

# DOES INWARD FOREIGN DIRECT INVESTMENT BOOST THE PRODUCTIVITY OF DOMESTIC FIRMS?

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*Abstract*—Are there productivity spillovers from FDI to domestic firms, and, if so, how much should host countries be willing to pay to attract FDI? To examine these questions, we use a plant-level panel covering U.K. manufacturing from 1973 through 1992. Consistent with spillovers, we estimate a robust and significantly positive correlation between a domestic plant's TFP and the foreign-affiliate share of activity in that plant's industry. Typical estimates suggest that a 10-percentage-point increase in foreign presence in a U.K. industry raises the TFP of that industry's domestic plants by about 0.5%. We also use these estimates to calculate the per-job value of these spillovers at about £2,400 in 2000 prices (\$4,300). These calculated values appear to be less than per-job incentives governments have granted in recent high-profile cases, in some cases several times less.

## I. Introduction

AN important part of globalization in recent years has been the ongoing rise in foreign direct investment (FDI). UNCTAD (2000) reports that from 1979 to 1999, the ratio of world FDI stock to world gross domestic product rose from 5% to 16% and the ratio of world FDI inflows to global gross domestic capital formation rose from 2% to 14%. One consequence is that an increasing share of countries' output is accounted for by foreign affiliates of multinational firms. The foreign-affiliate share of world production is now 15% in manufacturing and other tradables (Lipsey, Blomstrom, & Ramstetter, 1998).

An obvious policy issue for governments is whether incentives should be offered to multinational firms to induce local-affiliate production. In recent decades dozens of countries have altered laws to at least grant multinationals national treatment, if not to favor these firms via policies such as subsidies and tax breaks (UNCTAD, 2000).<sup>1</sup> Policy promotion of FDI is now common not just in developing

countries but in many developed countries as well. The exact values of FDI incentive packages are typically hard to know, but the values of many well-known FDI packages appear very high. In the late 1980s, the U.S. state of Kentucky offered Toyota an incentive package worth (in present value) \$125–\$147 million for a plant planning to employ 3,000 workers (Black and Hoyt, 1989). In 1994 the state of Alabama offered Mercedes an incentive package of approximately \$230 million for a new plant planning to employ 1,500 workers (Head, 1998). In 1991 Motorola was paid £50.75 million to locate a mobile-phone plant in Scotland, employing 3,000 workers. The factory closed in 2001, and Motorola paid back £16.75 million in grants. Siemens was paid £50 million in 1996 to locate a 1,000-worker semiconductor plant in Tyneside, in northeast England. The factory closed eighteen months later, at which point Siemens had to repay £18 million in grants.

Economic justification for this policy would arise if the social returns to FDI exceed the private returns; for example, if inward FDI raises the productivity of domestic plants by bringing new knowledge into the host country that is, at least partly, a public good. This positive externality of knowledge spillovers may arise along industry and/or regional lines.

There are thus two empirical questions that we seek to shed light on in this paper. First, are there productivity spillovers from FDI to domestic firms? Second, if so, how much should host countries be willing to pay to attract FDI? Despite the public interest and policy importance of these two questions, there is very little empirical evidence offering answers.

We examine whether the productivity of domestic plants (or firms) is correlated with FDI presence in the industry and/or region of the domestic plants.<sup>2</sup> Of the few other micro-level studies preceding ours, only one finds any evidence of positive spillovers. Haddad and Harrison (1993) find increased industry-level FDI is correlated with *lower* domestic-plant productivity in Moroccan manufacturing plants. Aitken and Harrison (1999) find the same negative result for Venezuelan manufacturing. They suggest these

ventures. See Hanson (2001) for an overview of issues involved in FDI policy.

<sup>2</sup> Other studies are case studies and industry studies. As for cases, Moran (2001) for example finds positive evidence of spillovers to host countries from FDI in electronics, machinery, and transportation industries. However, case studies do not always offer quantitative information and do not easily generalize. Industry-level studies (for example, Caves, 1974; Blomstrom, 1986; and Driffield, 2000) have documented a positive industry-level correlation between FDI inflows and productivity. This could be due to spillovers. But it may also be batting-average effects if inward FDI forces the exit of low-productivity domestic plants or raises the market share of the more productive foreign firms. Or it may be that multinationals tend to concentrate in high-productivity industries.

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<sup>1</sup> For example, as Aitken and Harrison (1999) document, before 1989 foreign firms in Venezuela were taxed at a higher rate than domestic firms (50% versus 35%), were forced to repatriate profits at officially fixed exchange rates, and could not enjoy confidentiality privileges in joint

negative spillovers reflect adverse effects of FDI due to competition, and further that FDI spillovers might not be positive in developing countries whose firms do not have the absorptive capacity. Chung Mitchell, and Yeung (1998) find that Japanese automobile firms operating in the United States did not boost the productivity of their American component-supplier firms via technology spillovers. Girma and Wakelin (2001) look at one industry, U.K. electronics, and find a positive correlation between domestic-firm productivity and regional Japanese FDI.<sup>3</sup>

To bring some fresh evidence to bear on this issue, we use a plant-level panel for all U.K. manufacturing from 1973 through 1992, where each plant reports information on nationality of ownership. Our main innovation is that we are, to the best of our knowledge, the first paper to study FDI spillovers using plant-level data for the whole of manufacturing for a developed country. The United Kingdom is of interest for a number of reasons. First, by virtue of being a high-income country that is among the top five R&D producers in the world (Keller, 2001), there is *ex ante* reason to suppose that it has sufficient absorptive capacity to realize FDI spillovers. Second, in recent decades the United Kingdom has seen substantial inflows of FDI. In our panel the foreign-affiliate share of manufacturing employment has risen from 12% in 1973 to 23% in 1992. Third, in recent years the U.K. government has spent hundreds of millions of pounds in incentives for foreign firms both to locate in the U.K. and to expand existing U.K. production. With estimates of spillovers, we can undertake simple calculations to evaluate these actual government outlays.<sup>4</sup>

Our general approach will be to regress domestic-plant-level output on domestic-plant-level inputs, measures of foreign presence in the plant's industry and region, and other control regressors. We interpret coefficient estimates on our FDI regressors as evidence consistent with spillovers from inward FDI to domestic-plant total factor productivity (TFP). We look at the robustness of our results to endogeneity, measurement, selection, and absorptive capacity.

Our main finding is evidence consistent with FDI spillovers along industry lines. Across a wide range of specifications, on our full sample we estimate a significantly positive correlation between a domestic plant's TFP and the

foreign-affiliate share of activity in that plant's industry. Typical estimates suggest that a 10-percentage-point increase in foreign presence in a U.K. industry raises the TFP of that industry's domestic plants by about 0.5%. We find no significant correlation between plant TFP and FDI presence by region.

We then use our typical estimates of FDI spillovers to calculate the amount by which an additional foreign job in a U.K. industry boosts the output of domestic plants in that industry. This amount is about £2,440 per year at 2000 prices. We then compare these spillover benefits with the per-job incentives governments have granted in several recent high-profile cases. The spillover magnitudes appear to be less than actual per-job incentives, in some cases several times less. This suggests that productivity spillovers alone might not justify some of the recent high-profile policy initiatives.

In the rest of the paper, section II briefly discusses the theory of productivity spillovers. Section III discusses our data, measurement, and estimation issues. Section IV presents our empirical findings, and Section V discusses their public-finance implications. Section VI concludes.<sup>5</sup>

## II. Multinationals and Theories of Productivity Spillovers

Many standard models of multinational firms assume they possess knowledge assets (such as patents, proprietary technology, and trademarks) that can be deployed in plants outside the parent country (for example, see Carr, MarKusen, & Maskus, 2001; and Dunning, 1981). If multinationals transfer knowledge from parents to their foreign affiliates, then it is possible that some of this knowledge "spills over" to domestic firms in the host country through nonmarket transactions. The general idea that interaction among firms can generate spillovers dates back to at least Marshall (1920), and Mansfield and Romeo (1980) presented survey evidence in which U.S. multinationals reported the frequency and pace at which their technology deployed in foreign affiliates reached host country competitors.

Theoretical work on the mechanics of spillovers suggests they fall along industry or regional lines. A formal industry example is Rodriguez-Clare (1996), in which affiliates increase a host country's access to specialized varieties of intermediate inputs, the improved knowledge of which raises the TFP of domestic producers. Less formally, it is suggested that domestic firms learn from affiliates in the same industry via a range of informal contacts (for example,

<sup>3</sup> Using data not on firms or plants but rather on patent citations, Branstetter (2000) looks for spillovers of Japanese FDI into the United States. Subsequent to our work on this paper, Harris and Robinson (2002) look for spillovers in a collection of twenty detailed U.K. industries. See footnote 18 for a comparison of our methods with theirs. Other work subsequent to ours includes Keller and Yeaple's (2003) U.S. analysis, Javorcik's (2004) analysis of Eastern Europe, and Blalock and Gertler's (2005) analysis of Indonesia.

<sup>4</sup> The appendix describes how the U.K. government subsidizes inward FDI. In general, the government offers incentives to many types of foreign-affiliate activity, where employment protection/expansion is a prominent criterion. Between 1985 and 1988, 58% of Regional Selective Assistance (RSA, the major source of U.K. government support for firms) went to plant expansions and 25% to new plants, and foreign firms received 60% of the value of RSA (PA Consultants, 1993, tables 2.3 and 11.1, respectively).

