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Tomáš Havránek, Zuzana Iršová

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Project Coordinator: Jan Babecký

Do Borders Really Slash Trade? A Meta-Analysis

Tomáš Havránek and Zuzana Iršová *

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 one-third reduction in international trade flows worldwide. The border effect varies across regions: it is substantial in emerging countries, but relatively small 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 in meta-analysis. 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.

Abstrakt

Existence hraničních bariér snižuje objem mezinárodního obchodu, ale většina odhadů tohoto efektu je až překvapivě veliká. V tomto článku ukazujeme, že nové metody pro odhady tohoto efektu představené během poslední dekády snižují implikovaný efekt hranic na 33% redukci mezinárodního obchodu. Efekt hranic se značně liší mezi regiony: je velký pro rozvíjející se země, ale relativně malý pro ekonomiky OECD. Pro tento výpočet jsme posbírali 1271 odhadů efektu hranic publikovaných v 61 studiích, rozčlenili jsme je podle 32 aspektů metodologie a použili bayesovské průměrování modelů k ošetření modelové nejistoty při metaanalýze. Naše výsledky naznačují, že metody výpočtu systematicky ovlivňují publikované efekty hranic. Důležitá je zejména úroveň agregace dat, měření vzdálenosti mezi zeměmi, ošetření multilaterální rezistence a způsob začlenění nebo vyloučení nulových obchodních toků. Nenalézáme žádné známky publikační selektivity.

JEL Codes: F14, F15.
Keywords: Bayesian model averaging, bilateral trade, borders, gravity, meta-analysis, publication selection bias.

*Tomáš Havránek, Economic Research Department, Czech National Bank, and Institute of Economic Studies, Charles University, Prague, tomas.havranek@cnb.cz
Zuzana Iršová, Institute of Economic Studies, Charles University, Prague, zuzana.irsova@ies-prague.org
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Nontechnical Summary

Dozens of researchers have attempted to estimate the effect of borders on international trade, but their results vary widely. We collect the available estimates of the relationship and examine the literature quantitatively using meta-analysis methods. Meta-analysis has been used in economics by, for instance, Card and Krueger (1995) on the employment effects of minimum wage increases, Disdier and Head (2008) on the impact of distance on trade, Havranek and 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. The method enables us to identify the factors that cause the heterogeneity in the literature and to construct a large synthetic study that estimates the border effect but corrects for potential publication or misspecification biases using the information presented in many empirical studies.

To our knowledge, the only other quantitative survey on this topic is presented by Head and Mayer (2014, pp. 160–165), who compute the mean and median reported estimates of several important coefficients in the gravity equation (the workhorse tool for estimating the effects of various factors on trade flows), including the border effect. They collect 279 estimates from 21 studies and compute a mean and median 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. Head and Mayer (2014) also do not explicitly examine the sources of heterogeneity in the literature and do not create a synthetic study aggregating the results of all researchers while correcting for potential mistakes in measurement.

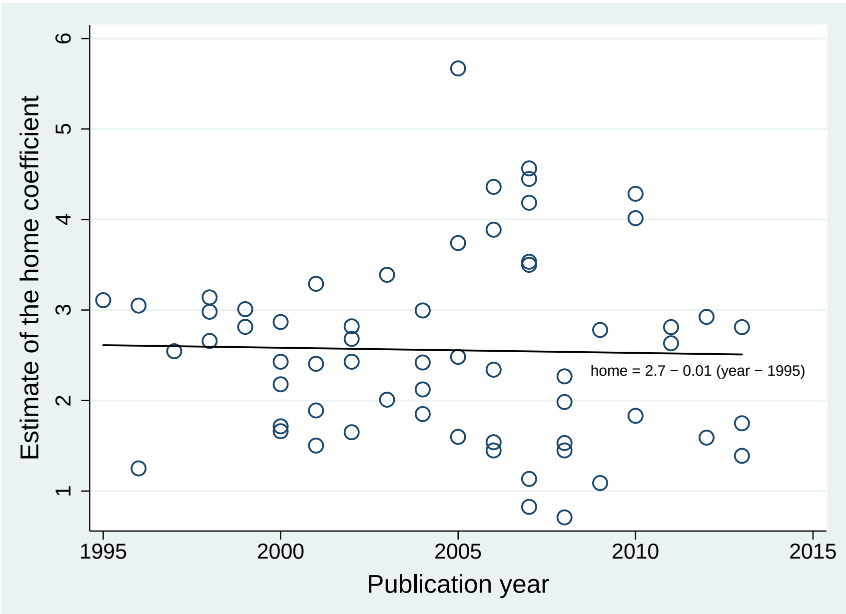
Our results suggest that many innovations in estimating the gravity equation systematically affect the reported border effect: for example, the use of disaggregated data, consistent measurement of within and between-country distance, data on actual road or sea distance traveled instead of the great-circle distance, control for multilateral resistance among countries, and the use of the Poisson pseudo-maximum likelihood estimator (a technique that allows, among other things, for the inclusion of observations of zero trade flows). 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 33% worldwide. The border effects differ significantly across regions—we obtain large estimates for emerging countries, but relatively small estimates for OECD countries (including the Czech Republic).

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 and 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 time trend.

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 and Krueger (1995) on the employment effects of minimum wage increases, Disdier and Head (2008) on the impact of distance on trade, Havranek and 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 worldwide by only one third. The border effects differ significantly across regions—we obtain large estimates for emerging countries, but relatively small estimates for most OECD countries.

We find little evidence of publication bias in the literature: researchers do not preferentially report small, large, or statistically significant estimates of the border effect. This result is remarkable considering a recent survey of estimates of publication bias, Doucouliagos and 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 and 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 the literature lacks the typical driver of publication selection. Estimates consistent with McCallum (1995) appear to be over-reported, but this fact does not bias the literature because McCallum's estimates are close to the overall mean and median.

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 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:

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

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 (for example, a free trade agreement).

