Offshoring, Firm Performance and Plant-level Employment – Identifying Productivity and Downsizing Effects

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Abstract:
This paper examines the channels through which offshoring affects employment in a representative sample of German establishments, using a difference-in-differences matching approach. Offshoring establishments are identified by an increase in the share of foreign to total inputs. We find that an average offshoring establishment has higher employment, higher productivity, and higher domestic and foreign market share than if it did not engage in offshoring. Furthermore, its production depth remains unchanged; indicating that offshoring predominantly operates through a substitution of domestic for foreign suppliers, rather than through a reduction of home production. This result enables us to isolate a positive productivity effect from offshoring on employment. However, employment in an establishment decreases - relative to its counterfactual - when it simultaneously engages in offshoring and restructuring of the home plant. Therefore, we are also able to isolate a negative downsizing effect of offshoring on employment.

Key words: offshoring, export performance, employment, difference-in-differences matching estimator, stable unit treatment value assumption.

JEL classification: F16, F23, C21.

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1) Introduction

It is probably fair to say that the effects of the growing international fragmentation of production chains on home country labor markets are still not fully understood. Over the past decades firms in industrialized countries have increasingly engaged in offshoring, by either relocating low-skilled labor intensive production steps to foreign affiliates (vertical FDI) or by buying intermediate inputs from unaffiliated foreign suppliers (international outsourcing).[1] In theory a higher degree of offshoring might have positive or negative effects on a plant's employment. Positive employment effects could arise if cost savings rendered firms more competitive and increased their market share worldwide, negative effects could result from downsizing and relocation of production abroad.[2] Which channel dominates is ultimately an empirical question.

A number of recent empirical studies have investigated the effects of offshoring on home country employment. Studies based on macro-data tend to find insignificant or small negative employment effects.[3] Analyses relying on micro-data entail mixed results – some find positive and others negative employment effects.[4] We suggest that contradictory results should not come surprising, since existing studies have not been able to disentangle the various channels through which offshoring can affect employment either positively or negatively. This is the aim of this paper.

On the methodological side, the preferred strategy is to use micro-data, since macro-data analyses suffer from aggregation bias, lack of appropriate control variables for firms and workers, and self-selection effects. Nevertheless most micro-studies still rely on industry-level measures of offshoring, using intermediate goods trade from input-output tables.[5] Offshoring measures at the industry-level cannot account for heterogeneity of firms with

1 We follow Helpman (2006) in defining offshoring as comprising both vertical FDI and international outsourcing.
2 The productivity effect of offshoring on employment plays a prominent role in recent offshoring models such as Kohler (2004) and Grossman and Rossi-Hansberg (2008).
3 See, for instance, Feenstra and Hanson (1996, 1999), Slaughter (2001), Geishecker (2002), Hijzen, Görg, and Hine (2005), Hsieh and Woo (2005), Egger and Egger (2003, 2005), and Hijzen (2007), where some of these studies focus on skill upgrading rather than net employment effects to test vertical FDI theory (Helpman, 1984, Venables, 1999).
4 Micro-level evidence of labor market effects from offshoring are provided inter alia by Desai, Foley and Hines (2009), Egger, Pfaffermayer and Weber (2007), Geishecker (2006, 2008), Geishecker and Görg (2008), Harrison and McMillan (2008) and Marin (2006). These studies typically find some evidence that offshoring leads to changes of the relative demand of labor, or a decreasing demand for labor across all skill types, or an increase in income inequality.
5 See, for instance, Geishecker and Görg (2008), Geishecker (2002, 2006, 2008) and Munch and Skaksen (2009).

respect to their use of intermediate inputs within an industry, nor can they help to disentangle different effects of offshoring on employment.[6]

An innovation of this paper is that we capture offshoring more precisely and at the same time broadly: We offer a plant level concept that covers both vertical FDI and international outsourcing. This approach has several advantages: it opens the door to identify channels that determine the employment effect of offshoring on a particular establishment and allows us to distinguish between direct employment effects from downsizing and indirect employment effects from productivity gains through offshoring. Also it allows testing for the differential effect of offshoring by applying difference-in-differences matching techniques, a non-parametric estimator that is robust to non-linearity and heterogeneity of relations across individuals and therefore generalizes regression analysis.[7] On the methodological side, this paper contributes by offering an alternative to the stable unit treatment value assumption that hitherto excludes general equilibrium effects in micro-data analysis (Heckman, Lochner, and Tabner, 1998). We achieve this by assuming that general equilibrium effects do not depend on the actions of particular agents. We can then identify a relative causal effect of treatment when an average agent undergoes treatment relative to what this agent had obtained had she not chosen treatment, conditional on the general equilibrium effect that actually took place during the data period.

We find a statistically and economically significant positive employment effect of an increase in the foreign intermediate input share in total inputs (offshoring) on the domestic plant. We show that offshoring does not affect production depth on average, hence, offshoring predominantly substitutes domestic for foreign *suppliers* rather than replacing own production by foreign supply. Our study finds that offshoring plants increase their average labor productivity, improving their competitiveness, and increase their domestic and foreign market share against “twin”-firms that do not offshore. Hence, we have identified the productivity channel through which offshoring affects employment of a plant, while the downsizing

[6] Some studies use firm-level measures of FDI rather than offshoring and others use some firm-level measure of offshoring, but do not address their impact on net employment. E.g. Barba-Navaretti and Castellani (2004), Debaere, Lee and Lee (2006) and Sethupathy (2008) investigate the impact of outward FDI as measured by becoming a multinational company, i.e. changing their investment status, or by having a foreign affiliate. Hijzen, Inui and Todo (2007) offer a firm-level measure of offshoring by using the value of subcontracting to foreign providers. Based on OLS and SGMM estimates, Hijzen, Inui and Todo (2007) find a positive effect of international outsourcing and vertical FDI on firm productivity. In a similar vein, Defevre and Toubal (2007) use firm-level data to investigate, whether a firm’s (foreign) sourcing mode depends on its total factor productivity.

[7] Angrist and Pischke (2009) show that regression analysis is a particular matching estimator with specific weighting function.

channel is shut off in the average offshoring plant. In addition, we identify also the downsizing channel separately by looking at the employment effects of those plants that simultaneously increase their share of foreign intermediate inputs to total inputs and undergo major restructuring by shutting down, selling-off or spinning-off parts of the plant. In this case, the downsizing effect on employment dominates the productivity effect, yielding less employment on average relative to the average employment in the matched control group. Our analysis also suggests that there might be an unobservable negative effect of offshoring on employment of domestic suppliers if they are substituted by foreign ones.[8] These results are robust, among others, to a careful investigation of whether self-selection into offshoring confounds these treatment effects.

There are a few related studies, namely Becker and Muendler (2008a, 2008b) and Sethupathy (2008), which investigate the effects of vertical FDI – rather than offshoring – on home country employment. Becker and Muendler (2008a) find that multinationals expanding abroad experience fewer worker separations at home, and Becker and Muendler (2008b) find a decrease in net employment due to a market-share switching effect: offshoring plants gain market share and increase employment while other domestic plants competing with the offshoring plants on the goods market loose market share and decrease employment. Sethupathy (2008) formalizes this market switching effect in a heterogeneous firm model with offshoring and tests it on data of U.S. FDI in Mexico during the incidence of an exchange rate and legislative shock in comparison to U.S. FDI in other Latin American countries. We differ from these studies in several ways. Our approach is broader since offshoring takes place whenever a plant increases the share of foreign intermediate inputs in total inputs. Thus offshoring can be a result of vertical FDI but could also result from increased sourcing of intermediate inputs from unaffiliated foreign suppliers. Also, we focus on a switching effect between domestic and foreign suppliers rather than switching of market shares between offshoring and non-offshoring firms on the final goods market. Finally, we are able to isolate the effect of offshoring on employment through downsizing and the productivity effect of offshoring on employment contrary to the studies cited above.

The paper is organized as follows: Section 2 gives a framework for a micro-data analysis of offshoring. Section 3 discusses briefly the data set and section 4 outlines the empirical methodology. Section 5 provides the estimation of the propensity score of offshoring and

[8] Unfortunately, this effect cannot be traced in existing data, because domestic suppliers of offshoring firms cannot be tracked and aggregate employment estimates are therefore possibly upward biased.

reports various auxiliary tests. Section 6 presents the main estimations of the average treatment effect on the treated of offshoring. Section 7 offers some extensions, before the last section concludes.

2) Framework for the identification of channels

As noted above, our main objective is to identify different channels through which offshoring affects employment. This requires an appropriate approach to capture the offshoring event. In the early literature offshoring was measured as the increase of the share of imported intermediate inputs in the total purchase of non-energy materials of an industry (Feenstra and Hanson, 1996, 1999). This measure of offshoring is also employed in some recent micro-data studies like for instance Geishecker (2002, 2006, 2008), Geishecker and Görg (2008) and Munch and Skaksen (2009), where the dependent variables are plant- or employee-specific, but the explanatory variable of main interest, the offshoring variable, remains industry-specific. In contrast, we propose a proxy for offshoring at the plant-level by measuring the increase of imports in intermediate goods of an establishment from all other sectors. Hence, our measure is closest in spirit to the broad definition in Feenstra and Hanson (1996, 1999), but more precise in practice by capturing firm heterogeneity.[9] While this opens the door for a more thorough microeconometric analysis, the theoretical underpinnings of such an analysis differ from similar industry studies, because the firm relations *within* an industry have to be taken into account.

