Export Credit Guarantees:

Direct Effects on the Treated and Spillovers to their Suppliers*

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Most countries use export credit agencies (ECAs) to mitigate export-related credit constraints. We evaluate the direct effects of this policy on treated firms, as well as spillovers to non-treated firms. Using data from the Danish ECA and applying a difference-in-difference matching estimator, we find large positive effects on total sales, exports and purchases of treated firms. Next, we derive new measures for horizontal and backward spillovers based on production and purchase statistics. Our results show that upstream firms benefit from export credit guarantees issued to their potential customers, but that domestic producers are not harmed by guarantees given to their competitors.

Keywords: Public export credit guarantees · Spillover effects · Export finance.

JEL codes: F12, F14, L15.

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1 Introduction

Most countries around the world promote exports through export credit agencies (ECAs), which provide state-backed credit guarantees for export transactions. Figure 1 shows the evolution of guarantees issued by EKF Danmarks Eksportkredit (EKF), the Danish export credit agency, over the period 2004–2015. The 2008–09 crisis hit exporters harder than other firms, and accordingly we see a substantial increase in the operations of EKF. For example, the total volume of newly issued export credit guarantees has increased from 4.4 billion DKK in 2004 to almost 14 billion DKK in 2015, with a sharp acceleration after 2008.

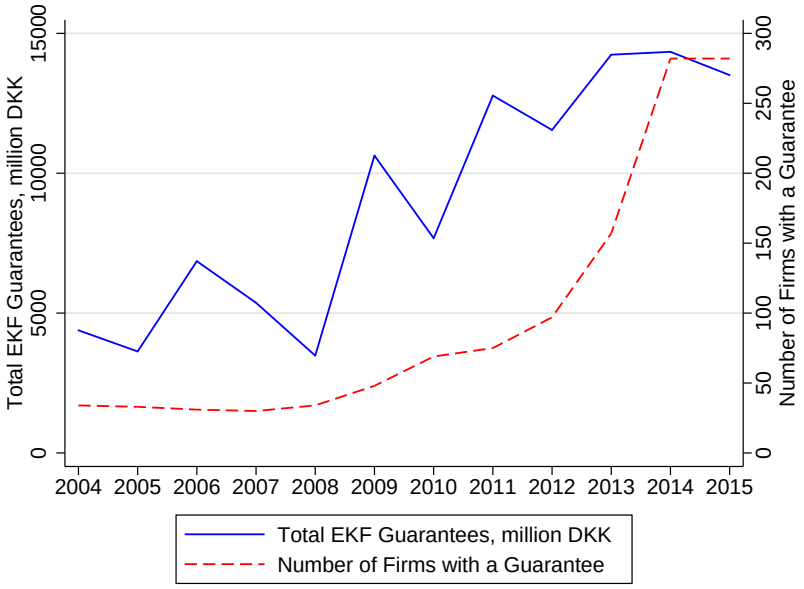


Figure 1: Evolution of New Export Credit Guarantees in Denmark

Figure 1 also reveals another important feature of state-backed export credit guarantees: only a small number of selected firms benefit directly. In Denmark, 282 firms were recipients of newly issued guarantees in 2015 (up from 34 firms in 2004).[1,2] Over recent years, a number of programs which are directed specifically towards small and medium-sized enterprises have been introduced. Nevertheless, firms that receive guarantees are, on average, larger and more internationalized than those that do not. In part, this pattern reflects the nature of state-backed export credit guarantees: these guarantees are used to alleviate credit constraints for

[1]In many cases, guarantees are, in fact, issued to the bank providing finance for the export transaction of the Danish firm; see Section 3 for more details. For conciseness (and following previous literature), we will refer to the firms whose export transactions are financed as the recipients of the guarantees.

[2]Export credit guarantees can have credit periods of several years. The figure only considers *newly* issued guarantees in each given year.

export projects which are hard to finance via private capital markets; e.g., because the export volume is particularly large and/or the insured exports are to markets which entail large risks.

Two important questions arise from these stark patterns in the data: first, do firms that receive state-backed credit guarantees perform better than other firms because they are favourably selected, or do credit guarantees actually help in further improving firm performance? Second, what are the indirect effects of export credit guarantees on the vast majority of firms that do not benefit directly? A joint analysis of the effects of credit guarantees issued by ECAs on outcomes for both treated and non-treated firms allows for a more complete picture of the workings of this policy tool. In particular, opponents of export credit agencies assert that "(...) these agencies benefit particular firms at the expense of other firms, which will face unfair competition" (James, 2011, p. 10). Thus, they contend that ECA credit provision could have negative spillovers to non-treated competing firms. In stark contrast, ECAs themselves often claim that the positive effects of their credit provision extends to suppliers of the firms that are the direct beneficiaries – i.e., that there will be positive spillovers to non-treated upstream firms. Empirical evidence on both sets of spillovers is, however, scant.

The first question on the direct effects on the treated is a standard question in economic policy evaluation, and it has been the main focus in the literature on export credit agencies to date. It is a particularly topical question in the case of export credit guarantees because private capital markets and state-backed export credit programs provide two alternative modes of financing export transactions.[3] We apply a difference-in-difference matching estimator to estimate the direct effect of export credit guarantees on firm performance. We find large effects of EKF guarantees on the treated: on average, sales growth is boosted by 9.8 percentage points, export growth by 16.4 percentage points and growth of total purchases by 7.3 percentage points.

The second question on the existence and size of potential spillovers to competitors and suppliers of the treated firms has received much less attention in the academic debate on ECAs. Here, we exploit detailed production and purchase statistics at the firm-product level to derive novel measures of horizontal and backward spillovers. First, we combine product-level information on intermediate input purchases of treated firms within the manufacturing industry with product-level production data for the non-treated firms. The product classification in both the production and purchase statistics is equivalent up to the four-digit level; and we can therefore

[3]Note, however, that in many countries – including Denmark – ECA financing can by law only be used in cases where alternative financing is unavailable; see Act 2016 104 (2020) for the legal provisions in Denmark and Freund (2016) for a discussion of the US case.

use these data to identify potential supplier-customer relationships. Second, we use the same firm-product-level production data to identify the competitors of the treated firms.

Previous literature on spillovers between firms has typically used industry-level input-output tables to identify backward linkages and a firm's main industry of activity to identify horizontal linkages between firms. In contrast, our spillover variables are derived based on firm-specific production and purchase patterns. This is particularly important in our framework because the treated firms from which spillovers emanate are often multi-industry firms. At a more general level, our approach allows for a more precise identification of the set of firms which are potentially affected by spillovers, thereby reducing measurement error and attenuation bias.

We find evidence of positive and significant backward spillovers from EKF guarantees to upstream firms. At the firm-product level, we predict that sales increase by 3.91 percent if the potential customers of a given product together receive export credit guarantees in the range of 7.45–74.5 million DKK (approx. 1–10 million EUR). This effect further increases to 5.47 percent if guarantees exceed 74.5 million DKK. We deem these effects to be economically significant, especially if one takes into account that they reflect an *average* effect across *all potential* suppliers. Moreover, these positive effects do not merely reflect a within-firm reallocation of resources across products: firm-level sales and employment also increase in response to an increase in EKF credit to a firm's potential customers. A simple back-of-the-envelope calculation reveals that the employment effects at upstream suppliers can, indeed, be larger than the direct employment effects at treated firms.

In contrast, we do not find evidence of horizontal spillovers to the competitors of the treated, independent on whether we consider outcomes at the firm-product or firm level. This finding is important for policy makers because it suggests that credit guarantees given to one firm do not crowd out production at its domestic competitors. The evidence against horizontal spillovers also implies that the Stable Unit Treatment Value Assumption (SUTVA) – stating that any individual firm's outcome does not depend on the treatment status of other firms in the same industry – is likely to be satisfied when we estimate the direct effects on the treated in our matching approach (cf. above).

This paper proceeds as follows: Section 2 provides a brief literature review. Section 3 explains the policy background, presents our conceptual framework and introduces the data. Section 4 discusses our empirical approach of identifying the direct effects of export credit guarantees on the treated and presents results from the matching estimator. Section 5 explains how we

construct measures of horizontal and backward spillovers and presents evidence on such spillovers at the firm and firm-product level. Section 6 concludes.

2 Related Literature

Our study is related to two strands of literature.

First, we build on the literature on public export credit guarantees. A first generation of empirical studies has employed industry- or country-level data; see, *inter alia*, Moser et al. (2008), Felbermayr & Yalcin (2013) and Auboin & Engemann (2014). Felbermayr et al. (2012) provide the first empirical analysis of ECAs using firm-level data. They find that state export credit guarantees in Germany increase firms' sales growth by about 4.5 percentage points. Though we estimate considerably larger treatment effects on firms' sales, we note that these estimates are not directly comparable because of differences in the coverage ratio (guarantees over sales) across samples. In addition to the firm-level outcomes considered in Felbermayr et al. (2012), we show that positive treatment effects also prevail in terms of firms' total purchases. These large, positive effects on firm sourcing motivate our analysis of backward spillovers of ECA credit guarantees to the potential suppliers of treated firms.

Using the same German data as Felbermayr et al. (2012), Heiland & Yalcin (2020) analyse heterogeneity in the effect of export credit guarantees: they show that guarantees are particularly effective for firms that are dependent on external finance, for projects with large values at risk and during periods when financing conditions on the private capital market are tight. Using Swedish data, Lodefalk et al. (2019) are in a position to study outcomes at lower levels of aggregation, such as a firm's destination-specific export probability and export value. Similar to Felbermayr et al. (2012) and our study, they do so using difference-in-difference matching techniques and find positive effects both on the extensive and the intensive margin of exporting.

In contrast to these latter two studies, we do not aim to provide a detailed analysis of the heterogeneity in the effectiveness of ECAs across firms. Rather, results from our matching procedure serve as a background and motivation for the analysis of spillovers from credit guarantees to the non-treated.[4]

Second, we build on a sizeable literature on spillovers between firms. In the field of international trade, this literature has focused on productivity spillovers from foreign direct invest-

[4]More broadly, our study is also related to the literature on the effectiveness of other types of export promotion services; see, in particular, Munch & Schaur (2018) and Buus et al. (2021) for evidence from Denmark.

ments[5] and knowledge spillovers from exporting to non-exporting firms.[6]

We contribute to this literature by proposing new measures of backward and horizontal spillovers. As noted by Barrios et al. (2011), most studies derive proxies for backward spillovers using aggregate industry-level input-output tables. These studies thereby rely on the assumption that the sourcing behaviour of treated firms does not differ from the one of non-treated firms. Using detailed firm-product-level purchase data allows us to depart from this assumption and exploit variation in sourcing behaviour not just across industries but also across firms.[7]

Most closely related to our work are two recent studies by Carballo et al. (2021) and Girma et al. (2020), both of which also study spillovers from specific export-related policy instruments. Carballo et al. (2021) study the indirect benefits of export promotion assistance, exploiting data on firm-to-firm transactions in Uruguay. Consistent with our findings, they show that policies that stimulate exports can have positive effects on sales and employment also at domestic suppliers of the treated. Girma et al. (2020) provide a joint analysis of direct effects and horizontal spillovers for the case of production subsidies in China. Interestingly, they find that positive effects on the probability of exporting for treated firms are counterbalanced by negative effects for non-treated competitors. Different from these two studies, we analyse spillovers to non-treated firms for the case of export credit policies, jointly addressing potential effects on both domestic suppliers as well as on domestic competitors of the treated.

3 Policy Background, Conceptional Framework and Data

3.1 Export Credit Guarantees in Denmark

EKF, the Danish export credit agency, is owned and guaranteed by the Danish Ministry of Industry, Business and Financial Affairs. Its stated aim is to "support Danish exports and to create growth and jobs in Denmark".[8] To this end, EKF provides Danish firms with export

[5]In her pioneering work, Javorcik (2004) provides the first study that extends the analysis of FDI spillovers to backward linkages across firms.

[6]See, e.g., Aitken et al. (1997), Greenaway & Kneller (2008), Koenig et al. (2010). Choquette & Meinen (2015) provide evidence of positive export spillovers using the same Danish register data that we exploit in our analysis.

[7]Previously, Javorcik & Spatareanu (2009), Godart & Görg (2013) and Gorodnichenko et al. (2014) have employed firm-level information on linkages with treated firms in order to derive better proxies for backward spillovers from foreign direct investments. These authors measure exposure to multinational firms downstream using survey information on whether an upstream reports having a multinational as customer. Our data does not allow us to directly identify such linkages from treated to non-treated firms. On the other hand, our approach has the benefit of using product-level sourcing behaviour of treated firms, providing our analysis with additional margins of variation.

[8]See www.ekf.dk for details.

credits, working capital guarantees and loans.[9]

Broadly, the different instruments offered by EKF can be classified into two groups: financing of a foreign buyer and finance of a Danish exporter. In the first case, the instrument is directed towards one specific export transaction. In the second case, the instrument covers expenses for new machinery, equipment, working capital, etc. and is aimed at helping Danish exporters take on more foreign customers, or bigger export orders. In our sample period, the first type of instrument is more important in terms of both the number of guarantees issued as well as in terms of total credit volume. In most parts of the analysis, we will not distinguish between the different types of instruments, and we will refer to them as "export credit guarantees" in the following. When making the distinction, we will refer to them as buyer finance and capital guarantees, respectively.