<sup>5</sup> Beyond knowledge spillovers, foreign presence may raise aggregate U.K. productivity by inducing exit of domestic firms and/or by exerting competitive pressure on domestic firms. Our focus on knowledge spillovers is for *surviving* domestic plants, but we consider foreign presence when addressing selection issues. We also try to control for competitive pressures. Relatedly, our analysis is only for domestic plants and does not address the relative performance of foreign and domestic plants (for example, Griffith, 1999; Oulton, 2000; Harris, 2001).

trade shows; supplier/distributor discussions; exposure to affiliate products, marketing, and patents; technical support from affiliates; reverse engineering). Regional spillovers might operate via labor turnover. For example, Song, Almeida, and Wu (2001) use U.S. patent records to trace the movement of scientists between domestic and foreign firms (also see Motta, Fofur, & Ronde, 1999; and Moen, 2000).

Overall, then, there is reason to suppose that inward FDI in the same region or industry may boost the productivity of domestic plants. Accordingly, we plan to investigate both empirically by looking for a correlation between domestic-plant productivity and industry and regional measures of foreign-plant presence. Such a correlation we will interpret as evidence consistent with the presence of productivity spillovers.<sup>6</sup>

### III. Data, Measurement, and Econometrics

#### A. Overview of the ARD Data Set

Details of our data can be found in Griffith (1999), Oulton (1997), Disney, Haskel, and Heden (2000), and the data appendix. Here we briefly set out the main features of the data, and concentrate on issues involved in calculating productivity and foreign presence.

Our main data set is the ARD (Annual Census of Production Respondents Database), which is the official U.K. business-level data collected by the U.K. Office of National Statistics (ONS). To build these data, the ONS maintains a register of businesses designed to capture the universe of production-sector activity (Perry, 1985). This register is the basis upon which the census forms are sent out to businesses, response to which is mandatory under the 1947 Statistics of Trade Act. These forms request extensive operational information on inputs and outputs that we use to estimate (total factor) productivity. Crucially for our purposes, the ONS also collects information on the business's industry, region, and nationality of ownership. It also maintains data on the structure of the business, for example, plants under common ownership. Each plant is assigned a unique identification number and also a number corresponding to the firm that owns them (so plants under common ownership share a common firm identifier).

In at least two ways, the U.K. government has reduced the reporting burden on firms. First, all plants with employment over some minimum size (100 in most years) are sampled, but plants with employment below this threshold are sampled with probabilities decreasing in size. The sampled plants are referred to as the "selected sample," while all

nonsampled plants constitute the "nonselected sample." Our analysis will use primarily the selected sample, which each year accounts for around 90% of total U.K. manufacturing employment (Oulton, 1997).<sup>7</sup>

A second reporting-burden issue is that multiplant firms have some latitude in the level of aggregation at which they report plant information. If a multiplant firm considers some of its individual plants to be too small to complete a full census form, it may report an amalgamation of plants. This reporting level is called an "establishment."

Computerized ARD records go back to 1972; paper records for earlier years have been destroyed. In 1993 and 1994, a complete recoding of identification numbers was undertaken that has generated nontrivial problems in matching plants before and after. Thus, our data run through 1992, a period that fortunately covered a substantial increase in FDI inflows.

The ARD structure raises many issues for our data analysis. One is the level of aggregation at which to investigate productivity spillovers. In principle, the ARD panel can be configured for plants, establishments, or firms. However, at the level of firms, spillovers might be obscured for multiplant firms in multiple regions and/or industries. And since multiplant firms that aggregate operations into establishments do not report data for each separate plant, at the level of plants we cannot measure TFP for all observations. Accordingly, we choose to work at the level of establishments, which is the most disaggregated level at which we can measure TFP. For brevity, we will use the terms "establishments" and "plants" interchangeably. But since ARD establishments can consist of more than one plant, we check the robustness of our results to this.<sup>8</sup>

#### B. Specification, Measurement, and Estimation Issues

*Specification* To investigate whether inward FDI generates productivity spillovers for domestic plants, we estimate variations of the following basic production function:

<sup>7</sup> Within the selected sample, in most years 50% of plants with employment from 50 to 100 are sampled, and 25% of plants with employment from 20 to 50. The very smallest plants each year are excluded from the census. An important research issue is what information, if any, can be used from the nonselected data. Since these businesses are not sent a full census form, we have no information on their inputs (such as material and investment). They do report on nationality of ownership. The ONS imputes their employment levels using turnover data from tax records. The ONS does check employment for plants with imputed employment of over 11. However, due to time lags in the provision of tax data and processing of imputations, such information typically refers to data from two years earlier (Perry, 1985). In addition, these checked plants are only around 20% of the nonselected sample. In sum, we cannot use the nonselected data for productivity calculations. But we could potentially use the employment data to measure foreign presence and/or to weight the selected sample. Our results are robust to alternative treatments of both these issues, as exemplified by column 3 of table 4.

<sup>8</sup> We cleaned the data via extensive checks for nonsense observations, outliers, coding mistakes, and the like. We dropped publicly owned plants (mainly in utilities), and plants that seemed to change ownership, industry, or region in unusual fashion. Our regressions drop plants in the top and bottom percentiles of changes in all plant-specific output and input variables.

<sup>6</sup> If multinational firms are aware of their ability to generate spillovers, then their operational decisions may be endogenous to this possibility—for example, they may attempt to minimize spillovers' benefits to competitors. Evidence consistent with this appears in Mansfield and Romeo (1980), where the age of technology transferred to affiliates varies with mode of foreign entry, and in Shaver and Flyer (2000), where larger foreign firms are found to be less likely to build U.S. plants near other competitors. See our discussion below for our treatment of endogeneity.

$$\ln Y_{it}^d = \alpha \ln INPUT_{it}^d + \sum_{k=0}^T \gamma_1^k FOR_{R,t-k} + \sum_{k=0}^T \gamma_2^k FOR_{I,t-k} + \delta Z_{it}^d + \varepsilon_{it}. \quad (1)$$

In equation (1), subscripts  $i$ ,  $t$ ,  $k$ ,  $R$ , and  $I$  denote plant, time, lag length, region, and industry;  $\alpha$ ,  $\gamma$ , and  $\delta$  are parameters to be estimated; and the superscript  $d$  denotes that plants are domestically owned. Output of domestic plants is denoted  $Y^d$ , their inputs denoted  $INPUT^d$ , and foreign presence in the region and industry  $FOR_R$  and  $FOR_I$ .  $Z^d$  are other control regressors, and  $\varepsilon$  is an unobserved influence on domestic plant productivity. An alternative strategy to equation (1) would be to calculate TFP and then regress calculated TFP on the noninput regressors in (1); as we report below, our robustness checks are robust to this alternative.

As in all micro-level empirical work with production functions, we face important concerns involving measurement and also estimation. We discuss each of these issues in turn, with additional measurement discussion in the data appendix.<sup>9</sup>

*Measurement* Output is gross output. For  $INPUT$  we use capital,  $K$ ; production and nonproduction labor,  $L^U$  and  $L^S$  (for unskilled and skilled); materials,  $M$ ; and hours,  $h$ .  $L^U$ ,  $L^S$ , and  $M$  are available directly from the *census* full-form surveys.  $L^U$  and  $L^S$  count employment of both part-time and full-time workers, and  $M$  measures the value of both energy and nonenergy materials purchases. Hours are available only at the two-digit industry level. Output and materials are deflated using industry-level price indexes as detailed as possible.<sup>10</sup> The ARD does not ask plants to report capital stocks, so we used plant investment data to calculate capital stocks. We chose industry-level starting capital stock values and depreciation rates for buildings, plant and machinery, and vehicles taken from O'Mahony and Oulton (1990). We deflated each component of investment by ONS industry-year investment deflators. We experimented with different capital stock computations (the two main variables affecting the capital stock path are starting values and depreciation rates), but these did not overly affect the results.

The  $FOR_R$  and  $FOR_I$  terms in equation (1) are foreign presence by region and by industry. Nationality of plant ownership is defined according to whether an overseas investor has an effective voice in the management of the enterprise, where an effective voice is taken as equivalent to

a holding of 20% or more in the foreign enterprise. In our data, then, foreign-affiliate plants are those plants owned at least 20% by an overseas business interest. Note that beyond this 20% cutoff, the ARD does not measure the degree of foreign ownership. Also note that domestic plants mix both U.K.-headquartered multinational firms and purely domestic U.K. plants, as the ARD does not provide any ownership distinction among domestically owned plants. Despite these caveats, one important advantage of the ARD over similar data sets for most other countries is that it reports nationality of ownership in every year.<sup>11</sup>

Given this information on nationality of ownership, we measure  $FOR_R$  as the share of total employment in region  $R$  accounted for by foreign-owned plants.  $FOR_I$  is constructed analogously, as the share of total employment in industry  $I$  accounted for by foreign-owned plants. There are several points to make regarding measurement of these important variables.

First, these shares capture the idea that what matters for spillovers is how prevalent foreigners are in the domestic region or industry, scaling for the overall size of that industry or region. Other micro-level spillover studies have used share measures of foreign presence.<sup>12</sup> To help evaluate the robustness of our results, below we also estimate specifications that enter total foreign employment and total employment separately.

