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Spillovers and Exports: A Meta-Analysis

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Abstract: This study uses meta-analysis to analyze the empirical literature on spillovers and exports. It collects 3,291 estimated spillover effects from 99 studies. The estimated spillover effects in the literature span a large number of types and measures of both exports and spillovers. As a result, we transform estimates to partial correlation coefficients (PCCs). We analyze these transformed effects using four different versions of Weighted Least Squares estimators, incorporating both meta-analytic "Fixed Effects" and "Random Effects". Our analysis produces three main findings. First, while we estimate a mean overall effect of spillovers on exports that is statistically significant, the size of the effect is economically negligible. Second, we find evidence of positive publication bias using conventional Funnel Asymmetry Tests. However, the size of the estimated publication bias is small, and disappears in some regressions when other explanatory variables are included in the analysis. Third, using both Bayesian Model Averaging and frequentist WLS estimation, we find that some data, estimation, and study characteristics are significant in some regressions. However, only a few of the characteristics are robust, and none are large in size.

Keywords: Spillovers, Exports, Meta-analysis; Meta-Regression Analysis; Bayesian Model Averaging, Partial Correlation Coefficient

JEL Classifications: D62; F10; F20; O30; C80

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I. INTRODUCTION

According to the theory of export-led growth, exports have played an important role in contributing to economic growth in both developed and developing countries.[1] Many reasons have been put forward to support the export-led growth proposition. These include increased demand for a country's output, reallocation of resources from the non-trade sector to the relatively more efficient export sector, exposure to better production and management practices, acquisition of knowledge about advanced technologies, and access to additional sources of financing. Expanded trade also allows firms to gain from specialization and increasing returns to scale. As a result, there is much interest in understanding the determinants of exports. Within this broad area, there has grown a substantial literature that has focused on the role of spillovers.

Spillovers are an intriguing, and challenging, subject because of the multitudinous channels that have been hypothesized for them. They are particularly of interest because they imply that markets, left to their own devices, may not function well. This creates a potential opportunity for the public sector to contribute to economic growth via subsidization or the direct supply of resources. Spillovers can affect exports directly; and indirectly, via their affect on productivity that in turn affects firms' export performance (Melitz, 2003).

The following is a non-exhaustive list of the many channels that have been hypothesized for spillovers to affect exports. Proximity to other exporters can produce spillovers that bring benefits such as lower costs and increased knowledge about foreign markets. Locational concentration of exporters can make it feasible to build specialised transportation infrastructure such as roads, railways, ports, airports, and storage facilities (Duranton & Puga, 2004). It also improves access to information about which goods to export to which markets (Aitken et al., 1997).

[1] For an excellent survey of the empirical literature on export-led growth, see Giles and Williams (2000).

Geographic concentration can help exporters establish networks. Krautheim (2008) shows how this leads to information sharing between firms exporting to the same country. Networks can reduce the fixed costs of exporting and thus increase the probability that a firm decides to export. In the same vein, agglomeration of exporters can reduce uncertainty between exporters and foreign buyers. Rauch and Watson (2003) argue that at the start of a commercial relationship, buyers may be uncertain about the ability of the supplier to deliver large orders. The concentration of exporters can increase buyers' information on the quality and reliability of foreign suppliers, and thus stimulate exports.

More generally, agglomeration can lead to lower transportation and transaction costs due to increased availability of services and goods from suppliers (Krugman, 1991). Further, geographic proximity allows the pooling of labour resources, reducing search and matching costs for workers and firms (Duranton & Puga, 2004).

Much research attention has focused on the role of multinational enterprises (MNEs) and foreign direct investment (FDI) in generating spillovers. MNEs have a multi-market presence in the export market and are thus a natural conduit for information about foreign markets, foreign consumers and foreign technology. FDI can have spillover effects on firms in the host country through direct and indirect technology transfers. Proximity to foreign firms is likely to result in imitation by local firms, resulting in skill upgrading and research and development (R&D) activities (Greenaway, Sousa & Wakelin, 2004). Local economies may also benefit from the competition arising from the entry of foreign firms (Kneller & Pisu, 2007). A more competitive market forces domestic firms to improve productivity, which can lead to greater export behaviour. While FDI reduces the sunk costs of entering foreign markets and positively affects the export propensity of recipient firms, it can also help the non-FDI recipient firms to overcome financial constraint through region-specific external economies. For example, these firms may gain access to transportation infrastructure and information about foreign consumers that MNEs or FDI recipients bring with them (Kemme, Nikolsko-Rzhevskyy, & Mukherjee, 2014).

The multifarious nature of spillovers makes them a natural subject for meta-analyses, as researchers try to gain an overall understanding of their economic impact. No less than eight meta-analyses on spillovers have been carried out to date, though these have all focused on productivity spillovers (Gorg and Strobl, 2001; Meyer and Sinani, 2009; Havranek and Irsova, 2010; Mebratie and van Bergeijk 2013; Irsova and Havranek, 2013; Iwasaki and Tokunaga, 2016; Demena and van Bergeijk, 2017; and Bruno and Cipollina, 2018).

This study also uses meta-analysis to study the effect of spillovers, but with a focus on how spillovers affect exports. Our final sample collects 3,291 estimated spillover effects from 99 studies. We proceed as follows. Section II explains the different empirical procedures we employ in our meta-analysis. Section III describes our data. Section IV presents our first set of results, reporting summary statistics and estimates of the overall impact of spillovers on exports. Section V presents our second set of results, focusing on meta-regression analysis and the role of data, estimation, and study characteristics in explaining differences in the estimated spillover effects. Section VI concludes.

II. METHODOLOGY

The sample of estimated effects. Our study aggregates the results of studies that estimate an "effect" of spillovers on export behavior. To be included in our meta-analysis, a study had to estimate something like the following regression equation:

(1) $$Exports = \alpha + \beta Spillovers + \sum_{k=1}^{K} \gamma_k Z_k + error,$$

where β is the effect of spillovers on exports and the Z_k are a set of control variables. (We discuss below how we handle the challenge of comparing spillover effects that use different measures of exports and spillovers.)

Our analysis focuses on three questions: (i) What is the mean, overall effect of spillovers on export behavior? (ii) Is the sample of estimated effects in the literature affected by "publication bias", the phenomenon by which certain estimates are censored because of their sign or lack of significance? and (iii), Are there data, estimation, and/or study characteristics that can explain why estimated effects differ across studies? In this section, we describe the methodology that we use to answer these questions.

Estimating the mean, overall effect. A simple approach to answering (i) is to average the sample of estimated effects. This is equivalent to using OLS to regress the estimated effects from the literature, $\hat{\beta}_i$, on a constant:

(2) $\hat{\beta}_i = \mu + \varepsilon_i$, $i = 1,2,\ldots,N$,

where N is the number of estimated spillover effects in the meta-analysis sample, and μ is the mean, overall effect of spillovers on export behavior. Ignoring publication bias and endogeneity for the moment, if our sample of estimated effects is a representative draw from a population of estimated effects, then the estimate of μ in Equation (2) will be unbiased and consistent.

While unbiased and consistent, simple averaging of the estimated effects will not be efficient. Some studies produce more precise estimates than others. Let SE_i be the standard error of the ith estimated effect. If all estimates come from a population with a single, true effect, so that the only source of variation in ε_i is proportional to sampling error -- i.e., $\mathrm{var}(\varepsilon_i) = (SE_i)^2\sigma^2$ -- then Weighted Least Squares (WLS) estimation of Equation (2) will produce an unbiased, consistent, and efficient estimate of μ, with the appropriate weight being the inverse of $(SE_i)^2$.[2]

However, many researchers believe that the assumption of a single, true effect is unrealistic. They would argue that spillovers have a range of effects on export performance, depending on any number of conditions and variables. As a result, researchers should aim for

[2] Strictly speaking, WLS will be unbiased only if the estimates of $\mathrm{var}(\varepsilon_i)$ are equal to their population values.

estimating the mean of the distribution of spillover effects on exports. This latter model is known in the meta-analysis literature as the "Random Effects" model. The single effect model is known as "Fixed Effects".[3]

Let τ^2 represent the component of the variance of ε_i that is due to differences in mean true effects. If we can assume that sampling error and variation in true effects are independent, and that the variance of ε_i is proportional to these two components, then $\mathrm{var}(\varepsilon_i) = [(SE_i)^2 + \tau^2]\sigma^2$. This leads to an alternative, "Random Effects" version of WLS, with the appropriate weight now being the inverse of $[(SE_i)^2 + \tau^2]$. We thus have two WLS estimators that we can use, depending on whether the "Fixed Effects" or "Random Effects" model is appropriate.

The two WLS models can be easily related to Equation (2) by dividing each term by the square root of the inverse of the respective weight, ω:

(3.a) $$\frac{\widehat{\beta}_i}{\omega_i} = \mu \cdot \left(\frac{1}{\omega_i}\right) + \frac{\varepsilon_i}{\omega_i}, \; i = 1,2,\ldots,N.$$

where

(3.b) $$\omega_i = \begin{cases} SE_i, & (FixedEffects1) \\ \sqrt{(SE_i)^2 + \tau^2}, & (RandomEffects1) \end{cases}.$$

OLS estimation of this transformed equation produces estimates equivalent to WLS. Additionally, when the meta-analysis sample consists of multiple estimates from the same study, it is standard practice to correct for non-independence of the error terms by using cluster robust standard errors.

Note that the "Random Effects" WLS estimator produces a more uniform distribution of weights than "Fixed Effects", since the weighting terms include a common constant, τ^2. Further, when τ^2 is large relative to $(SE_i)^2$, the weights will be approximately equal across

[3] This nomenclature is unfortunate, given the association of these same terms with panel data estimation. Nevertheless, given their ubiquitousness in the meta-analysis literature, we will perpetuate the practice of using "Fixed" and "Random Effects" to refer to models of homogeneous and heterogeneous effects, respectively.

observations, so that WLS will produce estimates close to OLS. While researchers generally agree that the "Random Effects" model most closely matches reality, there is some debate about which works best in practice (Doucouliagos & Paldam, 2013; Reed, 2015). Accordingly, our analysis uses both.

A related issue concerns the weighting of estimates versus studies. The number of estimates per study can vary widely. In our sample, the number of estimates per study ranges from 1 to 204, with a mean of 31.[4] The WLS estimators above implicitly give greater weight, sometimes dramatically so, to studies with more estimates. Accordingly, we employ an alternative weighting system that, ceteris paribus, gives equal weight to studies rather than individual estimates:

$$\text{(3.c)} \quad \omega_i = \begin{cases} SE_i \cdot \sqrt{n_{i\in S}}, & (FixedEffects2) \\ \sqrt{(SE_i)^2 + \tau^2} \cdot \sqrt{n_{i\in S}}, & (RandomEffects2) \end{cases}$$

where $n_{i\in S}$ is the number of estimates in study *S* from which estimate *i* was taken.

Next we address the problem that arises when studies use different measures for exports and spillovers. When this happens, estimated effects are not directly comparable, despite the fact that the associated studies are all concerned with estimating the "same thing", the effect of spillovers on exports. The problem of pooling estimated effects whose numerical values are not directly comparable is common in meta-analyses.

