9 Do Italian Firms Improve Their Performance at Home by Investing Abroad?

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9.1 Introduction

The effects at home of the transfer of economic activities abroad, either through foreign investment or through arm-length contracts, is a widely debated issue. This chapter contributes to this debate by assessing how the home activities of manufacturing firms in Italy change following the setting up of foreign subsidiaries abroad.

The debate on outsourcing in the United States and in the United Kingdom has mostly focused on services. In contrast, continental European countries are concerned with the loss of manufacturing jobs, particularly in traditional industries. Accordingly, policymakers in several European countries and in the European Union (EU) have responded to public concerns by introducing measures hindering this process.

However, public opinion and policy responses are often formed on a superficial understanding of outsourcing. Indeed, the issue is not simple. Theoretically, the transfer of national resources from home to foreign countries, either developed or developing, taking place through outward investment or arm's-length contracts may be predicted to have both positive and negative consequences on home activities. Empirically this process is hard to gauge, as it needs to be observed at the firm level, and sufficiently detailed firm-level data are rarely available.

This chapter contributes to the assessment of the effects of outsourcing by focusing on foreign direct investment, one of the channels through which firms transfer their activities abroad. It is based on a firm-level data set of Italian firms with foreign investment between 1993 and 1998. It has two key features.

First, it focuses on firms that change status from national to multinational by investing abroad for the first time. By restricting this analysis to firms changing status, we believe we are in a better position to isolate the effect of investing abroad. In fact, while we agree that the effects of the investment by nonswitchers are also important and should be taken into account, our aim here is capturing the effects of discrete changes in investment behavior (new investments rather than ongoing ones) on performance. We believe that focusing on first-time investors is an original way to isolate this factor. Including nonswitchers would make it difficult to isolate the impacts of new investments from those of ongoing ones.

Second, it uses propensity score matching to construct an appropriate counterfactual of national firms that do not invest abroad and a difference-in-difference (DID) estimator to compare the performance of the two types of firms. Constructing an appropriate counterfactual is essential. The effects of outward investments on home activities are relevant not only for themselves but also with respect to what would have happened if firms had not invested abroad. For example, although home employment is observed to decline, perhaps it would have declined even more if these firms had not invested. Albeit this cannot be observed, their hypothetical behavior can be proxied by the behavior of a sample of other firms that have not invested. Furthermore, the use of DID reinforces a causal interpretation of our results by comparing performance trajectories before and after the investment.

We find that the home performance of Italian firms that invested abroad for the first time during the period analyzed improved after the investment. The postinvestment rate of growth of output and productivity is higher than the one observed over the same period for the counterfactual of noninvesting firms. Also, there is no significant evidence of a slowdown of the rate of employment growth. Thus, the evidence supports the view that foreign investments strengthen rather than deplete economic activities at home.

This result is in line with those of Egger and Pfaffermayr (2003), who used a methodology similar to ours on a sample of Austrian firms and found that firms investing abroad also raised their investments in R&D and in intangible assets at home. Other studies on foreign direct investment (FDI) based on other methodologies also generally find evidence that outward investments do not deplete home activities. These earlier empirical works have examined the effects of outward FDI on output (Head and Ries 2001, Blonigen 2001), home employment (Brainard and Riker 1997a, 1997b; Braconier and Ekholm 2002; Konigs and Murphy 2001; Bruno and Falzoni 2003; Blomstrom, Fors, and Lipsey 1997; Lipsey 1999; Mariotti, Mutinelli, and Piscitello 2003; Marin 2004), and productivity (Braconier, Eckholm, and Midelfart Knarvik 2001; Van Pottelsberghe de la Potterie and Lichtenberg 2001). However, they focus at either the sectoral / regional level or, when addressing the question at the firm level, on the activities of multinational enterprises (MNEs) and thus fail to take into account the appropriate counterfactual to this problem.

Our result is also in line with other studies that analyzed this issue but by looking at other measures of outsourcing. For example, Gorg, Hanley, and Strobl (2004), measuring outsourcing by the share of imported inputs at the firm level, find that in a sample of Irish firms, the effect of outsourcing tends to be positive, particularly for large firms and those based in broader international exports (they are foreign owned or are exporters). Finally, work on outsourcing based on industry data does not find negative effect, for the outsourcing of services and material inputs (Amiti and Wei 2004).

The next section outlines the main analytical issues and our empirical strategies. Section 9.3 discusses the methodology used to construct DID estimators. Section 9.4 describes the sample and section 9.5 the construction of the counterfactual. Section 9.6 estimates the effects of investing abroad, and section 9.7 concludes.

9.2 Analyzing the Performance of Investing Firms: The Issue

9.2.1 Analytical Issues

The key concern for policymakers is the effect of foreign investment on the size of economic activity at home. Theories of horizontal and vertical investment do not provide a clear answer to this concern. In particular, both rationales for an increase and for a decrease of employment and output at home can be found in this literature (see Barba Navaretti et al. 2004 for a thorough discussion of this issue). The direction of the effect mainly depends on whether firms substitute domestic labor and output with foreign activities or whether expanding foreign activities complement domestic ones.

Typically horizontal investment is carried out to serve a foreign market and tends to be an alternative to exporting. In this perspective, it could cause a reduction in domestic output. Yet a number of arguments may counter this prediction. First, export may not always be a convenient option in certain sectors and markets (e.g., if tariffs of transport cost are too high), so horizontal investment need not substitute for exporting. Second, in multiproduct firms, FDI, by allowing access to foreign markets, may induce a sort of bandwagon effect for other varieties produced by the same firm, which could eventually be produced in home plants and exported. Third, horizontal investments may well determine an increase in the need for coordination activities and other headquarter services (such as finance, advertising, and R&D), which would largely be carried at home.

