The Effect of FDI on Worker Displacement*

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September 21, 2006

Abstract

A novel linked employer-employee data set is used to analyze whether multinational enterprises save or cut domestic jobs more frequently than national competitors. In contrast to prior research, a propensity score estimator explicitly allows enterprise performance to vary with foreign direct investment (FDI). Various alternative measures of a domestic job's exposure to enterprise FDI robustly show that FDI expansions significantly reduce the probability of domestic worker displacement. This finding is consistent with the hypothesis that, given global factor price differences, a reluctance of enterprises to exploit international wage differentials would cost domestic jobs. The job-saving effects are more pronounced for skilled workers.

Keywords: Multinational enterprises; international investment; demand for labor; worker layoffs; linked employer-employee data

JEL Classification: F21, F23, J23, J63

*We thank seminar and conference participants at UC San Diego, at the 81st WEA Annual Conference, the Munich-Tübingen Workshop in Trade, and Gordon Hanson and Andreas Waldkirch in particular, for useful comments and discussions. We thank Heinz Herrmann, Alexander Lipponer and Fred Ramb for access to and ongoing support with the BuBa firm data, and Stefan Bender, Iris Koch and Stephan Heuke for assistance with the BA employment records. Karin Herbst and Thomas Wenger at BuBa kindly shared their expertise on string-matching. Regis Barnichon, Nadine Gröpl, Robert Jäckle, Daniel Klein, and Stefan Schraufstetter provided excellent research assistance. We gratefully acknowledge financial support from the VolkswagenStiftung under its grant initiative *Global Structures and Their Governance*, and administrative and financial support from the Ifo Institute. Becker also gratefully acknowledges financial support from the Fritz-Thyssen-Stiftung.

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1 Introduction

The formation of multinational enterprises (MNEs) is a driving force of global integration. Much empirical research to date investigates how international factor price differences affect MNEs, given MNE characteristics such as size and performance. An expected answer is that international factor substitution within MNEs reduces MNE employment in industrialized countries. The importance of that issue notwithstanding, our present focus is different. We investigate how the exposure of domestic jobs to foreign expansions at MNEs affects employment stability—given the global factor price disparities under which firms compete. Importantly, we allow firm performance to vary. The answer to the former thought experiment, whether wage differences predict labor substitution within MNEs across regions, shows labor market consequences of trade in an unequally endowed world. That assessment is separate from the arguably more policy-relevant question of this paper: Do MNEs and their workers fare better when MNEs exploit existing factor price differences through foreign expansions than when they do not? Put differently, prevailing wage differentials across the world may eliminate jobs in industrialized countries, but we test whether a reluctance of domestic firms to exploit those wage differentials within the enterprise boundaries may wipe out even more jobs. In fact, our findings robustly show that FDI expansions significantly reduce the rate of job loss at MNE home establishments compared to their domestic competitors. Under the largely inevitable global competition on factor costs, MNEs' expansions abroad save jobs at home.

MNEs are important mediators of world trade. Trade in turn affects factor demand. Two in five imports to the U.S., for instance, are transacted within MNEs (Zeile 1997). The world's ten largest MNEs in 2000 produce almost one percent of world GDP, and the one hundred largest MNEs are responsible for more than four percent of world GDP (up from three-and-a-half percent in 1990).[1] Surprisingly, however, most existing research does not find MNEs to strongly affect home factor demands. Several studies conclude that MNE production in low-wage regions has no detectable impact on their labor demand in the home market (e.g. Slaughter (2000) for U.S. MNEs, and Barba Navaretti and Castellani (2004) for MNEs from EU countries), or find modest substitution between workers in domestic establishments and foreign affiliates (e.g. Konings and Murphy (2006), Harrison and McMillan

[1] UNCTAD press release TAD/INF/PR/47 (12/08/02).

(2006), Marin (2006), excepting Muendler and Becker (2006) who control for location selectivity).

We use a novel and comprehensive linked employer-employee panel data set for Germany to analyze how an enterprise's foreign direct investment (FDI) changes home labor demand at the level of the individual job. We calculate a domestic job's exposure to its enterprise's activity abroad and measure through propensity score matching how the assignment of additional FDI exposure changes the probability that the domestic job remains filled or that its holder suffers displacement. To fix ideas, consider the hypothetical experiment of treating a domestic workforce with a randomly assigned manager who is proficient in a foreign language and uniquely able to conduct FDI on the enterprise's behalf. The propensity score matching technique considers pairs of otherwise identical domestic jobs, one job of each pair randomly treated with exposure to foreign expansions by assignment of a proficient manager and the other job in the pair untreated. The propensity score estimator measures how the treatment alters the probability of displacement—allowing the establishment's and enterprise's subsequent performance to vary freely with the treatment but conditioning on a comprehensive set of initially identical worker, job, establishment, parent-firm and sector characteristics in the job pair.

Our results show that an increase in world-wide FDI exposure significantly reduces the rate of job loss and explains around half of the lower worker displacement rate of 14 percent among expanding MNEs compared to 18 percent among non-expanding firms. When distinguishing FDI expansions by foreign region, we find significant reductions in the rate of job losses of up to seven percent and never find outward FDI to increase the probability of home worker displacement. When distinguishing workers by educational attainment, and occupations by skill intensity, we find more educated workers to experience stronger job savings effects than their less educated colleagues but we find no marked difference across job types.