There is a widely employed solution: transforming estimated coefficients to partial correlation coefficients (*PCCs*):

$$\text{(4.a)} \quad PCC_i = \frac{t_i}{\sqrt{t_i^2 + df_i}},$$

where t_i and df_i are the *t*-statistic and degrees of freedom associated with the respective estimated effect. The corresponding standard error is given by:

[4] This is partly explained by the fact that studies commonly use multiple spillover measures in the same regression.

(4.b) $SE(PCC_i) = \sqrt{\frac{1-PCC_i^2}{df_i}}$.

Examples of meta-analyses that use *PCCs* are Bruno and Cipollina (2018); Valickova, Havranek, and Horvath (2015); Arestis, Chortareas, and Magkonis (2015); Wang and Shailer (2015); Nataraj et al. (2014); Iwasaki and Mizobata (2018); Cohen and Tubb (2018); Bijlsma, Kool, and Non (2018); Churchill and Mishra (2018); Merkle and Phillips (2018); and Churchill and Yew (2017).

In terms of the preceding analysis, all that changes is that $\hat{\beta}_i$ is replaced by PCC_i, SE_i now stands for $SE(PCC_i)$, and τ^2 now represents the variance of *PCC* above and beyond sampling error. Accordingly, Equation (3) is replaced by:

(5) $$\frac{PCC_i}{\omega_i} = \mu \cdot \left(\frac{1}{\omega_i}\right) + \frac{\varepsilon_i}{\omega_i},\ i = 1,2,\ldots,N,$$

where

$$\omega_i = \begin{cases} SE_i, & (FixedEffects1) \\ \sqrt{(SE_i)^2 + \tau^2}, & (RandomEffects1) \\ SE_i \cdot \sqrt{n_{i\in S}}, & (FixedEffects2) \\ \sqrt{(SE_i)^2 + \tau^2} \cdot \sqrt{n_{i\in S}}, & (RandomEffects2) \end{cases},$$

and SE_i and τ^2 are redefined as above. In this specification, μ represents the mean true effect of spillovers on exports measured as a partial correlation.

While the transformation of estimated coefficients to *PCCs* solves the noncomparability problem, it raises the question of how one should interpret μ. In particular, what values of μ constitute a large effect? A small effect? Like any correlation, *PCC* takes values between -1 and 1. Cohen (1988) suggests that correlations of 0.10, 0.30, and 0.50 (in absolute value) should be interpreted as “small”, “medium” and “large” effects, and his interpretation is widely accepted. However, Cohen’s taxonomy refers to simple, not partial, correlations.

To investigate partial correlation sizes, Doucouliagos (2011) collected over 22,000 estimates in empirical economics and transformed them to *PCCs*. He then ranked them from smallest to largest in absolute value. He defined the 25th, 50th, and 75th percentile values as "small", "medium", and "large" effects. While there was some difference across subfields of economics, *PCC* values of 0.07, 0.17, and 0.33 corresponded to "small", "medium" and "large" effect sizes in the full sample. This establishes a scale for comparing *PCC* values to other *PCC* values in the literature, and it is the standard we employ in interpreting our empirical results.

Publication bias. Publication bias arises when the estimates reported by researchers and/or the studies published by journals comprise a biased sample of the population of all estimates. This can happen when researchers/journals have preferences for estimates that are statistically significant and/or whose signs accord with expectations (Christensen & Miguel, 2018). "Publication bias" can occur even in working papers that are not published in journals. This can happen if researchers choose not to write up results because the initial analyses did not produce interesting/promising results.[5] In that case, even unpublished working papers can be characterized by publication bias.

Publication bias represents a serious challenge to the validity of meta-analysis. If the estimates in the literature are disproportionately large and significant, then averaging them will preserve this bias, producing a distorted estimate of the mean true effect. Thus, it is important to test for the presence of publication bias.

The most common test for publication bias in the economics literature is the Funnel Asymmetry Test (FAT). The FAT is carried out by adding the standard error variable, *SE*, to the constant-only specification above (Card & Krueger, 1995; Egger et al., 1997; Stanley, 2008). In the context of Equation (5), this means estimating

[5] Franco, Malhotra, and Simonvits (2014) report that the main source of publication bias is failure of researchers to write up results that are not significant or interesting.

(6) $\frac{PCC_i}{\omega_i} = \mu \cdot \left(\frac{1}{\omega_i}\right) + \rho \cdot \left(\frac{SE_i}{\omega_i}\right) + \frac{\varepsilon_i}{\omega_i}$, $i = 1,2,\ldots,N$,

where

$$\omega_i = \begin{cases} SE_i, & (FixedEffects1) \\ \sqrt{(SE_i)^2 + \tau^2}, & (RandomEffects1) \\ SE_i \cdot \sqrt{n_{i \in S}}, & (FixedEffects2) \\ \sqrt{(SE_i)^2 + \tau^2} \cdot \sqrt{n_{i \in S}}, & (RandomEffects2) \end{cases}$$

Rejection of $H_0: \rho = 0$ is taken as evidence that publication bias exists.

The specification of Equation (6) has a further benefit. Including the publication bias term, $\left(\frac{SE_i}{\omega_i}\right)$, adjusts the estimate of μ for the presence of publication bias. In this sense, the inclusion of this term is akin to including an inverse Mills ratio in a regression equation to adjust for sample selection (Stanley and Doucouliagos, 2012). Thus, estimation of Equation (6) allows one to not only test for publication bias, but also provides a bias-adjusted estimate of the overall, mean true effect, μ.

<u>The relationship between estimated spillover effects and data, estimation and study characteristics</u>. In addition to estimating the mean overall effect and testing for publication bias, meta-analysis is also useful for identifying relationships between the estimated spillover effects and specific data, estimation and study characteristics. This enables an understanding of why different studies report different estimated effects. It can also be useful for identifying conditions and circumstances where spillover effects have their largest effects.

Let X_m, $m = 1,2, \ldots, M$, be a set of variables measuring characteristics of the data, estimation procedures and study that are suspected to affect the size of the estimated spillover effects. Their impacts can be estimated via a meta-regression analysis (MRA), in which the respective variables are added to the specification of Equation (6):

(7) $\frac{PCC_i}{\omega_i} = \mu \cdot \left(\frac{1}{\omega_i}\right) + \rho \cdot \left(\frac{SE_i}{\omega_i}\right) + \sum_{m=1}^{M} \delta_i \left(\frac{X_{mi}}{\omega_i}\right) + \frac{\varepsilon_i}{\omega_i}$, $i = 1,2,\ldots,N$,

where

$$\omega_i = \begin{cases} SE_i, & (Fixed\ Effects1) \\ \sqrt{(SE_i)^2 + \tau^2}, & (Random\ Effects1) \\ SE_i \cdot \sqrt{n_{i\in S}}, & (Fixed\ Effects2) \\ \sqrt{(SE_i)^2 + \tau^2} \cdot \sqrt{n_{i\in S}}, & (RandomEffects2)) \end{cases}$$

Note that the publication bias term, $\frac{SE_i}{\omega_i}$, remains in the equation to correct for any publication bias that may exist.

A further advantage of MRA is that it allows one to explore the extent to which endogeneity affects estimates of spillover effects. Endogeneity can arise from simultaneity, sample selection and omitted variables. Some of the studies in our meta-analysis sample address endogeneity while others do not. By including variables in X_m to identify studies that attempt to correct for endogeneity bias, we can investigate whether these attempts resulted in larger/smaller estimated spillover effects.

III. DESCRIPTION OF DATA

Selection of studies. Our search for estimated effects followed the procedure outlined in Stanley et al. (2013). We employed two categories of keywords, "Export" keywords and "Spillover" keywords, and used the search engines Web of Science, Google Scholar, Scopus, JSTOR, EBSCO, ProQuest and RePEc.

The "Export" keywords consisted of "export", "trade", "export decision", "export propensity", "export intensity", "export share", "export performance" and "firm performance". The "Spillover" keywords consisted of "agglomeration", "urbanization", "localization", "external economies", "externality", "spillovers", "export spillovers", "FDI spillovers", "spatial spillovers", "geographical spillovers", "sectoral spillovers" and "industrial spillovers". We combined keywords from both categories when using the respective search engines. Our initial search yielded over 350 studies, including peer-reviewed journal articles, working papers, conference proceedings, doctoral dissertations and master theses.

We reduced the sample to 115 studies by eliminating any papers that did not satisfy the following inclusion criteria: (i) the study must be empirical, (ii) the dependent variable must measure export performance and (iii) the explanatory variables must include one or more spillover measures. Included in this sample were multiple versions of the same paper, usually a working paper and a published version. To avoid double counting, we only included the published version. This reduced the sample to 106 papers.

We further reduced the sample by eliminating studies where a spillover variable was specified in quadratic form or where it appeared as both a main and interaction effect. While marginal effects can be calculated in these cases, they require information that was not reported in the studies, such as the covariance of the respective coefficient estimates. Elimination of these observations reduced the sample to 99 studies and 3,359 estimated effects. Further analysis of this sample, described below, identified the existence of outliers. The elimination of outliers produced our final sample of 99 studies and 3,291 estimated effects.

Coding. Once the final sample was determined, a team of researchers coded various data, estimation and study characteristics that could affect the size of the estimated spillover effects. Two coders independently recorded the respective characteristics. Discrepancies were then noted and reconciled.

As noted above, the estimated spillover effects in our sample derive from studies that use many different measures of exports and spillovers. In order to investigate whether these differences can explain differences in the estimated spillover effects, we developed a set of variables to categorize differences in export and spillover measures. The remainder of this section describes the categories we used. It also provides examples for each category so that readers can better appreciate what the estimated spillover effects are actually estimating.

Measures of export performance. Among the measures that have been used by studies of export performance, some focus on the firm’s decision to export. Others focus on what market to export to, or what products to export, or what quantity to export. We group these export measures into two groups: categorical and continuous measures.

Examples of categorical measures include a binary measure indicating whether a firm has any exports; a dummy variable indicating whether a firm is a new exporter; a four-part classification indicating whether a firm is a permanent exporter, sporadic exporter, new exporter or non-exporter; and a dummy variable indicating whether the ratio of exports to sales is greater than 10%. Examples of continuous measures include the value of firm exports; the ratio of exports to sales; the number of markets the firm has exported to; export growth; export variety; exports per worker; and the share of a region’s exports to a given country, relative to GDP.

Spillover types. Researchers have posited many channels by which firms’ activities can spill over and affect the export performance of other firms. Two general categories of spillovers are “Regional” and “Industry”. Other spillovers are specifically associated with foreign-owned firms (“FDI”) or directly related to other exporters (“Exporters”). While these categories are further described below, it should be noted at the outset that the different categories are not mutually exclusive. A study could estimate a spillover effect that simultaneously belonged to all four categories.

TABLE 2 elaborates on the spillover types in our sample. “Regional/Localization” spillovers arise from the spatial concentration of firms in the same or related industries. Potential benefits of residing near other firms in the same industry include a shared labor pool that can facilitate the acquisition of specialized skills and/or knowledge and access to common suppliers. In contrast, “Regional/Urbanization” spillovers arise whenever there is a large concentration of firms from heterogeneous industries in the same area. Possible benefits here include sharing of a well-developed transportation infrastructure and a large labor pool with general human capital.

