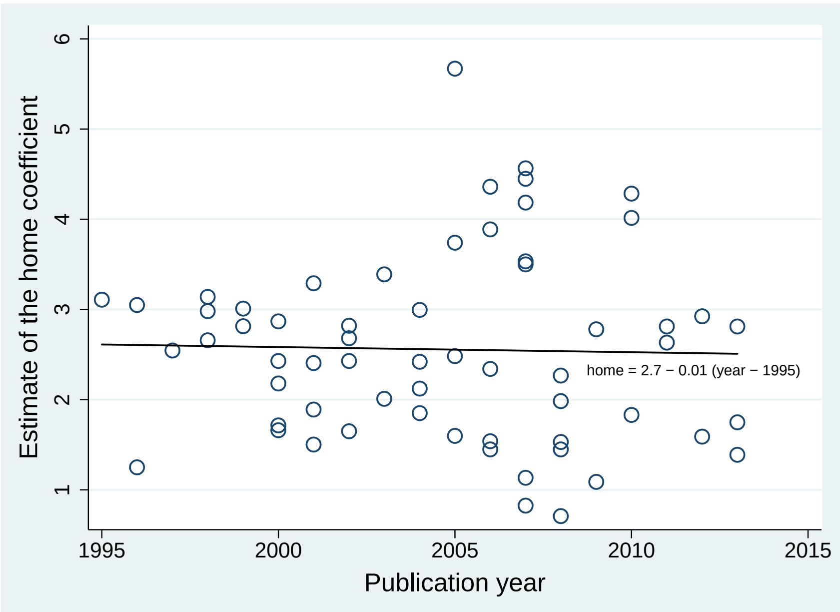
*An online appendix with data and code is available at `meta-analysis.cz/border`. Corresponding author: Zuzana Irsova, `zuzana.irsova@ies-prague.org`. We acknowledge support from the Czech Science Foundation (grant #15-02411S). The views expressed here are ours and not necessarily those of the Czech National Bank.

# 1 Introduction

The finding that international borders significantly reduce trade, first reported by McCallum (1995), has become a stylized fact of international economics. A high ratio of trade within national borders to trade across borders, after controlling for other trade determinants, implies large unobserved border barriers, an implausibly high elasticity of substitution between domestic and foreign goods, or both. Obstfeld & Rogoff (2001) include the border effect among the six major puzzles in international macroeconomics, and dozens of researchers have attempted to shrink McCallum's original estimates.

Researchers have proposed several methodological solutions to the border puzzle, such as the inclusion of multilateral resistance terms, consistent measurement of within and between-country distance, and use of disaggregated data. But the border effects reported in the literature are, on average, still close to those estimated by McCallum (1995): regions are likely to trade with foreign regions about fifteen times less than with regions in the same country.

Figure 1: The reported border effects diverge, not decrease




*Notes:* The figure depicts median estimates of the "home coefficient" (the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows) reported in individual studies. The border effect can be obtained by exponentiating the home coefficient: the mean is $\exp(2.7) = 15$. The horizontal axis measures the year when the first drafts of studies appeared in Google Scholar. The black line shows the linear fit.

Figure 1 shows that new methods and data sets used in the gravity equation, the workhorse tool for computing border effects, increase the dispersion of the results. The reported border effects do not diminish over time and do not converge to a consensus value that could be used for calibrations. Our goal in this paper is to collect the empirical estimates of the border effect, examine why they vary, and compute a benchmark value for different regions conditional on the implementation of major innovations in the gravity equation. That is, using previously reported results we construct a large synthetic study that estimates the border effect, but corrects for potential publication or misspecification biases.

We employ the framework of meta-analysis, the quantitative method of research synthesis (Stanley, 2001). Meta-analysis has been used in economics by, for instance, Card & Krueger (1995) on the employment effects of minimum wage increases, Disdier & Head (2008) on the impact of distance on trade, Havranek & Irsova (2011) on the relation between foreign investment and local firms' productivity, and Chetty *et al.* (2011) on the intertemporal elasticity of substitution in labor supply. We collect 32 aspects of studies, such as the characteristics of data, estimation, inclusion of control variables, number of citations, and information on the publication outlet. To explore how these characteristics affect the estimates of the border effect, we employ Bayesian model averaging (Raftery *et al.*, 1997). The method addresses the model uncertainty inherent in meta-analysis by estimating regressions comprising the potential subsets of the study aspects and weighting them by statistics related to the goodness of fit.

Our results suggest that many innovations in estimating the gravity equation systematically affect the reported border effect: for example, the use of disaggregated data, consistent measurement of within and between-country distance, data on actual road or sea distance instead of the great-circle distance, control for multilateral resistance, and the use of the Poisson pseudo-maximum likelihood estimator. When we put these influences together and compute the general equilibrium impact of borders conditional on best practice methodology, we find that borders reduce international trade by only 28% worldwide. The border effects differ significantly across regions—we obtain large estimates for developing and transition countries, but estimates close to zero for most OECD countries.

We find little evidence of publication bias in the literature: researchers do not preferentially report positive or statistically significant estimates of the border effect. This result is remarkable considering a recent survey of estimates of publication bias, Doucouliagos & Stanley (2013), who show that the problem of selecting intuitive and statistically significant estimates concerns most fields of empirical economics. For example, Ashenfelter *et al.* (1999) find evidence of publication bias in the literature on the returns from schooling, Görg & Strobl (2001) in the estimates of foreign direct investment spillovers, and Rusnak *et al.* (2013) in the literature on the transmission of monetary policy shocks to prices. Unlike many other important parameters in economics, it is easy for researchers to obtain statistically significant estimates of the border effect, so there is little motivation for publication selection.

The remainder of the paper is organized as follows. Section 2 describes how we collect data from studies and discusses the basic properties of the data set. Section 3 tests for publication bias in the literature. Section 4 explores the heterogeneity in the estimated border effects and constructs best practice estimates for different regions. Section 5 presents robustness checks. Section 6 discusses the potential pitfalls of meta-analysis. Section 7 concludes. Appendix A presents diagnostics of Bayesian model averaging, Appendix B shows the list of studies included in the meta-analysis, and the online appendix at `meta-analysis.cz/border` provides the data and code we use in the paper.

## 2 The Border Effects Data Set

The studies from which we collect estimates of the border effect assume that trade flows are generated by the following general definition of the gravity equation:

$$\text{Trade}_{ij} = G \cdot \text{Exporter}_i \cdot \text{Importer}_j \cdot \text{Distance}_{ij}^{-\alpha} \cdot \exp(home \cdot \text{Same country}_{ij}) \cdot \text{Access}_{ij}, \quad (1)$$

where $Trade_{ij}$ denotes the volume of trade flows from region $i$ to region $j$, $G$ is a "gravitational" constant, $Exporter_i$ denotes the exporting capabilities of region $i$ with respect to all trading partners, $Importer_j$ denotes the characteristics of region $j$ that affect imports from all trading partners, $Distance_{ij}$ denotes the distance between regions $i$ and $j$, $Same\ country_{ij}$ denotes a dummy variable that equals one if regions $i$ and $j$ belong to the same country, and $Access_{ij}$ denotes all other bilateral accessibility characteristics between regions $i$ and $j$.

The authors usually estimate a log-linearized version of (1) with exporter and importer fixed effects to control for multilateral resistance terms. Some authors use non-linear estimators, and even for the linear estimation there are many method choices the authors must make. We identify 32 aspects of study design that may potentially influence the estimate of the border effect and explain them in detail in Section 4. We collect estimates of *home* reported in studies, which is the semi-elasticity corresponding to the ratio of within to between-country trade flows; the border effect can be obtained by exponentiating the semi-elasticity. It is convenient to analyze the semi-elasticities because authors provide standard errors for them and the estimates should be approximately normally distributed.

Our data sources are studies that estimate the semi-elasticities; we call them primary studies and search for them using the RePEc database. We use the following search query for titles, keywords, and abstracts of papers listed in the database: `(border OR home bias) AND trade AND gravity`. The search yields 370 hits since 1995. We read the abstracts of all the studies and download those that show promise of containing empirical estimates of the border effect. Additionally, we examine the references of the studies and obtain other papers that might provide empirical estimates. We stop the search on January 1, 2014. The list of all studies examined is available in the online appendix at `meta-analysis.cz/border`.

We apply three inclusion criteria. First, the study must examine the effect of international borders. That is, we exclude studies estimating intranational border effects (for example, Wolf, 2000). We expect the mechanism driving border effects in intranational trade to be different enough to call for a separate meta-analysis. Second, we exclude papers that include the "same nation" dummy in the gravity equation as a control variable for territories, such as the overseas departments of France (for example, Rose, 2000). The "same nation" dummy has little variation and often captures trade between a large country and its small territories. Third, we only include studies that provide standard errors for their estimates—or statistics from which standard errors can be computed. Without estimates of standard errors we cannot test for publication bias in the literature. While we conduct the search using English keywords, we do not further exclude any studies based on the language of publication.

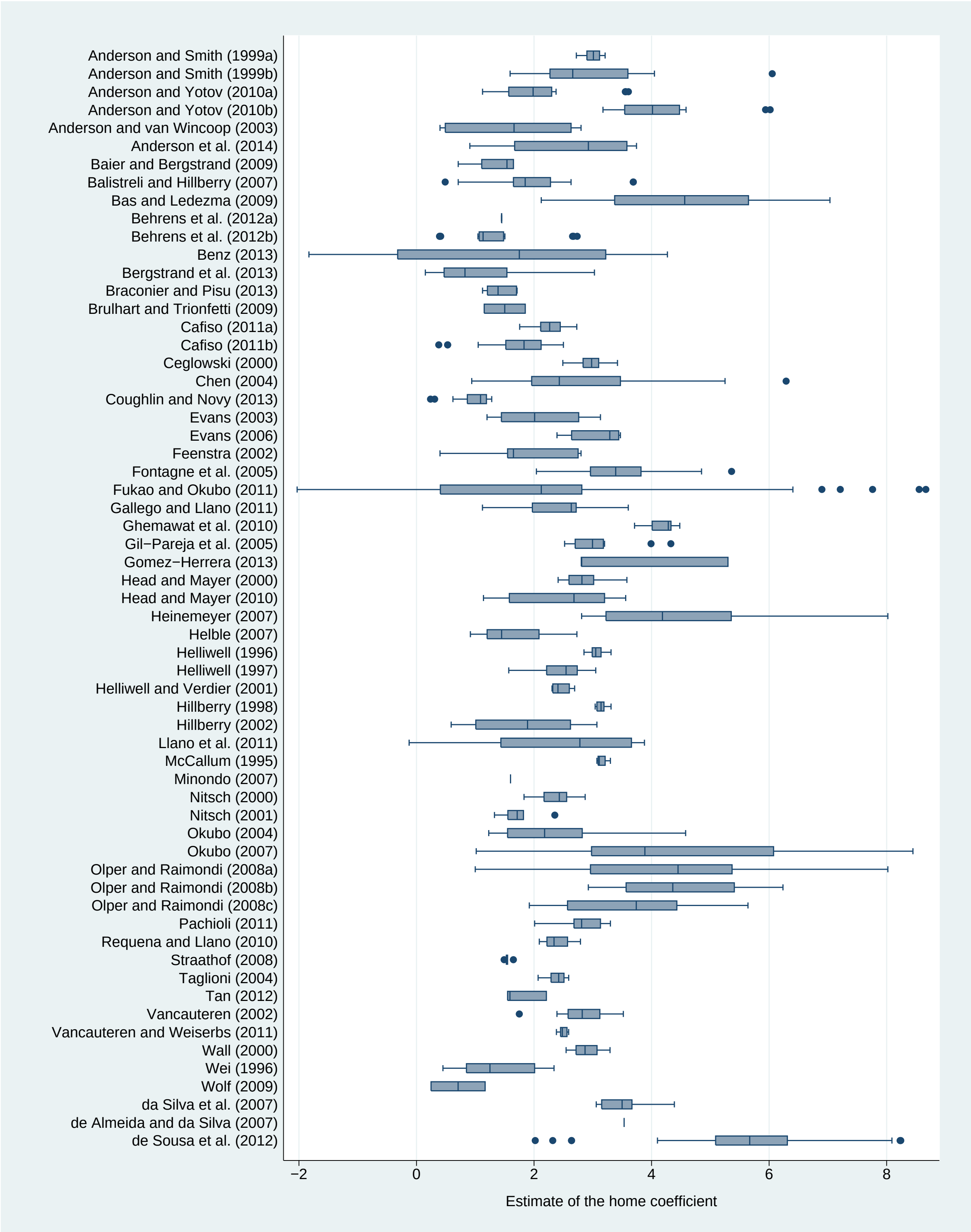
The 61 studies that conform to our selection criteria are listed in Appendix B. Of these, 48 are published in refereed journals and 13 are working papers or mimeographs; later in the analysis we control for the publication outlet of the study and other aspects of quality. The median study in our sample was published in 2007, which shows that the literature estimating border effects is alive and well, with more and more studies coming out each year. Together the studies have received almost 11,000 citations in Google Scholar, or about 800 on average per year, which suggests the importance of border effects for international economics.

