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Mitigating Liquidity Constraints: Public Export Credit Guarantees in Germany

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Abstract

Reportedly, firms often find it impossible to finance large and long-term projects despite positive net present values. Should governments step in and can their assistance be effective? This paper studies the case of public export credit guarantees in Germany. Covering the default risk of exporters' foreign customers, the policy is supposed to enable funding of international business opportunities that would otherwise remain unexploited. Using German firm-level data covering the universe of publicly insured firms for the years 2000 to 2010, this study tests for the causal effect of guarantees on sales and employment. It employs a difference-in-differences strategy combined with a matching approach, to create an appropriate control group of untreated firms. It finds that guarantees increase firm-level sales and employment on average by about 4.5 and 3.0 percentage points, respectively. During the financial crisis of 2008/09, effects turn out larger. These findings suggest the presence of credit constraints and provide an argument justifying the observed government intervention.

JEL-Code: F360, G280, H250, H810.

Keywords: public export credit guarantees, credit constraints, firm performance, treatment effects.

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1 Introduction

By now it is widely accepted that credit constraints can limit firm growth (Rajan and Zingales, 1998; Fisman and Love, 2007). Constraints are more likely binding when projects are long-term, large, hard to monitor, and risky. This fact begs an important but tricky public finance question: should governments help firms obtaining credit?

Banerjee and Duflo (2012) argue that, in the presence of a public credit program, unconstrained firms would simply substitute public for private instruments, as the former may be less costly than the latter, leaving the level of economic activity unchanged. Credit constrained firms, instead, would expand their activities. We apply this argument to the case of public export credit guarantees. Auboin (2007) and Jean-Pierre and Farole (2009) find that about 80 percent of all exporters make use of some form of trade finance such as export credit insurance.[1] However, for certain export destinations and large volumes appropriate insurance instruments seem unavailable, which makes it difficult for exporters to refinance international business activities. For this reason, an increasing number of countries issue public export credit guarantees, supposedly enabling firms to obtain credit to finance export transactions that would have not been feasible otherwise.[2]

Egger and Url (2006) estimate a gravity-model for Austrian exports on the industry level.[3] Felbermayr and Yalcin (2011) go beyond measuring direct export promoting effects. They use industry-level export data for Germany to show that public guarantees indeed seem to increase exports by alleviating financial frictions encountered by exporting firms.[4] To date, an analysis of firm-level data is still missing. Filling this gap, this paper studies the firm-level performance effects of export credit guarantees underwritten by the Federal Republic of Germany.

Working with firm-level data has several advantages. First, it allows to deal with the non-random selection of firms into public insurance programs, thereby allowing a *causal* interpretation of the results. There are several reasons to belief that assignment of treatment is not random. More successful firms may be better suited in simultaneously obtaining larger export contracts and securing public support. If this is the case, estimations based

[1] Antràs and Foley (2011) develop a theory of trade finance and provide evidence for a large single firm in the US poultry industry.

[2] Berne Union, the leading international association which brings together 48 national export credit agencies (ECAs), reckons that over US $1.4 trillion worth of export were covered by credit guarantees in 2010, facilitating about 10% of world trade (see Berne Union (2010)).

[3] Moser et al. (2008) run a similar exercise on aggregate data for Germany while Janda et al. (2012) repeat the exercise for Czech data.

[4] Earlier important theoretical and empirical contributions include Fleisig and Hill (1984), Abraham and Dewit (2000), Dewit (2001).

on ordinary least squares may yield spurious correlations. Similarly it is conceivable, that the government grants public insurance as an award for their export success. This would imply reverse causation. We deal with these possibilities by applying a matching approach (Rosenbaum and Rubin, 1983; Imbens, 2004; Abadie, 2005).[5] More precisely, in this paper we use a semi-parametric matching estimator proposed by Abadie and Imbens (2011) and combine it with a difference-in-difference strategy following Heckman et al. (1997) to better account for unobservable but time-invariant firm characteristics that may otherwise confound the relation between public insurance and firm performance. Existing macro studies have not been able to convincingly address this issue.

Second, our focus on firm-level data allows us to work with a larger array of variables that, presumably, should be affected by public guarantees such as total sales and employment, but also value added or wages. Indeed, politicians regularly rationalize the public underwriting of export credit risk on the basis of alleged positive employment effects. While we are not attempting a full-fledged general equilibrium analysis of the welfare effects of these guarantees, in the presence of constraints, positive sales and employment effects are necessary conditions for those to arise (Banerjee and Duflo, 2012). Focusing on total sales rather than exports ensures that what we capture is not just a reallocation of sales induced by moral hazard, in the sense that firms under public insurance schemes reallocate sales from less risky domestic and foreign markets to more risky ones, but a real increase in aggregate activity.

This paper draws on a data base that contains the universe of firms located in Germany that have received public credit guarantees in the period 2000 to 2010. The data is provided by Euler-Hermes, a private consortium that administers public export credit guarantees on behalf of the German government. Any losses or profits are consolidated into the federal budget. Germany is an interesting case to study. First, it is a major exporting country, rivalled only by China and the US.[6] Also, Euler-Hermes turns out to be one of the largest public export insurers. Total exposure to short-term export credit risk totalled 61 billion dollar for the US, 60 billion dollars for Germany, and 39 billion for China.[7] So, the case of Germany is of key interest.

Our paper relates to an increasing body of theoretical and empirical literature analyzing the role of financial frictions for exporters' performance in light of the dramatic drop of global export flows during the latest financial market crises. Amiti and Weinstein (2009) illustrate the importance of bank health for firms' export activities. Chor and Manova (2011) additionally show that sectors with greater external finance structure experienced a stronger

[5]For studies that have treated related problems of firm selection with matching methods see e.g. Wagner (2011) and Chari et al. (2009).

[6]From 2003 to 2008, Germany was actually the largest exporter in the world.

[7]According to statistics published by Berne Union (2012).

drop in sales during the financial crises. Paravisini et al. (2011) use matched data from Peru to show that about 15% of the total decline in exports was due to credit shortages. One conclusion emerging from this literature is that financial market frictions restrain cross-border sales. Felbermayr and Yalcin (2011) build on these insights and analyze the effects of public export schemes in the presence of financial constraints based on industry-level data. Following Chor and Manova (2011), the authors show that German export credit guarantees have helped mitigate the liquidity crunch during the financial crisis especially in financially vulnerable sectors. While their study provides new results with respect to the interplay of export credit guarantees and sectoral financial frictions, it does not allow a causal interpretation of the estimates. Following this strand of literature, we interpret the crisis of 2008/2009 as an exogenous shock to the availability of finance. This allows to analyze whether public guarantees indeed help mitigate financial frictions.

The empirical results in our paper suggest the following new conclusions. Firms receiving export credit guarantees experience 4 to 4.5 percentage points higher sales growth compared to similar firms without credit guarantee treatment in the year of the grant of a guarantee (average treatment effect on the treated firms, ATT). Employment growth is on average 2.5 to 3 percentage points higher for treated firms. Given that firms cover on average 6.6 percent of their yearly sales, the resulting magnitudes are plausible. The estimated treatment effects are very robust across different specifications and our results from a placebo treatment analysis strongly support their credibility.

The remainder of the paper is organized as follows. Section 2 describes export credit guarantees in Germany and provides some descriptive evidence. In Section 3 we briefly present an overview of our firm level data. Section 4 motivates and explains our empirical strategy and in Section 5 we discuss our choice of matching variables. In Section 6 we present our results and discuss them. Section 7 concludes.

2 German Export Credit Guarantees

The German government guarantees certain export credit claims of firms located in Germany. Guarantees are issued by a consortium made up by PriceWaterhouseCoopers-AG and the Hermes-Kreditversicherungs-AG on behalf of the Republic of Germany. Therefore, German export credit guarantees are also referred to as "Hermes guarantees".[8] Budgetary responsibility for this instrument lies with the Federal Government that decides on general coverage policy and the granting of guarantees in an Interministerial Committee (IMC). Due

[8] In ancient Greek mythology, Hermes, the son of Zeus and the Pleiade Maia, was the messenger of the gods to humans, and the protector of shepherds and cowherds, thieves, orators and wit, literature and poets, athletics and sports, weights and measures, invention, and of commerce in general.

to the public character of Hermes guarantees, profits and losses made by the consortium are directly incorporated into the German federal budget. Eligibility of countries, sectors, and costs for coverage is defined in a "gentlemen's agreement" amongst OECD member countries also known as the *OECD Arrangement* or the *OECD consensus*.[9] The OECD consensus is important because under WTO rules, "*the provision by governments (or special institutions controlled by governments) of export credit guarantee or insurance programmes*" qualifies as export subsidies and is, thus, outlawed. However, the WTO Agreement on Subsidies and Countervailing Measures exempts those schemes if at least twelve GATT members take part in an "*international undertaking on official export credits*" that regulates the use of those guarantees.[10]

In compliance with the OECD *Arrangement* the German export credit guarantee system offers three instruments. The first and quantitatively most important is the "*Einzeldeckung*" (EZD) which refers to single, well-defined projects (transactions) for specific sectors and countries. The second instrument, the "*Ausfuhrpauschalgewährleistung*" (APG) simultaneously covers several importers, potentially in different countries. The last instrument, "*revolving guarantees*", is negligible as it represents less than 2 percent of total coverage. The key objective of Hermes guarantees is to support exporters by assuming payment default risk for certain export transactions against the payment of a premium, which depends on country risk as classified by the OECD Arrangement.[11] Export credit claims against customers located in the European Union (EU) or other OECD countries (except Chile, Israel, Mexico, South Korea, and Turkey) with contract durations of less than 24 months are assumed to be marketable and therefore cannot be publicly insured.[12] Contracts of longer duration can principally be insured for all countries, subject to an array of conditions. For example, German value added must be at least 70% of the total sales contract to be insured or the value added content of the destination country must not exceed 23%.