"Industry" spillovers can be grouped in several ways. "Industry/Horizontal intra-industry" and "Industry/Horizontal inter-industry" refer to spillovers from firms in the same or other industries that are engaged in economically related activities. "Industry/Backward" and "Industry/Forward" refer to vertical spillovers from buyers in downstream industries and from suppliers in upstream industries, respectively.

The bottom panel of TABLE 2 gives examples of some of the spillover measures used by the studies in our sample. Note again that a single spillover measure can simultaneously belong to more than one "type" of spillover.

Spillover measures. In addition to their "type", spillover variables differ in their units of measurement. Some studies measure spillovers by the "Value" of their exports. Examples include the ratio of the value of exports to the value of total shipments; the ratio of the value of exports for a given industry and firm type to the value of exports from the manufacturing sector; the total value of exports of a particular product type to a particular destination country; and the ratio of a firm's exports to its total production.

Other studies measure spillovers by counting the number of firms producing the spillovers ("Number"). Examples are the number of firms operating in the same region and industry; the ratio of the number of exporting firms to the total number of firms in a given region and industry; and the number of exporters shipping to the same destination. Still other studies measure spillovers in terms of "Employment". Employment can be counted as total employment in the same region and industry; total employment in other exporting firms located in the same region; the share of employment in exporting firms over the share of total employment; and the number of skilled workers in the same region and industry.

Another approach to measuring spillovers looks at the output ("Output") produced by firms, regions or industries. For example, to study horizontal FDI, one study calculates the ratio of the output of foreign firms in a given industry to the total output of that industry. Measures of backwards and forwards FDI are constructed similarly, as ratios of outputs across industries. We also classify as output-based a measure that sums squares of industry output shares. Another type of spillover focuses on technology and research and development (R&D) transfers. Examples include the share of R&D expenditures over sales; patent applications per capita; and the number of patent applications for a given region.

Beyond all these are an assortment of other measures that we aggregate under the category of "Other". Some of these include a dummy variable to indicate the presence of at least one large exporting firm in the locality; the share of intangible assets held by foreign firms; product-level Herfindahl concentration indices; and the share of MNE expenditures on wages and salaries over all firms' expenditures on wages and salaries.

The variety of these measures of exports and spillovers highlights the difficulty of combining estimates of spillover effects across studies. They underscore why it is necessary to transform the associated estimates into *PCCs* if we are to gain an overall understanding of the empirical literature on spillovers and exports.

IV. DATA ANALYSIS: Part 1[6]

Distribution of *PCC* values. Our initial dataset consisted of 3,359 estimates from 99 studies.[7] TABLE 4 reports descriptive statistics for the *PCC* values, along with the corresponding *t*-statistics and degrees of freedom ("*df*") from which they are calculated. The mean and median *t*-values for the full sample are 2.86 and 1.41, respectively. We shall discuss the relatively small value of the median *t*-statistic later. At this point, we highlight the minimum and maximum *t*-values of -669.8 and 279.1. These are extraordinarily large and raise concern about outliers. A

[6] All the data and code necessary to replicate the empirical analysis in this study are publicly available at Dataverse: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/SJ5HP1.
7 Bibliographic information for the 99 studies included in this meta-analysis is provided in a document entitled "Studies" that is posted at https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/SJ5HP1.

similar concern applies to the *df* variable. It has mean and median values of 241,063 and 18,930, with minimum and maximum values of 50 and 5,776,129.

The associated distribution of *PCC* values ranges from -0.984 to 0.994. It has mean and median values of 0.016 and 0.008. Large (absolute) values of *PCC,* along with large *df* values, are potentially a problem. These two variables impact the weights in the four WLS estimators, *FixedEffects1*, *RandomEffects1*, *Fixed Effects2*, and *RandomEffects2*, which are inverse functions of $SE_i = \sqrt{\frac{1-PCC_i^2}{df_i}}$. *PCC* values close to 1, especially when accompanied by large *df* values, can generate exceptionally large weights, so that a few observations with *PCC* values close to 1 or -1 can exert a dominating influence. Accordingly, we proceed by truncating the top and bottom 1% of *PCC* values, leaving 3,291 observations. The truncated distributions of *t*-statistic, *df*, and *PCC* values are also reported in TABLE 4, immediately to the right of the full sample statistics. Corresponding histograms for the *t*-statistics and *PCC* values are presented in FIGURE 1. The two histograms in FIGURE 1 and the corresponding columns in TABLE 4 go far in answering our first question about the size of the effect of spillover effects on exports.

The mean and median *PCC* values for the truncated sample are 0.016 and 0.008. These are far away from the threshold value of 0.07 that Doucouliagos (2011) sets for "small". This suggests that spillovers have, at best, a positive but negligibly small effect on exports.

The reasons for the small *PCC* values are not hard to identify. First, a large number of estimates in the literature are statistically insignificant. The table immediately below the histogram in the top panel of FIGURE 1 reports that 53.0% of all *t*-values lie between -2 and 2. Compounding these relatively low *t*-values are very large sample sizes. The distribution of *df* values for the truncated sample ranges from 57 to 5,776,129, with a median value of 18,933. If we calculate the *PCC* value that corresponds to the median *t* and *df* values using equation (4.a), we obtain a value for *PCC* equal to 0.010.

However, there are two caveats. First, the numbers in TABLE 4 and the values represented in FIGURE 1 are unweighted. So we need to re-compute our estimate of the mean true effect, μ, using the different weighting schemes described above. Second, the analysis ignores publication bias.

Fixed Effects or Random Effects? While we are aware of no study that compares how frequently researchers use "Fixed Effects" versus "Random Effects" estimators in their meta-analyses, our sense is that "Fixed Effects" is generally preferred. TABLE 5 identifies our concern with "Fixed Effects". It calculates a "study weight" for each study in our sample, weighting the individual estimates of that study by the respective weighting scheme ("Fixed Effects"/"Random Effects") and then aggregating the weights at the study level. In this way, each study receives a weight, the sum of which equals 100%.[8]

If the 100% weight was divided equally across studies, given 99 studies, each study would receive a weight of 1.01%. Against this benchmark, "Fixed Effects" weights are highly skewed. The median weight is 0.03%, and the maximum weight for a single study is 42.6%.[9] The top 3 studies account for 62.9% of the total weight, and the top 10 studies comprise 90.0%. Thus "Fixed Effects" estimates will be disproportionately influenced by a very small number of studies that have large *PCC* values and/or use a large number of observations (*df*). This is particularly concerning if the export and spillovers measures used by this small set of studies is not representative of the literature.

In contrast, as noted above, the "Random Effects" estimator weighs estimates more uniformly - the median value is 1.14% compared to a mean value of 1.01%. The maximum weight any single study receives is 1.31%, and the top 10 studies account for 13.0%. While we report both "Fixed Effects" and "Random Effects" estimates, our preferred estimator is the latter.

[8] Study weights were calculated by $w_i / \sum w_i$, where $w_i = 1/(SE_i)^2$ or $w_i = 1/[(SE_i)^2 + \tau^2]$ depending on whether Fixed Effects or Random Effects were being used (cf. Rinquist, 2013, page 128).

[9] The ID for this study is 3. Its large weight is a function of its exceptionally large sample size (over 4,000,000 observations) and small *t*-values (cf. Equations 4.a and 4.b).

Publication bias. A common, informal tool for identifying publication bias is the "funnel plot". The funnel plot graphs estimated effect sizes (here, *PCC* values) against their respective standard errors (*SE*). A common representation is given in FIGURE 2. On the horizontal axis is the *PCC* value and on the vertical axis is its standard error (*SE*).

Note that the value at the top of the vertical axis is zero, so that *SE* values increase as one moves down the vertical axis. The vertical line extending upwards from the horizontal axis identifies the sample mean *PCC*, and the inverted "V" identifies the 95% confidence interval for *PCC* values that is estimated to arise from sampling error. For a given *SE* value, the associated confidence interval is given by a horizontal band extending from the left side of the 95% region to the right side. As *SE* increases, the length of the band likewise increases, so that it is narrow at the top of the funnel and wide at the bottom.

Publication bias introduces asymmetry to the funnel plot. For samples with large error variances, sampling error will produce a wide range of effect estimates. This creates an opportunity for researchers to selectively report estimates that are larger in absolute size, and hence more likely to get published. This can be intentional on the part of the researcher, but it can also occur unintentionally, as sampling error causes some researchers to get large and significant estimates, while others get small and insignificant estimates. The former get published and the latter do not, generating publication bias in the literature and introducing asymmetry to the funnel plot.

Two things are noteworthy from the funnel plots in FIGURE 2. First, rather than having the distribution of estimates narrow to a point as *SE* approaches zero, the top of the distribution is flat and spread out. This indicates that there is no single, true spillover effect, but rather a distribution of true spillover effects. This is a further argument for favoring the Random Effects over the Fixed Effects model. Second, there is evidence of positive publication bias. A disproportionate number of *PCC-SE* pairs reside to the right of the 95% confidence area.

As noted above, a formal test of publication bias is provided by the FAT. Rejection of $H_0: \rho = 0$ in Equation (6) is taken as evidence that publication bias exists. TABLE 6 reports the results of estimating Equation (6) using the four different WLS estimators, *FixedEffects1*, *FixedEffects2*, *RandomEffects1*and *RandomEffects2*. The first row reports the estimates of ρ, which is the coefficient on the publication bias term, *SE*. Across all four columns, we reject $H_0: \rho = 0$ at the 5 percent level of significance, indicating the existence of publication bias. The sign of the estimate indicates positive publication bias, suggesting sample selection that favors the publication of positive spillover effects on exports.

Estimates of the mean overall effect of spillovers on exports. The second row of TABLE 6 reports estimates of the constant term, representing μ, the mean overall effect of spillovers on exports, corrected for publication bias. In three of the four cases, the estimates of μ are significant at the 5 percent level. The exception is the *FixedEffects1* estimate of Column (1), which is significant at the 10 percent level. These results provide evidence that spillovers exist and they positively impact exports. However, the sizes of the estimates indicate that this effect is very small. The estimated effects range from 0.004 to 0.021, substantially below the 0.07 value that Doucouliagos (2011) associates with "small".

Columns (5) and (6) in TABLE 6 report the weighted average estimates of the mean true effect, uncorrected for publication bias, using the *RandomEffects1* and *RandomEffects2* estimators. The associated estimates are 0.015 and 0.026, which are close to the unweighted value of 0.016 reported in TABLE 4. These fall to 0.012 and 0.021, respectively, when *SE* is added to the specification to control for publication bias. Thus, while publication bias positively inflates estimates of the effect of spillovers on exports, it does not inflate them very much.

Comparison with Other Meta-Analyses of Spillover Effects. While this study is the only meta-analysis to investigate the relationship between spillovers and exports, no less than eight other meta-analyses have studied various aspects of the relationship between FDI and productivity spillovers (Bruno and Cipollina, 2018; Demena and van Bergeijk, 2017; Iwasaki and Tokunaga, 2016; Mebratie and van Bergeijk 2013; Irsova and Havranek, 2013; Havranek and Irsova, 2010; Meyer and Sinani, 2009; and Gorg and Strobl, 2001). For the most part, these studies have emphasized the influence of data and study characteristics on estimated spillover effects. For example, a common finding is that cross-sectional data produce larger spillover estimates than panel data. This emphasis on significant data and study characteristics has tended to obscure the fact that all of the studies find very small spillover effects.[10] TABLE 7 summarizes the estimates of mean, estimated spillover effects of FDI on productivity. Whether measured by *t*-stats, *PCC* values, or something else, the economic effects can be characterized as negligible. Our finding that spillovers do not have an important economic effect on exports is thus in line with what other studies have found with respect to productivity spillovers from FDI.

