Comment

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A huge volume of research on the impact of foreign direct investment (FDI) on host countries now exists. The issue is of first-order policy importance. As sources of new technology and foreign capital, multinational firms have the potential to be major contributors to economic development in poor countries. In policy prescriptions for economic liberalization emanating from Washington, the recommendation to remove barriers to FDI is standard. However, the enthusiasm for FDI goes beyond the desire to liberalize capital markets. In many quarters, multinationals are seen as a catalyst for industrialization and so deserve special treatment through subsidies or tax breaks. Many countries now offer tax inducements to multinationals.

In their helpful review of the literature, Lipsey and Sjöholm begin with the observation that the enthusiasm for FDI among policymakers is puzzling. While the theoretical case for opening to FDI (and for liberalizing capital markets in general) is solid, the case for subsidizing FDI depends on strong assumptions about the existence of positive spillovers associated with multinationals. As Lipsey and Sjöholm point out, empirical support for positive FDI spillovers is mixed, at best. The empirical results in the literature are either hard to interpret (in the case of wage spillovers) or all over the map (in the case of productivity spillovers).

It is useful to consider what would be convincing empirical evidence of positive FDI spillovers. Take the case of productivity spillovers, in which the arrival of multinationals raises the total factor productivity (TFP) of domestic firms (either in the same industry, as with horizontal spillovers, or in upstream or downstream industries, as with vertical spillovers). Let

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y_{it} be TFP in domestic firm i at time t, let x_{it} be a set of exogenous control variables for firm i, and let FDI_{it} be the multinational presence to which firm i is exposed. A positive correlation between y_{it} and FDI_{it} in the cross section—as many studies find for productivity and for wages—is not very informative. It does not reveal whether FDI raises productivity or whether multinationals are attracted to regions or industries in which domestic firms are more productive or workers are more skilled.

To remove the effects of unobserved (time-invariant) industry and region factors that affect firm productivity, a more appealing approach is to examine the correlation between the change in firm productivity and the change in FDI. The standard regression in the literature is similar to

$$\Delta y_{it} = \Delta x_{it}\beta + \Delta FDI_{it}\theta + \varepsilon_{it}$$

A positive estimate for θ is commonly interpreted as evidence of positive FDI spillovers. Since Aitken and Harrison (1999) estimated that θ was negative for their case study of Venezuela, a flood of studies attempting to overturn the result has occurred. Lipsey and Sjöholm report that the results are distressingly inconclusive. Estimates of θ vary widely, ranging from positive to negative and even zero depending on the country.

Why are the results on domestic productivity and FDI not robust? One explanation is that the standard regression equation is not identified. In the above regression, to estimate θ consistently requires that changes in FDI are exogenous to unobserved shocks to firm i's productivity. This is a tall order. There are myriad shocks to an industry or region that would both raise wages or productivity in domestic firms and attract multinationals. To identify θ, we need an alternative estimation strategy. One approach is to search for an instrument for FDI. But what factors are plausibly correlated with the attractiveness of an industry or region to FDI and uncorrelated with domestic firm productivity? An alternative approach is to search for a natural experiment. That is, to identify a control group that sweeps out the effects of unobserved shocks.

The Javorcik and Spatareanu, Blalock and Gertler, and Erdilek chapters all deal with this identification issue in a different way. Erdilek's main contribution is that it brings very disaggregated data to bear on the research and development (R&D) activities of multinational and domestic firms (in this case, in Turkey). Much previous literature fails to specify the precise channel through which multinationals affect domestic firms. Helpfully, Erdilek focuses on the single channel of R&D. He finds, not surprisingly, that multinationals engage in more R&D than do domestic firms (though there are differences between minority- and majority-owned multinationals). Erdilek's main finding is that national firms are more likely to engage in R&D when there are more multinational firms in their sector.

Is this evidence of FDI spillovers? I would suggest that the answer is "no." The random-effects estimator that Erdilek uses consistently estimates the

impact of FDI on domestic R&D activity under a strong assumption: that the only source of unobserved persistence in the plant decision to undertake R&D is time-invariant features of plants that are orthogonal to observed plant characteristics and to industry FDI. This is a stronger assumption than is required in the fixed-effects estimator discussed above. By my reading, consistency in Erdilek's case would require that R&D have no fixed costs (and so no dynamic interdependence in R&D decisions). Erdilek presents some very intriguing correlations between R&D in foreign and domestic firms. It appears that foreign firms are more likely to undertake R&D, but on the basis of these results we cannot say how FDI affects domestic firms or how policymakers should treat FDI.