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 serve to assess the plausibility of main competing hypotheses. MNEs can be considered to 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 and simultaneously expand FDI. Our first robustness check probes the plausibility of this hypothesis. We use Rosenbaum (2002) bounds and estimate that an unobserved confounding factor, such as simultaneous process or product innovation, would have to alter our estimate by more than 25 percent to overturn the findings—a sizeable and unlikely change for it would be equivalent to an increase in the secondary-schooled workforce from zero to a hundred percent of the workforce, for instance.

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. Our second 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 typical confidence bands, and no evidence for erroneous attribution. We conclude that the most plausible explanation for lower displacement rates at FDI-expanding firms is their FDI expansion itself.

We probe that explanation with further checks. Third, we show estimates under alternative control-group definitions and again find our results confirmed. Fourth, we use increases in MNE turnover abroad as an alternative treatment variable and confirm our results, now with an even larger average treatment effect on the treated. Fifth and last, we use several expansion thresholds to redefine the outward-FDI treatment increasingly restrictively with 1 percent, 5 percent, and 10 percent foreign employment expansions. We find overwhelmingly robust estimates and, for the main treatment measure of foreign expansions anywhere, at most slight changes within typical confidence bands. This result is consistent with the idea that the foreign expansion itself is the strongest explanatory factor for reduced displacement rates, and not the magnitude of the expansion.

The paper has six more sections. The next section briefly reviews related research. Section 3 discusses the methodology, Section 4 describes the construction of our linked employer-employee data. We present the main results in Section 5, and conduct a series of robustness checks in Section 6. Section 7 concludes. Methodological derivations and details of data construction are relegated to the Appendix.

2 Related Literature

To our knowledge, there is to date no job-level research into the effects of MNE activities using linked employer-employee data. In contrast to most existing research, which uses global factor price differences to predict home employment levels (Slaughter 2000, Muendler and Becker 2006), our linked employer-employee data allow us to investigate whether MNEs that expand abroad save or cut jobs compared to national competitors. A related literature on worker displacement is concerned with consequences of worker layoffs (e.g. Jacobson, LaLonde, and Sullivan 1993, Kletzer 1998, Kletzer 2001). Kletzer (2001) classifies sectors into import competing, or not, and assesses the cost of job loss. We concentrate on identifying the causes of worker displacement by estimating job displacement probabilities as a function of narrow, but well-defined, FDI exposure measures at the firm level.

Worker displacement is a direct indicator of changes to labor demand. In related research, Geishecker (2006) uses individual household survey data to study the effect of sectoral intermediate-goods imports on German workers. He finds cross-border outsourcing to significantly 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 are Egger and Pfaffermayr (2003) and Barba Navaretti and Castellani (2004), who apply propensity score matching to firm data. Egger and Pfaffermayr (2003) contrast home investment behavior of pure exporters with that of MNEs and find no significant difference. Barba Navaretti and Castellani (2004) assess the effect of first-time FDI on firm performance and do not report significant effects of outward FDI on MNE employment growth. To the contrary, we identify salient job savings effects at the displacement margin, both for first-time MNEs and for expanding MNEs. Our linked employer-employee data allow the propensity score to handle multiple sources of heterogeneity—worker, job and establishment characteristics beyond MNE and sector covariates—, and separate the decision maker (the MNE) from the treated unit (the job).

3 Methodology

Propensity score matching aims at reducing the bias in treatment-effect estimates when the sample is not random (Rosenbaum and Rubin 1983). Propensity score matching is regarded to provide a causal measure of the treatment effect on an outcome. The 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 establishment, the enterprises's foreign operations and the industry—comprehensively covers the pretreatment conditions so that treatment is ascribable to exogenous changes at the establishment, 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 single scalar, the propensity score. If units with the same propensity score value are randomly exposed to treatment, then the bias in estimated treatment effects is eliminated.

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) show that, if the propensity score $p(\mathbf{x}_i)$ is known, the ATT can be defined as

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

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 true.

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. Linked employer-employee data allow us to separate the treated unit (job) from the decision maker (the parent firm) in support of unconfoundedness. Comprehensive worker, job, establishment, enterprise and industry information in our data make treatment ascribable to exogenous shocks beyond the job level. To query unconfoundedness, we test whether the predictive power of job-level variables is zero once establishment, 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 (equation (2), we use the estimated propensity scores to pick pairs based on nearest-neighbor matching. We 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 $\mathbb{C}(i) \equiv \min_j \parallel p_i - p_j \parallel$, which is 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 $w_{ij} \equiv \frac{1}{N_i^C}$ if $j \in \mathbb{C}(i)$, and $w_{ij} = 0$ otherwise. Then, the nearest neighbor estimator of the ATT is:

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

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

4 Data

Our linked employer-employee data set is constructed from three confidential micro-data sources, assembled at Deutsche Bundesbank headquarters in Frankfurt, and is complemented with sector and country information. We define enterprises as groups of affiliated domestic and foreign firms and consider all firms within a group as potential *FDI firms* if at least one firm in the group reports outward FDI activities. 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 by the social insurance system in the years 1999-2001, representing around 80% of the German workforce.[2] The files contain worker and job characteristics such as age, education level, 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 with nominal wage changes.[3] We construct establishment-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.[4] The data provide information on affiliate employment, turnover, and balance sheets 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 Ger-

[2]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. Establishments within the same municipality may report under one single establishment identifier.

