

Offshoring, Firm Performance and Employment – Evidence from German Establishment Data

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Abstract:

This paper seeks to isolate empirically the productivity effect of offshoring on establishment employment (e.g. Grossman and Rossi-Hansberg, 2008) from a direct employment effect via downsizing. The analysis is pursued for a representative sample of German establishments from 1998 to 2004, using a difference-in-differences matching approach. First, we find a positive employment effect from a qualitative increase of foreign intermediate input usage (offshoring). Second, offshoring does not increase production depth. Hence, we conclude that offshoring plants predominantly substitute domestic for foreign inputs rather than replace their own production by offshoring. Third, offshoring plants increase their average productivity and thus their competitiveness relative to plants that do not offshore. Moreover, they gain domestic and foreign market share. This can explain the positive employment effect in offshoring plants. In a final step, we provide also evidence for employment losses through offshoring in the few cases when it is concomitant to restructuring of the plant.

Key words: offshoring, export performance, employment, difference-in-differences, matching estimator.

JEL classification: F16, F23.

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1) Introduction

There is a widely held view that the dramatic increase in the world-wide production sharing has been destroying jobs on a large scale in all industrial countries. The daily press may help induce this impression since plant closures regularly receive a wide coverage, while media reports on multinational firms that increase employment at home, e.g. due to gains of market share abroad, are rare.

In contrast to that the academic literature has found ambiguous results on the employment effects of offshoring. On the one hand, there is empirical evidence based on aggregate data by Feenstra and Hanson (1996, 1999), Geishecker (2002), Hijzen, Görg, and Hine (2005), Hsieh and Woo (2005), Egger and Egger (2003, 2005), Hijzen (2007) inter alia showing a skill upgrading in industrial countries through offshoring. Low-skilled intensive production stages move to countries where low-skilled labor is relatively cheap (e.g. Helpman, 1984; Venables, 1999) and hence relative demand for low-skilled labor decreases while it increases for high-skilled labor in industrial countries.

There is also a strong methodological case to re-examine this issue with micro data, since macro data suffer from aggregation bias, lack of appropriate control variables for firms and workers, and self-selection effects (Geishecker, 2008). Micro-level evidence of labor market effects on offshoring are provided inter alia by Egger, Pfaffermayer and Weber (2007), Geishecker and Görg (2008), Marin (2006), Munch and Skaksen (2008) and Geishecker (2006, 2008). 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. Likewise, the measures of offshoring used in other micro studies on Germany such as Geishecker and Görg (2008) and Geishecker (2006, 2008) rely on industry-level variables of intermediate goods trade from input-output tables.

Obviously, offshoring measures at the industry-level are neither able to capture that firms are heterogeneous with respect to their use of intermediate inputs within an industry, nor to establish causal effects of offshoring on employment. Becker and Muendler (2008a) offer a plant-level proxy by measuring FDI expansion as an expansion of employment abroad of a multinational firm.¹ Contrary to the public opinion, they find increased job security at home,

¹ Barba-Navaretti and Castellani (2004) and Debaere et al. (2006) investigate the impact of outward FDI as measured by becoming a multinational company, i.e. changing their investment status. Hijzen et al. (2007) offer a firm-level measure of offshoring by using the value of subcontracting to foreign providers. Based on OLS and

whenever a firm expands employment abroad. However, expansion of employment abroad may not only be driven by a relocation of plants from home- to host country that *reduces* employment at home but also by an increase of the international competitiveness of the firm that *increases* employment both at home and abroad. Hence, expansion of employment abroad may not be related to vertical FDI or offshoring.

To fend off this potential criticism on the treatment variable of Becker and Muendler (2008a), we employ a new offshoring measure on German data, which is closely related to the industry measure of offshoring by Feenstra and Hanson (1996, 1999), which implicitly defines offshoring. We measure offshoring as qualitative increase in the share of intermediate inputs of an establishment received from abroad. Then we employ a difference-in-differences matching approach in order to identify causal effects of offshoring on employment of establishments for a representative sample of Germany from 1998 to 2004, which is based on survey-data from the IAB establishment panel covering all sectors of the German economy.

Like Becker and Muendler (2008a), we find surprisingly a positive employment effect of offshoring on the domestic plant. In a second step, we investigate how offshoring affects production depth and find no significant effect. Hence, offshoring must substitute domestic for foreign suppliers in the majority of cases, rather than replacing own production by foreign supply. In a third step, we find that offshoring plants increase their average labor productivity, improving their competitiveness against firms that do not offshore, and increase their domestic and foreign market share. Through these findings we can isolate the productivity effect of offshoring on employment, which played a central role in the recent offshoring model of Grossman and Rossi-Hansberg (2008). In a final, step we constrain offshoring to those plants that restructure concomitantly. In these few cases, we find negative employment effects of offshoring through downsizing, which dominate the positive employment effects from the productivity gain of offshoring. Our analysis also suggests that there might be a negative effect of offshoring on the industry-level, if domestic suppliers are substituted by foreign ones. These results are robust, among others, to a careful investigation of whether self-selection into offshoring confounds these treatment effects.

