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Sascha O. Becker and Marc-Andreas Muendler

Abstract

Novel linked employer-employee data for multinational enterprises and their global workforces show that multinational enterprises that expand abroad retain more domestic jobs than competitors without foreign expansions. Propensity-score estimation demonstrates that the foreign expansion itself is a dominant explanatory factor for reduced worker separation rates. Bounding, concomitant variable tests, and further robustness checks show competing hypotheses to be less plausible. The finding is consistent with the hypothesis that, given global wage differences, a prevention of enterprises from outward FDI would lead to more domestic job losses. FDI raises domestic-worker retention more pronouncedly among highly educated workers.

KEYWORDS: multinational enterprises, international investment, demand for labor, worker layoffs, linked employer-employee data

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1 Introduction

The formation of multinational enterprises (MNEs) is a driving force of global integration. Much empirical research on MNEs investigates the economically important question how international wage differences affect MNEs' employment choices. We investigate in this paper the arguably more policy-relevant question whether preventing firms from exploiting these wage differences in-house would threaten even more jobs.

We use a propensity-score matching approach that makes expanding MNEs comparable to non-expanding firms. Our findings robustly show that foreign direct investment (FDI) expansions turn worker retentions significantly more likely at MNE home plants compared to domestic competitors without FDI expansions. Of course, intra-firm trade, as any other form of trade, should lead to employment shifts away from activities with no comparative advantage. Such labor reallocation, however, is associated with costs for firms and workers. Our results show MNEs' employment responses to global trade to be less disruptive than those at domestic competitors and to provide more job security.

We construct a novel linked employer-employee panel data set for Germany to conduct the analysis. Job-level data allow us to separate the decision maker (the MNE) from the treated unit (the job). This special feature and a comprehensive set of job, worker, plant, MNE and sector characteristics lend particular support to estimation under propensity-score matching. Propensity-score matching picks pairs of identical domestic jobs: one job in the pair randomly treated with an expansion of foreign direct investment (FDI), and the other job untreated. We focus on job separation as the outcome, a main component of job security that is well defined at the worker level.[1] The estimator then measures how an FDI expansion changes the probability that a domestic job holder suffers separation.

In recent work, Desai, Foley and Hines (2005a, 2005b) use GDP growth at an MNE's foreign location to instrument for FDI expansions. By design, that instrumentation strategy excludes FDI at the presence-establishing extensive margin. About half the home employment effect of FDI in our sample, however, is explained by adjustment at the extensive margin (Muendler and Becker 2006). Propensity-score matching is an alternative. It has the additional advantage that, even if an instrument covaries with the home employment outcome (say if Eastern European and German growth were correlated), unrelated worker separations are not attributed to FDI. The reason is that initial propensity-score estimation controls for MNE and host-country characteristics and subsequent outcome estimation controls for con-

[1] Net employment change is not a worker-level outcome. Worker accessions to new jobs are related to job creation but not directly associated with job security.

comitant economic changes. Methodological differences notwithstanding, findings for capital use across locations in Desai et al. (2005a) and for employment security in our paper are mutually supportive.

There is a separation rate of 14 percent among workers at MNEs, compared to 18 percent for workers at firms with no foreign affiliate.[2] Our propensity-score estimator shows FDI expansions to explain around half of the four-percentage points difference. When distinguishing FDI expansions by foreign region, we find significant reductions in the rate of job loss of up to seven percent and never find outward FDI to increase the probability of home worker separation. We find no marked difference between occupation types, but we find more educated workers to be retained more frequently after foreign expansions than their less educated colleagues.

We perform a series of robustness checks to quantify the potential influence of hidden bias (violations of the assumption of selection on observables) and concomitant variables, and probe the sensitivity of our results to alternative specifications and treatment definitions. These checks diminish the plausibility of main competing hypotheses. MNEs may possess ownership advantages, such as innovative processes or products, prior to FDI expansions. A pre-existing advantage manifests itself in observables, however, such as prior FDI or higher labor productivity, and we control for those. More important, firms might acquire an ownership advantage or experience a favorable demand or productivity shock and simultaneously expand both home employment and FDI abroad, particularly in fast growing locations. This identification issue affects both instrumental-variable and propensity-score estimation. We use Rosenbaum (2002) bounds to assess the plausibility of this hypothesis and estimate that an unobserved confounding factor, such as a simultaneous process or product innovation, would have to alter the odds of treatment by more than 25 percent to overturn the findings. This implied change is sizeable and unlikely: it would be equivalent to, for instance, an increase in the share of tertiary-schooled workers from zero to a hundred percent of the workforce.

There might be simultaneous sector-wide changes, such as trends in foreign trade, that affect FDI-exposed enterprises differently from domestic firms, but are unrelated to FDI expansions. Another robustness check queries whether such concomitant variables (variables that incidentally vary with the treatment) erroneously attribute measured effects to the treatment. We find only a slight change of the estimates, within common confidence bands, and no evidence for erroneous attribution. We examine the explanation with several further checks, using alternative control-group and treatment definitions, and find overwhelmingly robust estimates.

[2]These separation rates do not include workers with relatively instable employment histories. We restrict our sample to workers with a job at some employer twelve months prior to the day when we observe separation or retention.

These results suggest that the foreign expansion itself is the strongest explanatory factor for reduced separation rates.

Several interpretations are consistent with these findings. Vertical foreign expansions that fragment the production process can lead to cost savings, increased worldwide market shares, and domestic employment growth. Similarly, horizontal expansions that duplicate production at foreign locations can lead to improved market access with potentially beneficial consequences for headquarters employment.[3] The stability afforded by in-house relationships across borders, compared to arm's length trade, may result in more stable employment. Foreign expansions may signal attractive career paths to domestic workers and reduce worker quits (Prendergast 1999).[4]

To our knowledge, there is to date no job-level research into the effects of MNE activities. Much existing research investigates how global wage differences predict home employment at MNEs, using data for firms or higher levels of aggregation. Although MNEs are important mediators of world trade,[5] most existing research does not find FDI to strongly affect home factor demands. Several studies conclude that MNE production in low-wage regions has no detectable or only a modest impact on home labor demand at MNEs (Slaughter 2000, Konings and Murphy 2006, Marin 2006, Harrison, McMillan and Null 2007). An exception is Muendler and Becker (2006), where we control for location selectivity beyond earlier estimation strategies and find salient labor substitution across locations, both at the presence-establishing extensive and the affiliate-operating intensive margin.

In contrast to those approaches, linked employer-employee data allow us to compare jobs between MNEs and their national competitors. Geishecker (2006) uses household survey data to study the effect of sectoral intermediate-goods imports on German workers. He finds cross-border outsourcing to reduce individual employment security. This is not necessarily in contrast to our findings. FDI expansions abroad provide access to both suppliers and clients, and within-firm imports involve more capital-intensive intermediate goods than cross-firm imports (Antras 2003).

Methodologically related papers include Egger and Pfaffermayr (2003), Barba

[3] In practice, foreign affiliates do not fit the strict vertical-horizontal dichotomy. Feinberg and Keane (2006) document that less than a third of U.S. MNEs with Canadian affiliates satisfy the dichotomy; Ekholm, Forslid and Markusen (2007) alert to the importance of export-platform FDI.

[4] Complementarity between foreign and domestic factors is another candidate interpretation. Structural cost-function estimates suggest, however, that labor is a strong substitute across locations (Muendler and Becker 2006).

[5] UNCTAD (2006) estimates that about a third of world exports originate from foreign affiliates of MNEs in 1990 and 2005, and that the share of value added at MNE affiliates in world output is 10.1% in 2005, up from 6.7% in 1990.

Navaretti and Castellani (2004) and Jackle (2006), who apply propensity-score matching to MNE-level data. Egger and Pfaffermayr (2003) find no clear difference in investment behavior between exporters and MNEs. Similarly, Barba Navaretti and Castellani (2004) and Jackle (2006) do not report significant effects of outward FDI on MNE home performance for Italian and German MNEs. Beyond prior research, our linked employer-employee data allow the propensity score to account for multiple sources of heterogeneity—worker, job and plant characteristics beyond MNE and sector covariates—, and methodologically separate the MNE as decision maker from the treated job.

The paper has five more sections. Section 2 discusses the methodology, Section 3 describes the construction of our linked employer-employee data. We present the main results in Section 4, and assess competing explanations in Section 5. Section 6 concludes. Details of data construction and methodological derivations are relegated to the Appendix.

2 Methodology

Propensity-score matching aims at reducing the bias in treatment-effect estimates when the sample is not random (Rosenbaum and Rubin 1983). To fix ideas, consider management boards of two identical firms that vote on a foreign expansion, given the same observable evidence. Chance, such as accidental access to market expertise or the foreign language proficiency of a board member, induces one board to vote with an edge in favor of expansion, whereas the other board votes against—creating random variation.

We provide a brief methodological discussion in our context. Our estimator measures the *average treatment effect on the treated* (ATT), in our case the average treatment effect of an enterprise's FDI expansion abroad on the treated domestic job, which can either be kept or be cut. Absent a random assignment to treatment and control groups in non-experimental data, confounding factors may distort estimates of the treatment effect. Propensity-score matching removes the bias by comparing outcomes between treated and control units (jobs) that are initially identical and undergo treatment (an enterprise's FDI expansion abroad) almost randomly. A crucial assumption is that observable covariates exhaustively determine selection into treatment. The wealth of information in our data—on the worker, the job, the plant, the enterprises's foreign operations and the industry—comprehensively covers the pretreatment conditions so that treatment is plausibly ascribable to random changes at the plant, parent-firm or industry level. Beyond typical data sources, where the treated unit itself chooses selection into treatment, our linked employer-employee data allows us to separate the treated unit, the individual job, from the decision maker, the parent firm. Several tests of underlying assumptions, as well as a series of specification and robustness checks, assess the method's validity.

Matching treated units (jobs) on a vector of characteristics suffers dimensionality problems for large sets of characteristics. Propensity-score matching therefore summarizes pretreatment characteristics into a scalar, the propensity score. Exposing jobs with the same propensity score value to random treatment eliminates the bias in estimated treatment effects. Define the *propensity score* as the conditional probability of receiving treatment given pretreatment characteristics,

$$p(\mathbf{x}_i) \equiv Pr(d_i = 1 \mid \mathbf{x}_i) = \mathbb{E}[d_i|\mathbf{x}_i], \tag{1}$$

where d_i is the indicator of job i's exposure to treatment, taking a value of one iff the enterprise of job i expands its FDI exposure between years $t-1$ and t; and $\mathbf{x}_i$ is the vector of pretreatment characteristics in year $t-1$. (We omit time subscripts to save on notation.)

Rosenbaum and Rubin (1983) show that, if the exposure to treatment is random within cells defined by $\mathbf{x}_i$, it is also random within cells defined by the values of the scalar propensity score $p(\mathbf{x}_i)$. Rosenbaum and Rubin (1983) also show that, if the propensity score $p(\mathbf{x}_i)$ is known, the ATT can be defined as

$$\begin{aligned} ATT &\equiv \mathbb{E}[y_{1i} - y_{0i}|d_i = 1] \\ &= \mathbb{E}[\mathbb{E}[y_{1i} - y_{0i}|d_i = 1, p(\mathbf{x}_i)]] \\ &= \mathbb{E}\big[\,\mathbb{E}[y_{1i}|d_i = 1, p(\mathbf{x}_i)] - \mathbb{E}[y_{0i}|d_i = 0, p(\mathbf{x}_i)]\;\big|\,d_i = 1\big], \end{aligned} \tag{2}$$

where outer expectations are over the distribution of $p(\mathbf{x}_i)|d_i = 1$, and y_i is the outcome taking a value of one iff the holder of job i is displaced through a layoff or quit between t and $t+1$ (note the one-year lag between treatment and outcome). To denote the two counterfactual situations of, respectively, treatment and no treatment, we use shorthand notations $y_{1i} \equiv (y_i|d_i = 1)$ and $y_{0i} \equiv (y_i|d_i = 0)$. The derivation of the ATT estimator requires two intermediate results to hold.

