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Survey Article: Publication Bias in the Literature on Foreign Direct Investment Spillovers

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ABSTRACT *In this article we conduct a large quantitative survey of the literature on horizontal and vertical spillovers from foreign direct investment (FDI). We create a unique database of spillover estimates for each country examined in the literature. Next, we estimate the average effect corrected for publication selection bias (the preferential selection of positive and significant estimates for publication). Our results suggest that an average reported estimate of backward spillovers is statistically significant. Publication selection is evident only among studies published in peer-reviewed journals, and only among the estimates that authors consider most important. Authors with small data sets engage in more publication selection. The intensity of selection in the literature decreases over time, which supports the economics-research-cycle hypothesis.*

I. Introduction

Policy-makers, especially in transition and developing countries, usually encourage inward FDI (foreign direct investment) in expectation that domestic firms in the same sectors benefit from know-how brought by foreigner investors. Moreover, many policy-makers believe that firms in supplier sectors benefit from direct knowledge transfers from foreigners, and perhaps also that firms in customer sectors benefit from higher-quality intermediate inputs produced by foreigners. With an allusion to the production chain, the effect of foreign presence on the productivity of domestic competitors is typically labelled horizontal spillovers, the effect on domestic suppliers as backward spillovers, and the effect on domestic customers as forward spillovers; backward and forward spillovers together are called vertical spillovers. Although neither a necessary nor sufficient condition for the provision of government subsidies for FDI, spillover effects are highly policy-relevant. In consequence, the search for spillovers has given rise to a burgeoning stream of empirical literature in development economics, and we investigate 57 such papers in this meta-analysis.

Horizontal spillovers are usually thought to occur through three main channels. The first channel is the competition effect (for example, Aitken and Harrison, 1999): the entry of foreign firms increases competition in the domestic market. Increased competition forces domestic firms to use their inputs more efficiently, boosting their productivity. Nonetheless, increased

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competition also reduces the opportunities of domestic firms to exploit returns to scale, reducing their productivity. The second channel is the demonstration effect (for example, Blomstrom and Kokko, 1998): foreign investors bring technology more advanced than that of domestic firms, especially in transition and developing countries. In this way, foreigners ‘demonstrate’ up-to-date technology to domestic firms, which imitate and implement it. The third channel is labour turnover (for example, Görg and Greenaway, 2004): foreign firms train local employees, who accumulate know-how and experience with modern technology. Eventually, locals change employer or start a firm of their own, diffusing knowledge further.

Foreign affiliates will try to prevent the transfer of technology to their competitors; that is, they will try to minimise the positive effects of demonstration and labour turnover. Therefore if the detrimental effects of competition prevail, horizontal spillovers altogether may well become insignificant or even negative. On the other hand, foreigners have incentives to provide assistance to their local suppliers, since they want to ensure a high quality and on-time delivery of inputs.

Indeed, anecdotal evidence indicates that local suppliers may benefit from the interactions with foreign investors, even if neither investors nor suppliers are particularly knowledge-intensive. In a recent interview conducted by the authors of this article, the chief executive officer of a Czech printing house describes how the company benefited from the contacts with a Japanese investor: The investor, doing business in electronics, was seeking a local contractor to print millions of instruction manuals for the European market. After the Czech company had won the contract, the representatives of the Japanese investor inspected the company and requested specific improvements in quality management. The representatives had gained experience from their contacts with suppliers in Japan, and they asked no compensation for the advice. The Czech printing house, in turn, applied the improvements also in other areas of production.

Therefore, the recent literature (Javorcik, 2004; Blalock and Gertler, 2008) emphasises vertical linkages between foreign investors and domestic firms. The per-job value of spillovers stirred up by linkages can be compared with the amount of government subsidies, as Haskel et al. (2007) do; hence for policy recommendations precise estimates of spillovers are required. Since the results of individual studies vary broadly, a quantitative literature survey, meta-analysis, represents a useful method to obtain robust estimates of spillovers (Stanley, 2001). If, however, some particular results are more likely to be published (for example, those consistent with the mainstream intuition about spillovers outlined in the paragraphs above), a simple average of the reported results will be a biased estimate of the underlying spillover effect. The importance of publication selection bias in the spillover literature was stressed already by the first meta-analysis on this topic, Görg and Strobl (2001).

Publication bias has been identified in many areas of economics research (Doucouliagos and Stanley, 2012). It stems from the preference of authors, editors, or reviewers for some particular results; usually those that are statistically significant or consistent with theory (Stanley, 2005). Publication bias can seriously exaggerate the magnitude of the underlying effect, which has been the case, for example, of the negative effects of minimum-wage increases on employment (Doucouliagos and Stanley, 2009), the price elasticity of gasoline demand (Havranek et al., 2012), or the positive effects of currency unions on trade (Havranek, 2010). In a large survey of economics meta-analyses, Doucouliagos and Stanley (2012) find that the magnitude of publication bias decreases with more theory competition in the particular research area. Stanley et al. (2008) show that publication selection is a complex phenomenon affected, among other things, by the characteristics of individual researchers. To our knowledge, however, no study has yet systematically examined the micro-level determinants of publication bias.

In contrast to the earlier meta-analyses on FDI spillovers (Görg and Strobl, 2001; Meyer and Sinani, 2009), we examine backward and forward spillovers in addition to horizontal spillovers. Using a large data set, we employ modern meta-analysis methods developed by Stanley (2005, 2008) to estimate the underlying spillover effects and the magnitude of publication bias. We present individual surveys for each country inspected in the literature and construct a unique cross-country data set of estimated spillovers. Furthermore, we retrieve estimates of publication

bias for each study and examine how the intensity of publication selection depends on the characteristics of the authors, such as affiliation, experience, and tenure pressure.

The rest of the article is structured as follows: section II discusses how FDI spillovers are measured, section III describes how we extracted information from primary studies, section IV presents the estimation of publication bias and true spillover effects, section V focuses on the determinants of publication bias, and section VI concludes the article. The Appendix tables provide meta-analyses for individual studies and countries.

II. Measuring Productivity Spillovers

To estimate the size of productivity spillovers from foreign direct investment, researchers usually examine the relation between foreign presence and the productivity of domestic firms. The variable corresponding to foreign presence is defined depending on the type of spillover under investigation. For horizontal spillovers, foreign presence simply denotes the ratio of foreign activity to the total activity in the domestic firm's own sector. For backward spillovers, foreign presence is defined as the ratio of foreign activity in sectors that buy intermediate goods from the domestic firm. Finally, in the case of forward spillovers foreign presence denotes the ratio of foreign activity in sectors that sell intermediate goods to the domestic firm. Most researchers include all these variables in one regression, together with a number of control variables:

$$\ln \text{Productivity}_{ij} = e_0^h \cdot \text{Horizontal}_j + e_0^b \cdot \text{Backward}_j + e_0^f \cdot \text{Forward}_j + \alpha \cdot \text{Controls}_{ij} + u_{ij}, \quad (1)$$

where i denotes domestic firms and j denotes sectors.

Approximately 90 per cent of all studies use firm-level panel data to estimate Equation (1). A few cross-section sector-level studies have been published since 2000, even though Görg and Strobl (2001) showed that cross-section studies systematically overstate horizontal spillovers. Most authors use the share of sector output as a measure of foreign presence, but some use shares of sector employment or equity. The most common control variables include sector competition, demand in downstream sectors, and a measure of absorption capacity (such as the technology gap between domestic and foreign firms or domestic firms' expenditures on research and development).