Vertical investment involves breaking up activities and the transfering part of them abroad. This is likely changing the division of labor within a firm and shedding relatively more unskilled labor at home but increasing the need for skilled workers. Yet by transferring part of their production abroad, the firm may reduce unit costs and become more competitive; it could gain market share finally increase output in the other home plants.

The other key effect is the contribution of FDI to productivity growth. Even in this case, theoretical predictions are not clear-cut, for both horizontal and vertical investment. There are three main reasons that opening and running foreign subsidiaries affect domestic productivity: the exploitation of firm-level and plant-level scale economies, the change in the composition of inputs used in production, and the opening of new channels of international sourcing of technological and managerial knowledge. These sources of productivity change may work in both directions, depending on the features of the investment.

The effect of changes in output induced by horizontal investment depends on the interplay between plant- and firm-level scale economies; if firm-level scale economies are predominant, then productivity is likely to be enhanced by the expansion of the worldwide activities of the firm; if plant-level scale economies dominate instead, duplicating activities may increase unit costs.

As for vertical investment, home activities could be strengthened or impoverished by changes in their factor use. If, for example, labor-intensive activities are transferred abroad, human-capital-intensive activities at home might become more efficient. In contrast, if there are strong economies of integration between both types of activities, splitting them apart could be costly.

Finally, whatever the type of investment, technologies could be acquired in foreign markets or get lost foreign competitors;

In other words, that we find both positive and negative outcomes of outward investment on the scale and the efficiency of home activities is not inconsistent with theory. For this reason, the issue essentially boils down to an empirical one.

9.2.2 The Empirical Setting

An empirical test of the effects of foreign investments on performance at home poses several methodological problems. First, if we observe only MNEs, we cannot single out the hypothetical benchmark: performance if the firm had not invested abroad. Moreover, if we observe only MNEs, we do not know if changes in performance are due to unobservable shocks equally affecting all firms, national and multinational alike. It is therefore important to benchmark MNEs to a sample of national firms. However, when comparing the performance of MNEs and national firms, we face a second problem: we do not know if differences are due to other observable or unobservable characteristics of the two types of firms (e.g., size, ability of management) rather than to their being multinational or strictly national. In particular, foreign investments and performance are jointly determined. Given that investing abroad has large costs, with imperfect financial markets only the (ex ante), most productive firms will invest abroad. The recent theoretical literature on the decision to export and invest abroad with heterogeneous firms establishes a very clear link between ex ante performance and international activities (Helpman, Melitz, and Yeaple 2004). Thus, if we observe that ex post MNEs perform better than national firms, we do not know if this is so because of foreign investments or because these firms performed better anyway, even before the investment.

To address these problems empirically, it is possible to draw on the well-established literature investigating the effects of exporting on firms' performance (Bernard and Jensen 1999; Clerides, Lach, and Tybout 1998; Aw, Chung, and Roberts 2000; Castellani 2002; Delgado, Farinas, and Ruano 2002; Girma, Kneller, and Pisu 2002; Kraay 1999). The exporter faces the same problem as a firm investing abroad. Consequently, the analysis of the effects of these two decisions raises similar methodological problems.

To illustrate the kind of exercise we carry out in this chapter, it is useful to discuss figure 9.1 which we adapt to the case of foreign investments. We draw average hypothetical trajectories in home performance for three types of firms: those that are always MNEs, with at least one foreign subsidiary during the period observed; those that never have a foreign subsidiary in the period observed (NATIONAL); and those that open their first foreign subsidiary in the period observed and therefore switch from being national to being MNEs (SWs, or switching firms) at time *t*.

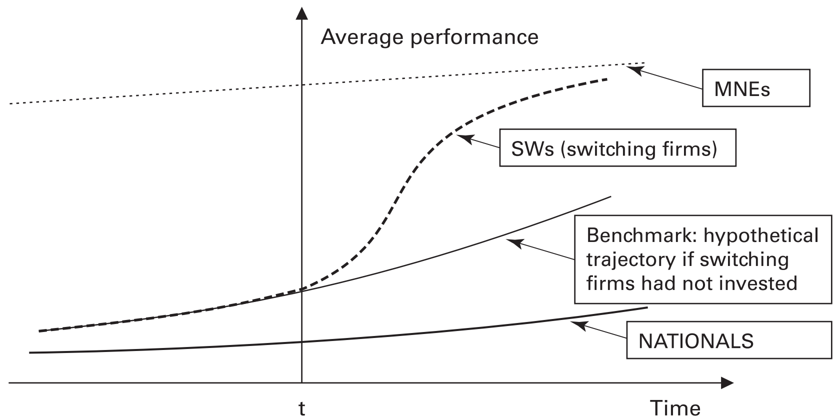

Figure 9.1
Performance trajectories in home plants
Source: Clerides, Lach, and Tybout (1998).