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 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 home coefficient. 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 home coefficients; 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 investigate 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.[1] 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.

[1] We also do not include Fidrmuc and Fidrmuc (2003), who study the evolution of within-group trade bias in the following groups of countries: the former Czechoslovakia, the Baltic states, Slovenia-Croatia, and Russia-Belarus-Ukraine *after* their disintegration.

We collect all estimates of the home coefficient 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 home coefficient; 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 and 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. We also include the few border effect estimates that use services trade data (Anderson et al., 2014), although almost all studies focus on the arguably less home-biased goods trade. 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`.

Table 1: Border Effects Differ Across Countries

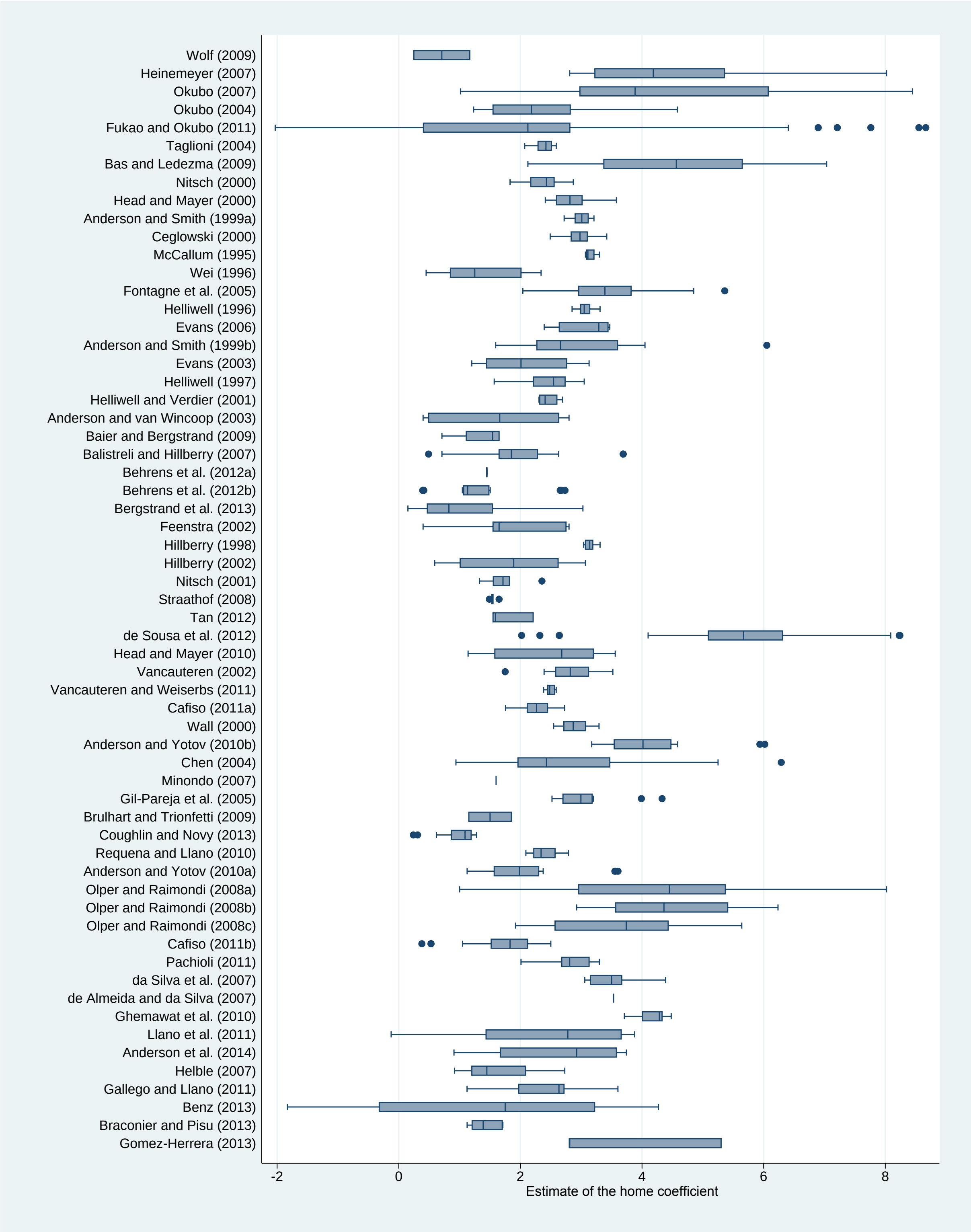
		Unweighted			Weighted		
	No. of estimates	Mean	95% conf. int.		Mean	95% conf. int.	
Canada	213	2.86	2.66	3.06	2.81	2.58	3.05
US	64	0.72	0.03	1.40	1.36	0.99	1.73
EU	263	2.55	2.04	3.05	2.59	2.18	2.99
OECD	98	2.35	1.71	3.00	2.41	1.90	2.91
Emerging	82	5.05	4.59	5.51	4.14	3.18	5.10
All countries	1,271	3.03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

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.

We say that an estimate corresponds to the border effect of a particular country if identification of the home coefficient comes from trade flows within the country. For example, if data on trade flows

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. Studies are sorted by mid-year of the sample in ascending order. Full references for the studies included in the meta-analysis are available in Appendix B.

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 and 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. (Also relatively common are estimates that identify the border effect for the entire world or that use internal trade for Japan, Germany, and Spain.)

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 and 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).[2]

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 home coefficients drive the results. The mean unweighted estimate of the home coefficient equals 3, virtually identical to the original estimate of the parameter by McCallum (1995). This home coefficient 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 home coefficients 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 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 emerging countries are substantially less integrated into global trade than developed countries.