The majority of authors employ total factor productivity (TFP) as the measure of productivity, while others use output, value added, or labour productivity for the response variable. When computing TFP, most authors take into account the endogeneity of input demand and use the Levinson-Petrin or Olley-Pakes method, but 10 per cent of all estimates are computed using ordinary least squares. Approximately a half of all studies estimate Equation (1) in differences. A general-method-of-moments estimator is employed by 9 per cent of the studies, and the translog production function instead of the usual Cobb-Douglas function is employed by 8 per cent of them.

Despite these various methods, the results of all studies boil down to the estimates of coefficients e from (1), which are directly comparable whenever the log-level specification is used. (The heterogeneity of estimates with respect to different estimation techniques in this literature is explored in detail in a companion article, Havranek and Irsova, 2011.) The coefficients represent the semi-elasticity of the productivity of domestic firms with respect to foreign presence:

$$e_0 \approx \frac{\% \text{ change in productivity}}{\text{change in foreign presence}}, \text{ foreign presence} \in [0, 1]; \quad (2)$$

that is, approximately the percentage increase in domestic productivity associated with a 1 percentage point increase in foreign presence. Semi-elasticity has been previously used in meta-analysis, for example, by Rose and Stanley (2005) and Feld and Heckemeyer (2011), and represents the natural choice of summary statistic for the spillover literature.

III. Studies on Spillovers from FDI

Because Görg and Strobl (2001) conducted a meta-analysis of horizontal spillover studies published before 2000, in this article we focus on studies originated after 2000, and especially on the new subset of the literature, vertical spillovers (another reason for focusing on post-2000 studies is that the earlier studies are often not directly comparable). Our intention is to collect all studies estimating backward and forward spillovers; nonetheless, if these studies also estimate horizontal spillovers, we use that information as well. Thus our meta-analysis can be viewed as a complete meta-analysis of vertical spillovers and a partial meta-analysis of horizontal spillovers.

We searched the EconLit, Scopus, Google Scholar, and RePEc databases for prospective studies. Additionally we examined the references of all identified studies published in the last year of our sample, 2010, and also the citations of the most influential article on vertical spillovers, Javorcik (2004). We excluded a few studies that estimated vertical spillovers but that did not define foreign presence as a ratio and hence could not be used to compute semi-elasticity (for example, Kugler, 2006; Bitzer et al., 2008). Approximately 20 per cent of all studies in our sample could be included only thanks to the cooperation with authors, who often sent us the standard errors of the estimated elasticities. Following the advice of Stanley (2001), ‘better err on the side of inclusion,’ we did not exclude any study based on the form, place, or language of publication. The last study was included on 31 March 2010. We do not update studies after that date – for example, if a working paper is included in our data set and gets published after 31 March 2010, we still use the working-paper version.

We collect all estimates reported in the studies. It would be inefficient to discard data; moreover, often it is not clear which estimate the authors prefer. Later in the analysis, as a robustness check, we use averages of all reported coefficients from each study. We gather 57 studies that include 3626 estimates of elasticity for different types of spillover. Coding such a large number of observations manually is a laborious exercise, and it is difficult to avoid mistakes in the process. To minimise the danger of typos, both of us collected all data independently; it is unlikely that both collectors would make the same mistake. At the end we compared our data sets, reached consensus for every data point that differed, and retrieved the data set.

We provide a summary of all studies in Appendix Table A1 and Table A2 and display representative spillover coefficients for each study. The representative semi-elasticities are estimated as inverse-variance-weighted averages with individual random effects to take into account heterogeneity within studies; the method is called the simple random-effects meta-analysis. Additional details on data properties and collection can be found in the working-paper version of this article, Havranek and Irsova (2010).

Most narrative reviews of empirical literature only consider studies published in high-quality journals. We begin the analysis with a set of such studies to illustrate how the restriction of the sample may, under realistic conditions, lead to biased conclusions concerning the strength of the examined phenomenon. We define high-quality journals for spillover literature as the leading outlets in international economics (*Journal of International Economics*), international business (*Journal of International Business Studies*), and development economics (*Journal of Development Economics*). Naturally, one study published in the *American Economic Review* is also included in the subset, increasing the number of identified studies to seven. The selected journals have the highest impact factor in the sample, and even if we added the journal with the next highest impact factor (*The World Economy*), the inference would be similar.

Table 1 summarises the qualitative results of studies published in high-quality journals. We add Kugler (2006) to the table since the study is frequently cited in the literature, even if its quantitative results are not comparable with studies in our sample; he does not estimate semi-elasticity, hence we cannot include the study in our quantitative analysis. From Table 1 it is apparent that the evidence for positive and significant backward spillovers is unequivocal, but no such consensus emerges for forward and horizontal spillovers: some researchers report positive effects of forward linkages and negative effects of horizontal linkages; others find insignificant effects.

Table 1. Qualitative results of studies published in high-quality journals

Study	Journal	Backward	Forward	Horizontal
Javorcik (2004)	American Economic Review	+	?	?
Bwalya (2006)	Journal of Development Economics	+		-
Kugler (2006)	Journal of Development Economics	+ ^a	+ ^a	?
Blalock and Gertler (2008)	Journal of International Economics	+		?
Javorcik and Spatareanu (2008)	Journal of Development Economics	+ ^b		-
Liu (2008)	Journal of Development Economics	+ ^c	?	+ ^c
Blalock and Simon (2009)	Journal of Int. Business Studies	+		?
Liu et al. (2009)	Journal of Int. Business Studies	+	+	-

Notes: +, -, and ? denote the finding of positive, negative, and insignificant spillover effects.

^aThe author does not discriminate between backward and forward spillovers.

^bPositive effect reported only for investments with joint foreign and domestic ownership.

^cPositive long-run effect, negative short-run effect.

Taking a simple average of all estimates reported in high-quality journals confirms this qualitative observation. The average semi-elasticity reaches 1.14 for backward spillovers, 0.54 for forward spillovers, and -0.13 for horizontal spillovers, all significant at the 5 per cent level. Most of the studies concentrate on backward spillovers and provide estimates of forward and horizontal spillovers only as a bonus. The practice reflects the recent view that domestic firms supplying foreign affiliates are the most likely beneficiaries of technology transfer and that the effect on competitors and customers is less important.

It is remarkable that the average estimates of spillovers do not change significantly if we broaden the sample to all published studies, but the averages shrink a lot if unpublished studies are considered as well. If unpublished studies are included, the average backward spillover reaches only 0.27, the average forward spillover is negative (-0.09) and insignificant, and the average horizontal spillover is negligible (0.01). All of this points to selection bias in published studies, which will be the topic of section IV.

Because the studies in our sample estimate spillover coefficients for many different countries, they allow us to construct a unique cross-country database of spillovers from foreign direct investment. The database is summarised in the Appendix in Table A3; we use the simple random-effects meta-analysis to estimate the average backward, forward, and horizontal spillover based on the entire available literature for each country. The results for European countries are depicted in Figure 1 (the graphical presentation of our results is not convenient for other continents as some large countries have not yet been studied in the spillover literature). It is apparent that the effects of backward linkages are relatively heterogeneous. While it is difficult to draw general conclusions, the figure, in line with Bitzer et al. (2008), suggests that central-eastern European countries may benefit relatively more from foreign investment than their advanced western-European counterparts. The average backward spillovers, estimated by the simple random-effects meta-analysis, also significantly differ between the income categories of countries in our sample: spillovers are larger for developing economies (0.22) than for developed economies (0.09). The cross-country heterogeneity in the sample is examined in detail in a companion article (Havranek and Irsova, 2011); in this article we focus on publication bias.