Second, to construct the shares we prefer employment as the activity measure because many spillover theories (section II) involve interpersonal contacts. One obvious alternative is to use a particular skill group. More-skilled non-production workers might embody most of the spillovers, for example, due to their greater knowledge of technology innovations. Or production workers might be those most familiar with specific production techniques (such as leaner assembly-line operations) that boost productivity. Below, we report results for these alternatives.

Third, as indicated in equation (1) we allow these foreign-presence measures to enter both contemporaneously and with lags. This is because although theory suggests that FDI spillovers may take time to arise (for example, labor turnover to domestic plants), there is not sharp empirical evidence on this issue as to exactly how long. Our specifica-

<sup>9</sup> See Bartelsman and Doms (2000) for a detailed discussion of data issues specific to micro-level data sets.

<sup>10</sup> Our lack of plant-level prices is a pervasive problem in the literature on micro panels. To preview our interest in the correlation between foreign presence and productivity, if inward FDI lowers industry prices, then there may be a spurious correlation between foreign presence and our measure of plant productivity. Without plant-level prices, we cannot assess the importance of this effect. But if it were important, then all plant-level studies should automatically find this correlation.

<sup>11</sup> In contrast, the widely used analogous U.S. database, the Longitudinal Research Database, does not track nationality of ownership. The only year in which nationality information was merged in (from the U.S. Bureau of Economic Analysis) was 1987 (see examination of this one year in Doms & Jensen, 1998).

<sup>12</sup> Different papers have used slightly different specifications of foreign presence, though. For example, Aitken and Harrison (1999) use  $FOR_I$  and also the interaction of foreign ownership in the same industry and region. One advantage of separating our foreign-presence measures by industry and region is that if spillovers along these different dimensions take different times, then our separated terms can be entered with different lag lengths. We tried various specifications with interacted measures, but these were consistently insignificant.

TABLE 1.—BASIC FACTS OF THE ARD PANEL

Year	No. of Plants (1)	No. of British Plants (2)	No. of Foreign Plants (3)	Percent Employment in Foreign Plants (4)
1973	21,413	20,418	995	0.12
1974	23,486	22,333	1,153	0.13
1975	21,798	20,665	1,133	0.13
1976	21,820	20,582	1,238	0.14
1977	21,860	20,363	1,497	0.16
1978	18,823	17,426	1,397	0.15
1979	17,965	16,441	1,524	0.16
1980	14,901	13,432	1,469	0.17
1981	14,717	13,155	1,562	0.18
1982	14,468	12,920	1,548	0.18
1983	14,046	12,493	1,553	0.17
1984	18,352	16,793	1,559	0.17
1985	13,783	12,416	1,367	0.17
1986	13,192	11,927	1,265	0.16
1987	13,316	12,026	1,290	0.16
1988	13,460	12,161	1,299	0.16
1989	18,982	17,370	1,612	0.18
1990	14,036	12,544	1,492	0.20
1991	13,926	12,319	1,607	0.22
1992	13,449	11,826	1,623	0.23

Note: In each year, a foreign-owned plant is defined as one in which a foreign business entity has at least a 20% ownership stake. All plants not meeting this criterion are defined as British owned. The employment shares in the final column report the share of overall U.K. manufacturing employment accounted for by foreign-owned plants. The sample of plants used for each year is the entire ARD (Annual Census of Production Respondents Database) selected sample, unweighted. See text for details on ownership and sampling issues.

tions will try many lag structures, which may help address concerns discussed below such as endogeneity.<sup>13</sup>

Fourth, theory offers no sharp prediction as to how narrowly or broadly regions and industries should be measured. We distinguish eleven different U.K. regions. These are commonly used regions originally identified in the U.K. censuses of population, and they fall across conventional political and other boundaries. For  $FOR_t$  we distinguish 22 different manufacturing industries; these are roughly comparable to two-digit Standard Industrial Classification industries for U.S. manufacturing. There was a major revision to the U.K. industry classifications in 1980. These reclassifications make it difficult to separate industries in greater detail with confidence, so to minimize potential measurement error our baseline is to use the 22 two-digit industries. For the post-1980 subsample we report results at the two-, three-, and four-digit levels. This practical issue aside, there may be reason to think industry-mediated spillovers are not “too narrow.” For example, inventory-management techniques in apparel production might apply to a wide range of apparel goods—men’s, women’s, and children’s. Or, as discussed in section II, spillovers may arise from supplier and/or customer interactions—for example, windshield producers learning from automobile firms.

Table 1 reports some basic ownership information in our ARD panel. As column 1 shows, we have usable data on 13,000–23,000 plants per year. Columns 2 and 3 show the bulk of those are British owned, but column 4 shows that the fraction of manufacturing employment accounted for by

<sup>13</sup> In Mansfield and Romeo’s (1980) surveys, U.S. multinationals report that their technology deployed in foreign affiliates reached host country competitors in anywhere from zero to over 6.5 years, with a modal response of 0.5 to 1.5 years and a mean response of about four years.

TABLE 2A.—SHARE OF FOREIGN EMPLOYMENT, BY REGION

Region	1977	1992
Southeast	0.26	0.31
East Anglia	0.23	0.27
Southwest	0.12	0.18
West Midlands	0.08	0.22
East Midlands	0.08	0.14
Yorkshire/Humberside	0.11	0.16
Northwest	0.12	0.20
North	0.11	0.23
Wales	0.18	0.33
Scotland	0.19	0.29
N. Ireland	0.22	0.27

Note: Each cell reports the share of that region-year’s total manufacturing employment accounted for by foreign-owned plants. The sample of plants used for each year is the entire ARD selected sample, unweighted. See text for details on sampling issues.

foreign affiliates grew from 12% in 1973 to 23% in 1992. The general decline in the number of British plants in table 1 is consistent with the general decline during our sample period in overall U.K. manufacturing activity.<sup>14</sup> Note that given how we construct  $FOR_R$  and  $FOR_t$ , this decline will tend to increase our foreign-presence measures even if there is no change in FDI activity. To control for this, we will estimate specifications that add to equation (1) the lagged number of British plants by region and industry. Entering separately the numerators and denominators of  $FOR_R$  and  $FOR_t$  will also control for this.

Tables 2A and 2B show the regional and industrial variation, respectively, in foreign-employment shares for 1977 and 1992. By region, foreign presence was highest in

<sup>14</sup> Office of National Statistics (1998) reports that total U.K. manufacturing employment fell from 6.446 million in 1980 to 4.084 million in 1992. There is a spike in the number of plants in 1984 and 1989 because the Central Statistical Office changed the compilation method of the register (see Disney et al., 2000).

TABLE 2B.—SHARE OF FOREIGN EMPLOYMENT, BY INDUSTRY

Two-Digit Industry	1977	1992
22 Metal manufacturing	0.05	0.19
23 Extraction of minerals not elsewhere specified	0.02	0.00
24 Manufacture of nonmetallic mineral products	0.11	0.13
25 Chemical industry	0.29	0.38
26 Production of man-made fibers	0.16	0.20
31 Manufacture of metal goods not elsewhere specified	0.10	0.19
32 Mechanical engineering	0.18	0.28
33 Manuf. of office machinery and data processing equipment	0.41	0.68
34 Electrical and electronic engineering	0.22	0.31
35 Manufacture of motor vehicles and parts thereof	0.34	0.48
36 Manufacture of other transport equipment	0.02	0.11
37 Instrumental engineering	0.40	0.29
41 Food and drink manufacturing industries <sup>1</sup>	0.12	0.09
42 Food, drink, and tobacco manufacturing industries <sup>2</sup>	0.11	0.25
43 Textile industry	0.04	0.08
44 Manufacture of leather and leather goods	0.04	0.00
45 Footwear and clothing industries	0.04	0.06
46 Timber and wooden furniture industries	0.03	0.06
47 Manuf. of paper and paper products; printing and publishing	0.16	0.22
48 Processing of rubber and plastics	0.23	0.28
49 Other manufacturing industries	0.14	0.13

Note: Each cell reports the share of that industry-year's employment accounted for by foreign-owned plants. The sample of plants used for each year is the entire ARD selected sample, unweighted. See text for details on sampling issues. Industries are by the U.K. Standard Industrial Classification.

<sup>1</sup>Oils, margarine, milk products; freezing, processing, and preserving of meat, fish, fruit, and vegetables; grain milling, bread, and flour confectionery.

<sup>2</sup>Sugar and sugar confectionery, cocoa, coffee, tea, animal feeds, and pet foods, and all others.

the southeast in the 1970s, but by 1992 Wales was the highest at one in three jobs. By industry, foreign presence was generally highest in office machinery, motor vehicles, and chemicals. But the ranking of foreign presence in regions and industries is not fixed, and the panel nature of our data allows us to exploit this variation.