V. DATA ANALYSIS: Part 2

Data, Estimation, and Study Characteristics. In this section, we investigate the extent to which various data, estimation and study characteristics are correlated with the estimated spillover effects in our sample. TABLE 8 describes the variables we created for this meta-regression analysis (MRA). In a few cases, we do not actually use a variable because there were insufficient observations in a category or because it was held out as the benchmark, but we report it to give a better understanding of the data. The variables omitted from the MRA are indicated by an asterisk. We generally do not have expectations about the coefficient signs of the respective effect estimates. Accordingly, the subsequent analysis should be interpreted as exploratory.

Firm-level indicates that the estimated spillover effect comes from a regression that

[10] Havranek and Irsova (2010), Iwasaki and Tokunaga (2016), and Irsova and Havranek (2013) are exceptions in that they make it clear that the estimated spillover effects are small in economic significance.

used firm-level data. The alternative is aggregated data, where the unit of analysis is a geographical jurisdiction, such as a state or region, or an industry aggregate. Almost all of our estimated spillover effects (92.3%) come from firm-level data. *Domestic* indicates that the estimated spillover effect focuses on domestic firms, as opposed to foreign, or a mix of foreign and domestic firms.

SampleYear measures the age of the dataset that was used to estimate the spillover effect. Country and regional variables (*OECD, EU, Developing, China*) were created to indicate the geographical source of the estimated effects. The categories are not mutually exclusive. For example, a study that used data from the EU would also be categorized as using data from the OECD. For this reason, and in order to focus on differences between OECD countries, China and the rest of the world, the MRA drops *EU* and *Developing.*

Categorical indicates that the export measure used in the original study was based on a discrete number of categories, as opposed to being continuous. We created variables to correspond to the four spillover types from TABLE 2 (*Exporters, Region, Industry,* and *FDI*) and the six spillover measures from TABLE 3 (*Number, Value, Employment, Output, R&D, and OtherMeasures*). Only the latter set of variables are mutually exclusive. Because the categories *Output* and *R&D* seldom appear in our sample, these, along with *Other Measures*, are held out as benchmark categories.

We created five categories to indicate industry-specific spillover effects: *Manufacturing, Service, IT, Food* and *Other Industry*, where *OtherIndustry* includes studies that spanned multiple industries. The categories are mutually exclusive. We omit *Service, IT,* and *Food* because they occur infrequently. Accordingly, our MRA only includes an industry variable for manufacturing, with all other industries constituting the omitted category. Estimation methods were categorized in three mutually exclusive categories: *Probit/Logit/Tobit, OLS/GLS* and *OtherEstimation,* with the latter two combined to form the comparison group.

Of particular interest is whether studies corrected for endogeneity. Endogeneity could arise either from simultaneity, sample selection or omitted variables. Accordingly, we created dummy variables if the estimated effect used an estimation procedure that addressed these: *IV*, *SampleSelection* and *FixedEffects*, respectively. If we are willing to argue that simultaneity and sample selection generate a positive bias (e.g., firms that export more are also likely to be located in areas with greater spillovers), then correcting this bias should result in smaller estimates. In the case of panel fixed effects correcting for omitted variables, it is not possible to sign the bias without further knowledge about the omitted, time-constant variables.

We also created variables to indicate commonly-included control variables, as these could also affect the spillover effects estimated by studies. We have dummy variables that indicate that a study controlled for firm size, firm productivity, labor quality, capital/assets and R&D expenditures. Lastly, we have variables that indicate various facets of study quality: whether the study was published in a peer-reviewed journal (*Journal*), the impact factor of the journal where the study was published (*Impact*) and the number of Google Scholar citations the study received (*Citations*).

Bayesian Model Averaging (BMA) Analysis. A straightforward approach to identify data, estimation and study characteristics that correlate with estimates of spillover effects is to put all the variables in a single regression equation, such as the MRA specification of Equation (7), and estimate the respective coefficients. We will do this. However, the problem with this approach is that multicollinearity among the variables can mask important relationships. Accordingly, we use two additional approaches.

The first approach is Bayesian Model Averaging, or BMA (Zeugner, 2011). Conceptually, BMA consists of estimating all possible regressions and averaging the associated coefficient and standard error estimates with weights determined by the likelihood values of the respective specifications. In our case, there are a total of 27 variables (the 26 variables plus *SE*), producing 2^{27} possible regressions. Rather than estimating all of these, BMA samples from the set of all possible specifications using Monte Carlo Markov Chain (MCMC) sampling. True to its Bayesian nature, BMA requires the user to specify a prior distribution. This typically specifies prior beliefs about the number of variables that belong in the "true" regression equation.

The advantage of BMA is that it provides a global assessment of the relationship between the explanatory variables and the dependent variable (the estimated spillover effect). The disadvantage is that the results do not represent any single specification. This can be a problem when interpreting coefficients for dummy variables. For example, we use three variables to represent spillover measures, *Number, Value* and *Employment*. The interpretation for *Number* when *Value* and *Employment* are included in the specification is different than when *Value* and *Employment* are omitted from the specification, because the comparison group changes. This is concerning given that so many of the data, estimation and study characteristics are dummy variables. Accordingly, we also use a frequentist approach to estimate a "best" variable specification of Equation (7) as an alternative method for identifying factors that affect spillover effect estimates.

TABLE 9 reports the results of two BMA analyses, using the *FixedEffects1* and *RandomEffects1* weightings. Four outputs for each BMA analysis are reported: *PIP, Cond. Mean, Cond. SD,* and *Cond Pos Sign. PIP* can be roughly interpreted as the weighted probability that the given variable belongs in the "true" specification. A *PIP* value of 1.000 suggests that the probability the variable belongs in the "true" specification is approximately 100%. Another way to look at the *PIP* is to note that each variable appears in half of the 2^{27} total possible specifications. If each regression had an equal probability of being true (i.e., equal likelihood values), then the *PIP* would be 0.50. Thus, values greater than 0.50 indicate that the regressions including the respective variable have a higher probability of being "true" than the regressions that do not include the variable.

Cond Mean reports a weighted average of that variable's estimated coefficients, with weights calculated as the posterior probability that a given specification is true. Similarly, *Cond SD* is the weighted average of that variable's estimated standard errors. *Cond Pos Sign* is the weighted average of indicator variables that take the value 1 if the variable's coefficient in a given regression is positive. A *Cond Pos Sign* value of 1.000 suggests that the probability the given variable has a positive coefficient in the "true" specification is virtually 100%.

TABLE 9 maintains the same order of variables as reported in TABLE 8. The left half of the table presents results for regressions using the *FixedEffects1* weights, while the right hand side shows results using the *RandomEffects1* weights. Variables having *PIP* and *Cond Pos Sign* values both equal to 1.000 are yellow-highlighted for each set of weighted regressions. Variables having *PIP* values equal to 1.000 and *Cond Pos Sign* values equal to 0.000 are highlighted in rose. A compelling result would be one where (i) *PIP* was equal to 1.000 in both the *FixedEffects1* and *RandomEffects1* regressions, (ii) the signs were consistently positive or negative (*Cond Pos Sign* either 1.000 or 0.000), (iii) the *Cond Mean* values were at least twice as large as the *Cond SD* values and (iv) the *Cond Mean* value was economically meaningful using the Doucouliagos (2011) guidelines.

There are no variables that satisfy all four criteria. Variables that satisfy the first three criteria are *SampleYear, China, Fixed Effects,* and *R&D.* The *Cond Mean* estimate for *SampleYear* (-0.001 for both *FixedEffects1* and *RandomEffects1*) indicates that a dataset the mid-sample "age" of which was 10 years older than another dataset would have, on average, an estimated spillover effect that was approximately 0.01 correlation points lower. This is very small in absolute size, but is roughly comparable in size to the bias-adjusted estimate of the mean overall spillover effect (cf. Columns 1-4 in TABLE 6).

The only other variable that has a consistently negative effect is *FixedEffects,* indicating that the associated spillover effect was estimated using panel fixed effects. The negative sign means that studies that used panel fixed effects generally had smaller estimates of spillover effects than other studies (cross-sectional studies and panel studies without fixed effects).

Studies that used Chinese data or that measured spillover effects from R&D expenditures generally estimated larger spillover effects. In the case of China, the associated partial correlations are 0.010 (*Fixed Effects1*) and 0.013 (*RandomEffects1*) larger. For studies focusing on R&D, the corresponding estimates are 0.022 (*FixedEffects1*) and 0.012 (*RandomEffects1*) larger.

Information for the remaining variables is reported in the table. We note that weighting makes a difference. The variable *Employment*, indicating that the measure of spillovers was based on labor, has a *PIP* of 1.000 and a *Cond Pos Sign* of 1.000 in the *FixedEffects1* regressions. In contrast, it has a consistently negative sign in the *RandomEffects1* BMA regressions (*Cond Pos Sign* equals 0.000 with a *PIP* of 1.000).

As these results are exploratory, they should be viewed as suggestive. If there is one main conclusion to be drawn from the BMA analysis, it is that no data, estimation or study characteristics achieves even a small effect on estimates of spillover effects, where we define small using Doucouliagos' (2011) guideline of 0.07.

<u>WLS estimation of selective specifications</u>. This section complements the BMA analysis above by using WLS to estimate Equation (7) for selected variable specifications. One of the variable specifications includes all variables in the same specification. We supplement this with another specification that uses stepwise regression to select the "best" set of additional variables to accompany the spillover type and measure variables. In particular, we lock in the spillover type and/or spillover measure variables and then use a backwards stepwise regression procedure that sequentially chooses the control variables that result in the lowest BIC/SIC

value. BIC/SIC is an information criterion measure that balances goodness of fit against model parsimony. It has the property that it is asymptotically consistent. Thus, our backwards stepwise algorithm is designed to select the additional variables that are most likely to belong in the true equation along with the different spillover variables.

TABLE 10 locks in the four spillover type variables, *Exporters, Region, Industry,* and *FDI*. TABLE 11 locks in the three spillover measure variables, *Number*, *Value* and *Employment*. TABLE 12 combines both spillover type and spillover measure variables. In addition, we also lock in the publication bias term, *SE*, in all specifications. We do this to investigate whether evidence of publication bias is sustained after other variables are added to the specification.

The results from TABLES 10-12 were largely foreshadowed by the BMA analysis of TABLE 9. Of the seven spillover variables, none are consistently signed and significant across all estimation procedures and variable specifications. *Employment* is generally significant, but it switches signs from positive (*FixedEffects*) to negative (*RandomEffects*), depending on the WLS weights. In addition, none of the estimated coefficients achieves economic significance, as the estimated coefficients are all less than 0.07.

Finally, we note that our previous finding of publication bias becomes suspect in light of the estimates from TABLES 10-12. While the publication bias term, *SE*, is statistically significant across all four weighting schemes in TABLE 6, it is never significant in the *RandomEffects* regressions. This is evidence that the significant *SE* coefficients in Columns (3) and (4) of TABLE 6 were illusory, generated by correlation with omitted data, estimation and study variables.