Our findings complement recent research by Becker and Muendler (2008b), who extend their earlier work and find that multinationals expanding abroad not only exhibit fewer worker

SGMM estimates, Hijzen et al. (2007) find a positive effect of international outsourcing and vertical FDI on firm productivity.

separations but also higher worker accessions. Furthermore, they show that the net employment effect of outward FDI can still be negative due to a market-share switching effect. While they hint also at negative employment effects of offshoring, their investigated channel is differently, focusing on market switching effects on the final goods market, while we focus on switching effects of domestic and foreign suppliers.

We contribute to this literature in two ways: First, we extend the literature on employment effects of offshoring by offering a new, refined variable for offshoring at the establishment-level. Contrary to the literature based on industry-level offshoring, this allows testing for applying non-parametric matching techniques. Second, we carefully seek to distinguish between different channels that determine the employment effect of offshoring on an establishment. In particular, we isolate the productivity effect of offshoring on employment, which plays a prominent role in the offshoring model of Grossman and Rossi-Hansberg (2008). Finally, we find a *negative* employment effect from offshoring when an increase in foreign sourcing goes along with downsizing in the domestic plant.

The rest of this paper is organized as follows: Section 2 gives a theoretical framework for a microdata analysis of offshoring. Section 3 discusses briefly the data set. Section 4 discusses the empirical methodology. Section 5 provides the estimation of the propensity score of offshoring and reports various auxiliary tests. Section 6 contains the main estimations of the average treatment effect on the treated of offshoring. Section 7 offers some extensions, before the last section concludes.

2) A theoretical concept for a microdata analysis of offshoring

We are interested in identifying theoretical channels of employment effects through offshoring. To pursue this endeavor, we need to clarify the term offshoring. The literature started out by measuring offshoring as the increase of imports in intermediate goods in an industry (Feenstra and Hanson, 1996, 1999), thereby implicitly assigning a definition to the term offshoring at the same time. This measure was also used in firm and employee studies (e.g. Geishecker, 2002, 2006, 2008, Geishecker and Görg, 2008). In contrast, we have an *establishment* measure of offshoring, measuring the increase of imports in intermediate goods of an establishment (rather than of an industry). This allows us to do a micro-level analysis of offshoring with establishment data. However, 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 of an establishment in the home country for imports from abroad² or it may substitute domestic supply for foreign supply. Only in the former case exists a direct employment effect, where the establishment under consideration exports jobs abroad by relocating its own production abroad or replacing domestic production by purchases from abroad. In the latter case, an establishment under consideration does not experience a direct employment effect, because no production is relocated from its own plant. Instead, domestic employment effects show up among domestic suppliers. Such employment effects are, however, not associated with offshoring using micro-data in the analysis, because the supplying firms cannot be traced back in existing datasets to date.

Instead, there is an indirect employment effect from outsourcing independently of whether own production or domestic supply is substituted for foreign supply. This arises from offshoring increasing productivity and cutting cost (for otherwise it would not have been done in the first place). Hence, firms that offshore gain competitiveness relative to firms that do not offshore (see for instance Grossman and Rossi-Hansberg, 2008). At a competitive advantage market share at home increases and therefore local sales, which in turn boost demand for labor. Similarly, market share abroad increases and therefore exports, again stimulating labor demand. We will coin 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 also the employment in other establishments. First, if an establishment is a supplier to an offshoring establishment who is substituted for a foreign one, there is an employment loss through fall in demand. Second, if a plant is a competitor who loses competitiveness against firms that offshore, there is an employment loss through fall in market share (Becker and Muendler, 2008b). We will not be able to capture these effects properly with our data, because we neither know the competitors of an offshoring establishment nor its domestic suppliers. Therefore, we do not investigate the overall employment effect of offshoring, but constrain

² To analyse employment effects of offshoring, we do not need to differentiate between buying intermediate goods through arms-length trade or from an own plant abroad.

the analysis to identifying the theoretical channels through which offshoring affects the *own* employment of an establishment.

Figure 1 summarizes these ideas. Offshoring of an establishment causes a direct employment effect if own production is substituted for foreign supply, thereby cutting employment through downsizing. 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, there is no direct employment effect in an establishment that shores off, since there is no change of the production depth in its plant. Still, these establishments save cost analogously to a productivity gain³ from offshoring (again for otherwise they would not have done it). 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 to identify the productivity effect, since the direct employment effect from downsizing is not present.

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

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

³ An increase in productivity means that more output can be produced with the same inputs. This implies that with a given budget for input factors more value added can be generated. But this happens, too, if inputs become cheaper. Then, more inputs can be bought for the same budget to produce more output.