EKF guarantees to a foreign buyer are primarily used in connection with purchases of capital goods and with longer credit periods. Moreover, these guarantees are more important for exports to certain, high-risk destinations, where private banks are unable or unwilling to provide credit. As a result, larger firms tend to benefit more from this type of instrument, because these firms are more likely involved in large export transactions to risky destinations.[10] Over time, however, new types of guarantees – especially, new types of capital guarantees – have been introduced, some of which were specifically directed towards small and medium sized firms. As a result, the fraction of firms receiving guarantees that are small has increased considerably, from 19 percent in 2004 to 74 percent in 2015.[11]

Consider the typical steps that are taken when financing an export transaction: in some cases, the foreign buyer will contact a local or an international bank to obtain financing for purchasing goods from a Danish company; in other cases, the Danish exporter might contact its own bank in order to extend credit to its foreign customer, or to obtain credit for its working capital. In either case, the bank – depending on its risk perception, capital and liquidity position – may suggest to involve EKF.[12] If EKF agrees to issue a guarantee, part of the risk of financing

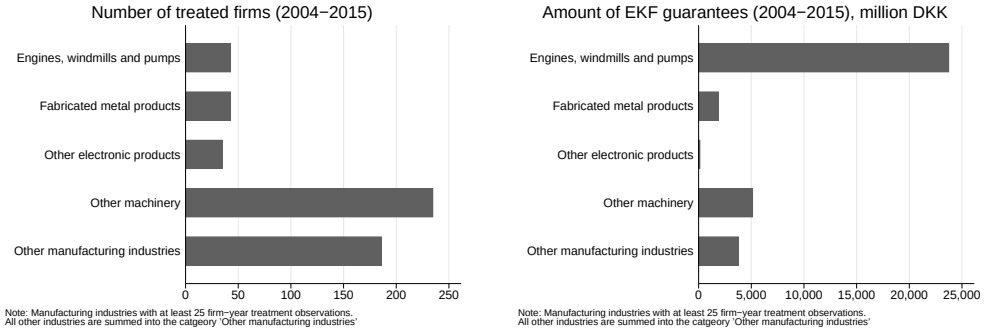
[9]Even though ECAs may be seen as providing prohibited export subsidies, the WTO Agreement on Subsidies and Countervailing Measures makes an exemption if at least twelve GATT members take part in an "international undertaking on official export credits"; see Felbermayr et al. (2012) for a discussion. For Denmark, the OECD Consensus Arrangement regulates the conditions for the issuance of guarantees.

[10]In our sample, the average treated firm has 318 employees, compared to 14 employees at the average non-treated firm; see also the summary statistics reported in Table B.1.

[11]Small firms are here defined as firms with 50 or less employees. Note that small firms accounted for 83 percent of all Danish firms in our sample in 2015; as a result, the probability of receiving a guarantee is nevertheless still positively related to firm size even at the end of the sample period.

[12]This series of events shows how private banks have a pivotal role in the application process for an export

Figure 2: EKF Credit Guarantees by Manufacturing Industry



(a) Number of Firm-Years with Treatment (b) Amount of EKF Credit Guarantees

the transaction is assumed by EKF.[13] At the same time, the exporter will need to pay a risk premium.[14]

EKF has provided us with access to detailed data on the universe of export credit guarantees for the period 2004 to 2015. Using a unique firm identifier, we can link these data with standard register data from Statistics Denmark. We describe the data in more detail in Sections 4.2 and 5.2 below and in Section A of the Online Appendix. Notably, the manufacturing sector is dominating in terms of the number of firms that receive guarantees.

Our analysis of spillover effects relies on a subsample of manufacturing firms. Figure 2 shows the distribution of treated firms across manufacturing industries. We observe only four industries with more than 25 firm-years of treatment: *Engines, windmills and pumps*; *Fabricated metal products*; *Other electronic products* and *Other machinery*. (For confidentiality reasons, we summarize all other industries under the headline *Other manufacturing industries*.) As can be seen, *Other machinery* is the industry with most firm-years with positive treatments, but *Engines, windmills and pumps* are dominating in terms of total credit received. Overall, these patterns accord with the circumstance that credit guarantees are mainly relevant for capital goods, where export transactions are large and the typical transaction involves long-term financing.

credit guarantee, and it might be important to control for private banking conditions when estimating the direct effects of EKF credit on the treated. We do so in our robustness analysis; cf. Section 4.4 below.

[13]We do not have information on instances where a firm that applied for a guarantee from EKF was denied credit. However, we note that such instances would not be random and could thus not be used to form a suitable control group for the treatment analysis.

[14]In fact, over recent years, EKF has consistently operated with a net profit; see EKF's annual reports, available at `https://www.ekf.dk/en/about-ekf/ekf-s-organisation/annual-reports`.

3.2 Direct Effects and Spillovers from Export Credit Guarantees

As highlighted above, the nature of export transactions that are supported by EKF implies that large firms are more likely the recipients of EKF's guarantees. An important question is, therefore, whether export credit guarantees are effective in promoting exports, or whether these guarantees merely support firms that are already successful exporters ex-ante. In Section 4, we use propensity score matching techniques to disentangle the selection from the treatment effect. In addition to firms' exports and total sales, we also study other outcomes of interest (such as domestic sales and purchases) that provide background and motivation for our spillover analysis in the second part of the paper.

There are indeed several channels through which firms that do not receive any guarantees themselves may nevertheless be affected indirectly by the policies of ECAs.

First, consider competitors of treated firms. Access to EKF guarantees may give treated firms a competitive advantage over non-treated firms, thereby reducing their competitors' sales (negative horizontal spillover). The extent to which such spillovers materialize will depend on whether treated and non-treated firms are actually competing on the same markets. Notably, export credit guarantees are mainly used in connection with sales to risky markets, or in connection with very large export transactions. Arguably, most of the non-treated firms (which are, on average, much smaller) are not competing with treated firms on these markets or for these types of contracts. However, export credit guarantees may have effects on the treated firms' sales not just on the specific export market for which a guarantee is received, but also on domestic markets where they are competing with mainly smaller, non-treated firms.

One channel through which domestic sales of treated firms could be affected, is the within-firm correlation of sales across markets. The sign of this correlation is, however, ambiguous. Under capacity constraints, domestic sales of treated firms might actually contract due to the increase in exports; see, *inter alia*, Ahn & McQuoid (2017). However, if domestic sales and exports are complements, the extension of EKF credit may have positive effects also on treated firms' domestic sales (Berman et al., 2015). For example, complementarity could arise through the positive effect of increased exports on firm liquidity, which may give a boost also to treated firms' domestic sales. In such case, domestic competitors of treated firms may suffer from the credit provision of ECAs.

Competitors of treated firms may be affected also through knowledge spillovers (positive horizontal (export) spillovers). In fact, knowledge about export opportunities and international

markets is often transmitted across firms (see, e.g., Aitken et al. (1997) and Koenig et al. (2010)). If EKF guarantees allow treated firms to venture into new export markets, such positive effects may benefit also competitors of the treated.[15]

Second, consider suppliers of the treated firms. If the increased economic activity of firms receiving EKF guarantees increases their demand for intermediate inputs, positive effects may result for upstream firms; i.e., domestic suppliers of the treated firms (positive backward spillovers). The extent of such positive backward spillovers will, however, depend on whether treated firms actually increase their domestic purchases or meet their increased demand for intermediate inputs entirely through an increase in international sourcing (i.e., firm imports). Moreover, learning effects in export markets operate also across industries due to supply chain linkages (Choquette & Meinen, 2015). Thus, upstream firms may benefit from positive export spillovers as well.

In sum, the direction (and magnitude) of any potential effects of export credit guarantees on non-treated firms remains an empirical question. Positive spillover effects imply that the benefits of export promotion may exceed the direct benefits to treated firms. On the other hand, negative spillovers have opposite implications. Thus, analysing these spillovers entails important insights for policy makers.

4 Direct Effects of Export Credit Guarantees on the Treated

This section investigates the direct effects of export credit guarantees on the treated. First, we are interested in whether firms receiving a guarantee experience an increase in their exports and total sales. Second, we consider effects on domestic sales, which might give rise to negative horizontal spillovers to non-treated competitors. Finally, we ask whether treated firms raise their purchases (of intermediate and/or capital goods), which could be a source of backward spillovers to domestic suppliers.

Due to the nature of export transactions which qualify for credit guarantees from ECAs, the group of firms receiving credit clearly does not constitute a random sample. We address this identification challenge by applying a difference-in-difference matching estimator.[16] In particular, we resort to propensity score matching (PSM) techniques and, thereby, identify the effect

[15]Export spillovers need not always be positive, however; see Ciliberto & Jäkel (2021) for evidence of negative competitive effects in export market decisions for the subset of large ("superstar") exporters.

[16]See Munch & Schaur (2018) for a recent application of this estimator in the context of export promotion services in Denmark.

of credit guarantees on the change in the outcome variables by creating a control group of firms that do not receive export credit guarantees but exhibit a statistically similar propensity to obtaining them.

4.1 Empirical Setup

We define a treatment dummy variable, EKF_{it}, that equals one for firms that receive an export credit guarantee in period t, but do not do so in period $t-1$, while it takes on zero for firms that do not receive any guarantees during both years. Firms that receive treatment in both periods are dropped from the estimation.[17] Our goal is to compute the difference between y_i^1 and y_i^0, where y_i denotes the outcome variable of interest for firm i, and the superscripts 1 and 0 indicate the firm's treatment status. More specifically, we are interested in the average treatment effect on the treated, defined as:

$$E[(y^1 - y^0)|EKF = 1] = E[(y^1)|EKF = 1] - E[(y^0)|EKF = 1]. \tag{1}$$

Equation (1) is subject to the fundamental evaluation problem since we do not observe the counterfactual outcome $E[(y^0)|EKF = 1]$; i.e., the expected outcome of a treated firm had it not received an export credit guarantee. One strategy for addressing this problem is based on the conditional independence assumption. In particular, we assume that – conditional on a set of observable firm-level characteristics that are unaffected by export credit guarantees – potential outcomes are independent of treatment. We therefore construct a group of comparison firms that are as close as possible to the treated firms in terms of their propensity to obtain a guarantee from EKF, given observable firm characteristics. We do so by following the insights presented by Rosenbaum & Rubin (1983a,b) and apply propensity score matching (PSM).[18]

The first stage of the matching approach involves estimating the probability of obtaining a guarantee:

$$Pr(EKF_{it} = 1) = \Phi\{z_{i,t-1}\}, \tag{2}$$

where $\Phi(\cdot)$ is the normal cumulative distribution function, indicating that we employ a probit model. $z_{i,t-1}$ contains firm-level control variables lagged by one year; i.e., one period before

[17] Indeed, some treated firms receive multiple treatments throughout our sample period. In our robustness analysis, we condition on two-years of non-treatment or focus on the first treatment, and confirm that both sample restrictions do not significantly affect any of our results.

[18] See Caliendo & Kopeinig (2008) for an overview of propensity score matching techniques.

obtaining the guarantee. We discuss the choice of these variables in Section 4.2 below.

The second step of the matching approach involves the search for a control group that is similar to the treated firms according to the propensity score estimated by the probit model. In particular, we apply nearest neighbor matching with a tight caliper and impose common support to ensure that the balancing property holds. In the baseline estimation, we allow for up to five nearest neighbours. In other words, for each treated firm, we search for a control group that consists of up to five non-treated firms that differ in terms of the propensity score by no more than a pre-specified maximum distance (i.e. a caliper of 0.01).[19] We can then compute the average treatment effect on the treated (ATET) as:

$$\text{ATET} = \frac{1}{N}\sum_{i}(y_i^1 - \sum_{j \in C_i} w_{ij} y_j^0), \tag{3}$$

where N refers to the number of treated firms, C_i to the set of control firms matched to each treated firm $i = 1, ..., N$, and w_{ij} is a weight such that $w_{ij} = \frac{1}{N_i^C}$ if $j \in C_i$ and zero otherwise, with N_i^C denoting the number of control firms. Below, we compute ATETs from differences in outcome variables, implying that we combine propensity score matching with difference-in-differences estimations (Smith & Todd, 2005). Even though this approach still relies on the assumption of "selection on observables", taking differences accounts for potential biases related to time-invariant firm characteristics. We compute ATETs by means of weighted regressions, using sampling weights obtained from the matching approach and clustering the standard errors at the firm level.

Identification of treatment effects via this matching estimator relies on the Stable Unit Treatment Value Assumption (SUTVA); i.e., the assumption that any individual firm's outcome does not depend on the treatment status of other firms. This assumption is violated in the presence of spillovers between firms. Depending on the sign of these spillovers, we may overestimate or underestimate the direct effects on the treated. Importantly, we match firms within industries, and thus assume that the SUTVA holds within industries but not necessarily across industries. The SUTVA would thus be violated only in the presence of horizontal (intra-industry) spillovers, for which we find little empirical support (cf. Section 5 below).[20]

[19] We implement the matching algorithm using the Stata program `psmatch2` written by Edwin Leuven and Barbara Sianesi. We estimate the probit model pooled across all industries and years, but ensure exact matching by year and industry in the second step of the matching approach.

[20] One caveat is that, due to the way in which we construct our spillover variables, even backward spillovers might be to some extent of an intra-industry nature. Importantly, we always estimate these effects to be positive.

4.2 Additional Data, Sample and Summary Statistics

Our initial sample covers the period 2004–2015. Since we condition on not receiving treatment in $t-1$, our final estimation sample for this part of the analysis only exploits the 2005–2015 period. In order to analyse the direct effects of export credit guarantees on the treated, we construct two different samples: Our first sample encompasses firms from all sectors.[21] Our second sample considers manufacturing firms only.

We combine the data provided by EKF with detailed register data from Statistics Denmark. We employ the general firm statistics and the accounting statistics to derive variables such as revenue, employment, value added, wages, etc. From the external trade statistics, we construct total firm-level exports and imports of goods. All data sources and the data cleaning are described in more detail in the Online Appendix.

Using these data sources, we choose the set of covariates used in the estimation of the propensity score in Equation (2) such that it includes factors which are expected to be related both to the probability of treatment as well as to the outcomes of interest. In particular, the vector $z_{i,t-1}$ includes measures of productivity (value added per employee), size (number of employees and total assets), and quality of the work force (average wages). All three variables – productivity, size and skill composition – are positively associated with firms' international engagement. In addition, we include indicators for export and import status in the pre-treatment year, as well as lagged sales growth.

Moreover, we note that firms' financial position is also likely related to both the probability of applying for a guarantee as well as our outcomes of interest. First, following Manova (2013) and others, we proxy a firm's collateral by tangible assets over total assets: Tangible assets may be used as collateral in raising outside finance; and firms with a higher ratio of tangible to total assets might thus be less vulnerable to financial frictions (and, consequently, be less likely to apply for an export credit guarantee). Second, we measure the liquidity ratio as cash and cash equivalence over total assets. Firms with a higher liquidity ratio may be less reliant on external capital to finance their operations. Third, following Felbermayr et al. (2012), we also use the current ratio (current assets over current liabilities) as an additional measure of firms' financial stance. This ratio measures a firm's ability to pay off its current liabilities with its current

Thus – to the extent that some of the backward spillovers identified in Section 5 indeed are of an intra-industry nature – our matching approach would *underestimate* the treatment effects on the treated.

[21] We drop observations from sectors where treatment is very rare; cf. Online Appendix A.

assets.

Finally, we add two-digit NACE industry and year dummies to the probit regressions.