First, the pretreatment variables need to be *balanced* given a valid propensity score (Rosenbaum and Rubin 1983, lemma 1): If $p(\mathbf{x}_i)$ is the propensity score, then

$$d_i \quad \perp \quad \mathbf{x}_i \quad | \quad p(\mathbf{x}_i). \tag{3}$$

As a consequence, observations with the same propensity score have the same distribution of observable (and unobservable) characteristics independent of treatment status. Put differently, exposure to treatment is random for a given propensity score so that treated and control jobs are, on average, observationally identical. The orthogonality of d_i and $\mathbf{x}_i$ conditional on the propensity score is empirically testable. We perform according balancing tests and compare changes in the goodness of fit for alternative sets of pretreatment variables $\mathbf{x}_i$.

Second, the assignment of the treatment needs to be *unconfounded* conditional on observable characteristics (Rosenbaum and Rubin 1983, lemma 2). If assignment to treatment is unconfounded, that is if

$$y_{1i}, y_{0i} \quad \perp \quad d_i \quad | \quad \mathbf{x}_i, \tag{4}$$

then assignment to treatment is unconfounded given the propensity score, that is

$$y_{1i}, y_{0i} \quad \perp \quad d_i \quad | \quad p\,(\mathbf{x}_i). \tag{5}$$

Equation (4) is a maintained assumption of our method. To query unconfoundedness, we test whether the predictive power of job-level variables is zero once plant, parent-firm and sector covariates are included in propensity score estimation.

We estimate the propensity score $Pr(d_i = 1 \,|\, \mathbf{x}_i) = F(h(\mathbf{x}_i))$ under the assumption of a logistic cumulated distribution function $F(\cdot)$, where $h(\mathbf{x}_i)$ is, in principle, a function of linear and higher-order terms of the covariates. We find linear terms on our comprehensive set of covariates to suffice for balancing (3) to be satisfied and omit higher-order terms.

To implement an estimator for the ATT (2), we use the estimated propensity scores to pick pairs based on nearest-neighbor matching. Denote by $\mathbb{C}(i)$ the set of control units matched to the treated unit i with an estimated value of the propensity score of p_i. Nearest-neighbor matching assigns $\mathbb{C}(i) \equiv \min_j \parallel p_i - p_j \parallel$, a singleton unless there are ties (multiple nearest neighbors). In the non-experimental sample, we observe y_{1i} only for treated jobs and y_{0i} for untreated jobs. The estimator therefore uses y_i^T from the treated subsample as treated outcome and y_j^C from the control sample as counterfactual outcome y_{0i}. We denote the number of controls matched to observation $i \in T$ by N_i^C and define weights $w_{ij} \equiv 1/N_i^C$ if $j \in \mathbb{C}(i)$, and $w_{ij} = 0$ otherwise. Then, the nearest neighbor estimator of the ATT is:

$$ATT^{NN} = \frac{1}{N^T} \sum_{i \in T} \left[y_i^T - \textstyle\sum_{j \in \mathbb{C}(i)} w_{ij} y_j^C \right], \tag{6}$$

where N^T denotes the number of treated and N^C the number of control observations. Our propensity score estimator is the mean difference in outcomes over matched pairs.

Estimator (6) is our main specification and based on a binary treatment (whether or not an MNE expands abroad). For robustness checks, we use Rosenbaum (2002) bounds and assess the effect that a hypothetical unobserved confounding factor would need to have so as to overturn the ATT estimate. Beyond the binary ATT estimator (6), we consider the Hirano and Imbens (2004) extension to propensity-score estimation to obtain the average causal effect of a continuous treatment (the magnitude of an MNE's foreign expansion). We discuss these extensions in Appendices C and D. We obtain both analytical and bootstrapped standard errors for our main specifications but find both standard-error estimates to be similar and choose to report only analytical standard errors.

3 Data

We construct our linked employer-employee data set for the manufacturing sector from three confidential micro-data sources, assembled at Deutsche Bundesbank headquarters in Frankfurt, and add industry and country information. We define enterprises as groups of affiliated domestic and foreign firms and consider all firms within a group as potential *FDI-conducting firms* if at least one firm in the group reports outward FDI activity. We weight the FDI exposure measures by the ownership shares that connect the firms in the group. Firms outside any group with FDI exposure are classified as *domestic firms*.

The first component of our linked employer-employee data set, worker and job information, comes from quarterly files extracted from the social-security records of the German Federal Labor Agency (BA). The observations are the universe of workers registered for unemployment insurance in the years 1999-2001, representing around 80% of the German workforce.[6] The files contain worker and job characteristics such as age, education, occupation and wages. Wages in the German social security data are censored above but not below. The upper bound is the contribution assessment ceiling for old-age insurance, which is annually adjusted for nominal wage changes.[7] We construct plant-level information by aggregation from the individual-level information.

Second, information on outward FDI comes from the MIDI database (MIcro database Direct Investment, formerly DIREK), collected by Deutsche Bundesbank (BuBa); see Lipponer (2003) for a documentation. The MIDI data on outward FDI cover the foreign affiliates of German MNEs above ownership shares of 10 percent.[8]

[6]Coverage includes full- and part-time workers of private enterprises, apprentices, and other trainees, as well as temporarily suspended employment relationships. Civil servants, student workers, and self-employed individuals are excluded and make up the remaining 20% of the formal-sector labor force. Plants within the same municipality may report under one single plant identifier.

[7]The ceiling is at an annual wage income of EUR 52,765 in 2000 and EUR 53,379 in 2001, except for miners (*Knappschaftliche Rentenversicherung*) with a ceiling of EUR 65,036 in 2000 and EUR 65,650 in 2001.

[8]In 1999 and 2000, reporting is mandatory for all foreign affiliates with a balance sheet total of more than EUR 5 million and at least a ten-percent ownership share of the German parent and for all foreign affiliates with a balance sheet total of more than EUR 0,5 million and at least a 50-percent ownership.

The German MNEs may in turn be owned by foreign enterprises. The data provide information on affiliate employment, turnover, and balance sheet items.

Third, in order to link the two data sources on domestic and foreign activities, we use the commercial corporate structure database MARKUS (from Verband der Vereine Creditreform) which allows us to identify all domestic parents and affiliates of FDI-reporting firms. Multinational enterprises are also multi-firm enterprises in the home economy so that outward FDI affects workers beyond the FDI-reporting firm's workforce. Moreover, many German enterprises bundle the domestic management of their foreign affiliates into legally separate firms (mostly limited liability *GmbHs*) for apparent tax and liability reasons. Those bundling firms then report FDI to MIDI as required by German law. The economic impact of the reporting firm's FDI, however, goes beyond the firm's formal legal boundary in that jobs throughout the corporate group can be affected. We consider all firms within a corporate group (an enterprise) as potential FDI firms if at least one firm in the group reports outward FDI activities.

The three data sources do not share common firm identifiers. We employ a string-match procedure to identify clearly identical firms and their plants (see Appendix A for a detailed description).[9] We use the year $t = 2000$ as our base period because it is the earliest year for which we have firm structure information and can adequately attribute outward FDI exposure to domestic jobs. The linked data provide a cross-section of plants in year $t = 2000$, including a total of 39,681 treated and 1,133,920 control plants out of 3.8 million plants in the full worker sample (1998-2002). Treated plants are those whose firms show an employment expansion at their foreign affiliates. We use a 5% random sample of workers (93,140 job observations) to reduce estimation runtime to acceptable durations.[10]

Our worker sample is from $t = 2000$ and spans the preceding and subsequent year (from second-quarter BA files, considered most representative for the year at BA). We restrict the sample to workers at t who also hold a job at some employer at $t - 1 = 1999$. This allows us to condition estimation on past worker and job

[9] We lose observations when combining data from the three sources. But our sample exhibits largely similar characteristics to the universe. To check whether the MNE data remain representative, we compare main moments in our combined data to the MIDI universe. Average foreign employment (turnover) at MNEs in our sample is 2,635 (EUR 752,400) with a standard error of 116 (EUR 39,800), and is 2,281 (EUR 658,900) in the MIDI universe. Foreign employment at the tenth and ninetieth percentiles of the firm distribution in our sample is 12 and 9,911, and 4 and 9,911 in the MIDI universe. Because we sample at the job level, we naturally retain larger firms in our combined sample than in the MIDI universe. Note that the means reported here are somewhat lower than those displayed in Table 1. The reason is that Table 1 reports averages over workers, whereas the numbers discussed here are at the firm level.

[10] The statistical software package (Stata) at BuBa requires the full data matrix to be loaded into memory so that a runtime of days results when hard drives need to operate virtual memory.

characteristics. The resulting exclusion of workers with a non-employment spell twelve months earlier reduces observed separation rates both at MNEs and non-MNEs. So, the sample restriction also helps us focus our job-security analysis on workers with relatively stable employment trajectories. Most pretreatment characteristics vary little between $t-1$ and t, and we simplify the timing of pretreatment to be at t in some specifications. The treatment period (for changes to a job's FDI exposure) runs from $t-1 = 1999$ (foreign-affiliate balance-sheet closing dates in 1999) to t (closing dates in 2000). The outcome (a worker's retention or separation) is observed between t and $t+1 = 2001$. In the estimation sample, we only keep workers with continuous employment between t and $t+1 = 2001$.

We complement the micro-data with annual information on imports by source country and exports by destination country from the German Federal Statistical Office and aggregate intermediate-goods imports, final-goods imports, and exports to world regions by German sector at the *NACE* 2-digit level.[11]

Outcomes. Our outcome variable is an indicator of a worker's separation from job i. We denote the outcome with y_i. It takes a value of one if the holder of the job is displaced from the employing plant between years t and $t+1$ (note the one-year lead between outcome and treatment), and is zero otherwise. This measure of worker separation includes both quits and layoffs.[12] A change of occupation within the employing plant is not considered a separation.

Treatments. The natural counterpart to separation as a worker-level measure of the change in gross labor demand is the change in FDI exposure. We mostly focus on positive exposure changes, or FDI expansions. The binary treatment indicator d_i takes a value of one for a job i if the employing enterprise expands its FDI exposure between years $t-1$ and t, and zero otherwise. Our main measure of FDI exposure is employment in foreign affiliates. For robustness checks, we also use the increase in affiliate turnover (a discrete indicator) and the magnitude of foreign employment expansions (a continuous treatment variable; Hirano and Imbens (2004)).

Using ownership shares as weights, we attribute FDI exposure measures to related firms and their jobs within the corporate group (see Appendix B for details of the procedure). We compute *cumulated* and *consolidated* ownership shares for all German firms that are in the same corporate group with at least one FDI-reporting firm. Cumulating means adding all direct and indirect ownership shares of a parent

[11] We calculate intermediate-goods imports by foreign location using import shares in sector inputs as reported by the German Federal Statistical Office under the assumption that source-country frequencies are similar for intermediate-goods imports and final-goods imports.

[12] The German social-security records do not distinguish quits from layoffs.

firm in a given affiliate. Consolidation removes the degree of self-ownership (α) from affiliates, or intermediate firms between parents and affiliates, and rescales the ultimate ownership share of the parent to account for the increased control in partly self-owning affiliates or intermediate firms (with a factor of $1/(1-\alpha)$).

We compute worldwide affiliate employment (WW) as well as region-specific affiliate employments. For the region-specific measures, we define four main foreign regions (see Table 11), among them two high-wage and two low-wage locations: Asia-Pacific Developing countries (APD), Central and Eastern European countries (CEE), European Monetary Union participating countries (EMU),[13] and Overseas Industrialized countries (OIN). We omit other developing countries, non-EMU member countries in Western Europe, and Russia and the Central Asian countries to create more homogeneous individual locations. World-wide (WW) expansions, however, include all countries. When we consider region-specific treatments, our baseline control group includes jobs at firms that exhibit no expansion in the region but possibly an expansion in any other region.