IV. Quantifying Publication Bias

Publication selection bias in the FDI spillover literature was first examined by the well-known meta-analysis of Görg and Strobl (2001). Following Card and Krueger (1995), they used two distinct tests of publication bias, and both tests indicated the presence of publication bias in the

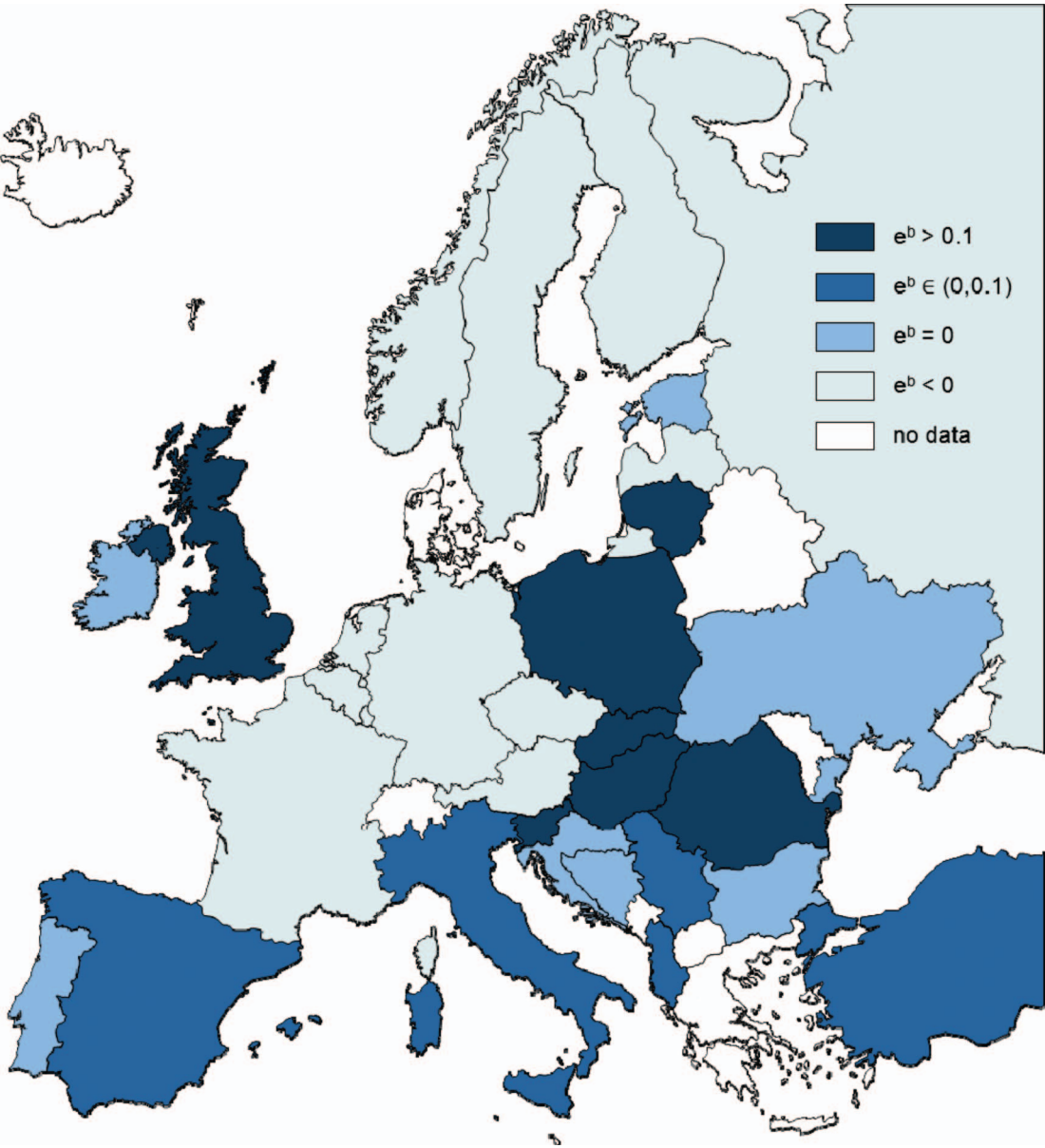


Figure 1. Cross-country evidence on backward spillovers.

literature. Nevertheless, the number of observations available to Görg and Strobl (2001) for the tests was only 23 and 16, respectively. We revisit the issue of publication bias in this broad literature taking the advantage of three times more new primary studies published after 2000 and more than 1,000 estimates for each type of spillover.

The first test of publication bias draws on Begg and Berlin (1988). It examines the relation between the reported t-statistic of the spillover coefficient and the number of degrees of freedom available for the estimation. Görg and Strobl (2001) argue that, in the absence of publication bias, the absolute value of t-statistic should increase with more degrees of freedom (roughly speaking, more observations make the estimation more precise and increase statistical significance). Specifically, Card and Krueger (1995) explain that the logarithm of the absolute value of the t-statistic should be directly proportional to the logarithm of the square root of the

number of degrees of freedom. As Stanley (2005) notes, it makes no practical difference whether the number of observations or degrees of freedom is used for this test, and because spillover studies directly report the number of observations, we employ the following specification:

$$\log|t_i| = \alpha_1 + \alpha_0 \log(\sqrt{\text{number of observations}})_i + \varepsilon_i, \quad (3)$$

where $\alpha_0 = 1$ if no publication bias is present. The specification is estimated separately for each type of spillover, and the results are summarised in Table 2. Similarly to Görg and Strobl (2001), we reject the hypothesis $\alpha_0 = 1$; the result of the test is the same for horizontal, backward, and forward spillovers.

Does this really mean that we have found evidence for publication selection bias? Stanley (2005) and Doucouliagos and Stanley (2009) show that specification (3), emphasized by both Card and Krueger (1995) and Görg and Strobl (2001), is not a proper one to test for publication bias. For example, consider the case when there is no genuine empirical effect. Since here we have $t = e/Se$ and $e = 0$, the absolute value of t-statistic does not increase with more observations, even though more observations reduce standard errors. Hence, if a weak relation between the t-statistic and the number of observation is found, it may suggest either the presence of publication bias or the absence of the underlying empirical effect.

Stanley (2005) argues that specification (3) should be interpreted as a test for a genuine empirical effect. Note that the relationship between the absolute value of the t-statistic and the number of observations is always positive and significant in Table 2, which may indicate that the underlying spillover effects are different from zero. Stanley (2005), however, warns that this test has large type I errors (false rejection of no relationship), especially if the literature suffers from misspecification biases. Since the meta-analysis of Görg and Strobl (2001) suggests that misspecifications indeed drive some results in this literature (specifically, they find that studies using cross-sectional data overstate spillover effects), we take the evidence from Table 2 with a grain of salt.

Instead, the second test presented by Card and Krueger (1995) and Görg and Strobl (2001) has become the cornerstone of modern meta-analysis, and can be used to detect the significance and magnitude of both publication bias and a genuine underlying effect (Egger et al., 1997; Stanley, 2005, 2008). Before turning to formal regression analysis, it is beneficial to introduce the graphical version of this test (Stanley and Doucouliagos, 2010). The so-called funnel plot is the most common method of detecting publication bias in medical meta-analyses (Sutton et al., 2000). It is a scatter plot of the size of the estimates on the horizontal axis and their precision, usually the inverse of standard errors, on the vertical axis. The most precise estimates will be close to the genuine underlying effect (that is, the top portion of the scatter should be narrow), while imprecise estimates will be more dispersed (that is, the bottom portion should be wide). In

Table 2. Test of publication bias following Görg and Strobl (2001)

	Backward	Forward	Horizontal
$\log \sqrt{\text{number of observations}}$	0.112*** (0.0355)	0.512*** (0.0446)	0.206*** (0.0400)
Constant	-0.253 (0.168)	-2.187*** (0.215)	-0.832*** (0.197)
Observations	1401	1067	1204
Studies	56	45	52
t-stat ($H_0: \alpha_0 = 1$)	25.0***	11.0***	19.9***

Notes: Estimated by OLS; heteroscedasticity-robust standard errors in parentheses.