Figure 1: Roadmap of Analysis

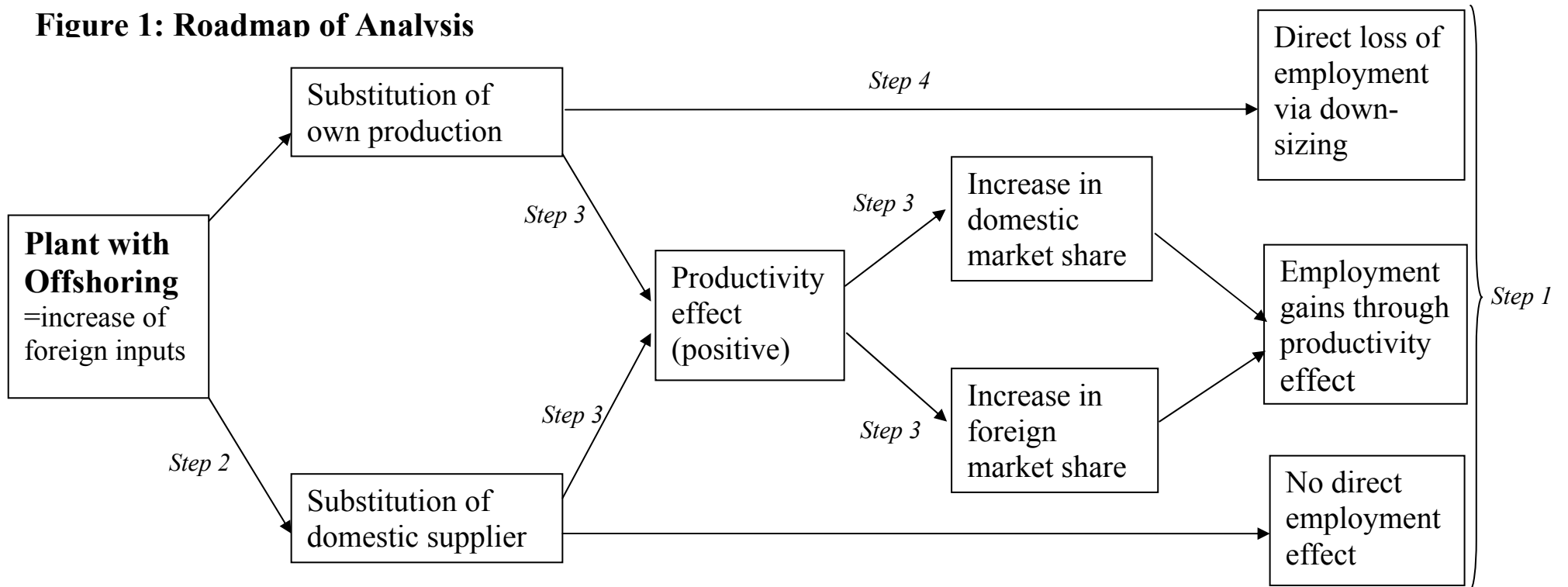


Table 1 explains the roadmap for the empirical analysis, which follows these four steps to identify the two theoretical channels by which offshoring of an establishment is affecting its own employment. We have a representative sample of establishments i at time t .

In a first step, we assess the overall effect of an increase in offshoring ($\Delta\text{Offshoring}_i$) on an establishment's employment change ($\Delta\text{Employment}_i$) relative to establishments that do not offshore.

In a second step, we examine which one of the two types of offshoring – substitution of own production for foreign supply vs substitution of domestic supply for foreign supply - is prevalent. For this purpose, we investigate the causal link between an increase of foreign sourcing of an establishment and its production depth ($\Delta\text{Intermediate_input-use}_i$). If an increase in sourcing abroad leads to a decrease of production depth, then own production is predominantly substituted for foreign supply. If there is, instead, not a significant change of production depth going along with an increase in foreign supply, then substitution of domestic for foreign supply is predominant.

In a third step, we identify the productivity channel. Unfortunately, we cannot obtain total factor productivity from our data and need to approximate it with the average labor productivity ($\Delta\text{Productivity}_i$). However, an increase of productivity yields an increase of market share both at home and abroad if it is supposed to have employment effects. Hence, we investigate the causal effect of an increase in offshoring on the change in sales (ΔSales_i) and exports ($\Delta\text{Exports}_i$) of a plant to identify indirectly the productivity channel.

Table 1: Steps of Empirical Analysis

<i>First step: Identification of employment effect</i>
$\text{Offshoring}_i \rightarrow \Delta \text{Employment}_i$
<i>Second step: Finding prevalent channel</i>
$\text{Offshoring}_i \rightarrow \Delta \text{Intermediate_Inputs}_i$
<i>Third step: Identification of productivity effect</i>
$\text{Offshoring}_i \rightarrow \Delta \text{Productivity}_i$
$\text{Offshoring}_i \rightarrow \Delta \text{Sales}_i$
$\text{Offshoring}_i \rightarrow \Delta \text{Exports}_i$
<i>Fourth step: Identification of direct employment effect via downsizing</i>
$\text{Offshoring}_i \times \text{Closure}_i \rightarrow \Delta \text{Employment}_i$

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

In a fourth step, we isolate those cases of offshoring that coincide with a partial closure of the plant ($Offshoring_i \times Closure_i$) and investigate the causal link to employment of those cases.

