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Which Foreigners Are Worth Wooing? A Meta-Analysis of Vertical Spillovers from FDI

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Which Foreigners Are Worth Wooing? A Meta-Analysis of Vertical Spillovers from FDI

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Abstract:
The principal argument for subsidizing foreign investment is the assumed spillover of technology to local firms. Yet researchers report mixed results on spillovers. To examine the phenomenon in a systematic way, we collected 3,626 estimates from 57 empirical studies on between-sector spillovers and reviewed the literature quantitatively. Our results indicate that model misspecifications reduce the reported estimates, but that journals select relatively large estimates for publication. The underlying spillover to suppliers is positive and economically significant, whereas the spillover to buyers is insignificant. Greater spillovers are generated by investors that come from distant countries and that have only slight technological advantages over local firms. In addition, greater spillovers are received by countries that have underdeveloped financial systems and that are open to international trade.

Keywords: Foreign direct investment; Productivity; Spillovers; Meta-analysis; Publication selection bias

JEL: C83; F23

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1 Introduction

Few topics in international economics have been examined as extensively as technology transfer from foreign affiliates to domestic firms, and the amount of empirical research in this area is still growing at an exponential rate, with more than a score of studies published in the last two years alone. The topic is so attractive because the assumed externality associated with the transfer, "technology spillover," constitutes the principal rationale for government subsidies to foreign direct investment (FDI). Many policy makers who encourage inward FDI expect that domestic firms in the same sectors can benefit from know-how brought by foreigners, that firms in supplier sectors can benefit from direct knowledge transfers from foreigners, and perhaps also that firms in customer sectors can benefit from higher-quality intermediate inputs produced by foreigners. While foreign affiliates will try to prevent the transfer of technology to their competitors, foreigners have incentives to provide assistance to domestic suppliers since they want to ensure a high quality and on-time delivery of inputs. Thus the recent literature particularly emphasizes the between-sector linkages (Javorcik, 2004a; Blalock & Gertler, 2008). 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.

But even if we could not estimate the general effect, we can still explore the determinants that drive spillovers. Indeed, most of the recent research focuses on the heterogeneity in spillovers due to the different characteristics of the host countries, domestic firms, and foreign investors. The theoretical model of Rodriguez-Clare (1996) implies that spillovers to supplier sectors rise with the transportation costs between the foreign affiliate and its headquarters; Javorcik *et al.* (2004) corroborates this proposition using data from Romania. For Lithuania, Javorcik (2004a) finds that fully owned foreign affiliates create less beneficial linkages than projects with joint domestic and foreign ownership. Using data from Indonesia, Blalock & Simon (2009) emphasize the role of the absorption capacity of domestic firms on spillovers. In a theoretical model and calibration exercise, Alfaro *et al.* (2010) identify the level of development of the financial system of the host country as a major spillover determinant.

To take a step beyond single-country case studies and examine the sources of heterogeneity in a systematic way, we employ the meta-analysis methodology (Stanley, 2001). Meta-analysis, the quantitative method of research synthesis, has been commonly used in economics for two decades (Card & Krueger, 1995; Smith & Huang, 1995; Ashenfelter *et al.*, 1999). Recent applications of meta-analysis in international economics include Disdier & Head (2008) on the effect of distance on trade, Cipollina & Salvatici (2010) on reciprocal trade agreements, and Havranek (2010) on the trade effect of the euro. Meta-analysis is more than a literature survey: it sheds light on the determinants of the examined phenomenon that are difficult to investigate in primary studies because of data limitations. In comparison with previous meta-analyses on productivity spillovers (Görg & Strobl, 2001; Meyer & Sinani, 2009), this paper concentrates on between-sector instead of within-sector spillovers. We also include more estimates to investigate the full variability in the literature: 3,626 compared with 25 and 121. Finally, the previous meta-analyses used the reported t-statistics to evaluate the statistical significance of spillovers, whereas this paper uses an economic measure of spillovers and employs new synthesis methods.

We seek answers to three main questions. First, what is the unconditional spillover effect? It would be helpful to determine whether the literature indicates some general effect, or whether all positive results are country- or sector-specific. Novel meta-analysis methods allow us to estimate the underlying economic effect net of publication selection (the possible preference for significant or positive results) and misspecification biases. Second, is FDI from certain countries systematically more beneficial for domestic firms? Primary studies on spillovers do not usually have access to detailed information on the nationality of foreign investors. The meta-analysis approach is convenient as it can exploit the results for all 47 countries examined in the literature: Using inward FDI stocks as weights, we construct variables reflecting the differences between the host country of FDI and its source countries. Third, do some host countries receive greater spillovers? We create country-specific variables capturing macroeconomic determinants of spillovers and control for the aspects of data, methods, and study quality.

The remainder of the paper is structured as follows: Section 2 describes how spillovers are estimated and how we collected the estimates. Section 3 examines the underlying effect beyond publication bias. Section 4 investigates structural and method heterogeneity in the literature. Section 5 concludes. Appendix A provides meta-analyses for individual studies and countries, and Appendix B lists all the studies used in the meta-analysis.

2 The Spillover Estimates Data Set

Studies on technology spillovers from FDI usually examine the correlation between the productivity of domestic firms and their linkages with foreign affiliates.[1] With an allusion to the production chain, the linkages are usually classified into horizontal (within-sector: from FDI to local competitors) and vertical (between-sector); vertical linkages are further bifurcated into downstream (backward: from FDI to local suppliers) and upstream (forward: from FDI to local buyers). Most researchers use data from one country and estimate a variant of the following model, the so-called FDI spillover regression:

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

where i, j, and t denote firm, sector, and time subscripts, and *Controls* denote a vector of either sector- or firm-specific control variables. The variable *Horizontal* is the ratio of foreign presence in firm i's own sector, *Backward* is the ratio of firm i's output sold to foreign affiliates, and *Forward* is the ratio of firm i's inputs purchased from foreign affiliates. Because firm-level data on linkages with foreign affiliates are usually unavailable the vertical linkages are computed at the sector level: *Backward* becomes the ratio of foreign presence in downstream sectors,

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[1] See Smeets (2008) for a survey of the broader literature on knowledge spillovers from FDI, and Keller (2009) for a survey on international technology diffusion.

Forward becomes the ratio of foreign presence in upstream sectors; the weight of each upstream or downstream sector is determined by the input-output table of the country.

The relative homogeneity of FDI spillover regressions allows us to meta-analyze the economic effect of spillovers. Since the response variable is in logarithm and linkage variables are ratios the estimates of coefficients e_0^h, e_0^b, and e_0^f can be interpreted as the semi-elasticities and thus constitute the natural common metric for the spillover literature. In meta-analysis, semi-elasticity was previously used by Rose & Stanley (2005) or Feld & Heckemeyer (2009). In our case semi-elasticity is convenient for interpretation since it approximates the percentage increase in the productivity of domestic firms following an increase in the foreign presence of one percentage point:

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

For instance, the estimate $e^b = 0.1$ implies that a 10-percentage-point increase in foreign presence is associated with a 1% increase in the productivity of domestic firms in upstream sectors. The estimates are directly comparable across studies that use the log-level specification.[2] Within this framework, however, researchers use different methodologies and data sets, which cause substantial differences in results. We address these differences in Section 4 by including variables capturing method and structural heterogeneity.

The term “spillover” is overused in the literature; both horizontal and vertical semi-elasticities in (1) also capture effects other than technological externalities. As for horizontal linkages, the entry of foreign companies can lead to greater competition in the sector. Greater competition can either increase (through reducing inefficiencies) or decrease (through reducing market shares) the productivity of domestic firms. Neither case represents a technology transfer, and the coefficient e_0^h thus captures the net effect of technology spillovers and competition on productivity. As for vertical linkages, in the supplier-customer relationship the recipient of technology is clearly identifiable, and foreigners may be able to internalize the benefits (Blalock & Gertler, 2008; Keller, 2009). Anecdotal evidence suggests that compensations may indeed occur, though usually in an indirect form. For instance, in transition countries multinational companies are known to be hard bargainers: the discounted price of inputs that they often require likely reflects the future assistance and considerable prestige associated with such orders. For simplicity, we follow the convention to call the productivity semi-elasticities the “spillovers.” The key takeaway is that even positive and economically significant estimates of semi-elasticities do not necessarily call for governments to subsidize FDI.

A vast majority of the recent studies on FDI spillovers concentrate on vertical linkages, and vertical linkages are also the main focus of this paper. The two meta-analyses on horizontal spillovers, however, could not have used the recently developed meta-analysis methods. For this reason, additionally we present a partial meta-analysis of horizontal spillovers. In the partial meta-analysis, we include only those semi-elasticities that are estimated in the same regression with vertical spillovers.

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[2] Estimates from studies that define foreign presence on the interval [0, 100] are normalized.

We employed the following strategy for literature search: After reviewing the references of literature surveys (Görg & Greenaway, 2004; Smeets, 2008; Meyer & Sinani, 2009) and a few recent empirical studies, we elaborated a baseline search query that was able to capture most of the relevant studies. The baseline search in EconLit yielded 108 hits.[3] Then, we searched three other Internet databases (Scopus, RePEc, and Google Scholar) and added studies that were missing from the baseline search. Finally, we investigated the RePEc citations of the most influential study, Javorcik (2004a). The three steps provided 183 prospective studies, which were all examined in detail. The last study was added on 31 March 2010.

Studies that failed to satisfy one or more of the following criteria were excluded from the meta-analysis. First, the study must report an empirical estimate of the effect of vertical linkages on the measure of the productivity of domestic firms. Second, the study must define vertical linkages as a ratio. Third, the study must report information on the precision of estimates (standard errors or t-statistics), or authors must be willing to provide it. Most of the identified studies, although related to the FDI spillover literature, did not estimate vertical spillovers. We excluded a few studies that estimated vertical spillovers but did not define linkages as a ratio and thus could not be used to compute semi-elasticity (for example, Kugler, 2006; Bitzer *et al.*, 2008). We often had to ask the authors for sample means of linkage variables or for clarification of their methodology: about 20% of the studies could be included thanks to cooperation from the authors.[4] No study was excluded on the basis of language, form, or place of publication; we follow Stanley (2001) and rather err on the side of inclusion in all aspects of data collection. We therefore also use studies written in Spanish and Portuguese, Ph.D. dissertations, articles from local journals, working papers, and mimeographs; and control for study quality in the analysis. The final sample consists of studies that are listed in Appendix B. The complete list of excluded studies with reasons for exclusion is available in an online appendix at `meta-analysis.cz/spillovers`.