Table B.1 in the Online Appendix provides summary statistics for the matching variables, and reports the coefficient estimates from the first-stage probit model. In both the exhaustive sample as well as the manufacturing subsample, we indeed see substantial differences between treated and control firms before matching. The table also presents information about the balancing properties of the covariates, confirming that the matching strategy leads to very similar means between treated and matched non-treated firms pre-treatment. In particular, the p-values indicate that we can never reject the null hypothesis of equal means, and we obtain fairly low numbers for the standardized bias.

4.3 Results

The main results for a number of outcome variables of interest are presented in Panels A and B of Table 1 for the exhaustive sample and manufacturing subsample, respectively. Even though the sample in Panel B is considerably smaller than the sample in Panel A, results from our matching estimator are broadly consistent across both.

In a first step, we estimate ATETs for the pre-treatment period. This analysis functions as a placebo test: prior to treatment, we would expect that firms differ neither in terms of the control variables (as confirmed by the balancing tests) nor the outcome variables. Indeed, the estimated effects are never statistically significant.

Next, we turn to the ATETs in the treatment year. We obtain positive and significant effects on firms' sales; cf. column (1). The coefficient estimates suggest that these effects are substantial, amounting to an increase in sales growth by 8.4 to 9.8 percentage points. A simple back-of-the-envelope calculation helps to put these numbers into perspective: they imply that, on average, a guarantee of 1 million DKK is predicted to increase a firm's sales by roughly the same amount.[22]

The results in columns (2) and (3) confirm that this effect is indeed due to an increase in treated firms' export growth, which is spurred by on average 17.8 percentage points in the exhaustive sample and 15.7 percentage points in the subsample of manufacturing firms. In con-

[22]Take, for example, the full sample of Panel A: the average sales of treated firms in the pre-treatment year amount to 256 million DKK; we thus predict sales to grow by 25 million DKK from the pre-treatment to the treatment year. Given that the average guarantee in this sample amounts to 23 million DKK, this is approximately a one-to-one increase. For the manufacturing subsample of Panel B, we similarly predict an effectiveness ratio (additional sales over guarantees issued) somewhat larger, but close to, 1.

Table 1: Estimation Results of the Matching Approach

	Total Sales (1)	Exports (2)	Domestic Sales (3)	Total Purchase (4)	Imports (5)	Domestic Purchase (6)	Employ-ment (7)	Value Added (8)
Panel A: Full sample								
ATET$_{t-1}$	-0.0155	-0.0032	-0.0185	-0.0025	0.0227	-0.0032	0.0082	0.0111
	(0.0223)	(0.0558)	(0.0378)	(0.0208)	(0.0687)	(0.0270)	(0.0115)	(0.0229)
ATETt	0.0982***	0.164***	0.0651+	0.0732***	0.124*	0.0459*	0.0435***	0.0755***
	(0.0218)	(0.0453)	(0.0401)	(0.0211)	(0.0684)	(0.0275)	(0.0111)	(0.0219)
Observations	3,181	2,745	2,749	3,164	2,662	3,020	3,173	3,129
N treated	568	495	491	568	483	543	568	560
Panel B: Manufacturing firms								
ATET$_{t-1}$	-0.0119	-0.0618	-0.0269	0.0019	-0.0158	-0.0163	0.0112	0.0145
	(0.0242)	(0.0593)	(0.0515)	(0.0239)	(0.0777)	(0.0302)	(0.0142)	(0.0280)
ATET$_t$	0.0842***	0.182***	0.0226	0.0968***	-0.0014	0.0777***	0.0567***	0.0417+
	(0.0225)	(0.0548)	(0.0544)	(0.0201)	(0.0835)	(0.0267)	(0.0123)	(0.0262)
Observations	1,662	1,529	1,400	1,665	1,467	1,618	1,661	1,633
N treated	307	286	261	307	270	297	307	302

Notes: All dependent variables are measured in log-changes. Standard errors, adjusted for clustering by firm, are given in parentheses. *,**,*** denote significance at the 15%, 10%, 5%, 1% levels, respectively. A common support restriction has been imposed.

trast, estimated treatment effects on domestic sales are not statistically significant at standard levels.[23] We consider this finding an important first indication that domestic competitors are unlikely to suffer adverse effects from ECA credit provision.

One should note that comparing our estimates to those in previous studies is complicated by the fact that estimated treatment effects will highly depend upon the (average) treatment intensity. For example, Felbermayr et al. (2012) estimate smaller treatment effects of 4–4.5 percentage points for firms' sales growth; but the coverage ratio (i.e., the ratio of the guarantees issued to a firms' total revenue) is also significantly smaller in their data compared to ours. In contrast, Lodefalk et al. (2019) find much larger treatment effects on exports measured at the firm-destination level compared to our treatment effect for total firm-level exports. Again, these numbers are not directly comparable, since the coverage ratio (which we might also think of as the "treatment intensity") is, by construction, much higher at lower levels of aggregation.

In column (4), we next present ATETs for firms' total purchases. Note that total purchases comprise both domestic purchases of goods and services as well as imports of goods (but not imports of services). Moreover, purchases of goods include both intermediate inputs as well as capital goods. Results confirm that treated firms see a notable increase in the growth of their purchases of 7.3 percentage points in the exhaustive sample to 9.7 percentage points in the manufacturing subsample; cf. column (4).

Next, we split total purchases into imports and domestic purchases. Total imports are derived

[23] In the exhaustive sample, the ATET has a p-value of 0.104, and is thus only marginally insignificant at the 10% level.

from the external trade statistics, while domestic purchases are constructed as the difference between total purchases and imports.[24] In the exhaustive sample, we find positive treatment effects on both outcomes, whereas effects are significant only for domestic purchases in the manufacturing subsample; cf. columns (5) and (6). Thus, importantly, the increased purchases at treated firms can be expected to at least partly benefit domestic suppliers. However, given the positive effect on firm imports in Panel A, part of the positive backward spillovers from the policy may indeed accrue to foreign firms.

In the last two columns, we examine treatment effects on employment and value added: in fact, one of the stated aims of the export credit agency is to "increase employment in Denmark". We indeed find positive and significant effects on employment of 4.4–5.7 percentage points. The estimated effect on value added is, in contrast, only significant in the full sample of Panel A.

Thus far, we have reported treatment effects for the year in which a guarantee is issued. However, guarantees may be issued in one year to finance export transactions in subsequent years. Next, we therefore test whether treatment effects continue to be significant in the year after treatment; i.e., the year after the guarantee is issued. With the exception of export sales in the exhaustive sample, we do not find much evidence that the growth in any of our considered outcomes at treated firms continues to increase even further in the subsequent period; i.e., from t to $t+1$. This finding is informative for our spillover analysis in the following section, since it also implies that any indirect effects on non-treated firms are likely to be largest in the year of treatment. Notably, estimated treatment effects from t to $t+1$ are also not negative; i.e., the positive effects of receiving treatment are not reversed in subsequent periods.

4.4 Robustness Analysis

Controlling for Bank-Level Covariates. Private banks play a pivotal role in obtaining an export credit guarantee in Denmark; cf. Section 3. As a robustness check, we therefore aim to account for differences in conditions faced by individual firms on the private banking market. Unfortunately, the necessary data are not available for all firms and years in our sample, implying a notable reduction in the number of treatment observations that can be exploited. For this reason, we report details of this analysis in Section B.2 of the Online Appendix. Overall, our results are broadly confirmed when controlling for bank-level covariates in our matching approach.

[24] Because the external trade statistics only cover imports of goods (but not services), domestic purchases will be measured with error. Given that only few firms engage in services trade (see Breinlich & Criscuolo (2011)), we expect such measurement error to be small.

Controlling for Previous Export Experience. Recall that export credit guarantees are more likely used in connection with specific types of export destinations and/or products. One potential concern is that systematic differences in export growth rates for these destination or product markets compared to other markets could bias our results. In Online Appendix B.3, we report two robustness checks which exploit information on firms' export experience prior to treatment to ameliorate this concern.

Buyer Finance vs. Capital Guarantees. An interesting question is whether the two types of guarantees – buyer finance vs. capital guarantees – have different effects on firm performance. When we distinguish the two types of instruments, we find positive (and fairly similar) ATETs for both; cf. Section B.4 in the Online Appendix.

Small vs. Large Firms. Since our analysis of backward spillovers in the following section only exploits a sample of treated firms with at least 50 employees, in Online Appendix B.5 we show that treatment effects are positive and significant for most outcome variables of interest (and, in particular, for firms' total purchases) also if we split each sample into firms with below and above 50 employees.

5 Spillovers from Export Credit Guarantees to the Non-Treated

In the previous section, we have provided suggestive evidence that *(i)* horizontal spillovers from export credit guarantees to domestic competitors of the treated are unlikely to be large (cf. the insignificant treatment effects on domestic sales);[25] and *(ii)* backward spillovers to domestic suppliers, in contrast, might be important (cf. the positive and significant treatment effects on firms' purchases). However, a complete analysis of horizontal spillovers needs to take into account that treated and non-treated firms compete both on domestic as well as on export markets. Moreover, given data constraints, we had to *infer* domestic purchases of treated firms in the previous section. In addition, directly studying spillovers using data for non-treated firms will allow us to study a diverse set of outcomes – both at the firm- and the firm-product level.

We exploit detailed firm-product-level data on intermediate input purchases and production in order to identify both the suppliers as well as the competitors of the treated firms. Our approach deviates from the standard approach of using industry-level input-output tables to measure horizontal and backward linkages between firms. Before we delve into the data and

[25]More in general, one should note that positive treatment effects on domestic sales need not automatically lead to negative effects for domestic competitors, since market size may not be fixed.

results, we briefly discuss why using firm-level data on inputs and outputs allows us to identify more accurately both the potential suppliers as well as the competitors of treated firms.

First, note that treated firms in our context tend to be larger firms. Using the same Danish data that we use in this paper, Ciliberto & Jäkel (2021) show that such large firms are typically multi-industry firms.[26] This fact implies that we would not accurately identify a treated firm's competitors by only looking at its main industry of activity, because such an approach would neglect secondary industries in which the treated firm is also active. For the same reason, industry-level input usage data for the treated firm's industry of main activity would not accurately capture its actual input use, which may span several output industries.

Second, when measuring backward spillovers, using industry-level input-output data would entail an additional challenge in our framework: In the typical industry, there are only few downstream firms that are treated, but many upstream firms that could potentially supply their output to the treated firms. Using industry-level data to identify backward linkages would therefore result in a mapping from few treated customers to a large pool of *potential* suppliers. The *actual* beneficiaries from backward spillovers would be identified very imprecisely, leading to attenuation bias in the estimated spillover effects for upstream firms. While attenuation bias could still be present in our framework, this bias should be significantly reduced.

5.1 Empirical Setup

5.1.1 Specification of Spillover Variables

We are interested in studying the spillovers of export credit guarantees on both firm-product-level outcomes – such as sales of products that are potentially affected by spillovers – as well as firm-level outcomes, such as aggregate output or employment. We therefore proceed in deriving measures of backward and horizontal spillovers at different levels of aggregation.

Spillovers at the firm-product level. Define $Competitor_{kipt}$ to be an indicator variable which is equal to one if both firm k and firm i produce product p. Similarly, define $Customer_{kipt}$ to be an indicator variable which is equal to one if firm k is a potential customer for product p from firm i (i.e., if product p is produced by firm i and purchased by firm k). Furthermore, EKF_{kt} is an indicator variable equal to one if firm k received a guarantee at time t.

Given these definitions, we construct firm-product-level measures of horizontal and backward

[26] Also see Bernard et al. (2010) and Boehm et al. (2019) for evidence on the importance of multi-industry firms in the US and India, respectively.

spillovers from export credit guarantees. We start by counting the number of competitors and potential customers receiving treatment:[27]

$$EKF\ competitors_{ipt} = \sum_{k \neq i} Competitor_{kipt} \cdot EKF_{kt} \tag{4}$$

$$EKF\ customers_{ipt} = \sum_{k \neq i} Customer_{kipt} \cdot EKF_{kt}. \tag{5}$$

We focus on the sample of non-treated firms for our spillover analysis. Therefore, the two variables in Equation (4) and (5) do not vary across firms within a product-year, and we will drop the i subscript in the following. In our empirical analysis, we also use (4) and (5) to derive alternative spillover measures: *(i)* a dummy variable for whether each of these two variables is positive; *(ii)* the shares of all competitors/potential customers, which receive treatment; and *(iii)* the amount of guarantees given to competitors or potential customers.

Spillovers at the firm level. Next, we also construct spillover variables at the firm level. Denote S_{it} as the set of products purchased by firm i, and P_{it} as the set of products produced. We define firms i and k to be competitors if they have at least one output product in common. Similarly, we define firm k to be a customer of firm i if firm i produces at least one product purchased by firm k:

$$Competitor_{kit} = \begin{cases} 1 & \text{if} \quad P_{it} \cap P_{kt} \\ 0 & \text{otherwise,} \end{cases}$$

$$Potential\ Customer_{kit} = \begin{cases} 1 & \text{if} \quad P_{it} \cap S_{kt} \\ 0 & \text{otherwise.} \end{cases}$$

Given these definitions, our measure for horizontal spillovers from guarantees given to firm i's competitors is given by:

$$EKF\ competitors_{it} = \sum_{k \neq i} Competitor_{kit} \cdot EKF_{kt}, \tag{6}$$

[27]Recall that export credit guarantees are often linked to a specific export transaction, and thereby might in some cases also be specific to a given product within the treated firm's product portfolio. We do not have information on treatment status at the firm-product level. Effectively, this implies that we treat all products within the firm's portfolio as treated. However, there could be within-firm spillover effects, whereby even products that are not directly covered by a guarantee may benefit; e.g., because they are exported as complementary goods along with the products that are covered, or because the positive effect of guarantees on firm liquidity benefits also un-treated products of the same firm.

and the proxy for backward spillovers from guarantees given to firm i's potential customers is:

$$Potential\ EKF\ customers_{it} = \sum_{k \neq i} Potential\ Customer_{kit} \cdot EKF_{kt}. \tag{7}$$

Importantly, these spillover variables vary across firms within an industry due to differences in firms' product portfolios. Again, we also consider different functional forms for these variables by, for example, accounting for the sum of guarantees issued to competitors and potential customers.