Compare the trajectory of MNEs and NATIONALs. We assume that MNEs perform better than NATIONALs. This assumption reflects what emerges from all available studies: on average, MNEs at home always perform better than NATIONALs (Barba Navaretti et al. 2004; Castellani and Zanfei 2006; Criscuolo and Martin 2002; Girma et al. 2003; Doms and Jensen 1998; Pfaffermayr and Bellak 2002; Bellman and Jungnickel 2002). However, the trajectory of MNEs could lie above the one of NATIONALs because they were the best-performing firms even before becoming multinational or because performance improved as a result of international production

More can be learned if we now focus on SWs—those that invest for the first time at t_0. If the investment has a positive effect on productivity, their trajectory becomes steeper at t_0 and performance converges to the one of MNEs. Thus, our empirical question can be answered by comparing their trajectory after the investment to the one that they would have followed had they not invested. If the investment does indeed improve performance, this hypothetical trajectory lies below the one of the SWs after t, as represented by the dotted line in figure 9.1. This comparison is important; if we focus on just effective performance, even if we observe that it improves, this could be the outcome of other unobserved random factors that have nothing to do with the investment. Unfortunately, the dotted line cannot be observed, and we need to proxy it. Good candidates for the counterfactual are NATIONAL firms, so we could compare the performance trajectory of SWs with the one of NATIONALs. However, we still have a problem. The trajectory of the appropriate counterfactual should differ from the one of SWs because of the different investment decision. But firms are heterogeneous, even within the same industry, and SWs could be different from NATIONALs even before the investment. The assumption of most of the literature is that operating in a foreign environment involves additional costs and risks, so only firms possessing some intangible capital, giving them a competitive edge over national firms, are able to overcome such disadvantages and invest abroad (Dunning 1993, Markusen 1995). Because of this self-selection, the average NATIONAL is not a good benchmark: it is ex ante different from the SW, and this difference may affect ex post performance. Accordingly, we draw the performance trajectory of SWs before the investment above the one of NATIONALs. If we want to isolate the effect of investing, we need to build a counterfactual made of NATIONALs that are as similar as possible to firms that have invested.

To do so, we derive a control group from a propensity score matching procedure. The performance trajectory of this control group is the closest approximation to the dotted line. We will then be able to compare postinvestment performances in the two groups. We will use standard matching estimators that essentially compare the post-t slope of the thick line to the one of the dotted line and DID estimators, which compare the change in the slope of the thick line and of the dotted line before and after t.

9.3 The Evaluation Problem: Propensity Score Matching and Difference-in-Difference Estimators

Our aim is to evaluate the effect of becoming a multinational firm on economic performance at home, Δy (where Δy denotes the rate of growth of employment, output, or total factor productivity, TFP).[1] To gather this effect, we need to understand what would have happened to the firm's economic performance had it not invested abroad. Let SW_{it} be an indicator taking a value equal to one if firm i switches to becoming a multinational by investing abroad for the first time at time t (i.e., between $t-1$ and t). Let also $\Delta y^1_{i,t+1}$ be firm i's postinvestment performance and $\Delta y^0_{i,t+1}$ the hypothetical performance achieved at $t+1$ had i not invested abroad. The effect of investing abroad on economic performances for firm i would then be measured by $\Delta y^1_{i,t+1} - \Delta y^0_{i,t+1}$. More formally, this average effect can be expressed as follows:[2]

$$\hat{\alpha} = E(\Delta y^1_{t+1} - \Delta y^0_{t+1} | SW_{it} = 1)$$

$$= E(\Delta y^1_{t+1} | SW_{it} = 1) - E(\Delta y^0_{t+1} | SW_{it} = 1). \quad (9.1)$$

The key problem is that the last term is unobservable, that is, we do not know what would have been the average performance of SWs if they had not invested. We need to find an appropriate measure for the last term in our sample; in other words, we need to construct an appropriate counterfactual based on the right control group. If we were to run a natural experiment, we could randomly draw a sample of firms from a population and let half invest and the other half not invest. The latter group would be the appropriate control group. Unfortunately, as argued in section 9.2, firms choose endogenously whether to invest.

To overcome the problem of self-selection, we use the method of matching, which aims at reestablishing the conditions of a natural experiment with nonexperimental data (Heckman, Ichimura, and Todd 1997; Blundell and Costa Dias 2002). This methodology has also been used to evaluate the effects of exporting and of acquisitions on firms′ performances and returns to scale by Girma, Greenaway, and Kneller (2004), Girma et al. (2005), Wagner (2002), and Girma and Gorg (2006). Egger and Pfaffermayr (2003) use matching estimators to analyze the effects of outward investments on the decision to invest at home in tangible assets and in R&D.

The idea is to construct an appropriate counterfactual by matching each investing firm with one with similar characteristics drawn from a sample of noninvesting ones. Here we use the nearest-neighbour matching, based on the propensity score method, which computes the probability of investing (the propensity score) conditional on a number of observables. To obtain a measure of the effect of investing abroad on performance at home as free as possible from any self-selection bias, we first estimate a probit model of the decision to become an MNE, which can be represented as

$$P(SW_{it} = 1 | X_{i,t-1}),$$

where $X_{i,t-1}$ is a vector of observable firm i′s characteristics at $t-1$.

It is then possible to compute the probability of switching (propensity score) for each firm and pair each investor with its nearest neighbor, that is, the noninvesting firm with the closest propensity score. In other words, we build a sample where for each investing firm, there is a firm that had a very similar ex ante probability of switching but remained national. This latter group is our counterfactual. Subsequently, the average treatment effect on the treated can be obtained by comparing average performances in the group of investing firms and in the counterfactual, as illustrated by the following equation,

$$\hat{\alpha}_{SM} = \Delta\bar{y}^{1}_{t+1} - \Delta\bar{y}^{0}_{t+1} \tag{9.2}$$

where $\Delta\bar{y}^{1}_{t+1}$ is the mean performance growth of investing firms after switching and $\Delta\bar{y}^{0}_{t+1}$ is a weighted mean of performance growth the control group over the same period.[3] In other words, the average treatment effect on the treated (ATT) can be thought of as a test for the equality of means in performance growth over the switching and the matched control groups.