Response variable: logarithm of the absolute value of t-statistic of the estimate of semi-elasticity.

***denotes significance at the 1 per cent level.

sum, the cloud of the estimates should resemble an inverted funnel. Publication bias is then indicated by the asymmetry of the funnel plot: in the absence of publication bias, all imprecise estimates have the same chance of being reported, and the funnel is symmetric.

The funnel plots constructed using estimates from both published and unpublished studies for the three types of spillover are depicted in the top panels of Figure 2. One thing that strikes an economics meta-analyst is the general symmetry of all three funnels: a relatively rare sight considering that the meta-meta-analysis of Doucouliagos and Stanley (2012) finds ‘substantial’ or ‘serious’ publication bias for two thirds of all areas in empirical economics. Close inspection of the funnels, however, reveals slight asymmetries. The right-hand part of the funnel for backward spillovers is a little heavier, suggesting possible selection in favour of positive estimates of backward spillovers. On the other hand, for horizontal spillovers the funnel is slightly skewed to the left, which indicates possible preferential selection of negative estimates. We can identify no sign of bias for forward spillovers, although the funnel is worse-shaped compared with the other two: perhaps there is greater heterogeneity among forward spillovers, which causes some precise estimates to lie far away from the mean value.

Despite its name, publication selection in economics usually is not restricted to published studies. Rational authors are likely to polish (that is, use a particular direction of estimates as a specification check and discard estimates with an unintuitive sign) even early drafts of their papers if they expect that some results are more likely to be accepted for publication. For this reason, most meta-analysts pool published and unpublished papers together when testing for selection bias. If the pattern of publication bias was stronger for published than for unpublished studies, we would have a reason to believe that, aside from self-censorship, there is an additional selection pressure from journal editors and reviewers.

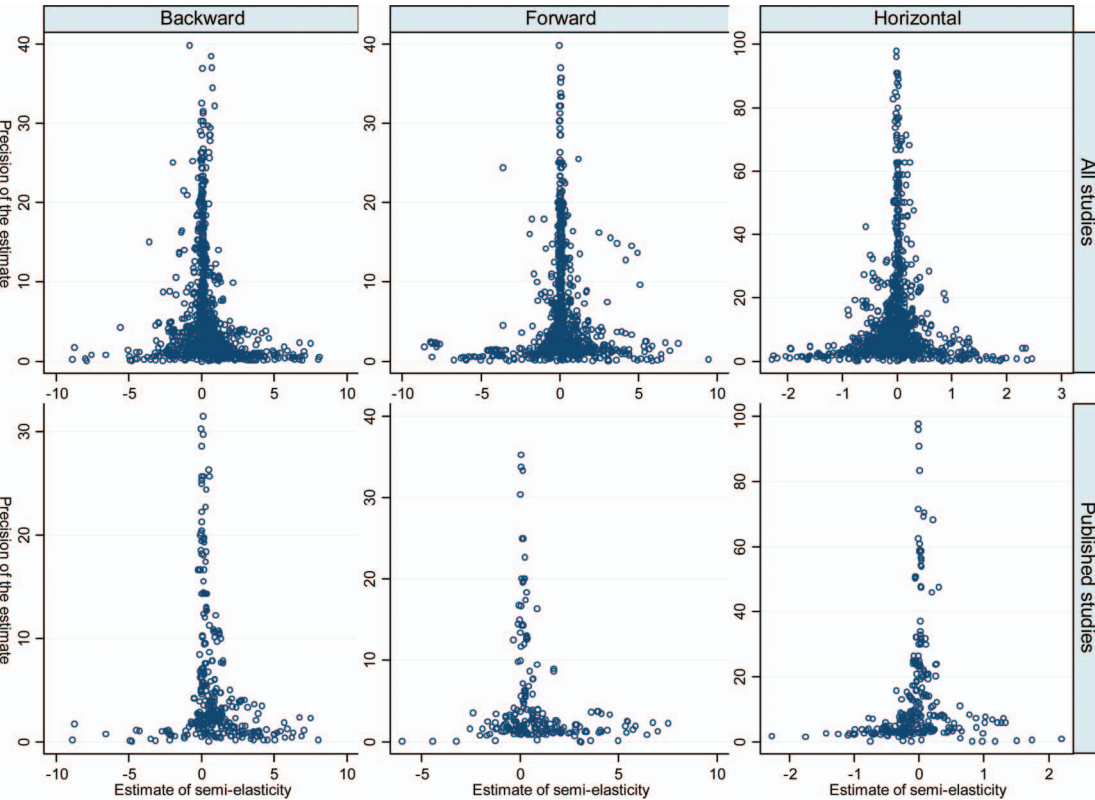


Figure 2. Funnel plots show publication bias in journal articles.

The bottom panels of Figure 2 show funnel plots for the three types of spillover when only published studies are considered. It is apparent at first sight that the slight asymmetries identified earlier in the funnels for all estimates now become much more pronounced. Clear biases emerge, upward for backward spillovers and downward for horizontal spillovers. Such a different direction of publication selection of related coefficients taken from the same literature is surprising.

When the first influential article on backward spillovers was published (Javorcik, 2004), the article in the *American Economic Review* stressed the contrast between its findings of large positive backward spillovers and negligible horizontal spillovers. Since the consensus at that time was that the evidence for horizontal spillovers was mixed at best (Görg and Greenaway, 2004), Javorcik (2004) argued that researchers ‘have been looking for spillovers in the wrong place.’ This appealing argument has been repeated many times in the burgeoning literature that has followed Javorcik (2004) and that has estimated backward spillovers for different countries. Now, if authors used this result as a specification check, upward selection bias for backward spillovers and downward bias for horizontal spillovers would follow. This is precisely what the funnel plots suggest.

The interpretation of funnel plots, however, is subjective; asymmetry of the funnel is difficult to detect precisely by mere visual inspection. Thus a formal version of the test for asymmetry is necessary. It follows from rotating the axes of the funnel plot, so that the effect size is now on the vertical axis, and from inverting the values of precision on the new horizontal axis. The inverted scatter plot can be interpreted as a regression relationship (Card and Krueger, 1995; Görg and Strobl, 2001):

$$e_i = e_0 + \beta_0 \cdot Se(e_i) + u_i, \quad (4)$$

where β_0 measures the asymmetry of the funnel plot and the strength of publication bias. In the absence of publication bias we would not expect the reported coefficients to be correlated with their standard errors, and the estimates would be randomly distributed around the true effect e_0 . Because equation (4) is heteroscedastic by definition, and the measure of heteroscedasticity is Se (the explanatory variable is a sample estimate of the standard deviation of the response variable; the heteroscedasticity is also apparent from the funnel plots), the specification is usually estimated by weighted least squares (Stanley, 2005):

$$\frac{e_i}{Se(e_i)} = t_i = \frac{e_0}{Se(e_i)} + \beta_0 + \xi_i. \quad (5)$$

Though simple, the specification is remarkably efficient for the estimation of both the magnitude of publication bias (β_0) and the true effect (e_0), as Monte Carlo simulations show (Stanley, 2008). Because we have a large number of observations, and results within studies are likely to be correlated, we include study dummies in the estimation to purge away individual study effects.¹ We use heteroscedasticity-robust standard errors clustered at the study level. As a robustness check, we also use only one estimate per each study; the representative estimate for each study is estimated by the simple random-effects meta-analysis. To account for possible outliers, we additionally run the regression with one estimate per study using a robust MM-estimator introduced by Verardi and Croux (2009), who show that this modern method outperforms all other commonly used robust estimators. The results for all three types of spillover are summarised in Table 3.