Following the recent trend in meta-analysis (Disdier & Head, 2008; Doucouliagos & Stanley, 2009; Cipollina & Salvatici, 2010), we use all estimates reported in the studies. If we arbitrarily selected the "best" estimate from each study, we could introduce an additional bias, and if we used the average reported estimate, we would discard a lot of information. Because the coding of the literature involved the manual collection of thousands of estimates with dozens of variables reflecting study design, both of us collected all data independently to eliminate errors. The simultaneous data collection took three months and the resulting disagreement rate, defined as the ratio of data points that differed between our data sets, was 6.7% (of more than 200,000 data points). After we had compared the data sets, we reached a consensus for each discordant data point. The retrieved data set with details on coding for each study is available in the online appendix.

A few difficult issues of coding are worth discussing. To begin with, some studies (3.7% of

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[3] The final query took the following form: (fdi* or "foreign direct investment*" or multinational* or transnational*) and (spillover* or externalit*) and (vertical or backward or forward or inter-industry or supplier*).

[4] We are grateful to Joze Damijan, Ziliang L. Deng, Adam Gersl, Galina Hale, Chidambaran Iyer, Molly Lesher, Marcella Nicolini, Pavel Vacek, and Katja Zajc-Kejzar for sending additional data, or explaining the details of their methodology, or both.

the observations; for instance, Girma & Wakelin, 2007) use the so-called regional definition of vertical spillovers. Researchers using the regional definition approximate vertical linkages by the ratio of foreign firms in the region, without using input-output tables. Such an approach does not distinguish between backward and forward linkages. Because the results are interpreted as vertical productivity spillovers from FDI, we include them in the analysis but create a dummy variable for this aspect of the methodology. Next, many researchers use more variables for the same type of spillover in one regression. For example, Javorcik (2004a) separately examines the effect of fully owned foreign affiliates and the effect of investments with joint foreign and domestic ownership. Since the distinction between those coefficients is economically important, we use both of them and create dummies for affiliates with full foreign ownership, partial ownership, and for more estimates of the same type of spillover taken from one regression. Finally, some studies report coefficients that cannot be directly interpreted as semi-elasticities. This concerns, most notably, specifications different from the log-level (1.7% of the observations); for these different specifications we evaluated semi-elasticity at sample means. Other studies use the interactions of linkage variables with other variables, typically absorption capacity (7.2% of the observations). Instead of omitting those estimates, we evaluate the marginal effects of foreign presence at sample means and control for this aspect in the multivariate analysis.[5]

The resulting data set includes 3,626 estimates of semi-elasticity taken from 57 studies, of which 27 are articles published in refereed journals, 2 are book chapters, and 28 are other publications including working papers and dissertations. The median number of estimates taken from one study is 45, and for each estimate we codified 55 variables reflecting study design. To put these numbers into perspective, consider Nelson & Kennedy (2009), who review 140 meta-analyses conducted in economics. They report that a median analysis includes 92 estimates (the maximum is 1,592) taken from 33 primary studies and uses 12 explanatory variables (the maximum is 41).

The oldest study in our sample was published in 2002 and the median study in 2008: in other words, a half of the studies was published in the last three years, which suggests that vertical spillovers from FDI are a lively area of research. The whole sample receives approximately 400 citations per year in Google Scholar. The median time span of the data used by these primary studies is 1996–2002, and all the studies combined use almost six million observations from 47 countries. While we cannot exploit the variability of the primary observations, we benefit from the work of 107 researchers that have analyzed these data thoroughly. The richness of the data sets and methods employed enables us to systematically examine the heterogeneity in results and to establish robust evidence for the effect of foreign presence on domestic productivity.

Several estimates of semi-elasticity do remarkably differ from the main population and remain so even after a careful re-checking of the data; a similar observation applies to the precision of the estimates (the inverse of standard error). Such extreme values, most of which come from

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[5] For example, if the spillover regression is specified in the following form: $\ln \text{Productivity}_{ijt} = e_1^b \cdot \text{Backward}_{jt} + e_2^b \cdot \text{Backward}_{jt} \cdot AC_{ijt} + \alpha \cdot \text{Controls}_{ijt} + u_{ijt}$, where AC denotes absorption capacity, we use the estimate of $e_0^b = e_1^b + e_2^b \cdot \overline{AC}$. to approximate semi-elasticity. We approximate the corresponding standard error as the estimate of $Se(e_0^b) = \sqrt{Se^2(e_1^b) + Se^2(e_2^b) \cdot \overline{AC}^2}$.

working papers and mimeographs, might lead to volatile results and degrade the graphical analysis. To account for outliers, some other large meta-analyses use the Grubbs test (Disdier & Head, 2008; Cipollina & Salvatici, 2010). But because we use precision to filter out publication bias, outlying values in precision could also invalidate the results. Thus, to detect outliers jointly in semi-elasticity and its precision, we use the multivariate method of Hadi (1994). By this procedure, run separately for each type of spillover, 4.87% of the observations are identified. It is worth noting that some researchers argue for using all observations in meta-analysis (Doucouliagos & Stanley, 2009). Nevertheless, under the assumption that better-ranked outlets publish more reliable results, the estimates identified here as outliers are of lower quality compared to the rest of the sample,[6] and although in the remainder of the paper we report the results for the data set without outliers, the inclusion of outliers does not affect the inference. These additional results are available on request.

The simple mean of the estimates of backward spillovers reaches 0.41 and is significantly different from zero at the 5% level, suggesting that an increase in foreign presence of 10 percentage points is associated with an increase in the productivity of domestic firms in upstream sectors of 4.1%, an economically important value. For forward spillovers the average is insignificant, and for horizontal spillovers it is statistically significant but economically negligible (-0.04). Nevertheless, these preliminary results should be treated with caution since they do not account for different study quality, within-study dependence, and, most notably, publication bias.

3 Consequences of Publication Bias

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 if we added the journal with the next highest impact factor (The World Economy) the inference would be similar.

Table 1 summarizes 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 incomparable with studies in our sample. 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

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[6] Studies that produce outliers have a significantly lower impact factor compared with the rest of the sample: the p-value of the t-test is 0.02 when the recursive RePEc impact factor is used. The advantage of the RePEc ranking is that it also includes working paper series; nevertheless, the results are similar when we use the Journal Citation Report (Thompson) impact factor, Scientific Journal Ranking (Scopus) impact factor, or eigenfactor score (`www.eigenfactor.org`).

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

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

Note: +, −, and ? denote the finding of positive, negative, and insignificant spillover effects.

[a] The author does not discriminate between backward and forward spillovers.

[b] Positive effect reported only for investments with joint foreign and domestic ownership.

[c] Positive long-run effect, negative short-run effect.

effects of horizontal linkages; others find insignificant effects. 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% 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 buyers is less important.[7]

The simple average will be a biased estimate of the "true" spillover if some results are more likely than others to be selected for publication. Publication selection bias, which has long been recognized as a serious issue in empirical economics research (De Long & Lang, 1992; Card & Krueger, 1995; Ashenfelter & Greenstone, 2004; Stanley, 2005), arises from the preference of editors, referees, or authors themselves for results that are statistically significant or consistent with the theory. Publication bias is likely to be stronger in areas with less theory competition, where a particular sign of estimates is inconsistent with any major theory; this hypothesis is supported empirically by Doucouliagos & Stanley (2008). Selection for significance amplifies this bias and creates a bias of its own every time the underlying effect is different from zero because the estimates with the wrong sign are less likely to be statistically significant.

The consequences of publication selection differ at the study and literature levels. For a well-known example, consider the effect of currency unions on within-union trade: it may be beneficial for an individual study to discard negative estimates since they are likely to result from model misspecification (or, in other words, there is no major theory consistent with the negative effect of common currency on trade). If, however, all researchers discard negative estimates, but some report large positive estimates that are also due to misspecification, the average impression from the literature will be biased towards a greater positive effect. This is precisely what the recent meta-analyses find (Rose & Stanley, 2005; Havranek, 2010). Publication bias affects

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[7] Keller & Yeaple (2009) use novel methods to show that horizontal linkages do significantly increase the productivity of domestic firms, at least in the USA. We exclude the study because it does not estimate vertical spillovers.