Covariates. We use a rich set of covariates to conduct propensity-score matching. The covariates are: worker characteristics (age, gender, education, monthly wage);[14] job characteristics (part-time or temporary work, apprenticeship, minor employment, occupation);[15] domestic plant characteristics (workforce size, workforce composition by worker and job characteristics, an East-West indicator); parent-firm foreign activity (foreign affiliate employment and turnover in four world regions); as well as sector-level measures of German foreign trade. To control for plant-level differences in productivity, we also estimate the plant-fixed component in German wages from a Mincer (1974) regression with June 2000 workers and include the plant-specific measure among the pretreatment characteristics. To the extent that FDI exposure is the result of enterprise characteristics such as productivity or capital intensity, we condition on the enterprise's past FDI exposure to control for their FDI-relevant aspects.

Descriptive statistics. Table 1 displays summary statistics for our main sample of workers in the manufacturing sector, separately for MNE and non-MNE plants.

[13] Twelve EU member countries that participate in Euro area in 2001, excluding non-participating EMU signatories.

[14] For schooling, we report differences between workers with and without tertiary schooling (beyond university-qualifying *Abitur*). Tertiary schooled workers have a college degree or are certified professionals who completed professional training or an apprenticeship program instead of college.

[15] In contrast to part-time work under an open-ended contract, temporary work status includes working family members in agriculture, employees past retirement age with temporary contracts, and sporadically employed workers.

Table 1: DESCRIPTIVE STATISTICS: MNE AND NON-MNE SUBSAMPLES

	MNEs		non-MNEs	
	mean	s.d.	mean	s.d.
Outcome: Worker separation				
Indic.: Displaced between t and $t+1$	.14	.34	.18	.38
Treatment: FDI exposure and expansion				
Employment abroad in $t-1$ (1,000s)	3.99	6.10	.00	.00
Indic.: Empl. growth abroad $t-1$ to t	.64	.48	.02	.15
Worker-level variables				
Annual wage in EUR	35,317.8	11,611.6	26,847.8	13,872.2
Age	41.01	10.44	40.69	11.77
Female	.23	.42	.33	.47
White-collar worker	.44	.50	.38	.49
Tertiary schooling	.16	.37	.08	.28
Current apprentice	.02	.15	.04	.19
Part-time employed	.05	.21	.12	.33
Plant-level variables				
Employment at domestic plant	2,683.8	7,935.3	926.9	3,153.3
Indic.: Plant in East Germany	.09	.29	.10	.30
Number of observations	38,041		55,099	

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing plants. Indicator variables (Indic.) take a value of one if the described condition is satisfied.

Separation rates differ markedly across MNE plants and non-MNE plants. 14 percent of workers employed in both 1999 and 2000 separate from MNE plants between the years 2000 and 2001, compared to 18 percent of workers from non-MNE plants.

In contrast to public perception, separation rates are lower in MNE plants than in non-MNE plants in the majority of manufacturing sectors, independent of the region of foreign investment (see Table 12 in the Appendix for separation probabilities by sector and region). The only exceptions are the chemical industry, where worker separation is lower in non-MNE plants, and the non-electrical machinery, electronics and optical equipment sector where separation rates do not differ between MNE and non-MNE plants.

A German MNE with domestic manufacturing plants employs around 4,000 workers abroad on average. 64% of the workers in MNE plants are subject to a foreign employment expansion between the years 1999 and 2000, whereas only 2% of the workers in non-MNE plants see their employer become an MNE for the first time and expand abroad.

MNE plants differ from non-MNE plants in several further dimensions. Workers in MNE plants earn more, are more highly educated, more likely to be white-collar workers, and less likely to be part-time employed than workers in non-MNE plants. MNE plants are bigger on average than non-MNE plants. Median employment is 644 and 103 for MNE and non-MNE plants, respectively.

4 Estimates

We investigate the effect of FDI *expansions* abroad on worker separation in the MNE's home labor market, conditional on past levels of MNE activity. FDI expansions (positive changes to FDI exposure) are the natural counterpart to separation as a worker-level measure of changes in labor demand. We choose a research design that contrasts changes (in outcomes) with changes (in treatment), rather than levels with levels, to lend more credibility to the balancing assumptions on pre-treatment characteristics. Table 13 in the Appendix shows for individual manufacturing sectors that separation probabilities from jobs exposed to FDI expansions are around two to five percent lower than from jobs not exposed to FDI expansions—similar to the unconditional four-percent difference between MNE and non-MNE status (Table 1).

We first estimate the propensity of FDI treatment using worker, job, plant, MNE and sector characteristics. The economic idea is to assign a propensity score to every job observation for subsequent comparison between jobs that were treated and observably identical jobs that were not treated. We provide evidence that propensity-score matching balances indeed the treated and control job sub-samples. Our comprehensive set of predictors covers relevant pre-treatment dimensions so that remaining differences are arguably random in nature. We then obtain ATT estimates of FDI expansions region by region, using nearest-neighbor matching based on the predicted propensity scores.

4.1 Propensity score estimation

The dependent variable in propensity score estimation is the binary indicator of an FDI expansion in region ℓ between 1999 and 2000. We start by looking at an indicator of at least one expansion in any foreign region (a worldwide expansion $\ell = WW$) and then discern region-specific expansions ($\ell =$*APD, CEE, EMU,* *OIN*). All our specifications control for current FDI exposure (the employment level in four world regions).

Table 2 displays odds ratios and corresponding standard errors of logit propensity score estimates for WW expansions (expansions anywhere worldwide). An odds ratio of one corresponds to no effect. Our basic *specification 1* (in columns 1 and 2 of Table 2) includes only worker characteristics alongside the FDI presence controls. We use worker characteristics from June 2000 to start (and add lagged worker characteristics for 1999 in specification 4). With the exception of age, all worker characteristics are significant predictors of FDI expansion in this short regression. Conditional on other worker and job characteristics, workers with higher wages, females and workers in non-standard forms of employment (minor employment, temporary job, apprentice position, part-time job) are more likely to be subject to FDI expansions.

In *specification 2*, we add plant characteristics (columns 3 and 4 of Table 2). All worker and job characteristics turn insignificant once plant variables are included. The loss of predictive power at the job level is consistent with the hypothesis that FDI expansions are not systematically related to workers or jobs, but based on separate decisions. This lends particular credibility to propensity-score matching. Lacking significance of job-level covariates is not an indication that higher levels of aggregation would satisfy requirements for propensity-sore matching. At the employer level, for instance, decision-making and treated unit would no longer be separate and subject to potential confounding of treatment and outcome. Among the plant variables is a plant-fixed wage effect from a Mincer regression to control for plant-level differences in labor productivity, which theory suggests to be a factor for selection into foreign expansions (e.g. Helpman, Melitz and Yeaple 2004).

We estimate propensity scores under two further specifications but find results to change little. *Specification 3* (not reported) adds three types of sector-level controls of foreign trade: imports of intermediate inputs, imports of final goods, and exports. In addition to the covariates from all prior specifications, *specification 4* also includes lagged wages and lagged plant information.[16] In those specifications, worker- and job-level controls remain insignificant and coefficients on plant-level covariates change little, also remaining significant.

In summary, plant, MNE and sector characteristics are significant and economically important covariates of FDI expansions, both for worldwide and region-specific FDI expansions. This shows that FDI expansions themselves are not random but a choice predictable by plant, MNE and sector characteristics. For we use a comprehensive set of worker, job, plant, MNE and sector variables, an arguably

[16]We include the worker's lagged wage in any prior job and do not restrict the sample to workers with two consecutive years of employment at the same plant.

Table 2: PROPENSITY SCORE SPECIFICATIONS

	Specification 1		Specification 2		Specification 4	
	Odds Ratio	Std. Err.	Odds Ratio	Std. Err.	Odds Ratio	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)
Worker-level variables						
Age	.994	.006	1.005	.006	1.011	.006
Age-squared	1.003	.007	.994	.007	.987	.007
Log Wage	4.980	.149***	1.039	.040	1.073	.059
Indic.: Female	1.242	.027***	1.027	.024	1.022	.025
Indic.: Tertiary schooling	1.097	.028***	.969	.027	.958	.027
Job-level variables						
Indic.: White-collar job	.748	.015***	1.016	.023	.995	.023
Indic.: Minor employment	4.967	.433***	1.215	.124	1.237	.128
Indic.: Temporary job	1.838	.154***	1.095	.098	1.024	.095
Indic.: Apprentice	2.584	.260***	.972	.107	1.086	.123
Indic.: Part-time job	1.549	.067***	1.005	.048	1.029	.050
Plant-level variables						
Employment			1.000	1.60e-06***	1.000	4.82e-06***
Average workforce age			.983	.003***	.986	.003***
Log wage FE			2.743	.491***	2.560	.460***
Annual average wage			1.001	.00008***	1.001	.00008***
Share: Females			1.353	.100***	1.340	.109***
Share: Tertiary schooling			1.216	.132*	1.525	.175***
Share: White-collar jobs			.548	.045***	.877	.074*
Share: Minor employments			.464	.098***	.379	.081***
Share: Temporary jobs			1.395	.600	12.946	5.540
Share: Apprentices			.033	.016***	.005	.002***
Share: Part-time jobs			.454	.074***	.536	.087***
Indic.: in East Germany			2.183	.086***	2.107	.086***
Const.	1.60e-06	3.93e-07***	.056	.020***	.050	.020***
Sector controls					yes	yes
Lagged predictors					yes	yes
Obs.	93,140		93,140		93,140	
Pseudo R^2	.069		.135		.165	

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing plants. Controlling for lagged levels of MNE employment in all world regions. Standard errors: * significance at ten, ** five, *** one percent.

considerable part of the unexplained variation in treatment probabilities is likely due to unobserved variations in host location characteristics. There is no evidence that FDI expansions are systematically related to workers or jobs. This lends additional support to the tenet that matching pairs of treated and control jobs by propensity score provides us with comparable samples for inference. Consequently, we discard specification 1, which included only worker and job variables.

4.2 Covariate balancing

Based on the estimated propensity score, we use nearest-neighbor matching to combine treated and control observations.[17] As Table 3 shows, our sample contains 15,000 to 25,000 treated jobs and 65,000 to 75,000 matched control jobs (columns 1 and 2), depending on region of expansion and specification. So, before matching, treated jobs make up between 15 and 25 percent of the estimation sample (column 3).

Covariate balancing assesses matching quality. Table 3 shows matching quality indicators for specifications 2, 3 and 4 by region of foreign expansion. Our first matching statistic, the pseudo R^2 from logit estimation of the conditional probability of FDI expansion, indicates the degree to which regressors $\mathbf{x}_i$ predict the treatment probability (columns 4 and 5). After matching, regressors $\mathbf{x}_i$ should have no explanatory power for selection into treatment if the treatment and matched control samples have balanced characteristics. Our results show that this is the case. The pseudo R^2 statistics drop from between 13 and 28 percent to between 2 and 7 percent.

Rosenbaum and Rubin (1985) propose a comparison between (standardized) treated unit means and (standardized) control unit means before and after matching as a second evaluation method for covariate balance. The standardized differences (standardized biases) between the means for a covariate $\mathbf{x}_i$ are defined as:

$$\begin{aligned} B_{\textit{before}}(\mathbf{x}_i) &= 100 \cdot \frac{\bar{\mathbf{x}}_{i1} - \bar{\mathbf{x}}_{i0}}{\sqrt{V_1(\mathbf{x}_i) + V_2(\mathbf{x}_i)/2}} \\ B_{\textit{after}}(\mathbf{x}_i) &= 100 \cdot \frac{\bar{\mathbf{x}}_{i1M} - \bar{\mathbf{x}}_{i0M}}{\sqrt{V_1(\mathbf{x}_i) + V_2(\mathbf{x}_i)/2}}, \end{aligned}$$

where $\bar{\mathbf{x}}_{i1}$ denotes the treated unit mean and $\bar{\mathbf{x}}_{i0}$ the control unit mean for covariate $\mathbf{x}_i$. As is commonly done in the evaluation literature, we show the median absolute

[17] We use a version of Edwin Leuven and Barbara Sianesi's Stata module *psmatch2* (2003, version 3.0.0, *http://ideas.repec.org/c/boc/bocode/s432001.html*) to perform propensity-score matching and covariate balance testing.