When both published and unpublished studies are considered together, Table 3 shows hardly any evidence of bias for any type of spillover. Only the coefficient for forward spillovers is significant at the 10 per cent level, but the magnitude is small (Doucouliagos and Stanley, 2011, would label it as ‘little to modest’) and the significance disappears in the robustness check. Thus formal meta-regression methods do not corroborate the slight asymmetries identified in the top

Table 3. Test of publication bias and true effect following Stanley (2005)

	Fixed effects		Robust
	All	Published	All
Backward spillovers			
Constant (publication bias)	-0.102 (0.315)	1.760*** (0.0426)	1.509 (1.038)
1/Se (true effect beyond bias)	0.124*** (0.0383)	0.0934*** (0.00519)	0.0371* (0.0188)
Observations	1402	401	56
Studies	56	27	56
Forward spillovers			
Constant (publication bias)	0.940* (0.524)	-0.160 (0.758)	-0.287 (0.710)
1/Se (true effect beyond bias)	-0.0388 (0.0567)	0.342** (0.123)	0.0294*** (0.0096)
Observations	1066	262	45
Studies	45	20	45
Horizontal spillovers			
Constant (publication bias)	-0.312 (0.261)	0.075 (0.0446)	0.800 (0.784)
1/Se (true effect beyond bias)	0.0214 (0.0147)	0.00649*** (0.0023)	0.00624 (0.00739)
Observations	1205	352	52
Studies	52	27	52

Notes: Heteroscedasticity-robust standard errors in parentheses. Response variable: t-statistic of the estimate of semi-elasticity.

Fixed effects = Study fixed effects are included. Robust = the simple random-effects meta-analysis is run for each study separately; then, using a robust MM-estimator following Verardi and Croux (2009), the meta-regression is run on the results. All = estimates from all studies. Published = only estimates from studies published in refereed journals.

***, **, and *denote significance at the 1 per cent, 5 per cent, and 10 per cent levels.

three funnel plots of Figure 2. If, on the other hand, only published studies are considered, the asymmetry of the funnel plot for backward spillovers is corroborated strongly: on average, published studies significantly overstate the magnitude of backward spillovers. The meta-regression does not support the asymmetry of the funnel plot for horizontal spillovers, however.

In sum, we found formal evidence of publication selection only for backward spillovers, and only for published studies. No selection was found for the estimates of forward and horizontal spillovers and for unpublished estimates of any type of spillover. In the spillover literature, horizontal and forward spillovers are generally viewed as less important than backward spillovers: for forward spillovers it is relatively difficult to find interpretation, and horizontal spillovers are often expected to be insignificant due to the competition effect (Görg and Greenaway, 2004). In the post-2000 literature, which constitutes our sample, the estimates of backward spillovers make the story of the paper; hence, they are more likely to be polished.

When estimating the genuine spillover effects after correction for within-study correlation and publication bias, it is meaningful to restrict our interpretation to the specifications that include all studies (if only published studies were considered, at least 70% of all observations would be discarded; we have also found that published studies exhibit more bias). Both estimates of the true effect for backward spillovers are positive and statistically significant at least at the 10 per cent level. We prefer the fixed-effects model, where information from all 1402 observations is used: in this case, the estimate of backward spillover is significant at the 1 per cent level and reaches 0.12. In other words, a 10 percentage point increase in foreign presence is on average

associated with an increase in the productivity of local firms in supplier sectors by 1.2 per cent, an economically significant effect.

The results for forward spillovers are mixed: the fixed-effects estimator reports an insignificant coefficient, while the robust estimator reports a coefficient that is highly significant (though small in magnitude). In either case, forward spillovers do not seem to be economically important. Finally, for horizontal spillovers both estimators show insignificant results.

As Stanley and Doucouliagos (2007) note, the estimates of e_0 from regression (5) may be biased downwards, and they recommend estimating the Heckman meta-regression, which assumes a quadratic relation between estimated effects and their standard errors in Equation (4). Because, however, in our case no publication selection is present in the sample of all studies, the coefficient estimated using the Heckman regression is very close to that estimated by (5), and so we do not report it.

V. Determinants of Publication Selection

As funnel plots depicted in Figure 2 show, the pattern of publication bias can vary significantly among subsets of studies and among related topics within a single empirical literature. In this section we aim to explore the sources of this heterogeneity and shed some light on the determinants of publication selection. We concentrate on backward spillovers, because we have already found evidence of a large difference in publication bias between published and unpublished studies in this area. The topic of this section is related to the meta-meta-analysis by Doucouliagos and Stanley (2012), who study the determinants of publication selection across economics literatures – that is, on the macro level. We, instead, focus on the individual studies' and authors' characteristics that may influence the strength of publication bias within one literature.

As the response variable, we employ study-level estimates of the extent of publication bias taken from regression (5). Because the intercept in (5) is a measure of publication bias, and the specification was estimated with fixed effects for individual studies, we use the estimated fixed effects to compute individual intercepts. These intercepts, in turn, measure the extent of publication bias for each study. A large data set is needed for such an exercise. Fortunately, we have 56 studies that provide more than one estimate of backward spillovers, and the vast majority of them provide more than 10 estimates. In any case, however, some of the individual intercepts will not be estimated precisely, and our response variable will therefore have a relatively large random sampling error. The estimation is likely to produce several outlying values of publication bias, especially for studies that provide only a few estimates of backward spillovers. For this reason, we employ a robust MM-estimator introduced by Verardi and Croux (2009) in all following regressions.

We select a dozen study-level variables that may potentially influence the intensity of publication selection in favour of significant positive estimates of backward spillovers; the summary statistics of these variables are reported in Table 4. First, we include a dummy variable that equals one if the study was published in a peer-reviewed journal: as shown in section IV, we expect that published studies exhibit more publication selection. Approximately half of the studies were published in journals, the rest of our observations were obtained from working papers and dissertations. Another explanatory variable is the impact factor of the journal or working-paper series: perhaps the pattern of publication bias differs between lower- and higher-quality outlets.

If the selection of some particular results increases the probability of publication, successful authors will have more experience with polishing (either intentional or unintentional), and we expect greater publication bias in studies they co-author. A fine measure of authors' success is the number of citations, and we include it as an additional explanatory variable (for each author we collect all RePEc citations received before the date of publication of the particular study). Next, because publication bias should be inversely related to sample size (Stanley, 2005), we expect that

Table 4. Summary statistics of explanatory variables

	Mean	Std. dev.	Min	Max
Study was published in a peer-reviewed journal	0.482	0.504	0	1
Journal or series impact factor	0.194	0.431	0	2.70
Number of citations of the most cited co-author (logarithm)	2.830	2.340	0	6.66
Number of observations used by the study (logarithm)	9.400	2.120	3.76	13.70
A co-author is native to the country under examination	0.723	0.444	0	1
A co-author is affiliated with a US-based institution	0.250	0.437	0	1
A co-author is affiliated with an academic institution	0.643	0.483	0	1
Year and month when the study was published (base year 2000)	7.770	1.730	2.79	10.50
Focus on the interpretation of spillover significance	0.536	0.503	0	1
The corresponding author has not completed PhD	0.268	0.447	0	1
The corresponding author has completed PhD 1–5 years ago	0.304	0.464	0	1
The corresponding author has completed PhD 6–10 years ago	0.250	0.437	0	1

Notes: Data on the number of citations and the impact factor (recursive) are taken from RePEc.

studies with larger data sets will be engaged in less publication selection. As shown in section IV, the underlying average backward spillover is positive; therefore authors with many observations will not have to search long for intuitive (that is, positive and significant) results.