5.1.2 Identification Strategy

Spillovers at the firm-product level. At the firm-product level, we estimate the following empirical specification:

$$\begin{aligned} \ln Sales_{ipt} = \alpha_1 EKF\ competitors_{pt} + \alpha_2 EKF\ customers_{pt} \\ + \gamma_{N_{compet}} + \gamma_{N_{cust}} + \gamma_{ip} + \gamma_{jt} + \varepsilon_{ipt}, \end{aligned} \tag{8}$$

In the main text, our focus is on log firm-product-level sales as dependent variable, but in Appendix C, we also discuss results when using export sales as dependent variable instead. Note that products with more competitors (potential customers) are also likely to have more treated competitors (treated potential customers). We therefore include a full set of fixed effects for the number of competitors and the number of potential customers of product p in year t. We denote these fixed effects by $\gamma_{N_{cust}}$ and $\gamma_{N_{compet}}$, respectively. Thus, α_1 for example is identified from an increase in the number of *treated* competitors, holding the *overall* number of competitors fixed.

The specification in (8) includes firm-product fixed effects γ_{ip} to control for unobserved determinants of sales at the product, firm, or variety-level. Importantly, γ_{ip} accounts for the fact that guarantees from EKF are concentrated in certain product groups (such as capital goods). Notably, given γ_{ip}, the coefficients α_1 and α_2 will be identified from the time-series variation in horizontal and backward spillovers for a given product. Furthermore, we also include industry-year fixed effects γ_{jt} (where the industry j refers to firm i's main industry of activity). γ_{jt} controls for industry-specific supply or demand shocks which may be correlated both with the issuance of EKF credit guarantees to that industry as well as with firms' sales. Conditional on γ_{jt}, our estimator exploits the variation in our spillover variables across firms within a given

industry-year.[28] In the Online Appendix, we also discuss results from alternative specifications.

Since the main explanatory variable of interest in Equation (8) varies at the product level, we cluster standard errors by product. In addition, we also cluster by firm to account for serial correlation in the error term over time as well as correlation across products within a firm.

Spillovers at the firm level. At the firm level, our empirical specification reads as follows:

$$\ln Y_{it} = \beta_1 EKF\ competitors_{it} + \beta_2 EKF\ customers_{it} + \gamma_{N_{compet}} + \gamma_{N_{cust}} + \gamma_{N_{products}} + \gamma_i + \gamma_{jt} + \varepsilon_{it} \quad (9)$$

where j denotes industries, $i \in j$, and Y_{it} is the outcome variable of interest, including firm revenue and employment. $\gamma_{N_{compet}}$ and $\gamma_{N_{cust}}$ are, respectively, fixed effects for the number of competitors and potential customers of firm f in year t. Note how these two variables are now firm-specific. In particular, firms with a larger product portfolio will tend to have both more competitors as well as more potential customers; controlling for such differences across firms is thus paramount for the identification of the coefficients β_1 and β_2. Moreover, $\gamma_{N_{products}}$ is a fixed effect for the number of products produced by firm i in year t, which might also be correlated with our variables of interest, $EKF\ competitors_{it}$ and $EKF\ customers_{it}$.

Note that our spillover variables vary across firms within an industry due to differences in firms' product portfolio, implying differential exposure to both horizontal and vertical spillovers. This fact allows us to account for industry-wide demand or supply shocks by including industry-year fixed effects γ_{jt} in the empirical model. Controlling for γ_{jt} is important if the issuance of EKF credit guarantees is correlated with such shocks. For example, we might expect that firms in growing industries are more likely to receive an export credit guarantee from EKF. The estimated effect of horizontal spillovers, β_1, may then be upward biased in a regression that does not account for γ_{jt}. Moreover, if positive demand or supply shocks are partly transmitted to an industry's suppliers, outcomes in upstream industries might be spuriously correlated with guarantees to downstream industries. Thus, our estimated coefficient for backward spillovers, β_2, may similarly be biased in a regression that does not account for γ_{jt}. In addition, we control for firm-specific factors that are constant across time by including firm fixed effects, γ_i.

Since the spillover variables that are of interest vary across firms, we now cluster standard

[28] Our preferred specification includes industry-year but not firm-year fixed effects. The reason is that, at the four-digit level, the median firm in our sample is a single-product firm. Thus, the majority of firms are dropped when conditioning on firm-year fixed effects.

errors by firm.

5.2 Data

In addition to the data described thus far, we use information from the production and purchase statistics (again provided by Statistics Denmark) in order to identify a firm's potential suppliers and competitors. Notably, both data sources are confined to a sample of firms with significant manufacturing activity.[29] Moreover, only firms with at least 10 employees are obliged to answer the production survey, and only firms with at least 50 employees participate in the survey on purchases.

Firms' purchases of intermediates and production of outputs are reported using slightly different product classifications. However, the first four digits of both classifications are equivalent to the first four digits of the CN classification. We therefore construct the spillover variables in Equations (4) to (7), exploiting production and purchase information aggregated up to the four-digit level for each firm and product. At this level of aggregation, our sample contains 977 different product codes.

Although we can only match a sub-sample of larger treated firms with significant manufacturing activity to the purchase statistics, the matched sample of treated firms accounts for 282 firm-year treatment observations. Focussing on those 280 firms with a main activity within manufacturing, these treatment observations together amount to 34.9 billion DKK (approx. 4.68 billion EUR) of export credit guarantees over the period 2004–2015 – 90 percent of credit guarantees given to manufacturing firms throughout the sample period. When we merge the data on EKF credit guarantees with the production statistics, we include a sample of 465 firm-year treatment observations. Again, focussing on firms with a main activity within manufacturing (445 firms in total), these treatment observations account for 35.2 billion DKK (approx. 4.72 billion EUR) worth of guarantees. This amounts to 91 percent of all guarantees given to manufacturing firms over the sample period).

In Appendix C, we discuss robustness checks to mitigate concerns regarding the differences in the samples of treated firms based on which we derive the two spillover variables. In our interpretation of results, we will also note that our estimates do *not* reflect the spillovers from an average guarantee issued by EKF but rather the spillovers from guarantees given to larger

[29] In both data sources, there is a small minority of firms which does not report manufacturing as their *main* economic activity. We keep these firms in our sample, since their inclusion in the survey implies that they do indeed engage in manufacturing (even if their main activity lies elsewhere).

Table 2: Production and Purchase: Treated vs. Non-Treated Firms

	(1) Non-Treated Firms	(2)	(3) Treated Firms	(4)
	mean	N	mean	N
N goods produced	2.275	33,560	3.275	465
Total output, 1000DKK	161.9	33,560	1,246	465
N intermediates purchased	23.70	11,201	33.69	282
Total purchases, 1000DKK	268.2	11,201	1,317	282

Notes: The sample in the first two rows includes firms that are part of the production statistics. 280 out of these 282 firms have their main economic activity within manufacturing. The sample in the second two rows includes firms that are part of the purchase statistics. 445 out of these 465 firms have their main economic activity within manufacturing.

firms.

As noted above, we drop treated firms from the sample when estimating the empirical specifications in Equations (8) and (9). Therefore, the spillover effects that we estimate capture only spillovers to the non-treated, while not accounting for potential spillovers to the treated.

Table 2 shows how production and purchase behaviour differs across treated and non-treated firms. The average non-treated firm produces two four-digit products. Treated firms tend to have a somewhat larger product portfolio, with an average of three four-digit products. (Of course, these numbers understate the differences in firms' actual product scope which would be revealed at more disaggregate levels.) Differences between treated and non-treated firms clearly become apparent when considering total output of own-produced goods, which is on average almost eight times larger at treated compared to non-treated firms. Turning to intermediate purchases, the average treated firm sources 34 four-digit products, compared to 24 products sourced at the average non-treated firm. Again, the amount of purchased inputs is larger by an order of magnitude at treated compared to non-treated firms.

Panel A of Table 3 provides summary statistics for spillover variables measured at the firm-product level. We refer to the combination of a firm and a four-digit product as a "variety" in the following. Around a fourth of varieties in our sample (25.7 percent) faces competition from treated competitors; cf. Panel A. The variable *EKF competitors*$_{pt}$ is highly skewed to the right, with an average smaller than one but a standard deviation approximately twice as large. Turning to backward spillovers, we find that *EKF Customers*$_{pt}$ is greater than zero for a much larger share (46.4 percent) of observations in our firm-product level sample. The average number of treated potential customers is close to two. Again, the distribution of *EKF customers*$_{pt}$ is highly skewed with a long right tail (the maximum number of treated potential customers is equal to 29).

Table 3: Spillovers from EKF Guarantees: Summary Statistics

	Mean	S.D.	N
Panel A: Firm-Product-Level Variables			
EKF competitors$_{pt}$>0	0.257	0.437	72,966
EKF competitors$_{pt}$	0.507	1.189	72,966
Share of treated competitors$_{pt}$	0.0159	0.0444	72,966
Log (1+ EKF amount to competitors$_{pt}$)	0.826	1.881	72,966
EKF customers$_{pt}$>0	0.465	0.499	72,966
EKF customers$_{pt}$	1.748	3.259	72,966
Share of treated customers$_{it}$	0.0459	0.0798	72,966
Log (1+ EKF amount to customers$_{it}$)	1.956	2.664	72,966
Panel B: Firm-Level Variables			
EKF competitors$_{it}$>0	0.379	0.485	31,750
EKF competitors$_{it}$	0.982	2.007	31,750
Share of treated competitors$_{it}$	0.0173	0.0448	31,750
Log (1+ EKF guarantees to competitors)$_{it}$	1.325	2.295	31,750
EKF customers$_{it}$>0	0.599	0.490	31,750
EKF customers$_{it}$	2.899	4.418	31,750
Share of treated customers$_{it}$	0.0557	0.0762	31,750
Log (1+ EKF guarantees to customers)$_{it}$	2.877	3.003	31,750

Notes: The table gives summary statistics for spillover variables measured at the firm-product level (Panel A) and firm-level (Panel B). See Section 5.2 for a description of how these variables are constructed.

Next, we turn to the spillover variables which we construct at the firm level; cf. Panel B of Table 3. Since the average firm produces more than two products (cf. Table 2), both *EKF Competitors$_{it}$* as well as *EKF Customers$_{it}$* now take on larger values than the corresponding variables in Panel A. Again, both variables are also highly skewed with a long right tail.

5.3 Results

5.3.1 Spillovers at the Firm-Product-Level

We first discuss results from the firm-product-level regressions in Equation (8). Table 4 gives results with sales per variety as dependent variable and different functional forms for the spillover variables.

Independent of how we measure horizontal spillovers, Table 4 does not provide any evidence that export credit guarantees have negative effects on the competitors of treated firms: we always find these effects to be statistically insignificant. Thus, guarantees issued by EKF do not seem to crowd out sales of competing varieties. At the same token, however, any knowledge spillovers regarding export opportunities that one might expect to arise from these guarantees are apparently not large enough to result in significant increases in competitors' total sales.[30]

In the following discussion, we will mainly focus on the backward spillover variables. We

[30]See Appendix C for additional results regarding export sales in particular.

Table 4: Spillovers from EKF Guarantees: Evidence at the Firm-Product Level

	Dependent variable: Log Sales$_{ipt}$			
	(1)	(2)	(3)	(4)
EKF competitors$_{pt}$>0	-0.0055			
	(0.019)			
EKF customers$_{pt}$>0	0.0348**			
	(0.017)			
EKF competitors$_{pt}$		-0.0080		
		(0.009)		
EKF customers$_{pt}$		0.0196***		
		(0.006)		
Share of treated competitors$_{pt}$			-0.1451	
			(0.157)	
Share of treated customers$_{pt}$			0.3149***	
			(0.068)	
Log (1+EKF guarantees to competitors)$_{pt}$				0.0041
				(0.006)
Log (1+EKF guarantees to customers)$_{pt}$				0.0112***
				(0.004)
Observations	72,966	72,966	72,966	72,966
R-squared	0.929	0.929	0.929	0.929

Notes: All regressions include the following fixed effects: Firm-Product, Industry-Year, and fixed effects for the number of customers and number of competitors. Standard errors, adjusted for clustering by firm and product, are given in parentheses. *,**,*** denote significance at the 10%, 5%, 1% levels, respectively.

start our analysis by including each spillover channel with an indicator equal to one if the respective spillover variable is greater than zero. The estimated coefficient on the indicator for backward spillovers from export credit guarantees is positive and statistically significant: the point estimate implies that having at least one potential customer receiving a guarantee from EKF increases variety sales by approximately 3.5 percent. This effect is sizeable given that it reflects an *average* effect for *potential* suppliers: in fact, only some of these upstream varieties will actually be purchased by the treated downstream firm(s).

Next, we include *EKF Competitors$_{pt}$* and *EKF Customers$_{pt}$* as continuous variables in column (2). The coefficient for *EKF Customers$_{pt}$* corroborates the evidence in support of backward spillovers: we predict that each additional treated customer increases a variety's sales by 1.96 percent. Increasing *EKF Customers$_{pt}$* by one standard deviation yields a predicted increase in variety sales of 6.38 percent.[31]

In column (3), we turn to a different specification of the spillover variables, which accounts for the fact that the number of treated competitors and potential customers is bound from above by the total number of competitors and potential customers. In particular, we measure spillovers from export credit guarantees with the share of competitors and potential customers which

[31]The skewness in the spillover variables included in column (2) may hamper identification. Specifically, when these variables are only included in levels, we impose the effect of adding additional treated customers or competitors to be constant. We address this caveat by additionally including the squared term of each of the two spillover variables. Results (available on request) do not show evidence of diminishing effects of backward spillovers, and also confirm the non-significance of horizontal spillovers.

receive treatment. In this specification, we can therefore drop the fixed effects for the number of competitors and customers.[32] Based on the estimate, we predict that a one standard deviation increase in *Share of treated customers*$_{pt}$ increases firm-product-level sales by 2.5 percent.

Finally, we include information on the amount of guarantees issued by EKF to a variety's competitors and potential customers. (Due to the large number of observations for which these amounts are zero, we add one to the variable before taking logs.) The point estimate, reported in column (4), implies that a 1 percent increase in export credit guarantees to potential customers increases upstream firms' product-level sales by 0.0115 percent. This effect is economically meaningful when interpreted against the fact that it reflects the average effect across all *potential* upstream suppliers. In Section 5.3.3, we report additional specifications which allow to further evaluate the economic significance of the backward spillovers that we estimate.

Alternative specifications. In Table C.1 in the Online Appendix, we report results from alternative specifications where some of the fixed effects in Equation (8) are dropped, or additional fixed effects are added. Importantly, our estimates for the backward spillover variables are consistent in sign, statistical significance, and magnitude across all specifications which appropriately condition on industry-year fixed effects. Moreover, coefficient estimates on the variable *EKF Competitors*$_{pt}$ continue to be statistically insignificant.