Table I shows the time behavior of German exports and the volume of Hermes guarantees over the last ten years. Aggregate exports almost doubled between 2000 and 2008 from 500 billion to almost one trillion Euros. During that period, the issuance of public export credit guarantees first declined from 3.3 to 1.8 percent of total exports in 2007. Following the collapse of financial markets after the Lehman Brothers bankruptcy, the share of exports

[9]Current participants to the arrangement are: Australia, Canada, the European Union, Japan, Korea, New Zealand, Norway, Switzerland and the United States.

[10]WTO Agreement on Subsidies and Countervailing Measures, Annex I, articles j and k.

[11]The classification is publicly available and known as Country Risk Classifications of the Participants to the Arrangement on Officially Supported Export Credits. It groups countries into eight groups according to their riskiness with 0 as the lowest risk level and 7 as the highest one.

[12]Due to the financial crisis in 2008/09 the list of eligible countries was extended during the period 2009-2012.

Table I: German Exports and Hermes Guarantees, 2000-2010

Year	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
Exports (in bn Euro)	597.44	638.27	651.32	664.46	731.54	786.27	893.04	965.24	984.14	803.31	959.50
Exports/GNP	29.2%	30.4%	30.5%	30.9%	33.3%	35.3%	38.6%	39.7%	39.8%	33.8%	38.7%
Credit constraint Indicator$^{a)}$				56.0%	48.4%	36.5%	24.2%	15.9%	28.6%	44.4%	33.8%
Hermes guarantees (in bn Euro)	19.50	16.56	16.43	15.99	21.07	19.77	20.55	16.94	20.68	22.38	32.46
Coverage	3.3%	2.6%	2.5%	2.4%	2.9%	2.5%	2.3%	1.8%	2.1%	2.8%	3.4%

Notes: Yearly German exports and GNP data are from the Federal Statistical Office of Germany. Aggregate Hermes guarantees represent the yearly sum of EZD, APG and revolving guarantees. The data was provided by Euler-Hermes. Coverage is calculated as sum of Hermes guarantees over Exports. *a*) ifo Credit Constraint Indicator: share of surveyed manufacturing firms indicating that credit access is "restricted".

covered increased again to 3.4 percent as of 2010. This reflects the substitution of public for private insurance and points towards a possible mitigating effect of public guarantees in the global trade collapse of 2008.[13]

Public export credit guarantees play an important role in emerging economies characterized by risky business environments. Figure I illustrates the distribution of exports and Hermes guarantees across different OECD country risk classes over time.[14] In all years, countries in the middle risk categories (categories 2 to 4, including emerging economies like China, Brazil and India) have absorbed 50 to 68 percent of Hermes guarantees, while only accounting for 15 to 24 percent of German exports. Middle and high risk countries together account for about 90 percent of Hermes guarantees before the crisis, but only for about one quarter of German exports. Low risk countries absorb more than two thirds of German exports but, before the crisis, account for not more than 15 percent of Hermes guarantees. In the aftermath of the global financial crisis, when OECD countries such as Greece become temporarily eligible for Hermes coverage, that share rose to 29 percent.

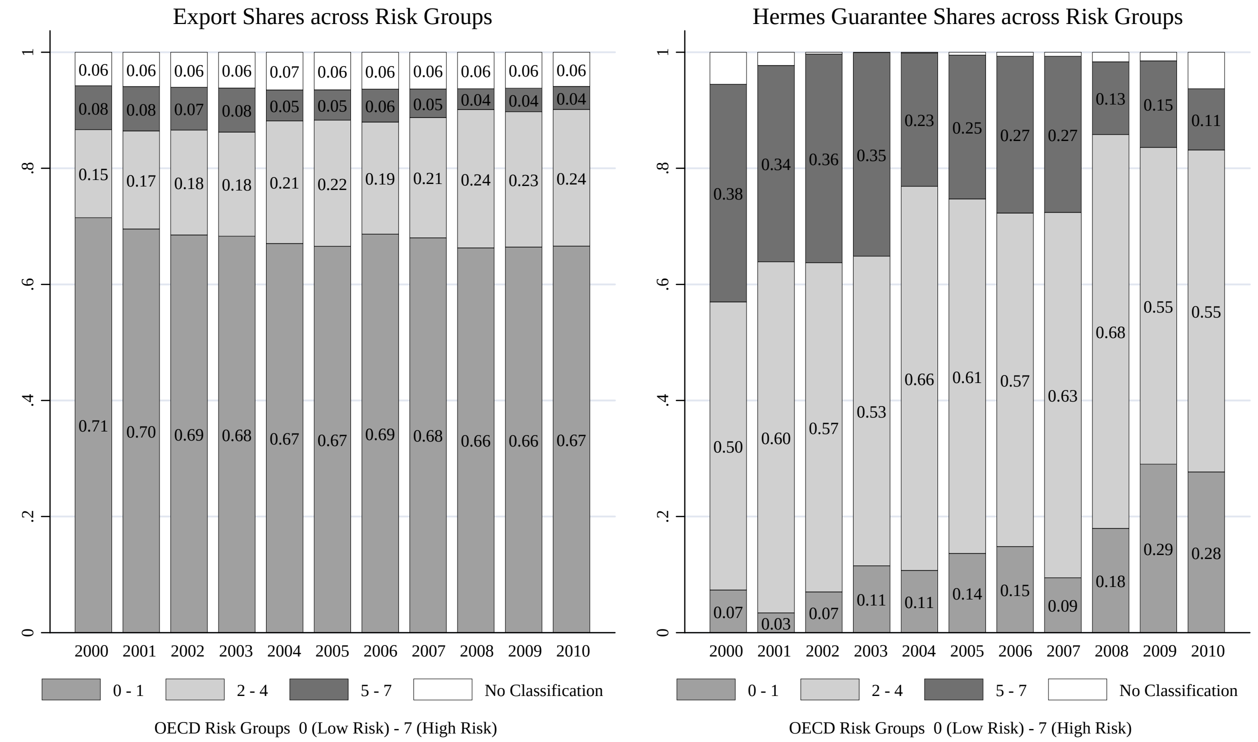
Figure II shows that the share of exports covered by Hermes (EZD only) differs strongly across industries (classified according to NACE rev. 2).[15] The highest coverage ratios are found in the aircraft industry (10%) followed by the machinery sector (6%). Other indus-

[13]In 2009, the World Bank reports a drop in global trade by 22% and in Germany by 18.4%. Eaton et al. (2011) and Yi (2009) argue that the major reason for that strong decline was a disproportionately strong drop in demand for traded goods. Chor and Manova (2011) among others additionally claim that constraints in trade finance played a crucial role during the recent economic collapse.

[14]Appendix A.1 lists the countries in each risk category.

[15]Only industries with positive Hermes EZD coverage are shown. Euler-Hermes does not report the distribution across industries of insurance instruments other than EZD.