We collect all estimates of the semi-elasticity from the primary studies. The approach yields an unbalanced data set, since some studies report many more estimates than other studies, but has three big advantages. First, it is demanding and sometimes impossible to select the authors' preferred estimate to represent each study, so by collecting all estimates we avoid the most subjective stage of meta-analysis. Second, throwing away information is inefficient, and many studies report estimates employing alternative methods or data sets, which increases the variation in our data set. Third, using multiple estimates per study we can employ study-level fixed effects, which removes all characteristics idiosyncratic to individual studies. In total, we gather 1,271 estimates of the semi-elasticity; the median primary study reports 13 estimates.

A few problems concerning data collection are worth mentioning. To start with, the variable capturing the border effect is not always defined in the same way as *Same country* in (1). Often it equals one for cross-border trade flows, in which case we simply take the negative of the estimated coefficient. Sometimes, however, the dummy variable equals one only for trade flows crossing the border in one direction (for example, Anderson & Smith, 1999). Following the common practice to "better err on the side of inclusion" in meta-analysis (Stanley, 2001, p. 135), we choose to include the estimates of directional border effects, but control for this aspect of methodology to see whether it yields systematically different estimates. Finally, the collection of data is labor-intensive, since we gather information on 32 aspects of estimation design for all 1,271 estimates. To alleviate the danger of typos and mistakes, both of us collect the data independently and correct inconsistencies by comparing the two data sets. The final data set is available in the online appendix at `meta-analysis.cz/border`.

Figure 2 shows a box plot of the estimates reported in the primary studies; the heterogeneity both between and within studies is substantial. It is apparent, however, that most studies report at least some estimates close to 3, near the original estimate by McCallum (1995). A large portion of the heterogeneity in the estimates may be due to differences in data, and especially different countries for which the border effect is evaluated. Table 1 shows the mean estimates for the countries and country groups that are examined most commonly in the literature.

Figure 2: Estimated border effects vary widely




*Notes:* The figure shows a box plot of the estimates of the home coefficient (the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows) reported in individual studies. Full references for the studies included in the meta-analysis are available in Appendix B.

Table 1: Border effects differ across countries

| | | Unweighted | | | Weighted | | |
|---|---|---|---|---|---|---|---|
| | No. of estimates | Mean | 95% conf. int. | | Mean | 95% conf. int. | |
| Canada | 213 | 2.86 | 2.66 | 3.06 | 2.81 | 2.58 | 3.05 |
| US | 64 | 0.72 | 0.03 | 1.40 | 1.36 | 0.99 | 1.73 |
| EU | 263 | 2.55 | 2.04 | 3.05 | 2.59 | 2.18 | 2.99 |
| OECD | 98 | 2.35 | 1.71 | 3.00 | 2.41 | 1.90 | 2.91 |
| Emerging | 82 | 5.05 | 4.59 | 5.51 | 4.14 | 3.18 | 5.10 |
| All countries | 1,271 | 3.03 | 2.54 | 3.53 | 2.59 | 2.23 | 2.95 |

*Notes:* The table presents mean estimates of the home coefficient (the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows) for selected countries and country groups. The confidence intervals around the mean are constructed using standard errors clustered at both the study and data set level (the implementation of two-way clustering follows Cameron *et al.*, 2011). In the right-hand part of the table the estimates are weighted by the inverse of the number of estimates reported per study.

We say that an estimate corresponds to the border effect of a particular country if identification of the semi-elasticity comes from trade flows within the country. For example, if data on trade flows between Canadian provinces are used, such as in McCallum (1995), we consider the estimated border effect Canadian, although the estimation also includes data on the US (flows between Canadian provinces and US states). Some authors used both province-to-province trade flows and state-to-state flows (for example, Anderson & van Wincoop, 2003); the resulting estimates of the border effect correspond to both Canada and the US and are not shown in the table. The estimates for all other countries and groups of countries are nevertheless included in the overall mean reported in the last row of the table.

Table 1 also shows the corresponding confidence intervals constructed using clustered standard errors. Many meta-analyses cluster standard errors at the study level, because estimates reported in the same primary study are likely to be dependent. Nevertheless, we are not aware of any meta-analysis that also tries to take into account the dependence in estimates due to the use of similar data sets. A few studies in our sample use the same data set, especially the one introduced by Anderson & van Wincoop (2003), but many others simply add a few years to data used elsewhere. So, we consider data sets to be the same or very similar if they provide data on the same region and start in the same year, and additionally cluster standard errors at the level of similar data sets. The implementation of two-level clustering follows the approach of Cameron *et al.* (2011).

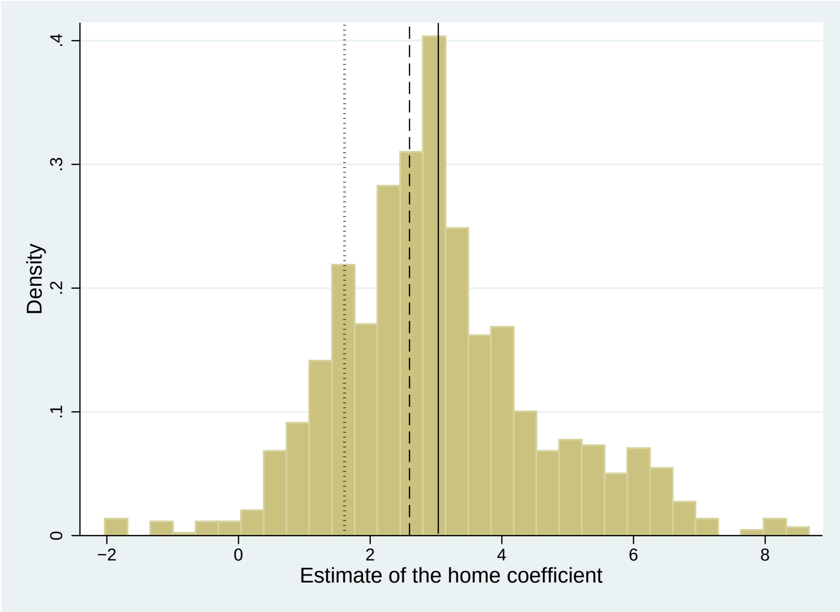
The left-hand part of the table shows unweighted estimates; the right-hand part shows estimates weighted by the inverse of the number of observations reported in each study. By using these weights we assign each study the same importance; otherwise studies reporting many semi-elasticities drive the results. The mean unweighted estimate of the semi-elasticity equals 3, virtually identical to the original estimate of the parameter by McCallum (1995). This semi-elasticity implies a border effect of $\exp(3) = 20$, which means that an average region in an average country trades twenty times more with regions in the same country than with foreign regions of similar characteristics. The 95% confidence interval for the mean estimate of the border effect is (13, 34), which shows substantial uncertainty due to differences in methodology.

The table documents that the semi-elasticities estimated for individual countries vary substantially. The smallest mean estimate corresponds to the US (implying a border effect of 2 in the case of the unweighted estimates), while the largest mean is obtained for emerging countries (implying a border effect of 156). The respective means for Canada, the EU, and OECD countries are close to the overall mean. When we weight the estimates by the inverse of the number of observations reported in each study, we obtain a smaller overall mean, implying a border effect of 13.3, and the country-specific estimates get less dispersed. In both cases the lower bound of the 95% confidence interval of the estimate for emerging countries is larger than the upper bounds of the confidence intervals for all other groups of countries. That is, the border effects estimated in the literature suggest that developing and transition countries are substantially less integrated into global trade than developed countries.

Figure 3 shows the histogram of the estimated semi-elasticities. We see that almost all the estimates are positive; in the data we only have 22 negative estimates, 1.7% of all the semi-elasticities. The median estimate is very close to the overall mean and equals 2.9. The median estimate of the median semi-elasticities reported in individual studies equals 2.6, which is virtually identical to the mean of the estimates weighted by the inverse of the number of estimates reported per study. The closeness of the mean and median together with the shape of the histogram suggests that there are no serious outliers in our data set, so we do not exclude any estimates from the meta-analysis.

The journals in which the primary studies are published differ greatly in prestige and rating. On the one hand, some studies are published in top field and general interest journals; on the other hand, many estimates come from studies published in local outlets. To illustrate the potential differences in quality we distinguish a group of studies published in top field or top or

Figure 3: Studies in top journals report smaller estimates




*Notes:* The figure shows the histogram of the estimates of the home coefficient (the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows) reported in individual studies. The solid vertical line denotes the median of all the estimates. The dashed line denotes the median of median estimates from studies. The dotted line denotes the median of estimates reported in studies published in the *American Economic Review*, *Journal of International Economics*, *International Economic Review*, *European Economic Review*, and *Journal of Applied Econometrics*.

second-tier general interest journals: the *American Economic Review*, *Journal of International Economics*, *International Economic Review*, *European Economic Review*, and *Journal of Applied Econometrics*. Eleven studies in our sample are published in these journals and they report a median semi-elasticity of 1.7, implying a border effect of 5.5, less than a third of the overall mean effect. Studies in respected journals seem to report smaller semi-elasticities, but the pattern may be explained by differences in methodology. Another potential reason for between-study differences in estimates is publication selection.

# 3 Publication Bias

Publication selection bias arises when estimates have a different probability of being reported based on their sign or statistical significance. Sometimes it is called the "file drawer problem" (Rosenthal, 1979): researchers may hide in their file drawers estimates that are insignificant or have an unintuitive sign and search for estimates that are easier to publish. Publication bias has been identified in empirical economics by, for example, DeLong & Lang (1992), Card & Krueger (1995), and Ashenfelter *et al.* (1999). In a survey of examinations of publication bias, Doucouliagos & Stanley (2013) find that most fields of empirical economics are seriously affected by the problem. Because the potential presence of publication bias determines the weights that should be used in meta-analysis, we test for the bias before we proceed to the analysis of heterogeneity.