In Table 2 we report the mean estimated home coefficients for particular subsets of methods and studies. When compared with Table 1, it seems that the effect of methods on the results is less pronounced than the effect of the choice of the region for which the border effect is estimated. Some method choices generate systematically different results, but the impacts get muted when we move to the right-hand part of the table, where each study is assigned the same weight. Estimates obtained using panel or disaggregated data tend to be somewhat larger, while the use of actual within-country trade flows (as opposed to approximating internal trade using production data) and the inclusion of zeros are associated with smaller estimates. Published studies report mean estimates

[2] The results do not change much if we use author-level clustering instead of study-level clustering.

Table 2: Border Effects for Subsets of Methods and Studies

		Unweighted			Weighted		
	No. of estimates	Mean	95% conf. int.		Mean	95% conf. int.	
Panel data	847	3.47	2.93	4.01	2.93	2.56	3.30
Disaggregated	724	3.50	2.90	4.10	2.88	2.42	3.33
Internal trade	538	2.44	1.90	2.98	2.35	1.89	2.81
Consistent dist.	1,094	3.10	2.54	3.65	2.56	2.13	2.99
Control for MR	784	3.29	2.64	3.94	2.58	2.05	3.11
Zeros included	436	2.49	1.93	3.06	2.41	2.04	2.79
Published	1,144	3.11	2.59	3.64	2.66	2.28	3.04
New studies	607	3.06	2.24	3.89	2.58	1.98	3.18
All estimates	1,271	3.03	2.54	3.53	2.59	2.23	2.95

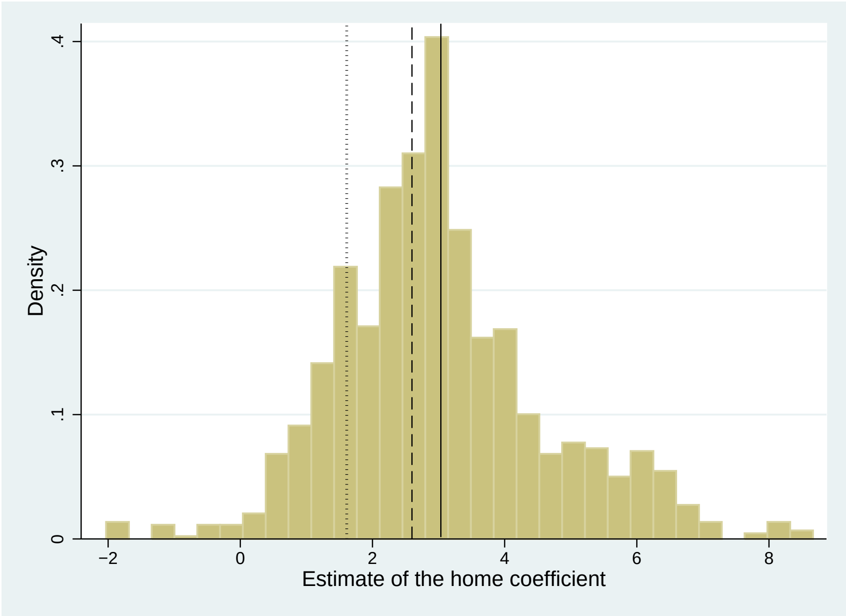
Notes: The table presents mean estimates of the home coefficient (the coefficient estimated in a gravity equation on the dummy variable that equals one for within-country trade flows) for estimates obtained using a particular methodology or reported in a particular study. Internal trade = within-country trade flows are directly observed in the data. Consistent dist. = within-country distance is measured in the same way as between-country distance. MR = multilateral resistance. New studies = studies published in 2007 (the median year of publication in our data) or later. 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.

virtually identical to those of unpublished studies, and the average results also do not change much over time. Because authors often change several data and method characteristics simultaneously, and there are many additional aspects of study design that might influence the estimates, in Section 4 we use meta-regression analysis to investigate in detail the marginal effects of data and method choices on the reported border effects.

Figure 3 shows the histogram of the estimated home coefficients. We see that almost all the estimates are positive; in the data we only have 22 negative estimates, 1.7% of all the home coefficients. The median estimate is very close to the overall mean and equals 2.9. The median estimate of the median home coefficients 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 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 home coefficient 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 home coefficients, but the pattern may be explained by differences in methodology. Another potential reason for between-study differences in estimates is publication selection.

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*.

3. Publication Bias

Publication selection bias arises when estimates have a different probability of being reported based on their magnitude 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 and Lang (1992), Card and Krueger (1995), and Ashenfelter et al. (1999). In a survey of examinations of publication bias, Doucouliagos and 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).

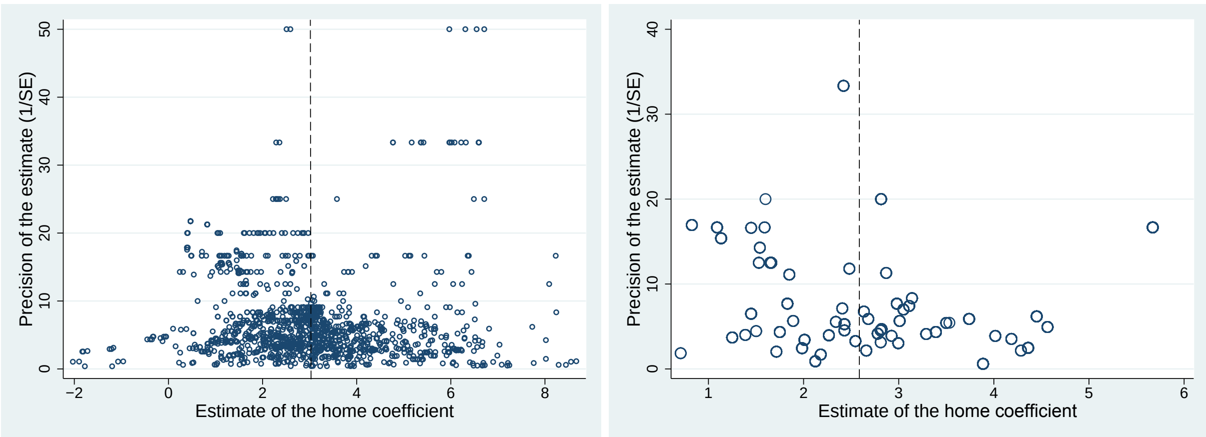
The presence of publication bias can be examined visually using the so-called funnel plot (Egger et al., 1997). It is a scatter plot showing the magnitude of the estimated effects on the horizontal axis and the precision (the inverse of the estimated standard error) on the vertical axis. If the literature 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 home coefficient. Second, the funnels are not hollow, and even estimates with very little precision (and, thus, small p-values) are reported. Third, 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 and 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 and Krueger (1995), we explore the relationship between the estimates of the home coefficient and their standard errors. Because the methods used by researchers to estimate the home coefficient yield a t-distribution (or another symmetrical distribution) for the ratio of estimates to their standard errors, the estimates and standard errors should be statistically independent quantities. In contrast, if statistically significant estimates are preferred, researchers will search for large estimates of the home coefficient in order to offset the standard errors and produce large t-statistics, which will lead to a correlation between the semi-elasticities and standard errors. Similarly, when researchers discard negative estimates, a positive relationship arises between the reported estimates and their standard errors because of heteroskedasticity (Stanley, 2008):