VI. CONCLUSION

This study uses meta-analysis to investigate the effect of spillovers on export performance. Exports have been linked to many positive economic outcomes, such as growth, employment, technology improvements and consumer welfare. Spillovers have likewise received considerable attention. Despite the very large literature on spillovers, including no less than eight meta-analyses, this study represents the first meta-analysis to investigate the relationship between spillovers and exports.

Our final sample consists of 3,291 estimated spillover effects from 99 studies, making it substantially larger than any previous meta-analysis of spillover effects. Our main finding is that spillovers have an economically negligible impact on exports. This conclusion follows directly from the fact that approximately half of the estimated spillover effects in the literature are statistically insignificant. This insignificance is particularly noteworthy given that the sample sizes of the underlying studies are generally very large. The mean and median sample sizes for the estimated effects in our sample are 245,911 and 18,933, respectively. The combination of insignificant estimates with very large sample sizes is indicative that spillovers do not have much effect on exports.

Two other findings from our study are noteworthy: While we find evidence of publication bias using the standard funnel asymmetry test (FAT) for publication bias, this result disappears when additional variables are included in some of the regressions. This is suggestive that the estimated publication bias in the FAT may simply reflect omitted variable bias. In any case, the size of the estimated publication bias has a very small effect on the overall estimate of spillover effects.

Our last finding is that we are unable to obtain compelling evidence that data, estimation and study characteristics affect estimated spillover effects. In particular, there is no robust evidence that estimated spillover effects are affected by how exports are measured, nor that some spillover types have larger effects than others. While some of the respective variables are statistically significant in some of the regressions, neither the Bayesian Model Averaging analysis nor the frequentist regression results produce robust results linking data, estimation and study characteristics to spillover effects.

It turns out that our results should not be surprising. Reviewing previous meta-analyses of spillover effects reveals that they also find economically insignificant effects from spillovers. This is somewhat obscured by the fact that, in many cases, previous studies have focused on statistical, rather than economic, significance. That is, they report that the overall mean effect of spillover effects is statistically significant, or that various data, estimation and study characteristics are significant, without commenting or discussing on the size of the effects. Low partial correlation coefficients and large numbers of insignificant effect estimates are, in fact, characteristic of the spillover literature.

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TABLE 1
Selected Measures of Export Performance

Categorical
• The decision whether firm exports or not in year t • The decision to start exporting to country j • Dummy = 1 if domestic firms in province i begin exporting product k to country j • Export status: permanent exporter, sporadic exporter, new exporter, non-exporter • Internationalisation modes: non-exporter, exporters, firm that exports and engages in horizontal FDI • Status: continue non-exporting, start exporting, continue exporting, exit from exporting • Export dummy = 1 if a firm's export share (direct exports over sales) ≥ 10%
Continuous
• The export scale of firm i for product k to country j at time t • Firm's export value • The ratio of export value to total sales • The share of firm i's exports in industry j • Firm's export volume • Export intensity – the number of macro areas that the firm has served through its exporting activity • Export intensity – the percentage of a firm's exports in output • Growth of exports • Export value / export quantity / export price / export quality • Export variety – the number of HS8 products exported by firm i at time t • Unit value of product j exported by firm i at time t • Export per worker • The share of state i's aggregate exports to country j in GDP

TABLE 2
Examples of Spillover Types

Regional
• Localization – spillovers from spatial agglomeration of related firms • Urbanization – spillovers from urban concentration that apply to all firms and industries in a specific region
Industry
• Horizontal intra-industry – spillovers from firms within the same group of companies within industry • Horizontal inter-interindustry – spillovers from other firms within industry • Backward – spillovers from other firms in industry *j* purchasing intermediate goods from industry *k* where firm *i* is located • Forward - spillovers from other firms in industry *j* supplying intermediate goods to industry *k* where firm *i* is located
FDI / Exporter
• Spillovers that flow from foreign-owned firms / other exporter firms
Examples
• The total number of manufacturing firms in the region • R&D expenditure by domestic firms in sector *j* / sales by domestic firms in *j* • The number of exporting firms outside region that are exporting to market *j* belonging to a different 2-digit industry than firm *i* • The number of other firms in the region operating in the same industry • Regional output of MNEs to domestic markets • The total number of export firms in the region (outside the industry of the firm in focus) • The ratio of foreign-owned firms over total number of firms in the same industry • The number of exporting firms within the same industry but outside region • The share of FDI investment in a certain region-industry • The share of exporters in area, same industry – same destination • The share of exports by foreign firms in total exports in an industry • The share of exports by foreign firms in total exports in a province • The share of exports by foreign firms in total exports in an industry within a province • (Exports by MNEs in sector j / total exports in j) / (total exports by MNEs / total exports) • Exports value / total shipments from plants in the state and in the SIC4 industry • The share of exporting employment in area, all industries – same destination

TABLE 3
Selected Spillover Measures

Value
• Exports value / total shipments from plants in the state and outside the SIC4 industry • The province-industry-firm-type share of national industry exports / the province share of national manufacturing exports • Foreign exports from province *i* of product *k* to country *j* • The ratio of the export volume to total production value
Number
• The number of other firms in the region operating in the same industry • The number of exporting plants / total plants for plants in the state and in the SIC4 industry • The province-firm-type share of national establishments / the province share of national establishments • The number of exporters in area, all industries – same destination • A region's number of firms within the same industry as percentage of a country's total number of firms within the same industry • A region's number of direct and indirect exporters as percentage of a region's total number of firms
Employment
• Exporting employment in area, all industries – same destination • The share of exporting employment in area, all industries – same destination • The number of total employment in the same region and industry (all plants / exporting plants / foreign-owned but non-exporting plants) • The number of skilled workers in the same region and industry (all plants / exporting plants / foreign-owned but non-exporting plants)
Output
• $Horizontal\_FDI_{jt} = \frac{Y_{jt}^{f}}{Y_{jt}}$, Y_{jt}^{f} is the output of foreign firms in industry j, Y_{jt} is the total output of industry *j*. • $Backward\_FDI_{jt} = \sum_{\forall k \neq j} \alpha_{kjt} Horizontal\_FDI_{kt}$, $\alpha_{kj} = \frac{Y_{kj}}{Y_k}$ where Y_{kj} is the output provided from industry *j* to industry *k*. • $Forward\_FDI_{jt} = \sum_{\forall h \neq j} \beta_{hjt} Horizontal\_FDI_{ht}$, $\beta_{hj} = \frac{Y_{hj}}{Y_j}$ where Y_{hj} is the output provided from industry *h* to industry *j*. • The sum of squares of an industry's output share by region

R&D
• R&D expenditure by domestic firms in sector j / sales by domestic firms in j • R&D expenditure by MNEs in sector j / sales by MNEs in j • Patent applications per capita in a particular region • The number of patent applications in the region
Other
• If at least one other large exporting firm is present in the region • The presence of foreign-owned firms' capital stock in the total capital stock of an industry, a province and an industry within a province • The share of intangible assets held by foreign firms in fixed assets in an industry, in a province and in an industry within a province • Horizontal spillovers$_{jt} = (\sum_{i\in j} Foreign\ share_{it} * Y_{it})/(\sum_{i\in j} Y_{it})$, $Foreign\ share_{it}$ is the share of foreign fixed capital stock in a foreign-invested enterprise (FIE) *i* at time *t*, Y_{it} is the total output of the same FIE at time *t*. • Backward spillovers$_{jt} = \sum_{k\neq j} \alpha_{jk} Horizontal_{kt}$, α_{jk} is the proportion of industry *j*'s output supplied to industry *k*. • Forward spillovers$_{jt} = \sum_{m\neq j} \varphi_{jm}[[\sum_{i\in m} Foreign\ share_{it} * (Y_{it} - EX_{it})] / [\sum_{i\in m}(Y_{it} - EX_{it})]]$, φ_{jm} is the share of inputs purchased by industry *j* from industry *m* in total inputs sourced by industry *j*, EX_{it} is the export value of FIE *i* at time *t*. • Aggregate shipping weight of exports from each region to each country • Related variety $= \sum_{g=1}^{G} P_g(\frac{1}{H_g})$, H_g is the Herfindahl concentration index calculated at the five-digit level within each two-digit level, P_g is the employment share of sector *g*. Sector *g* is located in the same region and related to sector *s* where firm *i* is located. • Urbanization $= \frac{LU_p}{Area_p}$, LU_p is the population in province *p* and $Area_p$ is the number of local units of the province area. • The average number of addresses per square kilometer within a circle of a one-kilometre ray to measure agglomeration • Share of MNEs' expenditures on wages and salaries on total expenditures on wages and salaries of the sector. • The ratio of foreign equity invested to total equity invested in the industry.

TABLE 4
Descriptive Statistics for Effect Size Variables

	t-Statistics		*df*		*PCC Values*	
	Full	***Truncated***	***Full***	***Truncated***	***Full***	***Truncated***
Mean	2.86	3.00	241,063	245,911	0.016	0.016
Median	1.41	1.41	18,930	18,933	0.008	0.008
Minimum	-669.8	-51.0	50	57	-0.984	-0.140
Maximum	279.1	147.4	5,776,129	5,776,129	0.994	0.199
Std. Dev.	16.5	9.0	657065	662,943	0.063	0.040
1%	-14.24	-12.61	115	119	-0.148	-0.083
5%	-3.01	-2.81	527	610	-0.040	-0.030
10%	-1.78	-1.71	1011	1137	-0.018	-0.016
90%	10.00	9.51	583,432	631,056	0.073	0.067
95%	18.04	17.00	1,683,178	1,685,285	0.109	0.101
99%	40.42	36.44	3,323,304	3,323,304	0.203	0.158
Obs	3,359	3,291	3,359	3,291	3,359	3,291

NOTE: The truncated sample is obtained from the Full Sample by deleting observations having the top and bottom 1% of *PCC* values.

TABLE 5
Study Weights

	Fixed Effects	*Random Effects*
Mean	1.01%	1.01%
Median	0.03%	1.14%
5%	0.00%	0.39%
10%	0.00%	0.42%
90%	1.78%	1.28%
95%	5.39%	1.30%
Maximum	42.6%	1.31%
Top 3	62.9%	3.9%
Top 10	90.0%	13.0%
Studies	99	99

NOTE: The methodology for calculating "study weights" is described in Footnote #8 in the text.

TABLE 6
Test of Publication Bias and Estimate of Mean Overall Effect

	Including Publication Bias Term				***Excluding Publication Bias Term***	
Variable	***FixedEffects1 (1)***	***FixedEffects2 (2)***	***RandomEffects1 (3)***	***RandomEffects2 (4)***	***RandomEffects1 (5)***	***RandomEffects2 (6)***
SE	1.724*** (2.73)	2.462*** (4.40)	0.348** (2.01)	0.397** (2.00)	----	----
Constant	0.004* (1.78)	0.004** (2.20)	0.012*** (3.59)	0.021*** (4.86)	0.015*** (5.68)	0.026*** (7.42)
Observations	3,291	3,291	3,291	3,291	3,291	3,291

NOTE: Estimates in Columns (1) through (4) come estimating Equation (6) in the text using Weighted Least Squares (WLS). The four WLS estimators (*FixedEffects1*, *FixedEffects2*, *RandomEffects1*, and *RandomEffects2*) are described in Section II. The estimates in Columns (5) and (6) come from estimating Equation (5). All of the estimation procedures calculate cluster robust standard errors. *, **, and *** indicate statistical significance at the 10-, 5-, and 1-percent level, respectively.