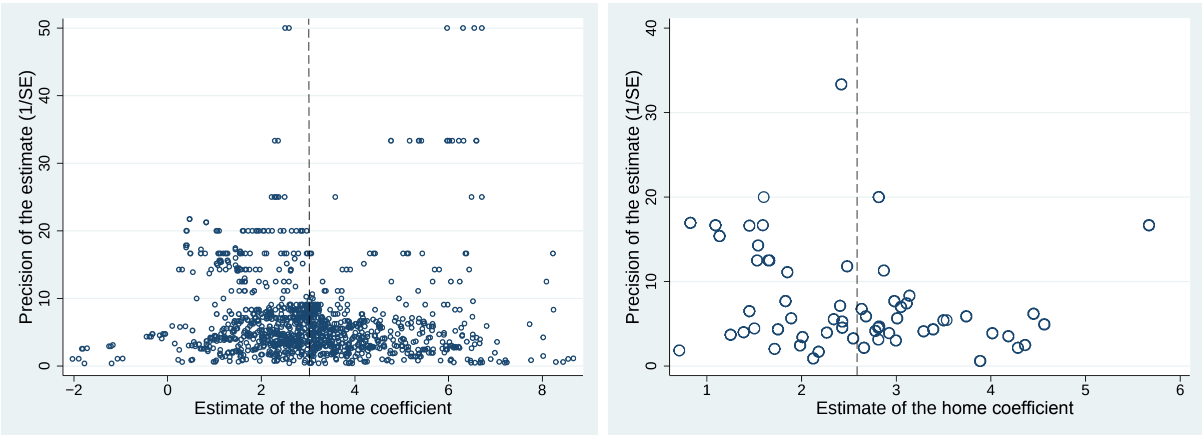
If researchers preferentially report estimates that are statistically significant and have the expected sign, the literature as a whole exaggerates the effect in question. For example, Stanley (2005) finds that the mean estimate of the price elasticity of water demand is exaggerated fourfold because of publication bias. The problem is widely recognized in medical science, and the best medical journals now require registration of clinical trials before publication, so that researchers can find the results of all trials, even though some are not submitted for publication. In a similar vein, the American Economic Association has agreed to establish a registry of randomized experiments "to counter publication bias" (Siegfried, 2012, p. 648).

The presence of publication bias can be examined visually using the so-called funnel plot (Egger *et al.*, 1997). It is a scatter plot showing the magnitude of the estimated effects on the horizontal axis and the precision (the inverse of the estimated standard error) on the vertical axis. If the literature is not influenced by publication bias, the most precise estimates of the effect will be close to the mean underlying effect. As the precision decreases, the estimates get more dispersed, forming a symmetrical inverted funnel. In the presence of publication bias the funnel becomes asymmetrical (if researchers discard estimates of a particular sign or magnitude), or hollow (if researchers discard statistically insignificant estimates), or both.

We report the funnel plot for the border effect literature in Figure 4. Panel (a) shows the funnel for all estimates; panel (b) only shows the median estimates for each study. We make three observations from the funnels. First, both funnels are relatively symmetrical, with the most precise estimates being close to the average reported semi-elasticity. Second, the funnels are not hollow, and even estimates with very little precision (and, thus, small p-values) are reported. Three, the funnel in panel (a) has multiple peaks, which suggests heterogeneity in the estimated border effects. Signs of heterogeneity are not surprising given our estimates of cross-country differences in the previous section. We conclude that typical funnel plots reported in economics meta-analyses show much clearer signs of publication bias than what we observe in the literature on border effects (see, for example, Stanley & Doucouliagos, 2010).

Figure 4: Funnel plots suggest little publication bias

(a) All estimates (b) Median estimates reported in studies




*Notes:* In the absence of publication bias the funnel should be symmetrical around the most precise estimates of the home coefficient (the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows). The dashed vertical lines denote the mean of all estimates in panel (a) and the mean of median estimates reported in studies in panel (b). Multiple peaks of the funnel suggest heterogeneity.

The funnel plot represents a simple visual tool for the evaluation of publication bias, but the presence of bias can be tested more formally. Following Card & Krueger (1995), we explore the relationship between the estimates of the semi-elasticity and their standard errors. Because researchers estimating the semi-elasticity assume that the estimates have a t-distribution, the reported semi-elasticities should be distributed approximately normally around the mean reported effect. In contrast, if statistically significant estimates are preferred, researchers will search for large estimates of the semi-elasticity in order to offset the standard errors and produce large t-statistics. Similarly, when researchers discard negative estimates, a positive relationship arises between the reported estimates and their standard errors because of heteroskedasticity (Stanley, 2008):

$$HOME_{ij} = HOME_0 + \beta \cdot SE(HOME_{ij}) + u_{ij}, \quad (2)$$

where $HOME_{ij}$ are estimates of the semi-elasticity, $SE(HOME_{ij})$ are the reported standard errors of the semi-elasticity estimates, $HOME_0$ is the mean semi-elasticity corrected for potential publication bias, $\beta$ measures the extent of publication bias, and $u_{ij}$ is a normal disturbance term. For example, if the true mean semi-elasticity was zero (implying no border effect) but all researchers reported the 5% of estimates that are positive and statistically significant, the estimated $HOME_0$ would be close to two: the researchers would need their t-statistics, $HOME/SE(HOME)$, to equal at least two.

Equation (2) can be interpreted as a test of funnel asymmetry, because it follows from rotating the axes of the funnel plot and inverting the values on the new horizontal axis to show standard errors instead of precision. Note that the test has low power if the true underlying value of the effect is close to zero and the only source of publication bias is selection for statistical significance: when $HOME_0$ is zero and insignificant estimates, positive or negative, are omitted, $\beta$ is zero, even though publication selection may be substantial (the funnel plot gets hollow, but not asymmetrical). Nevertheless, such a symmetrical selection does not create a bias in the mean of the reported estimates, so it is usually not a source of concern (Stanley, 2005).

We present the results of the funnel asymmetry tests in Table 2. Because (2) is heteroskedastic, we present robust standard errors, which are clustered at the level of individual studies and data sets. The first column of panel A shows estimates of the parameters from (2) using all 1,271 semi-elasticities in our sample. The coefficient corresponding to the extent of publication bias is statistically insignificant and close to zero, while the estimated semi-elasticity beyond publication bias is 2.9, close to the mean and median semi-elasticity reported in the literature. Therefore, neither visual nor formal tests show any evidence of publication selection, and the potential selection does not create any bias in the mean reported estimate of the border effect.

The second column of panel A in Table 2 estimates equation (2) using only the semi-elasticities reported in published studies. Perhaps editors or referees prefer large and statistically significant coefficients, which would pull the mean reported semi-elasticity up. Indeed, in a meta-analysis of vertical productivity spillovers from foreign direct investment, Havranek & Irsova (2011) find that studies published in refereed journals show substantially more publication bias than unpublished manuscripts. Our results concerning the border effect, however, show little difference between published and unpublished studies both in the extent of publication bias and in the mean underlying semi-elasticity beyond any potential bias.

Next, we include fixed effects for individual studies to control for method or other quality characteristics specific to individual studies. The fixed-effects estimation represents another

Table 2: Funnel asymmetry tests show no publication bias

| *Panel A: unweighted regressions* | All estimates | Published | Fixed effects | Instrument |
|---|---|---|---|---|
| SE (publication bias) | 0.604 | 0.599 | 0.383 | -0.797 |
| | (0.514) | (0.522) | (0.534) | (2.020) |
| Constant (effect beyond bias) | 2.852*** | 2.932*** | 2.918*** | 3.270*** |
| | (0.321) | (0.339) | (0.159) | (0.724) |
| Studies | 61 | 48 | 61 | 61 |
| Observations | 1,271 | 1,144 | 1,271 | 1,271 |
| *Panel B: weighted regressions* | Precision | Study | Impact | Citations |
| SE (publication bias) | 0.246 | 1.489 | 3.062 | 5.073 |
| | (1.964) | (1.170) | (2.024) | (4.272) |
| Constant (effect beyond bias) | 2.959*** | 2.204*** | 1.634*** | 1.235** |
| | (0.723) | (0.395) | (0.424) | (0.501) |
| Studies | 61 | 61 | 53 | 49 |
| Observations | 1,271 | 1,271 | 1,124 | 1,069 |

*Notes:* The table presents the results of regression $HOME_{ij} = HOME_0 + \beta \cdot SE(HOME_{ij}) + u_{ij}$. $HOME_{ij}$ and $SE(HOME_{ij})$ are the $i$-th estimates of the home coefficient (the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows) and their standard errors reported in the $j$-th studies. The standard errors of the regression parameters are clustered at both the study and data set level and shown in parentheses (the implementation of two-way clustering follows Cameron *et al.*, 2011). Published = we only include published studies. Fixed effects = we use study dummies. Instrument = we use the number of observations in the gravity equation as an instrument for the standard error. The regressions in Panel B are estimated by weighted least squares. Precision = we take the inverse of the reported estimate's standard error as the weight. Study = in addition to "Precision" the inverse of the number of estimates reported per study is taken as the weight. Impact = in addition to "Study" the RePEc recursive discounted impact factor of the outlet where the study was published is taken as the weight. Citations = in addition to "Impact" the number of Google Scholar citations received per year is taken as the weight. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

advantage of collecting multiple estimates per study. The results are very similar to the baseline specification reported in the first column; we get no evidence of publication bias, and the mean estimated semi-elasticity is still 2.9.

Specification (2) only includes one explanatory variable, the standard error. It is possible that some method choices affect both the estimated semi-elasticity and the corresponding standard error, which would cause the error term $u_{ij}$ to be correlated with $SE(HOME_{ij})$. In the last column of panel A in Table 2 we use the logarithm of the number of observations in the gravity equation as an instrument for $SE(HOME_{ij})$: the number of observations is correlated with the reported standard errors of the semi-elasticities, but little related to the methods of estimation. The instrumental variable estimation is less precise, but still reports the mean underlying semi-elasticity close to 3 and no evidence of publication bias.

In panel B of Table 2 we weight all the estimates by the precision. We have noted that equation (2) is heteroskedastic, and the explanatory variable directly captures the variance of the response variable. To achieve efficiency, many applications of meta-analysis divide (2) by the corresponding standard error, that is, they multiply the equation by the precision of the estimates. Such an approach has the additional allure of giving more importance to precise results. The first column of panel B shows that weighting by precision has little impact on our results.

The second column of panel B adds weighting by the inverse of the number of estimates reported in studies to the precision weights. In line with the summary statistics from the previous section, the mean semi-elasticity decreases when each study gets the same weight. Next, in column 3 we add weighting by the discounted recursive RePEc impact factor of the publication outlet. The estimated semi-elasticity decreases to 1.6: better journals seem to publish smaller estimates, which corroborates our interpretation of Figure 3. Finally, we also weight the estimates by the number of Google Scholar citations the study receives each year. The semi-elasticity decreases to 1.3, implying a border effect of 3.4. Thus, when we give more weight to highly-cited papers published in good journals, we are able to shrink the mean border effect more than five times. In the next section we explore how these differences between studies can be explained by variation in data and methodology.

# 4 Why Border Effects Vary

## 4.1 Variables and Estimation

We substitute the characteristics of estimates and studies for $SE(HOME_{ij})$ in equation (2). The previous section shows that the reported standard errors are not correlated with the estimates of the semi-elasticity, and the exclusion of the standard error has the additional benefit of removing the obvious heteroskedasticity. After we remove the standard error from the equation, we have little to gain by weighting our estimates by precision. Moreover, weighting by the estimates' precision introduces artificial variation into variables defined at the study level (for example, the use of disaggregated or panel data). Instead, we weight the regressions by the inverse of the number of estimates reported per study to give each study the same weight, and also report a robustness check using unweighted data.