Offshoring of an establishment may either substitute production at home for imports from abroad[10] or it may replace domestic intermediate input demand with foreign one. *Direct employment effects via downsizing* are confined to the former case, where the establishment under consideration exports jobs to a foreign country by relocating its own production abroad or replacing domestic production by purchases from abroad. In the latter case, the establishment does not experience a direct employment effect, because no production is relocated from its own plant.[11] In contrast to that, *indirect* employment effects from offshoring can be expected independently of whether own production or domestic supply is

[9] Feenstra and Hanson (1996, 1999) distinguish between two forms of international outsourcing. While the broad measure considers any imported intermediate inputs, the narrow measure confines to imported intermediate inputs from the same two-digit industry.

[10] To analyze employment effects of offshoring, we do not need to differentiate between buying intermediate goods through arms-length trade or from an own plant abroad.

[11] Instead, negative domestic employment effects might materialize among domestic suppliers. We will come back to this issue below.

substituted for foreign supply, because the offshoring decision is motivated by anticipated expected cost savings. Hence, firms that offshore gain on average (price) competitiveness relative to firms that do not offshore (see, for instance, Kohler, 2004, Grossman and Rossi-Hansberg, 2008, and Sethupathy, 2008). This competitive advantage tends to increase the offshorers' market share at home and therefore their local sales, which in turn boosts demand for labor. Similarly, offshoring firms expand their market share abroad, exemplified by increasing exports that again stimulate labor demand. We will call this causality chain the *productivity effect* of offshoring by an establishment on its employment.

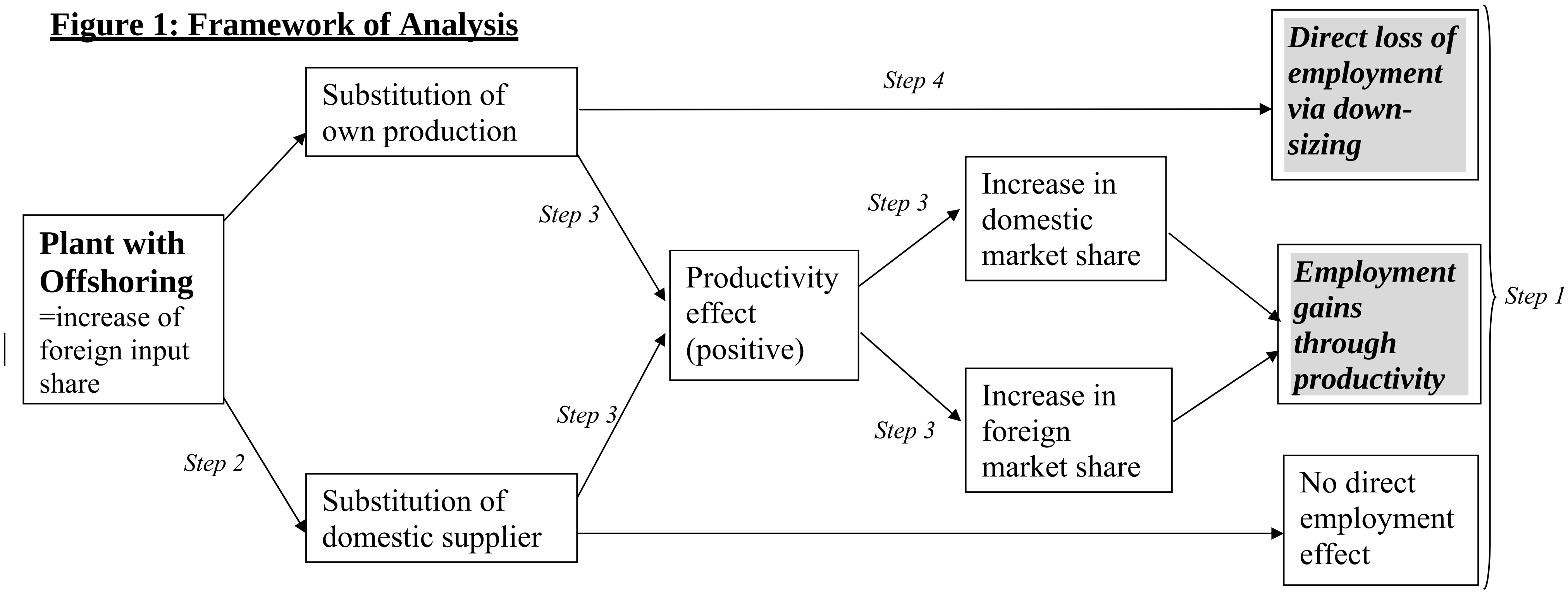
Overall, offshoring of an establishment does not only affect its own employment but potentially also alters the employment of other establishments in two ways. First, if an establishment is a supplier to an offshoring establishment who is substituted by a foreign one, it will have an employment loss through fall in demand. Second, if a plant is a competitor who loses competitiveness relative to firms that offshore, there is an employment loss through fall in market share (Becker and Muendler, 2008b, Sethupathy, 2008).[12]

Figure 1 summarizes these arguments. Offshoring of an establishment causes a direct employment effect via downsizing if own production is substituted for foreign supply, thereby cutting employment at home. Still, there may be a positive employment effect of offshoring on an establishment's own employment if the productivity gain from offshoring increases the establishment's competitiveness at home and/or abroad, thereby inducing employment gains through firm growth. Instead, if offshoring substitutes a domestic for a foreign supplier, we do not expect a direct employment effect in an establishment that shores off, since there is no change of its production depth. Nevertheless, these establishments a priori save costs analogously to a productivity gain[13] from offshoring. Likewise, there will be a gain in competitiveness, market share at home and/or abroad and an employment gain through the productivity effect. Importantly, offshoring that substitutes domestic for foreign supply will allow us identifying the productivity effect on employment, since the direct employment effect via downsizing is not present.

[12] Since we neither know the competitor(s) of an offshoring establishment nor its domestic supplier(s), we will focus in our analysis on the identification of different theoretical channels through which offshoring affects the *own* employment of an establishment rather than aiming at the quantification of an overall employment effect of offshoring.

[13] An increase in productivity implies that more output can be produced for a given quantity of input factors. For a given budget for input factors more value added can be generated. Similarly, declining costs for input factors allow for buying more inputs for a given budget, potentially boosting output.

Figure 1: Framework of Analysis



To identify the two channels of employment effects on our plant data, we proceed in our empirical analysis in four steps:

1) assessment of overall employment effect from offshoring on plants that undertake offshoring relative to those which do not,
2) analysis of which type of offshoring - substitution for domestic supply or substitution for own production – is dominant,
3) identification of productivity channel, and
4) measurement of direct employment effect of downsizing due to offshoring.

Table 1 explains the roadmap for the empirical analysis, which follows these four steps to identify the two theoretical channels by which offshoring of an establishment is affecting its own employment. We have a representative sample of establishments i at time t.

Table 1: Steps of Empirical Analysis

First step: Identification of employment effect
$Offshoring_i \rightarrow \Delta\ Employment_i$
Second step: Finding prevalent channel
$Offshoring_i \rightarrow \Delta\ Intermediate\_Inputs_i$
Third step: Identification of productivity effect
$Offshoring_i \rightarrow \Delta\ Productivity_i$
$Offshoring_i \rightarrow \Delta\ Sales_i$
$Offshoring_i \rightarrow \Delta\ Exports_i$
Fourth step: Identification of direct employment effect via downsizing
$Offshoring\_cum\_Restructuring_i \rightarrow \Delta\ Employment_i$

Notes: Indices are plants i at time t. → is causality relation; Δ is time difference operator;

In a first step, we assess the overall effect of an increase in offshoring (*Offshoring$_i$*) on an establishment's employment change (*ΔEmployment$_i$*) relative to similar establishments that do not offshore. In a second step, we examine whether one of the two types of offshoring – substitution of own production for foreign supply versus substitution of domestic supply for foreign supply - is prevalent. For this purpose, we investigate the causal link between offshoring and its production depth (*ΔIntermediate_Inputs$_i$*), measured as the sum of domestic and foreign material costs relative to total turnover. If an increase in the share of foreign sourcing (offshoring) leads to a decrease of production depth, then own production is predominantly substituted for foreign supply. If there is, instead, no significant change in production depth going along with offshoring, then substitution of domestic for foreign supply is dominant. In a third step, we identify the productivity channel. We investigate the effect of offshoring on average labor productivity (*ΔProductivity*$_i$).[14] Beyond that, we expect that an increase of productivity improves the competitiveness of an offshoring firm, leading to an increase of market share both at home and abroad. Such a positive productivity channel might be the underlying mechanism that explains the positive employment effects of offshoring. Hence, we investigate the causal effect of an increase in offshoring on the change in sales (*ΔSales*$_i$) and exports (*ΔExports*$_i$) of a plant to identify indirectly the productivity channel. In the last step, we isolate in a fourth step one extreme form of offshoring, namely those cases of offshoring that coincide with a partial closure of the plant (*Offshoring_cum_Restructuring*$_i$). Hence, we gauge the causal link of this form of offshoring on employment, [15] which is most prone to negative employment effects.[16]

3) Empirical methodology

To identify the two channels through which the decision to offshore has an influence on the employment of an offshoring plant, we employ a difference-in-differences matching technique. The basic idea of a matching estimator is to compare outcomes of establishments whose pretreatment characteristics have made it equally likely to offshore or not, implying that treatment is "purely random" for similar establishments. First, the probability that an establishment increases offshoring is estimated. Then one compares the average change in the outcome variable before and after treatment of the establishments that experience treatment with those that do not receive treatment but have (almost) the same probability of doing so (average treatment effect on the treated).