We include a dummy which equals one if at least one co-author of the study is native to the country under investigation, which holds for 72 per cent of studies in our sample. We consider an author native if he either was born in the country or obtained an academic degree there. We expect that such authors may have vested interest in the results and may be involved more in publication selection. Another included dummy equals one if a co-author is affiliated with a US-based institution (25% of all studies; we collect affiliations stated by the authors in the pdf versions of their studies). Because of the characteristics of the tenure system in the USA, such authors are likely to be under fierce pressure to publish. When this is the case, they may also be more tempted to polish their studies in order to increase the probability of publication. Testing a weaker hypothesis, Stanley (2005) shows that studies using US data exhibit more publication bias. Of course, in general any affiliation with an academic institution may stimulate publication selection because of the requirements for tenure, and we include a corresponding dummy variable.

Stanley et al. (2008), building on the work of Goldfarb (1995), discuss the issue of fashion and novelty in economics research: the so-called economics-research-cycle hypothesis. According to the hypothesis, studies in a newly emerged empirical area produce large and significant estimates of the underlying effect at first, but as the time passes, sceptical results become more interesting and soon begin to dominate the literature. Thus we should observe a downward trend in the reported t-statistics. If the underlying effect does not change in time, the downward trend in the reported results is entirely due to publication selection, and therefore implies a similar trend in the extent of publication bias. To test the hypothesis, we add a variable indicating the year and month of study publication.

We expect studies focusing specifically on the interpretation of the size and statistical significance of backward spillovers to exhibit more publication bias: because the most important results make the story of the paper, they are most likely to be polished. On the other hand, some studies estimate FDI spillover regressions, but concentrate on the heterogeneity of spillover effects and not on the magnitude of spillover effects for themselves (for example, they add interactions of foreign presence with the absorption capacity of domestic firms); we do not expect these studies to involve as much selection of spillover coefficients based on polarity or significance.

Finally, we include dummy variables reflecting the PhD vintage of the corresponding authors of the studies in our sample. Authors are divided into four groups: those who have not completed

PhD at the time when their study was published (27% of all studies), those who have completed PhD between 1 and 5 years ago (30%), those who have completed PhD between 6 and 10 years ago (25%), and those who have completed PhD more than 10 years ago (18%). Because the tenure pressure likely magnifies the selection bias, we expect stronger selection among studies published by researchers who have completed PhD less than 6 years ago.

Regression results are reported in Table 5. In the first column we include all explanatory variables except for PhD vintage dummies, which we add in the second column. The third column reports the results of the general-to-specific modelling approach: all insignificant variables are gradually excluded from the model. The R-squared of the regressions varies from 0.31 to 0.33. Because the data we use are in essence micro-level, such relatively low values of R-squared are not surprising; they are common in meta-analysis (see, for example, Disdier and Head, 2008). Moreover, as noted earlier, the response variable has a relatively large random sampling error, which means that a part of variation in the response variable cannot be explained by any regression model.

As expected, we find that publication in a peer-reviewed journal is associated with more selection bias. The results are in line with the intuition and evidence presented in Section IV, and imply that, other things being equal, selection bias in published studies is larger by 1 compared to unpublished studies. The difference is enough to move the incidence of publication selection from one category listed by Doucouliagos and Stanley (2012) to another: for example, from 'lesser to

Table 5. Determinants of publication selection toward large positive estimates

Explanatory variable	(1)	(2)	(3)
Study was published in a peer-reviewed journal	0.978*** (0.284)	1.028*** (0.304)	0.904** (0.337)
Journal or series impact factor	-1.578*** (0.224)	-1.564*** (0.265)	-1.631*** (0.303)
Number of citations of the most cited co-author	0.192*** (0.0612)	0.224*** (0.0805)	0.228*** (0.0605)
Number of observations used by the study	-0.362*** (0.0999)	-0.354*** (0.103)	-0.362*** (0.129)
A co-author is native to the country under examination	0.101 (0.430)	0.0879 (0.457)	
A co-author is affiliated with a US-based institution	1.041*** (0.365)	1.031*** (0.371)	0.959** (0.401)
A co-author is affiliated with an academic institution	-0.693** (0.286)	-0.708 (0.446)	
Year and month when the study was published	-0.350*** (0.113)	-0.330** (0.124)	-0.294* (0.159)
Focus on the interpretation of spillover significance	0.719** (0.298)	0.729** (0.305)	0.710** (0.289)
The corresponding author has not completed PhD		0.356 (0.528)	
The corresponding author has completed PhD 1–5 years ago		0.211 (0.484)	
The corresponding author has completed PhD 6–10 years ago		0.0522 (0.675)	
Constant	4.986*** (1.335)	4.471*** (1.656)	4.159** (1.555)
Observations	56	56	56
Pseudo R-squared	0.321	0.330	0.306
F-stat (H_0 : all coefficients for PhD vintage dummies are zero)		0.170	

Notes: Standard errors in parentheses. Response variable: the magnitude of publication bias in the study. Estimated using a robust MM-estimator following Verardi and Croux (2009).

***, **, and * denote significance at the 1 per cent, 5 per cent, and 10 per cent levels.

moderate' to 'substantial.' To be more specific, since the average standard error among the estimates of backward spillovers reaches 1.4, from Equation (4) it follows that published studies exaggerate backward spillovers by the same amount (1×1.4). Because the true underlying spillover is only 0.12, the exaggeration is more than tenfold (recall the average 1.14 from published studies reported in section III).

Our results indicate that studies published in better outlets (with a higher impact factor) are associated with less publication bias. This could be the case if especially lower-ranked journals used the finding of positive spillovers as an intuitive check of correct specification; on the contrary, high-quality journals are more likely to select studies according to their methodology and the rigour of analysis, giving less weight to intuition. Next, we find that frequently cited authors produce studies with greater publication bias. The evidence is in accordance with the intuition given above: successful authors have more experience with polishing their results. Likewise, it is not surprising that studies with larger data sets show less publication bias.

We find no evidence that authors native to the country examined in their studies would be involved in more selection compared with other researchers. On the other hand, we find substantially higher publication bias for US-affiliated authors; this result corroborates the findings of Stanley (2005). The coefficient estimated for the dummy variable for affiliation with an academic institution is negative, which is not consistent with our expectations. The coefficient, however, is statistically significant only in one out of three specifications.

Our results suggest a clear downward trend in publication bias, which is consistent with the economics-research-cycle hypothesis: studies published in early 2000s may have overstated the average backward spillover; in recent years, however, much lower estimates have been reported. Studies focusing on the interpretation of the size and significance of the spillover coefficient exhibit more publication bias, which corresponds with our expectations. The coefficient reaches approximately 0.72, which indicates that such studies on average exaggerate the estimate of backward spillovers by 1 (0.72×1.4). Finally, the dummies for PhD vintage of the corresponding authors are jointly insignificant, which means that, aside from affiliation with US-based institutions, our data show no further pattern of publication selection connected to tenure pressure. We also tried to include interactions of the dummy for US affiliation and dummies for PhD vintage; nevertheless, the interactions were not significant as well (this additional specification is available on request).

VI. Conclusion

The principal economic argument for the provision of subsidies for FDI is the assumed knowledge spillovers flowing from foreign affiliates to domestic firms. Because of the importance of FDI spillovers for economic policy, a vast body of empirical literature has attempted to quantify these effects. Nevertheless, the results of individual studies vary significantly (for example, depending on the exact method chosen to estimate the spillover effect), making it difficult for policy-makers to draw conclusions from the literature. One possibility is to focus solely on studies published in the most respected journals and discard all other outcomes. Studies published in respected journals may be expected to employ better methods than other studies, but altogether they only provide evidence for a few countries. Moreover, the selectivity of top journals may imply that strong (or, in other words, statistically significant) results have a higher probability of publication, which would distort inference.