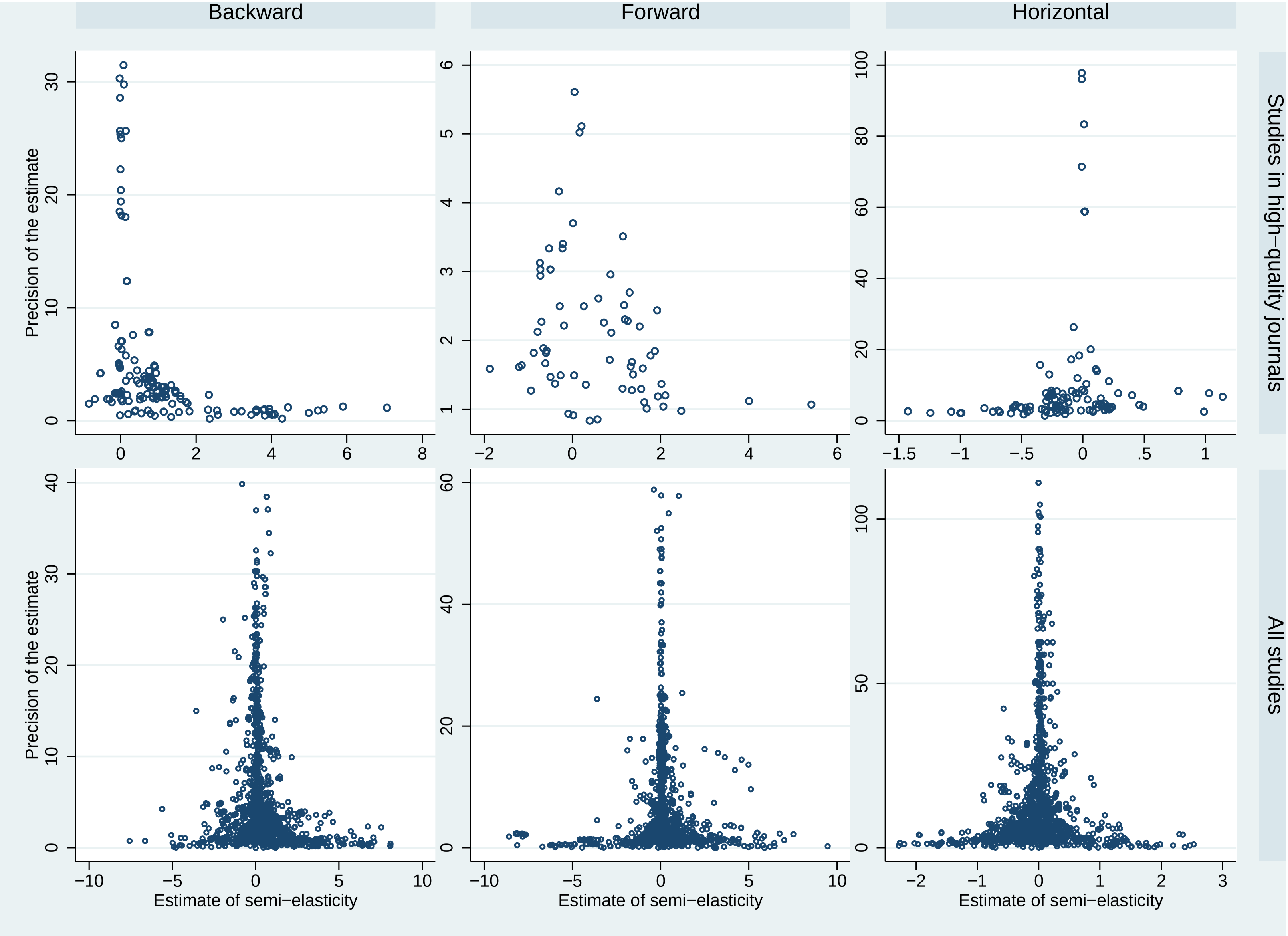

Figure 1: Funnel plots show publication bias in high-quality journals

both narrative and quantitative literature surveys, but the quantitative methods can identify the bias and estimate the true effect beyond.

While in the first meta-analysis of spillovers from FDI, Görg & Strobl (2001) identified publication bias among horizontal spillovers, in the last decade the selection for significance or positive signs has been more likely among backward spillovers. The change is due to increased theory competition for horizontal spillovers after the skeptical study of Aitken & Harrison (1999) was published, and to the last decade's consensus that backward spillovers are more important than forward and horizontal, following Javorcik (2004a) and Blalock & Gertler (2008). We first examine publication bias among studies published in high-quality journals, because outlets with higher standards require (or may be expected to require by authors) more intensive polishing, which could result in stronger publication bias.

A common method of detecting publication bias is an informal examination of the so-called funnel plot (Stanley & Doucouliagos, 2010). The funnel plot depicts the estimated semi-elasticity on the horizontal axis against the precision of the estimate on the vertical axis. While the most precise estimates are close to the true effect, the less precise are more dispersed; hence the cloud of estimates should resemble an inverted funnel. In the absence of publication bias the funnel is symmetrical since all imprecise estimates have the same chance of being reported.

The funnel plots for the estimates taken from high-quality journals are presented in the top panel of Figure 1. For backward spillovers, we detect strong publication bias: imprecise negative estimates of backward spillovers are almost entirely missing. According to the top portion of the funnel the simple average, 1.14, clearly exaggerates the true effect that seems to be small and hardly economically important; the average of the 10% of the most precise estimates is merely 0.05. On the other hand, forward and horizontal spillovers show only slight traces of publication bias: the right tail of the funnel for forward spillovers and the left tail for horizontal spillovers are somewhat heavier, but this can be due to sampling error. Although such visual tests are useful, they are inevitably subjective, and a more formal examination is thus necessary.

When the literature is free of publication bias the estimates of semi-elasticities are randomly distributed around the true population effect, e_0. If, however, some estimates end in the file drawer because they are insignificant or have an unexpected sign, the reported estimates will be correlated with their standard errors (Card & Krueger, 1995; Ashenfelter *et al.*, 1999):

$$e_i = e_0 + \beta_0 \cdot Se(e_i) + u_i, \quad u_i|Se(e_i) \sim N(0, \delta^2), \tag{3}$$

where β_0 measures the strength of publication bias. For instance, if a statistically significant effect is required, an author who has few observations may run a specification search until the estimate becomes large enough to offset the high standard errors. Specification (3) can be interpreted as a test of the asymmetry of the funnel plot; it follows from rotating the axes of the plot and inverting the values on the new horizontal axis. A significant estimate of β_0 then provides formal evidence for funnel asymmetry. Because specification (3) is likely heteroscedastic (the explanatory variable is a sample estimate of the standard deviation of the response variable; the heteroscedasticity is also apparent from the funnel plots), in practice it is usually estimated by weighted least squares (Stanley, 2005, 2008):

$$e_i/Se(e_i) = t_i = e_0 \cdot 1/Se(e_i) + \beta_0 + \xi_i, \quad \xi_i|Se(e_i) \sim N(0, \sigma^2). \tag{4}$$

Specification (4), often called the "meta-regression," likewise has a convenient interpretation: if the true semi-elasticity (e_0) is zero and if only positive and significant estimates are reported, the estimated coefficient for publication bias (β_0) will approach two, the most commonly used critical value of the t-statistic. It follows that the estimates of β_0 that are close to two signal serious selection efforts. Monte Carlo simulations and many recent meta-analyses suggest that this parsimonious test is also effective in filtering out publication bias and estimating true semi-elasticity (Stanley, 2008).

Since we use more estimates from each study, it is important to take into account that estimates within one study are likely to be dependent (Disdier & Head, 2008). Therefore, (4) is likely to be misspecified. A common remedy is to employ the mixed-effects multilevel model, which allows for unobserved between-study heterogeneity (Doucouliagos & Laroche, 2009; Doucouliagos & Stanley, 2009):

$$t_{ij} = e_0 \cdot 1/Se(e_{ij}) + \beta_0 + \zeta_j + \epsilon_{ij}, \quad \zeta_j|Se(e_{ij}) \sim N(0, \psi), \quad \epsilon_{ij}|Se(e_{ij}), \zeta_j \sim N(0, \theta), \tag{5}$$

where i and j denote estimate and study subscripts. The overall error term (ξ_{ij}) consists of study-level random effects (ζ_j) and estimate-level disturbances (ϵ_{ij}), and its variance is additive because both components are assumed to be independent: $\mathrm{Var}(\xi_{ij}) = \psi + \theta$, where ψ denotes within-study variance and θ between-study variance. When ψ approaches zero the benefit of using the mixed-effect multilevel estimator instead of simple ordinary least squares (OLS) becomes negligible. To put the magnitude of these variance terms into perspective the within-study correlation is useful: $\rho \equiv \mathrm{Cor}[t_{ij}, t_{i'j}|Se(e_{ij}), Se(e_{i'j})] = \psi/(\psi + \theta)$. It represents the degree of dependence of the estimates reported within the same study, or equivalently, the degree of between-study heterogeneity.

The mixed-effects multilevel model is analogous to the random-effects model commonly used in panel-data econometrics. The terminology, however, follows hierarchical data modeling: the model is called "mixed-effects" since it contains a fixed (e_0) as well as a random part (ζ_j). For the purposes of meta-analysis the multilevel framework is more suitable because it takes into account the unbalancedness of the data (the restricted maximum likelihood estimator is used instead of generalized least squares) and allows for nesting multiple random effects (author-, study-, or country-level), and is thus more flexible (Nelson & Kennedy, 2009).

Table 2 presents the results of the test of publication bias and the true effect for studies published in high-quality journals. Because we have few of such studies (especially for forward spillovers), the study-level random effect will hardly be normally distributed; hence, as a robustness check, we report OLS with standard errors clustered at the study level.[8] The results confirm that publication bias is present among the estimates of backward spillovers. Although

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[8] Although we also try clustering at the author and country level, the results are similar, as well as for nested models with country-, author-, and study-level random effects, and are therefore not reported. Likelihood-ratio

Table 2: Test of publication bias and true effect, studies in high-quality journals

	Backward		Forward		Horizontal	
	ME	OLS	ME	OLS	ME	OLS
Intercept (bias)	1.118*	1.960***	-0.615	1.513	-0.591	-0.399
	(0.583)	(0.487)	(1.235)	(1.071)	(0.533)	(0.563)
$1/Se$ (effect)	0.0302	-0.0482	0.367*	-0.360	0.0113	0.00715
	(0.0272)	(0.0363)	(0.218)	(0.158)	(0.0129)	(0.00970)
Observations	143	143	66	66	112	112
Studies	7	7	3	3	7	7

Note: Standard errors in parentheses. Response variable: t-statistic of the estimate of semi-elasticity.
ME = the mixed-effects multilevel model. OLS = ordinary least squares with clustered standard errors.
*** and * denote significance at the 1% and 10% levels.

in the mixed-effects model the estimate of β_0 is significant only at the 10% level (p-value = 0.055), evidence for publication bias is solid considering that this test is known to have relatively low power (Stanley, 2008) and that OLS reports an estimate that is significant at the 1% level. The magnitude of publication bias is high (1.12 for mixed effects and 1.96 for OLS), which implies a substantial selection for significance or positive signs in high-quality journals. The estimated true effect of backward linkages, net of publication selection, is insignificant. As for forward and backward spillovers, the estimated true effects are also insignificant, and we find no evidence of publication bias.