Matching estimation depends on three crucial assumptions: the conditional mean independence assumption, the overlap condition, and the assumption that observations are

[14] Unfortunately, the IAB-establishment panel does not report the plant's capital stock, which makes any attempt to measure total factor productivity prone to serious measurement errors. For this reason, we prefer to focus on the average labor productivity.

[15] Here, the implicit assumption is that any closure of parts of plant that coincides with offshoring is due to offshoring. While it appears reasonable that these two events are positively correlated , this need not necessarily be the case. Hence, our employment effects of this form of offshoring will be conservative, i.e. if offshoring and closure are not perfectly correlated in our data set the (negative) employment effects are overestimated. Instead, we underestimate the employment effect, since we do not have data on complete closure of plants.

[16] Note that negative employment effects of offshoring in the presence of partial plant closure are not a tautological relation, because there may still be an increase of employment through the productivity effect and gain of market share, boosting occupations other than those of the closed division. For example, Statistisches Bundesamt (2008) reports a self-assessment of 9361 German firms with more than 100 employees in manufacturing and services without the financial sector from the year 2007 that 188 600 jobs were scrapped due to plant relocation during the last decade while 105 500 new jobs were created in the home plant. Moreover, the new jobs were mostly high-skilled while the destroyed jobs were mostly low-skilled. Finally, 84% of all firms that relocated plants claim that they gained competitiveness, 77.4% claim that they saved labor costs.

independent draws from a random sample. The last assumption can be in conflict with general equilibrium effects if in our case the choice of offshoring of one particular establishment has an impact on the outcome of some other establishment that does not pursue offshoring.[17] To avoid such a complication, impacts of treatment on the control group are usually excluded by assumption.[18] We suggest, instead, a way to allow for a special form of general equilibrium effects without violating the assumption on the independence of observations. Suppose that general equilibrium effects depend only on a vector of aggregate measures M_t such as the share of establishments that decide in period t to outsource next period. This will be the case whenever the general equilibrium effect is based on price-changes in a competitive market. Any estimate of the average treatment effect on the treated is then conditional on the realized value of this aggregate measure during the data period.

To formalize the underlying assumptions of such a modified difference-in-differences estimator, we assume a general data generating process on the outcome variable y_{it} of establishment *i* at time $t = \{0,1\}$, where 0 is the period before treatment (offshoring) and 1 the period after treatment offshoring, and the outcome variable will be employment, sales, exports or intermediate input share of sales in our empirical analysis:

$$\begin{aligned} y_{it}^{T} &= g(x_{i0})t + f^{T}(x_{i0}, M_0)t + \delta_{it}^{T}\left(M_0\right)t + \gamma_i + U_{it}t + \varepsilon_{it} \\ y_{it}^{NT} &= g(x_{i0})t + f^{NT}(x_{i0}, M_0)t + \delta_{it}^{NT}\left(M_0\right)t + \gamma_i + U_{it}t + \varepsilon_{it}, \end{aligned}$$

where y_{it}^{T} denotes the outcome with the treatment (offshoring), and the outcome y_{it}^{NT} without treatment. The function $g(x_{i0})t$ captures the growth trend dependent on observable predetermined treatment selection characteristics x_{i0} but independent of the treatment, $f^{T}(x_{i0}, M_0)$ stands for the (possibly heterogeneous) causal impact of the treatment choice on the outcome dependent on characteristics x_{i0}.[19] $\delta_{it}^{T}\left(M_0\right)$ is the heterogeneous causal impact of treatment unobservable to the econometrician but possibly known to the firm, γ_i are time-

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[17] This can be either due to market switching effects or due to substitution effects on domestic suppliers.

[18] See Rosenbaum and Rubin (1983), calling this exclusion the stable unit treatment value assumption, and Heckman, Lochner and Tabner (1998) for evaluating the bias from applying matching estimators in the presence of general equilibrium effects by comparing estimates with a calibrated macro-model. Angelucci and de Giorgi (2009) capture general equilibrium income effects of the Mexican welfare program Progresa by randomizing by village rather than by individual. However, this confines the general equilibrium effect to a village and identification requires having data on many villages. If the general equilibrium effect were confined to a country, then one would need randomized micro-data on many countries according to their method. We discuss instead the case, where a general equilibrium effect is not confined to a subgroup of the sample.

[19] Heckman, Ishimura and Todd (1998) distinguish control variables from selection variables in their data generating process designed for cross-section data. However, Angrist and Pischke (2009) argue that difference-in-differences estimators are consistent even when excluding control variables.

invariant observable or unobservable characteristics that influence the outcome, U_{it} are time-variant unobservable characteristics that influence outcome independent of treatment. Without loss of generality, the unobservable random variables and the white noise error ε_{it} have an unconditional expected value of zero.

Importantly, because of the alternative set of assumptions introduced above, we can allow treatment having a (negative) impact on establishments that do not offshore in dependence of the mass of firms that decided in period 0 to offshore in period 1, M_0, and of observable characteristics $f^{NT}(x_{i0}, M_0)$ or unobservable characteristics $\delta_{it}^{NT}(M_0)$.

The difference in outcome of firm i between choosing to offshore and choosing not to offshore, conditional on the mass of firms M_0 that offshore, is then simply $f^{T}(x_{i0}, M_0) + \delta_{i1}^{T}(M_0) - f^{NT}(x_{i1}, M_0) - \delta_{i1}^{NT}(M_0)$. This is not observable, because one and the same establishment *i* is either observed when undertaking offshoring or when not undertaking offshoring, but not in both circumstances at the same time. For this reason, one can estimate at best an expected average difference in outcomes over all establishments. In addition, we need to condition on the size of the realized general equilibrium effect in the data period by conditioning on the mass of firms that actually decided to outsource, M_0. We confine our analysis to one causality measure, namely the average treatment effect on the treated (ATT). This is defined as the average causal effect of all observations that undergo treatment:

$$E\left[y_{i1}^{T} - y_{i1}^{NT} \middle| D_{i1} = 1, M_0\right] = E\left[f^{T}(x_{i0}, M_0) + \delta_{i1}^{T}(M_0) - f^{NT}(x_{i0}, M_0) - \delta_{i1}^{NT}(M_0) \middle| D_{i1} = 1, M_0\right],$$

where the treatment group indicator D_{it} is a binary variable, which takes the value of one in period 1, if offshoring actually takes place in an establishment, i.e. foreign input usage increases from period 0 to period 1, and zero otherwise. Importantly, this average causal effect is a relative measure. For example, a positive ATT on the outcome variable employment may mean that, on average, there are more jobs created than destroyed in offshoring firms and there is no impact on firms foregoing offshoring. But it may also mean that there is no positive employment effect on offshoring firms, but instead a negative employment effect on firms that have not offshored. Or it may be any combination of these two extreme cases. This relative measure is sufficient to identify theoretical channels through which treatment offshoring effects outcome employment, it is generally not sufficient to assess the net aggregate employment effect in the economy.[20]

The econometric problem addressed by the program evaluation literature consists of constructing a statistical counterpart to the unobservable counterfactual $E\left[f^{NT}(x_{i0}, M_0) + \delta_{i1}^{NT}(M_0) \middle| D_{i1} = 1, M_0\right]$. There are several estimators available. They all differ in the assumptions they invoke to obtain an estimate of the above term.

The difference-in-differences estimator is obtained from a regression of the change in the outcome variable on selection variables x_{i0} and the treatment variable D_{i1}:

$$\Delta y_{i1} = \beta_0 + \beta_1 x_{i0} + \beta_2 D_{i1} + \varepsilon_i ,$$

where $y_{it} = D_{it} \cdot y_{it}^{T} + (1 - D_{it}) \cdot y_{it}^{NT}$ is the observed outcome variable. The estimated treatment effect on the treated is the coefficient β_2 under the assumptions that i) there are no heterogeneous treatment effects based on observable characteristics, ii) the observable time trend determinants x_{i0} are exogenous, (iii) observable time trend determinants are linear in functional form, (iv) there is a common average time trend in outcomes conditional on observable characteristics x_{i0} among treated and untreated observations.[21] The latter implies that there is no self-selection of treatment choice according to unobservable time trend determinants ($E\left[U_{i1} \middle| D_{i1} = 1, x_{i0}, M_0\right] = 0$) and unobservable heterogeneous causal effects ($E\left[\delta_{i1}^{T}(M_0) - \delta_{i1}^{NT}(M_0) \middle| D_{i1} = 1, x_{i0}, M_0\right] = 0$).