Figure I: Export and Hermes Guarantee Shares Across OECD-Risk Groups

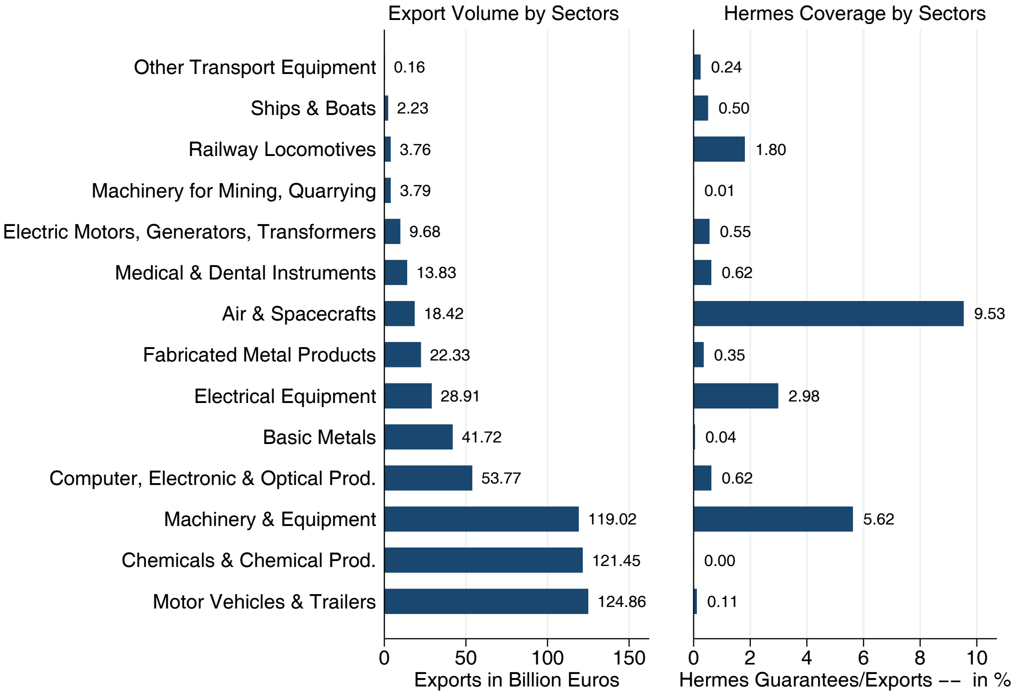


Notes: Export shares are calculated based on UN-COMTRADE data (BACI database with corrected trade flows). Export destination countries are categorized according to OECD Country Risk Classification. Countries with the lowest risk are classified as 0 (e.g. France), very risky ones as 7 (e.g Afghanistan). Hermes Guarantee volumes are provided by Euler-Hermes. Stacked bars in the left panel depict yearly German export distributions (1=100%) across three different groups categorized along the OECD risk classification. Accordingly, in each considered year German exports are predominantly sold to low risk destinations. Stacked bars in the right panel illustrate that a major share of German export credit guarantees are claimed for export destinations with a risk class larger than 1. Six percent of German trade goes to unclassified countries.

tries with high coverage are electrical equipment (3%) and railway locomotives (2%). These industries are characterized by long-term and large export deals. By contrast, the leading two export industries (motor vehicles and chemicals) which feature short-term and relatively smaller transactions exhibit a coverage of foreign sales with EZD below 1%. These patterns show that next to country-specific features (policy or business conditions), industry-specific aspects (size and duration of projects) appear to play a crucial role for the use of Hermes guarantees.

At times in the past, the issuance of public export credit insurance had led to substantial losses for the German tax payer. Accumulated losses from export credit claims amounted to 13.4 billion Euro in 1999, mostly because of losses related to the Russian financial crises of 1998. Since 2006, however, the cumulative effect of Euler-Hermes guarantees on the federal budget has been positive and stands at 2.6 billion Euro at the end of 2011. Nonetheless, given an exposure of almost 30 billion Euro per year, it is of obvious public and political interest to know whether those guarantees have indeed stimulated economic activity by increasing sales, employment, and value added relative to the hypothetical counterfactual of no public insurance. In this paper, we carry out such an analysis at the firm level. While our strategy

Figure II: Export Volumes and Hermes Coverage by Sectors in 2009



Notes: Sector classification is in NACE rev. 2. Sectoral export volumes are derived from HS-6 BACI data by using correspondence tables from the RAMON database and Eurostat. Export credit guarantees include Einzeldeckungen (EZD), which can be assigned to specific sectors. Appendix A.2 list the sectoral nomenclature. Sectors without Hermes guarantees are excluded. Source: Euler-Hermes and BACI database.

precludes general equilibrium and welfare considerations, it is the first to explore the causal effect of guarantees using firm-level data. In the next section we first present descriptive statistics about firms with and without Hermes guarantees treatment, before moving on to explaining our empirical strategy.

3 Firm-Level Data

Euler-Hermes has provided us with 4850 names and addresses of all German firms that received Hermes guarantees between 2000 to 2010. These firms are predominantly manufacturers. We merge these data with information from the Amadeus database provided by Bureau van Dijk Electronic Publishing GmbH (BvDEP).[16] The Amadeus data base oversamples large firms. However, in the more recent years we are able to identify up to 80% of all

[16]In Germany, private data, such as those from Euler-Hermes cannot be merged with official firm-level data (AfiD-panel) without making the resulting data set public. This was no available option in the present context. To satisfy German data protection regulation, the merge of Euler-Hermes and Amadeus firm data was carried on by the Economics and Business Data Center at Munich University.

"Hermes" firms. The Amadeus data for Germany does not contain information on exports. Therefore, we merge a separate database, DAFNE, also available at Bureau van Dijk E. P. GmbH. Merging Amadeus and DAFNE leads to some loss of observations. Moreover, the export variable in DAFNE is problematic, since it is not surveyed at a yearly basis. We use the available cross-section to identify firms as exporters and restrict our sample to those firms. Patchy time-coverage of the export share variable makes it impossible to use exports as a dependent variable in our analysis.

After eliminating duplicates and further inconsistencies, our sample contains 35,852 observations of exporting firms.[17] Out of these observations, 7,776 turn out to provide information about the main variables. Among them, in 1,391 observations we observe Hermes treatment.[18]

In 2002, the average size of a guarantee extended to a Hermes firm was 30 million Euros. In the succeeding years that average steadily decreased and reached a minimum of eight million Euros in 2007, reflecting at the same time a wider use of the instrument as more firms expanded into markets covered by Hermes and smaller projects. Interestingly, during the financial crisis the average provision of Hermes guarantees per firm increased only slightly staying at around 10 million Euros but increased significantly in 2010, mostly due to the easing of the OECD Arrangement between 2009 and 2012 as explained earlier. Figure A.1 in the Appendix contains details.

Table II: Mean Difference for Treated and Untreated Firms

Variable	Description	Exporter with Hermes guarantees	Exporter without Hermes guarantees	t-test on mean difference = 0 (p-value)
Age (years)		30	34	0.02
Sales (Euro, mn.)		357	131	0.00
Employment	*number of workers*	1,528	710	0.00
Average Wage (Euro, th.)	*total wage bill/workers*	57	51	0.00
Log TFP	*total factor productivity*	5.28	5.06	0.00
Total Assets (Euro, mn.)		293	83	0.00
Liquidity	*current liabilities/current assets*	0.72	0.51	0.28
Tangible Assets (Euro, mn.)		92	18	0.00

Notes: P-values refer to t-test on mean difference with unequal variances.

In Table II we present sample means for firms that received Hermes guarantees at least once between 2000 and 2010 and firms that never used Hermes guarantees. Accordingly, firms with public credit guarantees on average employ more people, are older and more productive,

[17]We drop firms reporting only consolidated accounts, firms reporting a status other than *active* and firms which are identical in all of our relevant variables, yet appear with different firm identifiers in the Amadeus database. Furthermore, we prune the dataset by dropping observations containing the 1% largest and 1% smallest realizations of our outcome variables.

[18]Table A.3 in the Appendix provides summary statistics of major variables in our sample.

realize higher sales, have more assets, and pay higher wages. Except for the liquidity ratio, all mean differences are significant at least at the 5 percent level. The apparent differences between the group of treated and untreated firms underlines the concern that selection into treatment is an issue. Fortunately, our data offers a rich set of firm characteristics from which we can select variables that we consider to be relevant for selection. We describe this choice of variables in detail in Section 5. Before, we describe our estimation strategy and how we deal with unobserved determinants of selection.

4 Empirical Strategy

4.1 Estimating The Treatment Effect of Hermes Guarantees

Let $H_{i,s,t} \in (0,1)$ be a dummy variable which takes the value one if a firm i in sector s received public export guarantees in year t and zero otherwise. We are mostly interested in the effect of $H_{i,s,t}$ on an outcome $Y_{i,s,t}$, such as employment or sales. A natural but probably naive linear model would be

$$\ln Y_{i,s,t} = \delta H_{i,s,t} + \beta' \mathbf{X}_{i,s,t} + v_{s,t} + v_i + \epsilon_{i,s,t}, \tag{1}$$

where the vector $\mathbf{X_{i,s,t}}$ contains control variables, $v_{s,t}$ is an industry-year effect, v_i captures firm-specific unobserved heterogeneity, and $\epsilon_{i,s,t}$ is an error term. The coefficient δ estimates the *average treatment effect* (ATE) of Hermes guarantees on our outcome variables $Y_{i,s,t}$, i.e., the average difference between the outcome of a treated and an untreated firm that is associated with the treatment status of a firm. A common strategy to absorb v_i is to first-difference the equation or to use fixed effects estimation. A fundamental but plainly problematic assumption for consistent estimation of δ is *random* assignment of Hermes guarantees to exporters conditional on controls.

In the absence of a suitable instrumental variable, we apply matching estimation to generate consistent estimates of the treatment effect δ. The general idea behind the matching approach is to construct the counterfactual outcomes by means of clone firms which are identical in every relevant respect except the treatment status. The method has been widely applied in the policy evaluation context, mostly on labor market programs (LaLonde (1986), Gobillon et al. (2012), to mention only very few examples,) but also on the trade effects of free trade agreements (Egger et al., 2008), or currency unions (Baldwin and Taglioni, 2007).[19] Besides dealing with endogenous selection into treatment, matching estimation does not rely on a functional form assumption regarding the relationship between observable characteristics

[19]Blundell and Dias (2009) and Imbens and Wooldridge (2009) provide an overview over methods.

and the outcome variable and it avoids predictions into ranges of observables that are outside the support of the group of treated units. Moreover, linear models estimate the average treatment effect (ATE), which, in the presence of endogenous selection into treatment, differs from the average treatment effect on the treated (ATT).[20] In the context of policy evaluation, however, the ATT is of larger interest as it reflects the effects of Hermes guarantees on those firms, that have actually taken part in the program.