Table 3: COVARIATE BALANCING, BEFORE AND AFTER MATCHING

	No. of treated	No. of controls	Share of treated before	Logit ps. R^2 before	Logit ps. R^2 after	Median bias before	Median bias after	Share of treated lost
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Specification 2: Worker and plant characteristics								
WW	25,640	67,500	.275	.131	.035	18.306	2.637	.00004
APD	14,643	78,497	.157	.195	.051	17.481	3.049	.002
CEE	18,914	74,226	.203	.147	.052	13.570	5.180	.0005
EMU	21,759	71,381	.234	.174	.055	19.583	3.412	.000
OIN	17,974	75,166	.193	.240	.055	16.878	5.652	.000
Specification 3: Spec. 2 plus sector-level trade measures								
WW	25,640	67,500	.275	.159	.031	18.742	3.682	.0002
APD	14,643	78,497	.157	.231	.021	25.274	2.935	.066
CEE	18,914	74,226	.203	.179	.059	18.648	6.692	.002
EMU	21,759	71,381	.234	.205	.036	20.926	3.272	.0002
OIN	17,974	75,166	.193	.280	.058	25.014	5.912	.000
Specification 4: Spec. 3 plus lagged wage and lagged plant size								
WW	25,640	67,500	.275	.162	.037	19.262	3.608	.0001
APD	14,643	78,497	.157	.232	.067	25.580	3.092	.003
CEE	18,914	74,226	.203	.180	.064	20.115	4.766	.002
EMU	21,759	71,381	.234	.205	.038	22.389	2.922	.0002
OIN	17,974	75,166	.193	.284	.075	26.703	6.327	.001

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI-exposed and non-FDI exposed manufacturing plants. Locations (see Table 11): WW (World-Wide abroad), APD (Asia-Pacific Developing countries), CEE (Central and Eastern European countries), EMU (European Monetary Union member countries), and OIN (Overseas Industrialized countries).

standardized bias before ($B_{before}(\mathbf{x}_i)$) and after matching ($B_{after}(\mathbf{x}_i)$), over all regressors $\mathbf{x}_i$ that enter the propensity score estimation (columns 6 and 7). Across regions of treatment and specifications, matching reduces the median absolute standardized bias by 70 to 90 percent. There seem to be no formal criteria in the literature to judge the size of standardized bias. Yet the remaining bias between 2 and 7 percent is in the same range as in microeconomic evaluation studies (e.g. Lechner (2002) and Sianesi (2004)).[18] In the post-matching standardized difference only treated units enter whose values fall within the common support with the control units. We impose a strict caliper of 1% to discard treated units outside the common support, but the share of treated observations outside the common support is miniscule (column 8).

There is no single specification with a bias consistently lower than that of other specifications for all regions. Other balancing statistics, such as those based on goodness-of-fit measures (Heckman, Ichimura and Todd (1997), for instance), tend to favor richer specifications over more parsimonious specifications. Heckman and

[18]Rosenbaum and Rubin (1985) suggest that a value of 20 is "large."

Table 4: AVERAGE TREATMENT EFFECT ON THE TREATED

	OLS	ATT		
		Spec. 2 worker & plant predictors	Spec. 3 *adding* sector predictors to (2)	Spec. 4 *adding* lagged predictors to (3)
	(1)	(2)	(3)	(4)
WW	-.045 (.003)***	-.021 (.010)**	-.014 (.012)	-.026 (.009)***
APD	-.043 (.003)***	-.007 (.018)	-.019 (.007)***	-.069 (.018)***
CEE	-.045 (.003)***	-.027 (.012)**	-.019 (.013)	-.068 (.017)***
EMU	-.043 (.003)***	-.031 (.009)***	-.022 (.009)**	-.007 (.011)
OIN	-.035 (.003)***	-.039 (.012)***	-.002 (.013)	-.056 (.018)***

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing plants. Controlling for lagged levels of MNE employment in all world regions. Analytic standard errors in parentheses: * significance at ten, ** five, *** one percent.

Navarro Lozano (2004) show, however, that adding variables that are statistically significant in the treatment choice equation does not necessarily result in a set of conditioning variables that satisfy the unconfoundedness assumption. We therefore do not select a single specification of the propensity score based on goodness-of-fit measures. Instead, we compare results from specifications 2, 3 and 4.

Overall, observable characteristics between treated and control observations are well balanced after propensity-score matching. To test the sensitivity of our results with respect to hypothetical unobserved influences, we will use Rosenbaum (2002) bounds after ATT estimation.

4.3 Average treatment effect on the treated

Having formed a matched sample of treated and control jobs, we estimate the ATT. Table 4 contrasts the results from propensity-score specifications 2 through 4 with ordinary least squares (OLS) estimates of the treatment effect. We report analytic standard errors.

Across specifications, the ATT estimate for an expansion in affiliate employment anywhere worldwide ranges between -.014 and -.023 percent. So, worldwide employment expansions reduce the probability of domestic worker separation by about 2 percentage points, or around half of the difference of 4 percentage points that OLS estimation detects (columns 1) and that we also found in unconditional differences between MNEs and non-MNEs (Table 1). We attribute the identified two-percent difference from propensity-score estimation to the foreign employment expansion itself.

We separate the ATT by region of foreign expansion to discern contributing expansions behind the measured worldwide ATT effect. The region-specific ATT estimates are again negative in all four cases. In specifications 2 (worker and plant predictors of treatment only) and 3 (sector predictors in addition to worker and plant variables), all estimated treatment effects are negative, though not always statistically significant. Although specifications 2 and 3 exhibited more favorable balancing properties than specification 4 for some regions, we regard the richest specification 4 to be our chief one. In specification 4, we keep sector predictors of treatment as in specification 3 but add lagged covariates from specification 2. Except for EMU, point estimates are overall higher than in either prior specification. This is consistent with the hypothesis that the domestic-worker retention effect of FDI expansions may be underestimated when not controlling for past determinants of plant performance.[19]

In the richest specification 4, ATT point estimates for APD, CEE and OIN exceed the OLS estimates in absolute value. So, when controlling for a possibly large set of treatment predictors, the detected ATT is even stronger than the unconditional difference in separation rates between expanding and non-expanding MNEs would suggest. This lends additional support to the hypothesis that it is the foreign employment expansion itself which contributes to reduced domestic separation rates.

Expansions into low-income economies are sometimes associated with vertical FDI, while those into high-income locations are considered to be more likely horizontal. Our estimates do not suggest a clear distinction by host income levels, while specification 4 points to a weaker effect of expansions into neighboring locations in the EMU and a stronger effect for expansions into distant locations. Interestingly, expansions into low-wage regions like Central and Eastern Europe (CEE) and remote high-wage locations such as OIN (including, Japan, the U.S. and Canada) predict treatment effects of similar magnitude. This is consistent with the hypothesis that, while horizontal expansion motives may outweigh factor-cost savings motives in some regions and not others, the performance effect on home separation rates is similar. It is also consistent with findings by Feinberg and Keane (2006) that MNEs simultaneously pursue horizontal and vertical investment strategies in the same host

[19] In our regional specifications, firms that do not expand into region ℓ are classified as controls but may expand in any other region. When we fix the control group to jobs only at firms who do not expand anywhere (the control group of the WW estimator), all point estimates continue to be negative and similar in magnitude but lose significance in some regions.

location. It is conceivable that performance gains of expansions within the highly integrated Euro area are small compared to non-expansions so that one might expect lacking significance for effects of EMU expansions. The ATT for expansions into EMU countries is indeed not statistically significant.

To summarize, in no single specification and for no single region is there a positive treatment effect. Our estimates invariably point towards reduced domestic-worker separation rates at foreign-employment expanding MNEs relative to non-expanding firms. An important branch of the prior literature uses simultaneous factor demand models, motivated by cost-function estimation, to assess the own-wage and cross-wage substitution elasticities for labor demand across regions—measuring factor-price effects while conditioning on output, as cost function estimation requires. In conditioning on current output, however, cost-function estimation precludes firm performance, as manifested by firm product market shares for instance, from affecting labor demand. The research design of the current study is guided by the complementary question, whether foreign expansions alter firm performance in the home labor market. Though we condition on pre-treatment characteristics of workers and plants (at $t-1$), we do not restrict the outcome between t and $t+1$ in any way. Given the factor-cost and product market environment across foreign locations, in which globally competing firms operate, MNEs that expand abroad retain more workers at home.

4.4 Worker and job heterogeneity

Employment expansions at MNEs abroad may affect workers and jobs differentially depending on their skill level. We use the job-level data to distinguish between education groups of workers and between jobs by skill intensity. Results show that FDI expansions in any foreign location increase domestic-worker retention rates for both education groups and for both job types—with no single statistically significant exception.[20]

Table 5 presents results for workers with and without tertiary education (beyond university-qualifying *Abitur*). Especially in specifications 2 and 3, worker-retention effects are typically stronger for workers with a tertiary schooling degree than for workers with less education. In our richest specification 4, we find FDI expansions anywhere worldwide to reduce separation rates by 11.9 percentage points for domestic workers with tertiary schooling but by only 2.7 percentage points for workers with less education. Employment expansions in EMU participants have no significant effect in specification 4.

[20]Observations for workers with tertiary education, or skill-intensive occupations, are more likely to be truncated at the upper-income ceiling than those for other education and skill groups.

Table 5: ATT, HIGH AND LOW EDUCATIONAL ATTAINMENT

	OLS	ATT		
		Spec. 2 worker & plant predictors	Spec. 3 *adding* sector predictors to (2)	Spec. 4 *adding* lagged predictors to (3)
	(1)	(2)	(3)	(4)
		WORKERS WITH TERTIARY EDUCATION		
WW	-.045 (.007)***	-.029 (.032)	-.071 (.016)***	-.119 (.033)***
APD	-.034 (.008)***	-.076 (.020)***	.002 (.043)	-.008 (.046)
CEE	-.048 (.008)***	-.118 (.040)***	-.144 (.040)***	-.057 (.041)
EMU	-.029 (.008)***	-.068 (.026)**	-.095 (.031)***	-.004 (.034)
OIN	-.025 (.008)***	-.046 (.027)*	-.122 (.041)***	-.018 (.041)
		WORKERS WITH LESS THAN TERTIARY EDUCATION		
WW	-.045 (.003)***	-.019 (.006)***	-.028 (.006)***	-.027 (.010)***
APD	-.045 (.004)***	-.060 (.018)***	-.023 (.018)	-.021 (.018)
CEE	-.046 (.003)***	-.019 (.011)*	-.029 (.016)*	-.027 (.013)**
EMU	-.047 (.003)***	-.023 (.008)***	-.006 (.011)	-.013 (.009)
OIN	-.038 (.003)***	-.028 (.010)***	-.039 (.011)***	-.041 (.016)***

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing plants. Controlling for lagged levels of MNE employment in all world regions. Analytic standard errors in parentheses: * significance at ten, ** five, *** one percent. Number of observations: 10,652 workers with tertiary schooling and 82,488 workers with less than tertiary education.

Table 6 repeats the exercise with a distinction between white-collar and blue-collar jobs. Interestingly, white-collar jobs exhibit hardly any statistically significant ATT. Though worker-retention effects of foreign employment expansions are significant for blue-collar workers, we find no clear differences in the ATT point estimates. The job-securing effect of foreign employment expansions appears to be shared across occupations.