TABLE 7
Summary of Meta-Analyses on FDI and Productivity Spillovers

Meta-analysis	***#Studies / #Estimates***	***Measure of Effect Size***	***Evidence of Economic Insignificance***
Gorg and Strobl (2001)	21 / 25	*t*-stat	Mean *t*-stat is 1.63 and 2.00 (cf. Table 2)
Meyer and Sinani (2009)	66 / 121	*t*-stat	Predicted t-stats range from -1.5 to 1.5 (cf. Figure 4)
Havranek and Irsova (2010)	67 / 97	*t*-stat and *PCC*	Median *t*-stat is 0.4 (cf. Table 1)
Irsova and Havranek (2013)	52 / 1,205	Semi-elasticity	Mean elasticity is -0.002 (cf. Table 1)
Mebratie and van Bergeijk (2013)	30/ 130	Absolute *t*-stat	Large percentage of insignificant estimates, ranging from 24% to 71%, depending on the type of spillover (cf. Table 2)
Iwasaki and Tokunaga (2016)	30 / 625	*t*-stat and *PCC*	Mean *PCC* = -0.0007, Mean *t*-stat = -0.0047 (cf. Figure 2)
Demena and van Bergeijk (2017)	69 / 1,450	*t*-stat and original estimates	53% of the t-values are insignificant (cf. Figure 2). Mean t-stats are -0006 and 0.0004 (cf. Table 2).
Bruno and Cipollina (2018)	52 / 1,133	*PCC*	Mean *PCC* effect ranges from 0.001 to 0.024 (cf. Table 6)

NOTE: Authors' summary. Table and figure numbers refer to the tables and figures in the respective meta-analyses.

TABLE 8
Description of Variables

Variable	*Description*	*Mean*	*Min*	*Max*
	DATA CHARACTERISTICS			
Firm-level	=1, if data are firm-level	0.923	0	1
Domestic	=1, if spillover effects focus on domestic firms	0.468	0	1
SampleYear	Mid-point of sample period	2001.4	1988	2011
	COUNTRIES			
OECD	=1, if sample consists of data from OECD	0.467	0	1
*EU**	=1, if sample consists of data from EU	0.380	0	1
*Developing**	=1, if sample consists of data from developing countries	0.526	0	1
China	=1, if sample consists of data from China	0.230	0	1
	DEPENDENT VARIABLE			
Categorical	=1, if dependent variable used a discrete number of categories	0.680	0	1
	SPILLOVER TYPE			
Exporters	=1, if spillovers are from exporters	0.544	0	1
Region	=1, if spillovers are from same region	0.524	0	1
Industry	=1, if spillovers are from same industry	0.521	0	1
FDI	=1, if spillovers are from FDI	0.394	0	1
	SPILLOVER MEASURES			
Number	=1, if spillovers are measured by number of firms	0.349	0	1
Value	=1, if spillovers are measured by export value	0.250	0	1
Employment	=1, if spillovers are measured by employment	0.092	0	1

Variable	*Description*	*Mean*	*Min*	*Max*
*Output**	=1, if spillovers are measured by output	0.050	0	1
*R&D**	=1, if spillovers are measured by R&D expenditures	0.018	0	1
*OtherMeasures**	=1, if spillovers are measured by other variables	0.245	0	1
	INDUSTRY			
Manufacturing	=1, if data are from manufacturing industry	0.762	0	1
*Service**	=1, if data are from service industry	0.028	0	1
*IT**	=1, if data are from IT industry	0.026	0	1
*Food**	=1, if data are from food industry	0.022	0	1
*OtherIndustry**	=1, if data are from other industries	0.162	0	1
	ESTIMATION METHOD			
Probit/Logit/Tobit	=1, if estimation method is probit, logit, or tobit	0.724	0	1
*OLS/GLS**	=1, if estimation method is OLS or GLS	0.261	0	1
*OtherEstimation**	=1, if estimation method is none of the above	0.015	0	1
IV	=1, if estimation method uses instrumental variables	0.122	0	1
SampleSelection	=1, if estimation method corrects for sample selection	0.067	0	1
Fixed Effects	=1, if estimation method uses fixed effects	0.356	0	1
	CONTROL VARIABLES			
Size	=1, if specification controls for firm size	0.822	0	1
Productivity	=1, if specification controls for firm productivity	0.648	0	1
LaborQuality	=1, if specification controls for firm labor quality	0.428	0	1
Capital	=1, if specification controls firm capital/assets	0.303	0	1
R&D	=1, if specification controls for R&D expenditures	0.221	0	1

Variable	*Description*	*Mean*	*Min*	*Max*
	STUDY QUALITY			
Journal	=1, if study published in a peer-reviewed journal	0.839	0	1
Impact	RePEc impact factor of journal (April 2018)	0.150	0	4.93
Citations	Number of Google Scholar citations (April 2018)	67.8	0	2861

NOTE*:* When the grouped variables include all possible categories, the categories omitted in the subsequent analysis (the benchmark categories) are indicated by an asterisk.

TABLE 9
BMA Analysis

Variable	*FixedEffects1*				*RandomEffects1*			
	PIP	*Cond Mean*	*Cond SD*	*Cond Pos Sign*	*PIP*	*Cond Mean*	*Cond SD*	*Cond Pos Sign*
FirmLevel	0.700	-0.003	0.003	0.001	0.848	-0.004	0.004	0.000
Domestic	0.860	0.002	0.001	1.000	0.997	-0.005	0.002	0.000
SampleYear	1.000	-0.001	0.000	0.000	1.000	-0.001	0.000	0.000
OECD	0.478	0.000	0.002	0.938	0.900	0.003	0.002	1.000
China	1.000	0.010	0.002	1.000	1.000	0.013	0.002	1.000
Categorical	1.000	0.009	0.001	1.000	0.691	0.000	0.002	0.957
Exporters	1.000	0.009	0.002	1.000	0.987	0.004	0.002	1.000
Region	0.877	0.002	0.001	1.000	0.998	0.005	0.002	1.000
Industry	0.998	0.003	0.001	1.000	0.986	0.004	0.001	1.000
FDI	1.000	-0.013	0.001	0.000	0.700	0.000	0.001	0.001
Number	0.994	0.005	0.002	1.000	0.924	-0.004	0.002	0.000
Value	0.985	-0.004	0.001	0.000	1.000	-0.012	0.002	0.000

Variable	*FixedEffects1*				*RandomEffects1*			
	PIP	*Cond Mean*	*Cond SD*	*Cond Pos Sign*	*PIP*	*Cond Mean*	*Cond SD*	*Cond Pos Sign*
Employment	1.000	0.020	0.001	1.000	1.000	-0.014	0.003	0.000
Manufacturing	0.623	-0.001	0.001	0.000	0.939	-0.004	0.002	0.000
ProbitLogitTobit	0.981	-0.003	0.001	0.000	1.000	-0.007	0.002	0.000
IV	1.000	-0.007	0.002	0.000	0.851	-0.003	0.002	0.000
SampleSelection	0.583	-0.002	0.003	0.000	1.000	-0.015	0.003	0.000
FixedEffects	1.000	-0.006	0.001	0.000	1.000	-0.011	0.002	0.000
Size	0.715	-0.001	0.001	0.000	0.942	0.004	0.002	1.000
Productivity	0.516	0.000	0.001	0.006	0.715	0.001	0.002	0.989
LaborQuality	0.568	0.001	0.001	0.999	1.000	-0.015	0.001	0.000
Capital	0.994	0.007	0.002	1.000	0.997	0.006	0.002	1.000
R&D	1.000	0.022	0.003	1.000	1.000	0.012	0.002	1.000
Journal	1.000	0.015	0.002	1.000	0.999	0.007	0.002	1.000
Impact	0.595	0.002	0.003	0.950	0.999	-0.013	0.004	0.000
Citations	0.928	0.000	0.000	0.000	0.735	0.000	0.000	0.991

NOTE: The column headings *PIP*, *Post Mean*, *Post SD* and *Cond Pos Sign* stand for Posterior Inclusion Probability, Posterior Mean, Posterior Standard Deviation and the likelihood-weighted probability that the respective coefficient takes a positive sign. These are described in the "Bayesian model averaging of control variables" subsection of Section V in the text. The Bayesian Model Averaging (BMA) analysis was done using the R package BMS, described in Zeugner (2011). The WLS estimators *FixedEffects1* and *RandomEffects1* are described in Section II. The table yellow-highlights variables that (i) have a *PIP* equal to 100%; and (ii) have a *Conditional Positive Sign* of 1.000 (i.e., are consistently positive). Variables that (i) have a *PIP* equal to 100% and (ii) have a *Conditional Positive Sign* of 0.000 (i.e., are consistently negative) are highlighted in rose.

TABLE 10
Meta-Regression Analysis
(*Omitting Spillover Measures*)

Variable	*FixedEffects1* (1)	*FixedEffects2* (2)	*RandomEffects1* (3)	*RandomEffects2* (4)
		All Control Variables Included		
SE	1.611*** *(3.89)*	1.918*** *(4.47)*	0.176 *(0.90)*	0.294 *(1.57)*
Exporters	0.009** *(2.36)*	0.012*** *(2.65)*	0.005 *(1.08)*	0.005 *(0.75)*
Region	0.003 *(1.07)*	0.010*** *(2.69)*	0.006 *(1.37)*	0.015** *(2.16)*
Industry	0.003 *(0.72)*	0.001 *(0.18)*	0.004 *(1.20)*	0.004 *(0.72)*
FDI	-0.014 *(-1.29)*	-0.009 *(-0.85)*	-0.001 *(-0.12)*	-0.003 *(-0.50)*
		Control Variables Selected Via Backwards Stepwise Regression		
SE	1.606*** *(4.31)*	1.761*** *(4.21)*	0.197 *(1.02)*	0.302 *(1.59)*
Exporters	0.009** *(2.49)*	0.012** *(2.58)*	0.004 *(1.05)*	0.005 *(0.70)*
Region	0.002 *(0.81)*	0.010** *(2.55)*	0.005 *(1.18)*	0.014** *(2.18)*
Industry	0.003 *(0.67)*	0.001 *(0.30)*	0.003 *(1.00)*	0.005 *(0.79)*
FDI	-0.013 *(-1.37)*	-0.010 *(-1.09)*	-0.000 *(-0.03)*	-0.002 *(-0.30)*

NOTE: The top panel reports the results of estimating Equation (7) with the full set of data, estimation, and study characteristic variables (the 26 variables of TABLE 7 plus the publication bias variable *SE*). The bottom panel locks in the spillover type variables *Exporters, Region, Industry* and *FDI,* along with *SE,* and then uses a backwards stepwise regression algorithm to select the control variables that minimize the BIC/SIC information criterion. The top value in each cell is the coefficient estimate, and the bottom value in parentheses is the associated *t*-statistic. The four WLS estimators (*FixedEffects1, FixedEffects2, RandomEffects1,* and

RandomEffects2) are described in Section II in the text. All four estimation procedures calculate cluster robust standard errors. *, **, and *** indicate statistical significance at the 10-, 5-, and 1-percent level, respectively.