For the ATT, we also compute a DID estimator. Whereas the ATT compares postinvestment performance growth for the two groups of firms, the DID estimator compares the difference between pre- and postinvestment performance growth in both groups. Formally DID, is given by

$$\hat{\alpha}_{DID} = (\Delta\bar{y}^{1}_{t+1} - \Delta\bar{y}^{1}_{t-1}) - (\Delta\bar{y}^{0}_{t+1} - \Delta\bar{y}^{0}_{t-1}), \tag{9.3}$$

where the over bars denote averages in each group performance before $t-1$ and after $t+1$, the investment year. In substance, the DID measures the differential performance in the group of investing firms relative to the noninvesting ones once ex ante differences in performance are accounted for. It eliminates time-invariant unobserved heterogeneity that might not be captured by matching.

Both ATT and DID can be obtained from ordinary least squares (OLS) estimation. In particular, DID can be estimated from the following regression (Meyer 1995),

$$\Delta y^{j}_{it} = c + \gamma_1 d_t + \gamma_2 d^{j} + \alpha_{DID} d^{j}_{t} + x^{j\prime}_{it}\delta + \varepsilon^{j}_{it}, \tag{9.4}$$

where $j = 0, 1$ denote the control and the switching firm′s groups, respectively; $t = 0, 1$ denote the pre- and postinvestment periods; and the d's are dummies taking the following values:

$d_t = 1$ if $t = 1$ and zero otherwise (the postswitching dummy)

$d^j = 1$ if $j = 1$ and zero otherwise (the switching firm dummy)

$d_t^j = 1$ if $t = 1$ and $j = 1$ and zero otherwise (the DID dummy)

The OLS estimate of $\hat{\alpha}_{DID}$ is the DID estimator of the effect of investing on performance growth. One of the advantages of this specification of DID is that it allows conditioning on a vector of covariates x, which might capture other sources of heterogeneity in the dependent variable. Setting $t = 1$, we can estimate

$$\Delta y_i^j = a + \alpha_{ATT} d^j + x_i^{j'} \delta + v_i^j, \tag{9.5}$$

where the OLS estimate of α_{ATT} is now the ATT.

9.4 Data and Description of the Sample

The data set we use combines the Reprint database of the Politecnico of Milan (which contains information on Italian multinationals and foreign firms operating in Italy) with the Aida database of Bureau Van Dijck (which has information on balance sheet and other economic data of Italian firms). The two databases have been merged by the Centro Studi Luca d'Agliano. The panel used in this chapter includes Italian firms with more that twenty employees with observations between 1993 and 1998. The Reprint database is the main source of information on multinational firms in Italy and provides a good approximation to the universe of activities of both foreign multinationals in Italy and Italian multinationals abroad. For the purpose of this work, we use the issues from 1993 to 1997 of the outward section of Reprint. For each firm in the Reprint database, we were able to count the number of foreign affiliates carrying out manufacturing activity. We thus excluded all sales offices and plants carrying out other nonmanufacturing activities abroad. This allows us to focus on investments involving the transfer of production by Italian firms to a foreign country. By comparing various issues of this directory, we have built an indicator flagging when each firm created its first establishment abroad. This allowed us to select a sample of 193 firms that became multinationals between 1993 and 1997.[4] In order to compute propensity score and DID estimates, we dropped firms switching in the first years of the period analyzed, since for these cases, we were not able to gather any preinvestment information. We ended up with a sample of 119 firms switching between 1995 and 1997. This was complemented with a sample of 1,000 firms with more than twenty employees randomly drawn from the Aida data set,[5] which will be the control group. For each firm from those two sources, we gathered balance sheet information from Aida on the period 1993–1997. In particular, we used information on the year of establishment, the sector of activity, turnover, number of employees, value added, cash flow, operating profits, return on investments (ROI), cost of intermediate materials, tangible fixed capital, capitalized R&D, total assets, and total liabilities. A measure of total factor productivity was derived from these data by estimating a Cobb-Douglas production function with fixed capital, materials, and labor as inputs. We controlled for endogeneity in input use by estimating a fixed-effect model and allowing autocorrelated disturbances (which should account for persistence in productivity). Furthermore, we allowed each sector to have different production functions by having input coefficients interact with sector dummies. Due to missing values in some of the variables, we were forced to drop a few observations. Table 9.1 summarizes the distribution of firms used in the analysis. In particular, we ended up with 114 switching firms and 2,918

Table 9.1
Number and distribution of firms in the sample, by year, sector, size class, and investing status

	National firms	Switching firms	Total
Year			
1995	962	28	990
1996	962	44	1,006
1997	968	42	1,020
Total	2,918	114	3,016
Pavitt sectors			
Scale intensive	28.7	24.6	28.5
Science based	4.9	7.0	5.0
Specialized suppliers	25.7	25.4	25.7
Supplier dominated	40.7	43.0	40.8
Employment classes			
21–49 employees	48.1	20.2	47.1
51–249 employees	45.1	51.8	45.4
250–499 employees	3.8	17.5	4.4
More than 500 employees	2.8	10.5	3.1
Total	100	100	100

observations in the control group. The sectoral distribution of the sample reflects the actual distribution of firms in the Italian economy, where a large proportion of firms is in traditional (supplier-dominated) or specialized supplier industries. Switching firms appear to be relatively more frequent in science-based industries than those in the control group. As far as size is concerned, the sample reflects the large weight of small and medium-sized enterprises in the Italian economy (approximately 50 percent of cases have fewer than fifty employees), but remarkable differences emerge between switching and controls, with the former being significantly more frequent in larger-sized classes. This suggests that switching firms may be very different from the average firm in the control group, so that a simple comparison of these two groups of firms may lead to biased conclusions, as the latter may not provide the more accurate approximation to the counterfactual. In table 9.2 we provide further support for this view by looking at means over a number of characteristics of both switching and control groups and show that the former are significantly larger, more productive, and more profitable (in terms of both ROI and operating profits per employee), while differences are not significant in terms of age, share of capitalized R&D in total assets, share of liabilities on total assets, and cash flow on fixed capital.