The difference-in-differences matching estimator relaxes the assumptions (i)-(iii) but requires instead the overlap condition that the treatment decision of offshoring has probability strictly smaller than 1 for each treated observation. The latter assumption is fulfilled in our case and henceforth ignored. Under the assumption of difference-in-differences mean independence (see Heckman, Ichimura and Todd, 1997), i.e.

$$E\left[\Delta y_{i1} \middle| x_{i0}, D_{i1} = 0, M_0\right] = E\left[\Delta y_{i1} \middle| x_{i0}, D_{i1} = 1, M_0\right] = E\left[\Delta y_{i1} \middle| x_{i0}, M_0\right]$$

[20] Since Angrist and Pischke (2009) show that an OLS regression estimator is a matching estimator with specific weights, the same implication applies to regression analysis. We conjecture that it is a general property of micro-data as such rather than a feature of a particular estimator that aggregate employment effects in levels cannot be derived. The study of Angelucci and de Giorgi (2009) relaxes the assumption of the absence of general equilibrium effects in micro-data analysis by confining them to subpopulations.

[21] See Abadie (2005) for a discussion of this assumption and how it relaxes the conditional independence assumption in cross-section data.

the ATT is by the law of iterated expectations equal to:

$$ATT = E[\delta_x | D_{i1} = 1, M_0],$$

where $$\delta_x \equiv E[\Delta y_{i1} | x_{i0}, D_{i1} = 1, M_0] - E[\Delta y_{i1} | x_{i0}, D_{i1} = 0, M_0]$$

and expected values can be replaced by sample means due to some law of large numbers if observations are drawn independently from a population and some mild regularity conditions apply such as finite higher-order moments of y_{it} and x_{i0}.

Conditioning on x_{i0} is not practicable because of the curse of dimensionality. However, Rosenbaum and Rubin (1983) have shown that conditioning on x_{i0} can be replaced by conditioning on the propensity score, i.e. the probability that offshoring is chosen by an establishment, $P(D_{i1} = 1) = P(x_{i0}) \equiv p_i$.[22]

Two problems remain to obtain consistent estimates of the ATT. First, the propensity score needs to be estimated. We employ a logit-specification. Second, for each treatment observation the expected value of the change in outcome conditional on the same probability of offshoring among the establishments without offshoring needs to be found. However, since the propensity score is a continuous variable, two identical propensity scores are generally zero-probability events in a random sample. Hence, an estimate of the expectation of the change in outcome without treatment conditional on a value of the propensity has to include control group observations with similar rather than identical propensity scores, giving rise to potential bias (Rosenbaum and Rubin, 1983).

Various propensity score matching estimators basically differ in their way of measuring similarity of the propensity scores, the set of neighbors included in the matched control group and the weights each of them obtains, respectively.[23] In general, such a difference-in-differences matching estimator of the ATT can be formalized according to Heckman, Ichimura and Todd (1997) in the following way:

$$\hat{\delta} = \sum_i D_{i1} \left[\Delta y_{i1} - \sum_j \left\{ \left(1 - D_{j1}\right) g\left(p_i, p_j\right) \Delta y_{j1} \right\} \right].$$

The non-parametric function *g(.)* determines in which way the observations of the control group will be weighted and, thereby, provides the counterfactual.

[22] Heckman, Ichimura and Todd (1998) compare the efficiency of conditioning kernel matching estimators on $p(x_{i0})$ rather than on x_{i0} and do not find any one of them dominating, but conjecture that the small sample efficiency of conditioning on the propensity score is superior.

[23] For a survey on alternative matching algorithms, see for instance Caliendo and Kopeinig (2008).

Our favorite estimator is a kernel matching algorithm given by[24]

$$g\left(p_i, p_j\right) = \frac{K\left(\left(p_j - p_i\right)/h\right)}{\sum_{j \in A(i)} K\left(\left(p_j - p_i\right)/h\right)}$$

with the Epanechnikov Kernel function *K(.)*, the set of control group observations $A\left(i\right) = \left\{ j \left\| p_i - p_j \right| < h \right\}$ and the bandwidth parameter h. This estimator includes a rather large number of control group observations in the calculation of the ATT, but matched control group members with propensity scores which are more distant to a treatment observation receive a smaller weight. Heckman, Ichimura and Todd (1998) have shown that the kernel density matching estimator of the ATT is consistent with an asymptotic normal distribution under some regularity conditions in addition to the matching assumptions even in the case when the propensity score is estimated.

The choice of the bandwidth h typically involves a trade-off. On the one hand, a relatively large bandwidth implies that some of the establishments that take part of the control group might be quite different in characteristics x_{i0} from the treated establishments, leading to a biased estimator (Rosenbaum and Rubin, 1983). On the other hand, variance of the ATT is expected to increase with a low bandwidth. An optimal trade-off between bias and efficiency can be found, using, for example, the cross-validation method (e.g. Cameron and Trivedi, 2005), which is computation intensive. For our purposes, it is sufficient to insure the robustness of the empirical results to different bandwidths.[25] We will present the results for bandwidth of 0.01, but our results are also robust to other choices.[26]

As a robustness check, we use also a k-nearest neighbor matching estimator, where only the k observations with the propensity score closest to each treatment observation are included in the matched control group such that:[27]

$$g\left(p_i, p_j\right) = \begin{cases} 1 & if\ \ j = \arg\min \left| p_i - p_j \right| \\ 0 & else \end{cases}$$

We choose k to be two. While such an estimator is inefficient, it has the smallest conditional bias from deviations of selection characteristics x_{i0} in treatment and matched control group (Dehejiha and Wahba, 2002).

[24] See, e.g. Heckman, Ichimura and Todd (1997).
[25] See, e.g., Heckman, Ichimura, Smith and Todd (1998) for such a heuristic approach.
[26] Results for the alternative bandwidths of 0.05 and 0.001 are very similar and available from the authors upon request.
[27] See Heckman, Ichimura and Todd (1997).

Next, we discuss, how to gauge the statistical significance of the treatment effect on the treated. A straightforward method common in the literature is to apply the bootstrap to calculate standard errors (see, e.g., Lechner, 2002, or Black and Smith, 2004). However, Abadie and Imbens (2008) formally show that standard errors obtained from bootstrapping with replacement are not valid in the case of nearest neighbor matching. Intuitively, the bootstrapped sample fails to replicate the distribution of the number of times a control group observation belongs to the group of nearest neighbors of any treatment group observation, because drawing with replacement implies that some observations from the data sample must end up several times in the bootstrapping sample while others do not at all. But then a control observation is nearest neighbor to each of the identical treatment observations, disproportionately increasing the number of times some control observations are nearest neighbors. Instead, Abadie and Imbens (2006) derive an analytical expression for the estimated asymptotic standard error for nearest neighbor estimators. In case of kernel matching estimators, Abadie and Imbens (2008) conjecture that bootstrapping yields valid inference. Hence, our standard errors for kernel matching estimators will be based on bootstrapping with 500 replications applying the STATA-modul PSMATCH2 from Leuven und Sianesi (2003), and our standard errors for nearest neighbor matching estimators are analytically derived in Abadie and Imbens (2006) and calculated using the STATA-modul NNMATCH from Abadie et al. (2004).

4) Data set

Our main data source constitutes the IAB Establishment Panel from the Institute for Employment Research (IAB).[28] This panel started in 1993 and included roughly 16,000 establishments nationwide in 2005 (see for instance Koelling, 2000; Bellmann, 1997). The IAB panel is drawn from a stratified sample of the establishments included in the employment statistics register, with the selection probabilities depending on the variation of the number of employees in the respective stratum. The stratum is defined over 16 industries, 10 categories of establishment size, and 16 German states (Länder). Large establishments are oversampled, but the sampling within each cell is random. Survey data is collected by professional interviewers of Infratest Sozialforschung on account of the German Institute of Employment Research. Participation of firms is voluntary but the response rate of more than 80% for

[28] The IAB-Establishment Panel data is confidential but not exclusive. They are available for non-commercial research by visiting the Research Data Centre (FDZ) of the Federal Employment Agency at the Institute of Employment Research in Nuremberg, Germany. For further information, we refer to http://fdz.iab.de/en.

repeatedly interviewed establishments is high. Our sample covers the period 1998 to 2004 and is centered around the three business years 1998, 2000 and 2002, where the establishments were asked about their use of imported intermediate goods in their production.[29] More precisely, we exploit information on, whether establishments have predominantly, partly or not at all received intermediate inputs, i.e. all raw materials and supplies purchased from other businesses or institutions from abroad. Our dataset includes manufacturing and non-manufacturing establishments, but we will provide a robustness check below that restrains the sample to the manufacturing sectors. Table A1 in the Appendix provides some summary statistics.