5.3.2 Spillovers at the Firm Level

Our results thus far have shown that upstream firms increase the output of those products demanded by downstream treated firms, and that there are, thus, positive backward spillovers at the firm-product level. A broader, and equally important question is whether these positive backward spillovers manifest themselves also in firm-level outcomes such as total output or employment (remember that one of the main motivations for the credit policy is to "promote jobs in Denmark"). We therefore proceed with the estimation of spillover effects for firm-level outcomes as given in Equation (9). In columns (1) to (4) of Table 5, we focus on firm revenue; and in the remainder of the table, we report results with firm employment as dependent variable. Again, we include the spillover variables using different functional forms.

Independent of how we measure horizontal spillovers, we do not find any evidence that export credit guarantees given to Danish firms crowd out sales or employment at the domestic com-

[32] Including or excluding these fixed effects in column (3) does, however, not lead to significant changes in the coefficient estimates.

Table 5: Spillovers from EKF Guarantees: Evidence at the Firm Level

	Log Total Revenue$_{it}$				Log Employment$_{it}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EKF competitors$_{it}$>0	0.0056				0.0033			
	(0.008)				(0.007)			
EKF customers$_{it}$>0	0.0141+				0.0129*			
	(0.009)				(0.008)			
EKF competitors$_{it}$		-0.0007				-0.0004		
		(0.003)				(0.002)		
EKF customers$_{it}$		-0.0004				-0.0025+		
		(0.002)				(0.002)		
Share of treated competitors$_{it}$			0.1280*				0.0756	
			(0.070)				(0.059)	
Share of treated customers$_{it}$			0.1179**				0.1260***	
			(0.055)				(0.044)	
Log (1+EKF amount to competitors)$_{it}$				0.0021				0.0006
				(0.002)				(0.002)
Log (1+EKF amount to customers)$_{it}$				0.0042**				0.0031*
				(0.002)				(0.002)
Observations	31,750	31,750	31,750	31,750	31,750	31,750	31,750	31,750
R-squared	0.952	0.952	0.951	0.952	0.954	0.954	0.952	0.954

Notes: All regressions include the following fixed effects: Firm, Industry-Year, and fixed effects for the number of customers number of competitors, and number of products produced by the firm. Standard errors, adjusted for clustering by firm, are given in parentheses. +, *,**,*** denote significance at the 15%, 10%, 5%, 1% levels, respectively.

petitors of the treated. This result reinforces the findings from the firm-product-level analysis, and it also suggests that the SUTVA assumption underlying our matching approach in Section 4 is likely to be satisfied in our framework.

In the following, we focus on the results for the backward spillover variables. In column (1) of Table 4, we measure backward spillovers by an indicator variable for whether the number of treated potential customers (cf. Equation (7)) is positive. Here, the coefficient estimate is not significant at standard significance levels in the case of total revenue, though only marginally so; cf. column (1) Table 5. For firm employment, it turns significant at the 10% level. Note, however, that the probability of having at least one treated competitor or customer is much higher at the firm level than it is at the firm-product level. For that reason, it might be harder to detect spillovers from credit guarantees when only considering the indicator variables.

In columns (2)–(3) and (6)–(7), we turn to the number of treated potential customers, *EKF customers*$_{it}$, and the share of treated customers as alternative measures of backward spillovers. Coefficient estimates are small and statistically insignificant for the former variable, but turn positive and statistically significant for both firm-level outcomes if we employ the latter variable. Based on the estimates in columns (3) and (6), we predict that an increase in *Share of treated customers*$_{it}$ by one standard deviation increases both firm revenue and employment by roughly one percent.

Finally, in columns (4) and (8), we consider the amount of guarantees given to customers. This specification confirms the significant spillovers of export credit guarantees to potential suppliers of the treated: A 10 percent increase in the amount of credit given to customers is predicted to increase firm revenue and employment by 0.046 and 0.034 percent, respectively. These effects might seem small, but they again have to be judged against the circumstance that they reflect an *average* effect across all *potential* suppliers.

5.3.3 Economic Significance of Spillover Effects

In Tables 4 and 5, we have used different variables to proxy the potential for spillovers across varieties and firms. Most of these variables were based on the number of treated firms, thereby mirroring very closely our analysis of the direct effects on the treated in Section 4, where we have not distinguished treatment effects by treatment intensity. However, the amount of guarantees given to customers – rather than the number of treated customers – should be the most suitable measure for the potential of backward spillovers.

To improve on the ad-hoc solution of adding 1 to the amount of guarantees before taking logs, we next present results in which the amount of guarantees is discretized into several binary indicator variables.[33] We can, thereby, also test whether larger amounts of guarantees given to customers entail larger spillover effects. We focus on results for backward spillovers, as we continue to not find any significant horizontal spillovers.

Figure 3 visualizes the coefficient estimates for different dependent variables of interest; see Appendix C for the full regression results. Figure 3(a) shows results for a specification with variety sales as dependent variable. The amount of guarantees is here split into three mutually exclusive intervals, with no guarantees to customers being the comparison group. Backward spillovers are small and statistically insignificant if treated potential customers in total have received less than 7.45 million DKK of guarantees (approx. 1 million EUR). However, the effects turn significant once we consider larger guarantee volumes: varieties whose set of potential customers together receive guarantees between 7.45 and 74.5 million DKK (approx. 1–10 million EUR) see their sales increase by 3.91 percent. The largest effect is found for sums of guarantees exceeding 74.5 million DKK: here, we predict an increase in variety sales of 5.47 percent (statistically significant at the 5%-level).

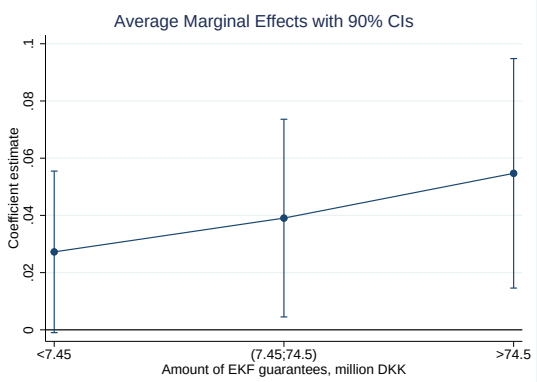
A similar pattern is observed if we consider outcomes at the firm-level; cf. Figures 3(b) and 3(c) for firm revenue and employment, respectively. Note that we can here further divide the last category into guarantees below and above 745 million DKK (100 million EUR). Again, we find that backward spillovers are insignificant if the amount of guarantees given to potential customers is small, but they turn statistically significant and economically large once we look at larger amounts. For example, we find that employment is predicted to increase by 2.69 percent for firms whose potential customers have received between 74.5 and 745 million DKK of export credit guarantees. With 3.06 percent, this effect becomes somewhat larger for guarantee volumes exceeding 745 million DKK.

5.4 The Importance of Spillover Effects: Back-of-the-Envelope Analysis

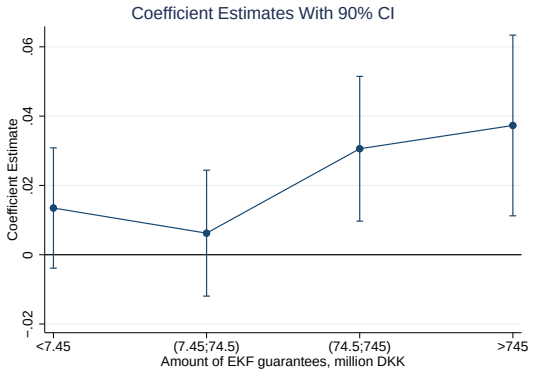
In this section, we provide a simple back-of-the-envelope calculation to highlight the relative importance of direct effects on the treated and indirect effects on their suppliers when evaluating how effective export credit policies are in supporting jobs or raising employment. Recall that there are 282 firm-year treatment observations based on which our backward spillover variables

[33] See Appendix C for summary statistics for these variables.

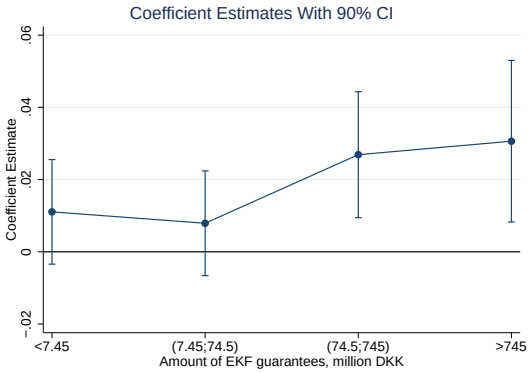
Figure 3: Spillover Effects and the Amount of Guarantees



(a) Variety Sales



(b) Firm Revenue



(c) Firm Employment

Note: The figure gives coefficient estimates, as well as 90% confidence intervals, from regressions where backward spillovers are measured by indicator variables for different intervals of the amount of EKF credit guarantees given to customers. Full regression results are reported in Appendix C.

are constructed. To ensure comparability, we focus on this same set of observations when calculating the jobs created or retained at treated firms and those created or retained at their suppliers. Thereby, our quantification only serves to gauge the relative importance of direct and spillover effects, rather than providing a full-blown analysis of the total employment effects of export credit guarantees in Denmark.

The upper part of Table 6 shows two different quantifications of the effects on total employment at treated firms: first, we use our estimates of the treatment effects for manufacturing firms from Section 4 (recall that the purchase statistics that we use for the spillover analysis only covers manufacturing firms). Second, we use estimates of the treatment effects for a sample of *large* manufacturing firms (recall that the purchase statistics only covers firms with more than 50 employees; see Online Appendix B for the results). Notably, the ATET on employment is small and statistically insignificant in this latter sample, confirming results in Lodefalk et al. (2019). The 282 treated firms that we exploit here employed, on average, 699 workers; in total,

Table 6: Quantification of Direct and Indirect Employment Effects

	Average employment	N firm-year observations	Total employment	Quantification
Panel A: Direct Effects on the Treated				
Treated Firms	699	282	197,055	
ATET(t): manufacturing firms				0.0567***
Quantification (1)				**11,173**
ATET(t): large manufacturing firms				0.0099
Quantification (2)				–
Panel B: Spillover effects to Potential Suppliers				
Firms with Backward Spillovers>0	76	19,008	1,446,859	
– Coefficient estimate				0.0129*
Quantification (1)				**18,653**
Firms with Guarantees to Customers ∈ 74.5-745	69	5,248	362,060	
– Coefficient estimate				0.0269*
– Quantification (2a)				9,735
Firms with Guarantees to Customers >745	70	5,968	414,908	
– Coefficient estimate				0.0306**
– Quantification (2b)				12,706
Quantification (2) (=2a+2b)				**22,441**

Notes: Panel A uses two alternative estimates of the ATET to quantify the predicted employment effects for those 282 firm-year-treatment observations which form the basis of our spillover analysis. Similarly, Panel B uses two alternative estimates of the backward spillover effects to quantify the employment effects at potential suppliers of the treated.

they employed 197,055 workers. Given the estimated ATETs, we predict that between zero to 11,173 jobs are created or sustained at these firms in the year of treatment due to the export credit policy.

The lower part of Table 6 shows two alternative quantifications of the effects of this same set of 282 export credit guarantees on employment at potential suppliers of treated firms. First, we use estimates of the backward spillover effects from column (5) of Table 5. Total employment at firms potentially benefiting from backward spillovers was 1,446,859 workers. Given the coefficient estimate of 0.0129, we predict an increase in employment of 18,653. Alternatively, we use the estimates from Figure 3 (focusing on those two coefficients which are statistically significant): here, predicted total employment effects at suppliers of treated firms are even larger (with 22,441 jobs predicted to be created or sustained).

In sum, effects on total employment at suppliers may, in fact, be larger than the direct employment effects at treated firms. This prediction resonates with the estimated ATETs which (at least in the subsample of manufacturing firms) are larger for treated firms' purchases than for their own value added; cf. Panel B of Table 1 and Table B.6 in the Online Appendix. The quantification in Table 6, thus, highlights how taking spillover effects on suppliers into account can be important when evaluating export credit policies.

6 Conclusion

This paper studies the direct effects of ECA credit provision on the treated firms as well as spillovers to their domestic competitors and suppliers. We find large positive effects both for firms receiving a state-backed export credit guarantee in Denmark, as well as for their suppliers. In contrast, we do not find evidence that guarantees crowd out production at domestic competitors of the treated. Together, our results imply that the positive effects of ECA credit provision may go well beyond the effects on just the small set of firms benefiting *directly* from this policy. As discussed in our introduction, there has been a heated political and economic debate about the activities of export credit agencies. We hope that our results can serve to inform this debate about one particular aspect which, this far, was in lack of empirical evidence – namely, the potential spillover effects from these policies.

We acknowledge that one important caveat regarding our spillover analysis remains, however: given data constraints, we were not in a position to analyse spillovers from Danish firms to *foreign* competitors. Interestingly, we find positive treatment effects also on firm imports (at least in the full sample including non-manufacturing firms) – i.e., increased international sourcing. This finding is not surprising, given that firms which receive export credit guarantees tend to be not just larger than other firms, but also more internationalized. However, it implies that there is at least a potential for export credit guarantees in one country to also have *positive* spillovers to certain types of foreign firms – namely, foreign suppliers.

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A Data Appendix

We merge all data sources using the firm's CVR number (firm identifier) and year, and the product code where applicable. All product-level information is aggregated up to the four-digit level of the HS classification, because this is the level at which purchase and production data are comparable.

A.1 Data on Export Credit Guarantees

EKF has provided us with detailed data on export credit guarantees issued to Danish firms. The data report information such as the amount granted, the issuance year of the guarantee, the credit period, etc. Based on this information, we construct a dummy variable EKF_{it} which is equal to one if a guarantee was issued to firm i in year t. Even though the credit period often spans several years, we expect to see an effect on firm outcomes in the year the guarantee was issued, or the following year. Some firms receive multiple guarantees for different transactions in a given year. Sometimes, the credit for a given transaction may also be split into multiple guarantees. Since it is hard to distinguish these different types of multi-treatments, we set EKF_{it} equal to one if the firm received *at least one* guarantee in that year.

We drop banks and insurance companies from the sample, since these firms are likely to be registered as recipients of EKF guarantees on behalf of their clients. Figure A.1 shows the distribution of export credit guarantees across broad economic sectors. As discussed in the main text, the manufacturing sector is most important in terms of the number of issued guarantees. The importance of *Knowledge Based Services* in terms of total credit volume might seem surprising, but note that it is mainly driven by projects classified by EKF as relating to "Wind power" and "Cement/lime/plaster".