9.5 Construction of the Counterfactual

The previous section highlighted that the switching firms are significantly different from the average firm in the control group. Therefore, we need to build an appropriate counterfactual group from a subsample of nationals that did not invest over the 1995–1997 period. To do so, we use the propensity score matching technique. We start by running a probit regression to derive the probability of investing as a function of observable firm-specific characteristics,

$$P(SW_{it} = 1 | Z_{i,t-1}, E_{i,t-1}, F_{i,t-1}, S_i, P_i, yr95, yr96), \qquad (9.6)$$

where $Z_{i,t-1}$ is a vector of firms' attributes such as size, age, and share of intangible assets on total assets; $E_{i,t-1}$ is a vector of efficiency and profitability measures such as TFP, lagged TFP growth, operating margin per employee, and ROI; $F_{i,t-1}$ is a vector of financial variables such as the ratio of debt to total assets and the share of cash flow in total capital; S_i and P_i are sector and province dummies; and $yr95$ and $yr96$

Table 9.2
Switching firms and unmatched control group, various characteristics (means)

Unmatched sample	Control	Switching	Differential	SE	Number of observations
(Log)TFP$_{i,t-1}$	−0.128	0.321	0.449**	(0.060)	3,016
Growth of TFP$_{i,t-2}$	0.010	0.003	−0.007	(0.010)	3,016
(Log)N. employees$_{i,t-1}$	4.113	4.957	0.844**	(0.106)	3,016
Profits per employee$_{i,t-1}$	0.219	0.385	0.166**	(0.048)	3,016
ROI$_{i,t-1}$	0.069	0.086	0.017**	(0.008)	3,016
(Log)Age$_{i,t-1}$	2.940	3.031	0.091	(0.062)	3,016
Capitalized R&D/total assets$_{i,t-1}$	0.001	0.002	0.001	(0.001)	3,016
Debt/total assets$_{i,t-1}$	0.671	0.678	0.008	(0.015)	3,016
Cash flow/fixed capital$_{I,t-1}$	0.756	0.521	−0.234	(0.154)	3,016

**$p < .05$

Table 9.3
Determinants of the probability of switching

Variable	(1) Whole sample	(2) Matched sample	(3) Matched sample
(Log)TFP$_{i,t-1}$	0.346**	−0.213	−0.144
	(0.157)	(0.156)	(0.365)
Growth of TFP$_{i,t-2}$	−0.678	−1.130	−1.303
	(479)	(0.938)	(1.204)
(Log)N. employees$_{i,t-1}$	0.422**	0.040	−0.010
	(0.067)	(0.093)	(0.155)
Profits per employee$_{i,t-1}$	0.004**	0.003	0.002
	(0.001)	(0.003)	(0.004)
ROI$_{i,t-1}$	0.015*	0.245	0.193
	(0.008)	(1.444)	(1.950)
(Log)Age$_{i,t-1}$	0.138*	0.114	0.165
	(0.081)	(0.137)	(0.181)
Capitalized R&D/total assets$_{i,t-1}$	0.047	−1.175	−2.625
	(0.031)	(4.559)	(5.459)
Debt/total assets$_{i,t-1}$	0.532*	0.460	0.006
	(0.309)	(0.625)	(0.788)
Cash flow/fixed capital$_{i,t-1}$	−0.108**	−0.119	−0.170
	(0.033)	(0.086)	(0.139)
Sector dummies	Yes	No	Yes
Province dummies	Yes	No	Yes
Year dummies	Yes	Yes	Yes
Number of observations	3,016	224	207
Pseudo R-squared	0.212	0.019	0.096
LR test[a]	206.48**	6.12	27.56

[a] Test for the hypothesis that all coefficients are jointly equal to zero.
** $p < .05$. * $p < .10$.

are two time dummies. The results of the estimation of equation 9.6 are reported in table 9.3 in column 1 and support the hypothesis that size, productivity, and profitability are important determinants for becoming a multinational firm. In other words, we confirm that multinationals have some ex ante advantage over national firms that likely compensate for the higher costs and risks of running a business abroad.

With the propensity score obtained from the estimation of equation 9.6, we are now able to build an appropriate counterfactual to our sample of switching firms by matching each of them to the control firms