4.1) Outcome Variables

Several outcome variables y are considered, where y represents net employment, sales, exports, average labor productivity, and intermediate input share in sales. Let y^{1}_{it+s} be the outcome variable at time $t+s$, $s \geq 0$, following the offshoring event for those firms that offshored. We will consider different time horizons, with t=1 being equivalent to the first year, in which the offshoring activity has been completed. For instance, if an establishment reports a higher share of imported intermediate inputs in the year 2000 as compared to 1998, the offshoring event must have taken place during the 1998-1999 period and we measure the outcome variable at the end of 1999 (t=1). Since changes triggered by the offshoring event might not materialize immediately, estimations for t=2 and t=3 will be reported as well. We will analyze the following five outcome variables:

- *Employment:* Logarithm of total employment at plant-level.
- *Sales:* Logarithm of total turnover at plant-level.
- *Exports:* Ratio of total exports over total turnover at plant-level.
- *Productivity:* Logarithm of total turnover over total employment at plant-level.
- *Intermediate inputs*: Ratio of (domestic and foreign) intermediate inputs over total turnover at the plant-level.

4.2) Treatment variables

Our principal treatment variable *Offshoring* is defined as an establishment's increase in its share of imported intermediate inputs in overall intermediate inputs. Our binary variable $Offshoring_{it}$ takes the value of one, if the establishment experienced an increase in imported

[29] For simplification we refer here to the business years the data is covering. These questions were asked in the survey years 1999, 2001 and 2003.

intermediate goods, and zero otherwise. Our data allows us to measure qualitatively such an increase as an establishment's increase in its share of intermediate goods from abroad from „not at all“ to „partly“ or from „partly“ to „predominantly“ from the business years 1998 to 2000 and 2000 to 2002, respectively.[30] As discussed in Section 2) our offshoring definition is closest in the spirit to the definition of international outsourcing à la Feenstra and Hanson (1996, 1999), but our measure is a plant measure.

4.3) Selection variables

The variables included in the propensity score model to explain the probability of the offshoring event have to fulfill two requirements: i) influence both the participation decision and the outcome variable and ii) be unaffected itself by the treatment or its anticipation. For this reason, only time invariant or lagged variables are considered. The choice of our selection variables is motivated by the existing empirical and theoretical literature on offshoring. In particular, we use plant size, the average wage, the share of high skilled workers and indicators for the technology level and foreign-ownership. Log employment is our proxy for the size of the plant. The average wage captures an important fixed cost of the plant. Obviously, wages might also reflect differing skill-compositions at an establishment, with higher average wages indicating a higher share of better educated employees. But we explicitly control also for the share of high-skilled workers in the selection equation. According to Marin (2004), the intra-firm imports from Eastern Europe to German firms depend inter alia positively on the size of the parent firm and its R&D intensity. Yeaple (2005) shows that firms pursuing international activities tend to pay higher wages, have more skilled workers and employ more advanced technologies. Our technology variable allows investigating, whether those firms that exhibit a superior technology within an industry tend to incur more offshoring or not. Finally, we incorporate a foreign ownership dummy, since we expect this variable to be positively correlated with multinationals. For instance, Helpman, Melitz and Yeaple (2004) present evidence in favor of a higher productivity of multinationals relative to non-multinational exporters.[31]

[30] We pool the two time periods for which we are able to define offshoring in order to profit from efficiency gains. Pooling tests confirm that this empirical strategy is valid. Furthermore, an increase from "partly" to "predominantly" as compared to an increase from "not at all" to "partly" does not yield significantly different effects on the outcome variables. Hence, the results reported below will be based on the pooled sample.

[31] For instance, Grossman and Helpman (2004), Antras and Helpman (2004) and Helpman, Melitz and Yeaple (2004) document a sorting of firms in different (international) organizational forms, depending on their productivity. We have also experimented with average productivity and the level of exports, but these variables do not enter significantly due to their high correlation with our size measure. Furthermore, in the spirit of Heckman and Hotz (1989) and Imbens and Wooldridge (2009) we have also tested, whether the lagged outcome variables have some explanatory power, but none of them turn out significantly. Hence, we prefer to continue with the more parsimonious specification.

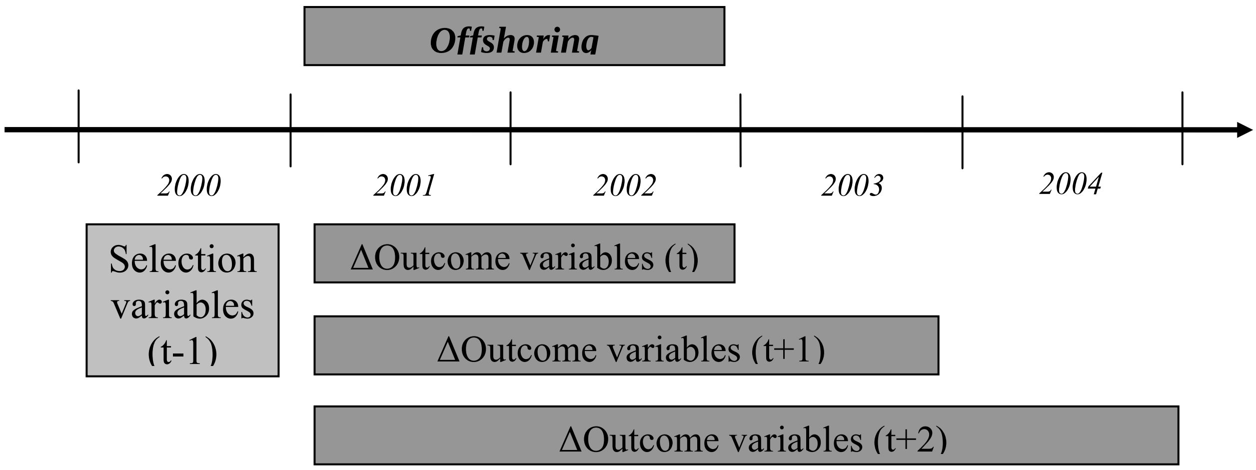
Hence, we include all time varying variables with a lag of one year:

- *Employment*$_{i,t-1}$: Logarithm of total employment at plant i in time t-1.
- *Wage_empl*$_{i,t-1}$: Logarithm of total wage per employee at plant i in time t-1.
- *Technology*$_{i,t-1}$: Dummy variable taking the value of one if the plant i uses state-of-the-art-technology or above-average technology in comparison to peer-group in time t-1.
- *High_skilled* $_{i,t-1}$: Share of high-skilled employees as percentage of total employees at plant i in time t-1.
- *Foreign:* Dummy variable taking the value of one if a foreign owner holds majority of plant i.
- *Time:* Dummy variable taking the value of one for the year 2002 and zero otherwise.

Finally, we control for industry-specific (D_B) and regional-specific effects (D_R). The error term υ is assumed to be independent of the explanatory variables and is assumed to follow a logistic distribution.

The following Figure 2 summarizes the timing in our data.

Figure 2: Timing in Data



Note: Figure 2 displays the timing of the empirical strategy for the difference-in-differences matching approach. The treatment variable *offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs in the business years 2000-2001 at the plant-level. The underlying questions for the construction of our offshoring variable are part of the survey year 2001 and 2003. Analogously, we are also able to measure the offshoring event for the business years 1999-2000. All time-invariant *selection variables* for the propensity score estimates are lagged by one period (t-1). The five Δoutcome variables are defined as *Δemployment* (logarithm of total employment), *Δintermediate_inputs* (ratio of total intermediate inputs over total turnover), *Δsales* (logarithm of total turnover), *Δexports* (ratio of total exports over total turnover) and *Δproductivity* (logarithm of total turnover over total employment), whereby we compute Δoutcome by subtracting the level of the outcome variable in the year 2000 from its level in the year 2002 (t), 2003 (t+1) and 2004 (t+2), respectively.

5) Auxiliary estimates and tests

Propensity Score

Column (1) of Table 2 reports our preferred logit specification. We find that the decision to offshore is positively and highly significantly correlated with the size of the plant and its average wage costs. Foreign-owned plants that exhibit relatively high level of technology and employ more high-skilled workers are more inclined to offshore.

Table 2: Logit Estimates of Propensity Score

	Offshoring preferred model	Offshoring modified model	Offshoring *cum* restructuring
Log total employment (t-1)	0.1303***	0.3966***	0.4572***
	(5.72)	(4.92)	(7.19)
Log total employment^2 (t-1)		-0.0327***	
		(3.44)	
Log wage per employee (t-1)	0.2275***	0.1636**	0.6957***
	(3.11)	(2.15)	(2.59)
Technology (t-1)	0.2194***	0.2208***	-0.5223***
	(3.08)	(3.10)	(2.60)
High-skilled (t-1)	0.3564***	0.4563***	1.0755**
	(2.75)	(3.42)	(2.54)
Foreign ownership	0.4166***	0.4360***	0.1826
	(3.49)	(3.65)	(0.62)
Time dummy	-0.0486	-0.0497	-0.2289
	(0.57)	(0.58)	(0.80)
Industry dummies	yes	yes	yes
Regional dummies	yes	yes	yes
Control group	no offshoring	no offshoring	no offshoring
Pseudo R-squared	0.06	0.06	0.16
Observations	8466	8466	7315

Notes: z-values in parenthesis; definition of variables included in the matching: *Total employment:* log of number of employees per plant, *Wage per employee:* log of average wage per employee, *Technology:* Dummy=1 if plant has above average or state-of-the art technology, *High-skilled:* share of high-skilled workers of total employment, *Foreign ownership:* Dummy=1 if a foreign owner holds the majority of the plant; industry and regional dummies are employed but not reported; *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs either in the years 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants that do not increase their vertical integration during the same time period; *offshoring cum restructuring* imposes the following additional restriction on the offshoring definition above: the plant is restructured during the offshoring event, i.e. parts of the plant are closed down, sold-off or spun-off.