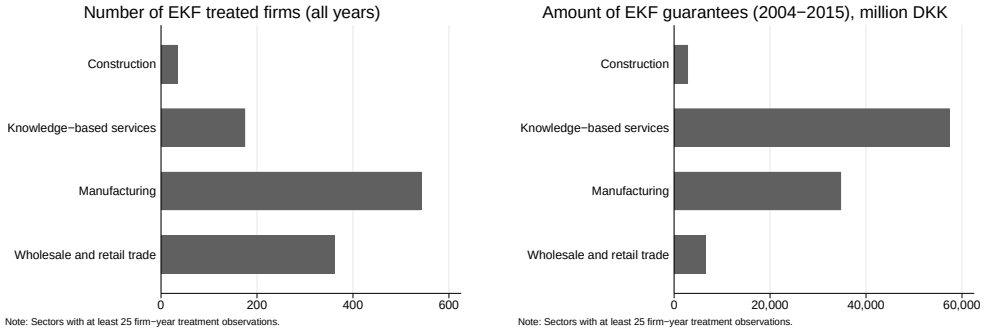
We restrict our sample for the treatment analysis in Section 4 to the four broad economic sectors listed in Figure A.1. This restriction implies dropping very few observations from sectors where treatment is extremely rare.

A.2 General Firm Statistics and Accounting Statistics

We obtain information on most of the firm-level outcome and control variables used in Section 4 from the General Firm Statistics (FIRM) and the Accounting Statistics (FIRE), provided by Statistics Denmark.

FIRM covers all firms within the Danish economy with positive employment, and we use these

Figure A.1: EKF Credit Guarantees by Sector



(a) Number of Firm-Years with Treatment (b) Amount of EKF Credit Guarantees

data to measure the number of full-time-equivalent employees, value added, total revenue and total purchases. Notably, information on firms' purchases comes from the firm's VAT statement and includes purchases of both raw materials, intermediate inputs as well as capital goods.[i] FIRE contains information only for larger firms, amounting to a sample of approximately 7,500 firms per year. We use these data to construct additional variables such as the measures of firm financial stance. We drop firm-year observations with negative, zero or missing values of total assets, total sales, total fixed assets, or wages.

A.3 External Trade Statistics

In the external trade statistics, firms report their exports and imports by product and destination. Products are reported according to the eight-digit level of the CN code. For the analysis in Section 4, we aggregate the export and import information up to the firm level by summing over all products and export destinations within a given firm. We also use the external trade statistics to calculate total exports at the firm or firm-product level, which we use in our robustness analysis in Appendix C.

A.4 Production and Purchase Statistics

Production statistics. In the production survey, firms are asked to report their sales volume for each product they produce. The reporting unit is the Kind of Activity Unit (KAU), which is the sum of a firm's workplaces engaged in the same economic activity (industry). The survey

[i] We could also have used the purchase statistics (cf. below) to measure sourcing of intermediate goods. In practice, however, we are left with an insufficient number of treatment observations when we employ these data. The reason is, first, that the purchase statistics only cover larger manufacturing firms, and, second, that we condition on not receiving treatment in $t-1$ in our treatment analysis, further reducing the sample.

comprises all manufacturing KAUs with at least 10 employees. Typically, only firms with a main activity within manufacturing are therefore included in the survey.[ii] Products are reported according to the eight-digit level of the CN code. Sales are recorded independent of in which market the product is sold and therefore include both domestic and export sales. We aggregate the production information up to the firm-CN4 ("variety") level.

Purchase statistics. The purchase survey describes the purchase of raw materials, semi-manufactured products, intermediary products, packaging and services in the production of industrial commodities. The survey includes all firms within manufacturing with at least 50 employees. Purchases of goods and services are included, regardless of whether they are domestic or imports. We focus our analysis on purchases of intermediates and packaging, and our analysis does thus not consider spillover effects to service firms.[iii] The first four digits of the product code used in the purchase statistics is equivalent to the four-digit HS code, and we therefore aggregate all information up to this level.

Fit between EKF credit data and purchase/production statistics. The sampling criteria in the production and purchase statistics (cf. above) imply that only larger treated firms with significant activity in the manufacturing industry can be linked to these data sources. Focusing on firms with a main activity in manufacturing, we can match approx. 52 percent (82 percent) of firm-year observations in the sample of treated EKF firms to the purchase (production) statistics. These matched firms account for 90 percent (91 percent) of EKF credit guarantees given to manufacturing firms throughout the sample period. Treated manufacturing firms that cannot be matched to the production and purchase statistics tend to be smaller firms. Importantly, it is the large firms from which we expect to find the largest spillovers. On the other hand, it needs to be kept in mind that our empirical results do *not* reflect the spillover effects of the *average* guarantee issued by EKF.

Table B.1: Treated vs. Control Firms before and after Matching

	Before Matching			After Matching				Probit Estimates	
	Treated	Control	*p*-value	Treated	Control	%Bias	*p*-value	Coefficient	SE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample									
Log Total Assets	10.653	8.496	0.000	10.631	10.638	-0.4	0.944	0.212***	(0.025)
Log Employment	3.315	1.696	0.000	3.300	3.345	-3.3	0.618	-0.0429*	(0.026)
Log Labour Productivity	-0.449	-0.644	0.000	-0.451	-0.458	1.4	0.821	-0.0818**	(0.037)
Log Wage/Employee	13.101	12.88	0.000	13.099	13.096	1.2	0.816	0.230***	(0.077)
Collateral	0.163	0.225	0.000	0.163	0.167	-2.2	0.664	0.0486	(0.164)
Liquidity Ratio	0.080	0.101	0.000	0.080	0.080	-0.3	0.958	-0.222	(0.183)
Current Ratio	0.737	0.697	0.000	0.737	0.736	0.6	0.905	0.421***	(0.135)
$\Delta \ln$ *sales*	0.050	0.034	0.410	0.049	0.065	-3.3	0.553	-0.0308	(0.038)
Exporter	0.905	0.310	0.000	0.905	0.901	1.1	0.815	0.603***	(0.062)
Importer	0.893	0.369	0.000	0.893	0.899	-1.6	0.727	0.253***	(0.058)
Observations	571	249,395		568	2,613			249,983	
Panel B: Manufacturing firms									
Log Total Assets	11.058	8.839	0.000	11.044	11.039	0.3	0.968	0.179***	(0.046)
Log Employment	3.894	2.164	0.000	3.885	3.904	-1.4	0.865	0.0636	(0.049)
Log Labour Productivity	-0.50	-0.67	0.000	-0.505	-0.519	3.2	0.700	-0.105*	(0.061)
Log Wage/Employee	13.075	12.875	0.000	13.074	13.059	6.2	0.366	0.462***	(0.142)
Collateral	0.207	0.336	0.000	0.207	0.210	-1.5	0.818	0.181	(0.226)
Liquidity Ratio	0.068	0.072	0.385	0.068	0.069	-1.1	0.901	-0.0395	(0.293)
Current Ratio	0.686	0.596	0.000	0.687	0.684	1.1	0.877	0.607***	(0.208)
$\Delta \ln$ *sales*	0.040	0.039	0.955	0.040	0.052	-3.2	0.677	-0.0228	(0.069)
Exporter	0.958	0.447	0.000	0.958	0.964	-1.7	0.678	0.416***	(0.105)
Importer	0.935	0.438	0.000	0.935	0.938	-0.8	0.869	0.236**	(0.09)
Observations	308	48,296		307	1,355			48,613	

Notes: The table summarizes characteristics of treated and control firms for the full sample (Panel A) and the manufacturing subsample (Panel B). Columns (1)-(2) and (4)-(5) show differences between treated and control firms before and after matching. Columns (3) and (7) report *p*-values from a test of the equality of means for the two groups. Columns (8) and (9) give the coefficient estimate and standard errors from the first-stage probit estimation, with treatment status as dependent variable, and industry and year fixed effects as additional control variables.

B Additional Results for the Matching Approach

B.1 Summary Statistics for Matching Variables

Panel A of Table B.1 contains information for the exhaustive sample, and Panel B for the sample of manufacturing firms only. Columns (1) and (2) show differences across the two groups of firms before matching. In both samples, treated firms are, on average, significantly larger than non-treated firms in terms of both assets and employment. Moreover, they are more productive and tend to pay higher wages. Looking at the variables measuring a firm's financial stance, treated firms have on average a lower ratio of tangible to fixed assets. This is in line with export credit

[ii] We keep the small set of firms which report to the production statistics even though they do not have their main activity within manufacturing in the sample. Excluding these firms does, however, not significantly affect any of our results.

[iii] This restriction is due to the fact that we do not have production statistics for services, and therefore purchases of services cannot be straightforwardly linked to the providers of these services.

guarantees being more important for firms or sectors which are more vulnerable to financial frictions. In contrast, the current ratio tends to be higher at treated firms. Finally, we do find significant differences across treated and non-treated firms in terms of the liquidity ratio only for the exhaustive sample in Panel A.

B.2 Including Bank Controls

Private banks play an important role in the process of obtaining an export credit guarantee. In this section, we consider the robustness of our matching results to the inclusion of additional bank covariates in the estimation of the propensity score.

From the private data provider Experian, we obtain information on the name(s) of firms' private bank(s). We have this information for only a subset of years in our sample, namely until 2011. The Experian data also contain information on the official Danish firm identifier (CVRNR), and these data can therefore straightforwardly be linked to the register data. However, Experian only covers a subset of firms, with inclusion being more likely for larger firms. From the 2004–2011 sample, we can indeed match more than 90 percent of treatment observations, but the pool of donor firms is somewhat reduced. Since the issuance of credit guarantees has increased dramatically over time (cf. Figure 1), the focus on the 2004–2011 period means a substantial loss in the number of treated firms. To ensure a sufficient number of treatment observations to proceed with the analysis, we use the information from 2011 to predict the bank control variables for 2012.[iv] Since all covariates refer to the pre-treatment year, we therefore exploit treatment information for the period 2005–2013.

Focusing on the sample of firms which can be linked to the Experian data, approximately 95 percent of firms report a single ("house") bank. In those instances where firms have several banks, we use information for the largest bank (as measured by the total number of corporate customers).

Based on the name of each bank, we also merge the data to balance sheet information for banks from Bankscope. The match between banks in Experian and Bankscope works better for larger banks. Thus, firms who are customers at such larger banks are oversampled in the estimation sample. However, given that treated firms tend to be customers at large banks (cf. below), we judge this problem to be a minor one.

[iv] Note that, throughout the period for which we have bank information, we see very few instances of treated firms that switch banks.

Table B.2: Matching Approach: Including Bank Covariates

	Total Sales	Exports	Domestic Sales	Total Purchase	Imports	Domestic Purchase	Employ-ment	Value Added
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATET (t-1)	0.0179	0.000793	-0.0157	0.0210	0.109	0.0367	0.0181	0.0502
	(0.0352)	(0.0624)	(0.0575)	(0.0296)	(0.109)	(0.0367)	(0.0209)	(0.0371)
ATET (t)	0.106***	0.190***	0.0161	0.0609*	0.0539	0.0400	0.0409**	0.0642+
	(0.0400)	(0.0673)	(0.0744)	(0.0318)	(0.114)	(0.0402)	(0.0174)	(0.0412)
Observations	1,280	1,113	1,082	1,281	1,068	1,202	1,281	1,253
Number of treated	231	206	197	231	198	222	231	228

Notes: All dependent variables are measured in log-changes. Standard errors, adjusted for clustering by firm, are given in parentheses. +, *,**,*** denote significance at the 15%, 10%, 5%, 1% levels, respectively.

We use these additional data sources to include bank-level covariates in the estimation of the propensity score. In particular, we would like to control for the identity of the bank at which a firm is customer. Notably, the Danish banking market is characterized by a few large banks together with a very large set of small banks.[v] Many of these smaller banks have only few (if any) treated customers. Including a full set of bank fixed effects would therefore lead to a significant loss of observations. Instead, we include indicator variables for those banks with five or more treated customers (a total of five banks). Additionally, we control for the banks' total number of corporate customers and their sum of loans. These variables should account for the fact that EKF guarantees are used in connection with large credit transactions, and firms requiring such credit may therefore be more inclined to turn to large banks.

Due to the significant loss of observations when conditioning on bank controls, we focus on the exhaustive sample in the following. The summary statistics for treated and control firms confirm that treated firms tend to be customers at larger banks, highlighting the potential importance of including bank covariates in the matching approach.

Table B.2 shows results from the matching estimator. Overall, the table reinforces results reported in the main text, though statistical significance is somewhat lower (which should be expected, given the much smaller number of treatment observations). The ATETs for exports and total sales are again statistically significant and comparable in magnitude to those in Table 1. Thus, conditioning on differences across firms in their private banking relations does not affect our results on the effectiveness of export credit guarantees in boosting exports and sales. Similarly, the effect on domestic sales remains statistically insignificant as before. For total purchases, we continue to find positive and significant effects; cf. column (4). However, our estimated ATETs turn statistically insignificant when splitting total purchases into imports and

[v]Though restructuring in the wake of the financial crises has left to a decrease in the number of banks, even in 2011 we still observe more than 100 banks in our sample.

domestic purchases; cf. columns (5) and (6). Finally, we also continue to find positive treatment effects for employment and value added.

B.3 Accounting for Previous Export Performance

Export credit guarantees are more likely issued in connection with sales to riskier markets or exports of capital goods, which often involve particularly large transactions and longer credit periods. If these markets and products have systematically different growth rates from others, the conditional independence assumption underlying the matching estimates presented in Section 4 may be violated. Here, we report two sets of additional results to ameliorate this concern.

First, we follow Volpe Martincus & Carballo (2008) and add more detailed measures of prior previous export experience as determinants of the propensity score. In particular, we add the number of products exported, the total number of destination markets served and the number of risky destinations (OECD risk classification score ≥ 5) to the set of covariates in the first stage probit estimation.[vi] Summary statistics show that, indeed, treated firms have more prior export experience not only broadly in terms of the number of destinations and products, but also specifically in terms of the number of destinations in the highest risk categories. Results from the matching approach with these additional covariates are reported in Table B.3, and corroborate our main findings. Most notably, point estimates are also quite similar to those reported in Table 1.