The distortion of reported results due to publication pressures is called publication selection bias, and it has been found strong in many areas of economics research (Doucouliagos and Stanley, 2011). Goldfarb (1995) formulates the economics-research-cycle hypothesis, which has been corroborated empirically for some fields of applied economics by Stanley et al. (2008) and Havranek (2010). According to this hypothesis, seminal contributions in applied economics (that is, papers which are the first to estimate a particular effect) tend to report large and significant estimates. Only strong results convince the editors, overcome the barriers to entry, and a new

empirical field is born. The large estimates are often corroborated by subsequent research – but, as the time passes, sceptical results become preferred, since they are considered more interesting by the editors, reviewers, and the readership in general. Because of the possibility of publication bias and the research cycle in the spillover literature, we prefer to evaluate a broad sample of empirical studies, making use of the work of dozens of development researchers. Moreover, in contrast to cherry picking, inference derived from the entire literature does not depend on any particular methodology employed by the primary study to estimate spillovers.

We gather 3626 estimates from 57 studies that focus on vertical spillovers – that is, the effect of foreign presence on domestic firms in supplier or customer sectors. Apart from a complete survey of vertical spillovers, we also conduct a partial survey of horizontal spillovers (the effect of foreign investors on domestic firms in the same sectors) by including only those coefficients that researchers estimate in the same regression with vertical spillovers. We employ modern meta-analysis methods to uncover the underlying economic effect of FDI on the productivity of domestic firms.

Our results suggest that the average effect of foreign affiliates on the productivity of their local competitors (horizontal spillover) is economically insignificant. The effect of foreign affiliates on their local customers (forward spillover) is likewise negligible. On the other hand, we detect a statistically significant and economically meaningful effect of foreign affiliates on their local suppliers (backward spillover). Specifically, a 10-percentage-point increase in foreign presence is associated with a 1.2 per cent boost to the productivity of domestic firms in supplier sectors. Such a spillover effect is consistent with subsidies for FDI. Nevertheless, policy-makers should exercise caution because the estimates capture more than externalities: studies on FDI spillovers do not account for possible compensations for the transfer of technology (Keller, 2009). An exception is Blalock and Gertler (2008), who examine the influence of foreign presence on the profits of Indonesian firms and confirm the positive externality.

While the average backward spillover is robustly positive, it differs significantly across countries. For example, the effect for all developing countries examined by the studies in our sample is twice as large as the average spillover reported for developed countries. The degree of economic development plays an important role in explaining the difference, but it is not the only one. In a companion paper (Havranek and Irsova, 2011) we examine in detail what causes the differences in the reported FDI spillovers. We find that both the characteristics of the host country and the characteristics of FDI matter. For example, a larger technology gap of domestic firms with respect to foreign investors is associated with fewer spillovers. On the other hand, a higher degree of trade openness is associated with more spillovers from inward FDI. The mode of entry of FDI is also important: fully foreign-owned investments generate less positive spillovers than joint projects of foreign and domestic firms. In the present article we take stock of the empirical research on FDI spillovers and provide a unique database of average estimates for each country examined in the literature.

Remarkably, we find no evidence of publication selection bias for any type of spillover when both published and unpublished studies are considered together. When only published studies are included, we detect substantial upward bias for backward spillovers, but no bias for horizontal and forward spillovers. Because the recent literature considers backward spillovers the most important spillover type, the results concerning backward spillovers are more likely to be polished. Moreover, theory diversity is lower for backward spillovers than for horizontal spillovers. If the competition effect of increased foreign presence gets negative and outweighs the effects of demonstration and labour turnover, horizontal spillovers altogether may well turn negative; on the contrary, no generally accepted theory exists that would allow for negative backward spillovers. Hence, in line with Doucouliagos and Stanley (2012), we find more publication bias in areas with less theory diversity.

Using the estimated meta-regression we retrieve the magnitude of publication bias for each study. Publication selection is stronger for studies that are co-authored by successful authors, where success is measured by the number of citations. Furthermore, authors affiliated

with US-based institutions appear to engage in more publication selection, possibly because of a higher pressure to publish. On the other hand, studies with larger data sets exhibit less publication bias, because they are likely to detect statistically significant results without much specification search. The extent of publication bias gradually decreases over time, which is consistent with the economics-research-cycle hypothesis. Finally, we find greater publication selection bias for studies that concentrate on the significance and magnitude of spillover coefficients.

In recent years, the methodology employed by studies estimating FDI spillovers has converged to what may be called ‘best practice’; at least given the existing data. The standard is to employ firm-level data (in contrast to data aggregated at the industry level, which were often employed in the past), use total factor productivity as the response variable and compute it by a method that takes into account the endogeneity of input demand (in contrast to ordinary least squares), estimate the resulting regression in differences, and control for the characteristics of firms (for example, the absorptive capacity) and industries (for example, the degree of competition). Larger data sets, available in recent years especially for emerging economies such as China, has allowed for more precise estimation and, according to our results, also helped reduce publication bias. In our view, the most important avenue for future research in this field is the examination of spillover determinants, both at the firm and country level. Our meta-analysis suggests that backward spillovers are positive and relatively large on average, but also that they differ significantly across countries. Once the sources of these differences are robustly identified, the literature may provide policy-makers with valuable guidance on which investors are the most beneficial to attract.

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Note

1. Some confusion exists in the meta-analysis literature concerning the term ‘fixed-effects estimation.’ Sometimes a simple OLS estimation is labelled ‘fixed effects’ to emphasise that no random-effect component is present; the traditional fixed-effects estimation, on the other hand, is rarely applied in meta-analysis since studies reporting only one estimate are dropped in the procedure (see, for instance, Nelson and Kennedy, 2009). In our case, though, the large data set of spillover estimates allows us to use the traditional fixed-effects estimation, which is less restrictive than random effects.

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Table A1. Meta-analyses for individual studies (published papers)

Study	Backward		Forward		Horizontal		N
	Effect	SE	Effect	SE	Effect	SE	
Atallah Murra (2006)	1.281***	0.132	0.848***	0.051	−0.023	0.079	20
Békés et al. (2009)	0.030	0.061	0.034**	0.017	0.040***	0.011	9
Blake et al. (2009)	0.065	0.040	0.002	0.006	−0.044***	0.012	21
Blalock and Gertler (2008)	0.087***	0.009			−0.009	0.007	10
Blalock and Simon (2009)	0.020	0.014			0.013*	0.007	24
Bwalya (2006)	1.108	0.734			−0.188***	0.067	22
Crespo et al. (2009)	0.058	0.149	−0.003	0.060	0.335	0.218	9
Gersl (2008)	1.389	0.926	0.962*	0.569	−0.152	0.203	12
Girma and Wakelin (2007)	0.280***	0.025	0.280***	0.025	0.099***	0.022	45
Girma and Gong (2008)	−0.083	0.112	0.185***	0.050	−0.001	0.003	120
Girma et al. (2008)	1.608**	0.712	−3.432	2.724	2.428***	0.736	75
Halpern and Muraközy (2007)	1.464***	0.131	−0.411	0.747	−0.223***	0.053	58
Jabbour and Mucchielli (2007)	0.088*	0.048	0.108***	0.035	−0.058***	0.013	33
Javorcik (2004)	3.267***	0.351	−0.445***	0.132	0.182*	0.096	80
Javorcik and Spatareanu (2008)	0.374***	0.075			−0.234***	0.044	66
Jordaan (2008)	0.625***	0.086	0.625***	0.086	−0.506***	0.061	38
Kolasa (2008)	0.211***	0.049	0.017	0.022	0.040***	0.009	12
Lin et al. (2009)	1.373***	0.117	3.553***	0.303	−0.114***	0.037	90
Liu (2008)	−0.174	0.125	0.046	0.094	−0.094*	0.051	18
Liu et al. (2009)	0.850***	0.073	1.26***	0.139	−0.010	0.045	108
Managi and Bwalya (2010)	5.086	4.135			7.135***	2.469	6
Qiu et al. (2009)	1.761***	0.123	−0.037	0.033	0.682***	0.117	21
Reganati and Sica (2007)	0.073***	0.023			0.079	0.085	6
Resmini and Nicolini (2007)	0.032***	0.005	0.027***	0.005			22
Sasidharan and Ramanathan (2007)	−0.044	0.338			0.050	0.125	6
Wang and Zhao (2008)	4.363***	0.718	4.363***	0.718	0.122***	0.034	14
Yudaeva et al. (2003)	−6.111***	1.162	−1.715***	0.256	1.547***	0.252	17
Zajc Kejzar and Kumar (2006)	0.138**	0.057	0.285***	0.060	0.025***	0.006	32

Notes: Spillover effects are estimated by the simple random-effects meta-analysis run separately for each study.