In column (2) we follow Dehejia (2005) and add a quadratic size term to our baseline specification as a robustness check. Furthermore, we propose a different treatment variable in column (3). While our control group remains the same, we now regard a plant as treated if it incurs offshoring as defined above and at the same time goes through a plant restructuring.

Restructuring is measured as a discrete organizational change, where parts of the plant are shut-down, sold-off or spun-off. Interestingly, "*offshoring cum restructuring*" is more likely to occur, if the plant is not at the technology frontier of its industry. Furthermore, foreign ownership does not have any significant explanatory power any longer. The average treatment effects of this specification are expected to shed some light on the employment and business effects, if the plant adjustment does not occur smoothly, but abruptly. It will allow us to isolate the productivity effect from a downsizing effect.

Balancing Tests

In the population the selection variables are balanced between the treatment and matched-control group conditional on the true propensity score (Rosenbaum and Rubin, 1983). This property of matching ensures that differences in outcome do not rely on differences in characteristics between treatment and matched control group other than treatment itself. A lack of balancing in the sample may be due to a misspecification of the estimated propensity score or due to a mismatch of propensity scores of treatment and matched control observations or due to an unfortunate draw of the sample (Rosenbaum and Rubin, 1985). We employ a number of different balancing tests to exclude systematic differences in characteristics. First, we calculate the standardized difference between treatment and matched-control group of all selection variables at a time (see, e.g., Rosenbaum and Rubin, 1985; Smith and Todd, 2005b; Caliendo and Kopeinig, 2008). There is no significance level on this statistic but Rosenbaum and Rubin (1985) consider the standardized difference large if it exceeds 20 percent. Second, we perform a mean-difference t-test with standard deviations differing in treatment and matched-control group. Third, we follow Smith and Todd (2005b) who propose a regression-based test. For each selection variable x_{it} that is used in the propensity score, the following regression is estimated

$$x_{it} = \alpha_t + \sum_{k=1}^{4} \beta_{kt} P(x_{it})^k + \sum_{k=1}^{4} \gamma_{kt} D_{it+1} P(x_{it})^k + \varepsilon_{it}$$

for the years t=1998 and 2000. Smith and Todd (2005b) argue that a joint significance test over the γ-coefficients would indicate that the balancing condition is not satisfied. Hence, we expect an insignificant Wald-test. Table A2 shows these three balancing tests. We do not find any indication for a violation of the conditional independence assumption with respect to the first balancing test. The standardized difference between treatment and matched-control group of all selection variables is displayed in column 4 (percent bias). Each selection variable exhibits a percent bias well below 20 percent, with the highest standardized bias being 4 percent. The second balancing test yields similar results. There is not a single case, where the mean-difference test between the treatment-group and the matched control-group is significant at conventional levels. Finally, the regression-based test does not indicate imbalancing of selection variables either.

Fourth, we perform the Hotelling test on quintiles that tests balancing within each quintile over all variables jointly. Tables A3 and A4 display the results for the Hotelling test as well as the distribution over the five quintiles considered, showing once more no significant imbalance. Furthermore, Dehejia (2005) suggests checking the sensitivity of the matching estimates to minor changes in the propensity score model. We added the squared total employment number to our baseline specification without any qualitative change in either the balancing tests or the matching results.[32] Hence, our estimated propensity scores secure balancing of selection variables in treatment and control group not only in the population but also in the sample and we can condition the ATT on the estimated propensity score.

Finally, we follow Imbens (2004) and Smith and Todd (2005a), who suggests a way to indirectly test for the conditional independence assumption using a test of Heckman and Hotz (1989). We estimate the average treatment effect on the treated for an outcome variable before treatment takes place. If this effect is zero, it renders the conditional independence assumption more plausible. Contrary to that, if it is not zero, this test indicates that there are systematic differences in outcomes between treatment and matched control group even before treatment, suggesting that the ATT is not caused by treatment alone. For instance, one could imagine that more dynamic or expanding firms tend to self-select into offshoring and, thereby, further increase their superb performance relative to its peer group. If this were the case, we would expect a significant average treatment effect. The treatment effect of the lagged outcome variable serves as a good candidate for such a test. For our purpose, we will employ the standard matching set-up on the lagged outcome variables employment, productivity, sales and exports. Table A5 shows no evidence of a significantly different distribution of any of the four lagged outcome variables, corroborating the conditional independence assumption.

6) Results on offshoring

Next, we present our empirical results, following the four steps of the identifications strategy as outlined in Table 1.

[32] Results are available from the authors upon request.

Step 1: Overall employment effect

We start in Table 3 with the results of difference-in-differences OLS and kernel matching estimators with bandwidth 0.01 for the outcome variable net employment. The matching estimators are based on the propensity score from specification (1) in Table 2. Standard errors from bootstrapping with 500 repetitions are displayed in parentheses.

Table 3: Impact of Offshoring on Log Employment (Kernel Matching)

Time	OLS	ATT Preferred Model	ATT Modified Model
	(1)	(2)	(3)
t	0.0195*	0.0214**	0.0200**
	(0.0109)	(0.0103)	(0.0102)
t+1	0.0419***	0.0418***	0.0401***
	(0.0143)	(0.0130)	(0.0129)
t+2	0.0602***	0.0439***	0.0453***
	(0.0161)	(0.0150)	(0.0151)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via boostrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase the share of imported intermediate inputs in overall intermediate inputs either in the years 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.

We find a *positive* and robust treatment effect on net employment at the plant-level. All point estimates have a positive coefficient and are similar in size across estimated models. The average treatment effect is in the range of 2.1 to 4.4 percent. These results indicate that an increase in offshoring has a discernible positive impact on net employment for those establishments that offshore.[33] The positive result on net employment suggests that negative direct effects through downsizing are overcompensated by the employment growth through productivity gains. In particular, this will be the case when the dominant type of offshoring is substitution of domestic for foreign suppliers. This will be investigated in the next step.

Step 2: Identification of the substitution process

We now test whether offshoring operates predominantly by replacing own production by substituting domestic suppliers. The outcome variable under consideration is intermediate

[33] This result can be indirectly related to a similarly result of Becker and Muendler (2008a). They find that German firms that expand employment abroad also expand employment at home. If the expansion of employment abroad is correlated with the expansion of vertical FDI and if employment effects from vertical FDI and international outsourcing are similar, then the treatment of offshoring should give average treatment effects on the outcome employment comparable to the ones with the treatment variable employment expansion abroad. In a similar vein, Buch and Lipponer (2007) find no evidence for higher elasticity for labor demand (in the home country) due to an increase in multinational firms' activities. Consequently, multinational activity does not increase job insecurity. In contrast to that Geishecker (2006) finds that greater openness increases job insecurity. His analysis relies on the German Socio-Economic Panel.

inputs as a share of total turnover. If offshoring replaces own production, then the intermediate input share will be expected to rise, since production steps of the home plant are replaced by intermediate goods from abroad. On the other hand if offshoring, leaves the level of intermediate inputs unchanged this means that domestic suppliers have been substituted for foreign suppliers.

Table 4: Impact of Offshoring on Imported Intermediate Goods (Kernel Matching)

Time	OLS	ATT Preferred Model	ATT Modified Model
	(1)	(2)	(3)
t	1.2183*	0.3117	0.3708
	(0.6886)	(0.6199)	(0.6261)
t+1	0.8595	-0.5948	-0.4279
	(0.8068)	(0.7137)	(0.7223)
t+2	0.2168	-0.6902	-0.5486
	(0.8556)	(0.7776)	(0.7915)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via boostrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs either in the years 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.

Table 4 shows an insignificant average treatment effect on the overall share of intermediate inputs used in German production. This means that those plants that incur offshoring do not significantly alter their overall intermediate inputs relative to comparable plant without offshoring. Hence our test suggests that the dominant type of offshoring in appears to be the substitution of domestic by foreign intermediate sourcing. This is consistent with an overall positive employment effect of offshoring at the plant-level. If domestic suppliers are replaced by foreign ones, we do not expect a strong direct employment loss in an establishment from downsizing. Rather employment might rather profit from increased competitiveness, productivity and sales of the plant. We will test this channel in the next step.