The meta-regression analysis shows how the estimated effect of backward linkages decreases from a large and significant value (the overall impression from Table 1 or the simple average) to a small and insignificant value when publication selection bias is filtered out from high-quality journals. An important finding is that the selection is more prominent among the results that are deemed to be more important (backward spillovers) than among the bonus results (forward and horizontal spillovers). Since the important results determine the main message of the study, they are more likely to be polished.

While estimates from studies published in high-quality journals are our most reliable observations, we need to include more studies to diminish the sampling error. The funnel plots for the full sample of studies are depicted in the bottom panel of Figure 1 and are clearly symmetrical. The meta-regression results, reported in Column 1 of Table 3, suggest that all types of spillover are free of publication bias. When we consider only estimates from studies published in refereed journals (Column 2), publication bias is detected for backward spillovers, and its magnitude is only slightly lower than in high-quality journals. A question remains open whether the selection is caused by the preference of journals or by the preference of authors. Nevertheless, since there is no publication bias in the literature as a whole, the results indicate that a majority of researchers do not expect significant and positive estimates to be more publishable and do not polish working papers in that respect. Indeed, the average reported estimate of backward spillovers reaches 0.88 in journal articles, but merely 0.22 in unpublished papers.

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tests suggest that the nested effects are insignificant, and we thus use study-level random effects (or study-level clustering) for all regressions in this paper. A Stata program is available at irsova.info/meta-analysis.

Table 3: Test of publication bias and true effect, all studies

	Mixed-effects multilevel			Robust
Backward Spillovers	All	Published	Homogeneous	All
Intercept (bias)	-0.0255	1.083*	-1.481	1.509
	(0.496)	(0.656)	(0.942)	(1.038)
$1/Se$ (effect)	0.168***	0.178***	0.307***	0.0371*
	(0.0241)	(0.0295)	(0.0380)	(0.0188)
Within-study correlation	0.38	0.64	0.51	
Observations	1311	370	568	56
Studies	55	26	39	56
	Mixed-effects multilevel			Robust
Forward Spillovers	All	Published	Homogeneous	All
Intercept (bias)	0.729	-0.437	1.657	-0.287
	(0.776)	(1.033)	(1.632)	(0.710)
$1/Se$ (effect)	0.0872***	0.258***	0.0669**	0.0294***
	(0.0287)	(0.0454)	(0.0288)	(0.00960)
Within-study correlation	0.37	0.77	0.79	
Observations	1030	241	591	45
Studies	44	19	30	45
	Mixed-effects multilevel			Robust
Horizontal Spillovers	All	Published	Homogeneous	All
Intercept (bias)	0.363	0.512	0.818	0.800
	(0.295)	(0.498)	(0.500)	(0.784)
$1/Se$ (effect)	0.00466	0.0137	0.000549	0.00624
	(0.00722)	(0.00837)	(0.0127)	(0.00739)
Within-study correlation	0.25	0.61	0.33	
Observations	1154	305	471	52
Studies	52	27	37	52

Note: Standard errors in parentheses. Response variable: t-statistic of the estimate of semi-elasticity.
Robust = the simple random-effects meta-analysis is run for each study separately; then, using an MM-estimator, the meta-regression is run on the results. All = all estimates. Published = only estimates from studies published in refereed journals. Homogeneous = only estimates for which no adjustment was needed, which use the standard definition of spillover variables, and which come from firm-level panel-data studies.
***, **, and * denote significance at the 1%, 5%, and 10% levels.

For all types of spillovers the within-study correlation is approximately two times higher among journal articles than among unpublished papers, which suggests that journal articles are more heterogeneous. Perhaps the greater heterogeneity arises from the greater originality that is required from a publishable manuscript. In any case the within-study correlation is substantial for all specifications (0.25–0.79), and the hypothesis of no between-study heterogeneity is rejected at the 1% level by likelihood-ratio tests in favor of the mixed-effects model.

As a robustness check, we consider only one estimate representing each study. Instead of arbitrarily selecting the "best" estimates, we approximate the representative estimates by the so-called simple random-effects meta-analysis. The simple meta-analysis weighs each estimate by its precision and adds an estimate-level random effect to account for within-study heterogeneity; the procedure is robust, and hence we also include the observations previously identified as outliers.[9] Since some studies provide only a few estimates, simple meta-analysis is more suitable for summarizing individual studies than is the meta-regression, because the meta-regression needs more degrees of freedom. The representative estimates for each study are reported in Table A1. Consequently, the meta-regression is run on the representative estimates using a robust MM-estimator (Verardi & Croux, 2009). The results are consistent with the mixed-effects model.

The estimated semi-elasticity beyond publication bias is consistently positive and significant across all specifications for vertical spillovers, but the semi-elasticity for horizontal spillovers is consistently insignificant. For inferences concerning the magnitude of spillovers we prefer a more homogeneous subset that consists only of estimates which come from firm-level panel-data studies, which use the standard definition of spillover variables, and for which no computation of the marginal effect was needed (Column 3 of Table 3). The preferred estimate suggests that a 10-percentage-point increase in foreign presence is associated with a 3% increase in the productivity of domestic firms in upstream sectors, an effect four times smaller than the simple average of estimates published in high-quality journals. For domestic firms in downstream sectors the increase in productivity is only 0.7%.

Therefore, when we use all available studies and account for publication bias and unobserved heterogeneity, backward spillovers are found to be economically important, forward spillovers to be statistically significant but small, and horizontal spillovers to be insignificant. Since these effects are average across all countries and methods, we need multivariate analysis to explain the vast differences in the reported effects. The reported effects may be systematically influenced by misspecifications or other quality aspects. In the next section, focusing only on backward spillovers as the most important channel of technology transfer, we relax the assumption that all heterogeneity across studies is unobservable and describe the determinants of spillovers.

4 What Explains Heterogeneity

The recent literature on productivity spillovers emphasizes that the benefits of FDI depend on the characteristics of host countries, individual recipient firms, and foreign investment (Crespo & Fontoura, 2007; Smeets, 2008; Meyer & Sinani, 2009). We label such differences in reported estimates "structural heterogeneity" to distinguish them from the heterogeneity that is caused by the use of different methods. Concerning cross-country structural heterogeneity, Figure 2 depicts the differences in the estimates of backward spillovers reported for European countries. The figure is based on the simple random-effects meta-analysis run separately for each country; the numerical results are summarized in Table A2. It is readily apparent that the effects of backward linkages are substantially heterogeneous. While at this point 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.

Although a lot of these differences are likely to be caused by the different methods used, we

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[9] The random-effects meta-analysis is a weighted average with the weight of the ith estimate from the jth study equal to $1/[Se^2(e_{ij}) + \hat{\tau}_j]$, where $e_{ij} \sim N(e_{j0}, \tau_j)$.