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

where $HOME_{ij}$ are i-th estimates of the home coefficient reported in j-th study, $SE(HOME_{ij})$ are the reported standard errors of the home coefficient estimates, $HOME_0$ is the mean home coefficient corrected for potential publication bias, β measures the extent of publication bias, and u_{ij} is a normal disturbance term. For example, if the true mean home coefficient was zero (implying no border effect) but all researchers reported the 5% of estimates that are positive and statistically significant, the estimated β 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, β 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).

In examinations of publication bias it is common to assume, as we have done so far in this section, that the selection criteria leading to the bias are based on the sign and statistical significance of the estimate in question. In the literature estimating the border effect, however, potential publication selection need not be driven by the sign and significance of the resulting coefficients, because negative and insignificant estimates are difficult to obtain due to the relatively large underlying border effect. Instead, researchers are likely to use the well-known results reported by McCallum (1995) as a benchmark, and in this case publication selection could assume the following two forms.

First, researchers may discard estimates inconsistent with McCallum (1995). The benchmark home coefficient presented by McCallum (1995) is 3.09 with a standard error of 0.13. Estimates close to McCallum's are reported frequently: those lying within one standard error from McCallum's central estimate account for 12% of all the estimates in the literature, twice the number we would expect if the estimates were normally distributed (given that the literature reports a mean estimate of 3.03 with a standard deviation of 1.6). The over-reporting of estimates similar to McCallum's might reflect the fact that researchers simply try to replicate his results as a part of their analysis, or it could point to genuine publication selection. In any case, because such a selection criterion is symmetrical (both small and large estimates inconsistent with McCallum are omitted), it does not create a bias. Note that the mean of all the home coefficients reported in the literature is very close to McCallum's central estimate, and that the mean would only change from 3.03 to 3.02 if we discarded all results lying inside the 95% confidence interval of McCallum's estimate.

Second, researchers may want to shrink the border effect reported by McCallum (1995) and preferentially select small estimates for reporting. Such a selection criterion is asymmetrical, and would result in a downward bias in the literature. Suppose, for example, that researchers would strive to report estimates significantly smaller than McCallum's result. They would need the ratio $(3.09 - HOME)/SE$, the relevant t-statistic, to be as large as possible, which would again give rise to a correlation between the nominator and denominator of the ratio and would show as a negative and statistically significant coefficient β in (2). In other words, the corresponding funnel plot would become asymmetrical because large estimates would be reported less often than small estimates with the same precision. Equation (2) measures the degree of asymmetry of the funnel plot and so it is able to detect any selection process that causes a systematic bias in the literature.

We present the results of the funnel asymmetry tests in Table 3. Because regression (2) is heteroskedastic, we report 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 home coefficients in our sample. The coefficient corresponding to the extent of publication bias is statistically insignificant and close to zero, while the estimated home coefficient beyond publication bias is 2.9, close to the mean and median home coefficient 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.

Table 3: 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.

The second column of panel A in Table 3 estimates equation (2) using only the home coefficients reported in published studies. Perhaps editors or referees prefer coefficients that are significantly smaller than the central estimate of McCallum (1995), which would pull the mean reported home coefficient down. Indeed, in a meta-analysis of vertical productivity spillovers from foreign direct investment, Havranek and 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 home coefficient beyond any potential bias. Next, in the third column 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 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 home coefficient 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 home coefficient 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 3 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 home coefficients, but little related to the methods of estimation. The instrumental variable estimation is less precise, but still reports the mean underlying home coefficient close to 3 and no evidence of publication bias.

In panel B of Table 3 we weight all the estimates by their 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 precision weights do not change 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 home coefficient 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 home coefficient 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 home coefficient decreases to 1.2, 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 home coefficient, 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 4 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. Third, we collect variables reflecting the general design of the analysis. Fourth, 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. Sixth, 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 and Mayer (2014).

Table 4: 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 (the 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 (reference category for this group of dummy variables: the border effect is estimated for the entire world or combinations of country groups).	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 (usually EU-15).	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 emerging 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

Continued on next page

Table 4: Description and Summary Statistics of Regression Variables (continued)

Variable	Description	Mean	SD	WM
Treatment of multilateral resistance				
Remoteness	=1 if remoteness terms are included (reference category for this group of dummy variables: multilateral resistance terms are controlled for by a method not listed here).	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 and 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 (reference category for this group of dummy variables: zero trade flows are treated by a method not listed here or the data set contains no 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 deleted.	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 home coefficient 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 and 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 home coefficient 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. 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 and 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). Also relatively common are estimates that identify the border effect using internal trade in Japan (7%), Spain (4%), and Germany (2%), the region pairs US-Canada (7%), US-EU (5%), EU-Japan (4%), and US-Japan (4%), and the entire world (5%).