5 Robustness Checks

Propensity-score estimation of the ATT suggests that expansions abroad lead to more frequent worker retentions at home. We argue that the most plausible explanation for added job security at FDI-expanding firms is indeed the FDI expansion

Table 6: ATT, WHITE-COLLAR AND BLUE-COLLAR WORKERS

	OLS	ATT Spec. 2 worker & plant predictors	ATT Spec. 3 *adding* sector predictors to (2)	ATT Spec. 4 *adding* lagged predictors to (3)
	(1)	(2)	(3)	(4)
		WHITE-COLLAR WORKERS		
WW	-.045	-.041	-.051	-.022
	(.004)***	(.019)**	(.019)***	(.024)
APD	-.041	-.042	-.018	-.012
	(.005)***	(.021)*	(.027)	(.043)
CEE	-.049	-.022	-.023	-.026
	(.005)***	(.024)	(.034)	(.025)
EMU	-.036	-.026	-.021	-.011
	(.004)***	(.019)	(.020)	(.016)
OIN	-.036	-.017	-.020	-.023
	(.005)***	(.026)	(.019)	(.022)
		BLUE-COLLAR WORKERS		
WW	-.045	-.016	-.035	-.023
	(.004)***	(.006)***	(.006)***	(.006)***
APD	-.045	-.008	-.021	-.022
	(.005)***	(.009)	(.009)**	(.009)**
CEE	-.044	-.017	-.011	-.009
	(.004)***	(.007)**	(.008)	(.008)
EMU	-.051	-.044	-.037	-.037
	(.004)***	(.009)***	(.008)***	(.008)***
OIN	-.036	-.010	.004	.007
	(.004)***	(.011)	(.012)	(.013)

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing plants. Controlling for lagged levels of MNE employment in all world regions. Analytic standard errors in parentheses: * significance at ten, ** five, *** one percent. Number of observations: 37,977 white-collar and 55,163 blue-collar workers.

itself. To make the case, we investigate main competing hypotheses that might give rise to a similar worker-retention pattern of FDI expansions, and find those competing hypotheses to be considerably less plausible.

MNEs arguably possess ownership advantages, such as innovative processes or products, prior to FDI expansions. A pre-existing advantage manifests itself in observables, however, such as prior FDI or higher labor productivity, and we control for a possibly large set of such predictors in Section 4. In this Section, we perform a series of robustness checks to investigate two more critical competing hypotheses: First, firms might acquire an ownership advantage or experience a favorable demand or productivity shock and simultaneously expand FDI, but retain more domestic workers because of the newly acquired ownership advantage. Second, simultaneous sector-wide changes, such as trends in foreign trade, may affect FDI-exposed enterprises differently from domestic firms but be unrelated to FDI expansions and incidentally retain more domestic workers. We quantify the potential influence of hidden bias (violations of the assumption of selection on observables) to assess the plausibility of the former competing hypothesis, and we check for concomitant variables to assess the plausibility of the latter competing hypothesis.

5.1 Sensitivity analysis with Rosenbaum bounds

Our first robustness check probes the plausibility of the competing hypothesis that unobserved confounding factors lead us to erroneously attribute additional worker retentions to foreign expansions. An unobserved confounding factor could be that firms acquire an ownership advantage over the course of the treatment year or experience a favorable productivity or demand shock and therefore retain more domestic workers, simultaneously expanding FDI. We use Rosenbaum (2002) bounds to estimate how large the effect of a hypothetically unobserved confounding factor would have to be to overturn our ATT estimate (see Appendix C for a detailed description).

Note that for an unobserved variable to be a source of selection bias, it must affect the probability that a job receives the treatment and must affect the outcome. In particular, an unobserved variable that differentially affects subgroups of jobs in the treatment group, but that does not have an effect on the outcome beyond the variables already controlled for, does not challenge the robustness of our results. Examples of such variables are economic changes or political reforms at the MNE's host locations, exchange rate moves, or varying trade costs. Only if groups of jobs differ on unobserved variables that simultaneously affect the assignment to treatment and the outcome, a hidden bias may arise on unobserved heterogeneity. We want to determine how strongly a hypothetically unobserved job-related variable would have to be to influence the selection process so that it could undermine the results of our matching analysis.

We perform a sensitivity analysis for all statistically significant ATT effects. For this purpose, we gradually increase the level of the critical value of the odds ratio where inference about the treatment effect starts to be overturned. We find that the critical value, for which the statistically significant ATT effects in Table 4 would become statistically indistinguishable from zero, varies between 1.15 and 1.25. Consider the effect of employment expansions in CEE under specification 4, for instance. We find the critical odds-ratio value to be 1.25. This means that all jobs with the same observed x-vector can differ in their odds of treatment by a factor of up to 1.25, or 25 percent, before the confidence band around the ATT estimate starts to include zero. This is a worst-case scenario. A critical value of 1.25 does not imply that there is indeed unobserved heterogeneity or that there is no effect of treatment on the outcome variable. A critical value of 1.25 only means that a hypothetical unobserved variable, such as a newly acquired ownership advantage or a favorable demand or productivity shock, would need to have an odds ratio of 1.25 to completely determine the outcome for the matched job pairs and overturn our ATT estimate.

Table 2 gives an idea of what an odds ratio of 1.25 on a hypothetical binary variable compares to. The coefficient on the fraction of workers with tertiary schooling in the plant's workforce is 1.216 (column 2 of Table 2). An unobserved effect challenging our conclusions would thus have to be stronger than the effect of raising the share of tertiary schooled workers from zero to 100 percent in the mean plant's workforce. We consider it implausible that a newly acquired ownership advantage, or any other favorable productivity or demand shock outside our rich list of regressors, would exert such strong an impact. We therefore view the statistically significant ATT treatment effects as robust to hidden bias.

5.2 Concomitant variables

Our second robustness check queries whether changes in foreign trade are concomitant predictors that incidentally covary with the treatment so that we would erroneously attribute FDI effects to the ATT. To gauge the effect of concomitant trade variables, we take the matched job sample and regress the outcome on the treatment indicator in the matched sample. This gives an ATT estimate (Rosenbaum 1984). We add to this regression 21 variables on sector-level changes in intermediate-goods imports, final-goods imports, and exports between t and $t+1$, separately for seven world regions. To exhaustively reflect German foreign trade, we include among the concomitant variables regressors for Other Developing countries (ODV), Other Western European countries (OWE) and Russia and Central Asian countries (RCA), beyond the four regions APD, CEE, EMU and OIN. In addition, we include an indicator whether the plant's firm was an MNE in the preceding period. Table 7 reports the results of this exercise for foreign-employment expansions anywhere worldwide under specification 4. Only one of the coefficients on the concomitant sector variables is statistically different from zero at the five-percent level. (Table 7 does not report the insignificant coefficient estimates for ODV, OWE and RCA.) Past MNE status of the plant's firm is not a significant predictor.[21] The most plausible explanation for lower separation rates at FDI-expanding firms is thus their FDI expansion itself.

[21] Note that OLS likely overstates significance. OLS replication regressions systematically understate standard errors. Whereas ATT standard errors take into account the repeated use of matched control observations, OLS regressions treat them as if they were independent observations. This is apparent from the standard error of .009 in Table 4 (column 4) with a standard error of .003 in Table 7 (column 1). The literature does not yet provide methods to adequately adjust standard errors in OLS replication regressions with controls.

Table 7: CONCOMITANT VARIABLES

	ATT repl. regression	ATT with controls 1	ATT with controls 2
	(1)	(2)	(3)
WW treatment effect	-.029	-.026	-.028
	(.003)***	(.003)***	(.005)***
Change of intermediate-goods imports 2000-01 from region			
APC		-.009	-.008
		(.023)	(.023)
CEE		.010	.007
		(.070)	(.070)
EMU		-.027	-.027
		(.015)*	(.015)*
OIN		-.021	-.023
		(.084)	(.084)
Change of final-goods imports 2000-01 from region			
APC		.007	.007
		(.003)**	(.003)**
CEE		.004	.004
		(.009)	(.009)
EMU		.009	.010
		(.015)	(.015)
OIN		.016	.016
		(.021)	(.021)
Change of exports 2000-01 to region			
APC		-.027	-.026
		(.020)	(.020)
CEE		-.003	-.006
		(.077)	(.077)
EMU		-.001	-.002
		(.015)	(.015)
OIN		-.017	-.017
		(.017)	(.017)
Indic.: MNE plant $(t-1)$			.004
			(.005)
Obs.	41,107	41,107	41,107

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing plants. Coefficient estimates for ODV, OWE and RCA not reported (all insignificant). Analytic standard errors in parentheses: * significance at ten, ** five, *** one percent. Regression on matched sample, including a constant. Changes in imports and exports at *NACE* 2-digit sector level.

Table 8: ROBUSTNESS OF ATT

	OLS	ATT		
		Spec. 2 worker & plant predictors	Spec. 3 *adding* sector predictors to (2)	Spec. 4 *adding* lagged predictors to (3)
	(1)	(2)	(3)	(4)
Main sample (from Table 4)				
1. WW	-.045	-.021	-.014	-.026
	(.003)***	(.010)**	(.012)	(.009)***
Alternative random sample				
2. WW	-.042	-.020	-.039	-.040
	(.003)***	(.010)*	(.011)***	(.010)***
Restricted control group: non-expanding MNEs				
3. WW	-.020	-.023	-.029	-.038
	(.004)***	(.016)	(.016)*	(.010)***
Alternative treatment: foreign turnover expansion				
4. WW	-.042	-.067	-.065	-.038
	(.003)***	(.011)***	(.012)***	(.011)***
Converse treatment: foreign employment contraction				
5. WW	-.011	.001	.005	.012
	(.004)***	(.005)	(.005)	(.005)**

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing plants. Controlling for lagged levels of MNE employment in all world regions. Analytic standard errors in parentheses: * significance at ten, ** five, *** one percent. Treatment is foreign employment expansion unless otherwise noted, control group includes both MNEs with no expansion and non-MNEs unless otherwise noted.

5.3 Additional robustness checks

We perform several additional robustness checks under alternative sample, control-group and treatment definitions to corroborate the plausibility of our hypothesis that foreign FDI expansions raise the retention rate of workers at home.

Alternative random sample. In drawing the five-percent worker sample and conducting propensity score matching, we restrict ourselves to a single random seed in all routines. We tried alternative seeds. In Table 8 (row 2), we report results from the alternative random seed that resulted in point estimates farthest from our original result in absolute value for specification 4. Estimates across all specifications reinforce our main findings.

Restricting the control group to non-expanding MNEs. The control group includes jobs at non-MNEs. A concern is that non-MNE jobs make an unreasonable comparison group to jobs at expanding MNEs. Although the propensity score estimator is not likely to pair-up jobs with strongly different attributes as nearest neighbors, the inclusion of non-MNE jobs in the control group might affect estimates. To assess the importance of non-MNE jobs in the control group, we exclude all non-MNE jobs and restrict the control group to workers at non-expanding MNEs. We report results in Table 8 (row 3). Interestingly, the OLS estimate now drops to the ATT level, while point estimates across all ATT specifications reinforce our main findings.

Foreign turnover expansion as treatment. Measuring the treatment with an FDI expansion in terms of foreign employment buildups is natural in our context where the outcome is domestic worker retention or separation. Turnover at foreign affiliates is an alternative treatment variable. We repeat the full propensity-score matching procedure and subsequent ATT estimation, now defining treatment as an increase in foreign-affiliate turnover. Results in Table 8 (row 4) show that all point estimates continue to be negative. Under specification 4, turnover expansions anywhere worldwide (WW) reduce the separation rate of domestic workers by 3.8 percentage points. This ATT is considerably stronger than the benchmark estimate of 2.9 percent (row 1).

Foreign employment contraction as treatment. We investigate the converse treatment: firms that contract foreign employment. As Table 4 (row 5) shows, OLS estimates would indicate that firms with foreign employment contractions retain fewer workers than firms with expansions, but that, surprisingly, contracting MNEs add to job security security compared to non-contracting MNEs and non-MNEs. Propensity score estimation, in contrast, demonstrates that foreign employment contractions make domestic jobs less, not more, secure. Specification 4 predicts that a foreign employment contraction is associated with a 1.2 percent higher rate of worker displacements from domestic jobs.