Turning to the control regressors  $Z$  in equation (1), one important set of controls is for product market competition. This is important to control for since a large literature suggests competition affects the productive efficiency of firms (see, for example, Nickell, 1996). It seems reasonable that the entry of foreign firms might raise the degree of competition and hence the effort level that domestic firms must exert to remain viable. This pro-competitive effect might be regarded as a spillover effect, but the welfare consequences of this are different from the knowledge spillovers that theory tends to focus on (Vickers, 1995). Knowledge spillovers are Pareto-improving positive externalities, whereas increased effort represents a welfare transfer away from the harder-working employees to shareholders and/or customers. Hours is our only possible effort measure thus far, so without direct controls for competition the coefficient on  $FOR_i$  might reflect both knowledge spillovers and the effects of competition. Indeed, Aitken and Harrison (1999) ascribe their finding of negative spillovers to competition: foreign entrants take domestic firms' market shares, and thereby force domestic incumbents up their average-cost curves. All this suggests the need to control for product-market competition.<sup>15</sup>

<sup>15</sup> Note that including inputs in equation (1) should help control for the output consequences of plants moving along their average-cost curves. Also, it seems unlikely that manufacturing plants compete along regional

Following Nickell (1996), we use four potential measures of product-market competition: industry concentration ( $CONC_{it}$ ), import penetration ( $IMPORT_{it}$ ), market share ( $MSHARE_{it}$ ), and rents ( $RENTS_{it}$ ).  $IMPORT$  is available at the industry level as imports as a share of domestic production.  $MSHARE$  is measured as plant output as a proportion of four-digit-industry output.<sup>16</sup> This is unlikely to be a reliable cross-section measure of market power, since it is affected by technological differences between industries (such as capital intensity) which also likely affect productivity. Accordingly, we use changes in market share,  $\Delta MSHARE$ , to measure changes in competitive pressure.  $RENTS$  aims to capture ex ante rents potentially available to workers and managers to take as increased leisure. It is defined as sales less material, capital, and labor costs, expressed as a proportion of net output (where we measure labor cost using industry-region average wages instead of actual plant wages).

*Estimation Issue: Omitted Variables* One important estimation issue is omission from equation (1) of unobserved variables. There are likely to be a host of plant-, industry-, time-, and region-specific influences that are unobservable to the econometrician but are known to the plant. These unobservables might underlie any observed correlation be-

lines. This suggests that the coefficients on  $FOR_R$  are unlikely to reflect increased effort.

<sup>16</sup> We also calculated market shares for three- and two-digit industries. Coefficient standard errors rose as we did this, suggesting that the measure becomes increasingly inaccurate as we use a broader base, which is plausible.

tween productivity and foreign presence. For example, sound infrastructure or high-quality management might both raise domestic productivity and attract foreign firms.

We attempt to address this omitted variables problem via time differencing and fixed effects. First, we estimate equation (1) on time-differenced data. In addition to removing any fixed plant-specific unobservable variation, differencing also removes fixed regional and industrial effects such as indicators of global engagement (for example, tariffs), infrastructure, and technological opportunity. One well-known cost of differencing is that it can aggravate measurement error in the regressors, and thereby introduce biases. Longer time differences tend to attenuate this problem (Griliches & Hausman, 1986), so we report results for one-year, three-year, and five-year differences. Longer time differences may also be more appropriate if spillovers take time to materialize.

Second, in our differenced specifications we also include full sets of time, industry, and region fixed effects. Thus, our findings rely not on differences in plant productivity and differences in foreign presence but on the deviation of differences in plant productivity and foreign presence from their year, region, and industry means.

If our differencing and fixed effects are sufficient, then in equation (1) the error term  $\varepsilon$  is left uncontaminated by omitted variables. This will not be the case, however, if there are important unobservables that vary both across plants and over time. For example, managerial talent may not be fixed over time within plants. Without measures of these plant-and-time-varying factors, estimates from equation (1) may still be biased. Olley and Pakes (1996) show that these remaining unobservable shocks can be proxied from investment behavior, on the assumption that these shocks influence current investment but, since investment takes time, not current output. Olley and Pakes (1996) implement their method on telecommunication plants, as does Pavcnik (2002) on Chilean manufacturing plants.

As Griliches and Mairesse (1995) discuss, however, this structural approach depends on a number of assumptions: for example, plants cannot undertake zero investment, other factors besides capital fully adjust to shocks each period, and markets are perfectly competitive. The sensitivity of this approach to violations of assumptions is an ongoing research question. For example, Levinsohn and Petrin (2003) propose using intermediate inputs rather than investment to address the underlying omitted variables problem. For our purposes, we prefer not to assume perfect competition in light of the emphasis in the micro-spillovers literature on the competitive effects of foreign entrants.<sup>17</sup>

In light of these omitted variables issues, we estimate versions of this differenced equation.

<sup>17</sup> Girma and Wakelin (2001) analyze productivity spillovers using both a specification similar to ours and the Olley-Pakes specification, and find that both approaches yield qualitatively identical results about spillovers.

$$\begin{aligned} \Delta \ln Y_{it}^d = & \alpha_1 \Delta \ln K_{it}^d + \alpha_2 \Delta \ln M_{it}^d + \alpha_3 \Delta \ln S_{it}^d + \alpha_4 \Delta \ln U_{it}^d \\ & + \alpha_1 \Delta \ln h_{it}^d + \sum_{k=0}^T \gamma_1^k \Delta FOR_{R,t-k} + \sum_{k=0}^T \gamma_2^k \Delta FOR_{I,t-k} \\ & + \delta_1 \Delta MSHARE_{it-2}^d + \delta_2 \Delta RENTS_{it-2}^d \\ & + \delta_3 RENTS_{it-2}^d + \lambda_t + \lambda_I + \lambda_R + v_{it} \end{aligned} \quad (2)$$

Equation (2) includes our variables for inputs, foreign presence, competition, and time, regional, and industry dummies ( $\lambda_t$ ,  $\lambda_R$ , and  $\lambda_I$ ). We tried all the competition variables discussed above in both levels and changes, but only those shown in equation (2) had any coefficients significantly different from zero in our main set of regressions. As we discuss next, versions of equation (2) are estimated using OLS and also IV, weighting all plants equally.<sup>18</sup>

*Estimation Issue: Endogeneity* Endogeneity is a particular concern for our key regressors of interest,  $FOR_R$  and  $FOR_I$ . For OLS to yield consistent coefficient estimates, it must be the case that the variation in these regressors is exogenous to the productivity of domestic plants. Given the time differencing and inclusion of fixed effects, this means that  $\Delta FOR_R$  and  $\Delta FOR_I$  must be uncorrelated with shocks to changes in domestic-plant productivity,  $\Delta v_{it}$ . These assumptions would hold both if cross-industry and/or cross-region variation in changes in foreign presence were driven by changes in the barriers facing multinationals (for example, political and natural trade costs) that drove differential cross-industry and/or cross-region FDI responses and if changes in such barriers were uncorrelated with changes in domestic plant productivity.

However, changes in  $FOR_R$  and  $FOR_I$  might not be exogenous to shocks to U.K. plant productivity. Foreign firms may be attracted to regions and/or industries with high-productivity growth, as might be the case if learning spillovers flow in both directions. Alternatively, foreign firms may be attracted to slow-growing regions and/or industries to gain a greater competitive advantage. These alternatives suggest that endogeneity of foreign presence could bias upward or downward OLS coefficient estimates on foreign presence in equation (2).

We pursue three strategies for addressing possible endogeneity bias. First, we use lagged measures of  $FOR_R$  and  $FOR_I$ . Above, we argued that lags may be appropriate

<sup>18</sup> Since we have the selected and nonselected data, we can construct sampling weights and run weighted regressions on the selected sample. However, there are at least two reasons why weighted regressions might be misleading. One is that the true marginal effects may differ across size groups. A second issue is that if the sampling weights are measured with error, then weighted least squares can yield biased coefficient estimates. This is a real concern, both because the precise details of the sampling rules used by the ONS every year are no longer on record and because employment in the nonselected data from which weights can be approximated is in most cases imputed. For more on weighting, see the discussion of table 4. Harris and Robinson (2002) include in their data plants from the nonselected sample, and for these plants all output and inputs data are imputed based on the nonselected employment information.

because spillovers take time to materialize. In addition, if lagged foreign presence is predetermined relative to current plant productivities, then though foreign presence may be contemporaneously correlated with domestic productivity shocks, it is uncorrelated with future productivity shocks.<sup>19</sup>

Our second endogeneity strategy is to replace changes in  $FOR_R$  and  $FOR_I$  with their initial levels. For OLS to yield consistent coefficient estimates here requires these initial levels of foreign presence (as opposed to changes in foreign presence) to be uncorrelated with plant-specific productivity shocks. This may well be a weaker identification assumption, especially for specifications with longer time differences. It may also better reflect the motivating theoretical framework of spillovers, to the extent that growth in domestic productivity reflects the initial presence of foreign firms.

Our third endogeneity strategy is to instrument for lagged changes in  $FOR_I$  using data on inward FDI into the United States. To the extent that cross-industry variation in U.K. inward FDI is driven by worldwide changes in barriers faced by multinationals, then this variation is likely to be similarly reflected in the cross-industry variation in U.S. inward FDI. That is, a global liberalization in some industry that boosts U.K. inward FDI in that industry is likely to also boost U.S. inward FDI in that same industry. This instrumenting strategy maintains the key assumption that these underlying liberalizations do not directly impact the productivity of U.K. domestic firms. This would assume, for example, that the liberalizations are not driven by technology innovations that are sufficiently global in scope to influence these domestic firms.<sup>20</sup>

*Estimation Issue: Measurement Error* A third estimation issue is measurement error, alluded to above in the context of time differencing the data. For our main regres-

<sup>19</sup> Because equation (2) is first-differenced,  $v_{it}$  is MA(1) and so the relation must hold for all  $t < s + 1$ . We also suspect that the competition regressors may be endogenous: for example, higher plant efficiency might raise rents and market share. We therefore lag  $RENTS$  and  $\Delta MSHARE$  by two years.