3) Empirical methodology

To identify the two channels through which offshoring choice impacts on employment of an offshoring plant, we employ difference-in-difference and matching techniques following Becker and Muendler (2008a,b) to separate self-selection effects from causal effects of offshoring. The basic idea of a matching estimator is to estimate the probability that an establishment increases offshoring. 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 the same probability of doing so.

To motivate the difference-in-difference 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 (increase in foreign input usage) and 1 the period after treatment offshoring, and the outcome variable will be either employment, sales, exports or intermediate input share of sales in our empirical analysis.

$$y_{it}^T = g(x_{i0})t + f^T(x_{it})t + \delta_{it}^T t + \gamma_i + U_{it} + \varepsilon_{it}$$

$$y_{it}^{NT} = g(x_{i0})t + f^{NT}(x_{it})t + \delta_{it}^{NT} t + \gamma_i + U_{it} + \varepsilon_{it}$$

We distinguish the outcome y_{it}^T in case that the treatment offshoring takes place, and the outcome y_{it}^{NT} if treatment does not take place. The function $g(x_{i0})t$ captures the growth trend based on observable characteristics independent of the treatment, $f^T(x_{i1})$ stands for the (possibly heterogeneous) causal impact of the treatment choice on the outcome dependent on observable characteristics, δ_{it}^T is the heterogeneous causal impact of treatment unobservable to the econometrician but possibly known to the firm, γ_i are time-invariant observable or unobservable characteristics that influence the outcome, U_{it} are time-variant unobservable characteristics. Importantly, we deviate from standard assumptions by allowing treatment to have a (negative) impact also on establishments that do not offshore based on observable

characteristics $f^{NT}(x_{i1})$ and unobservable characteristics.⁴ Without loss of generality, the unobservable random variables and the white noise error ε_{it} have expected value of zero.

The causal effect is then simply $f^T(x_{i1}) + \delta_{i1}^T - f^{NT}(x_{i1}) - \delta_{i1}^{NT}$, which 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 obtain at best an average causal effect over all establishments. We confine our analysis to the causality measure average treatment effect on the treated (ATT), which is defined as the average causal effect of all observations that undergo treatment:

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

where the treatment group indicator D_{it} is a binary variable with value 1 in period 1 if offshoring actually takes place in an establishment, i.e. foreign input usage increases substantially from period 0 to period 1, and 0 otherwise. Importantly, this average causal effect describes a differential outcome between treatment and no treatment if only one of the observations chooses to change its behavior. We exclude thus general equilibrium effects in the construction of a counterfactual.⁵

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

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

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

The 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

⁴ By this generalization, we capture general equilibrium effects.

trend determinants x_{i0} are exogenous, (iii) observable time trend determinants are linear in functional form, (iv) there is a common average time trend conditional on observable characteristics x_{i0} among treated and untreated observations (conditional (mean) independence assumption). The latter implies that there is no self-selection of treatment choice according to unobservable time trend determinants ($E[U_{it}|D_{it} = 1] = 0$) and unobservable heterogeneous causal effects ($E[\delta_{it}|D_{it} = 1] = 0$).

The difference-in-difference 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. Under the assumption of conditional mean independence, i.e.

$$E[\Delta y_{it}|x_{i0}, D_{it} = 0] = E[\Delta y_{it}|x_{i0}, D_{it} = 1] = E[\Delta y_{it}|x_{i0}]$$

$$E[\Delta y_{it}|x_{i0}, D_{it} = 0] = E[\Delta y_{it}|x_{i0}, D_{it} = 1] = E[\Delta y_{it}|x_{i0}]$$

the ATT is equal to:

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

where

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

and expected values can be replaced by sample means due to law of large numbers.

Conditioning on x_{i0} is not practicable because of the dimensionality problem. However, Rubinstein 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, $P(D_{it} = 1) = P(x_{i0})$. Two problems remain to obtain consistent estimates of the ATT. First, the propensity score needs to be estimated. We do this using a logit-specification. Second, for each treatment observation the expected value of the change in outcome

⁵ An alternative counterfactual would be that all offshoring firms choose not to offshore. The resulting causal effect from such a counterfactual cannot be estimated with microdata, but only calibrated with a general

conditional on the same probability of offshoring among the establishments without offshoring needs to be found. In general, such a difference-in-differences (DID) matching estimator of the ATT can be written according to Heckman et al. (1997) in the following way:⁶

$$\delta = \sum_{i \in O} \left(\Delta y_i - \sum_{i \in C} g(p_i, p_j) \Delta y_i \right),$$

where Δy represents the difference between the average outcome before and after the offshoring event. The non-parametric function $g(\cdot)$ determines in which way the observations of the control group will be weighted and, thereby, provides the counterfactual. Various propensity score matching estimators basically differ in their way of measuring the neighborhood of a treated establishment, the number of neighbors and the number of establishments that form a control group, respectively. Finally, members of the control group might be weighted in accordance to their proximity to the treated establishment.⁷ We employ a kernel matching algorithm that allows incorporating – depending on the bandwidth – a relatively large number of control group members, whereby more distant members – with respect to the propensity score – of the control group receive a smaller weight. The choice of the bandwidth typically involves a trade-off. On the one hand, a relatively high bandwidth implies that some of the establishments that take part of the control group might be quite different from the treated establishment, leading to a biased estimator. On the other hand, variance 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 vary the central parameter bandwidth to insure the robustness of the empirical results. We will present the results for bandwidth of 0.01, but our results are also robust to other specifications of the bandwidth. Another very common matching algorithm is the so-called nearest neighbour matching, where only the most similar observation(s) as measured by the propensity score is/are considered in the average treatment estimations. As a robustness check, we will present the results for nearest-neighbor-matching with 2 neighbors.