Table 3 lists all the variables that we collect from primary studies, explains their definition, and shows summary statistics. The last column presents the mean weighted by the inverse of the number of estimates reported in each study. We divide the variables into seven groups.

First, we collect information on data characteristics. Second, we control for regional differences in the estimates. Three, we collect variables reflecting the general design of the analysis. Four, we include dummy variables that capture how the authors treat multilateral resistance. Five, we distinguish between the different types of treatment of zero trade flows. Six, we include dummy variables reflecting whether the gravity equation uses control variables. Finally, we include information on publication and citation characteristics of the studies. Our intention is to introduce the possible reasons for heterogeneity in the estimated border effects, not to present a detailed survey of the methods used in estimating the gravity equation. For a survey of methods see Head & Mayer (2014).

Table 3: Description and summary statistics of regression variables

| Variable | Description | Mean | SD | WM |
|---|---|---|---|---|
| Home | The coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows (or minus the coefficient on the dummy variable that equals one for cross-border flows). | 3.03 | 1.60 | 2.59 |
| SE | The estimated standard error of *home*. | 0.30 | 0.35 | 0.26 |
| *Data characteristics* | | | | |
| Mid-year of data | The midpoint of the sample on which the gravity equation is estimated (base is the sample minimum: 1899). | 91.3 | 16.0 | 91.7 |
| Panel data | = 1 if panel data are used in the gravity equation. | 0.67 | 0.47 | 0.52 |
| Disaggregated | = 1 if trade flows are disaggregated at the sector or product level. | 0.57 | 0.50 | 0.41 |
| Obs. per year | The logarithm of the number of observations per year included in the gravity equation. | 6.89 | 1.31 | 6.93 |
| No. of years | The logarithm of the number of years in the data. | 1.27 | 1.04 | 0.91 |
| *Countries examined* | | | | |
| Canada | =1 if the border effect is estimated for Canada. | 0.17 | 0.37 | 0.18 |
| US | =1 if the border effect is estimated for the US. | 0.05 | 0.22 | 0.08 |
| EU | =1 if the border effect is estimated for the EU. | 0.21 | 0.41 | 0.23 |
| OECD | =1 if the border effect is estimated for OECD countries. | 0.08 | 0.27 | 0.06 |
| Emerging | =1 if the effect is estimated for developing or transition countries. | 0.06 | 0.25 | 0.05 |
| *Design of the analysis* | | | | |
| No internal trade | =1 if within-country trade flows are not observed but estimated using production data. | 0.58 | 0.49 | 0.43 |
| Inconsistent dist. | =1 if within-country distance is measured differently from between-country distance. | 0.14 | 0.35 | 0.21 |
| Actual distance | =1 if actual distance traveled by road or sea is used instead of the great-circle formula. | 0.06 | 0.24 | 0.07 |
| Total trade | =1 if total trade is used as the dependent variable and imports and exports are summed before taking logs. | 0.01 | 0.12 | 0.01 |
| Asymmetry | =1 if the estimate measures the difficulty of cross-border flows in one direction. | 0.29 | 0.45 | 0.14 |
| Instruments | =1 if instruments are used to correct for the endogeneity of GDP. | 0.06 | 0.25 | 0.06 |
| *Treatment of multilateral resistance* | | | | |

Continued on next page

Table 3: Description and summary statistics of regression variables (continued)

| Variable | Description | Mean | SD | WM |
|---|---|---|---|---|
| Remoteness | =1 if remoteness terms are included. | 0.06 | 0.24 | 0.10 |
| Country fixed eff. | =1 if destination and origin fixed effects are included. | 0.27 | 0.44 | 0.31 |
| Ratio estimation | =1 if trade flows are normalized by trade with self. | 0.31 | 0.46 | 0.11 |
| Anderson est. | =1 if the non-linear estimation method developed by Anderson & van Wincoop (2003) is used. | 0.02 | 0.15 | 0.06 |
| No control for MR | =1 if the gravity equation does not account for multilateral resistance terms. | 0.38 | 0.49 | 0.50 |
| *Treatment of zero trade flows* | | | | |
| Zero plus one | =1 if one is added to observations of zero trade flows. | 0.11 | 0.32 | 0.13 |
| Tobit | =1 if the gravity equation is estimated by the Tobit model. | 0.06 | 0.24 | 0.06 |
| PPML | =1 if the gravity equation is estimated by the Poisson pseudo-maximum likelihood estimator. | 0.07 | 0.26 | 0.11 |
| Zeros omitted | =1 if observations of zero trade flows are omitted. | 0.66 | 0.47 | 0.55 |
| *Control variables* | | | | |
| Adjacency control | = 1 if the gravity equation controls for adjacency. | 0.63 | 0.48 | 0.50 |
| Language control | = 1 if the gravity equation controls for shared language (when needed). | 0.78 | 0.42 | 0.73 |
| FTA control | = 1 if the gravity equation controls for free trade agreements (when needed). | 0.73 | 0.44 | 0.76 |
| *Publication characteristics* | | | | |
| Published | = 1 if the study is published in a peer-reviewed journal. | 0.90 | 0.30 | 0.79 |
| Impact | The recursive discounted RePEc impact factor of the outlet (collected in January 2014). | 0.46 | 0.90 | 0.45 |
| Citations | The logarithm of the mean number of Google Scholar citations received per year since the study appeared in Google Scholar (collected in January 2014). | 1.52 | 1.13 | 1.60 |
| Publication year | The year when the study first appeared in Google Scholar (base: 1995). | 9.46 | 4.32 | 9.62 |

*Notes:* SD = standard deviation. WM = mean weighted by the inverse of the number of estimates reported per study. All variables except for citations and the impact factor are collected from studies estimating the border effect (the search for studies was terminated on January 1, 2014, and the list of studies is available in Appendix B). Citations are collected from Google Scholar and the impact factor from RePEc. The data set is available in the online appendix at `meta-analysis.cz/border`.

**Data characteristics** We control for the age of the data by creating a variable that reflects the midpoint of the sample; perhaps the mean border effect shrinks with the continuing globalization and integration of emerging markets. The mean semi-elasticity in our sample is estimated using data from 1990. To see whether cross-sectional and panel data yield systematically different border effects, we include a corresponding dummy variable. Sixty-seven per cent of the estimates come from specifications using panel data, but 48% of the studies rely on cross-sectional data (that is, panel studies usually report more estimates).

Next, we control for the level of aggregation in the gravity equation and add a dummy that equals one if the data are disaggregated at the sector or product level; about a half of all studies employ some sort of disaggregation. Researchers suspect that aggregation across products and sectors creates a bias in the gravity equation, but the direction of the bias is unclear (Anderson & van Wincoop, 2004, pp. 727–729). We also include the logarithm of the number of observations per year used in the gravity equation and the logarithm of the number of years in the panel. The mean semi-elasticity in our sample is computed using 3 years of data and 1,000 estimates per year.

**Countries examined** Border effects in our sample are estimated for different regions, so we control for regional differences. Among other things, countries may display different elasticities of substitution between domestic and foreign goods, which would affect the estimated border effect. We include five regional dummies: Canada, the US, the EU, the OECD, and emerging countries (including both developing and transition economies). The first paper on the border effect, McCallum (1995), uses data on internal trade in Canada. Many others have followed, and 17% of all estimates in our sample use Canadian data. Another 5% of border effects are estimated for the US (for example, Anderson & van Wincoop, 2003), 21% for the EU (Nitsch, 2000), 8% for the OECD (Wei, 1996), and 6% for emerging countries (da Silva *et al.*, 2007). The remaining reported elasticities are estimated for other individual OECD countries or use combinations of internal trade flows for different regions.

**Design of the analysis** We distinguish studies that have data on within-country trade flows from studies that estimate trade with self using production data; about a half of the studies have access to data on internal trade. Regarding the studies that must compute data on trade with self, we distinguish between those that use the same definition for the computation of within and between-country distance and those that employ different definitions. Head & Mayer (2010) show that employing inconsistent measures of internal distance can exaggerate the reported border effect. About 14% of all estimates are obtained using different definitions of internal and external distance.

We also include a dummy variable that equals one for estimates obtained with a measure of distance computed from actual road or sea routes instead of the great-circle formula (6% of all estimates). We expect that the great-circle formula overstates internal distance and thus leads to an upward bias in the estimated border effect. Regions are likely to be connected more efficiently with other regions in the same country than with foreign regions that show the same great-circle distance (Braconier & Pisu, 2013). A couple of studies in our data set commit what Baldwin & Taglioni (2007) call the "silver medal mistake" in estimating the gravity equation: they use total or average trade flows as the response variable and compute the sum or average before taking logs. About 14% of studies use an asymmetric definition of border effects, which means that they examine the difficulty of crossing borders in one direction (for example, Anderson & Smith, 1999). Finally, we control for the case where researchers use instruments to account for the endogeneity of GDP in the gravity equation (6% of all estimates).

**Treatment of multilateral resistance** We include five dummy variables to control for the way the authors of primary studies account for the problem. The first attempts, usually prior to Anderson & van Wincoop (2003), involve including remoteness terms, and about 10% of studies in our sample do so. The most straightforward approach is to use destination and origin fixed effects (Feenstra, 2002), employed by 31% of studies. Another consistent estimation method involves normalizing trade flows by trade with self (Head & Mayer, 2000), and 11% of studies use this method. About 6% of studies use the non-linear technique introduced by Anderson & van Wincoop (2003). A half of the primary studies do not estimate the border effect consistently; that is, they either add the atheoretical remoteness terms or ignore multilateral resistance entirely.

**Treatment of zero trade flows** The simplest way to incorporate zeros is to add one to each observation and use the log-linear transformation. But as Head & Mayer (2014) note, in this case the results depend on the units of measurement. Many authors who choose this approach estimate the gravity equation using Tobit (6% of the studies). Next, 11% of primary studies use the non-linear method introduced by Silva & Tenreyro (2006), the Poisson pseudo-maximum likelihood estimator (PPML). The method allows for the incorporation of zero trade flows and addresses heteroskedasticity in the error term of the gravity equation. Finally, 55% of studies exclude zeros from their data sets. Some studies, especially those using aggregated OECD data, do not face the problem because they have no zero trade flows in their data.