<sup>20</sup> We constructed our instrument, *U.S. FOR<sub>I</sub>*, parallel to  $FOR_I$  from 1982 forward, which is the first year from which the U.S. Bureau of Economic Analysis (BEA) publicly released annual data on the activity of inward-FDI firms. For the numerator and denominator of this instrument we used sales, rather than employment as in our benchmark measure of  $FOR_I$ . This is because the publicly available data on U.S. employment of foreign-owned firms is much more censored than the analogous sales data, with a substantial fraction of values each year given in broad ranges rather than actual values. The U.S. data was concorded to U.K. industries based on a comparison of detailed descriptions of the two countries' respective SIC systems. In principle, we would have liked to measure inward FDI into the U.S. net of U.K. firms. But we did not do this because in practice, there is such widespread suppression of the publicly available data by industry for U.K. firms only that we would have lost the large majority of our potential firm-year observations. If U.K. FDI were concentrated in particular sectors, then we could exclude those sectors from our instrumental variables specifications. Within manufacturing, the BEA data show no obvious cross-industry concentration of U.K. FDI. U.K. investments do appear to be heavily concentrated in finance and insurance, but these sectors lie outside of our data. As an additional check, we also instrument  $FOR_I$  using lagged values of  $FOR_I$ .

sors of interest,  $FOR_R$  and  $FOR_I$ , one potential source of measurement error may be that the underlying ARD survey tags a plant as foreign owned without any additional information beyond the 20% criterion. The question then becomes the degree to which our counting all employees as foreign employees in these foreign-owned plants may introduce substantial error in measuring the theoretical idea of foreign presence.

Conceptually, it is not clear what degree of foreign ownership is required for the potential of knowledge spillovers to domestic firms. This potential almost surely rises with the degree of foreign ownership, but probably not linearly. For example, firms that are majority owned by foreigners very likely have key managerial decisions—in particular, those regarding knowledge dissemination and use—guided by these foreign owners regardless of the degree of ownership stake. For such firms, it is not clear that we would want our measure of foreign presence to prorate employment based on ownership shares were such shares available.

Empirically, in recent decades the share of minority-owned affiliates in total affiliate activity for U.S.-headquartered multinationals has been steadily declining from an already small fraction. Evidence on European-headquartered multinationals looks broadly similar.<sup>21</sup> Taking these two facts together suggests that most ARD plants tagged as receiving FDI are majority owned or even wholly owned—in which case, per our above conceptual point, our measures of FDI presence may be relatively error free. What measurement error there is is likely to bias toward zero our estimates of spillovers from foreign presence, away from our direction of interest. It is also something that our instrumenting strategy can address.

*Estimation Issue: Selection Bias* A fourth estimation issue is selection bias. Plants can choose to exit each period, but our data contain only the surviving plants. This might bias our estimates for foreign presence. Suppose that foreign presence truly does boost domestic-plant productivity, and thereby domestic-plant survival chances. In regions and/or industries with low foreign presence, we will observe only those plants whose unobservable offsetting benefits—for

<sup>21</sup> Desai, Foley, and Hines (2002) calculate such shares from 1982 to 1997, and they report as follows: "A comprehensive review of all U.S. overseas affiliate activity from 1982 to 1997 indicates that American multinational firms are decreasingly likely to establish their foreign affiliates as joint ventures. Aggregate activity by joint ventures has fallen considerably over time." Specifically, the fraction of all affiliates that were minority owned fell from 17.9% in 1982 to 10.6% in 1997. At the same time, the analogous fraction for wholly owned affiliates rose from 72.3% to 80.4%. The predominance of wholly owned affiliates was even more pronounced for a group of high-income host countries that included the United Kingdom. For the European evidence, Budd, Konings, and Slaughter (2002) analyze wage determination in a panel of European headquartered multinationals—both parents and affiliates—spanning fourteen different European countries over much of the 1990s. They report that of the nearly 6,000 affiliate-year observations in their panel, only about 10% are minority owned.



TABLE 3.—THE EFFECT OF FOREIGN-AFFILIATE PRESENCE ON PRODUCTIVITY: BASELINE SPECIFICATIONS OF EQUATION (2) WITH ALTERNATIVE TIME DIFFERENCES AND FOREIGN-PRESENCE VARIABLES

Regressor	One-Year Differences	Three-Year Differences	Five-Year Differences	One-Year Differences	One-Year Differences
	(1) OLS	(2) OLS	(3) OLS	(4) IV	(5) IV
$\Delta FOR_{I,t}$	0.049 (3.70)**	0.053 (3.16)**	0.063 (2.88)**		
$\Delta FOR_{R,t}$	0.004 (0.23)	-0.011 (0.45)	-0.018 (0.56)		
Observations	74,615	57,057	35,260		
Adjusted <i>R</i> -squared	0.56	0.70	0.76		
$\Delta FOR_{I,t-2}$	0.055 (3.67)**	0.043 (2.22)*	0.070 (3.03)**	0.158 (2.13)*	0.064 (1.16)
Observations	63,506	44,982	28,905	26,336	55,154
Adjusted <i>R</i> -squared	0.58	0.71	0.76	0.59	
$FOR_{I,t-2}$	0.045 (3.41)**	0.071 (3.09)**	0.064 (2.04)*		
Observations	68,339	50,146	31,752		
Adjusted <i>R</i> -squared	0.57	0.71	0.76		

Note: Robust *t*-statistics in parentheses. \*significant at 5% level; \*\*significant at 1% level. The dependent variable is the difference of the log real output. Other regressors in all specifications are the “basic” controls: the differenced logs of capital, materials, skilled employment, unskilled employment, and hours; year dummies; 20 two-digit industry dummies; 10 region dummies; and competition control variables. For brevity, these coefficient estimates are not reported here. The full set of nondummy coefficient estimates for the upper panel is reported in appendix table A1. See text for details. Columns 1 to 3 report coefficients on the foreign-presence variables shown. Columns 4 and 5 report results using as instruments FDI in U.S. industries and lagged levels, respectively. See text for details.

example, good management—allow them to survive. But in regions and/or industries with high foreign presence, we are much more likely to observe all plants. This suggests that selection bias may understate the true relationship between inward FDI and productivity. Conversely, if firms with lower productivity growth are less likely to survive when foreign presence is high, selection may overestimate the relationship between inward FDI and productivity. Therefore, the direction of the overall potential bias is unknown.

A standard approach to handling the selection issue is to condition equation (1) on an auxiliary equation containing variables that capture the probability of the establishment surviving. Olley and Pakes (1996) attempt to model selection structurally by postulating an explicit model of exit (see also Pavcnik, 2002; and Levinsohn & Petrin, 2003). In Olley and Pakes’s model, exit depends on an unobserved (to the econometrician) shock to productivity. This shock would be entirely captured by the investment (and capital) variables that would affect the entry/exit decision. Current output would not be affected by current investment, since it is assumed that investment takes time to materialize into additional capital.

Griliches and Mairesse (1995) argue that the structural approach followed by Olley and Pakes depends on strong assumptions: the probability of exit depends only on the current realization of productivity shocks, not on its whole history, and the determinants of unobserved shocks (investment in their model) is measured without error. In our case we find it hard to argue that investment could work as an exclusion restriction, since capital stock is itself estimated from plant-level investment. Given this, plus the more general point that research is ongoing as to the best estimator for addressing issues such as selection (and as to how different estimation results actually are across methods—

see Van Biesebroeck, 2004), in what follows we do not implement a structural approach to address selection.

## IV. Estimation Results

### A. Baseline Results

The upper panel of table 3 reports baseline OLS estimates of equation (2) using contemporaneous values of  $FOR_R$  and  $FOR_I$ . Each column reports a different length of time difference, with robust standard errors reported below coefficient estimates. We report coefficients on the spillover variables; see table A1 for all coefficients.

Column 1 shows the simplest specification, namely, current  $FOR_R$  and  $FOR_I$  with one-year differences. Both coefficient estimates are positive, consistent with positive productivity spillovers from foreign plants to domestic plants at both the regional and industry level, but the regional coefficient is very small and insignificantly different from zero. The coefficient on  $FOR_I$  suggests that a rise of 10 percentage points in  $FOR_I$  for some industry, ceteris paribus, would raise output in each domestic plant in that industry by about 0.5%. Because we control for inputs in estimating equation (2), this output increase is a TFP increase.

Columns 2 and 3 repeat this specification but using three-year and five-year time differences. The coefficient estimates on  $FOR_R$  turn negative and remain insignificantly different from zero. In contrast, the coefficient estimates on  $FOR_I$  remain significantly positive. Their magnitudes rise somewhat above 0.05, which is expected either if spillovers take time to materialize (and thus are somewhat missed by the one-year specification) or if measurement error is less severe in longer differences. Taken together, these baseline results in table 3 suggest that industry-mediated productivity spillovers are positive and significant, with a semi-

elasticity of around 0.05 as our central estimate. Our estimates of spillover effects along regional lines are insignificantly different from zero. This regional finding persisted in subsequent specifications, so for brevity subsequent tables report results for foreign presence only by industry.