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 and 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 and Pisu, 2013). A couple of studies in our data set commit what Baldwin and 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 and 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 and 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 and Mayer, 2000), and 11% of studies use this method. About 6% of studies use the non-linear technique introduced by Anderson and 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—this is what Baldwin and Taglioni (2007) call the "gold medal mistake" in estimating gravity equations. The reference category for this group of dummy variables is estimation that controls for multilateral resistance using a method different from those described above (for example, the spatial econometric technique employed by Behrens et al., 2012).

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 and 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 and 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. The reference category for this group of dummy variables is estimation that incorporates zero trade flows using a method different from those described above or that encounters no zero trade flows in the data (for example, studies using aggregated OECD data).

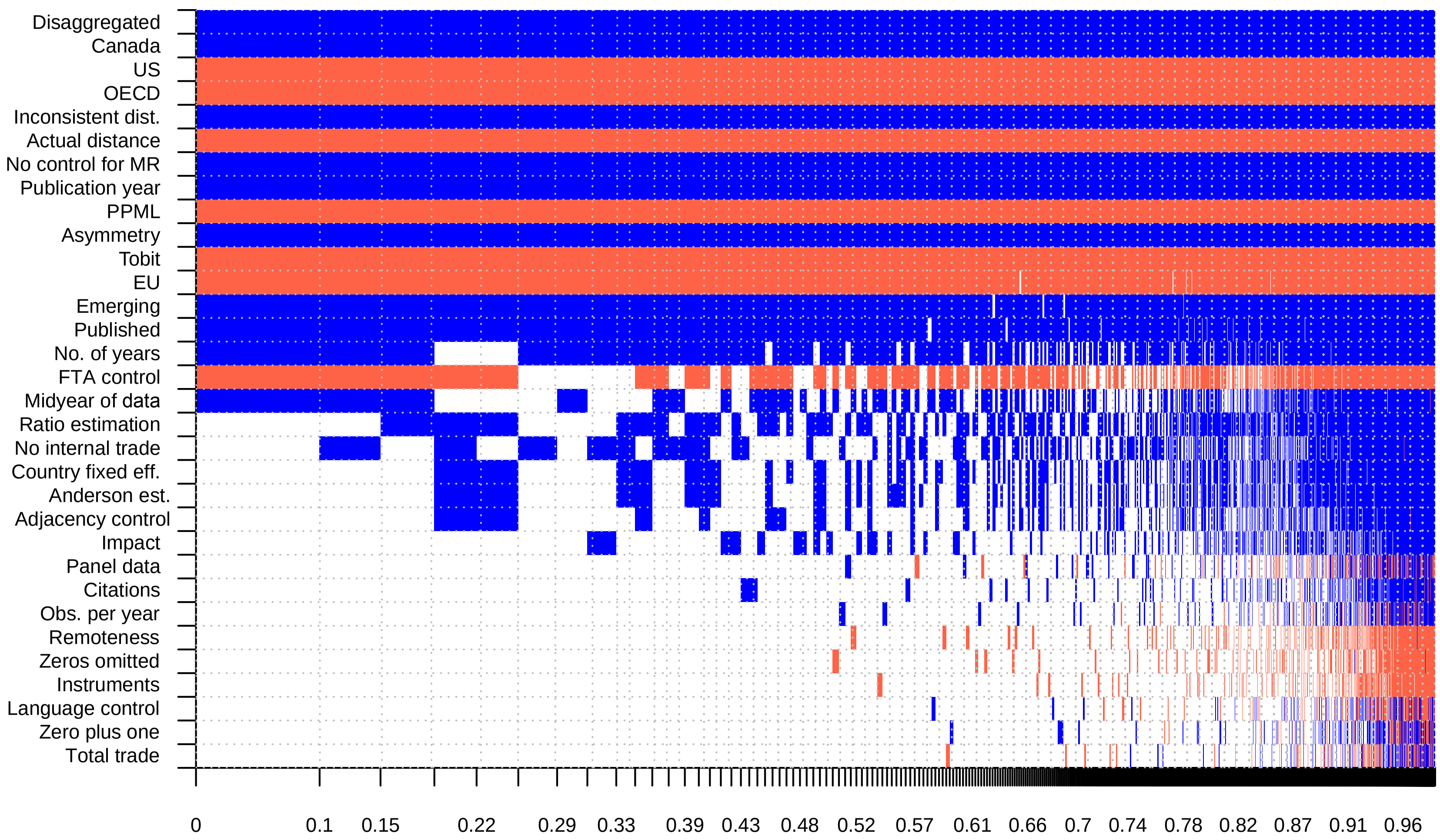
Control variables Studies estimating the border effect typically include three control variables: dummies for adjacency, common language, and membership in a free trade agreement. We examine whether the inclusion of these variables has a systematic influence on the estimated home coefficient. In many 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" set 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 when 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 home coefficient 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 4 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. With such a large number of explanatory variables we face substantial model uncertainty, which can be addressed by Bayesian model averaging (BMA).

BMA runs many regressions involving 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 and 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

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 5. A detailed description of all variables is available in Table 4.

posterior standard deviations reflecting the distribution 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. Note that while BMA can be used to select the "best" model with a particular set of explanatory variables, we focus on the average of all models weighted by the posterior probability; that is, we do not drop any explanatory variables. 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 5. 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 seem to help explain the variability in the estimates of the border effect (nevertheless, our baseline specification is the weighted average of models from BMA, which does not exclude any variables). 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 home coefficient 0.8 larger; the posterior inclusion probability of this variable is decisive. The result corroborates the findings of Anderson and Yotov (2010, p. 2167), who also find that aggregated data yield "significantly smaller" estimates of the border effect (they do not report the precise difference). In contrast, Hillberry (2002) finds that aggregation exaggerates the home coefficient by about 1. 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 emerging countries, followed by Canada. The smallest estimates are reported for the US. Balistreri and Hillberry (2007) discuss how the small estimates for the US may

Table 5: 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 4.