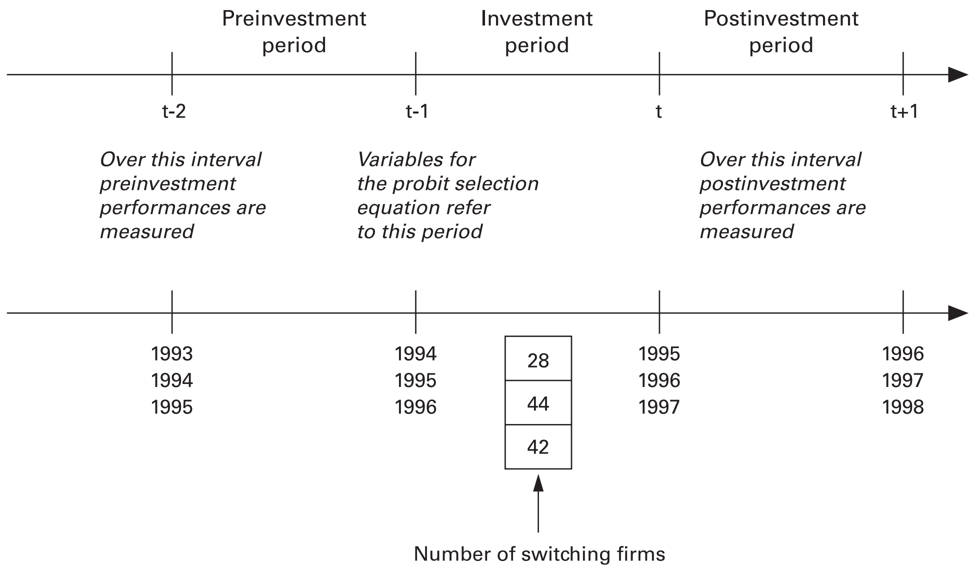

Figure 9.2
Time structure of investment patterns

with the closest propensity score (this is called nearest-neighbor matching). We run the nearest-neighbor algorithm[6] year by year, so that we match each switching firm to an observation from the control group in the same year the switching occurs. By doing this, we ensure that, for example, a firm switching in 1995 will be matched to a firm that prior to investment (in 1994) was similar to it. Then, as illustrated in figure 9.2, performance one year after the investment (1995–1996) will be compared in switching and control firms.

As we have already suggested, propensity score matching should account for ex ante differences in the sample of switching firms and in the counterfactual, therefore, generating a counterfactual with characteristics as close as possible to those of the investing firms. In formal terms, the matched sample should satisfy the balancing property, that is, the distribution of the vector of observables should be balanced across switching and control firms. We therefore need to assume conditional independence when we derive propensity scores, that is, we need to rule out that the choice of investing abroad is significantly affected by unobservable variables that also determine postinvestment performance. Because of data limitations, we cannot fully exclude that unobservables do indeed play a role, so we proceeded with caution. In

Table 9.4
Score of switching and control groups, before and after matching

	Unmatched sample		Matched sample		Difference in propensity score between each switching and its nearest neighbor
	Control	Switching	Control	Switching	
Number of observations	2,902	114	112	112[a]	112
Mean	0.033	0.150	0.141	0.141	0.004
Minimum	0.000	0.001	0.001	0.001	9.7E-08
10th percentile	0.002	0.019	0.019	0.019	6.6E-06
25th percentile	0.005	0.042	0.041	0.041	4.3E-05
Median	0.013	0.099	0.096	0.096	2.4E-04
75th percentile	0.039	0.208	0.192	0.191	0.002
90th percentile	0.085	0.362	0.328	0.339	0.011
Maximum	0.614	0.710	0.589	0.566	0.023

[a] Two switching firms fell outside the common support (the interval between the largest propensity score of a control firm and the lower propensity score of a switching firm) and were dropped by the matching algorithm.

what follows, we discuss all the steps taken to ensure that the balancing property is satisfied.

First, in estimating equation 9.6, we control for as many observable firms' characteristics as possible (including a large set of sector and province dummies) and reach a satisfactory result in terms of explained variance, as indicated by a pseudo-R^2 of 21.2 percent, which is in line with most existing works using matching techniques.

Second, in table 9.4 we provide some statistics on the propensity score of the switching and control groups before and after matching. The probability of switching is much higher in the group of firms that actually switched than in the unmatched control group. In the final column of table 9.4, we compare switching firms to the matched counterfactual. The two groups of firms have very similar propensity scores in terms of both mean and various percentiles. Furthermore, from the last column of table 9.4 we see that the difference in propensity scores between switching firms and their matched neighbor is very small (for more than 50 percent of the cases, it is lower than 0.02 percent, and on average it is only 0.4 percent). This result is confirmed by the graphical representation of these distributions provided in figure 9.3, where it is

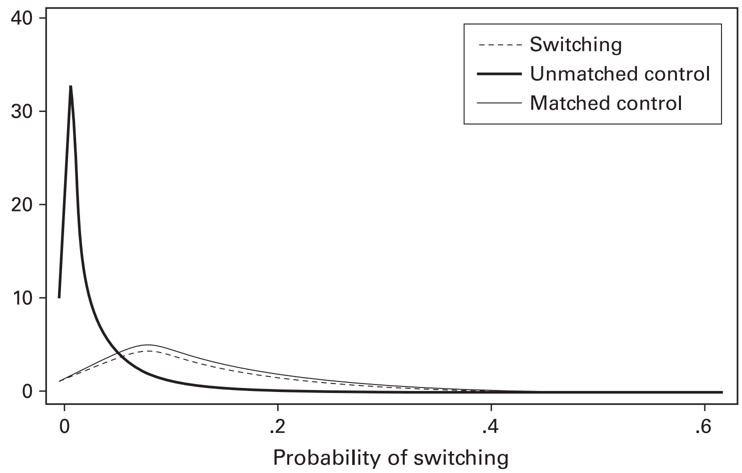

Figure 9.3
Distribution of the propensity score in switching and (matched and unmatched) control firms

clear that the distribution of the propensity score of the unmatched control group is very skewed leftward (thick line), whereas the one for the matched subsample (thin solid line) is very close to the one for the switching firms (dotted line).

Third, in table 9.5 we compare the characteristics of the matched control group with the switching firms and find that differences between the two groups of firms are nonsignificantly different from zero in all the characteristics used to estimate the propensity score. This result should be compared to the one reported in table 9.2, where the unmatched counterfactual is significantly different from the switching sample for several of the variables considered.