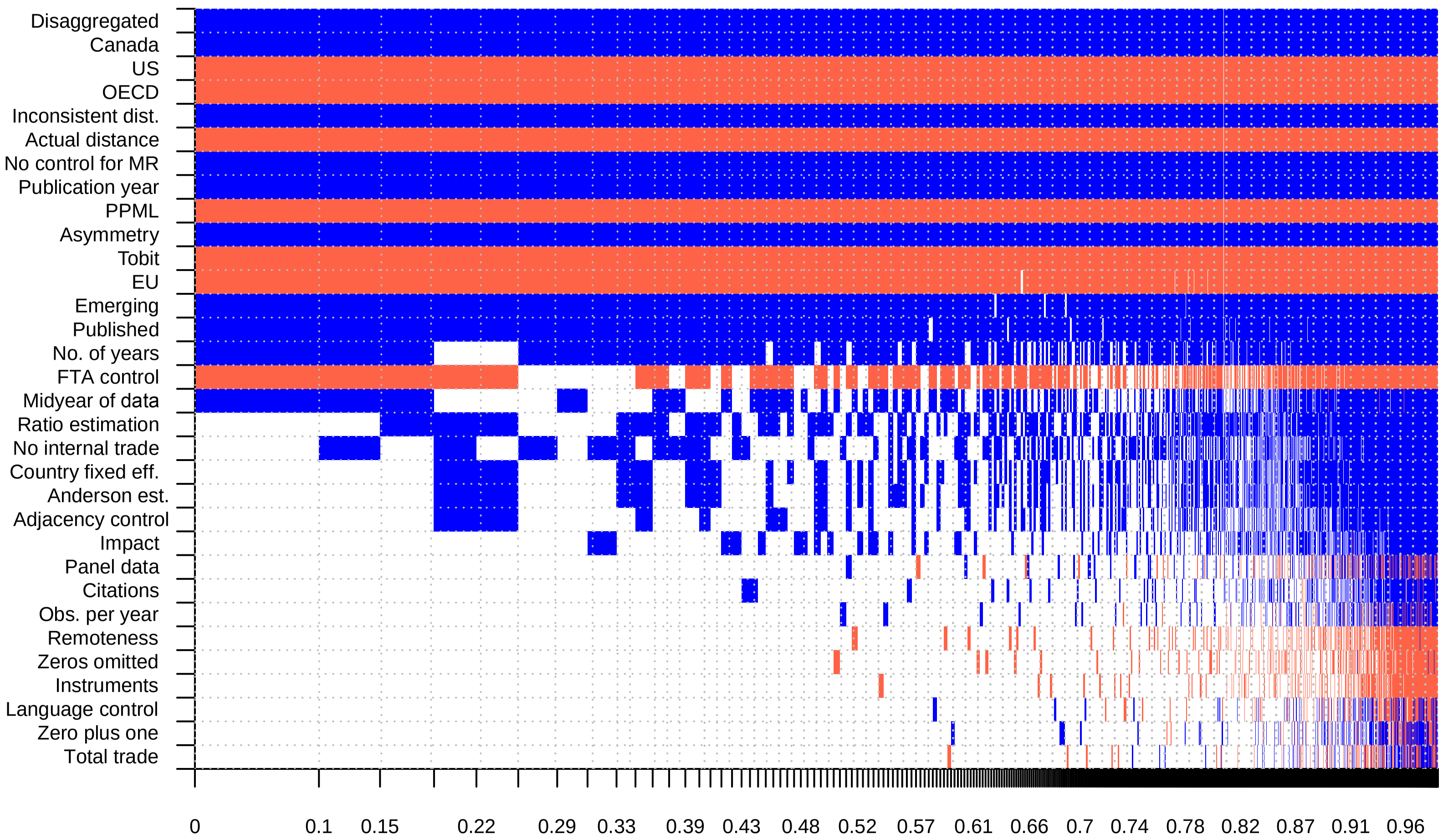
**Control variables** Studies estimating the border effect typically include three control variables: dummies for adjacency, common language, and membership in a free trade agreement. We examine whether the inclusion of these variables has a systematic influence on the estimated semi-elasticity. In many cases the primary studies cannot include the dummy variables for common language and free trade area membership, because the value of these dummies would be the same for all trading pairs in their data—for example, trade flows between Canadian provinces and US states. We code the variables such that "0" for common language and FTA control means that the control variable could be included but is omitted.

**Publication characteristics** To see whether published studies yield different results even after all the main aspects of methodology are controlled for, we include a dummy variable that equals one if the study is published in a peer-reviewed journal. To account for the different quality of publication outlets, we include the recursive discounted RePEc impact factor. The greatest advantage of RePEc with respect to other impact metrics is that it provides information on virtually all journals and working paper series. Next, we control for the number of citations of the study, which could reflect aspects of study quality not captured by the data and methodology variables described above. Finally, for each study we find the year when it first appeared in Google Scholar and examine whether there is a publication trend in the estimates of the border effect beyond advances in methodology.

We intend to run a regression with the semi-elasticity as the response variable and all the aspects of data, methodology, and publication as explanatory variables. The problem is that such a regression would probably contain many redundant variables, and we do not know a priori which of the variables introduced in Table 3 should be excluded. Ideally, we would also like to run regressions containing different subsets of the explanatory variables to see whether our results are robust. We face model uncertainty, which can be addressed by Bayesian model averaging (BMA).

BMA runs many regressions involving different subsets of the 32 potential explanatory variables. With $2^{32}$ possible combinations, it would take several months to estimate all the regressions, so our approach relies on a Monte Carlo Markov Chain algorithm that walks through the potential models (we use the `bms` R package by Feldkircher & Zeugner, 2009). For each model BMA computes a weight, called the posterior model probability, which is analogous to information criteria or adjusted R-squared and captures how well the model fits the data. The regression coefficients reported by BMA are weighted averages of the many estimated models; instead of standard errors, BMA reports posterior standard deviations reflecting the distribu-

Figure 5: Model inclusion in Bayesian model averaging




*Notes:* Response variable: estimate of the home coefficient (the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows). All regressions are weighted by the inverse of the number of estimates reported per study. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model. The horizontal axis measures cumulative posterior model probabilities. Numerical results of the BMA estimation are reported in Table 4. A detailed description of all variables is available in Table 3.

tion of the regression parameters retrieved from the individual models. For each variable we compute the posterior inclusion probability, which is the sum of the posterior model probabilities of the regressions in which the variable is included. The posterior inclusion probability reflects how likely it is that the variable should be included in the true model. Diagnostics of our BMA exercise are available in Appendix A. More details on BMA in general can be found, for example, in Raftery *et al.* (1997) or Eicher *et al.* (2011).

### 4.2 Results

Figure 5 reports our results concerning the model inclusion of different explanatory variables in the BMA exercise. The columns in the figure show the different regression models, and the width of the columns denotes the posterior model probability. The rows show the individual variables sorted by posterior inclusion probability in descending order. If the cell corresponding to a variable is empty, it means that the variable is not included in the model. Blue color (darker in grayscale) means that the variable is included and the estimated sign of the regression parameter is positive. Red color (lighter in grayscale) denotes a negative estimated regression parameter. We can see that approximately a half of the variables appear in the best models and that the signs of their estimated regression parameters are robust to including other control variables.

The numerical results of Bayesian model averaging are reported in Table 4. In addition, we show the results of an OLS regression which includes all but the 11 variables with a posterior inclusion probability lower than 0.3: these 11 variables do not help explain the variability in the estimates of the border effect. The OLS estimation produces results consistent with those of BMA. The estimated signs of the regression parameters are the same and variables with high posterior inclusion probability in BMA are usually statistically significant in the OLS estimation. Also, the estimated magnitudes of the regression parameters are similar in the two methods for the most important variables, that is, those with high posterior inclusion probabilities. When interpreting the posterior inclusion probability, we follow the approach of Eicher *et al.* (2011), who consider a value to be *weak* if it is between 0.5 and 0.75, *substantial* if it is between 0.75 and 0.95, strong if it is between 0.95 and 0.99, and *decisive* if it exceeds 0.99.

Some of the data characteristics systematically affect the reported estimates of the border effect. Researchers using disaggregated data tend to obtain estimates of the semi-elasticity 0.8

Table 4: Explaining the differences in the estimates of the border effect

| Response variable: | Bayesian model averaging | | | Frequentist check (OLS) | | |
|---|---|---|---|---|---|---|
| Estimate of Home | Post. mean | Post. SD | PIP | Coef. | Std. er. | p-value |
| *Data characteristics* | | | | | | |
| Mid-year of data | 0.003 | 0.004 | 0.542 | 0.001 | 0.011 | 0.915 |
| Panel data | 0.004 | 0.055 | 0.068 | | | |
| Disaggregated | 0.800 | 0.138 | 1.000 | 0.654 | 0.359 | 0.069 |
| Obs. per year | 0.001 | 0.008 | 0.048 | | | |
| No. of years | 0.136 | 0.079 | 0.811 | 0.147 | 0.107 | 0.170 |
| *Countries examined* | | | | | | |
| Canada | 0.718 | 0.126 | 1.000 | 0.741 | 0.322 | 0.021 |
| US | -1.177 | 0.134 | 1.000 | -1.135 | 0.239 | 0.000 |
| EU | -0.518 | 0.165 | 0.992 | -0.639 | 0.391 | 0.102 |
| OECD | -0.981 | 0.176 | 1.000 | -0.958 | 0.356 | 0.007 |
| Emerging | 0.947 | 0.267 | 0.990 | 0.808 | 0.388 | 0.037 |
| *Design of the analysis* | | | | | | |
| No internal trade | 0.166 | 0.210 | 0.441 | 0.491 | 0.404 | 0.224 |
| Inconsistent dist. | 0.783 | 0.142 | 1.000 | 0.514 | 0.302 | 0.089 |
| Actual distance | -0.933 | 0.153 | 1.000 | -0.666 | 0.313 | 0.033 |
| Total trade | 0.000 | 0.049 | 0.025 | | | |
| Asymmetry | 0.536 | 0.121 | 0.999 | 0.540 | 0.246 | 0.028 |
| Instruments | -0.005 | 0.043 | 0.035 | | | |
| *Treatment of multilateral resistance* | | | | | | |
| Remoteness | -0.007 | 0.045 | 0.048 | | | |
| Country fixed eff. | 0.213 | 0.311 | 0.368 | 0.220 | 0.305 | 0.471 |
| Ratio estimation | 0.402 | 0.475 | 0.520 | 0.602 | 0.584 | 0.303 |
| Anderson est. | 0.229 | 0.347 | 0.350 | 0.079 | 0.353 | 0.822 |
| No control for MR | 0.826 | 0.299 | 1.000 | 0.719 | 0.308 | 0.019 |
| *Treatment of zero trade flows* | | | | | | |
| Zero plus one | 0.001 | 0.023 | 0.029 | | | |
| Tobit | -0.636 | 0.156 | 0.996 | -0.553 | 0.312 | 0.077 |
| PPML | -0.707 | 0.154 | 1.000 | -0.774 | 0.493 | 0.117 |
| Zeros omitted | -0.004 | 0.026 | 0.042 | | | |
| *Control variables* | | | | | | |
| Adjacency control | 0.071 | 0.136 | 0.258 | | | |
| Language control | -0.001 | 0.018 | 0.030 | | | |
| FTA control | -0.213 | 0.177 | 0.661 | -0.366 | 0.347 | 0.292 |
| *Publication characteristics* | | | | | | |
| Published | 0.339 | 0.108 | 0.976 | 0.330 | 0.265 | 0.212 |
| Impact | 0.018 | 0.044 | 0.183 | | | |
| Citations | 0.003 | 0.014 | 0.063 | | | |
| Publication year | 0.075 | 0.012 | 1.000 | 0.058 | 0.031 | 0.062 |
| Constant | 0.087 | NA | 1.000 | 0.922 | 1.058 | 0.383 |
| Studies | 61 | | | 61 | | |
| Observations | 1,271 | | | 1,271 | | |

*Notes:* Home = the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows. PIP = posterior inclusion probability. SD = standard deviation. In the frequentist check we only include explanatory variables with PIP > 0.3. The standard errors in the frequentist check are clustered at both the study and data set level (the implementation of two-way clustering follows Cameron *et al.*, 2011). More details on the BMA estimation are available in Table A1 and Figure A1. A detailed description of all variables is available in Table 3.

larger; the posterior inclusion probability of this variable is decisive. The result corroborates the findings of Anderson & Yotov (2010, p. 2167), who also report that aggregated data yield smaller estimates of the border effect. In contrast, Hillberry (2002) finds that aggregation exaggerates the border effect. Next, more years of data available for the estimation translates into larger border effects, but the posterior inclusion probability of this variable is only 0.81. For all other variables in this category we get weak posterior inclusion probabilities.

Regional differences help explain the heterogeneity in the estimated border effects; the posterior inclusion probabilities for all the region dummies are decisive. Researchers typically obtain the largest border effects for developing and transition countries, followed by Canada. The smallest estimates are reported for the US. Balistreri & Hillberry (2007) discuss how the small estimates for the US may be affected by the characteristics of the Commodity Flow Survey, the source of data typically used for this estimation.