equilibrium model. See Heckman, Lochner und Tabner (1998) for a related exercise.

⁶ A number of recent studies have underlined that the combination of matching with a difference-in-differences methodology has the advantage of eliminating unobserved time-invariant differences in outcomes between the treated and the untreated firms (e.g. Blundell and Costa Dias, 2000; Smith and Todd, 2005b; Girma and Görg, 2008).

⁷ For a survey on alternative matching algorithms, see for instance Caliendo and Kopeinig (2008).

Next, is discussed, how to gauge the statistical significance of the treatment effect. 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). While Abadie and Imbens (2008) formally show that bootstrapped standard errors are not valid in the case of nearest neighbor matching, they suggest that bootstrapping allows for valid inference in the case of kernel-based matching estimators. Our standard errors will be based on bootstrapping with 500 replications. All our estimations rely on the STATA-Modul PSMATCH2 that has been developed by Leuven und Sianesi (2003).

Finally, we address the assumption of conditional mean independence. This assumption cannot be directly tested for, but we will provide below an indirect test which is able to indicate, whether this assumption appears plausible or not. Moreover, conditioning the ATT on the propensity score instead of on each selection variable is appropriate only if the selection variables are identically distributed among control and treatment group. Rosenbaum and Rubin (1983) showed that this is always true in the population. However, it may not be true in a small sample, because of an unfortunate draw of the sample or because the estimated propensity score deviates from the true one, or because the matching is based on similar rather than identical “neighbors”. For this reason, balancing tests are invoked, which we will also describe in detail below.

4) Data set

Our main data source constitutes the IAB Establishment Panel from the Institute for Employment Research (IAB).⁸ This panel started in 1993 with 4,265 establishments in West Germany (see for instance Kölling, 2000; Bellmann, 1997) and included roughly 16,000 establishments nationwide in 2005 due to several waves of additional establishments. 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

⁸ 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 Nürnberg, Germany. For further information, we refer to <http://fdz.iab.de/en>.

Research. Participation of firms is voluntary but the response rate of more than 80% for 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.⁹ More precisely, we exploit information on, whether establishments have predominantly, partly or not at all received intermediate inputs and external costs, 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_{it+s}^1 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 than in the year 1998, the offshoring event must have taken place in 1998-1999 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*: value of (domestic and foreign) intermediate inputs as a share of total turnover.

4.2) Treatment variables

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

⁹ For simplification we refer here to the business years the data is covering. These questions were asked in the survey years are 1999, 2001 and 2003.

if the establishment experienced an increase in imported 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.¹⁰

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. 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 if plant is located in West Germany and zero otherwise.

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

5) Auxiliary estimates and tests

Propensity Score

Column (1) of Table 2 reports our preferred logit specification. We find that the decision for offshoring is positively and highly significantly correlated with the size of the plant and its

¹⁰ 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.

average wage costs. Furthermore, foreign-owned plants that exhibit relatively high level of technology and employ more high-skilled workers are more inclined to offshoring. In column (2) we follow Dehejia (2005) and add a quadratic size term to our baseline specification as a robustness check.

Table 2: Logit Estimates of Propensity Score

	Offshoring preferred model	Offshoring modified model	Offshoring <i>cum</i> restructuring
<i>Log total employment (t-1)</i>	0.1303*** (5.72)	0.3966*** (4.92)	0.4572*** (7.19)
<i>Log total employment² (t-1)</i>		-0.0327*** (3.44)	
<i>Log wage per employee (t-1)</i>	0.2275*** (3.11)	0.1636** (2.15)	0.6957*** (2.59)
<i>Technology (t-1)</i>	0.2194*** (3.08)	0.2208*** (3.10)	-0.5223*** (2.60)
<i>High-skilled (t-1)</i>	0.3564*** (2.75)	0.4563*** (3.42)	1.0755** (2.54)
<i>Foreign ownership</i>	0.4166*** (3.49)	0.4360*** (3.65)	0.1826 (0.62)
<i>Time dummy</i>	-0.0486 (0.57)	-0.0497 (0.58)	-0.2289 (0.80)
<i>Industry dummies</i>	Yes	yes	Yes
<i>Regional dummies</i>	Yes	Yes	Yes
<i>Control group</i>	no offshoring	no offshoring	no offshoring
<i>Pseudo R-squared</i>	0.06	0.06	0.16
<i>Observations</i>	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 vertical integration 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.