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 home coefficients tend to be about 0.8 larger; the result is in line with the findings of Head and Mayer (2010), who also report that inconsistent measurement of within and between-country distance exaggerates the home coefficient (by more than 1). When the authors of primary studies use actual road or sea distances instead of employing the great-circle formula, they report home coefficients about 0.9 smaller. Braconier and Pisu (2013) also find that using the actual distance reduces the estimated border effect (though only by 15%). Next, asymmetric estimates of the border effect (measuring the difficulty of cross-border flows in one direction) 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, which also suggests that the size of the regions used for the definition of within-country trade flows does not matter much for the reported border effect. Furthermore, it seems that the "silver medal mistake" in estimation (summing imports and exports before taking logs) does not affect the resulting border effects, but very few papers in our data set commit this mistake.

In contrast, the "gold medal mistake" (omitting multilateral resistance) 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 home coefficients 0.8 larger. This result contrasts with the findings of Balistreri and Hillberry (2007), who report that the decrease in border effects found by Anderson and 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 method used seems to matter little. In a similar vein, Feenstra (2002) finds little difference between the magnitude of the border effect estimated using fixed effects and the estimator developed by Anderson and van Wincoop (2003).

The treatment of zero trade flows affects the estimated border effect as well. If Tobit or PPML is used, the resulting home coefficients tend to be on average about 0.7 smaller. This finding contrasts with the results of Cafiso (2011), who finds slightly larger home coefficients in the EU using PPML compared with OLS (by about 0.2). The inclusion of control variables for adjacency, common language, and mutual trade agreement does not seem to matter much for border effects. Concerning publication and other study characteristics, papers published in refereed journals tend to report home coefficients 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 home coefficients 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 believe there is value in correcting the mean reported coefficients for the marginal effects of method choices that arguably create problems in the identification of the gravity equation. We show that, when evaluated together, the major innovations introduced into the estimation of gravity equations in the last decade substantially alleviate the border puzzle.

For each variable in Table 5 we select a preferred value (or leave the value unchanged for a given estimate if we have no preference on the value of the variable) and compute the implied home coefficient for different regions as the mean predicted estimate of the home coefficient. 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, 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 disregarding zero trade flows. For all other variables we keep the actual values of the sample.

Table 6: Advances in Methodology Shrink the Border Effect

	Weighted				Unweighted			
Best practice	Estimate	95% conf. int.		Diff.	Estimate	95% conf. int.		Diff.
Canada	2.19	1.26	3.12	-0.63	2.60	1.19	4.01	-0.25
US	0.67	-0.27	1.62	-0.69	0.56	-0.50	1.63	-0.15
EU	1.46	0.44	2.49	-1.12	0.83	-0.51	2.17	-1.72
OECD	0.54	-0.59	1.67	-1.86	0.63	-0.79	2.05	-1.72
Emerging	3.16	1.73	4.59	-0.98	3.21	1.97	4.44	-1.85
All countries	1.76	0.84	2.67	-0.84	1.82	0.53	3.11	-1.21

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 (Table 5) and predict the 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 8).

Table 6 presents the results; the overall mean home coefficient is reported in the last row and 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 5; the right-hand part is based on regressions not weighted by the inverse of the number of estimates reported per study (Table 8). The two sets of results are qualitatively similar, but the unweighted specification yields somewhat smaller estimates for the US and the EU and larger estimates for Canada, the OECD, and emerging countries. We focus on the results obtained from the weighted regressions, because in this framework studies reporting many estimates do not drive the results.

From Table 6 we see that giving more weight to studies that correct for the traditional problems in gravity equations and use novel methods decreases the estimated home coefficients significantly for each region. The overall mean home coefficient is 1.76, which translates into a border effect of 5.8—almost four times smaller than the border effect based on the sample mean of the home coefficients reported in the literature. The border effect for the US and OECD countries is even smaller: only $\exp(0.67) = 1.95$ and $\exp(0.54) = 1.72$; in contrast, the effect is still substantial for emerging countries: $\exp(3.16) = 23.6$. Regions in emerging countries tend to trade almost twenty-four times more with regions in the same country than with similar foreign regions.

A qualification concerning the precision of our best-practice estimates is in order. The confidence intervals presented in Table 6 only reflect the uncertainty surrounding the estimates of the regression parameters in Table 5, not the uncertainty associated with defining the best-practice values of various variables. Therefore, the reported confidence intervals understate the total uncertainty surrounding our estimates. Nevertheless, we believe that the unmeasured uncertainty is skewed downward, since plausible adjustments of the definition of best practice would yield even smaller estimates of the border effect. For example, giving preference to PPML would further reduce the resulting home coefficient. Similarly, the reduction in the home coefficient would be even larger if we expressed no preference for the values of 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. We prefer the use of disaggregated data, but one could make the argument that in some cases disaggregated data are not representative; withdrawing our preference for disaggregation would further reduce the estimate. The reduction in the size of the border effect presented in Table 6 is entirely (and equally) driven by our preference for the following three method characteristics: the inclusion of multilateral resistance terms, consistent measurement of within and between-country distance, and the use of actual road or sea distance. It is also worth noting that our final estimate of the home coefficient (1.76) is close to the median home coefficient reported in the best journals (1.7; discussed at the end of Section 2).

To put our estimates into perspective, we compute the ad-valorem tariff equivalent of the implied border effects. 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 and 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 $\exp(1.76/5.03) - 1 = 42\%$. For OECD countries the tariff equivalent of border barriers falls to 11.4%, which is comparable to 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 87%.