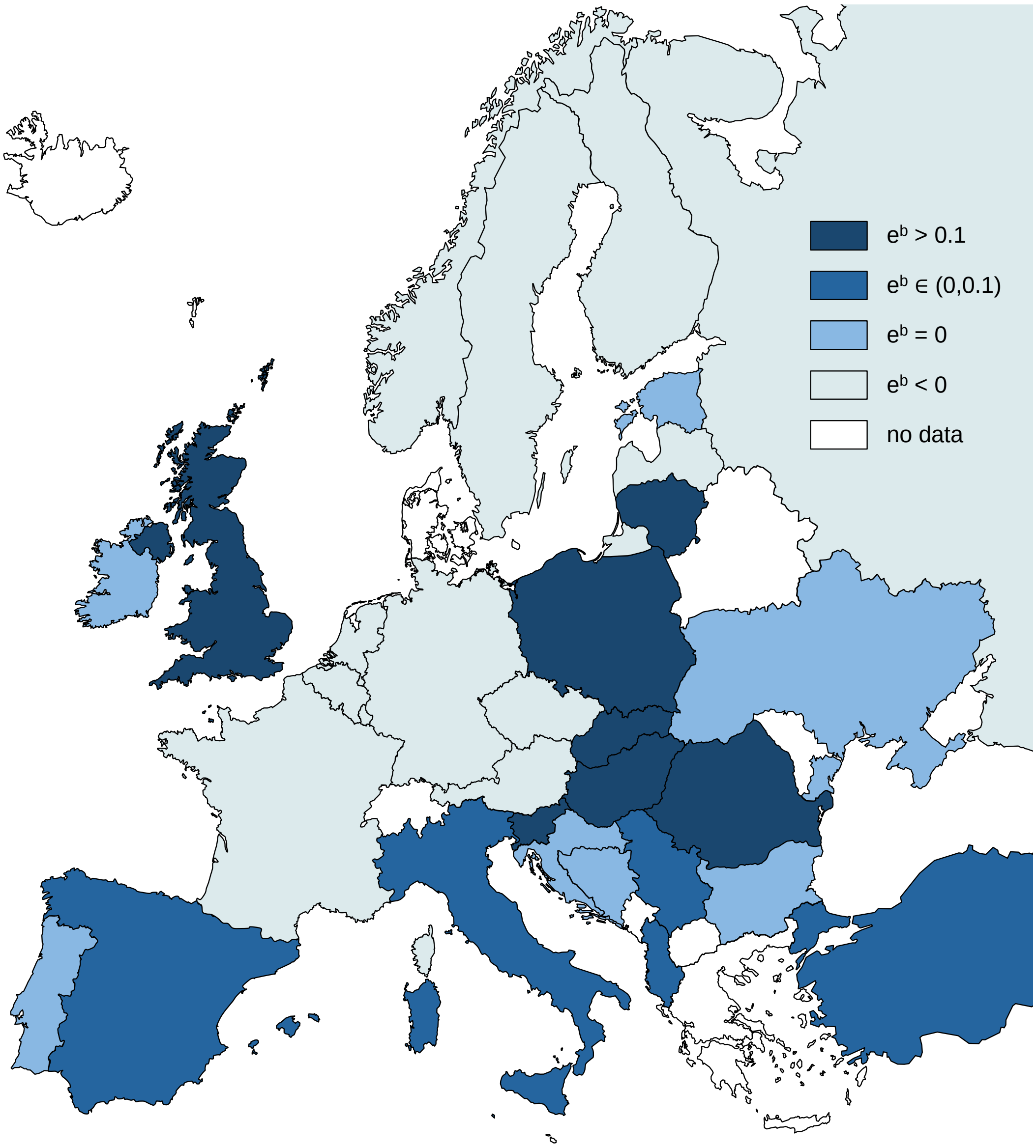

Figure 2: Cross-country heterogeneity in backward spillovers

find heterogeneity even among the results of studies employing the same method to examine more countries (for instance, Gersl *et al.*, 2007) or among countries examined by many studies. The results for Romania and the Czech Republic provide an illustrative example. Since researchers often choose transition countries to investigate FDI spillovers, for both countries we have eight studies employing a large variety of methods. The estimated semi-elasticity for Romania reaches 0.27, but for the Czech Republic it is negative and reaches -0.15, both significant at the 5% level. On the other hand, to visualize the high degree of heterogeneity due to different methodologies, Figure A1 shows a box plot of studies on China. Clearly both structural and method heterogeneity play an important role in the spillover literature and have to be accounted for in a multivariate framework. While it is the structural heterogeneity that is of principal interest, ignoring the differences in method and publication characteristics could lead to misleading results because some countries are only examined by one study.

Table 4 presents the descriptions and summary statistics of variables that may influence the reported magnitude of spillovers. We divide them into five blocks: variables explaining structural heterogeneity represent the real determinants, data characteristics represent the properties of the data used, specification characteristics represent the basic design of the tested models, estimation characteristics represent the econometric strategy, and publication characteristics represent the differences in quality not captured by the data and method variables.

Table 4: Description and summary statistics of regression variables

Variable	Description	Mean	SD
t-statistic	The t-statistic of the estimate of semi-elasticity. The response variable.	0.803	4.997
$1/Se$	The precision of the estimate of semi-elasticity.	5.465	6.640
Structural heterogeneity			
Distance	The logarithm of the country's FDI-stock-weighted distance from its source countries of FDI (kilometers).	7.769	0.621
Technology gap	The logarithm of the country's FDI-stock-weighted gap in GDP per capita with respect to its source countries of FDI (USD, constant prices of 2000).	9.816	0.419
Openness	The trade openness of the country: (exports + imports)/GDP.	0.704	0.330
Financial development	The development of the financial system of the country: (domestic credits to private sector)/GDP.	0.614	0.428
Patent rights	The Ginarte-Park index of patent rights of the country.	2.993	0.800
Fully owned	=1 if only fully owned foreign investments are considered for linkages.	0.069	0.253
Partially owned	=1 if only investments with joint domestic and foreign ownership are considered for linkages.	0.070	0.256
Services	=1 if only firms from service sectors are included in the regression.	0.046	0.209
Data characteristics			
Cross-sectional	=1 if cross-sectional data are used.	0.079	0.269
Aggregated	=1 if sector-level data for productivity are used.	0.033	0.178
Time span	The number of years of the data used.	7.090	3.788
Firms	The logarithm of [(the number of observations used)/(time span)].	7.598	2.040
Average year	The average year of the data used (2000 as a base).	-1.053	3.798
Amadeus	=1 if the Amadeus database by Bureau van Dijk Electronic Publishing is used.	0.223	0.416
Specification characteristics			
Forward	=1 if forward spillovers are included in the regression.	0.655	0.475
Horizontal	=1 if horizontal spillovers are included in the regression.	0.866	0.341
Employment	=1 if employment is the proxy for foreign presence.	0.142	0.349
Equity	=1 if equity is the proxy for foreign presence.	0.060	0.238
All firms	=1 if both domestic and foreign firms are included in the regression.	0.252	0.435
Absorption	=1 if the specification controls for absorption capacity using technology gap or R&D spending.	0.070	0.256
Competition	=1 if the specification controls for sector competition.	0.272	0.445
Demand	=1 if the specification controls for demand in downstream sectors.	0.075	0.263
Regional	=1 if vertical spillovers are measured using the ratio of foreign firms in the region as a proxy for foreign presence.	0.037	0.188
Lagged	=1 if the coefficient represents lagged foreign presence.	0.127	0.334
More	=1 if the coefficient is not the only estimate of backward spillovers in the regression.	0.459	0.499
Combination	=1 if the coefficient is a marginal effect computed using a combination of reported estimates.	0.072	0.259
Estimation characteristics			

Continued on next page

Table 4: Description and summary statistics of regression variables (continued)

Variable	Description	Mean	SD
One step	=1 if spillovers are estimated in one step using output, value added, or labor productivity as the response variable.	0.429	0.495
Olley-Pakes	=1 if the Olley-Pakes method is used for the estimation of TFP.	0.187	0.390
OLS	=1 if OLS is used for the estimation of TFP.	0.107	0.309
GMM	=1 if the system GMM estimator is used for the estimation of spillovers.	0.089	0.285
Random	=1 if the random-effects estimator is used for the estimation of spillovers.	0.031	0.174
Pooled OLS	=1 if pooled OLS is used for the estimation of spillovers.	0.157	0.364
Year fixed	=1 if year fixed effects are included.	0.854	0.353
Sector fixed	=1 if sector fixed effects are included.	0.494	0.500
Differences	=1 if the regression is estimated in differences.	0.456	0.498
Translog	=1 if the translog production function is used.	0.076	0.266
Log-log	=1 if the coefficient is taken from a specification different from log-level.	0.017	0.128
Publication characteristics			
Published	=1 if the study was published in a refereed journal.	0.288	0.453
Impact	The recursive RePEc impact factor of the outlet. Collected in April 2010.	0.238	0.453
Study citations	The logarithm of [(Google Scholar citations of the study)/(age of the study) + 1]. Collected in April 2010.	1.160	1.110
Native	=1 if at least one co-author is native to the investigated country.	0.712	0.453
Author citations	The logarithm of (the number of RePEc citations of the most-cited co-author + 1). Collected in April 2010.	3.114	2.480
US-based	=1 if at least one co-author is affiliated with a US-based institution.	0.397	0.489
Publication date	The year and month of publication (January 2000 as a base).	7.865	1.637

Note: SD = standard deviation.

Structural heterogeneity The structural block includes five variables that are computed at the host-country level and three dummy variables that reflect the characteristics of FDI and domestic firms. For the country-specific variables, we select values from 1999, the median year of the data used in primary studies. This approach can be supported by three reasons: First, because of data limitations it is not feasible to construct the variables as study-specific averages over the data periods of the individual studies. Second, all the studies were published between 2002 and 2010, and most of them use short and similar data periods. Third, we are interested in the relative differences between countries. When studies pool together data for more countries in one spillover regression (there are two such studies), we use population-weighted values for all variables.

Our main aim is to test the implications of the theoretical model by Rodriguez-Clare (1996), which indicates that positive backward spillovers are more likely to occur when the costs of communication between the foreign affiliate and its headquarters are high and when the source and host country of FDI are not too different in terms of the variety of intermediate goods produced. As suggested by Rodriguez-Clare (1996), communication costs can be approximated by the distance between the host and source countries of FDI, and country similarity can be approximated by the difference in the level of development. Both implications have an intuitive interpretation: On the one hand, investors from distant countries are likely to use more local inputs since it is expensive for them to import inputs from home countries; on the other hand, investors from much more developed countries are likely to use less local inputs since local firms are often unable to produce intermediate goods that would comply with the quality standards of the investors. A higher share of local inputs indicates more linkages with local firms and a greater potential for technology transfer.

To create a variable that would reflect the distance between the host country and its source countries of FDI, we need each country's geographic breakdown of inward FDI stocks, but such information is not always directly available. Hence, we use breakdowns of outward FDI positions of OECD countries provided by the OECD's International Direct Investment Statistics to reconstruct the breakdowns of inward FDI for all 47 countries that have been examined in the spillover literature. In 1999, OECD countries accounted for more than 85% of the world stock of outward FDI. We additionally obtain breakdowns from the statistical offices of the next three most important source countries of FDI: Hong Kong, Taiwan, and Singapore, which increases the total coverage to 95%. It is necessary to take into account that some authors already separate the linkage effects of investors of different nationalities; for example, many studies on China separate ethnic Chinese investors (Hong Kong, Macao, Taiwan) from Western investors. Hence, we use three different breakdowns for China: the first for all investors, the second for Western investors, and the third for ethnic Chinese.

The data on distances come from the CEPII database (`www.cepii.org`) and are computed following the great circle formula. The distance variable is then calculated using FDI breakdowns as weights. For example, if 70% of inward FDI stock in Mexico originated in the USA, 20% in Germany, and 10% in Japan, the average distance of foreign affiliates in Mexico from their headquarters would be $0.7 \cdot 1{,}600 + 0.2 \cdot 9{,}500 + 0.1 \cdot 11{,}000 = 4{,}120$ kilometers. We employ a similar approach to calculate the average technology gap of host countries with respect to the stock of inward FDI, measuring the development of the country as GDP per capita. The source of the data, similar to all remaining country-specific variables with the exception of patent rights, is the World Bank's World Development Indicators.

Another important determinant of spillovers is the international experience of domestic firms, which we approximate by the trade openness of the country. Firms with international experience may benefit more from backward linkages since they are used to trading with foreign firms and, for example, have employees with the necessary language skills. Such firms have a higher capacity to absorb spillovers; on the other hand, since they are already exposed to foreign firms in international markets they may have less potential to learn from foreign investors. But firms exposed to international competition are also more likely to produce intermediate goods required by foreign affiliates, and hence, in line with Rodriguez-Clare (1996), may benefit from greater spillovers.