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 branch. 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.

Balancing Tests

Matching estimators are biased unless the conditional independence assumption holds, i.e. the outcome in the treatment and the non-treatment case are independent of unobservable characteristics of the participation decision. Under this assumption the selection variables are balanced between the treatment and matched-control group (Rosenbaum and Rubin, 1983). A lack of balancing points at a violation of the conditional independence assumption or other misspecifications of the propensity score estimates. We employ a number of different balancing tests. First, we calculate the standardized difference between treatment and matched-control group of all selection variables at a time (see, e.g., Smith and Todd, 2005b; Caliendo and Kopeinig, 2008). Although there is no significance level on this statistic available, Rosenbaum and Rubin (1985) call the standardized difference large if it exceeds 20 percent. Second, a mean-difference t-test with standard deviations differing in treatment and matched-control group is performed. Third, we follow Smith and Todd (2005b) who propose a regression-based test. For each control variable X that is used in the propensity score, the following regression is estimated:

$$X = \alpha + \sum_{k=1}^4 \beta_k \hat{P}(X)^k + \sum_{k=1}^4 \gamma_k D \hat{P}(X)^k + \varepsilon,$$

where $P(X)$ represents the estimated propensity score and D is dummy variable that takes the value of one if an establishment offshored and zero otherwise. Smith and Todd (2005b) argue that a significant joint significant test over the γ -coefficients would indicate that balancing condition is not satisfied. Hence, we expect an insignificant F-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 control-group is significant at the conventional levels. The regression-based test also supports the propensity score chosen.

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 significance 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.¹¹ Hence, our estimated propensity scores secure balancing of selection variables in treatment and control group. Hence, we can condition the ATT on the estimated propensity score rather than each single selection variable.

Finally, we follow Imbens (2004), 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 treatment for an outcome variable that is predetermined and, hence, should not be affected by the treatment. If this effect is indeed zero, it renders the conditional independence assumption more plausible. Contrary to that, if it is not zero, this test indicates that the distribution of the lagged outcome variable is not comparable between the treatment and control group. 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, turnover, exports and productivity. Table A5 shows no evidence of a significantly different distribution of any of the four lagged outcome variables, corroborating the conditional independence assumption. We can also interpret this result as evidence that self-selection does not seem to play an important role here.

To sum up, none of the balancing tests indicate that treatment and control groups differ by observable characteristics. There is also no indication of a “treatment effect” before treatment actually takes place, excluding that time-invariant unobservable characteristics are responsible for differences in means of treatment and control group. This implies that differences in the outcome variables between the two groups will stem from differing offshoring strategies rather than from self-selection effects.

¹¹ Results are available from the authors upon request.

6) Results on offshoring

We present next our empirical results, following the four steps of the identifications strategy laid down in Table 1.

Step 1: Overall employment effect

Table 3 gives the results of difference-in-difference 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.

Interestingly, we find a *positive* and robust treatment effect on net employment at the plant-level. All point estimates have a positive coefficient and also vary relatively little with respect to the modified model. 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 the net employment situation for German establishments.

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

Time	OLS	ATT Preferred Model	ATT Modified Model
	(1)	(2)	(3)
1	0.0195 (0.0157)	0.0214** (0.0103)	0.0200** (0.0102)
2	0.0241** (0.0914)	0.0418*** (0.0130)	0.0401*** (0.0129)
3	0.0561** (0.0249)	0.0439*** (0.0150)	0.0453*** (0.0151)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via bootstrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in vertical integration 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.

This result is surprising, because it contradicts the conventional wisdom that offshoring destroys jobs. However, it can be indirectly related to a similarly surprising 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 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.¹² We can reconcile this result also with theory if the negative employment effect from downsizing is overcompensated by the employment growth through productivity gains. This will be the case in particular when the dominant type of offshoring is substitution of domestic for foreign suppliers. This will be investigated further in the next step.

Step 2: Finding prevalent channel

To investigate whether offshoring predominantly replaces own production or substitutes domestic suppliers, we turn now to the outcome variable intermediate inputs that measures the intermediate inputs as a share of total turnover. If offshoring, i.e. an increase in foreign inputs, substitutes domestic suppliers, then the intermediate input share will remain constant, since it contains both domestic and foreign inputs. If offshoring replaces, instead, own production, then the intermediate input share will rise, since production steps of the home plant are replaced by intermediate goods from abroad after offshoring.

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

Time	OLS	ATT Preferred Model	ATT Modified Model
	(1)	(2)	(3)
1	1.2183 (0.9871)	0.3117 (0.6199)	0.3708 (0.6261)
2	0.8404 (1.4643)	-0.5948 (0.7137)	-0.4279 (0.7223)
3	0.2168 (1.2686)	-0.6902 (0.7776)	-0.5486 (0.7915)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via bootstrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in vertical integration 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 counterparts without offshoring. Given that the intermediate inputs share of treated plants remains roughly stable, it becomes more likely that domestic intermediate input suppliers in the same industry are to certain extent substituted by foreign ones. Hence, we have an indication that the dominant type of offshoring in Germany may be the substitution of domestic by foreign supply. This is

¹² 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

consistent with an overall positive employment effect of offshoring, because there is no direct employment loss in an establishment from downsizing if domestic suppliers are merely replaced by foreign ones, but there may be a gain in competitiveness, market share, and employment if offshoring saves costs. For this potential explanation to be viable, we need to show next that offshoring does indeed increase productivity, sales, and/or exports. This will be done 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, which we cannot reliably estimate from our data. Then, we turn to the outcome variables establishment sales and exports.