TABLE 11
Meta-Regression Analysis
(*Omitting Spillover Types)*

Variable	***FixedEffects1* (1)**	***FixedEffects2* (2)**	***RandomEffects1* (3)**	***RandomEffects2* (4)**
		All Control Variables Included		
SE	1.611*** *(3.89)*	1.918*** *(4.47)*	0.176 *(0.90)*	0.294 *(1.57)*
Number	0.005 *(1.62)*	0.003 *(0.55)*	-0.004 *(-0.80)*	-0.008 *(-0.91)*
Value	-0.004 *(-1.24)*	-0.010 *(-1.59)*	-0.013** *(-2.23)*	-0.014 *(-1.53)*
Employment	0.021*** *(4.02)*	0.014 *(1.57)*	-0.014** *(-2.33)*	-0.021*** *(-3.02)*
		Control Variables Selected Via Backwards Stepwise Regression		
SE	1.621*** *(4.35)*	1.800*** *(4.25)*	0.230 *(1.21)*	0.302 *(1.58)*
Number	0.006* *(1.77)*	0.002 *(0.42)*	-0.006 *(-1.01)*	-0.007 *(-0.88)*
Value	-0.003 *(-1.12)*	-0.010* *(-1.68)*	-0.013** *(-2.23)*	-0.014 *(-1.44)*
Employment	0.020*** *(4.17)*	0.015* *(1.68)*	-0.016** *(-2.52)*	-0.021*** *(-2.97)*

NOTE: The top panel reports the results of estimating Equation (7) with the full set of data, estimation, and study characteristic variables (the 26 variables of TABLE 7 plus the publication bias variable *SE*). The bottom panel locks in the spillover measure variables *Number*, *Value* and *Employment*, along with *SE*, and then uses a backwards stepwise regression algorithm to select the control variables that minimize the BIC/SIC information criterion. The top value in each cell is the coefficient estimate, and the bottom value in parentheses is the associated *t*-statistic. The four WLS estimators (*FixedEffects1*, *FixedEffects2*, *RandomEffects1*, and *RandomEffects2*) are described in Section II in the text. All four estimation procedures calculate cluster robust standard errors. *, **, and *** indicate statistical significance at the 10-, 5-, and 1-percent level, respectively.

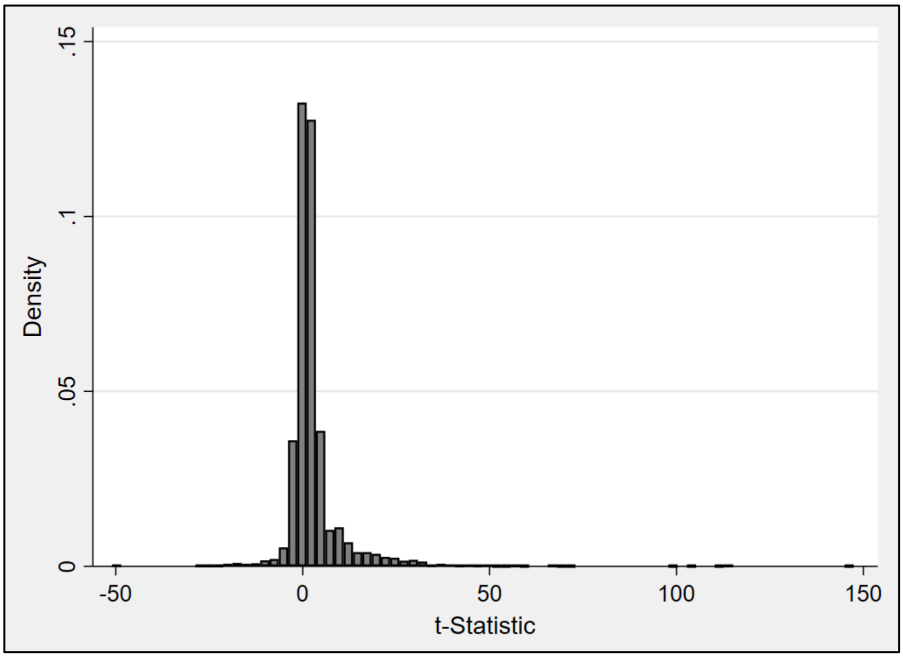
TABLE 12
Meta-Regression Analysis
(*Spillover Types and Measures Included)*

Variable	***FixedEffects1* *(1)***	***FixedEffects2* *(2)***	***RandomEffects1* *(3)***	***RandomEffects2* *(4)***
Control Variables Selected Via Backwards Stepwise Regression				
SE	1.606*** *(4.31)*	1.794*** *(4.23)*	0.231 *(1.20)*	0.303 *(1.59)*
Exporters	0.009** *(2.49)*	0.012*** *(2.83)*	0.006 *(1.33)*	0.005 *(0.70)*
Region	0.002 *(0.81)*	0.010** *(2.57)*	0.006 *(1.60)*	0.014** *(2.18)*
Industry	0.003 *(0.67)*	0.001 *(0.25)*	0.004 *(1.22)*	0.005 *(0.79)*
FDI	-0.013 *(-1.37)*	-0.010 *(-1.09)*	0.000 *(0.07)*	-0.002 *(-0.30)*
Number	0.005 *(1.58)*	0.002 *(0.39)*	-0.006 *(-0.97)*	-0.008 *(-0.89)*
Value	-0.004 *(-1.16)*	-0.010 *(-1.65)*	-0.013** *(-2.25)*	-0.013 *(-1.42)*
Employment	0.020*** *(4.10)*	0.014 *(1.66)*	-0.016** *(-2.50)*	-0.021*** *(-3.01)*

NOTE: The table reports the results of estimating Equation (7) by locking in both spillover type and spillover measure variables *Exporters*, *Region*, *Industry*, *FDI*, *Number*, *Value* and *Employment*, along with *SE*, and then using a backwards stepwise regression algorithm to select the control variables that minimize the BIC/SIC information criterion. The top value in each cell is the coefficient estimate, and the bottom value in parentheses is the associated *t*-statistic. The four WLS estimators (*FixedEffects1*, *FixedEffects2*, *RandomEffects1*, and *RandomEffects2*) are described in Section II in the text. All four estimation procedures calculate cluster robust standard errors. *, **, and *** indicate statistical significance at the 10-, 5-, and 1-percent level, respectively.

FIGURE 1
Distribution of t-and PCC Values

A. *t*-Statistics



Distribution of t-statistics	*Percent*
t < -2.00	7.7
-2.00 ≤ t ≤ 2.00	53.0
t > 2.00	39.3

B. PCC Values

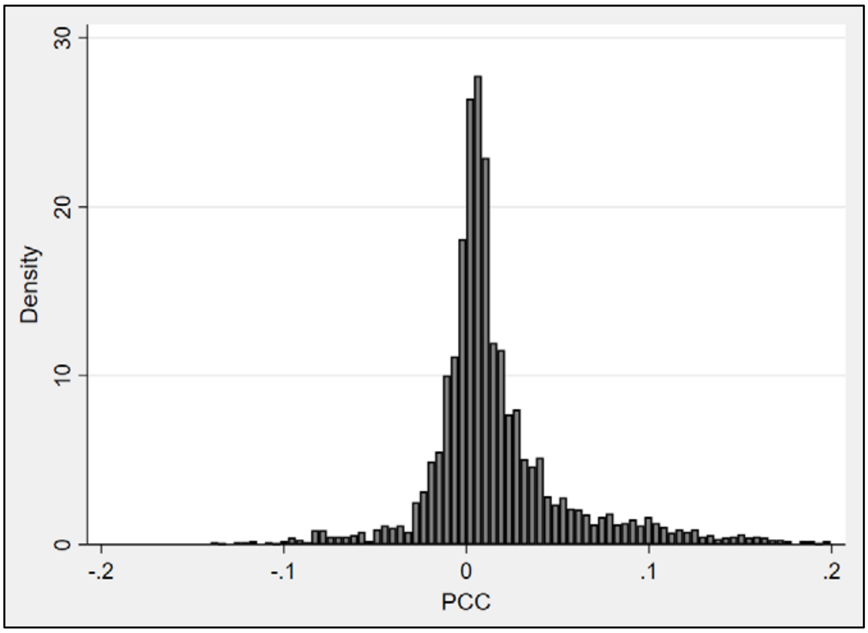
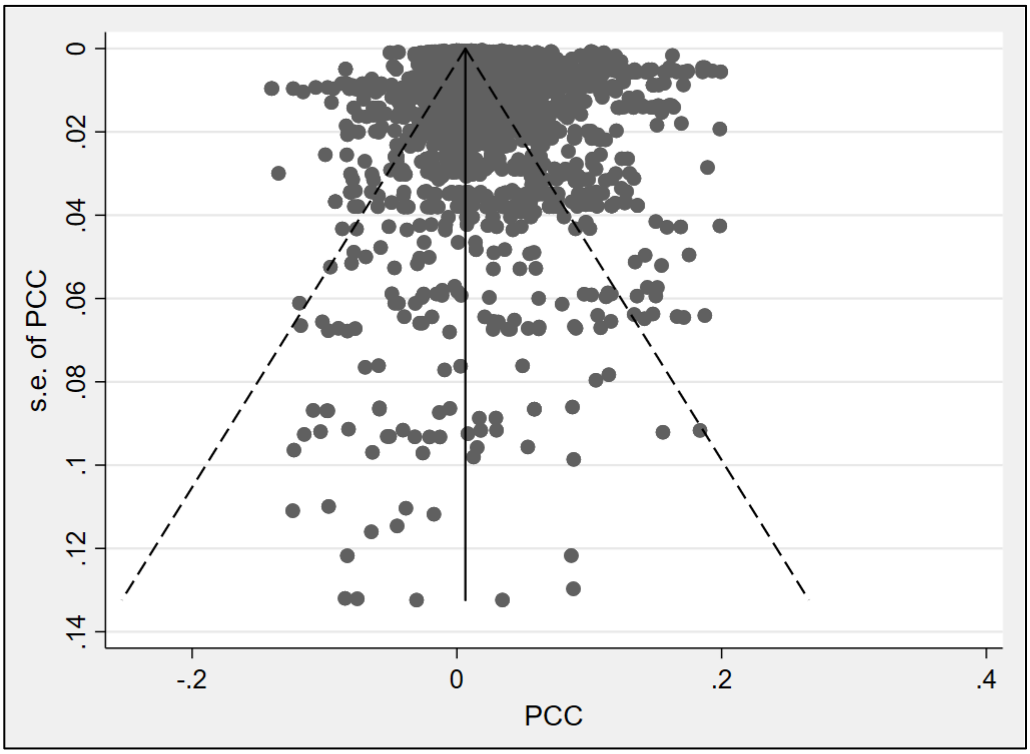
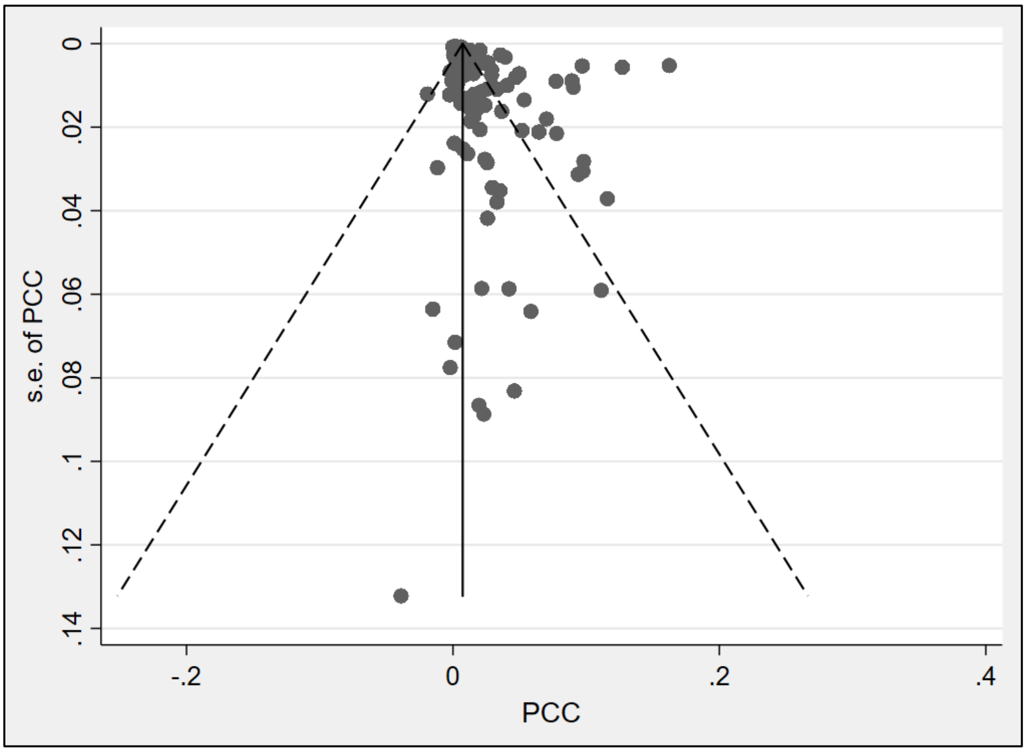