As a major precondition of positive spillovers, many researchers stress the financial development of the host country (Javorcik & Spatareanu, 2009; Alfaro *et al.*, 2010): if domestic firms have difficulty obtaining credits, they react rigidly to the demand of foreign affiliates, and the sluggish response can result in fewer linkages. On the other hand, if the inflow of FDI eases the existing credit constraints of domestic firms by bringing in scarce capital (Harrison *et al.*, 2004), better credit terms reflect in higher productivity, and the benefits of FDI are more important in countries with tougher credit constraints. We approximate the development of the financial system by the ratio of private debts to GDP.

Since countries with weak protection of intellectual property rights are likely to attract relatively low-technology investors (Javorcik, 2004b), the potential for technology transfer in these countries is likely to be lesser. If a smaller technology gap, however, contributes to more linkages because of the greater similarity between foreign and domestic firms then the effect will be opposite. Additionally, weak protection of intellectual property enables domestic firms to copy foreign technology with less cost. To approximate the protection of intellectual property, we choose the Ginarte-Park index of patent rights; the source of the data is Walter G. Park's website[10] and Javorcik (2004b). The index is calculated once every five years, and values for 1999 are unavailable. Because Javorcik (2004b) computed the 1995 index for most of the originally missing transition countries that we need, we use the values for 1995. If we replace them by values for 2000 the results will remain similar.

The other structural variables are dummies capturing the degree of foreign ownership used to define foreign presence or the investigated sector of the domestic economy. Many researchers argue that fully owned foreign affiliates create fewer spillovers compared with joint foreign and domestic projects (Javorcik & Spatareanu, 2008) since joint projects will arguably use technology that is more accessible to domestic firms. Some authors estimate spillovers separately for service sectors, which allows us to test the hypothesis that firms in services, compared with manufacturing firms, are less likely to benefit from linkages. Firms in services may lack international experience since they exhibit lower export propensity.

Data characteristics Following Görg & Strobl (2001) we include dummy variables for cross-sectional data and aggregation at the sector level, even though more than 90% of the estimates come from firm-level panel-data studies. Because the size of data sets used by primary studies varies substantially, we control for the number of years and firms to find out whether smaller studies report systematically different outcomes. We include the average year of the data period to control for possible structural changes in the effects of FDI. Finally, because a large part of studies on European countries use data from the same source (the Amadeus database), we include a corresponding dummy variable.

Specification characteristics The variables capturing method heterogeneity are roughly divided into specification and estimation characteristics. Concerning specification characteristics, we construct dummies for the inclusion of the other spillover variables in the same regression (forward and horizontal), the proxy for foreign presence (most studies use share in output, others in employment or equity), the subset of firms used for the estimation of spillovers (whether all firms or only domestic are included), the inclusion of important control variables (sector competition and demand in downstream sectors), the control for absorption capacity, and the use of a lagged, instead of a contemporaneous, linkage variable.

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[10] `http://www1.american.edu/cas/econ/faculty/park.htm`

Estimation characteristics Although the majority of studies use total factor productivity (TFP) as the measure of productivity, some estimate spillovers in one step using output, value added, or labor productivity as the response variable. When computing TFP, most authors take into account the endogeneity of input demand and use the Levinsohn-Petrin or Olley-Pakes method, but 10% of all estimates are computed using OLS. In the second step, TFP is regressed on the linkage variable, and the estimation is usually performed using firm fixed effects. We create dummies for random effects and pooled OLS as well as for the inclusion of year and sector fixed effects. Approximately a half of the regressions are estimated in differences. A general-method-of-moments estimator (GMM) is employed by 9% of the regressions, and the translog production function instead of the Cobb-Douglas function is employed by 8% of them.

Publication characteristics To control for the different quality of studies, we include a dummy for publication in refereed journals, the recursive RePEc impact factor of the outlet (the results are similar when different impact factors are used), the number of Google Scholar citations of the study discounted by study age (citations from Thompson or RePEc provide much less variation), and the number of RePEc citations of the co-author who is most frequently cited. We also include a dummy variable for studies where at least one co-author is "native" to the examined country. We consider authors to be native if they either were born in the examined country or obtained an academic degree there. We hypothesize that such researchers are more familiar with the data at hand, which could contribute to the quality of analysis. To account for any systematic difference between the results of researchers affiliated in the USA (for our sample it usually means highly ranked institutions) and elsewhere, we add a dummy for studies where at least one co-author is affiliated with a US-based institution. Finally, publication date (year and month) is included to capture the publication trend: possibly the advances in methodology that are otherwise difficult to codify.

Although we have additionally codified other variables reflecting data and methodology (among others the degree of aggregation of the linkage variable and the number of input-output tables used), the variation in these variables is too low to bring any useful information.

To investigate the pattern of heterogeneity in the spillover literature, we add the explanatory variables listed in Table 4 into (3), and again divide the resulting equation by the standard error to correct for heteroscedasticity and add the random-effects component to account for within-study dependence. The multivariate meta-regression then takes the following form (Doucouliagos & Stanley, 2009; Cipollina & Salvatici, 2010):

$$t_{ij} = \beta_0 + e_0/Se(e_{ij}) + \boldsymbol{\beta}\mathbf{x}'_{ij}/Se(e_{ij}) + \zeta_j + \epsilon_{ij}, \qquad (6)$$

where $\mathbf{x}_{ij} = (x_{1ij}, \ldots, x_{pij})$ is the vector of explanatory variables, $\boldsymbol{\beta} = (\beta_1, \ldots, \beta_p)$ is the vector of the corresponding regression coefficients, and the exogeneity assumptions are $\zeta_j | Se(e_{ij}), \mathbf{x}_{ij} \sim N(0, \psi)$ and $\epsilon_{ij} | Se(e_{ij}), \mathbf{x}_{ij}, \zeta_j \sim N(0, \theta)$. Here e_0 is conditional on $\mathbf{x}$; that is, it represents the true effect in the reference case ($\mathbf{x}_{ij} = \mathbf{0}$).

The high degree of unbalancedness of the data makes a reliable testing of the exogeneity assumptions difficult.[11] Hence, as a specification check, meta-analysts usually employ OLS with clustered standard errors (Disdier & Head, 2008; Doucouliagos & Laroche, 2009). In the previous section, however, we have shown that the within-study dependence in our data is substantial and thus that OLS is misspecified. The principal problem with OLS is that it gives each estimate the same weight, which causes that studies reporting lots of estimates become overrepresented. The mixed-effects multilevel model, on the other hand, gives each study approximately the same weight if the within-study dependence is high (Rabe-Hesketh & Skrondal, 2008, p. 75). For all specifications in our analysis, the significance of within-study dependence is confirmed by likelihood-ratio tests at the 1% level. Yet large differences between the estimates based on OLS and on mixed effects may signal a violation of the exogeneity assumptions, and we therefore report both models, although the mixed-effects model is preferred.

We begin by including all explanatory variables into the regression; this general model is not reported, but is available on request. The only substantial correlations appear between the structural country-specific variables, and all variance inflation factors are lower than 10, suggesting only slight multicollinearity. To obtain a more parsimonious model, we employ the Wald test and exclude the control (data, method, and publication) variables that are jointly insignificant at the 10% level, but keep all structural variables. The results for structural variables are reported in Table 5; the significant control variables are included in all regressions (the results for control variables are reported in Table A3). All structural variables are included in the specification reported in Column 1; the specifications in Columns 2 and 3 omit some of them to avoid the relatively high correlations (the highest one reaches 0.68), but the coefficients do not change a lot. The results are similar even if the effects of the country-specific variables are examined one by one in separate regressions.

There are two structural variables that are individually insignificant, and they are also jointly insignificant with the previously excluded control variables. Omitting all jointly insignificant variables yields our preferred "specific" model; that is, the model without redundant variables. The specific model is then re-estimated using OLS with standard errors clustered at the study level. Although three structural variables become less significant using OLS (their p-values range between 0.1 and 0.2), the coefficients for all structural and control variables retain the same sign, which indicates that the mixed-effects model is correctly specified. Moreover, two of the three less significant structural variables become significant at standard levels when country-level clustering is used. The pseudo R^2s of about 0.4 show that a lot of heterogeneity still remains unexplained. But such values are common for meta-analysis because of the microeconomic nature of the data (see, for instance, Disdier & Head, 2008). All of the qualitative results are robust to the inclusion of outliers.

Our most important finding concerns the effects of the nationality of foreign investors on the

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[11] Fixed effects in the panel-data sense are generally inappropriate for meta-analysis since some studies report only one usable estimate; additionally, fixed effects make it impossible to examine the effect of study-level explanatory variables. As Nelson & Kennedy (2009, p. 358) put it: "The advantages of random-effects estimation [in meta-analysis] are so strong that this estimation procedure should be employed unless a very strong case can be made for its inappropriateness."