Second, we perform difference-in-difference regressions, which allow us to account more thoroughly for differences in growth rates depending on the precise destination(s) which were served in the pre-treatment year, and the products which were exported. In particular, our specification is as follows:

$$\ln y_{it} = \beta EKF_{it} + \delta' \mathbf{X}_{i,t-1} + \gamma_i + \gamma_{jt} + \sum_c \gamma_c \cdot \mathbf{1}(Exports_{ic,t-1} > 0) + \sum_p \gamma_p \cdot \mathbf{1}(Exports_{ip,t-1} > 0) + \varepsilon_{it} \quad \text{(B.1)}$$

where γ_i and γ_{jt} are firm- and industry-year fixed effects, respectively, and $\mathbf{X}_{i,t-1}$ is the same set of lagged firm-specific covariates that we used in the estimation of the propensity score. In addition, we now add the following detailed indicators of previous export experience: $\mathbf{1}(Exports_{ic,t-1} >$

[vi] All three variables are measured in logs. In order to be able to include also firms that did not export in $t-1$, we add one to each variable before taking logs.

Table B.3: Matching Approach: Including Measures of Export Experience

	Total Sales	Exports	Domestic Sales	Total Purchase	Imports	Domestic Purchase	Employ-ment	Value Added
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Full sample								
ATET (t-1)	-0.0041	-0.0158	-0.0208	0.0129	0.0819	0.0141	0.0126	0.0250
	(0.0223)	(0.0558)	(0.0406)	(0.0207)	(0.0676)	(0.0272)	(0.0116)	(0.0230)
ATET (t)	0.0809***	0.197***	0.0537	0.0812***	0.0951	0.0523*	0.0429***	0.0561**
	(0.0225)	(0.0496)	(0.0410)	(0.0204)	(0.0696)	(0.0276)	(0.0112)	(0.0225)
Observations	3,071	2,628	2,645	3,087	2,580	2,929	3,105	3,053
Number of treated	564	486	483	564	472	538	564	555
Panel B: Manufacturing firms								
ATET (t-1)	-0.0004	-0.0400	-0.0409	0.0203	0.0496	-0.005	0.003	0.0286
	(0.0257)	(0.0576)	(0.0538)	(0.0251)	(0.0813)	(0.0311)	(0.0157)	(0.0304)
ATET (t)	0.0853***	0.182***	0.0138	0.102***	0.00187	0.0906***	0.0385***	0.0152
	(0.0227)	(0.0684)	(0.0569)	(0.0218)	(0.0859)	(0.0274)	(0.0126)	(0.0264)
Observations	1,531	1,487	1,326	1,533	1,375	1,527	1,534	1,532
Number of treated	293	282	248	293	266	290	291	290

Notes: All dependent variables are measured in log-changes. Standard errors, adjusted for clustering by firm, are given in parentheses. +, *,**,*** denote significance at the 15%, 10%, 5%, 1% levels, respectively.

Table B.4: Difference-in-Difference Estimates of Treatment Effects

	Log Sales			Log Exports		
	(1)	(2)	(3)	(4)	(5)	(6)
EKF_{it}	0.1069***	0.0963***	0.0928***	0.2348***	0.2059***	0.2169***
	(0.019)	(0.019)	(0.020)	(0.050)	(0.050)	(0.052)
Observations	490,361	490,361	490,361	101,216	101,216	101,216
Lagged Firm Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lagged Export Destinations		Yes	Yes		Yes	Yes
Lagged Exported Products			Yes			Yes

Notes: Standard errors, adjusted for clustering by firm, are given in parentheses. +, *,**,*** denote significance at the 10%, 5%, 1% levels, respectively.

0) is an indicator variable for whether firm i had positive exports to destination c in year $t-1$, with corresponding coefficient γ_c. Similarly, $\mathbf{1}(Exports_{ip,t-1} > 0)$ is an indicator for whether firm i exported product p in year $t-1$, and γ_p is the corresponding coefficient. Since our interest is in the effects on overall firm performance, we continue to focus on outcome variables $\ln y_{it}$ measured at the firm level.

Table B.4 reports results for two main outcome variables of interest: total sales and exports. For comparison, we start with a specification including only those covariates used in the matching approach (columns (1) and (4)), and then successively add the sets of lagged export destination indicators (columns (2) and (5)) and lagged export product indicators (column (3) and (6)). Two main findings can be highlighted: (*i*) our results from the matching approach are robust to this alternative econometric method – both qualitatively as well as quantitatively; and (*ii*) coefficient estimates are only marginally reduced when allowing for sales and exports to evolve

differently across firms, depending on the types of products exported, and the sets of destinations that were served in the past.

B.4 Heterogeneity in Treatment Effects: Buyer Finance vs. Capital Guarantees

Are the two broad types of guarantees – buyer finance vs. capital guarantees – equally effective in increasing firm performance? To answer this question, we report ATETs for the two types of guarantees in Panel A and B of Table B.5. In each case, the sample is restricted to firms that did not benefit from the given type of guarantee in $t-1$ but potentially did so in year t. Firms which received the respective other type of guarantee in either t or $t-1$ are dropped from the sample. In very few cases, firms received both types of guarantees in a given year. These observations are also excluded.

Before we turn to the interpretation of the results, a few differences between the two types of guarantees should be noted. First, they involve somewhat different credit volumes: in the case of buyer finance, the average guarantee in the sample amounts to 41 million DKK, compared to 5.7 million DKK in the case of capital guarantees. However, firms receiving a capital guarantee also tend to be smaller than firms benefiting from buyer finance guarantees. As a result, the average coverage ratio (EKF guarantees over a treated firm's total sales) is, in fact, similar across both types. Second, throughout most of our sample period, practically all guarantees were buyer finance guarantees. Only in the later years do we see an increasing number of capital guarantees. Interestingly, in the last two years of our sample (2014–15), we see more firms benefiting from capital guarantees than from buyer finance guarantees. These patterns imply that the number of treatment observations based on which we identify effects are somewhat smaller in the case of capital guarantees. Moreover, the treatment analysis for capital guarantees only exploits data from the 2010–15 period, i.e., the years after the financial crisis.

Keeping these patterns in mind, Table B.5 shows fairly similar treatment effects for both types of guarantees. In particular, we find positive and statistically significant treatment effects on sales growth; cf. column (1). These effects are also strikingly similar in magnitude. Applying these growth rates to average pre-treatment sales implies an effectiveness ratio (additional sales per DKK of guarantee) of 95 percent in both cases.

Maybe the most notable difference between the two types of guarantees is that we find positive and large effects of buyer finance on firm export growth, while the corresponding effect

Table B.5: Treatment Effects: Buyer Finance vs. Capital Guarantees

	Total Sales (1)	Exports (2)	Domestic Sales (3)	Total Purchase (4)	Imports (5)	Domestic Purchase (6)	Employ-ment (7)	Value Added (8)
Panel A: Buyer Finance								
ATET (t-1)	-0.0121	-0.00325	-0.00921	-0.0167	0.00739	-0.00234	0.00614	-0.00354
	(0.0295)	(0.0623)	(0.0523)	(0.0295)	(0.0878)	(0.0343)	(0.0141)	(0.0303)
ATET (t)	0.0841***	0.197***	0.0698	0.0675**	0.108	0.0504	0.0394***	0.0770**
	(0.0294)	(0.0536)	(0.0591)	(0.0309)	(0.0948)	(0.0359)	(0.0144)	(0.0305)
Observations	1,849	1,667	1,547	1,855	1,612	1,751	1,852	1,816
Number of treated	332	301	276	332	289	315	332	323
Panel B: Capital Guarantees								
ATET (t-1)	-0.0210	0.0388	-0.0147	0.0190	0.0327	0.0357	0.0208	0.00510
	(0.0343)	(0.0996)	(0.0572)	(0.0271)	(0.104)	(0.0423)	(0.0186)	(0.0347)
ATET (t)	0.0805**	0.131+	0.0545	0.0956***	0.124	0.0262	0.0672***	0.0956***
	(0.0322)	(0.0803)	(0.0538)	(0.0245)	(0.101)	(0.0394)	(0.0165)	(0.0309)
Observations	1,299	1,078	1,212	1,302	1,087	1,246	1,296	1,290
Number of treated	223	184	204	223	185	216	223	223

Notes: All dependent variables are measured in log-changes. Standard errors, adjusted for clustering by firm, are given in parentheses. +, *,**,*** denote significance at the 15%, 10%, 5%, 1% levels, respectively.

for capital guarantees is somewhat smaller and marginally insignificant. However, note that the 95% confidence intervals for these two effects are, in fact, overlapping.

The most important difference with respect to our main results in Table 1 is that estimated treatment effects for imports and domestic purchases are insignificant for both types of guarantees. Notably, however, effects on total purchases remain positive and statistically significant. Thus, these results do confirm that treated firms receiving either type of guarantee increase their purchases – even if the results do not allow us to disentangle whether this effect is stemming from increased domestic purchases or increased purchases from foreign suppliers.

B.5 Heterogeneity in Treatment Effects: ATETs by Firm Size

Our analysis in Section 5 derives backward spillover effects from a sample of large treated firms within manufacturing with more than 50 employees. Since positive direct effects are a prerequisite for these indirect effects, in Table B.6 we report additional results for four different subsamples: Panels A and B give results where our exhaustive sample of firms is split into those with below and above 50 employees. Similarly, Panels C and D give results where the sample of manufacturing firms is split into these two size categories. Note that the estimates for medium-sized and large firms are based on smaller samples; statistical significance is therefore expected to be lower.

In accordance with the findings in Lodefalk et al. (2019), most of the point estimates of our ATETs for small firms (Panels A and C) are greater than those for medium-sized and large firms

(Panels B and D). However, estimates from these different samples are not strictly comparable, because the binary treatment indicator hides substantial heterogeneity in the treatment intensity across samples. For example, in Panels A and B, simple back-of-the-envelope calculations show that the effectiveness of guarantees is, in fact, larger for firms with ≥ 50 employees.[vii]

Most importantly, Table B.6 shows large and significant treatment effects on firm sales and purchases for all samples. (The ATET for sales in Panel D has a p-value of 0.105. Given the small number of treatment effects, we consider this effect to be statistically significant.) These positive treatment effects are the source of the backward spillovers that we identify in Section 5. Interestingly, the ATET for firm employment turns insignificant for large manufacturing firms; cf. Panel D. However, our quantification in Section 5.4 shows that the overall employment effects of export credit guarantees can nevertheless be positive – if one takes into account indirect effects on employment at suppliers of the treated.

B.6 Other Robustness Checks

We have also performed a number of other robustness checks for the matching approach.

First, note that this far we have presented propensity score matching results where we match each treated firm with up to five nearest neighbours. As a first robustness check, we estimate treatment effects using 1-1 matching. Second, note that in Table 1 we condition on at least one year of non-treatment. One might be concerned that this is not enough to avoid mixing up multiple treatments. In our second robustness check, we therefore condition on two years of non-treatment to choose the estimation sample. Finally, in the same spirit of making sure not to mix up multiple treatments, we re-estimate treatment effects focusing on the first year of treatment within our sample period. For all three alternative approaches, we confirm that estimated treatment effects remain largely comparable to those in Table 1 in sign, size, and statistical significance.

[vii] Taking average sales in the pre-treatment year as a base, applying the estimated ATET and comparing it with the average amount of guarantees dispersed to treated firms reveals an effectiveness ratio (additional sales per DKK of guarantee) of 1.1 for medium-sized and large firms in Panel B, compared to an effectiveness ratio of 0.65 for small firms in Panel A.

Table B.6: Matching Approach: ATETs by Firm Size

	Total Sales (1)	Exports (2)	Domestic Sales (3)	Total Purchase (4)	Imports (5)	Domestic Purchase (6)	Employ-ment (7)	Value Added (8)
Panel A: Firms with $<$ 50 Employees								
ATET (t)	0.122***	0.229***	0.0399	0.0734**	0.122	0.0731*	0.0626***	0.109***
	(0.0318)	(0.0612)	(0.0520)	(0.0291)	(0.0997)	(0.0380)	(0.0159)	(0.0301)
Observations	2,127	1,743	1,874	2,135	1,649	2,018	2,133	2,099
Number of treated	370	302	322	370	289	350	370	364
Panel B: Firms with $\geq$ 50 Employees								
ATET (t)	0.0778***	0.112+	0.0909+	0.0885***	0.0792	0.0512+	0.0243*	0.0454+
	(0.0263)	(0.0757)	(0.0597)	(0.0268)	(0.0893)	(0.0341)	(0.0125)	(0.0305)
Observations	953	906	795	951	908	939	949	943
Number of treated	188	179	158	188	178	184	186	188
Panel C: Manufacturing Firms $<$ 50 Employees								
ATET (t)	0.0646*	0.228**	-0.0671	0.0891***	-0.0939	0.101**	0.0704***	0.0444
	(0.0354)	(0.0968)	(0.0798)	(0.0300)	(0.162)	(0.0397)	(0.0196)	(0.0416)
Observations	861	797	748	867	686	838	865	843
R-squared	0.005	0.013	0.002	0.015	0.001	0.013	0.016	0.002
Number of treated	152	140	133	152	122	148	152	150
Panel D: Manufacturing Firms $\geq$ 50 Employees								
ATET (t)	0.0485+	0.0875*	0.0744	0.0693**	0.118	0.0489	0.0099	0.0417
	(0.0299)	(0.0524)	(0.0861)	(0.0298)	(0.0884)	(0.0413)	(0.0145)	(0.0307)
Observations	724	681	569	722	690	721	721	713
R-squared	0.006	0.005	0.003	0.014	0.005	0.004	0.001	0.004
Number of treated	144	133	117	143	137	142	142	142

Notes: All dependent variables are measured in log-changes. Standard errors, adjusted for clustering by firm, are given in parentheses. +, *,**,*** denote significance at the 15%, 10%, 5%, 1% levels, respectively. A common support restriction has been imposed. ATETs in year $t-1$ (not reported) are never statistically significant.

C Additional Results for the Spillover Analysis

C.1 Additional Results for Spillovers at the Firm-Product Level

In Table C.1, we report additional results for our spillover analysis at the firm-product level where we include different sets of fixed effects. To economise on space, we focus on specifications with the spillover variables specified as in column (2) of Table 4. At the bottom of the table, we also report the total number of fixed effects included in each column.