Regarding the general design of the gravity equation, it matters for the estimated border effect whether internal and external distances are measured consistently. If not, the reported semi-elasticities tend to be about 0.8 larger; the result is in line with the findings of Head & Mayer (2010). When the authors of primary studies use actual road or sea distances instead of employing the great-circle formula, they report much smaller estimates of the semi-elasticity (0.9 smaller). Braconier & Pisu (2013) also find that using the actual distance reduces the estimated border effect. Next, asymmetric estimates of the border effect are on average larger than those using the symmetric definition. The border effects estimated using "trade with self" computed from production statistics differ little from the estimates obtained when data on within-country trade are directly available. It seems that the "silver medal mistake" in estimation does not affect the resulting border effects, but very few papers in our data set commit this mistake.

In contrast, the "gold medal mistake" in estimating gravity equations has important consequences for the border effect: if authors do not control for multilateral resistance terms, they are likely to report semi-elasticities 0.8 larger. This result contrasts with the findings of Balistreri & Hillberry (2007), who report that the decrease in border effects found by Anderson & van Wincoop (2003) is primarily due to the specifics of the data and not due to the control for multilateral resistance. The posterior inclusion probabilities for the specific types of control for multilateral resistance are weak: when estimating the border effect, it is important to control for multilateral resistance, but the exact methods used seems to matter little.

The treatment of zero trade flows affects the estimated border effect as well. If Tobit or PPML is used, the resulting semi-elasticities are about 0.7 smaller. In contrast, the inclusion of control variables does not seem to matter much for border effects. Concerning publication and other study characteristics, papers published in refereed journals tend to report semi-elasticities about 0.3 larger. The impact factor of the journal and the number of citations are not important for the reported border effects when we control for the characteristics of data and methods. The reported border effects seem to increase slightly over time: the semi-elasticities are 0.075 larger on average each year.

In the next step we try to piece the puzzle together by computing a mean estimate of the border effect conditional on avoiding the gold medal, silver medal, or any other potential mistake in estimation. This part of our analysis is the most subjective, because it involves defining "best practice" in the estimation of border effects, and different researchers may have different opinions on what the best practice is. Nevertheless, we show that the major innovations introduced into the estimation of gravity equations in the last decade substantially alleviate the border puzzle, and seem to solve it at least for some regions.

For each variable in Table 4 we select a preferred value, or a sample mean if we have no preference, and compute the implied semi-elasticity as a linear combination of all the regression parameters. In other words, we construct a synthetic study with a large number of observations, the best practice methodology, and the maximum number of citations and other publication characteristics. We select sample maxima for the mid-year of the data (that is, we put an emphasis on studies using recent data), panel data, disaggregated data, the number of observations per year, the number of years in the data, actual distance, PPML, the inclusion of control variables, publication in a refereed journal, the impact factor, and the number of citations. We plug in sample minima for the dummy variable corresponding to unavailability of within-country data, inconsistent measurement of internal and external distance, summing trade flows before taking logs, estimating an asymmetric border effect, adding remoteness terms, disregarding multilateral resistance, adding one to zero trade flows and using Tobit for estimation, and disregarding zero trade flows. All other variables are set to their sample means.

Table 5 presents the results; the overall mean semi-elasticity is reported in the last row and

Table 5: Advances in methodology shrink the border effect

| | Weighted | | | | Unweighted | | | |
|---|---|---|---|---|---|---|---|---|
| *Best practice* | Estimate | 95% conf. int. | | Diff. | Estimate | 95% conf. int. | | Diff. |
| Canada | 1.95 | 1.09 | 2.81 | -0.86 | 2.14 | 0.80 | 3.49 | -0.72 |
| US | 0.06 | -1.02 | 1.13 | -1.30 | -0.51 | -1.73 | 0.71 | -1.23 |
| EU | 0.72 | -0.62 | 2.05 | -1.87 | -0.17 | -1.60 | 1.25 | -2.72 |
| OECD | 0.25 | -1.12 | 1.62 | -2.16 | 0.08 | -1.40 | 1.55 | -2.27 |
| Emerging | 2.18 | 0.67 | 3.69 | -1.96 | 2.02 | 0.62 | 3.41 | -3.03 |
| All countries | 1.13 | 0.04 | 2.23 | -1.46 | 0.93 | -0.43 | 2.29 | -2.10 |

*Notes:* The table presents estimates of the home coefficient for selected countries and country groups implied by Bayesian model averaging and our definition of best practice. That is, we take the regression coefficients estimated by BMA and construct fitted values of *home* conditional on control for multilateral resistance, consistent measurement of within and between-country distance, and other aspects of methods and data (see the text for details). Diff. = the difference between these estimates and the simple means reported in Table 1. The confidence intervals are approximate and constructed using the standard errors estimated by OLS. The right-hand part of the table presents results based on the robustness check using unweighted regressions (Table 7).

region-specific estimates in the remaining rows. The column labeled "Diff." shows the difference between our new estimates and the simple means reported in Table 1. The left-hand part of the table shows the baseline results constructed from Table 4; the right-hand part is based on regressions not weighted by the inverse of the number of estimates reported per study (Table 7). The two sets of results are qualitatively similar, but the unweighted specification yields smaller estimates for all regions except Canada, and even reports negative semi-elasticities for the US and EU. We focus on the results obtained from the weighted regressions.

From Table 5 we see that giving more weight to studies that correct for the traditional problems in gravity equations and use novel methods decreases the estimated semi-elasticities significantly for each region. (The difference would be even larger if we plugged in sample means for publication characteristics and the number of observations and years in the data instead of giving more weight to large, broadly cited studies published in good journals.) The overall mean semi-elasticity is 1.1, which translates into a border effect of 3.1—almost seven times smaller than the border effect based on the sample mean of the semi-elasticities reported in the literature. The border effect for the US and OECD countries is negligible: only $\exp(0.06) = 1.06$ and $\exp(0.25) = 1.28$; in contrast, the effect is still substantial for emerging countries: $\exp(2.18) = 8.85$. Regions in emerging countries tend to trade almost nine times more with regions in the same country than with similar foreign regions.

To put these numbers into perspective, we compute the ad-valorem tariff equivalent of the border effect. The tariff equivalent can be expressed as $\exp(\text{home}/\text{trade costs elasticity}) - 1$, so we need an estimate of the elasticity of trade with respect to trade costs. We use the survey of Head & Mayer (2014), who find a median elasticity of 5.03 estimated in studies controlling for multilateral resistance and using tariff variation to identify the elasticity. For an average region the tariff equivalent is $\exp(1.13/5.03) - 1 = 25\%$. For OECD countries the tariff equivalent of border barriers falls to 5.2%, which is less than a half of the mean tariff equivalent of core non-tariff barriers to trade of 12% estimated by Kee *et al.* (2009). In contrast, our estimates of the border effect for emerging countries suggest a high tariff equivalent of 54%.

One of the main points of Anderson & van Wincoop (2003) is that the general equilibrium trade impact of borders, which takes into account price index, wage, and GDP changes in response to changes in trade costs, is smaller than the partial equilibrium impact reflected in the coefficient estimated in the gravity equation. We approximate the general equilibrium effect using our estimate of the partial equilibrium effect and the approach based on exact hat algebra (Dekle *et al.*, 2007) described in Head & Mayer (2014, pp. 167–170, who also provide a Stata code for the computation). Employing the data provided by Head & Mayer (2014) on bilateral trade flows of 84 countries for which values of internal trade can be computed, we obtain a general equilibrium border effect of 2.15 for regions in the same country and 0.72 for regions across borders. That is, our results suggest that for an average country borders reduce international trade by 28% and increase within-country trade by 115%.

## 5 Robustness Checks

We present two additional sets of results. First, we use alternative priors for Bayesian model averaging. Second, we employ unweighted regressions. We show that the results are similar to the baseline in terms of the estimated effects of the different aspects of study design on the estimated semi-elasticities, and that the resulting "best practice" estimates of the border effect are close to those reported in the previous section.

In the baseline specification we use the unit information prior for Zellner's g-prior, which means that the prior (each regression coefficient equals zero) provides the same amount of information as one observation in the data set. Because we have 1,271 observations, the prior does not drive the posterior results. The second important choice is the model prior, which determines the prior probability of each model. In the baseline specification we employ the uniform model prior, which gives each model the same prior probability. Eicher *et al.* (2011) shows that these intuitive priors yield the best predictive performance. Nevertheless, there are obviously many other ways of choosing the priors, and the choice could influence our results.

The disadvantage of the uniform model prior is that it gives more weight to models with the mean number of variables, which is $32/2 = 16$ in our case. Such models appear most frequently among the subsets of all the $2^{32}$ possible models. Nevertheless, the true model may only contain a few variables, so the emphasis on large models may be counterproductive. An alternative is the beta-binomial prior advocated by Ley & Steel (2009), which gives the same prior probability to each *model size*, and thus does not prefer large models. An often-used alternative to the unit information prior is the BRIC g-prior (for example, Fernandez *et al.*, 2001).

Table 6 summarizes the results of Bayesian model averaging with the alternative priors; we provide more details and diagnostics in Table A2 and Figure A2 in Appendix A. The results are very similar to our baseline specification concerning the estimated posterior inclusion probabilities for the explanatory variables, the signs of the regression coefficients, and their magnitude. The semi-elasticity conditional on best practice is 1.02, implying a partial equilibrium border effect of 2.8, slightly below the estimate presented in the last section. The region-specific semi-elasticities are also similar: 1.85 for Canada, -0.06 for the US, 0.60 for the EU, 0.15 for the OECD, and 1.99 for emerging countries.

The second robustness check involves unweighted regressions, which means that studies presenting many estimates wield more influence in the meta-analysis. Table 7 shows that the posterior inclusion probabilities differ from the baseline specification for some variables. Concerning data characteristics, the age of the data seems to be important: the reported semi-elasticity decreases each year by about 0.025. Studies that do not have direct data on within-country trade flows report larger estimates of the border effect. Adding one to zero trade flows typically yields lower semi-elasticities (by about 0.7). Moreover, the impact factor of the journal and the number of citations of the study seem to be important: better journals tend to report smaller estimates, while broadly cited studies usually report larger estimates. Nevertheless, the best practice estimates of the border effect for the entire world and for individual regions are again very close to our baseline results, as shown in the right-hand part of Table 5. The overall mean semi-elasticity is 0.93, implying a partial equilibrium border effect of 2.5.