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

[4]In 1999 and 2000, reporting is mandatory for all foreign affiliates with an asset total of at least EUR 10 million and at least a ten-percent ownership share of the German parent, or an asset total of at least EUR 1 million and at least a 50-percent ownership.

man enterprises bundle the domestic management of their foreign affiliates in legally separate firms (mostly limited liability *GmbHs*) for 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-matching procedure to identify clearly identical firms and their establishments (see Appendix A for a detailed description). We use the year $t = 2000$ as our base year because it is the earliest year for which we have firm structure information and can thus adequately attribute outward FDI exposure to domestic jobs. The linked data provide a cross-section of establishments around year $t = 2000$, including a total of 39,681 treated and 1,133,920 control establishments out of 3.8 million establishments in the full worker sample (1998-2002). We use a 5% random sample of workers (93,147 job observations) to reduce estimation runtime to acceptable length.

We observe pretreatment characteristics of workers, jobs and domestic establishments at $t-1 = 1999$ (from BA files in June 1999; June files being the most reliable during the year). Most pretreatment characteristics vary little between $t-1$ and t, so 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 continued employment or displacement) is observed between t and $t+1 = 2001$.

We complement these 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.[5]

[5] We calculate intermediate-goods imports by foreign location using the import share 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.

Outcomes. Our outcome variable is an indicator of a worker's displacement 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 establishment between years t and $t{+}1$ (note the one-year lead between outcome and treatment), and is zero otherwise. This measure of worker displacement includes both quits and layoffs.[6] A change of occupation within the employing establishment is not considered a displacement.

Treatments. The natural counterpart to displacement 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 because it relates foreign to domestic jobs. For robustness checks, we also use affiliate turnover.

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 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 world-wide affiliate employment (WW) as well as region-specific affiliate employments. For the region-specific measures, we define four main foreign regions (see Table 14), 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),[7] and Overseas Industrialized countries (OIN). We omit other developing countries, non-EMU member countries in Western Europe

[6] The German social-security records do not distinguish quits from layoffs. In practice, apparently voluntary quits tend to preempt layoffs, rendering the theoretical distinction between quits and layoffs unclear for empirical work.

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

and Russia and the Central Asian countries to create more homogeneous individual locations. World-wide (WW) expansions, however, include all countries.

Covariates. We use a rich set of covariates that can predict worker displacement. The covariates are: worker characteristics (age, gender, education, monthly wage); job characteristics (part-time work, occupation); domestic establishment characteristics (workforce size and 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 establishment-level differences in productivity, we also estimate the establishment-fixed component in German wages from a Mincer (1974) regression with June 2000 workers and include the establishment-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 establishments. Displacement rates differ markedly across workers in MNE establishments and non-MNE establishments. 14 percent of workers separate from non-MNE establishments between the years 2000 and 2001, whereas 18 percent of workers separate from non-MNE establishments.

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

The German MNE to which domestic MNE establishments belong employs about 4,000 workers abroad on average. 64% of the workers in MNE establishments are subject to a foreign employment expansion between the years 1999 and 2000, whereas only 2% of the workers in non-MNE establish-

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

	MNE subsample		non-MNE subsample	
	mean	s.d.	mean	s.d.
Outcome: Worker displacement				
Displaced between t and t+1	.14	.34	.18	.38
Treatment: FDI exposure and expansion				
Total employment abroad in 1,000s in ($t-1$)	3.99	6.10	.00	.00
Indic.: Foreign employment change from $t-1$ to t	.64	.48	.02	.15
Foreign employment growth from $t-1$ to t in 1,000s	.65	2.99	.009	.17
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
Upper-secondary schooling or more	.16	.37	.08	.28
Current apprentice	.02	.15	.04	.19
Part-time employed	.05	.21	.12	.33
Establishment-level variables				
Employment at domestic establishment	2,683.8	7,935.3	926.9	3,153.3
Indic.: Establishment in East Germany	.09	.29	.10	.30
Number of observations	38,046		55,101	

Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing establishments.

ments see their employer become an MNE and expand abroad.

MNE establishments differ from non-MNE establishments in several further dimensions. Workers in MNE establishments 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 establishments. MNE establishments are bigger on average than non-MNE establishments. Median employment is 644 and 103 for MNE and non-MNE establishments, respectively.

5 Estimates

We investigate the effect of FDI *expansions* abroad on worker displacement 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 displacement 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 displacement 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, establishment, 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 indeed balances the treated and control job sub-samples. Our comprehensive set of predictors covers relevant pre-treatment dimensions so that remaining differences are likely random in nature. We then obtain ATT estimates of FDI expansions region by region, using nearest-neighbor matching based on the predicted propensity scores.

5.1 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 world-wide 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—to ensure that treatment effects measure the consequence of FDI expansions.

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 (marginal employment, apprentices, part-time employment) are more likely to be subject to FDI expansions.