Alternative treatment thresholds. We investigate the sensitivity of our ATT estimates to the choice of cutoff for a foreign employment expansion. In the baseline definition of a foreign expansion so far, we consider an employment buildup of any magnitude. We now turn to alternative expansion thresholds that redefine the outward-FDI treatment increasingly restrictively with 1 percent, 5 percent, and 10 percent foreign employment expansions. We re-estimate specification 4 under those redefined treatments. As Table 9 shows, we find overwhelmingly robust point estimates. The ATT estimates are most frequently statistically significant when considering more-than-five-percent employment expansions as treatment. For the

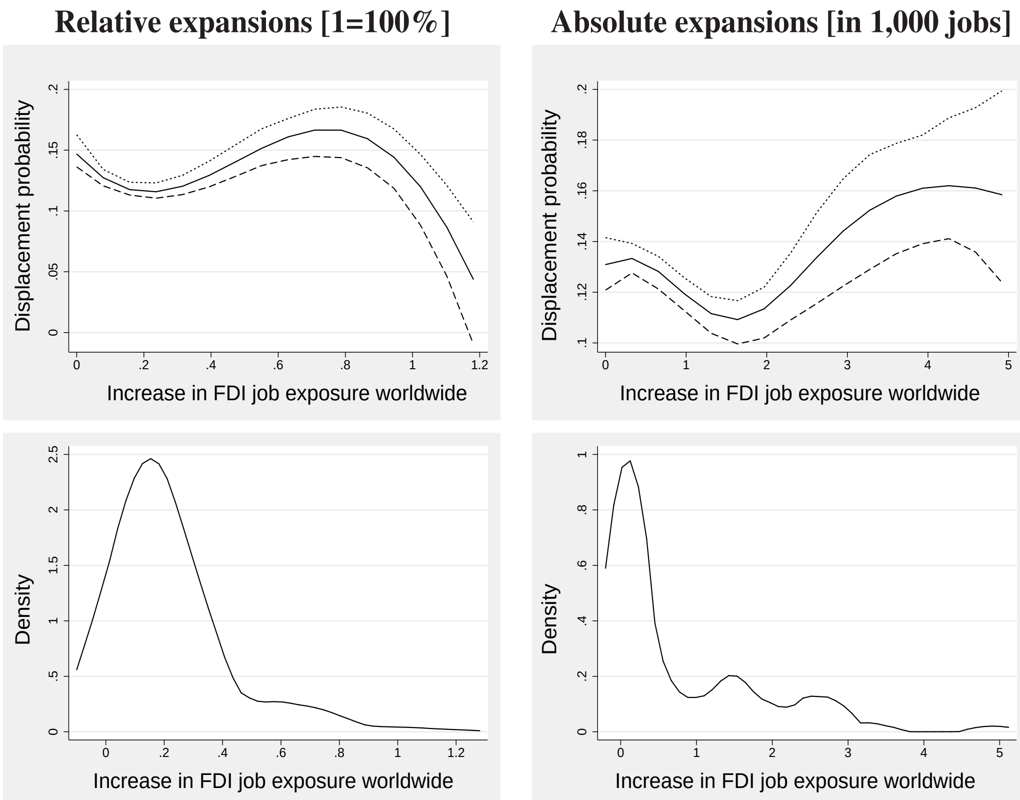
Table 9: ATT FOR VARYING EMPLOYMENT EXPANSION THRESHOLDS

	OLS (1)	Std. Err. (2)	ATT (3)	Std. Err. (4)
Treatment: Employment expansion > 1 percent				
WW	-.044	.003***	-.021	.014
APD	-.043	.003***	-.017	.023
CEE	-.046	.003***	-.067	.017***
EMU	-.042	.003***	-.031	.012**
OIN	-.035	.003***	-.014	.012
Treatment: Employment expansion > 5 percent				
WW	-.043	.003***	-.024	.005***
APD	-.043	.003***	-.011	.018
CEE	-.046	.003***	-.043	.019**
EMU	-.041	.003***	-.040	.012***
OIN	-.035	.003***	-.068	.015***
Treatment: Employment expansion > 10 percent				
WW	-.045	.003***	-.018	.014
APD	-.040	.004***	-.019	.026
CEE	-.046	.003***	-.024	.018
EMU	-.047	.003***	-.018	.023
OIN	-.025	.003***	-.013	.007*

Results for specification 4.
Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing plants. Controlling for lagged levels of MNE employment in all world regions. Analytic standard errors in parentheses: * significance at ten, ** five, *** one percent. Treatment is foreign employment expansion, control group includes both MNEs with no expansion and non-MNEs.

main treatment measure of foreign expansions anywhere, there are at most slight changes to the ATT estimate within typical confidence bands. This result is consistent with the idea that the foreign expansion itself is the strongest explanatory factor for reduced separation rates, regardless of the magnitude of the expansion.

Continuous treatment variables. In a final assessment, we consider continuous treatments and estimate their effects. Binary indicators of foreign expansions, even when defined for alternative treatment thresholds, may conceal varying effects of different magnitudes of foreign expansions. We consider two definitions of treatment: relative foreign-employment expansions (measured as a percentage increase of the foreign workforce over the preceding year) and foreign-employment buildups



Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing plants. Hirano and Imbens (2004) dose-response functions (upper panels), with analytic confidence bands at the 5-percent level. Densities of treatment variable (lower panels) based on Epanechnikov kernel with bandwidth .1.

Figure 1: **Responses to Foreign Employment Expansions**

in absolute terms (counted in thousands of jobs). Hirano and Imbens (2004) derive an extension of the Rosenbaum and Rubin (1983) propensity-score method to estimate the average local effects of continuous treatments. We discuss this relatively recent method, known as generalized propensity score matching, in Appendix D.

The estimates from generalized propensity score matching are typically reported with a so-called dose-response function. In our case, the dose-response function depicts the displacement probability as a function of the absolute or relative employment increase abroad. Figure 1 plots the dose-response functions (upper panels), together with standard kernel densities (lower panels, using an Epanechnikov kernel and a bandwidth of .1).

The densities in the lower panels document that the bulk of foreign expansions occurs for worldwide employment buildups of less than 40 percent and less than 1,500 foreign workers. In this range, foreign employment expansions reduce domestic displacement probabilities and make home jobs safer. There is a point of inflection at foreign expansions of 20 percent or more (1,500 foreign workers or more), however, from which on the probability of home-job losses increases and ultimately surpasses the estimated job-loss probability for zero expansions. Those major expansions are, however, rare, as the kernel density estimates show. Even in this upper range range of foreign expansions, the domestic displacement probability is estimated to reach at most 16%, compared to 18% for non-MNEs.

6 Conclusion

Are home jobs safer when MNEs expand abroad than when they do not? We use a propensity-score matching method for various measures of a domestic job's exposure to parent-firm FDI and find that FDI expansions into foreign regions significantly decrease the probability of domestic worker separation. MNEs' employment expansions abroad reduce the rate of domestic job loss by about two percentage points—or half the unconditional difference in separation rates between MNEs (with lower separation rates) and non-MNEs. Much of the previous literature looks at factor use within MNEs to ask how international wage differences affect MNEs' labor demands. There is evidence in favor of the hypothesis that labor is a substitute across locations within MNEs, just as labor is a substitute under cross-border trade between firms. Our MNE and non-MNE comparison documents, however, that MNEs are able to respond to global competition with more stable home employment policies than non-MNEs.

We perform numerous sensitivity checks and show that results are robust to several specifications, and to various alternative control group and treatment definitions. We find no evidence that concomitant variables influence the estimates. These findings make two alternative hypotheses implausible: First, although firms might acquire an employment-augmenting ownership advantage or experience a favorable demand or productivity shock and simultaneously expand foreign employment, the magnitude of this unobserved effect would have to be implausibly large to overturn our results. Second, there is no evidence for the alternative hypothesis that simultaneous sector-wide changes, such as trends in foreign trade, determine the treatment effect. So, the most plausible explanation for lower separation rates at FDI-expanding firms is their FDI expansion itself. The data do not allow us to discern involuntary layoffs from voluntary quits so that the added job security at firms with foreign expansions can be employer or worker initiated, or both.

We conclude that there is no empirical evidence on domestic job security that would justify interventions to hinder the formation of MNEs. To the contrary, our findings are consistent with the idea that preventing domestic MNEs from exploiting international wage differentials in house, or hampering MNEs' access to foreign product markets through FDI, would increase domestic worker separations at MNEs.

Appendix

A Linked employer-employee data

We link jobs to their FDI exposure throughout German corporate groups. This requires a two-step procedure. First, we identify all MIDI firms that are in the commercial company structure database MARKUS. Departing from the MIDI firms in MARKUS, we move both down and up in the corporate hierarchy of MARKUS to select the affiliates and ultimate parents of the MIDI firms. Second, we string-match all domestic plants in the BA worker database to the so-selected MARKUS firms for identification of all plants related to *FDI-conducting firms*. We also string-match the domestic plants to MIDI itself for identification of all those FDI reporting firms that are not part of a corporate group (but stand-alone firms).

We link the data based on names and addresses. By law, German plant names must include the firm name (but may be augmented with qualifiers). Before we start the string-match routine, we remove clearly unrelated qualifiers (such as manager names or municipalities) from plant names, and non-significance bearing components from plant and firm names (such as the legal form) in order to compute a link-quality index on the basis of highly identifying name components. Our string-match script computes link-quality indices as the percentage of words that coincide between any pair of names. We take a conservative approach to avoid erroneous links. We keep two clearly separate subsets of the original data: First, plants that are perfect links to MARKUS or MIDI, i.e. plant names that agree with firm names in every single letter. Second, plants that are perfect non-links, i.e. plant names that have no single word in common with any FDI-related MARKUS or MIDI firm. We drop all plants with a link-quality index between zero and one from our sample, i.e. plants whose name partially corresponds to an FDI firm name but not perfectly so. Those plants cannot be told to be either treatment or control plants without risk of misclassification.[22] The procedure leaves us with a distinct treatment group of FDI

[22]The string-match routine runs for several weeks, checking 3.8 million plants against 65,000 *FDI*

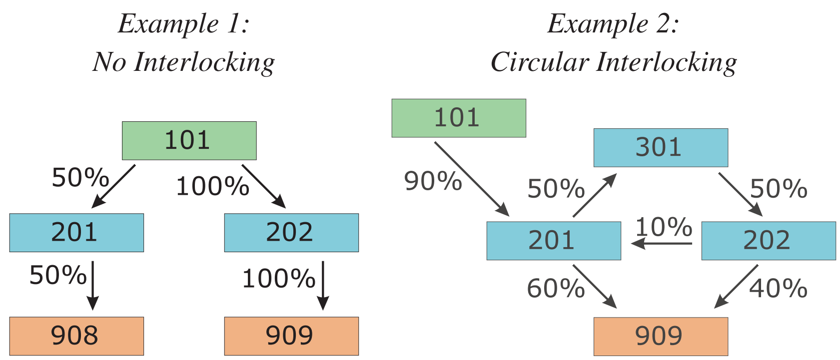


Figure 2: **Examples of Corporate Groups**

plants and a distinct control group of non-FDI plants.

The BA plant name file dates from November 2002 and contains names of plants that are no longer active so that we know exiting and entering plants. It is particularly important to capture exits after 1999 because one margin of separation is plant closure. Firm names in the MARKUS database are from three vintages of data, November 2000, November 2001 and November 2002. This is to make sure that in case of name changes in one of the years 2000 through 2002, we do not miss string matches.