Since this magnitude of 0.05 is common to several specifications reported below, it is worth putting into context. The observed rise in  $FOR_t$  over the sample period 1973–1992 is about 11 percentage points. By our estimates in table 3, this implies that industry spillovers raised U.K. manufacturing industry TFP by about 0.5%. Since actual TFP in U.K. manufacturing rose by about 10% over the estimation period, our estimates suggest that spillovers explain about 5% of the observed 1973–1992 rise in U.K. manufacturing TFP.<sup>22</sup>

### B. Results Examining Endogeneity Bias

To explore endogeneity bias, the middle panel of table 3 uses two period lags of  $FOR_R$  and  $FOR_t$ . Lags are appropriate if lagged foreign presence is predetermined relative to current plant productivities, and/or if spillovers take time to materialize.<sup>23</sup> As the results show, the lagged rather than contemporaneous measures of foreign presence are significant and similar in magnitude. These are consistent with spillovers taking time.<sup>24</sup>

The rest of table 3 further addresses possible endogeneity, as discussed in section IIIB. First, in the lower panel, columns 1 through 3 we replace lagged changes in with the initial levels of  $FOR_t$  for one-, three-, and five-year time differences. In all three cases the estimates on  $FOR_t$  remain significantly positive, at about or slightly larger than 0.05.

Second, in column 4 we instrument for lagged changes in  $FOR_t$  using data on inward FDI into the United States. The result is positive and significant, but a Hausman test does not reject the null hypothesis of equal IV and OLS coefficient estimates on  $FOR_t$ .<sup>25</sup> Finally, in column 5 we use the

Arellano-Bond GMM estimator. We add to equation (2) a lagged dependent variable and once-lagged input variables, with Arellano-Bond estimation that instruments for this lagged dependent variable using past values and instruments for changes in  $FOR_t$  using past levels. A similar long-run coefficient estimate is obtained, though now with a lower  $t$ -statistic (perhaps due to weak instruments).<sup>26</sup>

### C. Robustness Checks

Table 4 reports results for nine additional robustness checks for various measurement and estimation issues beyond endogeneity concerns. All checks are based on the column 1, table 3 middle-panel specification of a two-year lag on measuring foreign presence and one-year time differences. These robustness checks were estimated on a wide range of specifications, but for brevity for each check we report just one.

The first five columns of table 4 check measurement of our key foreign-presence regressors,  $FOR_R$  and  $FOR_t$ . Column 1 uses just U.S. employment to measure foreign presence. It might be expected that spillovers are more forthcoming from higher-technology firms. During our sample period the United States was, on average, the world technology leader (O'Mahony, 1999)—and also accounted for over 70% of foreign employment. Consistent with this, the coefficient in column 1 is significantly positive.

We obtained similarly robust results in columns 2 to 5, where we measure foreign presence using, respectively: nonproduction employment (where these workers may be more likely to facilitate spillovers); both the selected sample and nonselected sample;  $\Delta FOR_{t, t-2}$  and  $\Delta FOR_{R, t-2}$  disaggregated between their numerators and denominators; and adding regressors controlling both for the number of British plants by industry and for the number by region (with both controls lagged one year).

0.074 ( $t = 3.92$ ). To check whether the instrument is strong, we follow Angrist and Krueger (2001, pp. 79–80) and replace the U.K. regressor  $FOR_t$  with the instrument U.S.  $FOR_t$ . We obtain a  $t$ -statistic of 2.11, suggesting that we do not suffer a weak-instruments problem. We also tested the value of our instrument as suggested in Stock and Staiger (1997) by regressing  $FOR_t$  on U.S.  $FOR_t$  and other exogenous variables. In this regression the coefficient on U.S.  $FOR_t$  is significantly different from zero with an  $F$ -statistic over 100, consistent with the necessary correlation between  $FOR_t$  and U.S.  $FOR_t$ . Lastly, we might ask why the IV coefficient estimate is larger than the OLS estimate. As discussed in section IIIB, if foreign firms increasingly locate in unobservably faster-growing domestic industries, then the OLS estimate would be upward biased and hence exceed the IV one. On the other hand, if foreign firms prefer to invest when domestic plants are weak, so that they face less domestic competition, then the OLS estimate would be downward biased and hence be less than the IV one. The fact that the latter is the case suggests that any net endogeneity bias might be driven by a preference for foreign firms to face weaker rivals. But this suggestion should not be overstated, given the Hausman test does not reject the hypothesis that the IV and OLS coefficients are equal.

<sup>26</sup> The  $p$ -value for the test of no MA(1) error in the residuals was zero, rejecting the null of no autocorrelation, which is to be expected since first differencing should induce MA(1) residual autocorrelation. However, the  $p$ -value for the test of no MA(2) error in the residuals was 0.16, which fails to reject the null of no autocorrelation.

<sup>22</sup> To undertake this calculation, we needed to calculate total manufacturing TFP in a manner consistent with the regression from which we use the coefficients for  $FOR_t$ . We do this by subtracting from the change in log real output the weighted changes in the logs of  $K$ ,  $M$ ,  $S$ ,  $U$ , and  $h_t$  with the weights being the coefficients taken from estimates of equation (2). As an additional initial check on our foreign-presence measures, we reestimated our baseline specifications replacing  $FOR_t$  and  $FOR_R$  with a full set of industry and region dummies. The industry dummies were jointly significant ( $F(19; 82,424) = 8.99$ , Prob.  $> F = 0.0000$ ), while the regional dummies are jointly insignificant ( $F(10; 82,424) = 1.42$ , Prob.  $> F = 0.1659$ ).

<sup>23</sup> We also used five-year lags and current,  $t - 1$ ,  $t - 2$ , and  $t - 3$  together. These results are set out in the working-paper version of this paper, Haskel, Slaughter, and Pereira (2004), and are similar to those reported here. Coefficients on regional presence were insignificant and so are omitted.

<sup>24</sup> Because these lagged estimates are similar to those in table 3, and because of the reasons discussed above, in subsequent tables we mostly use lagged rather than contemporaneous measures of foreign presence.

<sup>25</sup> As discussed in section IIIB, annual data for our instrument U.S.  $FOR_t$  are available from just 1982 forward. The OLS estimate using our U.K. regressor  $FOR_t$  from just 1982 forward for one-year time differences is

TABLE 4.—THE EFFECT OF FOREIGN-AFFILIATE PRESENCE ON PRODUCTIVITY ROBUSTNESS-CHECK SPECIFICATIONS OF EQUATION (2)

Robustness Check	Foreign Presence Uses U.S. Employment (1)	Foreign Presence Uses Skilled Employment (2)	Foreign Presence Uses Nonselected Sample (3)	Foreign Presence Uses Levels, Not Shares (4)	Control for Number of U.K.-Owned Plants (5)
$\Delta FOR_{i,t-2}$	0.047 (2.64)**	0.067 (4.61)**	0.043 (3.72)**	$2.47 \times E-7$ (3.22)**	0.055 (3.66)**
Observations	68,270	63,506	63,506	63,506	63,503
R-squared	0.56	0.57	0.57	0.57	0.57

  

Robustness Check	Vary by Each Sector, All Input Coefficients (6)	Use Constructed TFP as Regressand (7)	Exclude Observations with Multiple Plants (8)	Exclude Observations in Wales (9)
$\Delta FOR_{i,t-2}$	0.057 (3.85)**	0.081 (5.00)**	0.053 (2.89)**	0.050 (3.33)**
Observations	63,506	63,908	42,003	61,366
R-squared	0.58	0.03	0.57	0.57

Note: Robust *t*-statistics in parentheses. \*significant at 5% level; \*\*significant at 1% level. All regressions shown here use one-year time differences with foreign presence lagged two years, analogous to column 1 of table 4. But for column 7, the dependent variable is the difference of the log real output. Other regressors are the differenced logs of capital, materials, skilled employment, unskilled employment, and hours; year dummies; 20 two-digit industry dummies; 10 region dummies; competition control variables; and the similarly lagged changes in  $FOR_{it}$ . For brevity, these coefficient estimates are not reported. Column 1 measures foreign activity using U.S. employment only rather than total employment. Column 2 measures foreign activity using skilled employment rather than total employment. Column 3 measures foreign presence accounting for employment estimates from the nonselected sample. Column 4 measures foreign presence using the absolute number of foreign employees, rather than employment shares. Column 5 adds as control regressors the lagged number of U.K.-owned plants by industry and region. Column 6 estimates a separate set of the five input coefficients for each two-digit industry. Column 7 uses as the regressand TFP calculated by the standard method that assumes perfect competition, and thus omits the input regressors. Column 8 excludes all observations that cover multiple plant locations. Column 9 excludes all observations located in Wales. See text for details.