Table 2: Specifications 1 and 2 of the propensity score

	Specification 1		Specification 2	
	Odds Ratio	Std. Err.	Odds Ratio	Std. Err.
	(1)	(2)	(3)	(4)
Age	.994	.006	1.005	.006
Age-squared	1.003	.007	.994	.007
ln($wage$)	4.980	.149 ***	1.039	.040
Female	1.242	.027 ***	1.027	.024
In marginal employment	4.967	.433 ***	1.215	.124
In other type of employment	1.838	.154 ***	1.095	.098
White-collar worker	.748	.015 ***	1.016	.023
Upper-secondary schooling or more	1.097	.028 ***	.969	.027
Current apprentice	2.584	.260 ***	.972	.107
Part-time employed	1.549	.067 ***	1.005	.048
Share with upper sec. school or more			1.216	.132 *
Average age			.983	.003 ***
Share in apprenticeship			.033	.016 ***
Share in marginal employment			.464	.098 ***
Share in other types of employment			1.395	.600
Share of females			1.353	.100 ***
Share in part-time employment			.454	.074 ***
Average yearly wage in EUR			1.001	.00008 ***
Share of white-collar workers			.548	.045 ***
Plant-fixed wage component			2.743	.491 ***
Const.	1.60e-06	3.93e-07 ***	.056	.020 ***
Obs.	93,147		93,147	
Pseudo R^2	.069		.135	

Standard errors: * significance at ten, ** five, *** one percent.
Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing establishments.

In *specification 2*, we add establishment characteristics (columns 3 and 4 of Table 2). All worker and job characteristics turn insignificant once establishment averages 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 separate decisions. This lends additional credibility to propensity score matching in our context because the FDI decision-making unit can be considered distinct from the treated unit. Among the establishment variables is an establishment-fixed effect from a Mincer wage regression on the worker cross section to control for establishment-level differences in labor productivity, which theory suggests to be a factor for selection into foreign expansions (e.g. Helpman, Mélitz, and Yeaple 2004).

We estimate propensity scores under two further specifications. *Specification 3* 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 establishment information.[8] Wages are the main time-varying covariate for workers. Worker- and job-level controls remain insignificant and coefficients on plant-level covariates change little (remaining significant), so we do not report coefficient estimates here.[9]

In summary, establishment, MNE and sector characteristics are significant and economically important covariates of FDI expansions, both for world-wide and region-specific FDI expansions. This shows that FDI expansions themselves are not random but a choice predictable by establishment and MNE characteristics. There is no evidence, however, 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 disregard specification 1 that included only worker and job variables.

5.2 Covariate balancing

Based on the estimated propensity score, we use nearest-neighbor matching to combine treated and control observations.[10] 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 treatment and specification. Treated jobs are matched to between three and five control jobs on average (see fractions of treated in column 3).[11]

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 to which degree regressors $\mathbf{x}_i$ predict the treatment probability. After matching, regressors $\mathbf{x}_i$ should have no explanatory power for selection into treatment if the treatment and

[8]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.

[9]Results are available at *econ.ucsd.edu/muendler/research*.

[10]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 Mahalanobis and propensity score matching and covariate balance testing.

[11]Our ATT estimator will take unweighted averages of the matched control jobs when pairing them with the treated jobs.

Table 3: Covariate Balancing, Before and After Matching

	No. of treated	No. of controls	% of treated before	Logit ps. R^2 before	Logit ps. R^2 after	Median bias before	Median bias after	% 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 14): 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).

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) suggest 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:

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

where $\bar{\mathbf{x}}_{i1}$ denotes the treated unit mean and $\bar{\mathbf{x}}_{i0}$ the control unit mean for covariate $\mathbf{x}_i$. The pre-matching standardized difference $B_{before}(\mathbf{x}_i)$ is the difference of the sample means in the full treated and nontreated subsamples as a percentage of the square root of the average of the sample variances in the full treated and nontreated groups. The post-matching standardized difference $B_{after}(\mathbf{x}_i)$ is the difference of the sample means in the matched treated and matched nontreated subsamples as a percentage of the square root of the average of the sample variances in the full treated and nontreated groups. 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 fraction of treated observations falling outside of the common support is minimal (column 8).

As is commonly done in the evaluation literature, we show the median absolute standardized bias before and after matching over all regressors $\mathbf{x}_i$ that enter the propensity score estimation. Across regions of treatment and specifications, matching reduces the median absolute standardized bias by 70 to 90 percent (columns 6 and 7). 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)).[12] There is no single specification whose bias is consistently lower than that of other specifications for all regions.

Further balancing statistics based on goodness-of-fit measures (Heckman, Ichimura, and Todd 1997, for instance) tend to favor richer specifications over more parsimonious specifications for propensity-score estimation. Heckman and 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 unobserved influences, we will use Rosenbaum (2002) bounds after ATT estimation.

[12]Rosenbaum and Rubin (1985) suggested that a value of 20 to be “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)***

Standard errors (in parentheses): * significance at ten, ** five, *** one percent.
Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing establishments.

5.3 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 OLS estimates of the treatment effect. We report analytic standard errors.[13]

Across specifications, the ATT estimate for an expansion in affiliate employment anywhere worldwide ranges between -.014 and -.026 percent. So, worldwide employment expansions reduce the probability of domestic worker displacement 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-

[13]We found bootstrapped standard errors to be close in specifications for which we obtained both analytic and bootstrapped standard errors.

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 job-saving effect of FDI expansions may be underestimated when not controlling for past determinants of establishment performance.

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 displacement 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 displacement rates.

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 displacement rates is similar. The ATT for expansions in Euro area participating countries, however, is not statistically significant. If performance gains of expanding MNEs relative to non-expanding MNEs are small in the highly integrated Euro area, the lacking significance of the ATT for EMU would be expected.