Our procedure is designed to remove laterally related firms (sisters, aunts, or nieces) from the sample so that they neither enter the treatment nor the control group. Take Example 1 of Figure 2 and consider firm 201 to be the FDI-conducting (and FDI-reporting) firm in the depicted corporate group. The first step of our procedure identifies firm 201 in MARKUS and its affiliate and parent 908 and 101 but does not identify firms 202 (a sister to 201) and 909 (a niece to 201). If any name component of plants in firms 202 or 909 coincides with those of 101, 201 or 908 (but the plant name is not an identical match to 101, 201 or 908), the plants in firms 202 and 909 are discarded and neither enter the treatment nor the control group. If no single name component of plants in firms 202 or 909 is the same as that of 101, 201 or 908, the plant may enter our control group. If one considers sisters, aunts, and nieces with no single identical name component to be equally affected by FDI of firm 201 as those with common names or direct relations, their inclusion in the control group would make the control group more similar to the treatment group than it should be. If anything, however, the reduced difference would work against our outcome estimates. Moreover, interlocking (of which Example 2 of Figure 2 is a special case) limits the number of only laterally related firms.

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firms. It is infeasible to manually treat possible links with imperfect link-quality rates.

Table 10: Ownership Inference

Affiliate-parent pair	Iteration (Length of Walk) 1	2	3	5	9	100
201-101	.9	.90	.900	.92250	.92306	.92308
201-202	.1					
201-301		.05		.00125		
202-101			.225	.22500	.23077	.23077
202-201		.25		.00625		
202-301	.5					
301-101		.45	.450	.46125	.46153	.46154
301-201	.5					
301-202		.05		.00125		
909-101		.54	.540	.64350	.64609	.64615
909-201	.6		.100		.00006	
909-202	.4	.06		.00150		
909-301		.20	.030	.00500	.00001	

B Corporate ownership and FDI exposure

We infer the economically relevant ownership share of a domestic firm in any other domestic firm. The relevant ownership share can differ from the recorded share in a firm's equity for two reasons. First, a firm may hold indirect shares in an affiliate via investments in third firms who in turn control a share of the affiliate. We call ownership shares that sum all direct and indirect shares *cumulated* ownership shares. Second, corporate structures may exhibit cross ownership of a firm in itself via affiliates who in turn are parents of the firm itself. We call ownership shares that remove such circular ownership relations *consolidated* ownership shares. This appendix describes the procedure in intuitive terms; proofs based on graph theory are available from the authors upon request.

Consolidation removes the degree of self-ownership (α) from affiliates, or intermediate firms between parents and affiliates, and rescales the ultimate ownership share of the parent to account for the increased control in partly self-owning affiliates or intermediate firms (with a factor of $1/(1-\alpha)$). Investors know that their share in a firm, which partly owns itself through cross ownership, in fact controls a larger part of the firm's assets and its affiliates' assets than the recorded share would indicate. In this regard, cross ownership is like self-ownership. Just as stock buybacks increase the value of the remaining stocks because investors' *de facto* equity share rises, so do cross-ownership relations raise the *de facto* level of control of the parents outside the cross-ownership circle.

We are interested in *ultimate* parents that are not owned by other domestic firms, and want to infer their *cumulated and consolidated* ownership in all affiliates. Consider the following example of interlocking (Example 2 in Figure 2). The ultimate parent with firm ID 101 holds 90 percent in firm 201, which is also owned by firm 202 for the remaining 10 percent. However, firm 201 itself holds a 25 percent stake in firm 202—via its holdings of 50 percent of 301, which has a 50 percent stake in 201. Firms 201 and 202 hold 60 percent and 40 percent of firm 909. Our cumulation and consolidation procedure infers the ultimate ownership of 101 in all other firms.

We assemble the corporate ownership data in a three-column matrix:[23] the first column takes the affiliate ID, the second column the parent ID, and the third column the effective ownership share. Table 10 shows this matrix for Example 2 in Figure 2 (the third column with the direct ownership share is labelled 1, representing the single iteration 1).

On the basis of this ownership matrix, our inference procedure walks through the corporate labyrinth for a prescribed number of steps (or iterations). The procedure multiplies the ownership shares along the edges of the walk, and cumulates multiple walks from a given affiliate to a given ultimate parent. Say, we prescribe that the algorithm take all walks of length two between every possible affiliate-parent pair (in business terms: two firm levels up in the group's corporate hierarchy; in mathematical terms: walks from any vertex to another vertex that is two edges away in the directed graph).

We choose the following trick to infer the *cumulated and consolidated* ownership for ultimate parents: we assign every ultimate parent a 100 percent ownership of itself. This makes the procedure *cumulate and consolidate* the effective ownership share for all affiliates of ultimate parents, at any length of walks. There are seven distinct possibilities in the example to move in two steps through the corporate labyrinth. Table 10 lists these possibilities as iteration 2 (all entries in or below the second row). With our trick, there is now an eighth possibility to move from affiliate 201 to parent 101 in two steps because we have added the 101-101 loop with 100-percent ownership. As a result, our procedure cumulates ownerships of ultimate parents for all walks that are of length two or shorter. The procedure starts to consolidate shares as the length of the walk increases. Iteration 3 in Table 10 shows the cumulated and partially consolidated ownership of ultimate parent 101 in affiliate 201, for all three-step walks, including the first cycle from 201 through 202 and 301 back to 201 and then to 101.

[23] We assemble cleared ownership data by first removing one-to-one reverse ownerships and self-ownerships in nested legal forms (such as *Gmbh & Co. KG*).

Table 11: REGIONS

Region codes	Description
FOCAL REGIONS	
APD	Asia-Pacific Developing countries including China, Mongolia and North Korea; including Hong Kong, South Korea, Singapore, Taiwan; including dominions of OIN and EMU countries; excluding South Asia (India, Pakistan)
CEE	Central and Eastern European countries including EU accession countries and candidates excluding Russia and Central Asian economies
EMU	European Monetary Union participants 12 EU members that participate in Euro in 2001 excluding Denmark, Sweden, the UK and CEE countries (non-participating EMU signatories)
OIN	Overseas Industrialized counries including Canada, Japan, USA, Australia, New Zealand
OTHER REGIONS	
ODV	Other Developing countries including South Asia (India/Pakistan), Africa, Latin America, the Middle East; and EMU, OIN, OWE dominions
OWE	Other Western European countries including Denmark, Norway, Sweden, Switzerland, the UK
RCA	Russia and Central Asian economies;

In 2000, the maximum length of direct (non-circular) walks from any firm to another firm is 21. So, for all ultimate parents, the *cumulated and consolidated* ownership shares are reported correctly from a sufficiently large number of iterations on. Table 10 shows iteration 100. The ownership share of 101 in 201 has converged to the correct measure ($.9/(1-.1 \cdot .5 \cdot .5) = .\overline{923076}$) at five-digit precision. Firm 101 controls 92.3 percent of firm 201's assets, among them firm 201's foreign affiliates.

To calculate the FDI exposure at any hierarchy level in the corporate group, we use a single-weighting scheme with ownership shares. The economic rationale behind single-weighting is that ultimate parents are more likely to be the corporate decision units (whereas FDI conducting and reporting firms in the group may be created for tax and liability purposes). We first assign FDI exposure measures from domestic affiliates to their ultimate domestic parents. Our exposure measures are foreign affiliate employment and turnover by foreign region (see Table 11 for the definition of regions). Suppose firm 201 in Example 2 of Figure 2 conducts FDI in the corporate group. We assign 92.3 percent of 201's FDI exposure to firm 101, the ultimate domestic parent. We then assign the same 92.3 percent of 201's FDI exposure to all affiliates of 101 (201 itself, 202, 301, 909). So, jobs throughout the group (including those at 201 itself) are only affected to the degree that the ultimate parents can control foreign-affiliate employment (or turnover). We assign only 92.3 percent of 201's FDI exposure to 201 itself because the ultimate parent only has 92.3 percent of the control over employment at 201.[24]

For we choose single-weighting in the domestic branches of the MNE, we also single-weight foreign-affiliate employment (and turnover) by the ownership share of the domestic parent in its foreign affiliates. Mirroring the minimal ownership threshold of 10 percent in the MIDI data on foreign affiliates, we also discard domestic affiliates with ownership shares of less than 10 percent in our singe-weighting assignment of FDI exposure to domestic jobs throughout the corporate group.

C Rosenbaum bounds for binary outcomes

We outline the idea behind Rosenbaum (2002) bounds. Rewrite the probability that job i with observed characteristics $\mathbf{x}_i$ is treated with an FDI expansion to:

$$p\left(\mathbf{x}_i\right) = Pr(d_i\!=\!1|\mathbf{x}_i) = F(\beta\mathbf{x}_i + \gamma u_i), \tag{C1}$$

where u_i is the unobserved variable of concern (a newly acquired ownership advantage, for instance) and γ is the effect of u_i on the treatment probability. If the estimator is free of hidden bias, γ is zero and the participation probability is solely determined by $\mathbf{x}_i$. However, if there is hidden bias, two jobs with the same observed covariates x have differing chances of receiving treatment. Take a matched pair of observations i and j, and consider the logistic distribution F. The odds that the jobs

[24]An alternative assignment scheme would be double-weighting, first weighting FDI exposure by ownership and then assigning the FDI exposure to jobs throughout the corporate group using ownership weights again. We decide against double-weighting. Any weighting scheme results in exposure measures that are weakly monotonically decreasing as one moves upwards in the corporate hierarchy because ownership shares are weakly less than one. Double-weighting aggravates this property. Revisit Example 1 in Figure 2 and suppose firm 201 conducts FDI. Single-weighting assigns 50 percent of 201's exposure to affiliate 908, double-weighting only 12.5 percent. If 908 itself conducts the FDI, single-weighting assigns 25 percent of its own FDI exposure to 908, double-weighting only 6.25 percent. In economic terms, double-weighting downplays the decision power of intermediate hierarchies in the corporate group further than single-weighting so that we favor single-weighting. Recall that purely laterally related firms (sisters, aunts and nieces) are excluded from our treatment group so that firms 202 and 909 in Example 1 of Figure 2 are not relevant for the choice of weighting scheme.

receive treatment are $p(\mathbf{x}_i)/(1-p(\mathbf{x}_i))$ and $p(\mathbf{x}_j)/(1-p(\mathbf{x}_j))$ so that the odds ratio is given by

$$\frac{\frac{p(\mathbf{x}_i)}{1-p(\mathbf{x}_i)}}{\frac{p(\mathbf{x}_j)}{1-p(\mathbf{x}_j)}} = \frac{p(\mathbf{x}_i)(1-p(\mathbf{x}_j))}{p(\mathbf{x}_j)(1-p(\mathbf{x}_i))} = \frac{\exp(\beta\mathbf{x}_i+\gamma u_i)}{\exp(\beta\mathbf{x}_j+\gamma u_j)} = \exp[\gamma(u_i-u_j)]. \tag{C2}$$

If both jobs share the same observed covariates after propensity-score matching, the x-vector cancels. The jobs nevertheless differ in their odds of receiving treatment by a factor that involves the parameter γ and the difference in the unobserved variable u. It is the objective of sensitivity analysis to evaluate how inference about the treatment effect is altered by changing the values of γ and (u_i-u_j).

Assume for the sake of simplicity that the unobserved covariate is an indicator variable with $u_i \in \{0,1\}$ (indicating the acquisition of an ownership advantage). Rosenbaum (2002) shows that equation (C2) then implies the following bounds on the ratio of the odds that either of the two matched jobs will receive treatment:

$$\frac{1}{e^\gamma} \leq \frac{p(\mathbf{x}_i)}{p(\mathbf{x}_j)}\frac{(1-p(\mathbf{x}_j))}{(1-p(\mathbf{x}_i))} \leq e^\gamma. \tag{C3}$$

The two matched jobs have the same probability of being treated only if the odds ratio $e^\gamma = 1$. If the odds ratio $e^\gamma = 2$, then individuals who appear to be similar (in terms of x), could differ in their odds of receiving the treatment by as much as a factor of 2.

We compute critical values of the odds ratio e^γ based on the Mantel and Haenszel (1959) test statistic, as suggested by Rosenbaum (2002). The Mantel and Haenszel test statistic assesses the strength of hidden bias that would be necessary to overturn our ATT estimate.