Let Y_i^0 and Y_i^1 denote the potential outcomes for firm i depending on its treatment status $H_i \in (0,1)$. Then, the ATE is given by $ATE = \mathbb{E}(Y_i^1 - Y_i^0)$, where $\mathbb{E}$ is the expectation operator. The ATT is obtained from a comparison of potential outcomes only for firms that are indeed treated, i.e. it is given by $ATT = \mathbb{E}(Y_i^1 - Y_i^0 | H_i = 1)$. The ATT differs from the ATE if the difference in potential outcomes depends on firm characteristics and the average treated firm differs from the average untreated firm with respect to these characteristics. Identification and consistent estimation relies on three assumptions. The first one is the *conditional independence assumption* ("selection on observables", (Rosenbaum and Rubin, 1983)):

$$(Y_i^0, Y_i^1) \perp H_i | X_i, \tag{2}$$

where $\perp$ denotes independence. It states that conditional on a set of observable characteristics X_i, the potential outcome for each firm is independent of the treatment status. Hence, under the conditional independence assumption the counterfactual outcome for any firm is identical to the outcome of its control firm which exhibits identical observable characteristics but the opposite treatment status, and therefore, any difference in outcomes between a pair of matched firms can, in expectations, be attributed to the treatment. The second assumption is the *overlap assumption*, which requires that firm characteristics X_i do not perfectly predict the treatment status. In other words, for each combination of firm characteristics there must potentially exist both firms with and without treatment.[21] The third requirement is the *stable unit treatment value assumption* (SUTVA), which states that the impact of Hermes guarantees on one firm is independent of the allocation of treatment among the other firms.

Under these assumptions, a consistent estimate of the sample average treatment effect

[20] The ATE can be interpreted as the expected value of the treatment effect for a firm that has the average characteristics of the sample. In contrast, the ATT reflects the expected value of the treatment effect for a firm that has the average characteristics of the subsample of treated firms.

[21] If the conditional independence and the overlap condition simultaneously hold Rosenbaum and Rubin (1983) refer to this as "strong ignorability".

(SATE) is given by

$$\widehat{SATE} = \frac{1}{N}\sum_{i=1}^{N}(\hat{Y}_i^1 - \hat{Y}_i^0) \tag{3}$$

where $\hat{Y}_i^1 = Y_i$ if $H_i = 1$ and $\hat{Y}_i^1 = \left(\sum_{j=1}^{k}\hat{Y}_{ij}\right)/k$ else; and $\hat{Y}_i^0 = \left(\sum_{j=1}^{k}\hat{Y}_{ij}\right)/k$ if $H_i = 1$ and $\hat{Y}_i^0 = Y_i$ else.[22] N denotes the sample size, k equals the number of control firms used in the construction of the counterfactual outcome for firm i and Y_{ij} $\forall$ $j = 1, ..., k$ are the outcomes of the k control observations for firm i.

Accordingly, the sample average treatment effect on the treated is consistently estimated as

$$\widehat{SATT} = \frac{1}{N^1}\sum_{i=1|H_i=1}^{N}(Y_i - \hat{Y}_i^0) \tag{4}$$

where Y_i is the observed outcome of the treated firms, $\hat{Y}_i^0 = \frac{1}{k}\sum_{j=1}^{k}\hat{Y}_{ij}$ the average outcome of the control observations for firm i and N^1 denotes the number of treated firms in the sample. The choice of appropriate control firms is based on a metric that summarizes the distance of two firms in the multidimensional space of firm characteristics. Following Abadie and Imbens (2011), we use the Mahalanobis distance metric. We present results based on propensity scores in our robustness checks.[23]

The estimation routine proposed by Abadie et al. (2004) allows to specify variables on which matching is performed exactly, hence, it enables us to match firms within narrowly defined sector-year cells without estimating a huge set of parameters in a first step. The Mahalanobis distance $m_{ij}(x_i, x_j)$ between two firms i and j with opposite treatment status is calculated as $m_{ij}(x_i, x_j) = \sqrt{(x_i - x_j)^T\mathbf{S}^{-1}(x_i - x_j)}$, with $\mathbf{S}$ representing the sample covariance matrix or a diagonal matrix of sample variances of $\mathbf{X}$. Hence, the Mahalanobis metric is based on the Euclidean distance $\parallel x_i - x_j \parallel$ in matching variables $\mathbf{X}$ between firms i and j. In a one-to-one match ($k = 1$), the firm j that is closest to firm i in terms of m_{ij} is chosen as control observation and receives a weight one. Besides one-to-one matching, the method also permits k-nearest neighbor matching, in which the k nearest neighbors are chosen as controls

[22]Note that the estimator for the *sample* average treatment effect is identical to the estimator of the population average treatment effect. Differences arise in the estimation of the variance (cf. Abadie and Imbens, 2011).

[23]The propensity score, i.e. the predicted probability of obtaining treatment given observed covariates $\widehat{P}(H_i = 1|X_i)$, is usually obtained from a first stage Probit or Logit estimation. Since we want to control for sector and time specific unobserved heterogeneity, conditional Logit with fixed effects would be the optimal choice for our case. However, given the relatively small number of treated firms in our sample and the large number of parameters to be estimated in the first stage, we prefer the Mahalanobis metric as distance measure for the matching.

entering with weights $w_{ij} = 1/k$. In our empirical evaluation, we let k vary from 1 to 5.

Abadie and Imbens (2006) show that estimates of the treatment effects from finite samples suffer from a bias due to remaining differences in covariates, with the severity of the bias increasing in the number of continuous covariates. Abadie and Imbens (2011) propose a *bias-correction* for the estimators that accounts for differences in covariates within the matches, by correcting for differences in predicted outcomes obtained from ordinary least squares. The bias-corrected estimator replaces $\hat{Y}_i^0$ in (4) by

$$\widetilde{Y}_i^0 = \frac{1}{k} \sum_{j=1}^{k} \left(Y_{ij} + \hat{\mu_0}(X_i) - \hat{\mu}_0(X_{ij}) \right), \tag{5}$$

where $\hat{\mu_0}$ is the predicted outcome of the linear model $\hat{\mu}_0(X) = \hat{\beta}_{00} + \hat{\beta}_{01} X$.[24] We use this bias adjustment in all of our baseline specifications.

4.2 DiD-Matching

To relax the selection on observables assumption one can use matching in differences (DiD-matching). Comparing *changes* in the outcome variables of the group of treated firms to *changes* in the outcome of the control group allows to neglect time-constant unobserved factors that simultaneously affect the treatment status and the level of the outcome variables. Assuming that the treatment occurred between period t and $t-1$, the SATT based on DiD-matching can be estimated by comparing changes in the outcome variable of treated firms between $t-1$ and t with the respective changes in the outcome variable in the control group:

$$\widehat{SATT}^{DID,bc} = \frac{1}{N^1} \sum_{i=1|H_i=1}^{N} \left((Y_{i,t}^1 - Y_{i,t-1}^1) - (\widetilde{Y}_{i,t}^0 - \widetilde{Y}_{i,t-1}^0) \right), \tag{6}$$

where $\widetilde{Y}_{i,t}^0$ and $\widetilde{Y}_{i,t-1}^0$ are defined in (5).

A sharp definition of the pre-treatment period, which is necessary to consistently determine the changes in the outcome variables, implies that we can only use those treatment observations where we observe *changes in the treatment status* to identify the treatment effect. We therefore define the treatment dummy *to Hermes* which equals one in period t if the firm changes from no treatment in $t-1$ to treatment in period t and zero otherwise.[25]

[24] Coefficients are estimated by weighted least squares (WLS) based on the subsample of control observations weighted by the number of times they are used as control firms (for details see Abadie and Imbens, 2011).

[25] Alternatively, we could also use the exit from treatment to identify the effect. However, the high likelihood that treatment occurs in lagged periods prevents us from doing so.

While DiD-matching reduces the number of treatment observations, it strongly enhances our confidence in the reliability of our results.

Finally, when matching is based on pre-treatment variables, differences in time trends in the covariates across the groups of treated and control firms can lead to biased estimates. Heckman et al. (1997) propose a *regression-adjusted matching estimator* that controls for different time trends in observable covariates. The regression-adjusted estimator for the SATT is then

$$\widehat{SATT}^{DID,ra} = \frac{1}{N^1} \sum_{i=1|H_i=1}^{N} \left((\breve{Y}^1_{i,t} - \breve{Y}^1_{i,t-1}) - (\breve{Y}^0_{i,t} - \breve{Y}^0_{i,t-1}) \right) \tag{7}$$

where $\breve{Y}^1_{i,t} = Y^1_{i,t} - X_{i,t}\hat{\beta}_0$ and $\breve{Y}^0_{i,t} = \frac{1}{k}\sum_{j=1}^{k}\left(Y^0_{ij} - X_{ij,t}\hat{\beta}_0\right)$. Instead of the conditional independence assumption this estimator requires that the distributions of unobservable characteristics be equal across treated and untreated firms (Heckman et al., 1998).[26] Regression-adjusted matching allows us to check the robustness of our results from the preferred specification with respect to the common time trend assumption.