In columns (1) and (2) of Table C.1, we start with specifications which are more parsimonious than our benchmark model in Equation (8). In both columns, we drop the firm-product fixed effects and replace it with firm fixed effects γ_i and product fixed effects γ_p. In column (1), we furthermore replace the industry-year fixed effect γ_{jt} with industry and year fixed effects included separately. For comparison, column (3) reports the estimate from our benchmark regression (cf. column (2) of Table 4).

Across all specifications, the coefficient estimate for *EKF competitors*$_{pt}$ is negative but statistically insignificant at standard levels. Thus, we continue to not find any convincing evidence of negative horizontal spillovers to domestic competitors. Again, we will therefore focus on the backward spillover variable in the following.

In column (1), the coefficient on *EKF Customers*$_{pt}$ loses its significance. In contrast, when including industry-year fixed effects in column (2), the effect turns statistically significant and is remarkably similar to our benchmark estimate in column (3), also in magnitude.

Next, we further investigate the importance of demand and supply shocks. In particular, recall that our data is at the firm-product-year level, but the industry-year fixed effects that we include only account for industry-specific shocks for a firm's industry of main activity. With multi-product, multi-industry firms, however, these fixed effects may not account for all relevant product-specific shocks affecting sales. In column (4), we exploit the circumstance that products in our data are identified at the four-digit level. We can therefore include time-varying product fixed effects at the two-digit level. Importantly, this specification reinforces the evidence for important backward spillovers from EKF guarantees.

In the last column, we additionally include firm-year fixed effects. In this regression, we only exploit variation across varieties within a given firm and year in their exposure to spillovers from export credit guarantees. Note that only firms with multiple four-digit products are included in this regression, leading to a reduction in sample size. Nevertheless, the coefficient estimate

Table C.1: Spillovers at the Firm-Product Level: Robustness Analysis

	Dependent variable: Log Sales$_{ipt}$				
	(1)	(2)	(3)	(4)	(5)
EKF competitors$_{pt}$	-0.0168	-0.0222	-0.0080	-0.0105	-0.0123
	(0.013)	(0.016)	(0.009)	(0.009)	(0.013)
EKF customers$_{pt}$	0.0104	0.0198*	0.0196***	0.0188***	0.0195**
	(0.010)	(0.011)	(0.006)	(0.007)	(0.010)
Observations	75,747	75,670	72,966	72,915	54,333
R-squared	0.619	0.622	0.929	0.930	0.939
N customer + N competitor FE	Yes	Yes	Yes	Yes	Yes
Firm FE, Product FE	Yes	Yes			
Industry FE, Year FE	Yes				
Industry-Year FE		Yes	Yes	Yes	
Firm-Product FE			Yes	Yes	Yes
CN2-Year FE				Yes	
Firm-Year FE					Yes
Total Number of FE	6,433	7,009	14,144	15,123	23,787

Notes: Standard errors, adjusted for clustering by firm and product, are given in parentheses. +, *,**,*** denote significance at the 15%, 10%, 5%, 1% levels, respectively.

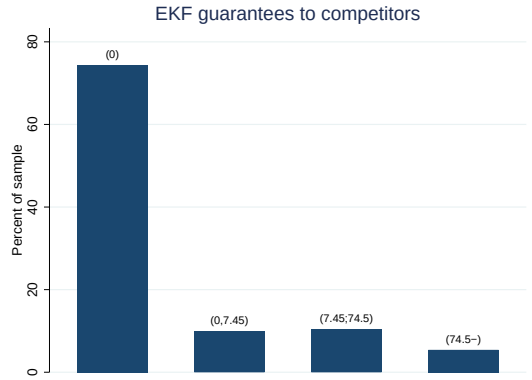
for *EKF Customers*$_{pt}$ remains positive and statistically significant, as well as very similar in magnitude to our benchmark estimate.

C.2 Robustness for the Construction of Spillover Variables

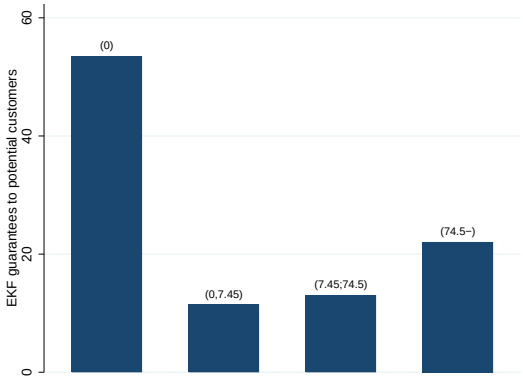
As discussed in Section 5.2, due to differences in coverage across datasets, there are more treated firms that can be matched to the production statistics than to the purchase statistics. As a result, there is less measurement error in the horizontal spillover variables than in the backward spillover variables. Moreover, horizontal spillovers are measured based on a different set of firms and types of guarantees – namely, they also cover smaller treated firms which tend to receive smaller amounts of guarantees. In the following, we discuss two sets of robustness checks which address the concern that these differences could affect the results from the spillover analysis reported in the main text.

First, we estimate regressions where we include each of the spillover channels one at a time. These regressions have the purpose of investigating whether the estimated spillover effects are sensitive to the omission of the respective other spillover channel. Moreover, we note that a given pair of treated and non-treated firms can be simultaneously competing in one product category and be in a customer-supplier relationship for another product category. This circumstance implies that there might be some overlap in our backward and horizontal spillover variables, implying potential multicollinearity issues. Estimates confirm the evidence in favour of backward spillovers, and the implied magnitudes are also strikingly similar to those reported in the main text. Horizontal spillovers remain statistically insignificant.

Figure C.1: Indicator Variables used in Section 5.3.3: Firm-Product Level

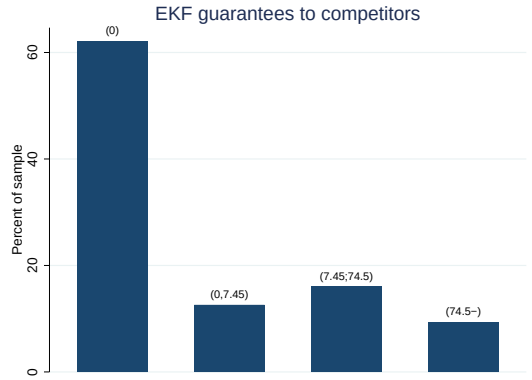


(a) Amount of Guarantees to Competitors

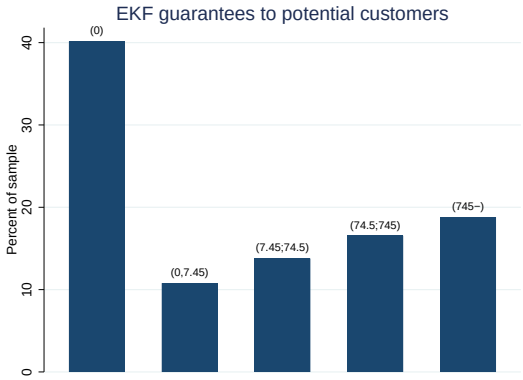


(b) Amount of Guarantees to Customers

Figure C.2: Indicator Variables used in Section 5.3.3: Firm Level



(a) Amount of Guarantees to Competitors



(b) Amount of Guarantees to Customers

Second, we reconstruct the spillover variables after restricting the sample of treated firms to those with information in both the production and the purchase statistics. This restriction implies that we can derive spillovers based on 274 treatment observations over the period 2004–2015. Since most firms that report to the purchase statistics also report to the production statistics, this alternative approach implies only minor changes to our backward spillover variables, but somewhat larger changes to the horizontal spillover variables. Nevertheless, results from this robustness check show remarkably similar coefficient estimates to those reported in the main text.

C.3 Summary Statistics and Regression Results for Section 5.3.3

Figures C.1 and C.2 summarize the information for the indicator variables used in Section 5.3.3.

In column (1) of Table C.2 and columns (1)–(2) of Table C.3, we report the complete regression results underlying Figure 3 in the main text. Note that the coefficients on the indicator

Table C.2: Spillovers at the Firm-Product-Level: Economic Significance

	Log Sales$_{ipt}$	Log Exports$_{ipt}$
	(1)	(2)
Horizontal Spillovers		
EKF Guarantees to Competitors ∈ (0;7.45)	-0.0119	0.0124
	(0.021)	(0.027)
EKF Guarantees to Competitors ∈ (7.45;74.5)	-0.0098	-0.0002
	(0.024)	(0.035)
EKF Guarantees to Competitors >74.5	0.0449	0.0395
	(0.044)	(0.062)
Backward Spillovers		
EKF Guarantees to Customers ∈ (0;7.45)	0.0272+	-0.0544*
	(0.017)	(0.030)
EKF Guarantees to Customers ∈ (7.45;74.5)	0.0391*	-0.0078
	(0.021)	(0.027)
EKF Guarantees to Customers > 74.5	0.0547**	0.0139
	(0.024)	(0.035)
Observations	72,966	45,371
R-squared	0.929	0.884
Firm-Product FE + Industry-Year FE	Yes	Yes
N customer FE + N competitor FE	Yes	Yes

Notes: This table report results where the sums of the amount of EKF guarantees issued to competitors or potential customers is discretized into several binary variables. Guarantees are measured in million DKK. Standard errors, adjusted for clustering by firm and product, are given in parentheses. +, *,**,*** denote significance at the 15%, 10%, 5%, 1% levels, respectively.

variables for horizontal spillovers are statistically insignificant and small in magnitude in all reported regressions. We have therefore solely focussed on backward spillovers in Section 5.3.3 in the main text.

C.4 Robustness Analysis: Other Outcomes of Interest

Throughout our spillover analysis, we have focused on firm-product-level sales, total firm revenue and firm size as outcome variables. Here, we briefly discuss results from three other dependent variables of interest.

First, note that we obtain information on total firm revenue from the FIRM statistics, and revenue thus includes not only sales of own-produced goods but also sales of traded goods, contract work done for others, etc. In column (3) of Table C.3, we focus strictly on the sales of own-produced goods (constructed as the sum over sales of all the goods reported by a firm in the production statistics). This outcome is thus more closely linked to the firm-product-level regressions that we estimate. Results are remarkably similar to those for firm revenue.

Second, we ask whether we can detect significant spillover effects when taking value added as a measure of economic activity instead. Once more, we find positive coefficient estimates for backward spillovers when the sums of guarantees issued to potential customers are sufficiently

large (>74.5 million DKK, approx. 10 million EUR).

Finally, we investigate the question of whether export credit guarantees might entail spillovers in terms of knowledge about export markets. In fact, one particular channel through which the provision of export credit guarantees might affect non-treated firms, is by providing competing firms and suppliers with knowledge about export opportunities. Thus, in the presence of such export spillovers, we would expect firms to be able to increase their export sales after competitors and/or customers have received a guarantee from EKF. On the other hand, since export credit guarantees affect treated firms' sales only on export markets (cf. Table 1), any negative competitive effects of these guarantees should also be more easily identified when considering exports as the relevant outcome.

In column (2) of Table C.2 and column (5) of Table C.3, we report results for variety-level and firm-level exports as dependent variable, respectively. We find only limited evidence that export credit guarantees issued by EKF might in fact have positive export spillover effects for competitors of the treated. In particular, effects on variety export sales are always statistically insignificant. At the firm-level, in contrast, we do find positive and statistically significant effects for two out of three of the indicator variables for horizontal spillovers.

We also do not find much evidence in favour of export spillovers to potential suppliers: the only coefficient which returns statistically significant, is the one for smaller sums of guarantees to customers in the range $(0; 7.45)$ million DKK in the case of firm-product-level export sales; cf. Table C.2. Interestingly, this effect is estimated with a *negative* sign, implying that suppliers to the treated firms redirect their sales from export to domestic markets in response to increased demand from domestic customers receiving treatment. In contrast, all other coefficients for the backward spillover variables in both column (2) of Table C.2 and column (5) of Table C.3 are statistically insignificant and very small in magnitude.

In the literature on export spillovers, much attention is paid to the fact that these spillovers operate at the market-level; i.e., in order to properly identify export spillovers one should focus on exports at the firm-product-destination level. We do not attempt to measure spillovers at more disaggregate levels here, but hope this line of research will be investigated in future research. However, we note that we have found insignificant horizontal spillovers throughout this paper. This finding implies that, even if positive export spillovers for competitors could have been detected at more disaggregate levels, they are not large enough to lead to significant changes in competitors' total revenue or employment.

Table C.3: Spillovers at the Firm-Level: Economic Significance

	Log Total Revenue$_{it}$	Log Employ-ment$_{it}$	Log Sales Produced Goods$_{it}$	Log Value Added$_{it}$	Log Exports$_{it}$
	(1)	(2)	(3)	(4)	(5)
Horizontal Spillovers					
EKF Guarantees to Competitors ∈ (0;7.45)	-0.0040	0.0005	0.0012	-0.0143	0.1104***
	(0.010)	(0.008)	(0.011)	(0.012)	(0.029)
EKF Guarantees to Competitors ∈ (7.45;74.5)	0.0166+	0.0071	0.0094	0.0164	0.0743**
	(0.010)	(0.008)	(0.013)	(0.012)	(0.032)
EKF Guarantees to Competitors > 74.5	0.0089	-0.0005	0.0211	0.0158	0.0215
	(0.015)	(0.012)	(0.020)	(0.017)	(0.045)
Backward Spillovers					
EKF Guarantees to Customers ∈ (0;7.45)	0.0135	0.0111	0.0087	0.0189+	0.0406
	(0.011)	(0.009)	(0.013)	(0.012)	(0.034)
EKF Guarantees to Customers ∈ (7.45;74.5)	0.0062	0.0079	-0.0069	0.0133	-0.0011
	(0.011)	(0.009)	(0.014)	(0.013)	(0.035)
EKF Guarantees to Customers ∈ (74.5; 745)	0.0306**	0.0269**	0.0270+	0.0398***	0.0155
	(0.013)	(0.011)	(0.017)	(0.015)	(0.041)
EKF Guarantees to Customers > 745	0.0373**	0.0306**	0.0527**	0.0488***	-0.0167
	(0.016)	(0.014)	(0.025)	(0.019)	(0.053)
Observations	31,750	31,750	31,750	31,750	24,913
R-squared	0.952	0.954	0.944	0.928	0.901
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N customer FE + N competitor FE + N product FE	Yes	Yes	Yes	Yes	Yes

Notes: This table report results where the sums of the amount of EKF guarantees issued to competitors or potential customers is discretized into several binary variables. Guarantees are measured in million DKK. Standard errors, adjusted for clustering by firm, are given in parentheses. +, *,**,*** denote significance at the 15%, 10%, 5%, 1% levels, respectively.