The second row of table 4 addresses other specification and sampling issues. Column 6 allows the various  $\alpha$  coefficients on inputs in equation (2) to vary across all two-digit industries (by interacting input terms with industry dummies). Column 7 drops the inputs and replaces the regressand with TFP calculated from sample data on inputs, outputs, and cost shares. Both estimates are consistent with statistically significant positive spillovers from foreign industry presence. Column 8 then drops observations with multiple plants; again results are robust. Finally, column 9 reports results for the subsample that excludes all plants located in Wales, which as reported in table 2A was one of the regions with the largest increase in foreign presence during our sample period. Our findings appear robust to these four checks as well.

We also examined the role of absorptive capacity. We split our plants into three groups (0–25th, 25–75th, and above-75th percentiles) based on three alternative performance measures: their industry-year employment, TFP, or skill intensity. Full details are in the working-paper version of this paper, but the main result was no strong evidence of different coefficient estimates on foreign presence across different percentile groupings. This suggests no differences in absorptive capacity of our plants (or that our groupings do not adequately capture capacity differences).

Finally, for the post-1980 subsample we constructed  $FOR_{it}$  more finely at the three- and four-digit levels (see discussion in section IIIB). The coefficients on  $\Delta FOR_{it}$  were 0.074 at the two-digit level ( $t = 3.92$ ); 0.054 at the three-digit level ( $t = 2.04$ ), and 0.034 at the four-digit level ( $t = 1.59$ ). These qualitative differences in results across levels of industrial aggregation could mean that domestic firms learn from foreign-owned customers or suppliers in “nearby” industries (that is, customers or suppliers that are

not in the exact same four-digit industry as the domestic firms but are in the same broader industries).<sup>27</sup>

## V. Public-Finance Implications: How Much Should Governments Pay to Attract FDI?

In the introduction we reported estimated costs of government FDI subsidies for several high-profile cases in the United Kingdom and United States. In this subsection we use our estimates of the FDI spillover benefits to a host country to attempt some calculations to compare these costs and benefits on a present-value, per-worker basis.

The subsidy costs per worker can be calculated from the reports above. However, it is important to note the uncertainty surrounding both these quantities, as the reports are culled mainly from press reports without systematic verification of either values or jobs involved and may be a selected sample of the most expensive programs. For the four cases mentioned in the introduction, the costs per worker (all expressed in 2000 U.K. pounds) are Siemens (U.K.) £35,417; Motorola (U.K.) £14,356; Toyota (Kentucky, U.S.) £39,827; and Mercedes (Alabama, U.S.) £117,178.<sup>28</sup>

<sup>27</sup> As an additional check of our spillovers interpretation, we examined industry-level correlations between foreign presence and industry-structure variables: employment growth, capital investment, price changes, firm entry, and industry concentration. Theory work by Packer and Saggi (2001) and empirical work by Blalock and Gertler (2005) suggests that FDI spillovers should trigger industry-level changes including firm entry, employment, and capital stock growth, and falling concentration. When we aggregated our plant-year panel up to an industry-year panel we found (controlling for year and industry) many of the predicted effects: a positive and significant effect of foreign presence on growth rates of employment, capital stock, and the number of plants, and a negative but insignificant effect on industry concentration.

<sup>28</sup> We converted U.S. dollars to U.K. pounds using market exchange rates, and then converted all values into 2000 prices using the U.K. GDP deflator.

The subsidy benefits per worker arise from the TFP boost enjoyed by domestic plants via the inward FDI. Because our estimates of productivity spillovers are for each year, they accumulate over the duration of foreign presence. This means we need to calculate the per-year output boost for domestic plants per extra foreign job, and then discount these output boosts over the length of that job. Consider a foreign plant coming into a particular industry  $I$ . If this new plant raises our foreign-presence measure  $FOR_I$  by  $\Delta\phi_I$ , then the percentage rise in output in each domestic plant in that industry is equal to  $(\gamma_I)(\Delta\phi_I)$ , where  $(\gamma_I)$  is the spillover coefficient in equation (1). If the initial output across all domestic plants in that industry is given by  $Y_{10}^d$ , then the level rise in domestic output in that industry,  $\Delta Y_I^d$ , per rise in foreign-employment share,  $\Delta\phi_I$ , is given by  $\Delta Y_I^d = (Y_{10}^d)(\gamma_I)(\Delta\phi_I)$ . Using the definitional relation between a rise  $\Delta\phi_I$ , and the rise in foreign employment,  $\Delta N_I^f$ ,  $\Delta\phi_I = (\Delta N_I^f / (N_{10}^f + N_{10}^d))(1 / (1 + N_{10}^f / N_{10}^d + \Delta N_I^f / N_{10}^d))$ , where  $N_{10}^d$ , and  $N_{10}^f$  are the number of domestic and foreign jobs in the industry in the base period, we can write the extra domestic output per foreign job,  $\Delta Y_I^d / \Delta N_I^f$ , as the following:

$$\frac{\Delta Y_I^d}{\Delta N_I^f} = \gamma_I Y_{10}^d \frac{1}{(N_{10}^f + N_{10}^d)} \frac{I}{(1 + N_{10}^f / N_{10}^d + \Delta N_I^f / N_{10}^d)} \quad (3)$$

The extra domestic output per extra foreign job consists of four terms. The first,  $\gamma_I$ , is the estimated coefficient that gives the percentage change in domestic-plant output in response to a rise in foreign-employment share. The second term in equation (3),  $Y_{10}^d$ , converts this percentage change into a level change. The third and fourth terms convert the rise in foreign-employment share to rise in foreign employment in actual levels.

An expression similar to (3) would hold for productivity spillovers along regional lines, and we could therefore calculate the extra domestic output in region per foreign job created in a region. Our estimates of regional productivity spillovers were mostly small and insignificant, however, so we do not attempt any regional calculations.

Using data for the last year of our sample, 1992, we apply equation (3) to calculate the extra domestic output per foreign job. This quantity  $\Delta Y_I^d / \Delta N_I^f$  varies by industry: we estimated  $\gamma_I$  to be the same across industries, but each industry has different values of the other three components of the right-hand side of equation (3). Averaging our calculations across all industries, we obtain an average value of  $\Delta Y_I^d / \Delta N_I^f$  of £2,097 in 1992 prices. This figure says that, ceteris paribus, each new foreign worker stimulates an extra £2,097 in output across all domestic plants in that worker's industry. This amount is about £2,440 at 2000 prices.<sup>29</sup>

We can now compare our calculations of subsidy costs and benefits. To do this, we need to remember that the

subsidy benefits accrue per year, and accordingly measure costs and benefits over the same time spans. For the two U.S. cases, note that we are assuming that our estimates of U.K. productivity spillovers apply in the same way to the United States. We have no way to evaluate this assumption, but maintain it simply for the sake of discussion.

Consider the two U.K. cases. The U.K. Siemens plant stayed open eighteen months. At a discount rate of 5%, £2,440 for eighteen months is £3,430: this is the value of spillover benefits per worker at this plant. The subsidy cost £35,417 per worker, an order of magnitude more than our best guess as to its spillover benefits. The U.K. Motorola plant survived ten years. At a discount rate of 5%, this translates into present-value spillover benefits of £18,841 per worker. The subsidy cost £14,356 per worker, so in this case the government cost of the subsidy was about equal to its estimated productivity benefits.

The two U.S. cases are harder to judge, both because of the spillover caveat mentioned above and because the plants remain open today. The Toyota plant opened in 1988, and so thus far has generated a present-value spillover benefit of £22,920 per worker. The subsidy cost per worker is £39,827 in this case. This amount would be the present value of spillover benefits if the plant operates for 35 years, suggesting the Toyota plant must remain open 22 more years to "break even." The Mercedes plant opened in 1994, with an implied spillover benefit of £14,119 for its seven years of operation. This is an order of magnitude smaller than our calculated subsidy cost per worker of £117,178, which suggests that for this case the subsidy cost will exceed its productivity-spillover benefits.

A number of comments regarding these calculations are worth making. The first and most important is to reiterate that these calculations are only suggestive, as they rely on many assumptions and caveats. In particular, we have not considered benefits to foreign presence beyond the single issue of productivity spillovers. Foreign plants may bring benefits we have not considered (for example, civic benefits of "good citizen" employers). We also have not specified from where new foreign employees come. A new employee at a foreign plant may come from abroad, or from employment in a different domestic plant, or from unemployment. In the last case, the social value of the new foreign job may be higher.<sup>30</sup>

A second comment is to stress the ceteris paribus nature of these calculations. For a foreign plant to continue generating spillovers over time, it needs to maintain its boost to the foreign-affiliate share of its industry employment. It is not length of plant life that is at issue, strictly speaking, but rather the length of increase in foreign-affiliate employment share. These calculations assume no other growth or decline in employment among all other plants. In reality, this may

<sup>29</sup> As a benchmark, in 1992 gross output per domestic worker averaged about £73,000 and domestic wages about £15,000.

<sup>30</sup> Another important assumption is that government payments and other subsidies are permanent (or that, if temporary, these temporary policies have no permanent effects).

not be the case. For example, if over time spillovers stimulate hiring at domestic plants, then a foreign plant's boost to the foreign-affiliate employment share declines over time.

A final consideration is the incidence of subsidy costs and benefits. In the four cases we considered, host country governments directly pay the subsidy costs. But these governments do not directly realize the subsidy benefits. Productivity spillovers accrue to domestic firms, not domestic governments. In principle, subsidies could be paid by coalitions of domestic firms that organize to pool contributions used as incentives to foreign firms. In practice, the standard collective-action problem of free riding may make such coalition-forming difficult.