4.3 Identification Strategy

We define a firm as treated in t if it experienced a change in its Hermes status from no guarantee in $t-1$ to a guarantee in t and compare the average change in the outcome variable in the group of firms treated in that way to the average change in the control group. Control firms are selected based on average *pre-treatment* values of appropriately chosen control variables; see the discussion below. We take account of the bias arising from differences in covariates as described by Abadie and Imbens (2006) by applying the *bias-correction* in (5) to our DiD-estimator in our baseline specification. To take account of the potential bias arising from different time trends in covariates we also estimate the treatment effect using *regression-adjusted* matching as described in (7). Besides removing time-constant firm specific unobserved heterogeneity through differencing, we also control for sector-time specific influences by matching *within sector-year cells.* In our baseline estimation, sectors are defined on the 2-digit level of the NACE rev. 2 classification.[27]

To assess the validity of the conditional independence assumption, we test for the balancing property and estimate pseudo treatment effects. The latter allows to assess whether our

[26]This procedure is analogous to a difference-in-difference estimation along the weighted linear regression specification in equation (1) estimated in changes. Weights are derived from the nearest neighbor matching based on the Mahalonobis distance metric calculated from pre-treatment values of the covariates X.

[27]Naturally, a stronger disaggregation of sectors comes at the cost of a smaller pool of potential control firms. In our robustness analysis we test the sensitivity of our results with regard to this choice.

groups of treated and control firms exhibited significantly different changes in the outcome variables in any periods *other than the period of treatment.*

Altogether, we find very robust evidence for a positive effect of Hermes guarantees on firms' sales and employment and we are confident that our approach identifies *causal* effects. Before we present the results in detail in Section 6, we first discuss our choice of matching variables and the common support assumption in the following section.

5 Matching Variables and Common Support

5.1 Matching Variables

Our strategy requires that all variables which influence the change in the treatment status and in the outcome variables are accounted for in the matching process. Although the choice of variables is crucial, the econometric literature provides little guidance on how to choose covariates. So, we use relevant economic theory and related empirical work to guide our choice of matching variables. According to recent heterogeneous firms theory (based on Melitz, 2003), the share of exports in total sales is higher in firms that are more productive, larger, and older, and have relatively more skilled employees compared to only domestically active firms.[28] Hence, a first set of matching variables include measures of total factor productivity (TFP), size, age and a measure of skill intensity of production.[29] We use *total assets*, *tangible assets*, *employment*, and *sales* to capture firm size and *total wage bill over number of employees* to approximate *skill* intensity, (see also Wagner, 2011). A second set of variables is motivated by recent trade literature focusing on the role of trade credit frictions. Chor and Manova (2011) illustrate that sectors with higher external financial dependence experienced a stronger reduction in foreign sales. Following Chor and Manova (2011) and others, we construct two indicators that measure firms' access to external finance. The first one is the stock of *tangible assets*, where we expect that a higher stock of tangible assets mitigates credit constraints as tangible assets can serve as collateral. The second measure is *liquidity* measured as current liabilities over current assets. A smaller liquidity ratio then indicates better access to external finance.

This constitutes our set of firm characteristics. We match on pre-treatment averages of the firm variables as all of them (except age) are more or less likely endogenous to the treatment itself. Furthermore, we add pre-treatment averages of *sales growth.* This choice of

[28] A large strand of empirical literature provides solid evidence for these theoretical results (see e.g. Bernard and Jensen, 1999, 2004; Wagner, 2007; Bernard and Wagner, 2001).

[29] *TFP* is estimated using the methodology developed by Olley and Pakes (1996).

matching variables confines us to a sample of firms that report sufficiently detailed data and are observed for at least two consecutive years. Since data availability is strongly correlated with the size of firms, our estimation sample is not representative for the subpopulation of firms in the Amadeus database. However, since our concern is an ex-post evaluation of export credit guarantees by means of the sample average treatment effect on the treated, this does not constitute an obstacle to our analysis. We assess the association of the matching variables with the treatment status by means of probit estimations and pairwise correlations. Results are collected in Table A.4 and Table A.5 in the Appendix. Both exercises show that our matching variables and the treatment status are strongly correlated.

5.2 Assessing Common Support and the Balancing Property

As described above, identification of treatment effects relies on the validity of the *common support* or *overlap assumption*. For each set of firm characteristics, we must potentially be able to observe treated and untreated firms. Ideally, the assessment of the validity of the overlap assumption would be based on the multivariate distribution of the matching variables. Since this is not feasible, we compare the marginal distribution of each covariate for Hermes and non-Hermes firms. Figure A.3 in the Appendix presents kernel densities for our final choice of variables before matching. We find substantial overlap for all matching variables.[30] Furthermore, we drop firms in sector-year cells in which no treated or no untreated firms are present to ensure overlap in our exact matching variables. As expected from our discussion of selection into treatment, for the pre-matching samples the estimated densities differ significantly between the groups of treated and untreated firms.

We assess the balancing property by comparing mean difference tests for the pre- and post-treatment samples. As illustrated in the upper part of Table III, the null hypothesis of identical means between treated and untreated firms (t-test) is rejected for all relevant variables in the pre-matching sample. Equally, the Kolmogorov-Smirnov test rejects equality of the respective distributions. The lower part of Table III shows the same statistical tests for the relevant variables after our preferred Mahalanobis nearest neighbor matching (with one nearest neighbor). In the matched sample, differences have decreased significantly. Accordingly, the null hypothesis of identical means between treated and untreated firms can no longer be rejected and the Kolmogorov-Smirnov test does not reject equality of the respective distributions (except for average total assets). We treat these results as support that our matched sample fulfills the balancing property.[31]

[30] We minimally trim our sample by dropping the largest 0.01% of observations of all relevant matching variables.

[31] We find qualitatively similar results for the samples obtained from matching k>1 nearest neighbors.

Table III: Differences between Treated and Untreated Firms Before and After Matching

Variable (Two-year average)	Enterprises with Hermes guarantees	Enterprises without Hermes guarantees	t-test on Mean Difference (t-statistics)	Kolmogorov-Smirnov-Test (p-values) H0: Equality of Distribution btw treated and untreated firms	H1: Difference in favor for untreated firms	H2:Difference in favor for treated firms
Before matching						
Age	37.15	42.41	2.32	0.007	0.999	0.003
Δ ln Sales	0.06	0.05	-0.47	0.038	0.019	0.027
ln Employment	5.56	5.57	0.20	0.534	0.273	0.576
ln Liquidity	-1.03	-0.86	2.95	0.013	0.983	0.007
ln Sales	11.20	11.11	-1.06	0.142	0.071	0.888
ln Skill	3.95	3.89	-3.04	0.002	0.001	0.935
ln Tangibles	8.32	8.58	2.02	0.264	0.997	0.132
ln TFP	5.31	5.15	-3.44	0.000	0.000	0.941
ln Total assets	10.66	10.53	-1.39	0.006	0.003	0.838
After matching						
Age	37.15	37.74	0.21	0.112	0.719	0.056
Δ ln Sales	0.06	0.06	-0.02	0.351	0.176	0.567
ln Employment	5.56	5.57	0.16	0.929	0.554	0.691
ln Liquidity	-1.03	-0.93	1.29	0.457	0.896	0.231
ln Sales	11.20	11.18	-0.15	0.350	0.176	0.789
ln Skill	3.95	3.95	0.03	0.712	0.448	0.376
ln Tangibles	8.32	8.48	0.9930	0.291	0.146	0.329
ln TFP	5.31	5.26	-0.7693	0.728	0.386	0.550
ln Total assets	10.66	10.57	-0.8213	0.044	0.022	0.922

Notes: Compared variables are two year averages before treatment occurs. The matching sample is obtained from matching k=1 nearest neighbors with Δ ln Sales as outcome variable. Similar results hold for higher k and Δ ln Employment as outcome variable. The t-test on mean difference assumes unequal variances for both groups of firms. Regarding the Kolmogorov-Smirnov test, H0 test for equality of the respective distributions, H1 tests whether the distribution of firm characteristics of untreated firms stochastically dominates those of the Hermes firms and H2 test the opposite hypothesis. *Sales, Skill (Wage bill/workers), Total assets, Tangibles* are in thousand Euro. *Liquidity* is defined as current liablities/current assets.

6 Evaluating Public Export Credit Guarantee Effects

6.1 Results from DID-Matching

Table IV shows the estimated treatment effects on sales and employment from the different matching strategies discussed in Section 4. We estimate four different specifications (which are found in columns one to four) and use different numbers of nearest neighbors in the construction of the counterfactuals (reported in rows one to five).