To summarize, in no single specification and for no single region is there a positive treatment effect. Our estimates invariably point towards job saving effects at foreign-employment expanding MNEs relative to non-expanding firms. This finding stands only in seeming contrast to previous studies. These results complement earlier findings. 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—conditional 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 inspired 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 establishments (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 international locations in which internationally competing firms have to operate, MNEs that expand abroad save jobs at home.

5.4 Worker and job heterogeneity

Employment expansions at MNEs abroad may affect workers and jobs differentially depending on their skill level. We distinguish two education groups of workers and separate jobs by two skill intensity levels. Results show that FDI expansions in any foreign location save jobs for both education groups and for both job types—with no single statistically significant exception.

Table 5 shows results for workers with and without an upper-secondary schooling degree (the university-qualifying *Abitur*). Especially in specifications 2 and 3, job-savings effects are typically stronger for workers with an upper-secondary schooling degree than for workers with less education. In our richest specification 4, we find FDI expansions anywhere worldwide to reduce displacement rates by 11.9 percentage points for domestic workers with complete upper-secondary 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.

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 job-savings effects of foreign employment expansions are significant for blue-collar workers, we find no clear differences in the ATT point estimates. So, the job savings effects of foreign employment expansions appear to be widely shared across job types.

6 Robustness Checks

Propensity-score estimation of the ATT, the effect of foreign employment expansions on home employment, suggests that expansions abroad save home

Table 5: ATT, High and Low Education Levels

	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)
WORKERS WITH UPPER-SECONDARY EDUCATION OR MORE				
WW	-.045	-.029	-.071	-.119
	(.007)***	(.032)	(.016)***	(.033)***
APD	-.034	-.076	.002	-.008
	(.008)***	(.020)***	(.043)	(.046)
CEE	-.048	-.118	-.144	-.057
	(.008)***	(.040)***	(.040)***	(.041)
EMU	-.029	-.068	-.095	-.004
	(.008)***	(.026)**	(.031)***	(.034)
OIN	-.025	-.046	-.122	-.018
	(.008)***	(.027)*	(.041)***	(.041)
WORKERS WITH LESS THAN UPPER-SECONDARY EDUCATION				
WW	-.045	-.019	-.028	-.027
	(.003)***	(.006)***	(.006)***	(.010)***
APD	-.045	-.060	-.023	-.021
	(.004)***	(.018)***	(.018)	(.018)
CEE	-.046	-.019	-.029	-.027
	(.003)***	(.011)*	(.016)*	(.013)**
EMU	-.047	-.023	-.006	-.013
	(.003)***	(.008)***	(.011)	(.009)
OIN	-.038	-.028	-.039	-.041
	(.003)***	(.010)***	(.011)***	(.016)***

Standard errors (in parentheses): * significance at ten, ** five, *** one percent.
Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing establishments. Number of observations: 10,652 workers with upper secondary education and 82,495 workers with less than upper secondary education.

jobs. We argue that the most plausible explanation for lower job displacement rates at FDI-expanding firms indeed is the FDI expansion itself. To make the case, we investigate main competing hypotheses that might give rise to a similar job-saving 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 controlled for a possibly large set of such predictors in Section 5. In this Section, we perform a series of robustness checks to investigate two more critical competing hypotheses: First, firms might acquire an owner-

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 (.004)***	-.041 (.019)**	-.051 (.019)***	-.022 (.024)
APD	-.041 (.005)***	-.042 (.021)*	-.018 (.027)	-.012 (.043)
CEE	-.049 (.005)***	-.022 (.024)	-.023 (.034)	-.026 (.025)
EMU	-.036 (.004)***	-.026 (.019)	-.021 (.020)	-.011 (.016)
OIN	-.036 (.005)***	-.017 (.026)	-.020 (.019)	-.023 (.022)
		BLUE-COLLAR WORKERS		
WW	-.045 (.004)***	-.016 (.006)***	-.035 (.006)***	-.023 (.006)***
APD	-.045 (.005)***	-.008 (.009)	-.021 (.009)**	-.022 (.009)**
CEE	-.044 (.004)***	-.017 (.007)**	-.011 (.008)	-.009 (.008)
EMU	-.051 (.004)***	-.044 (.009)***	-.037 (.008)***	-.037 (.008)***
OIN	-.036 (.004)***	-.010 (.011)	.004 (.012)	.007 (.013)

Standard errors (in parentheses): * significance at ten, ** five, *** one percent.
Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing establishments. Number of observations: 37,981 white-collar and 55,166 blue-collar workers.

ship advantage and simultaneously expand FDI, but save jobs 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 save domestic jobs. 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 probe the plausibility of the latter competing hypothesis.

6.1 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 saved domestic jobs to foreign expansions. An unobserved confounding factor could be that firms acquire an ownership advantage over the course of the treatment year and therefore save domestic jobs, simultaneously expanding FDI. We use Rosenbaum (2002) bounds to estimate how large the effect of any unobserved confounding factor would have to be to overturn our ATT estimate.

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. However, 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 an unmeasured variable must influence the selection process in order to undermine the implications of our matching analysis.