Table 5: Structural heterogeneity in backward spillovers

	Mixed-effects multilevel				OLS
	Full	Subset 1	Subset 2	Specific	Specific
Distance	0.247^{***}	0.258^{***}		0.249^{***}	0.217^{***}
	(0.0538)	(0.0520)		(0.0536)	(0.0671)
Technology gap	-0.513^{***}		-0.462^{***}	-0.386^{***}	-0.370^{***}
	(0.141)		(0.0880)	(0.103)	(0.131)
Openness	0.441^{***}	0.646^{***}		0.409^{***}	0.266
	(0.125)	(0.0997)		(0.122)	(0.192)
Financial development	-0.344^{***}		-0.591^{***}	-0.339^{***}	-0.219
	(0.122)		(0.0956)	(0.121)	(0.167)
Patent rights	-0.0673	0.0250			
	(0.0514)	(0.0334)			
Fully owned	-0.203^{***}		-0.209^{***}	-0.216^{***}	-0.281^{***}
	(0.0602)		(0.0603)	(0.0566)	(0.0946)
Partially owned	0.0203	0.0804	0.0227		
	(0.0561)	(0.0535)	(0.0564)		
Services	-0.220^{***}	-0.234^{***}	-0.220^{***}	-0.222^{***}	-0.387
	(0.0766)	(0.0771)	(0.0772)	(0.0765)	(0.350)
Pseudo R^2	0.39	0.36	0.38	0.40	0.46
Observations	1308	1308	1311	1311	1311
Studies	55	55	55	55	55

Note: Standard errors in parentheses. Response variable: t-statistic of the estimate of semi-elasticity.
All explanatory variables are divided by the standard error of the estimate of semi-elasticity.
OLS = ordinary least squares with clustered standard errors. The intercept, precision, and variables controlling for methodology, data, and quality are included in all specifications (these results are reported in Table A3).
*** denotes significance at the 1% level.

magnitude of backward spillovers. The distance between the host and source country of FDI has a robustly positive and significant effect, which suggests that investors from far-off countries create more beneficial linkages. We thus corroborate the findings of Javorcik *et al.* (2004), who report that American and Asian investors in Romania generate greater spillovers than European investors. Furthermore, our results indicate that a high technology gap between foreign affiliates and domestic firms impedes technology transfer. Since, however, a very low or even negative technology gap may leave little room for technology transfer, we also test for a possible quadratic relationship between spillovers and the technology gap. Contrary to the recent meta-analysis on horizontal spillovers by Meyer & Sinani (2009), who use host-country-level data for GDP as a proxy of the technology gap and do not account for the difference between the host and source country, the quadratic term is insignificant and the linear specification fits the data better.

We find that firms in countries open to international trade benefit more from FDI, which corresponds to Meyer & Sinani (2009). Thus both horizontal and vertical spillovers seem to be especially important for firms with international experience. On the other hand, the financial development of the host country has a negative effect on spillovers, which supports the view that foreign affiliates help domestic firms ease credit constraints. Indeed, according to the survey evidence reported by Javorcik & Spatareanu (2009) for the Czech Republic, a quarter of suppliers of foreign affiliates claimed that the supplier status helped them to gain more financing.

The results suggest that the degree of protection of intellectual property rights is insignificant for the magnitude of spillovers. Better patent rights can attract more investors using advanced technology, but they also increase the costs of imitating foreign technology, and thus shrink the benefits. On the other hand, the degree of foreign ownership of investment projects is important. The dummy variable for investments with full foreign ownership is consistently negative and significant, suggesting that projects with full foreign ownership generate lower spillovers than projects with partial ownership (according to the specific model the semi-elasticity is lower by about 0.22). The coefficient for the variable capturing partial ownership is positive but insignificant; the insignificance is, however, largely due to the connection with the variable capturing full foreign ownership. When we drop the variable for full foreign ownership from the regression (Column 2 of Table 5) the p-value corresponding to the variable for partial ownership decreases to 0.13. These findings are consistent with the negative effect of the technology gap on spillovers: fully owned foreign affiliates are likely to use more advanced technology, which increases the technology gap. Likewise, the smaller effect on domestic firms in service sectors is consistent with the importance of international experience for the adoption of spillovers.

Seventeen variables reflecting the characteristics of the data, specification, estimation, and quality are significant, suggesting that results depend on study design in a systematic way. The results are affected by the level of aggregation, age, and source of the data. The omission of the standard control variables (sector competition, downstream demand), the definition of the response variable, and the method of computing TFP matter. Furthermore, we find an upward trend in the results: other things equal, the use of new data increases the reported semi-elasticity by 0.03 each year. Concerning quality characteristics, unpublished studies report estimates that are systematically lower by 0.28 compared with published studies. Studies with no co-author native to the investigated country report estimates lower by a remarkable 0.46.

The results of the multivariate meta-regression can be used to estimate the underlying semi-elasticity conditional on study design. Since the majority of researchers consider some aspects of study design misspecifications, we plug the preferred values of method dummies into the specific model. This approach is called the "best-practice" estimation. The best practice, however, is subjective as different researchers may prefer different methodologies. For simplicity, we define the best practice following Javorcik (2004a), the study published in the American Economic Review: Javorcik (2004a) uses firm-level data, computes TFP by a method that accounts for the endogeneity of input demand, estimates the regression in differences, and control for sector fixed effects, sector competition, and demand in downstream sectors.

Furthermore, we extend the definition of the best practice to represent the "ideal" study. We prefer studies published in refereed journals and studies with a co-author native to the investigated country. We plug in the sample maximums for study citations, author citations, and average year of the data. Other variables, including all structural variables, are set to their sample means. In other words the best-practice estimate is conditional on some characteristics of data, methodology, and quality, but unconditional on the characteristics of host countries and FDI. The best-practice estimate of the underlying semi-elasticity, e_0, reaches 1.07 and is significant at the 1% level with the 95% confidence interval (0.79, 1.35). The whole procedure yields similar results when outliers are included (1.12) or when OLS is used (1.06).

Therefore, taking into account publication bias and observable differences in data, methods, and quality, our preferred estimate implies that a 10-percentage-point increase in foreign presence is associated with an increase in the productivity of domestic firms in supplier sectors of almost 11%: a large, economically important effect. The estimate further increases to 1.24 if we plug in the sample maximum of publication date. The use of output instead of TFP as the response variable (e.g., Blalock & Gertler, 2008) lowers the estimate from 1.07 to a still highly significant 0.72. When all variables reflecting quality characteristics are set to their sample means, the best-practice estimate declines from 1.07 to 0.73. When additionally average data characteristics are considered, the estimate further diminishes to 0.62. Finally, when average methods are also plugged in, the estimate shrinks to 0.02 and loses significance at conventional levels. A mirror image of the best practice estimation (the only exception is that firm-level data are still considered) even gives a significantly negative estimate, −0.74.

Our analysis thus suggests that negative estimates are largely due to misspecifications. Indeed, the best-practice estimates are positive and significant for all countries in the sample even if we consider the effect of fully owned foreign affiliates on domestic firms in service sectors. The average estimate published in high-quality journals, compared with the average estimate in lesser journals or in working papers, is closer to our definition of best practice in all aspects of methods and data, which indicates that some of the journal preference for positive results is caused by the selection of higher-quality studies. It does not explain, though, the asymmetry of reported results.

A similar multivariate analysis, available on request, shows that no country-specific variable matters for the degree of forward spillovers, and that the best-practice estimate of forward spillovers is insignificant. These findings corroborate the view that backward linkages are more important than forward linkages.

Table 6: Backward spillovers and differences between the host and source country

	Source country of FDI					
Host country of FDI	United States		Germany		South Korea	
Mexico	0.921	(0.144)	1.559	(0.172)	2.122	(0.254)
Romania	1.320	(0.162)	1.015	(0.154)	1.812	(0.208)
China	0.928	(0.205)	1.009	(0.185)	0.819	(0.171)

Note: Standard errors in parentheses.
Estimates are based on best practice and are all significant at the 1% level.

To illustrate the economic significance of the effects of distance and the technology gap on spillovers, consider the example of three source countries of FDI (the United States, Germany, and South Korea) and three host countries (Mexico, Romania, and China) reported in Table 6. The estimates are based on best practice and show that the same investment has different effects in different host countries. In Mexico the greatest spillovers are generated by Korean FDI followed by German FDI; investments from the nearby USA generate the least spillovers.

Since Mexico has a similar technology gap with respect to the USA and Germany, the difference between the estimated spillover effects, 0.64, is largely due to different distances. Likewise, the distance from Mexico to Germany is similar to the distance from Mexico to Korea, and the difference in spillovers, 0.57, is due to different technology gaps. When both effects are put together, one dollar of FDI from Korea creates more than twice as many benefits for domestic Mexican firms than one dollar of FDI from the USA. A similar interplay of distance and the technology gap can be observed for Romania and China. It follows that, under realistic conditions, the origin of FDI is economically important for the effect on domestic firms.

5 Conclusion

In a meta-analysis of data from 47 countries, we find robust evidence consistent with technology transfer from foreign investors to domestic firms in supplier sectors (backward spillovers), but no economically important effect on firms in customer sectors (forward spillovers) or in the same sector (horizontal spillovers). Similar to Görg & Strobl (2001), we detect publication bias in the literature: positive or significant estimates are more likely to be selected for publication, especially in high-ranked journals. This upward bias is present only among the estimates of backward spillovers from journal articles; unpublished studies and estimates of forward and horizontal spillovers exhibit no selection. On the other hand, misspecifications tend to bias the results downwards.

The analysis brings three policy-relevant results. First, our preferred estimate suggests that a 10-percentage-point increase in foreign presence is associated with an increase in the productivity of domestic firms in supplier sectors of 11%. Such a strong spillover is consistent with subsidies for FDI. For example, if Haskel *et al.* (2007) used this estimate to calculate the per-job value of spillovers, the result would exceed the per-job value of recent subsidies. 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. An exception is Blalock & Gertler (2008), who additionally examine the influence of foreign presence on domestic profits and confirm the positive externality.

Second, greater spillovers are generated by FDI from distant countries with slight technological advantages over domestic firms. The results are in line with the theoretical model of Rodriguez-Clare (1996) and, in the case of distance, corroborate the findings of Javorcik *et al.* (2004). When investors come from distant countries, it is more expensive for them to import intermediate inputs from home; when the technology gap is not too large, local suppliers are able to produce the inputs of sufficient quality. In both cases, investors are likely to create more linkages with domestic firms. It follows that subsidy programs, if in operation, are best targeted at such investors.

Third, greater spillovers are received by countries that are open to international trade and that have underdeveloped financial systems. As for openness, firms used to trading with foreign firms will create linkages with investors more easily; the result corresponds with the findings of Meyer & Sinani (2009) for horizontal spillovers. As for financial development, if foreign presence helps domestic firms alleviate their credit constraints (Harrison *et al.*, 2004), a less developed financial system implies a higher potential to benefit from FDI. In addition, fewer spillovers are generated by fully owned foreign affiliates compared with joint ventures, and fewer spillovers are received by domestic firms in services compared with manufacturing.