One of the main points of Anderson and 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 and Mayer (2014, pp. 167–170, who also provide a Stata code for the computation). In short, we need to compute the ratio of trade in the actual world to that in the counterfactual (borderless) world. The approach assumes that labor endowment is fixed and that trade deficits are exogenously given on a per-capita basis, which implies that trade deficits are specified in units of labor of each country. The formula for the share of a country's expenditure on goods from another country provided by Dekle et al. (2007) enables us to compute the equilibrium change in GDP. Employing the data provided by Head and 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 3.77 for regions in the same country and 0.67 for regions across borders (compared with the partial equilibrium border effect of 5.8). That is, our results suggest that for an average country borders reduce international trade by 33% and increase within-country trade by 277%.

5. Robustness Checks

We present three additional sets of results. First, we use alternative priors for Bayesian model averaging. Second, we employ unweighted regressions in the BMA exercise. Third, we use OLS and study fixed effects. 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 home coefficients, 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) show 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 and 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 7 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 home coefficient conditional on best practice is 1.67, implying a partial equilibrium border effect of 5.3, slightly below the estimate presented in the last section. The region-specific home coefficients are also similar: 2.04 for Canada, 0.52 for the US, 1.41 for the EU, 0.40 for the OECD, and 3.06 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 8 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 home coefficient 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 home coefficients (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 6. The overall mean home coefficient is 1.82, implying a partial equilibrium border effect of 6.2.

In the third robustness exercise we solely use frequentist estimation methods to check whether our reliance on Bayesian techniques drives the conclusions. The left-hand part of Table 9 presents the

Table 7: 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 and 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 4.

Table 8: 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 4.

Table 9: Robustness Check—OLS and Fixed Effects

Response variable:	OLS			Fixed effects		
Estimate of Home	Coef.	Std. er.	p-value	Coef.	Std. er.	p-value
Data characteristics						
Midyear of data	-0.002	0.011	0.874	-0.059	0.039	0.130
Panel data	-0.381	0.474	0.422	-0.035	0.205	0.864
Disaggregated	0.681	0.342	0.046	0.155	0.475	0.745
Obs. per year	-0.097	0.094	0.301	0.195	0.134	0.151
No. of years	0.267	0.258	0.301	-0.015	0.090	0.866
Countries examined						
Canada	0.822	0.351	0.019	1.096	0.321	0.001
US	-1.046	0.237	0.000	-1.251	0.221	0.000
EU	-0.535	0.395	0.176	-0.436	0.175	0.016
OECD	-0.833	0.336	0.013	-0.434	0.232	0.066
Emerging	0.653	0.248	0.009	1.129	0.558	0.048
Design of the analysis						
No internal trade	0.333	0.357	0.352	0.117	0.451	0.796
Inconsistent dist.	0.665	0.342	0.052	0.919	0.248	0.000
Actual distance	-0.640	0.335	0.056	-0.754	0.034	0.000
Total trade	-0.264	0.342	0.440	0.142	0.154	0.360
Asymmetry	0.376	0.236	0.111	0.171	0.123	0.170
Instruments	-0.156	0.311	0.615	0.001	0.138	0.992
Treatment of multilateral resistance						
Remoteness	-0.275	0.341	0.419	0.304	0.124	0.017
Country fixed eff.	0.163	0.335	0.625	0.059	0.127	0.643
Ratio estimation	0.900	0.504	0.074			
Anderson est.	0.202	0.329	0.539	0.419	0.130	0.002
No control for MR	0.643	0.347	0.064	0.117	0.166	0.485
Treatment of zero trade flows						
Zero plus one	0.195	0.356	0.584	0.522	0.375	0.170
Tobit	-0.673	0.473	0.155	-0.747	0.354	0.039
PPML	-0.744	0.717	0.300	0.211	0.771	0.785
Zeros omitted	0.045	0.233	0.848	-0.093	0.180	0.605
Control variables						
Adjacency control	0.297	0.240	0.215	0.078	0.103	0.449
Language control	-0.014	0.274	0.959	-0.269	0.103	0.011
FTA control	-0.452	0.345	0.191	0.347	0.162	0.037
Publication characteristics						
Published	0.326	0.333	0.328			
Impact	0.119	0.203	0.558			
Citations	-0.067	0.105	0.523			
Publication year	0.047	0.037	0.211			
Constant	2.055	1.384	0.138	6.149	3.842	0.115
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. Fixed effects = we use study dummies. The standard errors are clustered at both the study and data set level (the implementation of two-way clustering follows Cameron et al., 2011). In the fixed effects estimation we exclude variables that do not vary within studies. A detailed description of all variables is available in Table 4.

results of OLS; in the right-hand part of the table we include study fixed effects (which means that we also eliminate all variables that do not vary within studies, such as the number of citations). The OLS results corroborate our previous findings concerning the factors most relevant for the explanation of the differences in the reported border effects: the level of data aggregation, consistent measurement of within and between-country distance, the use of actual road or sea distance, and control for multilateral resistance terms. Aggregation and control for multilateral resistance lose statistical significance when we add study fixed effects, but that is because the two variables show little within-study variation: most studies either use aggregated or disaggregated data and, apart from a few studies written around 2003, usually either ignore or control for multilateral resistance in all estimations. The home coefficient implied by our definition of best practice is 2.02 for OLS and 1.34 for fixed effects, with our baseline BMA estimate (1.76) representing approximately the midpoint of these two numbers.

6. 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 33% in international trade due to borders. The innovations introduced in the last decade to estimating the gravity equation alleviate the border puzzle worldwide and almost solve it for some OECD countries. Nevertheless, even after controlling for the advances in methodology we obtain large border effects for emerging countries.

To our knowledge, the only other quantitative survey on this topic is presented by Head and 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 and 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 and 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 and 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 and 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).