Step 3: Identification of productivity channel

Our identification strategy for the productivity channel rests on first investigating the outcome variable average productivity as a rough proxy for total factor productivity. Then, we turn to further outcome variables namely sales and exports of the plant. Table 5 shows results of offshoring on average labor productivity. We find a positive and highly significant short-term productivity gain of 3.6 percentage points. The point coefficients suggest a slightly declining productivity difference between offshorers and non-offshorers over time and for t=3 the effect even becomes insignificant. This productivity effect is sizable and in line with the results of for instance Grossman and Rossi-Hansberg (2008).

Table 5: Impact of Offshoring on Productivity (Kernel Matching)

Time	OLS	ATT Preferred Model	ATT Modified Model
	(1)	(2)	(3)
t	0.0446***	0.0362***	0.0366***
	(0.0141)	(0.0136)	(0.0137)
t+1	0.0466***	0.0298*	0.0308*
	(0.0161)	(0.0168)	(0.0170)
t+2	0.0475**	0.0256	0.0254
	(0.0189)	(0.0191)	(0.0188)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via boostrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs either in the years 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.

However, it has to be noted that our proxy for productivity as measured by the average productivity, i.e. the logarithm of total sales over total employment, is not ideal. Other studies support our results, however. For example, Hijzen, Inui and Todo (2007) find a positive, even though not necessarily causal, effect of offshoring on total factor productivity for a Japanese sample. Moreover, Barba Navaraetti and Castellani (2004) find that Italian multinationals experience a positive effect of FDI on productivity. Finally, Görg, Hanley and Strobl (2007) present evidence from an Irish manufacturing panel that positive effects from international outsourcing are confined to services inputs for exporters.[34]

Having established a positive impact of offshoring on an establishment's average productivity, we further ask whether this does increase domestic and foreign market share and therefore its sales and exports. The empirical results for the outcome variable sales are presented in Table 6.

Table 6: Impact of Offshoring on Log Sales (Kernel Matching)

Time	OLS	ATT Preferred Model	ATT Modified Model
	(1)	(2)	(3)
t	0.0446***	0.0502***	0.0495***
	(0.0141)	(0.0144)	(0.0144)
t+1	0.0449***	0.0583***	0.0611***
	(0.0163)	(0.0175)	(0.0175)
t+2	0.0529***	0.0743***	0.0769***
	(0.0193)	(0.0199)	(0.0196)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via boostrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs between in the years 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.

[34] Olsen (2006) provides a survey on the productivity effects of offshoring.

We find a very robust positive average treatment effect in the range of 5 to 7.4 percent at the 99-percent confidence level. Thus, establishments that increase their share of foreign intermediate inputs exhibit higher turnover than comparable establishments that abstain from it. Assuming that growth in turnover is positively correlated with growth in profits, we can expect that treated establishments gain competitiveness at home and abroad alike. Companies with strong cash-flows have a greater flexibility in financing new investments. Consequently, they are more capable of staying near the technological-frontier in their respective industry. Furthermore, stronger turnovers stemming from increased offshoring will likely be associated with stronger international competitiveness, which allows such companies to sustain or even increase their international market share. At the same time, restrictions on offshoring that hinder plants to profit from their optimal input-mix between domestic and foreign input factors are expected to have a detrimental effect on competitiveness.[35]

The productivity effect is reconfirmed when looking at the average treatment effects on exports in Table 7, indicating that treated plants increase their export share due to offshoring (at least at the 95 percent confidence level).

Table 7: Impact of Offshoring on Exports (Kernel Matching)

Time	OLS	ATT Preferred Model	ATT Modified Model
	(1)	(2)	(3)
t	1.2985***	0.9359***	0.9175**
	(0.3832)	(0.3570)	(0.3575)
t+1	1.7530***	1.1623***	1.2177***
	(0.4799)	(0.4384)	(0.4561)
t+2	2.6114***	1.3176**	1.4682***
	(0.5888)	(0.5578)	(0.5653)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via boostrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs either in the years 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.

Hence, these plants tend to become more open on the exporting and importing side. This finding complements the results of a recent survey article by Bernard et al. (2007) in an interesting way. The authors show for a new U.S. dataset from 1992 to 2000 that trade is very rare and highly concentrated and that importing firms exhibit many of the same features as exporting firms. Furthermore, Bernard et al. (2007) explain the positive correlation between export and import volume by the international fragmentation of production, i.e. offshoring.

[35] Once more our results are for instance in line with the output-enhancing results of vertical FDI for Italian multinationals (Barba Navaretti and Castellani, 2004).

Beyond that our results indicate a causal effect of increased imports of intermediate inputs on exports. Considering that the average share of exports to total turnover in our sample is about 6.6, the average treatment effects in the range of 0.9 to 1.3 appear economically relevant. These findings on sales and exports provide indirect evidence that the productivity channel is at work and that firms that increased their share of imported intermediate goods perform significantly better.

To sum up our results so far: we find an increase of employment in offshoring plants resulting from a substitution of domestic for foreign suppliers and an increase in average labour productivity, sales and exports. Hence, our results suggest that on average productivity effects of offshoring plants have dominated downsizing. Next we turn to the test direct effects of downsizing.

Step 4: Identification of direct employment effect via downsizing

To extract the direct employment effect via downsizing, we redefine the treatment variable. We now consider only offshoring plants, where a restructuring is reported at the same time. We implicitly assume thus that restructuring is due to offshoring whenever offshoring is simultaneous with restructuring. Such cases of offshoring are rather rare (about one out of eight offshoring cases). However, in these cases there are significant negative employment effects, as can be seen from Table 8. This can be explained with the direct employment effect via downsizing. Note that the productivity increase is nevertheless present and offshoring plants are able to expand their sales abroad. For offshoring cum restructuring, we can conclude that the direct employment effect dominates the productivity effect due to the substitution of own production by foreign one.

Table 8: The Impact of Offshoring *cum* Restructuring on Log Employment, Log Sales, Exports and Log Productivity (Kernel Matching)

Time	Employment	Sales	Exports	Productivity
T	-0.0871*	0.0318	4.2855***	0.1065**
	(0.0494)	(0.0567)	(1.633)	(0.0456)
t+1	-0.1873***	-0.0361	5.3028***	0.1189*
	(0.0629)	(0.0796)	(2.059)	(0.0645)
t+2	-0.1691**	-0.0541	3.9936	0.0909
	(0.0704)	(0.0869)	(3.1173)	(0.0846)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via boostrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring cum restructuring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs either in the years 1999-2000 or 2001-2002 for a certain plant, if the plant is restructured at the same time, i.e. parts of the plant are closed down, sold-off or spun-off; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.

7) Robustness checks

We conclude our empirical analysis with a number of robustness checks. First, one might argue that establishments in different industries should not be compared within the same homogenous matching framework, because they might differ substantially for instance in their market structure. On the one hand, it is worth noting here that one of the characteristics that enter the propensity score is already an industry classification. Given that the standardized biases for all industries are very low, it is pretty unlikely that a significant share of observations from another industry enter the matching estimates. On the other hand, we can explicitly restrict the matching algorithm to consider only matches within the same industry in order to insure better comparability. The results for matching within 16 industries presented in Table B1 are very similar with respect to the point coefficients and significance levels for the outcome variables employment, sales and exports, but the results for the outcome variable productivity weaken and are only significant on the 10-percent level for t=1.

Second, we test the robustness of our results by using a different matching algorithm. We employ nearest neighbor matching with two neighbors. Thereby, we rely on NNMatch from Abadie et al. (2004). Table B2 in the appendix demonstrates that all results prove to be very similar with respect to the point coefficients and the level of significance.

Third, we restrict our matching estimates to establishments in the manufacturing sector only. Once more, the overall picture does not change much, but some interesting patterns emerge. While the employment effects in manufacturing are similar to the full sample, the positive effect on sales and productivity seems to be more pronounced in the manufacturing sector.

Finally, we provide another robustness check. We distinguish between offshoring to countries belonging to the European Union (EU Offshoring) and countries that do not belong to the European Union (Non-EU Offshoring). The second group of countries also includes "new" European member states like Poland, the Czech Republic or Hungary, since these countries were not part of the European Union by the time of the survey. The results show some heterogeneity along these two regions. The positive employment effect of offshoring within the European Union turns out to be stronger and the effect of offshoring to outside the European Union is insignificant. This is an interesting side result of our study, since for instance Geishecker (2006) finds a decline in relative demand for manual workers in Germany due to international outsourcing to Central and Eastern European Countries. In a similar vein, Debaere, Lee and Lee (2006) report a negative (neutral) effect of outward FDI on employment growth of South Korean multinationals, if the investment goes to less (more) advanced countries.

8) Conclusion

This paper provides what is to our knowledge the first granular analysis of various effects of offshoring on employment of plants. Using a plant-level measure of offshoring, we deploy difference-in-differences matching techniques. This has the double advantage of being able to deal with firm heterogeneity and non-linearity.

Our empirical strategy allows us to identify two theoretical channels that have not been disentangled in the previous literature: 1) An increase in the share of foreign intermediate inputs in total inputs (offshoring) may substitute for own production thereby reducing employment through downsizing. 2) Offshoring also may substitute domestic for foreign suppliers. Still, there is a positive employment effect left from cost savings, increased competitiveness and increased market share (productivity effect of offshoring on employment).