Meta-analysis can only filter out misspecifications that have been overcome by a sufficient number of researchers. If a misspecification is shared by the entire literature and influences the estimates in a systematic way, meta-analysis will give biased results. This problem is important for the point estimate of the spillover effect while less so for the investigation of spillover determinants. Several researchers have emphasized that the traditional definition of linkage variables in spillover regressions is valid only under specific conditions. Concerning backward spillovers, Barrios *et al.* (2009) construct alternative measure of linkages using, for example, input-output tables for investors' home countries to account for different sourcing behavior. Vacek (2007) constructs firm-level linkage variables that reflect the actual ratio of the output of domestic firms sold to foreign affiliates. Concerning horizontal spillovers, Keller & Yeaple (2009) use an instrumental-variable estimator and take into account that foreign affiliates are active in more than one sector. All of these studies find that using the new measures results in stronger evidence of positive spillovers. These improvements, however, have so far been sparsely applied, and their examination in a meta-regression analysis is left for further research.

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Appendix A: Supplementary Material

Table A1: Meta-analyses for individual studies

	Backward		Forward		Horizontal		
Study	Effect	SE	Effect	SE	Effect	SE	N
Articles published in refereed journals							
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 & Gertler (2008)	0.087***	0.009			-0.009	0.007	10
Blalock & Simon (2009)	0.02	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 & Wakelin (2007)	0.280***	0.025	0.280***	0.025	0.099***	0.022	45
Girma & 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 & Muraközy (2007)	1.464***	0.131	-0.411	0.747	-0.223***	0.053	58
Jabbour & 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 & 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 & 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 & Sica (2007)	0.073***	0.023			0.079	0.085	6
Resmini & Nicolini (2007)	0.032***	0.005	0.027***	0.005			22
Sasidharan & Ramanathan (2007)	-0.044	0.338			0.050	0.125	6
Wang & 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 & Kumar (2006)	0.138**	0.057	0.285***	0.060	0.025***	0.006	32
Book chapters, working papers, and dissertations							
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)	0.01	0.027			0.030***	0.011	104
Fernandes & 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 & Pomfret (2008)	1.062***	0.140			-0.825***	0.152	39
Lesher & 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 & Schoors (2005)	-0.690***	0.167	2.293***	0.457	-0.073	0.166	45

Continued on next page

Table A1: Meta-analyses for individual studies (continued)

Study	Backward		Forward		Horizontal		
	Effect	SE	Effect	SE	Effect	SE	N
Merlevede & Schoors (2007)	0.097	0.170	0.476***	0.160	-0.044	0.046	60
Merlevede & 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 & 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 & Yłlmaz (2008)	0.035**	0.015	0.064**	0.029	0.106**	0.052	53
Tong & Hu (2007)	0.228	0.415	0.228	0.415	-0.185	0.325	8
Vacek (2007b)	0.048	0.060	-0.003	0.038	0.013	0.012	48
Vacek (2007a)	0.526***	0.044	-0.001	0.014			92

Note: 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%, 5%, and 10% levels.

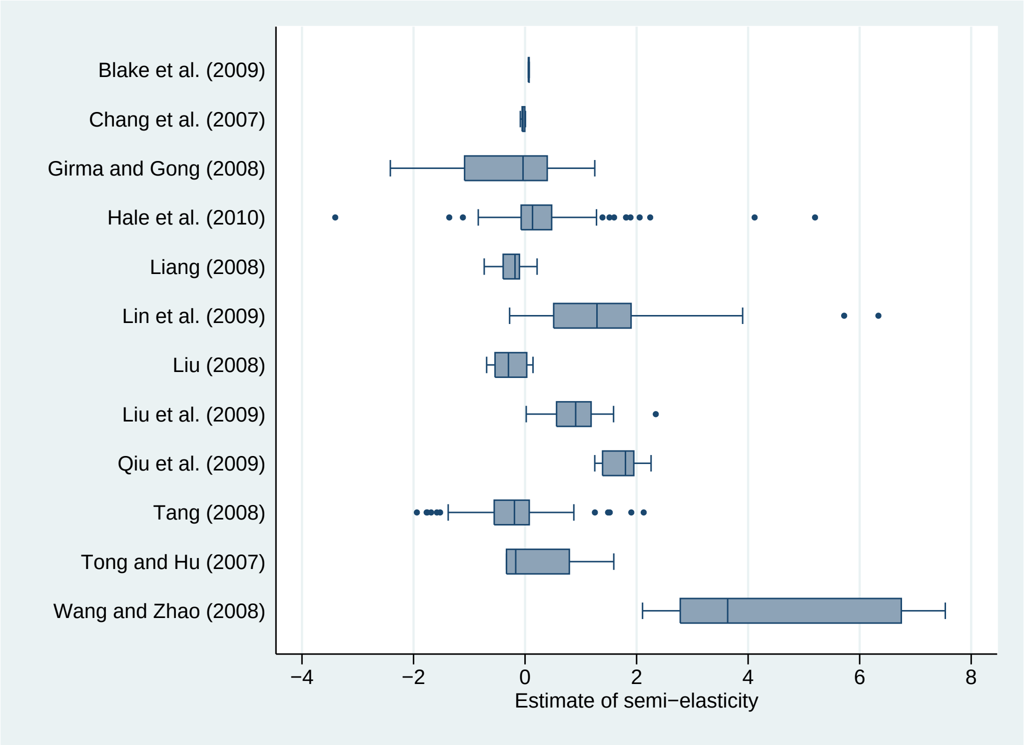

Figure A1: Box plot of backward spillovers in China shows method heterogeneity

Table A2: Meta-analyses for individual countries

	Backward		Forward		Horizontal		
Country	Effect	SE	Effect	SE	Effect	SE	N
Advanced OECD countries[a]	-0.341***	0.102	-0.125	0.142	-0.047**	0.023	172
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	0.44***	0.023	-0.004	0.006	1001
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	-0.15**	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	-0.93***	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	0.060	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
Transition countries[b]	0.085***	0.008	0.035***	0.007	0.02***	0.003	231
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
Viet Nam	0.079	0.049	-3.059***	0.281	-0.038	0.040	243
Zambia	1.108	0.734			-0.188***	0.067	22

Note: 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. Outlying observations are included.

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

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

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

[b] Albania, Georgia, Kazakhstan, Serbia.

Table A3: Control variables of the multivariate meta-regression

	Mixed-effects multilevel				OLS
	Full	Subset 1	Subset 2	Specific	Specific
Intercept	0.397	0.242	0.339	0.385	0.670**
	(0.375)	(0.396)	(0.378)	(0.371)	(0.298)
$1/Se$	2.785*	-2.890***	4.250***	1.293	1.554
	(1.643)	(0.523)	(0.952)	(1.190)	(1.563)
Data characteristics					
Aggregated	1.206***	1.213***	1.224***	1.193***	1.187***
	(0.145)	(0.140)	(0.145)	(0.144)	(0.190)
Average year	0.0349***	0.0236***	0.0277***	0.0323***	0.0301***
	(0.00789)	(0.00719)	(0.00754)	(0.00763)	(0.00837)
Amadeus	-0.686***	-0.489***	-0.861***	-0.680***	-0.603***
	(0.0950)	(0.0855)	(0.0874)	(0.0946)	(0.127)
Specification characteristics					
Employment	-0.168*	-0.149*	-0.131	-0.158*	-0.323*
	(0.0929)	(0.0825)	(0.0930)	(0.0921)	(0.171)
Competition	-0.315***	-0.353***	-0.368***	-0.333***	-0.306***
	(0.0673)	(0.0664)	(0.0655)	(0.0649)	(0.106)
Demand	0.567***	0.487***	0.581***	0.596***	0.615***
	(0.0995)	(0.0985)	(0.0944)	(0.0967)	(0.192)
Estimation characteristics					
One step	-0.348***	-0.302***	-0.304***	-0.353***	-0.447***
	(0.0783)	(0.0788)	(0.0779)	(0.0780)	(0.137)
Olley-Pakes	-0.318***	-0.305***	-0.324***	-0.346***	-0.464***
	(0.0824)	(0.0827)	(0.0802)	(0.0794)	(0.154)
OLS	-0.388***	-0.349***	-0.354***	-0.400***	-0.587***
	(0.102)	(0.102)	(0.102)	(0.101)	(0.173)
Pooled OLS	0.155***	0.174***	0.150***	0.155***	0.221***
	(0.0430)	(0.0430)	(0.0433)	(0.0430)	(0.0429)
Sector fixed	0.119***	0.140***	0.135***	0.128***	0.117*
	(0.0401)	(0.0380)	(0.0393)	(0.0393)	(0.0617)
Differences	0.107*	0.0415	0.0211	0.0989*	0.0583
	(0.0578)	(0.0568)	(0.0543)	(0.0569)	(0.0674)
Publication characteristics					
Published	0.276***	0.273***	0.274***	0.283***	0.407***
	(0.0786)	(0.0798)	(0.0777)	(0.0782)	(0.0958)
Study citations	0.0799**	0.0878***	0.108***	0.0820**	0.0421
	(0.0324)	(0.0323)	(0.0320)	(0.0322)	(0.0281)
Native	0.449***	0.466***	0.389***	0.461***	0.449***
	(0.0626)	(0.0634)	(0.0562)	(0.0617)	(0.0522)
Author citations	-0.0682***	-0.0574***	-0.0752***	-0.0739***	-0.0266
	(0.0190)	(0.0152)	(0.0185)	(0.0184)	(0.0214)
Publication date	0.0669**	0.0476**	0.105***	0.0756***	0.0503
	(0.0270)	(0.0239)	(0.0252)	(0.0261)	(0.0351)
Pseudo R^2	0.39	0.36	0.38	0.40	0.46
Observations	1308	1308	1311	1311	1311
Studies	55	55	55	55	55

Note: Standard errors in parentheses. Response variable: t-statistic of the estimate of semi-elasticity.

All explanatory variables are divided by the standard error of the estimate of semi-elasticity.

OLS = ordinary least squares with clustered standard errors. Variables capturing structural heterogeneity are included in all specifications (these results are reported in Table 5).

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

Appendix B: Studies Used in the Meta-Analysis

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