Governments may be willing to pay subsidy costs based on the tax revenues they gain from the domestic-output boost. But if governments care only about their tax-revenue gain, then the cost they should be willing to incur equals just their share of the output bonus. In 1992 the maximum corporate tax rates were 33% in the United Kingdom and 34% in the United States. This means that spillover benefits accruing to governments are only about one-third the total benefits calculated above, which makes the cost-benefit calculations even more unfavorable. Alternatively, governments might care about more than their tax-revenue gain, and thus may somehow internalize the spillover benefits enjoyed by domestic firms.<sup>31</sup>

## VI. Conclusions

A large number of countries pay subsidies to attract FDI. One justification is that the social returns to FDI exceed the private returns, because of productivity spillovers from FDI to domestic firms. In this paper we examined two issues. First, are there productivity spillovers from FDI to domestic firms? Second, if there are such spillovers, what level of subsidies would be justified? Using a plant-level panel for U.K. manufacturing covering 1973–1992, we estimated production functions for domestic plants augmented with terms measuring foreign presence in the industry and region. Our major findings are as follows.

- (a) We estimate a significantly positive correlation between a domestic plant's TFP and the foreign share of employment in that plant's industry. Typical estimates suggest that a 10-percentage-point increase in foreign presence in a U.K. industry raises the TFP of that industry's domestic plants by about 0.5%. This correlation is consistent with productivity spillovers from inward FDI to domestic plants. We do not find significant effects for foreign share of employment by region. Our estimates seem robust across several issues of endogeneity, measurement, and specification.

- (b) Our estimates suggest that the per-job value of spillovers are less than per-job incentives governments have granted in recent high-profile cases, often several times. In future work we hope to address what the channels of productivity spillovers are—for example, access to suppliers and labor market turnover—using direct spillover measures.

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<sup>31</sup> If part of the subsidy package governments offer is tax breaks, then the relevant effective tax rates are even lower. Courant (1994) surveys how to evaluate tax policy when used to foster economic development.

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## Data Appendix A

### Variable Definitions and Sources

$\Delta \ln Y_t$	The log change in total manufacturing real gross output (£s in 1980) (direct from ARD), deflated by four-digit annual output price deflators supplied by the ONS.
$\Delta \ln K_t$	The log change in total manufacturing real net capital stock (£s in 1980). Capital stock is estimated from establishment-level investment in plant and machinery, vehicles, and buildings, using perpetual inventory methods with the starting values and depreciation rates taken from O'Mahony and Oulton (1990), using the selected sample only. Depreciation rates: buildings, 2.91%; plant and machinery, 11.097%; and vehicles, 28.1%. Buildings and plant and machinery are deflated by two-digit industry deflators, vehicles by annual deflators. Deflators were supplied by Rachel Griffith at the Institute for Fiscal Studies (IFS). In addition, establishments may disappear and appear from the ARD data due to sampling. This clearly creates problems for the perpetual inventory method. If we drop all establishments that disappear and reappear for at least one year,

TABLE A1.—THE EFFECT OF FOREIGN-AFFILIATE PRESENCE ON PRODUCTIVITY  
BASELINE SPECIFICATIONS OF EQUATION (2)

Regressor	One-Year Differences (1)	Three-Year Differences (2)	Five-Year Differences (3)
$\Delta FOR_{I,t}$	0.049 (3.70)**	0.053 (3.16)**	0.063 (2.88)**
$\Delta FOR_{R,t}$	0.004 (0.23)	-0.011 (0.45)	-0.018 (0.56)
$\Delta \ln K_t$	0.105 (19.27)**	0.074 (17.49)**	0.073 (16.47)**
$\Delta \ln S_t$	0.090 (31.64)**	0.102 (37.71)**	0.103 (31.41)**
$\Delta \ln U_t$	0.260 (68.24)**	0.262 (73.60)**	0.253 (62.37)**
$\Delta \ln M_t$	0.468 (162.98)**	0.527 (169.28)**	0.547 (162.98)**
$\Delta \ln h_{it}$	0.204 (2.41)*	0.241 (2.66)*	0.463 (4.06)**
$\Delta MSHARE_{i,t-2}$	-0.088 (1.73)		
$RENTS_{i,t-2}$	-0.037 (22.16)**		
$\Delta RENTS_{i,t-2}$	-0.000 (1.85)		
Observations	74,615	57,057	35,260
Adjusted R-squared	0.56	0.70	0.76

Note: Robust *t*-statistics in parentheses. \*significant at 5% level; \*\*significant at 1% level. The dependent variable is the difference of the log real output. Other regressors are year dummies, 20 two-digit industry dummies, 10 region dummies, and competition control variables. For brevity, these coefficient estimates are not reported here. The lagged competition variables are omitted from the long-difference columns, to minimize the loss of observations.

we lose almost 50% of our selected sample. To fill in the missing year's investment data, we multiplied that year's industry investment by the establishment's average share of industry investment over the establishment's lifetime. After some experimentation, we used this method to interpolate for establishments with at most three year's missing data. This means we lose only 10% of the sample. Although investment is of course volatile, establishments' investment shares by industry are in fact extremely stable, and so we feel the induced inaccuracies are likely to be small relative to very large gain in sample size.

$\Delta \ln L_t$	The log change in total manufacturing employment (direct from ARD).
$\Delta \ln S_t$	The log change in total manufacturing nonmanual employment (direct from ARD).
$\Delta \ln U_t$	The log change in total manufacturing manual employment (direct from ARD).
$\Delta \ln M_t$	The log change in total manufacturing real intermediate inputs (£s in 1980) (direct from ARD), deflated by four-digit input price deflators supplied by the ONS.
$\Delta \ln h_{it}$	The log change in manual hours at the two-digit industry level, from the New Earnings Survey as published in the <i>Department of Employment Gazette</i> .
$\Delta MSHARE_{i,t-2}$	The lagged change in market share, $(t-2) - (t-3)$ . The market share is establishment nominal gross output as a share of four-digit industry nominal gross output.
$RENTS_{i,t-2}$	Rents lagged twice. It is defined as rents over net output, where rents are net output less material, capital, and labor costs, expressed as a proportion of net output. Labor costs are the region- and four-digit industry-specific average manual and nonmanual wage.
$\Delta RENTS_{i,t-2}$	The lagged change in rents, $(t-2) - (t-3)$ .
$\Delta FOR_{I,t}$	The change in employment in a foreign-owned plant as a share of total employment in the

industry. Industry is defined at the two-digit level; there are 22 two-digit industries.

$\Delta FOR_{R,t}$  The change in employment in a foreign-owned plant as a share of total employment in the region. There are eleven standard regions in the United Kingdom.

## Appendix B

### Payments to Foreign Firms Operating in the United Kingdom

The U.K. government supports firms in many ways.<sup>32</sup> EU legislation restricts such support to special cases, such as investment that can be shown to be of social benefit in low-income areas designated by the EU as Assisted Areas. There are thus two main sources of support that are available in these areas.<sup>33</sup>

1. EU money from the European Structural Funds. This money is mostly paid out to large infrastructure projects.
2. Money from the U.K. government. These are discretionary grants made to support both small (i.e., less than £500,000) and large (i.e., above £500,000) private investment projects.

Most funding for foreign investment is for larger projects and comes from Regional Selective Assistance (RSA). The projects must either create new employment or safeguard existing employment in the Assisted Areas. To be eligible for RSA, before investment goes ahead applicants have to disclose the investment size as well as its expected employment creation and duration. Foreign companies are eligible for RSA for greenfield investments as well as expansions or modernizations of existing operations. RSA is available for up to 15% of eligible project costs (mostly the costs of capital investment).

A government official judges whether an RSA-applied investment will create jobs and for how long. It is difficult to assess exactly how this judgement is made. An indication of the process involved is given by the following excerpt from the standard RSA application form; it states that all of the listed criteria must be met for the grant application to be considered.

The project:  
Takes place in an Assisted Area.  
Is aimed at more than a local market.  
Is based on forecast growth in the market sector to ensure that displacement is not an issue.  
Will involve a minimum capital expenditure of £500,000 on fixed assets.  
Will directly create or safeguard job in the business.  
Expects the business as a whole to be financially viable and profitable within three years.  
If the project is undertaken by a member of a group, the group will be financially stable.  
Needs RSA as essential for the project to proceed.

<sup>32</sup> For more information, see <http://www.Invest-in-the-UK.com>. General information about grants is at <http://www.dti.gov.uk/support> and <http://www.invest.uk.com>. See [http://news.bbc.co.uk/1/hi/english/business/the\\_company\\_file/newsid\\_332000/332560.stm](http://news.bbc.co.uk/1/hi/english/business/the_company_file/newsid_332000/332560.stm) for information on the Siemens case and [http://news.bbc.co.uk/1/hi/english/uk/scotland/newsid\\_1294000/1294662.stm](http://news.bbc.co.uk/1/hi/english/uk/scotland/newsid_1294000/1294662.stm) for information on the Motorola case.

<sup>33</sup> Assisted Areas are designated as Tier 1, 2, or 3 depending on their deprivation level. U.K. examples of Tier 1, that is, poorest, areas are the Sheffield and Liverpool areas.

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