We briefly 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:

$$p\,(\mathbf{x}_i) = Pr(d_i\!=\!1|\mathbf{x}_i) = F(\beta\mathbf{x}_i + \gamma u_i), \tag{7}$$

where u_i is the unobserved variable of concern (the newly acquired ownership advantage) and γ is the effect of u_i on the treatment probability. Clearly, 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 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

$$\frac{\frac{p\,(\mathbf{x}_i)}{1-p\,(\mathbf{x}_i)}}{\frac{p\,(\mathbf{x}_j)}{1-p\,(\mathbf{x}_j)}} = \frac{p\,(\mathbf{x}_i)(1-p\,(\mathbf{x}_j))}{p\,(\mathbf{x}_j)(1-p\,(\mathbf{x}_i))} = \frac{\exp\,(\beta\mathbf{x}_i + \gamma u_i)}{\exp\,(\beta\mathbf{x}_j + \gamma u_j)} = \exp[\gamma(u_i - u_j)]. \tag{8}$$

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 now the task of sensitivity analysis to evaluate how inference about the treatment effect is altered by changing the values of γ and $(u_i - u_j)$.

We assume for the sake of simplicity that the unobserved covariate is a dummy variable with $u_i \in \{0, 1\}$ (indicating the acquisition of an ownership advantage). Rosenbaum (2002) shows that equation (8) then implies the following bounds on the ratio of the odds that either of the two matched jobs will receive treatment:

$$\frac{1}{e^{\gamma}} \leq \frac{p(\mathbf{x}_i)}{p(\mathbf{x}_j)} \frac{(1 - p(\mathbf{x}_j))}{(1 - p(\mathbf{x}_i))} \leq e^{\gamma}. \tag{9}$$

The two matched jobs have the same probability of being treated only if $e^{\gamma} = 1$. If $e^{\gamma} = 2$, 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 4.

We compute critical values of e^{γ} based on the Mantel and Haenszel (1959) test statistic, as suggested by Rosenbaum (2002). The Mantel and Haenszel (1959) test statistic assesses the strength of hidden bias that would be necessary to overturn our ATT estimate. For details on the Mantel and Haenszel (1959) test statistic, see Appendix C. We perform a sensitivity analysis for all statistically significant ATT effects. For this purpose, we gradually increase the level of e^{γ} until inference about the treatment effect is overturned.

We find that the critical value of e^{γ}, for which the statistically significant ATT effects in Table 4 would become statistically indistinguishable from zero, varies between $e^{\gamma} = 1.15$ and $e^{\gamma} = 1.25$. Consider the effect of employment expansions in CEE under specification 4, for instance. We find the critical value of e^{γ} 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 $e^{\gamma} = 1.25$ 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 $e^{\gamma} = 1.25$ only means that the unobserved variable, such as a newly acquired ownership advantage, would need to have an odds ratio of 1.25 to almost perfectly determine the outcome for every matched job pair and thus overturn our ATT estimate.

Table 2 gives an idea of what an odds ratio of 1.25 on a binary unobserved variable compares to. The coefficient on the fraction of workers with upper-secondary schooling or more in the establishment'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 upper-secondary schooled workers from zero to 100 percent in the mean establishment's workforce. We consider it implausible that a newly acquired ownership advantage, or any other factor outside our rich list of regressors, would exert such strong an impact. We therefore consider the statistically significant ATT treatment effects robust to hidden bias.

6.2 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 add 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.

Table 7 reports the results of this exercise for foreign-employment expansions anywhere worldwide under specification 4. Not a single coefficient on the concomitant variables is statistically different from zero. We do not report coefficients for ODV, OWE and RCA; they too are not statistically significant. We conclude that the most plausible explanation for lower displacement rates at FDI-expanding firms is their FDI expansion itself.

6.3 Further robustness checks

We perform a series of additional robustness checks under alternative control-group and treatment definitions to corroborate the plausibility of our hypothesis that foreign FDI expansions save domestic jobs.

Table 7: CONCOMITANT VARIABLES

	Replication regression ATT	Replication regression Std.Err.	Regression with controls ATT	Regression with controls Std.Err.
	(1)	(2)	(3)	(4)
WW treatment effect	-.026	.004***	-.021	.004***
Change of intermediate-goods imports 2000-01 from region				
APD			-.015	.020
CEE			.010	.056
EMU			.001	.014
OIN			.025	.067
Change of final-goods imports 2000-01 from region				
APD			-.002	.003
CEE			-.002	.007
EMU			-.005	.013
OIN			-.013	.018
Change of exports 2000-01 to region				
APD			-.007	.017
CEE			.008	.060
EMU			.0002	.012
OIN			-.004	.013
Obs.	36,140	36,140	36,140	36,140

Standard errors (in parentheses): * significance at ten, ** five, *** one percent. *Sources*: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing establishments. Regression on matched sample, including a constant. Changes in imports and exports at *NACE* 2-digit sector level.

Fixing the control group for treatment. In our regional specifications, firms that do not expand into region ℓ were classified as controls. So, whereas we did control for regional presence at time t–1, we did not exclude the possibility that MNEs who do not expand in region ℓ are simultaneously expanding into other regions. To probe robustness with respect to this definition of the control group, we fix the control group to jobs at those firms who do not expand anywhere worldwide (the control group of the WW estimator). Table 8 shows the results under this control group definition. All point estimates continue to be negative: Foreign employment expansions tend to save jobs. The ATT estimates lose significance in some regions, however. Under

Table 8: ATT under WW Control Group

	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)
APD	-.050	-.035	-.020	-.014
	(.003)***	(.022)	(.022)	(.019)
CEE	-.050	-.031	-.030	-.048
	(.003)***	(.015)**	(.014)**	(.015)***
EMU	-.048	-.066	-.017	-.019
	(.003)***	(.015)***	(.019)	(.012)
OIN	-.040	-.042	-.017	-.018
	(.003)***	(.018)**	(.019)	(.021)

Standard errors (in parentheses): * significance at ten, ** five, *** one percent.
Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing establishments.

specification 4, only the ATT of employment expansions in CEE remains significant. It is somewhat smaller than under the less restricted control group (in Table 4) but as large in magnitude as the unconditional OLS estimate of the treatment effect.