Fourth, we follow Sianesi (2004) and run the selection probit on the matched sample with and without sector and location controls (third and second column of table 9.3, respectively). As expected, we find that pseudo-R^2 drops significantly (from .212 in the whole sample to .096 in the matched sample), and no regressor is significant either individually or jointly.[7] These results support the presumption that the characteristics included in the probit are not statistically significant either individually or jointly when we use the matched sample.

Fifth, and finally, we test that the whole distribution, not only the means, of the characteristics of firms in the switching and control

Table 9.5
Switching firms and matched control group, various characteristics (means)

Matched sample	Control	Switching	Differential	SE	Number of observations
(Log)TFP$_{i,t-1}$	0.422	0.308	−0.114	(0.094)	224
Growth of TFP$_{i,t-2}$	0.018	0.004	−0.015	(0.014)	224
(Log)N. employees$_{i,t-1}$	4.994	4.934	−0.059	(0.149)	224
Profits per employee$_{i,t-1}$	0.432	0.377	−0.055	(0.098)	224
ROI$_{i,t-1}$	0.091	0.087	−0.004	(0.011)	224
(Log)Age$_{i,t-1}$	2.986	3.021	0.035	(0.088)	224
Capitalized R&D/total assets$_{i,t-1}$	0.003	0.003	−0.001	(0.003)	224
Debt/total assets$_{i,t-1}$	0.666	0.676	0.010	(0.021)	224
Cash flow/fixed capital$_{I,t-1}$	0.870	0.526	−0.344	(0.302)	224

Table 9.6
Kolmogorov-Smirnov test for equality of distribution functions: Switching versus control firms (various characteristics)

	Unmatched sample		Matched sample	
	K-S	p-value	K-S	p-value
(Log)TFP$_{i,t-1}$	0.337	[0.000]	0.085	[0.795]
Growth of TFP$_{i,t-2}$	0.056	[0.854]	0.090	[0.732]
(Log)N. employees$_{i,t-1}$	0.422	[0.000]	0.160	[0.103]
Profits per employee$_{i,t-1}$	0.162	[0.004]	0.079	[0.859]
ROI$_{i,t-1}$	0.079	[0.444]	0.119	[0.377]
(Log)Age$_{i,t-1}$	0.109	[0.116]	0.133	[0.253]
Capitalized R&D/total assets$_{i,t-1}$	0.041	[0.990]	0.045	[1.000]
Debt/total assets$_{i,t-1}$	0.048	[0.943]	0.084	[0.803]
Cash flow/fixed capital$_{I,t-1}$	0.115	[0.085]	0.111	[0.471]

group, does not differ after matching. This is done by testing for the difference in the distribution function using the Kolmogorov-Smironov statistics (in table 9.6). This analysis suggests that the distribution of all variables becomes very similar in the matched controls and in the switching group, regardless of how different they are in the whole sample. Furthermore, the Kolmogorov-Smirnov does not reject equality of distribution in any of the variables in the matched sample.

9.6 Effects of Investing Abroad: Results

We now use the matched sample to estimate the impact of the creation of foreign subsidiaries on firms' performances. We estimate both the average treatment effect (ATT) and the difference-in-difference (DID) estimator.[8] Our outcome variables are three indicators of firms' economic performances: TFP growth, employment growth, and output (measured by total sales) growth. Here we mainly concentrate on a robust estimation of the partial effect of investing abroad on the three indicators, but since we are aware of possible relations among these three indicators, we partially allow the possibility that the effect of investment on the expansion of output might also explain the growth in employment and productive efficiency (through economies of scale). However, a full account of the interlinkages (such as the impact of an increase in TFP on output growth, through an increase of international competitiveness or through factor mix reallocation) and the channels through which these effects occur is left for further investigation.

Table 9.7
Effect of investing abroad on firms' performances, 1993–1998

Dependent variable	Turnover growth			
Estimator	SM	DID		
Effect of investing (α)	0.079** (0.027)	.110** (.043)		
Switching firm dummy (γ_2)		−0.030 (0.029)		
Postswitching dummy (γ_1)		−0.146** (0.032)		
Constant	−0.044** (0.023)	0.101** (0.021)		
Observations	224	448		
R^2	0.03	0.05		
Dependent variable	**TFP growth**			
Estimator	**ATT**	**DID**	**ATT**	**DID**
Effect of investing (α)	0.041** (0.021)	0.059** (0.025)	0.004 (0.010)	0.014 (0.016)
Switching firm dummy (γ_2)		−0.016 (0.014)		−0.004 (0.010)
Postswitching dummy (γ_1)		−0.043** (0.020)		0.016 (0.010)
Turnover growth			0.457** (0.121)	0.407** (0.065)
Constant	−0.021 (0.017)	0.020 (0.011)	−0.001 (0.008)	−0.021** (0.009)
Observations	224	448	224	448
R^2	0.01	0.02	0.39	0.44

In table 9.7 we report estimates of the effect of investing abroad, using both DID and ATT (from equations 9.4 and 9.5). We compute bootstrapped standard errors to adjust for additional sources of variability introduced by the estimation of the propensity score as well as the matching process. As illustrated in figure 9.2, switching may occur at three different points in time (1995, 1996, or 1997). In the estimation of the effect of investing abroad on performance at home, we pooled all firms investing in the three years. However, we made sure that each switching firm is compared with observations from the same period of time, as we have matched them year by year. For example, per-

Table 9.7
(continued)