FIGURE 2
Funnel Plots

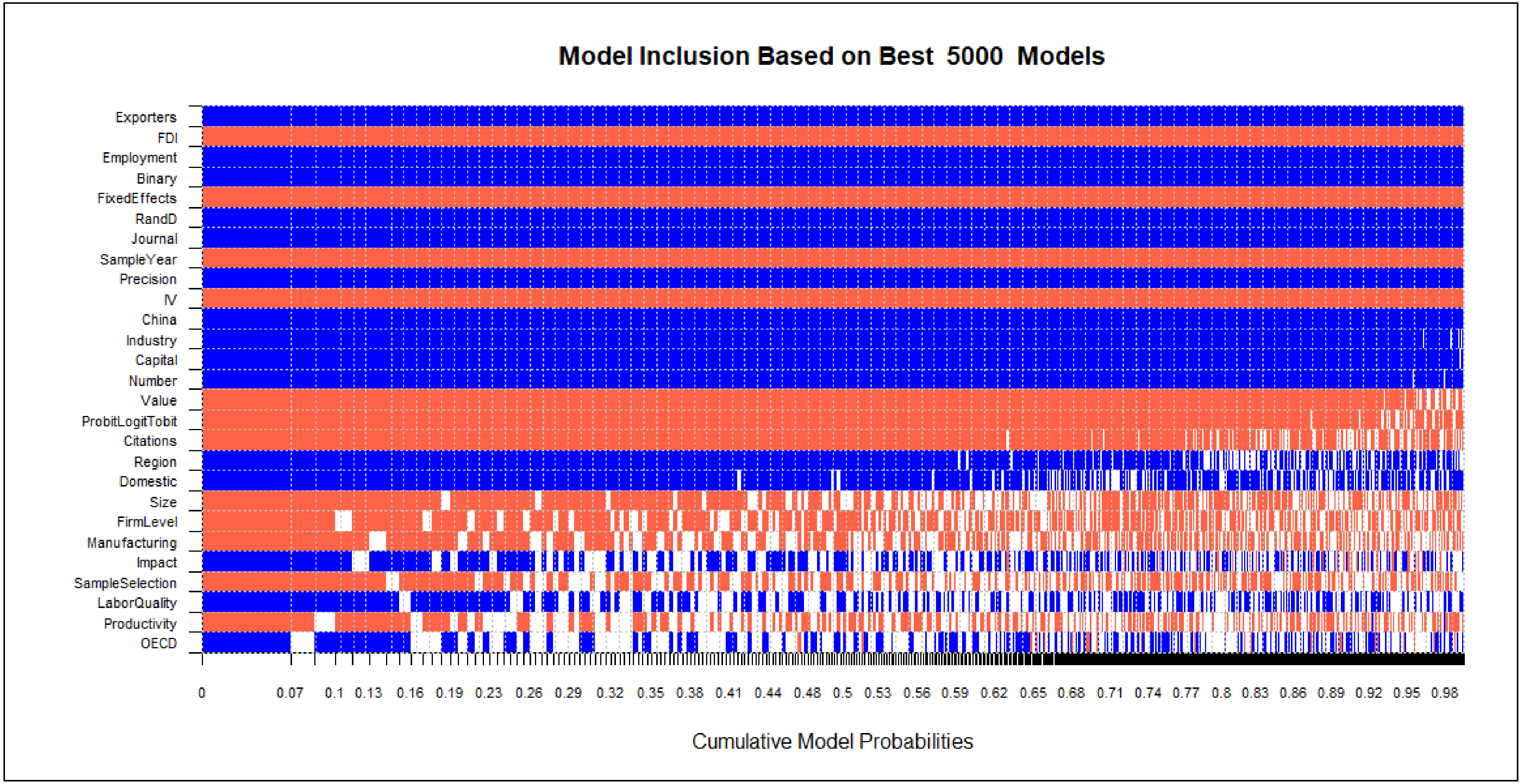
A. Individual Estimates



B. Mean Study Estimates



APPENDIX 1:
Visual Representation of BMA Analysis (*FixedEffects1*)

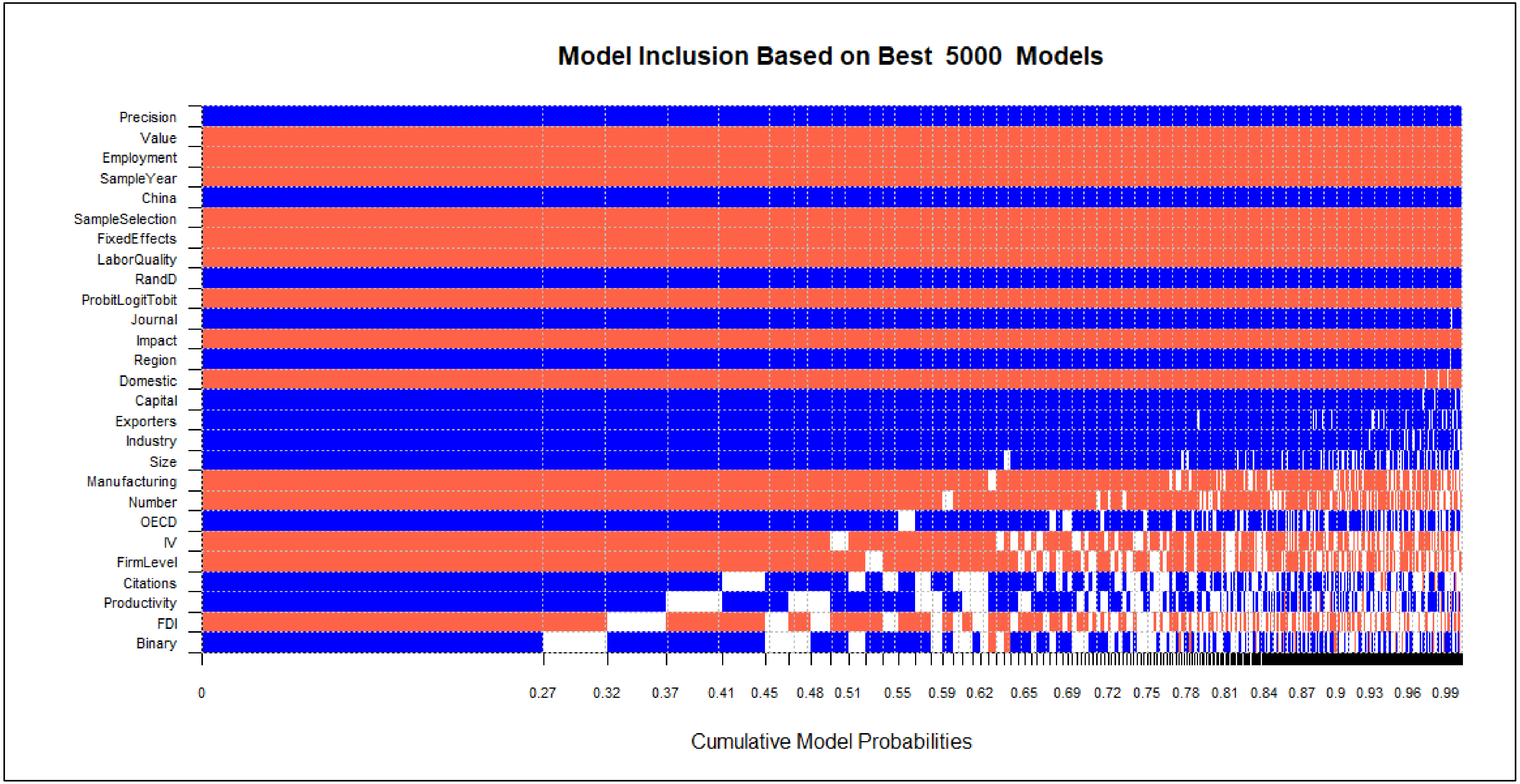


NOTE: Each column represents a single model. Variables are listed in descending order of posterior inclusion probability (PIP) and have all been weighted according to the *Fixed Effects1* case. Blue indicates that the variable is included in that model and estimated to be positive. Red indicates the variable is included and estimated to be negative. No color indicates the variable is not included in that model. Further details about this plot is given in Zeugner (2011).

SUMMARY STATISTICS:

Mean no. regressors "23.3595"	Draws "1e+07"
Burnins "1e+07"	Time "28.06786 mins"
No. models visited "4610663"	Modelspace 2^K "1.3e+08"
% visited "3.4"	% Topmodels "99"
Corr PMP "0.9999"	No. Obs. "3291"
Model Prior "random / 13.5"	g-Prior "hyper (a=2.0006077)"

APPENDIX 2:
Visual Representation of BMA Analysis (*RandomEffects1*)



NOTE: Each column represents a single model. Variables are listed in descending order of posterior inclusion probability (PIP) and have all been weighted according to the *RandomEffects1* case. Blue indicates that the variable is included in that model and estimated to be positive. Red indicates the variable is included and estimated to be negative. No color indicates the variable is not included in that model. Further details about this plot is given in Zeugner (2011).

SUMMARY STATISTICS:

Mean no. regressors "25.2064"	Draws "1e+07"
Burnins "1e+07"	Time "1.046804 hours"
No. models visited "2589905"	Modelspace 2^K "1.3e+08"
% visited "1.9"	% Topmodels "100"
Corr PMP "1.0000"	No. Obs. "3291"
Model Prior "random / 13.5"	g-Prior "hyper (a=2.0006077)"