Table A2: Summary of BMA Estimation, Alternative Priors

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

Notes: The "random" model prior refers to the beta-binomial prior advocated by Ley and 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 A1: Model Size and Convergence, Baseline Specification

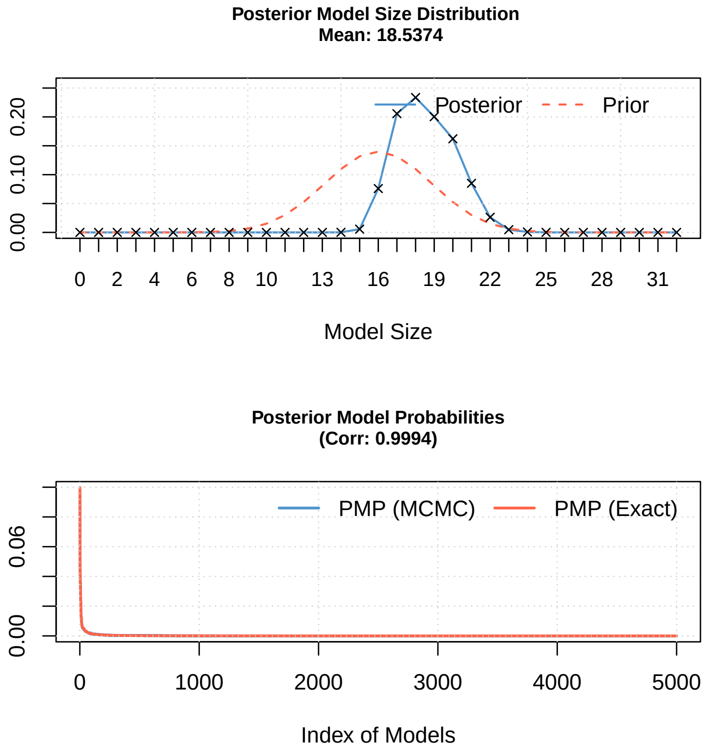


Figure A2: Model Size and Convergence, Alternative Priors

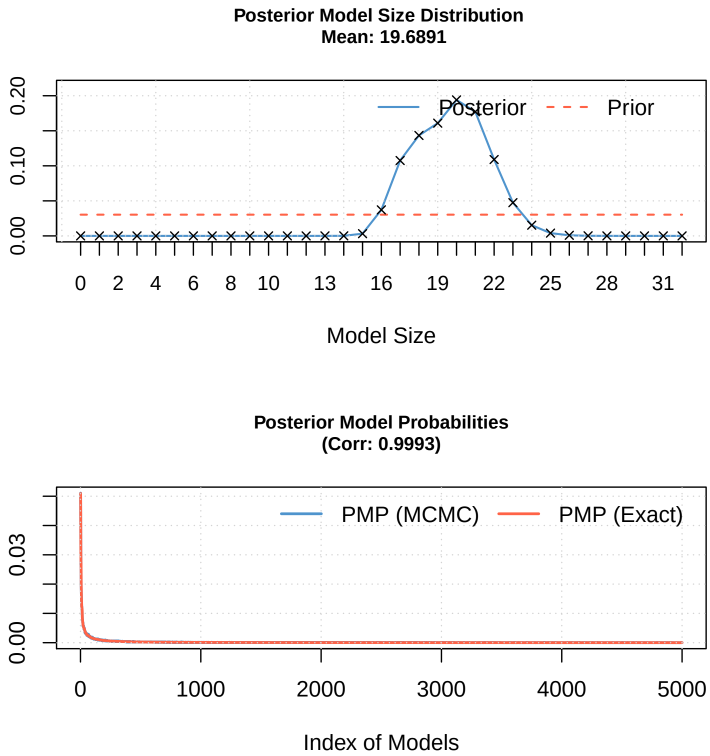
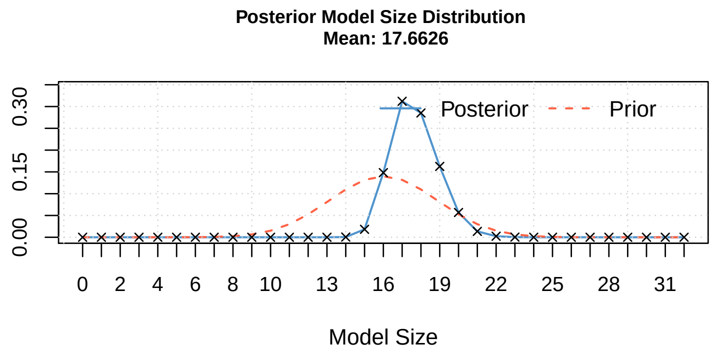


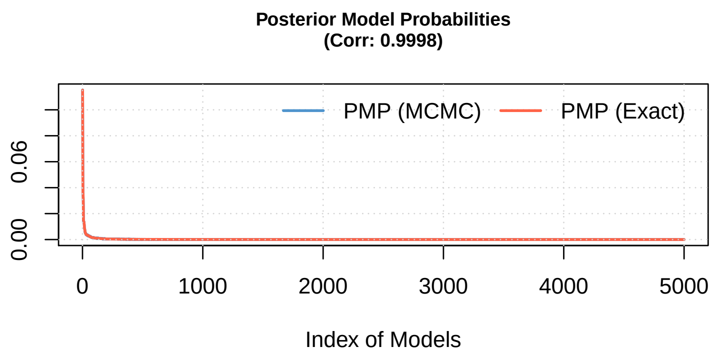
Table A3: Summary of BMA Estimation, Unweighted Regressions

Mean no. regressors	*Draws*	*Burn-ins*	*Time*
17.6626	$2 \cdot 10^6$	$1 \cdot 10^6$	7.121633 minutes
No. models visited	*Modelspace*	*Visited*	*Topmodels*
350,260	$4.3 \cdot 10^9$	0.0082%	98%
Corr PMP	*No. Obs.*	*Model Prior*	*g-Prior*
0.9998	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 A3: Model Size and Convergence, Unweighted Regressions





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Czech National Bank
Economic Research Department
Na Příkopě 28, 115 03 Praha 1
Czech Republic
phone: +420 2 244 12 321
fax: +420 2 244 14 278
http://www.cnb.cz
e-mail: research@cnb.cz
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