The results of Table 5 on average labor productivity show indeed a positive and highly significant short-term productivity gain of 3.6 percentage points. The point coefficients hint at a slightly declining productivity difference between offshorers and non-offshorers and for $t=3$ the effect even becomes insignificant. While this productivity effect is noteworthy and also in line with for instance Grossman and Rossi-Hansberg (2008), 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 et al. (2007) find a positive, even though not necessarily causal, effect of offshoring on total factor productivity. Moreover, a positive effect of FDI on output is found by Barba Navaretti and Castellani (2004).

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

Time	OLS	ATT Preferred Model	ATT Modified Model
	(1)	(2)	(3)
1	0.0446*** (0.0134)	0.0362*** (0.0136)	0.0366*** (0.0137)
2	0.0449** (0.0150)	0.0298* (0.0168)	0.0308* (0.0170)
3	0.0475** (0.0160)	0.0256 (0.0191)	0.0254 (0.0188)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via bootstrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in vertical integration 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.

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.

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

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

Time	OLS	ATT Preferred Model	ATT Modified Model
	(1)	(2)	(3)
1	0.0446*** (0.0134)	0.0502*** (0.0144)	0.0495*** (0.0144)
2	0.0455*** (0.0131)	0.0583*** (0.0175)	0.0611*** (0.0175)
3	0.0529*** (0.0157)	0.0743*** (0.0199)	0.0769*** (0.0196)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via bootstrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in vertical integration 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.

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). 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. 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.

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

Time	OLS	ATT Preferred Model	ATT Modified Model
	(1)	(2)	(3)
1	1.2985** (0.4400)	0.9359*** (0.3570)	0.9175** (0.3575)
2	1.7321*** (0.5155)	1.1623*** (0.4384)	1.2177*** (0.4561)
3	2.6114*** (0.7538)	1.3176** (0.5578)	1.4682*** (0.5653)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via bootstrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in vertical integration 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.

Our analyses so far provides indirect evidence for productivity gains realized by firms that increased their share of imported intermediate goods.

To sum up results so far, an increase of employment in offshoring plants can be consistently explained by noting first that offshoring predominantly substitutes domestic for foreign supply, which shuts off the negative direct employment effect via downsizing in the offshoring plant, isolating the productivity effect on employment. Second, offshoring goes along with an increase in average labour productivity, sales and exports. Hence, our results are consistent with the existence of the productivity effect on employment in offshoring plants, which plays an important role in the recent offshoring model of Grossman and Rossi-Hansberg (2008). But is there also evidence for the negative direct employment effect via downsizing?

Step 4: Identification of direct employment effect via downsizing

To extract the direct employment effect via downsizing, we redefine the treatment variable as these offshoring cases, where is reported a restructuring of the offshoring plant concomitantly. We implicitly assume thus that restructuring is due to offshoring whenever offshoring is concomitant to restructuring. Such cases of offshoring are rather rare (about 1/10th of all 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, increasing (at least) foreign market share. Then, the direct employment effect must dominate the productivity effect if offshoring substitutes own production for foreign supply.

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

Time	Employment	Sales	Exports	Productivity
1	-0.0871* (0.0494)	0.0318 (0.0567)	4.2855*** (1.633)	0.1065** (0.0456)
2	-0.1873*** (0.0629)	-0.0361 (0.0796)	5.3028*** (2.059)	0.1189* (0.0645)
3	-0.1691** (0.0704)	-0.0541 (0.0869)	3.9936 (3.1173)	0.0909 (0.0846)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via bootstrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring cum restructuring* is defined as an increase in vertical integration 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 finish our empirical analysis by a number of robustness checks. First, one might argue that establishments in different industries should not be compared within a matching framework, because they are very different. 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 neighbour matching with two neighbours. 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 greatly change, but some interesting patterns emerge. While the employment effects are very similar in the overall economy as compared to the manufacturing sector, the positive effect on sales and productivity seems to be more pronounced in the manufacturing sector.

Finally, we provide two further robustness checks. On the one hand, 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. While the positive employment effect for the European Union turns out to be even stronger, the effect of offshoring to outside the European Union appears to be neutral. 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. On the other hand, we investigate, whether the offshoring effect depends on the degree of offshoring. Weak (strong) offshoring represents an increase of intermediate inputs from abroad from “not all” (“partly”) to “partly” (predominantly). While the point coefficients for employment and productivity are quite similar, strong offshoring seems to be associated with stronger sales growth, but no export growth. Still, the results for strong offshoring should be interpreted more cautiously, because there are relatively few observations underlying this class.