Column (1) in Table IV contain the treatment effects estimates on sales and employment, respectively, obtained from the *bias-corrected DiD-estimator* as specified in (6). This constitutes our preferred specification. The estimates show that export credit guarantees trigger sales growth of 3.9 to 4.8 percentage points and employment growth of 2.5 to 3 percentage points, respectively. Comparing the coefficient estimates to those in column (2), where we present the uncorrected matching estimates, shows that the bias correction primarily makes a

Table IV: Baseline Results: Sample Average Treatment Effects on the Treated (SATT)

# matches	(1) Abadie-Imbens bias corr.	(2) Abadie-Imbens no correction	(3) Abadie-Imbens regression adj.	(4) Prop.-score estimation
Outcome variable: $\Delta \ln$ Sales				
1	0.0469***	0.0458***	0.0809**	0.0369*
	(0.0173)	(0.0173)	(0.0305)	(0.0207)
2	0.0483***	0.0420***	0.0654**	0.0302*
	(0.0154)	(0.0154)	(0.0251)	(0.0174)
3	0.0467***	0.0412***	0.0496**	0.0219
	(0.0143)	(0.0143)	(0.0232)	(0.0167)
4	0.0387***	0.0364***	0.0522**	0.0203
	(0.0135)	(0.0136)	(0.0234)	(0.0163)
5	0.0437***	0.0399***	0.0553**	0.0192
	(0.0135)	(0.0136)	(0.0221)	(0.0159)
N	7090	7090	7090[a]	7090
N treated	289	289	289[a]	287
Outcome variable: $\Delta \ln$ Employment				
1	0.0253***	0.0270***	0.0490***	0.0203*
	(0.0095)	(0.0094)	(0.0175)	(0.0120)
2	0.0273***	0.0264***	0.0310**	0.0209**
	(0.0081)	(0.0082)	(0.0154)	(0.0098)
3	0.0303***	0.0294***	0.0333**	0.0227**
	(0.0077)	(0.0077)	(0.0145)	(0.0090)
4	0.0280***	0.0276***	0.0247*	0.0253***
	(0.0077)	(0.0077)	(0.0145)	(0.0086)
5	0.0277***	0.0268***	0.0313**	0.0233***
	(0.0074)	(0.0074)	(0.0144)	(0.0082)
N	7053	7053	7053[a]	7053
N treated	280	280	280[a]	275

Notes: Treatment effects are estimated as changes in log outcomes in the year of the treatment, where treatment in t is defined as the change in treatment status from no treatment in $t-1$ to treatment in t. Matching variables are pre-treatment two-year averages of *TFP, skill, tangible assets, liquidity, employment, sales, sales growth, age, and total assets*. Matching is performed within sector-year cells, where sectors are defined on 2-digit level of NACE rev. 2. *,**,*** indicate significance on the 10,5, and 1% significance level, respectively. a) N and N *treated* refers to the number of firms entering the matching process in the first stage. The number of firms entering the second stage depends on the number of nearest neighbors used in the first stage and on the availability of data on contemporaneous changes in the covariates. The number of treated (untreated) firms in the second stage from the estimation with $k = 1, 2, 3, 4, 5$ nearest neighbors equals 35,45,53,56,59 (35,61,94,120,149) for sales and 32,50,52,55,59 (32,67,99,126,159) for employment.

difference for sales, where coefficients from the uncorrected estimation turn out to be slightly smaller. Employment effects are hardly affected. Column (3) presents the treatment effect estimates obtained from the *regression-adjusted matching estimator* described in (7). Sales and employment effects turn out positive and significant. The coefficients are larger both in the case of sales and employment, in particular for small numbers of nearest neighbors. This could indicate that differences in the time trends of the covariates matter. However, it must also be taken into account that we lose a significant number of observations as the regression-adjusted matching estimator requires information on contemporaneous changes in the control variables which are not available for all firms in our quasi-experimental dataset. As the number of nearest neighbors increases, the coefficient estimates come much closer to those obtained from our preferred specification. Additionally, in column (4) we present results from propensity score matching, which, in the case of employment confirm the result from the Mahalanobis matching. In the case of sales we find significant treatment effects only for $k < 3$ nearest neighbors.

Summarizing, we find that, throughout our different specifications , Hermes guarantees have positive effects on sales and employment, which are statistically and economically significant. In our sample, treatment leads to an average increase in sales growth by about 3.9 to 4.8 and in employment growth by 2.5 to 3 percentage points, respectively, in the year a Hermes Guarantee is granted. Considering that the treated firms in our sample cover on average 6.6 percent of their sales by a guarantee, the magnitudes are plausible. The effects are also economically significant; we find that the average increase in sales induced by the grant of a guarantee amounts to 4.25 million Euro, corresponding to an average guarantee volume of 6.04 million Euro. Employment increases by 55 employees on average.[32]

Based on these results, we further check the robustness of our estimations with respect to a larger sample, a different level of sectoral disaggregation and the choice of matching variables. Table A.6 in the Appendix summarizes the results of the additional specifications. We perform the same matching procedure in a sample of 47,000 firms to increase the number of potential controls. Unfortunately, we lack information on exporting status of these firms. However, since the other matching variables, in particular, TFP, size measures, and the proxy for skill intensity, all correlate strongly with export status, we believe that this robustness check is sensible. We find that the results are robust in terms of significance and in terms of size to the sample composition (cp. columns (1) and (3)). Only for the case of firm sales performance do the effects turn out to be slightly smaller, dropping to a range of 3.5 to 4 percentage points and are insignificant for $k = 1$. We repeat the exercise now defining sector cells more restrictively on the 4-digit level of the NACE rev. 2 industry classification, which is feasible in the larger sample. The results presented in columns (2) and (4) are hardly affected.

Next, based on the original sample, we choose a larger set of matching variables by including pre-treatment averages of employment growth and TFP growth as they might potentially affect the selection into Hermes. The larger number of matching variables comes at the cost of a lower number of observations in both the group of treated and potential

[32]To quantify the effects, we use a sales (employment) weighted average of the estimated individual treatment effects obtained from matching with five nearest neighbors.

control firms. However, we find that results are not affected in terms of significance and in terms of size for larger number of nearest neighbors (cp. columns (5) and (6)). Using lagged values of the treatment variables instead of pre-treatment averages also does not change the results by much (columns (7) and (8)).

6.2 Treatment Effects on Additional Outcome Variables

In Table V we present estimated treatment effects of Hermes guarantees on different outcome variables, namely value added, value added per worker, the average wage (firm level wage bill divided by number of workers), and profits over sales (the EBIT/sales ratio). Except for the latter, all variables are in logs. Compared to sales or employment, these outcome variables are more problematic because they are constructed from balance-sheet positions reported by firms. In particular, value added poses problems since it can turn negative. By taking logs, these observations drop out so that the sample differs from the one used to compute treatment effects on sales or employment.

For all outcome variables and regardless of the number of nearest neighbors (k), we estimate positive treatment effects that are at maximum 6 percent. We find that treatment increases growth of value added by between 4.4 and 6.1 percentage points, while the growth of value added per worker goes up by between 2.7 and 3.0 percentage points.[33] These results suggest that Hermes guarantees boost value added by more than employment, the relative importance of employment being about a third. We further decompose value added into a wage and a profit component and find that the change in average wages increased by about 1.5 percentage points. We also find marginally significant positive effects on the profit sales ratio.

6.3 Assessment of the Conditional Independence Assumption

We assess the validity of the conditional independence assumption by means of pseudo treatments, i.e. we compute treatment effects on our variables in years where no treatment took place. As Imbens and Wooldridge (2009) point out, pseudo treatments cannot be considered a test of the conditional independence assumption. However, they can be used to rule out obvious violations of the assumption, such as significant differences in changes in the outcome variables between the group of treated and controls in periods where no treatment took place. The absence of pseudo treatment effects enhances our confidence that the real treatment effects are causal.

[33]These results suggest (e.g., for $k = 1$) that employment should go up by about 2 percent. This is almost exactly what we find in Table IV.

Table V: Treatment Effects on Additional Outcome Variables

# matches	Outcome variable: Δ ln Value added	Δ ln Value added per worker	Δ ln Average wage	Δ EBIT/sales
1	0.0599***	0.0329	0.0149*	0.0065
	(0.0193)	(0.0202)	(0.0082)	(0.0040)
2	0.0533***	0.0307*	0.0134*	0.0069*
	(0.0169)	(0.0179)	(0.0071)	(0.0038)
3	0.0509***	0.0296*	0.0152**	0.0043
	(0.0159)	(0.0167)	(0.0065)	(0.0035)
4	0.0436***	0.0244	0.0159**	0.0043
	(0.0151)	(0.0160)	(0.0065)	(0.0033)
5	0.0473***	0.0269*	0.0152**	0.0058*
	(0.0150)	(0.0158)	(0.0063)	(0.0033)
N	7223	6398	6851	7016
N treated	287	254	273	284

Notes: Treatment effects are estimated as changes in log outcomes in the year of the treatment, where treatment in t is defined as the change in treatment status from no treatment in $t-1$ to treatment in t. Matching variables are pre-treatment two-year averages of *TFP, skill, tangible assets, liquidity, employment, sales, sales growth, age, and total assets.* Matching is performed within sector-year cells, where sectors are defined on 2-digit level of NACE rev. 2. *,** ,*** indicate significance on the 10,5, and 1% significance level, respectively.

Table VI summarizes such placebo treatment effects on four lags and for future changes of our outcome variables sales and employment. We find that, with few exceptions, there are no significant differences between changes in the outcome variables in our groups of treated and control firms. Only for sales do we find that firms treated in t experienced a significantly different change in $t+2$ and in $t-3$. The estimated differences are *negative*, weakly significant and not robust across estimations with different numbers of nearest neighbors. Moreover, the further we move away from the time of the treatment, the less firms we observe in the groups of treated and untreated firms and hence, the less representative the estimates become. In $t+2$, for example, we observe only 35% of the firms treated in t and about 50% of the potential control firms. We therefore, do not overemphasize the placebo effect on sales in those other periods.