Moreover, our approach allows us to cover both vertical FDI and international outsourcing events. In addition, we discuss the implication of general equilibrium effects on our estimators. We replace the usual stable unit treatment value assumption by the assumption that general equilibrium effects depend only on an aggregate measure of the amount of treatment observations, but not on whether a particular observation undergoes treatment. Then the average treatment effect on the treated obtains the interpretation of a differential causal effect conditional on the amount of treatment that actually took place during the sample period.

Overall, we find that plants which offshore have on average a larger employment than as if they had not offshored it conditional on that a certain mass of plants did offshore during the data period. However, the production depth remains on average unchanged through offshoring, indicating that most offshoring substitutes domestic for foreign suppliers, which shuts off the downsizing channel but keeps the productivity channel of offshoring on employment. In addition, offshoring plants tend to have larger labour productivity, domestic sales, and exports. Hence, the positive differential employment effect of offshoring is consistent with the productivity effect of offshoring on employment. To identify also the employment effect of offshoring through downsizing, we confine treatment to offshoring that coincides with partial plant closure. Such plants have less employment than as if they had not done it, indicating that the employment effect of offshoring via downsizing dominates the productivity effect on employment. A minor, but interesting result is an economically significant effect of increased imports of intermediate inputs on exports. This complements findings by Bernard et al. (2007) and is consistent with offshoring increasing productivity.

We conclude that on average the productivity effect dominates possible downsizing effects. An important caution is that these results at the plant level cannot simply be extrapolated to the economy wide level, since offshoring plants might be destroying jobs in the domestic supply industry, which cannot be traced in our data. Moreover, we can only determine differential employment effects of offshoring plants but not aggregate employment effects in the economy if general equilibrium effects of offshoring on non-offshoring plants exist.

A by-product of our analysis is that offshoring combined with restructuring is more likely in plants that are technological laggards. One may suspect that plants that fall behind in the technological race are more likely to be forced to undergo accelerated adjustment and that these plants use offshoring, spin-off and closing of plants as a measure to catch up. It might be a fruitful avenue for future research to investigate, why these firms fell behind in the first place and whether offshoring helps them to turn the tide in order to secure survival in the medium-run.

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Appendix A

Table A1: Summary Statistics

	Outsourcing Plants		Non-outsourcing Plants	
	Mean	Standard deviation	Mean	Standard deviation
Log employment	3.7161	1.7401	3.0832	1.7133
Log wage per employee	7.4083	0.5865	7.2360	0.6110
Technology	0.6898	0.4627	0.6335	0.4818
High-skilled	0.4151	0.2860	0.4078	0.3061
Foreign ownership	0.1012	0.3017	0.0429	0.2027
Log sales	15.3182	2.1623	14.4034	2.1016
Exports	12.6912	22.4274	5.5727	16.1358
Intermediate inputs	55.5620	21.7463	49.8634	23.8991
Number of observations	1265		7201	

Notes: *Log employment:* log of number of employees per plant, Log *wage per employee:* log of average wage per employee, *Technology:* Dummy=1 if plant has above average or state-of-the art technology, *High-skilled:* share of high-skilled workers of total employment, *Foreign ownership:* Dummy=1 if a foreign owner holds the majority of the plant, Log sales: log of total turnover of the plant, Exports: ratio of turnover aboard to total turnover at the plant, Intermediate inputs: ratio of intermediate inputs to output.

Table A2: Balancing Tests from Kernel Matching

Covariate	Mean treatment group	Mean control group	Percent bias	Percent bias reduction	Mean-diff. t-stat (p-value)	Regression-based tests Wald statistic (p-value)
Total employment	3.7338	3.7142	1.2	96.5	0.26 (0.79)	0.83 (0.51)
Wage per employee	7.3771	7.3779	-0.2	99.5	-0.04 (0.97)	1.24 (0.29)
Technology	0.7389	0.7338	1.1	91.2	0.27 (0.79)	0.63 (0.63)
High-skilled	0.3801	0.3818	-0.6	59.4	-0.14 (0.89)	1.54 (0.19)
Foreign ownership	0.0968	0.0867	4.0	82.0	0.81 (0.41)	2.15 (0.07)

Notes: Definition of variables included in the matching: *Total employment:* log of number of employees per plant, *Wage per employee:* log of average wage per employee, *Technology:* Dummy=1 if plant has above average or state-of-the art technology, *High-skilled:* share of high-skilled workers of total employment, *Foreign ownership:* Dummy=1 if a foreign owner holds the majority of the plant; Balancing of industry, regional and time dummies is not reported; all dummies have a percent bias below 3; mean-diff. is mean difference test with standard deviations differing between treatment and control group. Regression based Wald test statistic follows Smith and Todd (2005b).

Table A3: Hotelling's T-squared Tests by Propensity Score Quintile

Quintile	T-squared statistics	F-test statistics	p-value
First	41.000	1.254	0.157
Second	20.536	0.609	0.961
Third	40.495	1.200	0.202
Fourth	31.485	0.905	0.626
Fifth	35.927	1.065	0.369

Table A4: Frequency Distribution of Treated and Non-treated plants by Propensity Score Quintile

Quintile	Outsourcing plants	Non-outsourcing plants
First	76	1380
Second	124	1331
Third	201	1255
Fourth	300	1155
Fifth	383	1072

Table A5: Heckman and Hotz (1989): Evidence for Self-selection into Offshoring ? Log Employment, Log Sales, Exports and Log Productivity (t=-1)

Time	Employment	Sales	Exports	Productivity
Kernel Matching	-0.0094	0.0071	0.6515	0.0184
	(0.0126)	(0.0176)	(0.5076)	(0.0173)
OLS	0.0082	0.0213	0.3350	0.0213
	(0.0154)	(0.0168)	(0.3561)	(0.0168)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via boostrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs between in the years 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.

Appendix B

Table B1: The Impact of Offshoring on Log Employment, Log Sales, Exports and Log Productivity (Kernel Matching within Industries)

Time	Employment	Sales	Exports	Productivity
1	0.02295*	0.0458***	1.0392***	0.0307*
	(0.0125)	(0.0174)	(0.3542)	(0.0166)
2	0.0400**	0.0567***	1.0393**	0.0291
	(0.0163)	(0.0194)	(0.4454)	(0.0191)
3	0.0529***	0.06804***	1.3492**	0.0142
	(0.0186)	(0.0234)	(0.5579)	(0.0219)

Notes: Kernel matching, whereby matches are only allowed between plants *within* the same industry (16 industries) and the average treatment effect on the treated is equivalent to the average ATT's over the 16 industries. Bootstrapped standard errors (500 replications) are in parentheses. *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs either in the years 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants within the same industry that do not increase their vertical integration during the same time period.

Table B2: The Impact of Offshoring on Log Employment, Log Sales, Exports and Log Productivity (Nearest Neighbor Matching)

Time	Employment	Sales	Exports	Productivity
1	0.0315***	0.0515***	1.1143***	0.0334**
	(0.0121)	(0.0168)	(0.3868)	(0.0163)
2	0.0532***	0.0613***	1.3417***	0.0172
	(0.0156)	(0.0206)	(0.4779)	(0.0192)
3	0.0541***	0.0832***	1.2853**	0.0226
	(0.0176)	(0.0237)	(0.5932)	(0.0209)

Notes: Standard errors in parentheses. Nearest-neighbor matching with two neighbors and caliper=0.05. For the matched sample heteroskedasticity-consistent standard errors are generated with NNMatch from Abadie et al. (2004); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs between either in the year 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.

Table B3: The Impact of Offshoring on Log Employment, Log Sales, Exports and Log Productivity – Manufacturing only (Kernel Matching)

Time	Employment	Sales	Exports	Productivity
1	0.0240*	0.0733***	1.0462	0.0596***
	(0.0139)	(0.0222)	(0.6495)	(0.0192)
2	0.0381**	0.0809***	1.5793**	0.0577***
	(0.0167)	(0.0267)	(0.7953)	(0.0216)
3	0.0317*	0.0952***	1.8756**	0.0544**
	(0.0193)	(0.0305)	(0.9048)	(0.0238)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via boostrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs between either in the year 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.

Table B4: The Impact of Offshoring on Log Employment, Log Sales, Exports and Log Productivity – Further Results (Kernel Matching)

Time	Employment	Sales	Exports	Productivity
Baseline Offshoring	0.0214**	0.0502***	0.9359***	0.0362***
(Tables 3;5-7, t=1)	(0.0103)	(0.0144)	(0.3570)	(0.0136)
EU Offshoring	0.0466***	0.0433**	0.7049	0.0340**
	(0.0125)	(0.0186)	(0.4782)	(0.0168)
Non-EU Offshoring	-0.0043	0.0295	1.2008	0.0539**
	(0.0172)	(0.0237)	(0.7810)	(0.0230)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via boostrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *EU Offshoring* and *Non-EU Offshoring* is defined as an increase in the share of imported intermediate inputs in overall intermediate inputs to EU or Non-EU countries between either in the year 1999-2000 or 2001-2002 for a certain plant; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.