The Javorcik and Spatareanu study takes a quite different approach. They use survey data drawn from managers' responses to determine how multinationals have affected domestic firms (in this case, in Latvia and the Czech Republic). Javorcik and Spatareanu then examine whether firm productivity or firm employment growth is higher or lower in firms that have been more exposed to multinationals. The results are mixed. Productivity is higher in firms that learned new technologies from multinationals but it is also higher in firms that said they lost employees to multinationals. Domestic firms that are exposed to multinationals look different from other domestic firms, but we are unsure why.

What is intriguing about this study is the potential for using survey responses to look for evidence of FDI spillovers. The usefulness of survey responses hinges critically on how managers interpret questions. Consider two questions that ask about the relation between industry characteristics and foreign presence. The first question is, "Did your firm become more productive after the arrival of foreign firms?" By asking about the change in industry conditions associated with the arrival of foreign firms, the question implicitly controls for industry-fixed effects. But the question is not posed in a way that allows causal inference. We do not know whether foreign presence changed industry productivity or vice versa. The second question is, "Did the growth rate of productivity in your firm increase after foreign firms arrived in your industry?" By asking about the rate of change, the question is posed as a difference in difference. Still, it does not permit causal inference because we do not know why foreign presence increased. But if we could conduct surveys before and after events thought to trigger FDI, then we might be able to choose episodes in which the change in FDI appears driven by factors that are orthogonal to domestic firm productivity (e.g., changes in tax policy in source countries for FDI). Since manager surveys are conducted on a small scale, it might be feasible to time them to take advantage of external events that are likely to change FDI.

The Blalock and Gertler chapter is the most serious of the four about addressing identification issues associated with FDI spillovers. This study (1) identifies an explicit control group in running regressions, (2) looks for indirect effects of FDI that would be inconsistent with an endogeneity story,

and (3) uses natural experiments to identify the effect of foreign ownership on firms. In the first part of the analysis, they ask whether Indonesian plants had higher TFP growth in regional industries with faster growth in downstream demand by multinationals. By controlling for region-year effects, the implicit control group is the average plant in the region. However, this specification does not account for why FDI increased, forcing us to assume that it is exogenous.

To gauge the plausibility of the exogeneity assumption, Blalock and Gertler question whether industries with faster downstream growth by multinationals experienced a larger fall in regional industry concentration or industry prices. The answer is "yes" on both counts. The opposite movements would be predicted by one source of endogeneity in FDI, in which regional demand shocks affected both firm productivity and multinational location decisions. This sort of external validation of FDI spillovers is all too rare in the literature. It is useful not just as a check on the assumption about the exogeneity of FDI but also to verify other FDI effects, which theory predicts.

In their final exercise, Blalock and Gertler ask whether, relative to domestic plants, foreign plants had higher growth after the Asian financial crisis than before. This approach makes clever use of Indonesia's financial crisis as a natural experiment to determine whether access to foreign capital affects firm performance. It does. This use of exogenous variation in the importance of FDI is also all too rare in the literature.

As international economists, what can we tell policymakers in developing countries about how they should treat multinational firms? Based on empirical work to date, the answer, unfortunately, is "not much." The literature is just beginning to seriously consider empirical issues about FDI's effect on domestic firms. Thus, the Blalock and Gertler study is a welcome addition to the literature, but it is atypical in the attention it pays to the econometric identification of FDI spillovers. Given the developing state of the field, it is important that policymakers realize that we do not know how multinational firms affect their economies. There are good reasons to liberalize capital markets, but an abundance of evidence that FDI generates positive spillovers does not exist. So far, researchers have yet to uncover robust empirical support for the kinds of subsidies that many countries have begun to offer multinational enterprises.

Reference

Aitken, Brian J., and Ann E. Harrison. 1999. Do Domestic Firms Benefit from Direct Foreign Investment? Evidence from Venezuela. *American Economic Review* 89, no. 3: 605–18.