6.4 The Effect of Hermes Guarantees in the Financial Crisis 2008/2009

As discussed in Section 2, public export credit guarantees aim at mitigating frictions on financial markets that prevent otherwise profitable export business from being realized. A natural implication of this is that the effect of Hermes guarantees should be particularly strong in times of financial distress, when access to external finance is difficult. An exogenous shock like the recent financial crisis (2008/2009) offers the possibility to analyze this hypothesis. Comparing the treatment effect of Hermes guarantees during the financial crisis to the average

Table VI: Placebo Treatment Effects

time of placebo treatment	# matches 1	2	3	4	5	N	N treated
	Outcome variable: $\Delta \ln$ **Sales**						
t-4	-0.0074	-0.0128	-0.0142	-0.0152	-0.0102	3234	112
	(0.0224)	(0.0192)	(0.0189)	(0.0183)	(0.0176)		
t-3	-0.0293	-0.0285*	-0.0317**	-0.0237	-0.0255*	4535	172
	(0.0186)	(0.0157)	(0.0152)	(0.0147)	(0.0144)		
t-2	0.0146	-0.0021	-0.0018	0.0014	-0.0027	6420	244
	(0.0115)	(0.0103)	(0.0100)	(0.0098)	(0.0095)		
t-1	-0.0054	0.0034	0.0046	0.0035	0.0037	7580	305
	(0.0102)	(0.0088)	(0.0085)	(0.0083)	(0.0082)		
t	0.0469***	0.0483***	0.0467***	0.0387***	0.0437***	7090	289
	(0.0173)	(0.0154)	(0.0143)	(0.0135)	(0.0135)		
t+1	0.0135	-0.0006	0.0097)	0.0071	0.0012	5251	196
	(0.0248)	(0.0222)	(0.0205)	(0.0201)	(0.0197)		
t+2	-0.0657*	-0.0561*	-0.0503*	-0.0437*	-0.0391	3552	102
	(0.0342)	(0.0291)	(0.0265)	(0.0261)	(0.0251)		
t+3	0.0718	0.0510	0.0466	0.0413	0.0379	2323	56
	(0.0536)	(0.0361)	(0.0317)	(0.0298)	(0.0298)		
t+4	-0.0050	0.0134	0.0121	0.0108	0.0132	1431	35
	(0.0496)	(0.0409)	(0.0417)	(0.0400	(0.0375)		
	Outcome variable: $\Delta \ln$ **Employment**						
t-4	-0.0093	-0.0155	-0.0146	-0.0129	-0.0102	1624	66
	(0.0169)	(0.0119)	(0.0114)	(0.0111)	(0.0111)		
t-3	-0.0059	0.0069	0.0096	0.0112	0.0098	2682	122
	(0.0117)	(0.0112)	(0.0103)	(0.0099)	(0.0100)		
t-2	0.0046	-0.0002	0.0002	0.0001	0.0032	4429	185
	(0.0084)	(0.0080)	(0.0077)	(0.0075)	(0.0075)		
t-1	0.0122	0.0067	0.0088	0.0100	0.0088	6123	252
	(0.0107)	(0.0093)	(0.0088)	(0.0087)	(0.0083)		
t	0.0253***	0.0273***	0.0303***	0.0280***	0.0277***	7053	280
	(0.0095)	(0.0081)	(0.0077)	(0.0077)	(0.0074)		
t+1	-0.0045	-0.0057	0.0022	0.0044	0.0058	5304	172
	(0.0103)	(0.0097)	(0.0097)	(0.0099)	(0.0097)		
t+2	-0.0254	-0.0168	-0.0126	-0.0109	-0.0105	3670	102
	(0.0165)	(0.0134)	(0.0135)	(0.0131)	(0.0132)		
t+3	-0.0038	-0.0061	-0.0081	-0.0013	0.0011	2381	62
	(0.0255)	(0.0211)	(0.0196)	(0.0189)	(0.0190)		
t+4	0.0136	0.0271	0.0171	0.0184	0.0111	1429	38
	(0.0219)	(0.0212)	(0.0204)	(0.0194)	(0.0195)		

Notes: Treatment effects are estimated as changes in (log) outcomes between τ and $\tau-1$ for $\tau = t-4, ..., t, ..., t+4$ based on pre-treatment averages of the matching variables in t using the bias corrected matching estimator described in (6). Matching variables are pre-treatment averages of *TFP, labor productivity, skill, tangible assets, liquidity, employment, sales, value added, age, total assets*. Matching is performed within sector-year cells, where sectors are defined on the 2-digit level of NACE rev. 2. *,** ,*** indicate significance on the 10,5, and 1% significance level, respectively.

effect in years prior to or after the financial crisis (2000-2007, 2010), we find that the effect of Hermes guarantees on both outcome variables was significantly larger during the crisis. Table VII provides the results. While in the years of no financial crisis treated firms on average had 3 percentage points higher sales growth, the additional sales growth during the financial crisis is estimated to lie between 5.5 and 9 percentage points. For employment, we find that the effect in years of no crisis was between 2 and 2.6 percentage points, while during the crisis it ranged between 2.8 and 3.5 percentage points. Of course, this comparison relies on the assumption that the average treatment itself was constant over the two periods. Comparing average treatment volumes per firm in our sub-samples shows that during the years 2008/2009 the average treatment volume per firm was not significantly different (see Figure A.1 in the Appendix).[34] This implies that we cannot attribute the larger effects during the crisis simply to higher treatment volumes on the firm level. Hence, our results confirm the hypothesis that Hermes guarantees affect firm performance at least partly by mitigating financial constraints.

Table VII: Financial Crisis 2008/09

	Outcome variable: $\Delta \ln$ Sales			Outcome variable: $\Delta \ln$ Employment		
# matches	total	no crisis	crisis	total	no crisis	crisis
1	0.0469***	0.0246	0.0919***	0.0253***	0.0195*	0.0327*
	(0.0173)	(0.0173)	(0.0335)	(0.0095)	(0.0100)	(0.0176)
2	0.0483***	0.0319*	0.0672**	0.0273***	0.0234***	0.0290*
	(0.0154)	(0.0172)	(0.0276)	(0.0081)	(0.0088)	(0.0152)
3	0.0467***	0.0318*	0.0619**	0.0303***	0.0281***	0.0284**
	(0.0143)	(0.0163)	(0.0250)	(0.0077)	(0.0087)	(0.0137)
4	0.0387***	0.0288*	0.0542**	0.0280***	0.0253***	0.0345**
	(0.0135)	(0.0156)	(0.0241)	(0.0077)	(0.0086)	(0.0141)
5	0.0437***	0.0305**	0.0625**	0.0277***	0.0264***	0.0341**
	(0.0135)	(0.0155)	(0.0242)	(0.0074)	(0.0084)	(0.0134)
N	7090	4396	2694	7053	4102	2951
N treated	289	173	116	280	160	120

Notes: Treatment effects are estimated as changes in log outcomes in the year of the treatment, where treatment in t is defined as the change in treatment status from no treatment in $t-1$ to treatment in t. Matching variables are pre-treatment two-year averages of *TFP, skill, tangible assets, liquidity, employment, sales, sales growth, age, and total assets.* Matching is performed within sector-year cells, where sectors are defined on 2-digit level of NACE rev. 2. Columns (1)&(4) contains the average effect over all years, in columns (2)&(5) only the years 2000-2007,2010 are considered and columns (3)&(6) considers only treatments during 2008 and 2009. *,**,*** indicate significance on the 10,5, and 1% significance level, respectively.

6.5 SATE vs SATT: How Important is Self-selection?

In Table VIII we present the estimation results for the SATE on sales and employment and compare it to the SATTs in Table IV to assess the importance of self-selection. We find that the SATE, which is a weighted average of the treatment effect on the treated and the

[34]This suggest that the increase in the total amount of guarantees granted took place at the extensive rather than the intensive margin

treatment effect on the untreated, is smaller than the SATT. This implies that the average treatment effect on the untreated is smaller than that on the treated, thus providing support for the selection hypothesis. While a firm with the average characteristics of the population of Hermes firms experienced additional sales growth of about 4.5 percentage points, a firm with the average characteristics of our sample would have had additional sales growth of about 3 percentage points. The estimated employment effects lie in a similar range. Regarding the lower precision of the ATE estimate, we note that it is based on two estimated counterfactuals rather than one. Besides the counterfactual outcome for the treated group it furthermore requires estimating the counterfactual for the group of untreated firms. And since the number of treated firms is small relative to the number of non-treated firms, finding good matches for all untreated firms is significantly harder. The last column of Table VIII presents estimates of the ATE obtained from linear estimation in first differences. We find fairly similar effects. Hence, there is no evidence that the coefficient estimates obtained from linear models suffer from an additional bias due to the assumed linearity or unbalancedness of the sample.