Table 6: Robustness check—alternative priors for BMA

| Response variable: | Bayesian model averaging | | | Frequentist check (OLS) | | |
|---|---|---|---|---|---|---|
| Estimate of Home | Post. mean | Post. SD | PIP | Coef. | Std. er. | p-value |
| *Data characteristics* | | | | | | |
| Mid-year of data | 0.003 | 0.003 | 0.466 | -0.001 | 0.012 | 0.926 |
| Panel data | 0.004 | 0.062 | 0.102 | | | |
| Disaggregated | 0.745 | 0.143 | 1.000 | 0.545 | 0.306 | 0.075 |
| Obs. per year | 0.000 | 0.008 | 0.060 | | | |
| No. of years | 0.113 | 0.082 | 0.738 | 0.100 | 0.098 | 0.310 |
| *Countries examined* | | | | | | |
| Canada | 0.724 | 0.126 | 1.000 | 0.823 | 0.317 | 0.010 |
| US | -1.183 | 0.133 | 1.000 | -1.131 | 0.227 | 0.000 |
| EU | -0.518 | 0.161 | 0.995 | -0.548 | 0.383 | 0.152 |
| OECD | -0.975 | 0.176 | 1.000 | -0.902 | 0.343 | 0.009 |
| Emerging | 0.868 | 0.268 | 0.990 | 0.602 | 0.322 | 0.062 |
| *Design of the analysis* | | | | | | |
| No internal trade | 0.184 | 0.209 | 0.508 | 0.361 | 0.389 | 0.354 |
| Inconsistent dist. | 0.754 | 0.145 | 1.000 | 0.521 | 0.304 | 0.087 |
| Actual distance | -0.907 | 0.155 | 1.000 | -0.716 | 0.331 | 0.030 |
| Total trade | -0.001 | 0.062 | 0.041 | | | |
| Asymmetry | 0.518 | 0.121 | 0.999 | 0.492 | 0.246 | 0.045 |
| Instruments | -0.008 | 0.054 | 0.055 | | | |
| *Treatment of multilateral resistance* | | | | | | |
| Remoteness | -0.016 | 0.066 | 0.090 | | | |
| Country fixed eff. | 0.362 | 0.334 | 0.601 | 0.214 | 0.272 | 0.431 |
| Ratio estimation | 0.628 | 0.491 | 0.721 | 0.738 | 0.506 | 0.145 |
| Anderson est. | 0.389 | 0.376 | 0.579 | 0.162 | 0.308 | 0.599 |
| No control for MR | 0.961 | 0.314 | 1.000 | 0.641 | 0.297 | 0.031 |
| *Treatment of zero trade flows* | | | | | | |
| Zero plus one | 0.004 | 0.033 | 0.050 | | | |
| Tobit | -0.640 | 0.155 | 0.998 | -0.600 | 0.321 | 0.062 |
| PPML | -0.726 | 0.155 | 1.000 | -0.860 | 0.529 | 0.104 |
| Zeros omitted | -0.007 | 0.035 | 0.074 | | | |
| *Control variables* | | | | | | |
| Adjacency control | 0.125 | 0.156 | 0.453 | 0.341 | 0.245 | 0.163 |
| Language control | -0.001 | 0.022 | 0.046 | | | |
| FTA control | -0.253 | 0.167 | 0.778 | -0.466 | 0.321 | 0.147 |
| *Publication characteristics* | | | | | | |
| Published | 0.346 | 0.103 | 0.986 | 0.276 | 0.272 | 0.311 |
| Impact | 0.021 | 0.045 | 0.230 | | | |
| Citations | 0.003 | 0.014 | 0.077 | | | |
| Publication year | 0.074 | 0.011 | 1.000 | 0.055 | 0.032 | 0.083 |
| Constant | 0.081 | NA | 1.000 | 1.267 | 1.135 | 0.264 |
| Studies | 61 | | | 61 | | |
| Observations | 1,271 | | | 1,271 | | |

*Notes:* Home = the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows. PIP = posterior inclusion probability. SD = standard deviation. In the frequentist check we only include explanatory variables with PIP > 0.3. The standard errors in the frequentist check are clustered at both the study and data set level (the implementation of two-way clustering follows Cameron *et al.*, 2011). In this specification we use the beta-binomial prior advocated by Ley & Steel (2009) (the prior model probabilities are the same for all possible model sizes) and set Zellner's g prior following Fernandez *et al.* (2001). More details on the BMA estimation are available in Table A2 and Figure A2. A detailed description of all variables is available in Table 3.

Table 7: Robustness check—unweighted regressions

| Response variable: | Bayesian model averaging | | | Frequentist check (OLS) | | |
|---|---|---|---|---|---|---|
| Estimate of Home | Post. mean | Post. SD | PIP | Coef. | Std. er. | p-value |
| *Data characteristics* | | | | | | |
| Mid-year of data | -0.025 | 0.003 | 1.000 | -0.027 | 0.006 | 0.000 |
| Panel data | 0.215 | 0.165 | 0.695 | 0.283 | 0.155 | 0.069 |
| Disaggregated | 0.619 | 0.120 | 1.000 | 0.537 | 0.235 | 0.022 |
| Obs. per year | 0.060 | 0.054 | 0.617 | 0.105 | 0.127 | 0.407 |
| No. of years | 0.022 | 0.050 | 0.195 | | | |
| *Countries examined* | | | | | | |
| Canada | 0.996 | 0.137 | 1.000 | 0.940 | 0.293 | 0.001 |
| US | -1.655 | 0.181 | 1.000 | -1.730 | 0.285 | 0.000 |
| EU | -1.317 | 0.114 | 1.000 | -1.313 | 0.258 | 0.000 |
| OECD | -1.069 | 0.159 | 1.000 | -1.062 | 0.263 | 0.000 |
| Emerging | 0.870 | 0.164 | 1.000 | 0.810 | 0.233 | 0.001 |
| *Design of the analysis* | | | | | | |
| No internal trade | 1.239 | 0.164 | 1.000 | 1.128 | 0.283 | 0.000 |
| Inconsistent dist | 0.016 | 0.071 | 0.074 | | | |
| Actual distance | -0.655 | 0.215 | 0.970 | -0.722 | 0.301 | 0.016 |
| Total trade | 0.005 | 0.056 | 0.030 | | | |
| Asymmetry | 0.001 | 0.023 | 0.028 | | | |
| Instruments | -0.007 | 0.055 | 0.038 | | | |
| *Treatment of multilateral resistance* | | | | | | |
| Remoteness | -0.001 | 0.028 | 0.026 | | | |
| Country fixed eff. | -0.002 | 0.044 | 0.040 | | | |
| Ratio estimation | 0.035 | 0.111 | 0.125 | | | |
| Anderson est. | 0.001 | 0.039 | 0.026 | | | |
| No control for MR | 0.489 | 0.131 | 0.990 | 0.470 | 0.177 | 0.008 |
| *Treatment of zero trade flows* | | | | | | |
| Zero plus one | -0.686 | 0.181 | 0.986 | -0.571 | 0.308 | 0.064 |
| Tobit | -0.131 | 0.221 | 0.309 | -0.436 | 0.252 | 0.084 |
| PPML | -0.969 | 0.174 | 1.000 | -1.024 | 0.388 | 0.008 |
| Zeros omitted | -0.001 | 0.025 | 0.028 | | | |
| *Control variables* | | | | | | |
| Adjacency control | 0.093 | 0.147 | 0.336 | 0.294 | 0.221 | 0.184 |
| Language control | -0.001 | 0.021 | 0.029 | | | |
| FTA control | -0.015 | 0.062 | 0.083 | | | |
| *Publication characteristics* | | | | | | |
| Published | -0.001 | 0.032 | 0.031 | | | |
| Impact | -0.186 | 0.055 | 0.979 | -0.188 | 0.125 | 0.131 |
| Citations | 0.182 | 0.047 | 0.992 | 0.173 | 0.106 | 0.103 |
| Publication year | 0.097 | 0.015 | 1.000 | 0.089 | 0.039 | 0.023 |
| Constant | 2.750 | NA | 1.000 | 2.678 | 0.974 | 0.006 |
| Studies | 61 | | | 61 | | |
| Observations | 1,271 | | | 1,271 | | |

*Notes:* Home = the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows. PIP = posterior inclusion probability. SD = standard deviation. In the frequentist check we only include explanatory variables with PIP > 0.3. The standard errors in the frequentist check are clustered at both the study and data set level (the implementation of two-way clustering follows Cameron *et al.*, 2011). In this specification we do not weight the regressions by the inverse of the number of estimates reported per study. More details on the BMA estimation are available in Table A3 and Figure A3. A detailed description of all variables is available in Table 3.

## 6 Criticisms of Meta-Analysis

In this section we list potential problems with conducting meta-analysis in economics and discuss how we address them. We identify 13 claims about meta-analysis that may cast doubt on the method:

1. *Studies of low quality should be excluded.* Our data set includes estimates from studies published in top journals, but also from studies not published in good outlets. As an alternative to meta-analysis, Slavin (1995) proposes "best evidence synthesis," which would only take into account good studies. The obvious problem is where to draw the line between good and bad ones. We prefer to include as many papers as possible and give weight to different aspects of study design according to what we believe is the consensus on best practice methodology. In this way we can explore the influence of different methods on the estimated border effects. We also control for the impact factor of the publication outlet and for the number of citations each study gets.

2. *The analysis omits some studies.* We try to include as many studies as possible, but may still miss some. To allow other researchers to replicate our analysis, we use the query described in Section 2 to search for studies estimating the border effect. We believe it is not a problem to miss some studies, as long as their results do not differ systematically from the results of the studies included. With 1,271 estimates taken from 61 studies, our paper ranks among the largest meta-analyses conducted in economics (according to the survey by Doucouliagos & Stanley, 2013).

3. *Studies reporting many estimates dominate the meta-analysis.* When each estimate gets the same weight, the unbalanced nature of data in meta-analysis means that studies with many estimates drive the results. One remedy involves the mixed-effects multilevel model, which gives each study approximately the same weight if the within-study correlation of the estimates is large (Havranek & Irsova, 2011). The problem is that the method introduces study-level random effects, which may be correlated with explanatory variables. With so many explanatory variables defined at the study level, we prefer to simply weight the regressions by the inverse of the number of estimates reported per study.

4. *Authors' preferred estimates should get more weight.* Studies examining the border effect

usually present many estimates, and often prefer a subset of these estimates (many results are shown as robustness checks). Some authors make it clear what their preference is, but for many studies it is impossible to select the preferred estimates. We control for data and methodology instead, which is easier to code and should capture most of the authors' preferences, for example, the control for multilateral resistance.

5. *Individual estimates are not independent, because authors use similar data.* Meta-analysis was originally designed for synthesizing medical research, where individual clinical trials can be considered approximately independent. In contrast, the regression results reported in economics are not independent, but neither are the observations in most economics data sets. To account for the dependence among observations we cluster the standard errors at the level of individual studies and data sets.

6. *Weighting by precision is inappropriate in economics because some methods underestimate standard errors.* Meta-analysts often use precision weights to remove heteroskedasticity in the regression estimating publication bias. We find no evidence of publication bias, so we can exclude the standard error from the equation and do not have to weight the estimates by precision to yield efficiency. Section 3 also illustrates that weighting by precision has little effect on the estimated border effect.

7. *Standard errors are not exogenous to the estimated coefficients.* When the choice of method systematically affects both the magnitude of the estimated border effect and its standard error, the explanatory variable in (2) will be correlated with the error term. Our solution is to use the number of observations as an instrument for the standard error: studies with more observations yield more precise estimates, but the number of observations is little correlated with the choice of methodology.

8. *The analysis omits some factors that may cause heterogeneity in the reported estimates.* We collect 32 aspects of data, methodology, and studies that may affect the estimated border effects. More specifics of study design could be included: for example, the exact method for computing internal distance (we only include a dummy variable which equals one if the method differs from the computation of external distance); but we have to draw a line somewhere for the data collection to be feasible. Still, we collect more variables than

most meta-analyses in economics. Nelson & Kennedy (2009) review 140 meta-analyses and report that a median analysis uses 12 explanatory variables; the largest meta-analysis has 41 variables.

9. *There are too many potential explanatory variables and it is not clear which should be included.* With so many aspects of study design one cannot find a theory that motivates the inclusion of all of them. For example, we would like to give more weight to large studies published in good journals, but it is not obvious why they should report systematically different results. We prefer to collect as many variables as possible and use Bayesian model averaging to resolve the resulting model uncertainty. The variables picked by BMA contain the ones that we feel should be included, such as the control for multilateral resistance and the measurement of internal distance.