Dependent variable	Employment growth			
Estimator	ATT	DID	ATT	DID
Effect of investing (α)	0.042**	0.036	0.013	−0.005
	(0.021)	(0.031)	(0.019)	(0.030)
Switching firm dummy (γ_2)		0.004		0.014
		(0.023)		(0.020)
Postswitching dummy (γ_1)		−0.090**		−0.036**
		(0.020)		(0.019)
Turnover growth			0.368**	0.385**
			(0.080)	(0.062)
Constant	−0.010	0.083**	0.006	0.046
	(0.017)	(0.013)	(0.013)	(0.014)
Observations	224	448	224	448
R^2	0.01	0.05	0.23	0.28

Note: DID estimates are gathered from the following equation: $\Delta y_{it}^{j} = c + \gamma_1 d_t + \gamma_2 d_t^{j} + \alpha_{\mathrm{DID}} d_t^{j} + x_{it}^{j\prime}\delta + \varepsilon_{it}^{j}$, where d^{j} takes value 1 for switching firms (both before and after investing) and zero for controls, d_t takes value 1 in the year after investment (for both groups of firms), and d_t^{j} takes value 1 for switching only after investment. The coefficient associated with the last term (α) is the DID estimate of the effect of investing abroad. ATT have been obtained from estimating the same equation, after setting $d_t = 1$. Asterisks denote significance level at 5 percent (**) and 10 percent (*), based on bootstrapped standard errors (500 repetitions).

formance of a firm switching in 1995 will be observed one year after investment (in the period 1995–1996), and the control firm will be observed over the same time interval.

Results support a significant effect of investing abroad for the first time on output growth: switching firms have a 7.9 percent higher growth rate in output than their counterfactual in the year following the investment. This effect is even larger (11 percent) if we account for preswitching output dynamics (DID estimators). The ATT and DID are positive (although slightly lower than in the case of output) and significant also in terms of TFP, though we find no significant effect on the rate of employment growth after accounting for preinvestment performance (DID). In columns 3 and 4 of table 6, we investigate to what extent the effect of investment on TFP and employment may be due to scale economies and an indirect effect of output growth. For this purpose, we estimate ATT and DID in terms of TFP and employment growth controlling for the contemporaneous growth in output. Results suggest that the effect of investing on TFP and employment is much lower once we account for output growth and both ATT and DID turn nonsignificantly different from zero.

9.7 Conclusions

This chapter examines the effects of foreign investment on the home activities of MNEs in Italian manufacturing. Contrary to widespread concerns about international outsourcing, we find that investing abroad significantly boosts performance at home. The rate of growth of total factor productivity and output is significantly higher for investing firms, and it accelerates after the investment takes place. This result is robust to the inclusion of different controls. We also find that investing has no significant effect on employment growth. In this perspective, actions aimed at discouraging foreign investments and the creation of foreign employments seem shortsighted and risk weakening the domestic economy rather than strengthening it.

As a note of caution, we should stress that our analysis provides a limited picture of the outsourcing process. First, our sample includes all types of investments. We cannot distinguish between vertical investments aimed at fragmenting production internationally (which is what is normally meant by outsourcing) and horizontal investments aimed at entering foreign markets that do not necessarily involve the transfer abroad of activities previously carried out at home. Second, we cannot control whether firms in our sample internationally outsource their activities through channels other than FDI, for example, by undertaking arm's-length agreements with firms based in foreign countries.

Notes

1. This is usually defined the outcome in the evaluation literature. See Blundell and Costa Dias (1999, 2002) and Wooldridge (2002, chap. 18) for reviews.

2. In the literature, this is referred to as the average treatment effect on the treated (ATT). The original idea is derived from natural sciences, where some outcome from individuals who receive a treatment (such as a medical treatment) is compared to identical individuals (randomly drawn from a population) who did not receive treatment. In economics, things are complicated by the fact that nontreated individuals are nonrandomly selected.

3. In the nearest-neighbor matching used in this chapter, weighting is simply used to account for the fact that one control can be matched to more than one switching firm.

4. One referee noted that the analysis should also include subsequent investments made by firms that are already multinationals. Although we agree that the effects of investment

by nonswitchers are also important and should be taken into account, we prefer to focus this chapter on the effects of discrete changes in investment behavior (new investments rather than ongoing ones) on performance at home. We believe that focusing on first-time investors is a good and original way to isolate this factor. Including all investors would make it difficult to isolate the impacts of new investments from those of ongoing ones.

5. The draw was carried out on firms that appeared not to be multinational firms in the 1993–1997 period and were not foreign owned.

6. We used the -psmatch2- command in Stata.

7. The second column drops sector and province dummies, and the third column also includes those dummies. The number of observation drops in the latter case as some observations are perfectly predicted by those dummies (there may be only one firm in each sector-province).

8. Bertrand, Duflo, and Mullainathan (2004) raise some concerns on the use of DID estimators. They warn that standard errors in DID estimators may be inconsistent due to serial correlation, and this may yield a false rejection of the null hypothesis of no treatment effect. However, their problem is not likely to affect our results, for both theoretical and empirical reasons. From the theoretical point of view, at least two of the three sources of serial correlation in DID mentioned in Bertrand et al. (p. 251) do not seem to apply in our context. In fact, (1) we do not have a long time series, since our data span only five years, and (2) our dependent variables are growth rates, which usually do not display strong serial correlation patterns. From an empirical point of view, we implement two of the solutions proposed by Bertrand et al. In particular, we consider only the information on pre- and postinvestment, thus collapsing the time series into two points, and we rely on bootstrapped standard errors.

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