Turnover as treatment. Measuring FDI in foreign employment terms is natural in our context where the outcome is domestic worker displacement. Turnover at foreign affiliates, however, is a sensible 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. Table 9 shows that all point estimates continue to be negative. Under specification 4, turnover expansions anywhere worldwide (WW) reduce the displacement rate of domestic workers by 3.8 percentage points. This ATT is considerably stronger than the comparable estimate of 2.6 percent in Table 4. When distinguishing by region of turnover expansion, however, ATT estimates lose statistical significance at conventional levels except for Overseas Industrialized countries (OIN). This finding is consistent with the hypothesis that turnover expansions matter more in high-income locations such as OIN where product-market seeking horizontal expansions

Table 9: ATT with Foreign Turnover as Treatment

	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	-.042	-.067	-.065	-.038
	(.003)***	(.011)***	(.012)***	(.011)***
APD	-.047	-.061	-.040	-.049
	(.003)***	(.032)*	(.032)	(.030)
CEE	-.039	-.053	-.020	-.016
	(.003)***	(.016)***	(.018)	(.017)
EMU	-.035	-.016	-.022	-.013
	(.003)***	(.009)*	(.009)**	(.009)
OIN	-.038	-.139	-.075	-.074
	(.003)***	(.022)***	(.020)***	(.018)***

Standard errors (in parentheses): * significance at ten, ** five, *** one percent.
Sources: Linked MIDI and BA data, $t = 2000$. 5% random sample of workers in FDI exposed and non-FDI exposed manufacturing establishments.

arguably prevail, whereas employment expansions matter mostly in locations with low factor costs where low-value turnover is associated with manufacturing cost savings.

Alternative treatment thresholds. In our final check, we investigate to what extent the magnitude of the foreign employment expansion matters for the ATT. We use several expansion thresholds to redefine the outward-FDI treatment increasingly restrictively with 1 percent, 5 percent, and 10 percent foreign employment expansions. We then re-estimate specification 4 under those redefined treatments. 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 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 displacement rates, regardless of the magnitude of the expansion.

Table 10: ATT for Varying Employment Expansion Thresholds

	OLS	Std. Err.	ATT	Std. Err.
	(1)	(2)	(3)	(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 establishments.

7 Conclusion

Do MNEs and their workers fare better when MNEs expand abroad than when they do not? In contrast to that question, much of the previous literature has asked whether international wage differentials affect MNE expansions and domestic labor demand, conditional on firm performance. We use a propensity-score matching method for various measures of a domestic job's exposure to parent-firm FDI. Our main finding is that, when allowing firm performance to change contrary to labor demand estimation based on cost functions, FDI expansions into most foreign regions significantly decrease the probability of domestic worker displacement. Our results consistently show that, relative to the displacement rates at non-expanding firms, MNEs' employment expansions anywhere worldwide significantly reduce the rate of domestic job losses by about two percentage points—or half the unconditional difference in displacement rates between foreign-employment expanding MNEs (with lower displacement rates) and non-expanding enterprises.

We perform several sensitivity checks and show that results are robust to various specifications, and to alternative control group and treatment definitions. We find that unobserved factors of hidden bias would have to be implausibly large to overturn our treatment effect estimates. 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 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. We conclude that the most plausible explanation for lower displacement rates at FDI-expanding firms is their FDI expansion itself.

This finding suggests that a reluctance of domestic MNEs to exploit international factor-cost differentials, or a failure to improve foreign product market access through FDI, would cost even more domestic jobs at MNEs than are being lost at MNEs to globalization in the absence of in-house labor offshoring.

Appendix

A Linked employer-employee data

Our goal is to 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 establishments in the BA worker database to the so-selected MARKUS firms for identification of all establishments of *FDI firms*, and to MIDI itself for identification of all those FDI reporting firms that are not part of a corporate group (stand-alone firms).

We link the data based on names and addresses. By law, German establishment names must include the firm name (but may by augmented with qualifiers). Before we start the string-matching routine, we remove clearly unrelated qualifiers (such as manager names, legal forms or municipalities) from establishment names, and non-significance bearing components from establishment and firm names, including the legal form, in order to compute a link-quality index on the basis of highly identifying name components. Our string-matching is implemented as a Perl script and 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, establishments that are perfect links to MARKUS or MIDI, i.e. establishment names that agree with firm names in every single letter. Second, establishments that are perfect non-links, i.e. establishment names that have no single word in common with any FDI-related MARKUS or MIDI firm. We drop all establishments with a link-quality index between zero and one from our sample, i.e. establishments whose name partially corresponds to an FDI firm name but not perfectly so. Those establishments cannot be told to be either treatment or control establishments without risk of misclassification.[14] The procedure leaves us with a distinct treatment group of FDI establishments and a control group of non-FDI establishments.

[14]The string-matching routine runs for several weeks, checking 3.8 million establishments against 65,000 *FDI firms*. It is infeasible to manually treat possible links with imperfect link-quality rates.