10. *Meta-analysis compares apples with oranges.* Meta-analysis in economics examines heterogeneous estimates. Different estimates are produced using different methods, and we try to control for the differences in the design of primary studies. We also provide separate results for the regions examined in the literature. To increase the comparability of the estimates in our data set, we choose to only include the results concerning the effect of *international* borders on trade and omit the large literature on intranational border barriers.

11. *Meta-analysis may disagree with large primary studies.* The major reason for conducting meta-analyses in medical science is to increase statistical power by combining small but costly clinical trials. Because individual clinical trials use similar methods, a comparison of a meta-analysis with a later, large clinical trial provides a viable test of the reliability of the meta-analysis. In economics the methods differ, and meta-analysis can be thought of as a weighted average of many different approaches. It would be difficult to construct a primary study reflecting all recent advances in the methodology of the gravity equation and all possible aspects of our definition of best practice.

12. *Mistakes in data coding are inevitable.* The collection of data for meta-analysis involves months of reading and coding the data. We do not use research assistants for this work, because it is too tempting to jump directly to regression tables and code the data without

reading much of the primary studies. We cannot exclude errors, but we do our best to minimize their number by collecting the data independently and then comparing and correcting the data sets.

13. *Publication bias invalidates meta-analysis.* When researchers prefer to report estimates showing a particular sign or statistical significance, the mean reported estimate will get biased. We test for publication bias in Section 3 and find little evidence of preferential selection. When we correct for any potential publication bias, we obtain a border effect close to the simple mean and median estimates. In general the file drawer problem matters for any type of literature synthesis, but meta-analysis can correct for the bias.

## 7 Concluding Remarks

We conduct a meta-analysis of the effect of international borders on trade. Using 1,271 estimates from 61 studies and controlling for differences in study quality, we show that the available empirical evidence suggests a mean reduction of 28% in international trade due to borders. The innovations introduced in the last decade to estimating the gravity equation alleviate the border puzzle worldwide and solve it for most OECD countries. Nevertheless, even after controlling for the advances in methodology we obtain large border effects for transition and developing countries.

To our knowledge, the only other quantitative survey on this topic is presented by Head & Mayer (2014, pp. 160–165), who compute the mean and median reported estimates of several important coefficients in the gravity equation, including the home coefficient. They collect 279 estimates from 21 studies and compute a mean and median home coefficient close to 2; in contrast, we find a mean and median close to 3. They focus primarily on studies published in top journals, while we gather more studies and control for study quality. Furthermore, Head & Mayer (2014) also collect estimates of the regression coefficient for the "same nation dummy," which serves as a control variable in many applications focusing on issues other than the border effect: for example, the trade effect of currency unions.

The same nation dummy usually has little variation and in most cases captures trade flows between large countries and their territories, such as between France and its overseas departments. The estimated coefficient for the dummy is often statistically insignificant and close to zero (see, for example, the results presented in Rose, 2004), which is the primary reason why Head & Mayer (2014) obtain a smaller mean border effect than we do. They also include estimates of intranational home bias (for example, Wolf, 2000), which we prefer to exclude and focus on the effect of international borders. In consequence, only 10 primary studies overlap in the two meta-analyses.

Head & Mayer (2014) do not explicitly explore the heterogeneity in the estimates, but compute separate summary statistics for studies that control for multilateral resistance. For these studies they report a mean home coefficient of 1.9 and a median of 1.6. That is, Head & Mayer (2014) also find that disregarding multilateral resistance exaggerates the estimated home coefficient, but their meta-analysis indicates that the bias is less than 0.4. Our results suggest that this aspect of methodology is more important: the omission of multilateral resistance terms biases the home coefficient by about 0.8, or about a quarter of the effect reported by McCallum (1995). In addition, we stress the importance of data aggregation, heterogeneity across regions, measurement of internal and external distance, and the treatment of zero trade flows.

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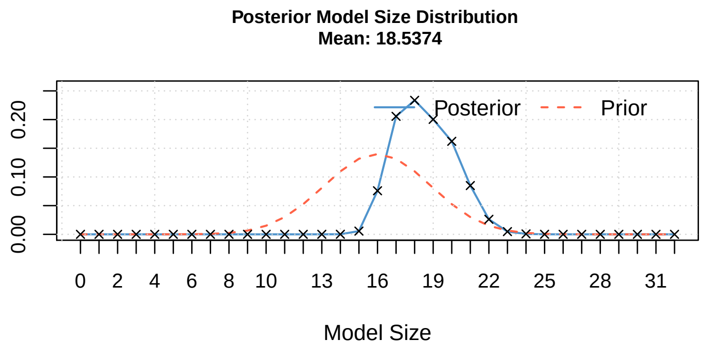
# Appendix A: Diagnostics of BMA

Table A1: Summary of BMA estimation, baseline specification

| | | | |
|---|---|---|---|
| *Mean no. regressors* | *Draws* | *Burn-ins* | *Time* |
| 18.5374 | $2 \cdot 10^6$ | $1 \cdot 10^6$ | 6.914583 minutes |
| *No. models visited* | *Modelspace* | *Visited* | *Topmodels* |
| 311,863 | $4.3 \cdot 10^9$ | 0.0073% | 98% |
| *Corr PMP* | *No. Obs.* | *Model Prior* | *g-Prior* |
| 0.9994 | 1,271 | uniform | UIP |
| *Shrinkage-Stats* | | | |
| Av= 0.9992 | | | |

*Notes:* In this specification we employ the priors suggested by Eicher *et al.* (2011) based on predictive performance: the uniform model prior (each model has the same prior probability) and the unit information prior (the prior provides the same amount of information as one observation of data).

Figure A1: Model size and convergence, baseline specification



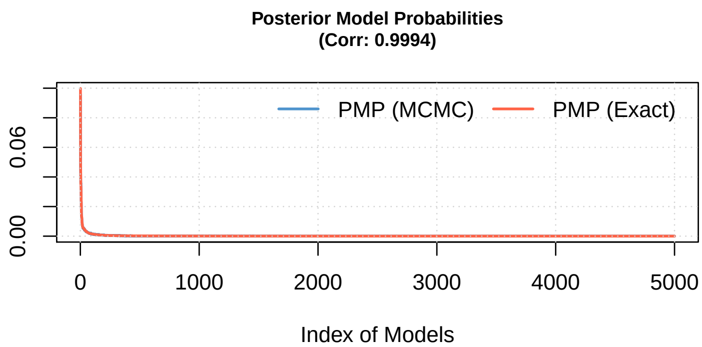



Table A2: Summary of BMA estimation, alternative priors

| | | | |
|---|---|---|---|
| *Mean no. regressors* 19.6891 | *Draws* $2 \cdot 10^6$ | *Burn-ins* $1 \cdot 10^6$ | *Time* 7.2395 minutes |
| *No. models visited* 394,789 | *Modelspace* $4.3 \cdot 10^9$ | *Visited* 0.0092% | *Topmodels* 96% |
| *Corr PMP* 0.9993 | *No. Obs.* 1,271 | *Model Prior* random | *g-Prior* BRIC |
| *Shrinkage-Stats* Av= 0.9992 | | | |

*Notes:* The "random" model prior refers to the beta-binomial prior advocated by Ley & Steel (2009): the prior model probabilities are the same for all possible model sizes. In this specification we set Zellner's g prior following Fernandez *et al.* (2001).

Figure A2: Model size and convergence, alternative priors

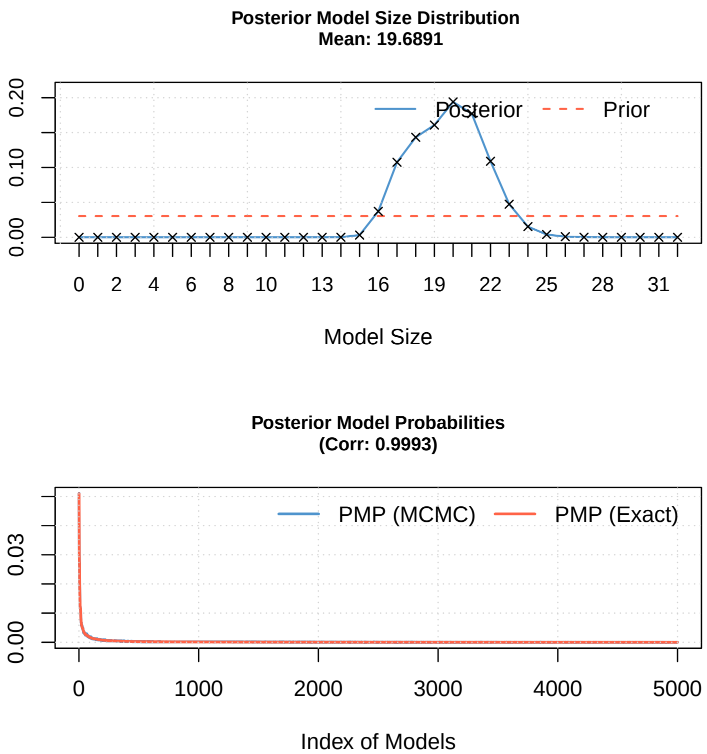
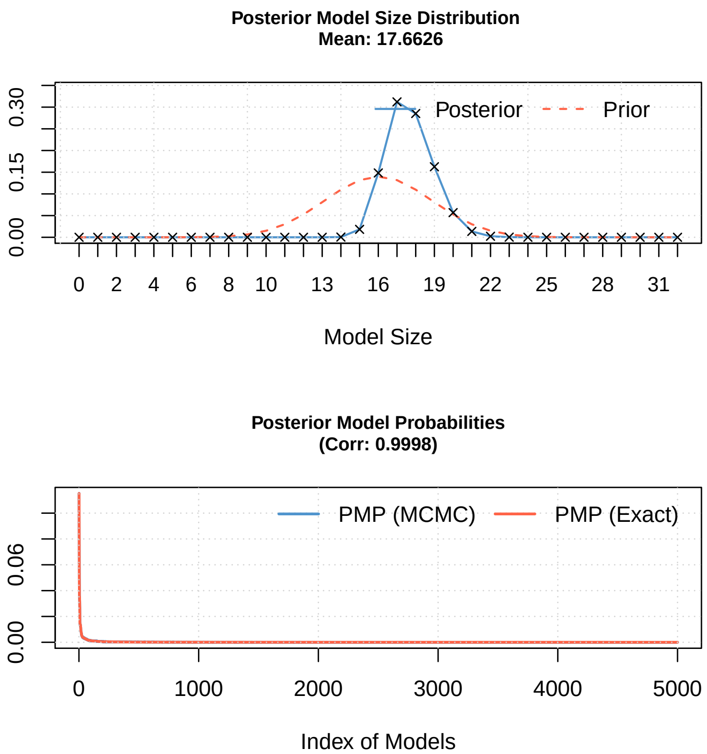


Table A3: Summary of BMA estimation, unweighted regressions

| | | | |
|---|---|---|---|
| *Mean no. regressors* 17.6626 | *Draws* $2 \cdot 10^6$ | *Burn-ins* $1 \cdot 10^6$ | *Time* 7.121633 minutes |
| *No. models visited* 350, 260 | *Modelspace* $4.3 \cdot 10^9$ | *Visited* 0.0082% | *Topmodels* 98% |
| *Corr PMP* 0.9998 | *No. Obs.* 1, 271 | *Model Prior* uniform | *g-Prior* UIP |
| *Shrinkage-Stats* Av= 0.9992 | | | |

*Notes:* In this specification we employ the priors suggested by Eicher *et al.* (2011) based on predictive performance: the uniform model prior (each model has the same prior probability) and the unit information prior (the prior provides the same amount of information as one observation of data).

Figure A3: Model size and convergence, unweighted regressions



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