The non-parametric Mantel and Haenszel (1959) test compares the successful number of individuals in the treatment group to the same expected number under the null hypothesis that the treatment effect is zero. Denote with N_{1s} and N_{0s} the numbers of treated and non-treated individuals in stratum s, where $N_s = N_{0s}+N_{1s}$. y_{1s} is the number of treated jobs with a separation outcome, y_{0s} is the number of non-treated jobs with a separation outcome, and y_s is the number of total separations in stratum s. The MH test-statistic Q_{MH} asymptotes the standard normal distribution and is given by

$$Q_{MH} = \frac{|y_1 - \sum_{s=1}^S E(y_{1s})| - .5}{\sqrt{\sum_{s=1}^S Var(y_{1s})}} = \frac{|y_1 - \sum_{s=1}^S (\frac{N_{1s}y_s}{N_s})| - .5}{\sqrt{\sum_{s=1}^S \frac{N_{1s}N_{0s}y_s(N_s-y_s)}{N_s^2(N_s-1)}}}. \tag{C4}$$

Our propensity-score matching procedure minimizes differences between treatment and control group observations so that the MH test (designed for random samples) is applicable. Take the possible influence of a binary hidden variable with an effect $e^{\gamma} > 1$ on the outcome. For fixed $e^{\gamma} > 1$, Rosenbaum (2002) shows that the MH test statistic Q_{MH} can be bounded by two known distributions. If $e^{\gamma} = 1$, the bounds are equal to the baseline scenario of no hidden bias. With increasing e^{γ}, the bounds move apart, reflecting uncertainty about the test statistic in the presence of unobserved selection bias.

Consider two scenarios. First, let Q^{+}_{MH} be the test statistic given that we overestimate the treatment effect and, second, let Q^{-}_{MH} the case where we underestimate the treatment effect. The two bounds are then given by:

$$Q^{+}_{MH} = \frac{|y_1 - \sum_{s=1}^{S} \widetilde{E}^{+}_{s}| - .5}{\sqrt{\sum_{s=1}^{S} Var(\widetilde{E}^{+}_{s})}} \tag{C5}$$

and

$$Q^{-}_{MH} = \frac{|y_1 - \sum_{s=1}^{S} \widetilde{E}^{-}_{s}| - .5}{\sqrt{\sum_{s=1}^{S} Var(\widetilde{E}^{-}_{s})}}, \tag{C6}$$

where $\widetilde{E_s}$ and $Var(\widetilde{E_s})$ are the large sample approximations to the expectation and variance of the number of successful participants when the hidden variable is binary and γ given.[25]

D Generalized propensity scores

Index a sample of jobs with $i = 1, \ldots, N$ and consider the *unit-level dose-response function* of outcomes $Y_i(t)$ as a function of treatments $t \in \mathscr{T}$. In the binary treatment case $\mathscr{T} = \{0, 1\}$. In the continuous case, we allow $\mathscr{T}$ to be an interval $[t_0, t_1]$. We restrict $t_0 > 0$ to study the range of employment expansions that we used to summarize with a treatment indicator of one and in order to exclude the probability mass at zero treatment in accordance with the Hirano and Imbens approach. We are interested in the average dose-response function across all jobs i, $\mu(t) = E[Y_i(t)]$. We observe the vector X_i, the treatment T_i, and the outcome corresponding to the level of treatment received, $Y_i = Y_i(T_i)$. We drop the index i for simplicity and assume that $Y(t)_{t\in\mathscr{T}}, T, X$ are defined on a common probability

[25]The large sample approximation to $\widetilde{E}^{+}_{s}$ is the unique root of the quadratic equation $\widetilde{E}^{2}_{s}(e^{\gamma} - 1) - \widetilde{E}_{s}[(e^{\gamma} - 1)(N_{1s} + y_s) + N_s] + e^{\gamma} y_s N_{1s}$, after addition of $max(0, y_s + N_{1s} - N_s \leq \widetilde{E_s} \leq min(y_s, N_{1s}))$ to select the root. $\widetilde{E}^{-}_{s}$ follows by replacing e^{γ} with $1/e^{\gamma}$. The large sample approximation to the variance is $Var(\widetilde{E}_s) = [1/\widetilde{E}_s + 1/(y_s - \widetilde{E}_s) + 1/(N_{1s} - \widetilde{E}_s) + 1/(N_s - y_s - N_{1s} + \widetilde{E}_s)]^{-1}$.

space, that t is continuously distributed with respect to a Lebesgue measure on $\mathscr{T}$, and that $Y = Y(T)$ is a well defined random variable.

In this setting, the definition of unconfoundedness (4) for binary treatments generalizes to *weak unconfoundedness* for continuous treatments

$$Y(t) \quad \perp \quad T|X \quad \text{for all} \quad t \in \mathscr{T}. \tag{D1}$$

Jobs differ in their characteristics x so that they are more or less likely to be exposed to FDI expansions. The weak unconfoundedness assumption says that, after controlling for observable characteristics X, any remaining difference in FDI expansions T across jobs is independent of the potential outcomes $Y(t)$. Assumption (D1) is called weak unconfoundedness because it does not require joint independence of all potential outcomes, $Y(t)_{t\in[t_0,t_1]}, T, X$. Instead, it requires conditional independence to hold at every treatment level.

Hirano and Imbens (2004) define the generalized propensity score (GPS) as

$$R = r(T, X), \tag{D2}$$

where $r(t, x) = f_{T|X}(t|x)$ is the conditional density of the treatment given the covariates. The GPS is assumed to have a *balancing property* similar to that of the conventional propensity score under binary treatment: within strata with the same value of $r(t, X)$, the probability that $T = t$ does not depend on the value of X. In other words, when looking at two jobs with the same probability (conditional on observable characteristics X) of being exposed to a particular FDI expansion, their treatment level is independent of X. That is, the GPS summarizes all information in the multi-dimensional vector X so that

$$X \quad \perp \quad 1\{T = t\}|r(t, X).$$

This is a mechanical property of the GPS, and does not require unconfoundedness. In combination with unconfoundedness, the balancing property implies that assignment to treatment is *weakly unconfounded given the generalized propensity score* (see Hirano and Imbens (2004) for a proof): if assignment to the treatment is weakly unconfounded given pre-treatment variables X, then

$$f_T(t|r(t, X), y(T)) = f_T(t|r(t, X)) \tag{D3}$$

for every t. This result says that we can evaluate the GPS at a given treatment level by considering the conditional density of the respective treatment level t. In that sense we use as many propensity scores as there are treatment levels, but never more than a single score at one treatment level.

We eliminate biases associated with differences in the covariates in two steps (for a proof that the procedure removes bias, see Hirano and Imbens (2004)):

1. Estimate the conditional expectation of the outcome as a function of two scalar variables, the treatment level T and the GPS R, $\beta(t,r) = E[y|T = t, R = r]$

2. Estimate the dose-response function at a particular level of the treatment by averaging this conditional expectation over the GPS at that particular level of the treatment, $\mu(t) = E[\beta(t, r(t, X))]$.

It is important to note that, in the second step, we do not average over the GPS $R = r(t, X)$; rather we average over the score evaluated at the treatment level of interest, $r(t, X)$. In other words, we fix t and average over X_i and $r(t, X_i)$ $\forall i$.

Table 12: RAW SEPARATION PROBABILITIES BY SECTOR AND REGION OF FDI EXPOSURE

	WW	APD	CEE	EMU	ODV	OIN	OWE	RCA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
plants without FDI exposure in region l								
food and tobacco	.217	.207	.210	.215	.208	.208	.209	.207
textile, apparel, leather	.203	.201	.197	.199	.193	.196	.194	.191
wood and paper products	.210	.189	.192	.200	.191	.195	.196	.191
chemicals	.136	.139	.135	.140	.142	.140	.141	.142
non-metallic products	.154	.152	.149	.153	.151	.152	.151	.146
metallic products	.172	.162	.160	.170	.162	.162	.167	.156
non-electrical machinery	.138	.136	.138	.135	.137	.136	.133	.132
electronics and optic. equipmt.	.168	.182	.179	.171	.176	.176	.174	.170
transportation equipm.	.166	.146	.144	.153	.150	.153	.143	.120
other manufacturing	.219	.206	.208	.217	.206	.208	.213	.205
plants with FDI exposure relative to plants without FDI exposure								
food and tobacco	-.066	-.048	-.058	-.065	-.046	-.042	-.044	-.047
textile, apparel, leather	-.037	-.102	-.039	-.028	-.027	-.037	-.033	-.056
wood and paper products	-.071	-.026	-.031	-.053	-.046	-.061	-.051	-.062
chemicals	.039	.046	.058	.035	.035	.043	.036	.082
non-metallic products	-.020	-.031	-.008	-.021	-.022	-.026	-.017	-.001
metallic products	-.056	-.060	-.039	-.056	-.058	-.046	-.060	-.049
non-electrical machinery	-.001	.004	-.003	.005	.000	.004	.012	.034
electronics and optic. equipmt.	.005	-.043	-.030	-.002	-.022	-.016	-.014	.001
transportation equipm.	-.070	-.061	-.048	-.058	-.063	-.065	-.048	-.021
other manufacturing	-.067	-.046	-.043	-.075	-.043	-.049	-.069	-.044

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI-exposed and non-FDI exposed manufacturing plants. Locations (see Table 11): WW (World-Wide abroad), APD (Asia-Pacific Developing countries), CEE (Central and Eastern European countries), EMU (European Monetary Union member countries), ODV (Other Developing countries), OIN (Overseas Industrialized countries), OWE (Other Western European countries), and RCA (Russia and Central Asian countries).

Table 13: RAW SEPARATION PROBABILITIES BY SECTOR AND REGION OF FDI EXPANSION

	WW (1)	APD (2)	CEE (3)	EMU (4)	ODV (5)	OIN (6)	OWE (7)	RCA (8)
plants without FDI exposure in region l								
food and tobacco	.211	.207	.207	.210	.206	.207	.208	.208
textile, apparel, leather	.198	.195	.193	.197	.189	.193	.197	.190
wood and paper products	.195	.189	.193	.192	.190	.188	.192	.188
chemicals	.160	.144	.152	.153	.149	.151	.138	.148
non-metallic products	.152	.147	.146	.150	.153	.151	.149	.147
metallic products	.164	.159	.162	.169	.153	.155	.160	.155
non-electrical machinery	.138	.136	.137	.133	.138	.133	.130	.139
electronics and optic. equipmt.	.176	.181	.179	.177	.176	.174	.177	.170
transportation equipm.	.147	.134	.149	.145	.130	.139	.129	.116
other manufacturing	.204	.207	.201	.201	.204	.204	.208	.206
plants with FDI exposure relative to plants without FDI exposure								
food and tobacco	-.053	-.047	-.038	-.054	-.062	-.041	-.045	-.069
textile, apparel, leather	-.035	-.084	-.019	-.045	.012	-.035	-.097	.060
wood and paper products	-.054	-.035	-.052	-.045	-.066	-.040	-.044	-.062
chemicals	-.021	.036	.002	-.002	.020	.007	.067	.029
non-metallic products	-.025	-.009	-.001	-.017	-.044	-.041	-.017	-.012
metallic products	-.052	-.066	-.056	-.074	-.030	-.030	-.052	-.054
non-electrical machinery	-.003	.003	-.002	.014	-.006	.014	.034	-.022
electronics and optic. equipmt.	-.022	-.048	-.041	-.024	-.036	-.014	-.059	.003
transportation equipm.	-.049	-.052	-.060	-.054	-.048	-.046	-.031	.003
other manufacturing	-.022	-.058	.012	.010	-.031	-.061	-.062	-.090

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI-exposed and non-FDI exposed manufacturing plants. Locations (see Table 11): WW (World-Wide abroad), APD (Asia-Pacific Developing countries), CEE (Central and Eastern European countries), EMU (European Monetary Union member countries), ODV (Other Developing countries), OIN (Overseas Industrialized countries), OWE (Other Western European countries), and RCA (Russia and Central Asian countries).

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