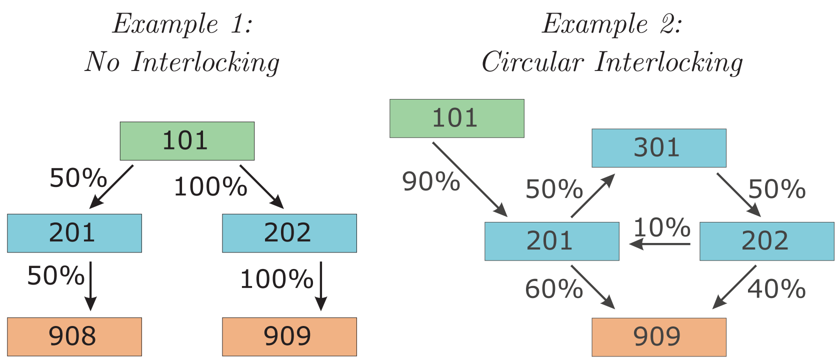

Figure 1: **Examples of Corporate Groups**

The BA establishment name file is from November 2002 and contains names of establishments that are no longer active so that we include exiting and entering establishments. To capture exits after 1999 is particularly important for us, because one margin of displacement is establishment 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 out on 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 1 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 establishments in firms 202 or 909 coincides with those of 101, 201 or 908 (but the establishment name is not an identical match to 101, 201 or 908), the establishments in firms 202 and 909 are discarded and neither enter the treatment nor the control group. If no single name component of establishments in firms 202 or 909 is the same as that of 101, 201 or 908, the establishment 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

Table 11: Ownership Inference

Affiliate-parent	Iteration (Length of Walk)					
pair	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	

be. If anything, however, the reduced difference would work against our outcome estimates. Moreover, interlocking (of which Example 2 of Figure 1 is a special case) limits the number of only laterally related firms.

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; graph-theoretic proofs 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 buy-backs increase the value of the 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 1). 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:[15] the first column takes the affiliate ID, the second column the parent ID, and the third column the effective ownership share. Table 11 shows this matrix for Example 2 in Figure 1 (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 causes the procedure to *cumulate and consolidate*

[15] In practice, 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*).

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 11 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 11 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.

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 11 shows iteration 100. The ownership share of 101 in 201 has converged to the exact 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 (foreign affiliate employment by foreign region, or turnover) from domestic affiliates to their ultimate domestic parents. Suppose firm 201 in Example 2 of Figure 1 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.

An alternative assignment scheme is 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 1 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 1 are not relevant for the choice of weighting scheme.

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 the FDI exposure of 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 observe outcome y for both treated and non-treated jobs. If y is unaffected by different treatment assignments, treatment d is said to have no effect. If y is different for different assignments, then the treatment has some positive (or negative) effect. To be significant, the test statistic $t(d, y)$ of the treatment effect has to surpass a minimum significance level. The non-parametric Mantel-Haenszel (MH) test statistic compares the successful number of individuals in the treatment group against the same expected number under the null hypothesis that the treatment effect is zero.

We 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 displacement outcome, y_{0s} is the number of non-treated jobs with a displacement outcome, and y_s is the number of total displacements in stratum s. The MH test-statistic Q_{MH} asymptotes the standard normal distribution and is given by

$$Q_{MH} = \frac{|y_1 - \sum_{s=1}^{S} E(y_{1s})| - .5}{\sqrt{\sum_{s=1}^{S} Var(y_{1s})}} = \frac{|y_1 - \sum_{s=1}^{S} (\frac{N_{1s} y_s}{N_s})| - .5}{\sqrt{\sum_{s=1}^{S} \frac{N_{1s} N_{0s} y_s (N_s - y_s)}{N_s^2 (N_s - 1)}}}. \tag{C1}$$

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:

$$Q_{MH}^{+} = \frac{|y_1 - \sum_{s=1}^{S} \widetilde{E}_s^{+}| - .5}{\sqrt{\sum_{s=1}^{S} Var(\widetilde{E}_s^{+})}} \tag{C2}$$

and

$$Q_{MH}^{-} = \frac{|y_1 - \sum_{s=1}^{S} \widetilde{E}_s^{-}| - .5}{\sqrt{\sum_{s=1}^{S} Var(\widetilde{E}_s^{-})}}, \tag{C3}$$

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.[16]

[16]The large sample approximation of $\widetilde{E}_s^{+}$ is the unique root of the quadratic equation $\widetilde{E}_s^2(e^{\gamma} - 1) - \widetilde{E}_s[(e^{\gamma} - 1)(N_{1s} + y_s) + N_s] + e^{\gamma} y_s N_{1s}$, with the addition of $max(0, y_s + N_{1s} - N_s \leq \widetilde{E_s} \leq min(y_s, N_{1s}))$ to decide which root to use. $\widetilde{E}_s^{-}$ is determined by replacing e^{γ} with $\frac{1}{e^{\gamma}}$. The large sample approximation of the variance is $Var(\widetilde{E}_s) = \left(\frac{1}{\widetilde{E}_s} + \frac{1}{y_s - \widetilde{E}_s} + \frac{1}{N_{1s} - \widetilde{E}_s} + \frac{1}{N_s - y_s - N_{1s} + \widetilde{E}_s}\right)^{-1}$.

Table 12: Raw displacement 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 14): 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 displacement 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 14): 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 14: Regions

Region codes	Description
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 countries including Canada, Japan, USA, Australia, New Zealand as well as Iceland and Greenland

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