8) Conclusion

We have investigated employment effects of offshoring using a new plant level measure of offshoring for German establishment data from 1998-2004. When applying difference-in-difference matching techniques, we find surprisingly that employment increases when offshoring takes place. We explain this finding by noting that offshoring does not significantly increase production depth on average. Hence, offshoring substitutes in the majority of cases domestic for foreign supply. In this case, the only employment effect of offshoring on a plant derives from cost savings through offshoring (productivity effect), which increases the competitiveness of the offshoring plant relative to plants that do not

offshore. Through the gain in competitiveness, offshoring plants can gain market share at home and abroad, increasing their demand for labor.

An important consequence of this channel is that offshoring of this type may not destroy jobs in the offshoring plant itself but in the supply industry instead. Hence, overall employment effects will have to be gauged with caution, since suppliers cannot be traced back in data available to date.

Finally, when constraining offshoring to those cases where offshoring is concomitant to restructuring, then we do find a negative employment effect of offshoring. Since there is at the same time an increase in productivity and (foreign) market share, the negative direct employment effect via downsizing dominates the positive employment effect through the productivity gain on average when offshoring replaces production in the own plant with foreign supply.

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Appendix

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 abroad 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 F-stat (p-value)
<i>Total employment</i>	3.7338	3.7142	1.2	96.5	0.26 (0.79)	0.83 (0.51)
<i>Wage per employee</i>	7.3771	7.3779	-0.2	99.5	-0.04 (0.97)	1.24 (0.29)
<i>Technology</i>	0.7389	0.7338	1.1	91.2	0.27 (0.79)	0.63 (0.63)
<i>High-skilled</i>	0.3801	0.3818	-0.6	59.4	-0.14 (0.89)	1.54 (0.19)
<i>Foreign ownership</i>	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 F-stat follow 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 (0.0126)	0.0071 (0.0176)	0.6515 (0.5076)	0.0184 (0.0173)
OLS	0.0082 (0.0154)	0.0213 (0.0168)	0.3350 (0.3561)	0.0213 (0.0168)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via bootstrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in vertical integration 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* (0.0125)	0.0458*** (0.0174)	1.0392*** (0.3542)	0.0307* (0.0166)
2	0.0400** (0.0163)	0.0567*** (0.0194)	1.0393** (0.4454)	0.0291 (0.0191)
3	0.0529*** (0.0186)	0.06804*** (0.0234)	1.3492** (0.5579)	0.0142 (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 vertical integration 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** (0.0136) [0.0126]	0.0515*** (0.0172) [0.0169]	1.1129** (0.4447) [0.3937]	0.0333* (0.0177) [0.0164]
2	0.0532*** (0.0174) [0.0159]	0.0612*** (0.0229) [0.0205]	1.3401** (0.5406) [0.4851]	0.0172 (0.0216) [0.0188]
3	0.0539*** (0.0197) [0.0182]	0.0833*** (0.0266) [0.0241]	1.2853* (0.6991) [0.5894]	0.0228 (0.0252) [0.0218]

Notes: Standard errors in parentheses. Nearest-neighbor matching with two neighbors and caliper=0.05. For the matched sample standard errors are generated via bootstrapping (500 replications); furthermore, in squared brackets analytical standard errors are shown (Lechner, 2002); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in vertical integration 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* (0.0139)	0.0733*** (0.0222)	1.0462 (0.6495)	0.0596*** (0.0192)
2	0.0381** (0.0167)	0.0809*** (0.0267)	1.5793** (0.7953)	0.0577*** (0.0216)
3	0.0317* (0.0193)	0.0952*** (0.0305)	1.8756** (0.9048)	0.0544** (0.0238)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via bootstrapping (500 replications); *** denotes 99% significance level, ** 95% significance level, * 90% significance level; the treatment-variable *offshoring* is defined as an increase in vertical integration 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 (Tables 2-5, t=1)	0.0214** (0.0103)	0.0502*** (0.0144)	0.9359*** (0.3570)	0.0362*** (0.0136)
EU Offshoring	0.0466*** (0.0125)	0.0433** (0.0186)	0.7049 (0.4782)	0.0340** (0.0168)
Non-EU Offshoring	-0.0043 (0.0172)	0.0295 (0.0237)	1.2008 (0.7810)	0.0539** (0.0230)
Weak Offshoring	0.0304*** (0.0195)	0.0303** (0.0141)	0.8902** (0.4325)	0.0337** (0.0149)
Strong Offshoring	0.0269 (0.0244)	0.0654* (0.0353)	-0.0691 (1.4384)	0.0541 (0.0387)

Notes: Standard errors in parentheses. For the matched sample standard errors are generated via bootstrapping (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 vertical integration to EU or Non-EU countries between either in the year 1999-2000 or 2001-2002 for a certain plant; *Weak Offshoring* (*Strong Offshoring*) measures an from no (partly) imported inputs to partly (predominantly) imported inputs; non-treatment is defined as those plants that do not increase their vertical integration during the same time period.