SE = standard error. N = number of the estimates of spillovers taken from the study.

***, **, and * denote significance at the 1 per cent, 5 per cent, and 10 per cent levels.

Table A2. Meta-analyses for individual studies (unpublished papers)

Study	Backward		Forward		Horizontal		N
	Effect	SE	Effect	SE	Effect	SE	
Barrios et al. (2009)	0.267	0.173	−0.791***	0.170	0.694***	0.164	71
Blyde et al. (2004)	0.375***	0.062	−0.096**	0.042	0.181***	0.057	188
Chang et al. (2007)	−0.027***	0.005	0.042***	0.005	0.105***	0.013	112
Damijan et al. (2003)	0.092*	0.052	−0.220***	0.083	0.015**	0.006	29
Damijan et al. (2008)	0.010	0.027			0.030***	0.011	104
Fernandes and Paunov (2008)			0.125***	0.009			52
Gersl et al. (2007)	−0.344	0.471	−1.041**	0.423	−0.065	0.068	153
Gonçalves (2005)	0.668***	0.120					2
Gorodnichenko et al. (2007)	0.084***	0.008	0.035***	0.007	0.020***	0.003	243
Hagemejer & Kolasa (2008)	2.919***	0.405	−0.159**	0.071	0.196***	0.032	36
Hale et al. (2010)	0.095**	0.041	0.047	0.036			160
Javorcik et al. (2004)	4.450***	0.652			0.452***	0.079	24
Le and Pomfret (2008)	1.062***	0.140			−0.825***	0.152	39
Leshner and Miroudot (2008)	−0.341***	0.102	−0.125	0.142	−0.047**	0.023	172
Liang (2008)	−0.216***	0.036	0.438***	0.049	0.008*	0.004	72
Lileeva (2006)	0.126*	0.075	1.544***	0.113	−0.322***	0.037	159
Merlevede and Schoors (2005)	−0.690***	0.167	2.293***	0.457	−0.073	0.166	45
Merlevede and Schoors (2007)	0.097	0.170	0.476***	0.160	−0.044	0.046	60
Merlevede and Schoors (2009)	0.692	1.003	0.181	2.263	2.251***	0.706	42
Nguyen et al. (2008a)	−0.158***	0.057	−3.327***	0.293	0.016	0.043	184
Nguyen et al. (2008b)	0.097	0.103	−0.487	0.320	−0.024	0.069	20
Schoors and van der Tol (2002)	2.794***	0.244	−3.902***	0.328	0.279***	0.064	54
Stancik (2007)	−1.715***	0.204	−0.279	0.189	−0.158***	0.034	69
Stancik (2009)	−0.787***	0.138	0.322	0.224	−0.023	0.037	84
Tang (2008)	−0.189***	0.043			−0.266***	0.022	257
Taymaz and Yilmaz (2008)	0.035**	0.015	0.064**	0.029	0.106**	0.052	53
Tong and Hu (2007)	0.228	0.415	0.228	0.415	−0.185	0.325	8
Vacek (2007a)	0.526***	0.044	−0.001	0.014			92
Vacek (2007b)	0.048	0.060	−0.003	0.038	0.013	0.012	48

Notes: Spillover effects are estimated by the simple random-effects meta-analysis run separately for each study.

SE = standard error. N = number of the estimates of spillovers taken from the study.

***, **, and * denote significance at the 1 per cent, 5 per cent, and 10 per cent levels.

Table A3. Meta-analyses for individual countries

Country	Backward		Forward		Horizontal		N
	Effect	SE	Effect	SE	Effect	SE	
Bosnia and Herzegovina	0.553	1.317			−0.268	0.362	8
Bulgaria	−0.333	0.564	−0.501*	0.268	−0.116	0.098	27
Canada	0.126*	0.075	1.544***	0.113	−0.322***	0.037	159
Chile			0.125***	0.009			52
China	0.145***	0.015	0.440***	0.023	−0.004	0.006	1,001
Colombia	1.281***	0.132	0.848***	0.051	−0.023	0.079	20
Croatia	0.160	0.108			0.020	0.040	8
Czech Republic	−0.150**	0.063	0.005	0.026	−0.036**	0.014	332
Estonia	0.119	0.253	1.311	1.066	−0.003	0.021	27
Hungary	1.479***	0.121	−0.930***	0.139	−0.023	0.024	148
India	−0.044	0.338			0.050	0.125	6
Indonesia	0.052***	0.011			0.002	0.004	34
Ireland	0.267	0.173	−0.791***	0.170	0.694***	0.164	71
Italy	0.073***	0.023			0.079	0.085	6
Latvia	−0.819*	0.465	0.110	0.579	−0.005	0.023	27
Lithuania	2.845***	0.350	−0.436***	0.129	0.081	0.084	89
Mexico	0.625***	0.086	0.625***	0.086	−0.506***	0.061	57
Poland	1.478***	0.220	−0.092**	0.042	0.099***	0.018	75
Portugal	0.058	0.149	−0.003	0.06	0.335	0.218	9
Romania	0.269**	0.111	1.327***	0.327	0.034	0.055	263
Russian Federation	−6.111***	1.162	−1.715***	0.256	1.547***	0.252	17
Slovakia	0.281*	0.165	−0.442	0.413	0.032	0.027	20
Slovenia	0.127**	0.062	−0.033	0.206	0.011***	0.004	40
Spain	0.088*	0.048	0.108***	0.035	−0.058***	0.013	33
Turkey	0.035**	0.015	0.064**	0.029	0.106**	0.052	53
Ukraine	15.051	12.755			−0.164	0.231	8
United Kingdom	0.293***	0.032	0.279***	0.024	0.104***	0.025	138
Venezuela	0.375***	0.062	−0.096**	0.042	0.181***	0.057	188
Vietnam	0.079	0.049	−3.059***	0.281	−0.038	0.040	243
Zambia	1.108	0.734			−0.188***	0.067	22
Advanced OECD countries ^a	−0.341***	0.102	−0.125	0.142	−0.047**	0.023	172
Transition countries ^b	0.085***	0.008	0.035***	0.007	0.020***	0.003	231

Notes: Spillover effects are estimated by the simple random-effects meta-analysis run separately for each country. Meta-analyses for countries for which we have less than five estimates are not reported, but are available on request.

SE = standard error. N = number of the estimates of spillovers for the country.

***, **, and * denote significance at the 1 per cent, 5 per cent, and 10 per cent levels.

^a Austria, Belgium, Finland, France, Germany, Luxembourg, Netherlands, Norway, Sweden.

^b Albania, Georgia, Kazakhstan, Serbia.