Table VIII: Sample Average Treatment Effects

	Bias-corrected Mahalanobis matching # matches 1	2	3	4	5	OLS FD
Outcome variable: $\Delta \ln$ Sales						
SATE	0.0258	0.0309*	0.0293*	0.0218	0.0182	0.0358**
	(0.0174)	(0.0165)	(0.0155)	(0.0150)	(0.0147)	(0.0127)
Outcome variable: $\Delta \ln$ Employment						
SATE	0.0272**	0.0249**	0.0253***	0.0257***	0.0262***	0.0289**
	(0.0108)	(0.0099)	(0.0092)	(0.0088)	(0.0086)	(0.0102)

Notes: Treatment effects are estimated as changes in log outcomes in the year of the treatment, where treatment in t is defined as the change in treatment status from no treatment in $t-1$ to treatment in t. Matching variables are pre-treatment two-year averages of *TFP, skill, tangible assets, liquidity, employment, sales, sales growth, age, and total assets.* Matching is performed within sector-year cells, where sectors are defined on 2-digit level of NACE rev. 2. *,** ,*** indicate significance on the 10,5, and 1% significance level, respectively. No. of observations 7,090 and 7,053, no. of treated 289 and 280 for sales and employment, respectively.

7 Conclusion

Almost all governments in the world offer public export credit guarantees to their exporters. They justify those programs by assuming that private financial markets fail to provide insurance for long-term and large-scale export projects to certain markets. This disables potential exporters to refinance their export business so that projects with positive net present value remain unrealized. In this paper, we exploit data from the German export credit insurance scheme (Hermes) to test whether firms that have access to publicly guarantees really expand their activity instead of only substituting subsidized insurance for private insurance or reallocating their sales portfolio to more risky markets. The key challenge is to create a quasi-experimental setup, so that observationally identical firms either have access to the program or not.

Earlier empirical work on export credit guarantees used industry-level data and linear gravity-type econometric models. We have the universe of all firms that have obtained Hermes guarantees from 2000 to 2010 and combine these data with the Amadeus data set for Germany. Rather than studying the effect of Hermes on exports, we look at total sales and employment, thereby focusing on the overall size of firms' operations. We construct our quasi-experimental data set using matching methods and conduct a differences-in-differences analysis to account for unobserved heterogeneity with respect to firms' selection into the Hermes program.

We find that firms which use Hermes guarantees experience a significant additional increase in employment and sales compared to untreated firms. The additional sales growth due to a provision of a Hermes Guarantee ranges between 4 to 4.5 percentage points in the year of the grant, additional employment growth of treated firms amounts to about 2.5 to 3 percentage points. Our results are robust across a wide range of specifications. Furthermore, we also find that the effect of export credit guarantees was larger during the financial crisis. This supports the hypothesis that credit guarantees work through the mitigation of financial constraints and help firms to expand the scale of activity.

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A Appendix

Table A.1: Country Risk Classifications of the Participants to the OECD Arrangement

Risk Category	Country Name
0	Australia, Austria, Belgium, Canada, Cyprus, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Japan, Korea, Luxembourg, Malta, Netherlands, New Zealand, Norway, Portugal, Singapore, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, United Kingdom, United States
1	Chinese Taipei, Hong Kong (China)
2	Bahrain, Botswana, Brunei, Chile, China, Kuwait, Malaysia, Oman, Poland, Qatar, Saudi Arabia, Trinidad and Tobago, United Arab Emirates
3	Algeria, Bahamas, Brazil, Costa Rica, Estonia, India, Israel, Lithuania, Mauritius, Mexico, Morocco, Namibia, Panama, Peru, Russian Federation, South Africa, Thailand, Tunisia
4	Bulgaria, Colombia, Egypt, El Salvador, Kazakhstan, Latvia, Philippines, Romania, Turkey, Uruguay
5	Azerbaijan, Cape Verde, Croatia, Dominican Republic, Guatemala, Indonesia, Jordan, Lesotho, Macedonia (FYROM), Netherlands Antilles, Papua New Guinea, Paraguay, Viet Nam
6	Albania, Angola, Antigua and Barbuda, Armenia, Bangladesh, Belize, Benin, Cambodia, Cameroon, Gabon, Georgia, Ghana, Honduras, Iran, Jamaica, Kenya, Libya, Madagascar, Mali, Mongolia, Montenegro, Mozambique, Nigeria, Senegal, Sri Lanka, Swaziland, Syria, Tanzania, Turkmenistan, Uganda, Uzbekistan, Yemen, Zambia
7	Afghanistan, Argentina, Belarus, Bolivia, Bosnia and Herzegovina, Burkina Faso, Burundi, Central African Republic, Chad, Congo, Congo (Dem. Rep.), Côte d'Ivoire, Cuba, Ecuador, Equatorial Guinea, Eritrea, Ethiopia, Gambia, Guinea, Guinea-Bissau, Haiti, Iraq, Korea (Dem. Republic, North), Kyrgyzstan, Laos, Lebanon, Liberia, Malawi, Maldives, Mauritania, Moldova, Myanmar, Nepal, Nicaragua, Niger, Pakistan, Rwanda, Serbia, Sierra Leone, Somalia, Sudan, Tajikistan, Togo, Ukraine, Venezuela, Zimbabwe
Not Classified	American Samoa, Andorra, Aruba, Barbados, Bermuda, Bhutan, Cayman Islands, Channel Islands, Comoros, Djibouti, Dominica, Faroe Islands, Fiji, French Polynesia, Greenland, Grenada, Guam, Guyana, Isle of Man, Kiribati, Kosovo, Liechtenstein, Macao, Marshall Islands, Mayotte, Micronesia, Monaco, New Caledonia, Northern Mariana Islands, Palau, Puerto Rico, Saint Kitts and Nevis, Saint Lucia, Saint Vincent and the Grenadines, Samoa, San Marino, Sao Tome and Principe, Seychelles, Salomon Islands, Suriname, Timor-Leste, Tonga, U.S. Virgin Islands, Vanuatu, West Bank and Gaza

Notes: Within the OECD *Arrangement* the country risk classification system uses a scale of eight risk categories (0-7). Accordingly, the country risk classification of high income OECD countries and other high income Euro-zone countries is category 0. The country risk classifications of all other countries are determined through the application of rules which account for the payment experience of the participants, the financial situation and the economic situation. Countries with the highest risk are classified in category 7. The listed classifications prevailed during the period between July 3 and October 30, 2009.

Table A.2: Observed Sectors - NACE rev. 2 Classification

1	Crop and animal production, hunting and related service activities
2	Forestry and logging
3	Fishing and aquaculture
5	Mining of coal and lignite
6	Extraction of crude petroleum and natural gas
7	Mining of metal ores
8	Other mining and quarrying
9	Mining support service activities
10	Manufacture of food products
11	Manufacture of beverages
12	Manufacture of tobacco products
13	Manufacture of textiles
14	Manufacture of wearing apparel
15	Manufacture of leather and related products
16	Manufacture of wood and of products of wood and cork, except furniture; manufacture of articles of straw and plaiting materials
17	Manufacture of paper and paper products
18	Printing and reproduction of recorded media
19	Manufacture of coke and refined petroleum products
20	Manufacture of chemicals and chemical products
21	Manufacture of basic pharmaceutical products and pharmaceutical preparations
22	Manufacture of rubber and plastic products
23	Manufacture of other non-metallic mineral products
24	Manufacture of basic metals
25	Manufacture of fabricated metal products, except machinery and equipment
26	Manufacture of computer, electronic and optical products
27	Manufacture of electrical equipment
28	Manufacture of machinery and equipment n.e.c.
29	Manufacture of motor vehicles, trailers and semi-trailers
30	Manufacture of other transport equipment
31	Manufacture of furniture
32	Other manufacturing
33	Repair and installation of machinery and equipment
35	Electricity, gas, steam and air conditioning supply
36	Water collection, treatment and supply
37	Sewerage
38	Waste collection, treatment and disposal activities; materials recovery
39	Remediation activities and other waste management services
41	Construction of buildings
42	Civil engineering
43	Specialised construction activities
45	Wholesale and retail trade and repair of motor vehicles and motorcycles
46	Wholesale trade, except of motor vehicles and motorcycles
47	Retail trade, except of motor vehicles and motorcycles
49	Land transport and transport via pipelines
50	Water transport
51	Air transport
52	Warehousing and support activities for transportation
53	Postal and courier activities
55	Accommodation
56	Food and beverage service activities
58	Publishing activities
59	Motion picture, video and television programme production, sound recording and music publishing activities
60	Programming and broadcasting activities
61	Telecommunications
62	Computer programming, consultancy and related activities
63	Information service activities
64	Financial service activities, except insurance and pension funding
65	Insurance, reinsurance and pension funding, except compulsory social security
66	Activities auxiliary to financial services and insurance activities
68	Real estate activities
69	Legal and accounting activities
70	Activities of head offices; management consultancy activities
71	Architectural and engineering activities; technical testing and analysis
72	Scientific research and development
73	Advertising and market research
74	Other professional, scientific and technical activities
75	Veterinary activities
77	Rental and leasing activities
78	Employment activitie
79	Travel agency, tour operator and other reservation service and related activities
80	Security and investigation activities
81	Services to buildings and landscape activities
82	Office administrative, office support and other business support activities
84	Public administration and defence; compulsory social security
85	Education
86	Human health activities
87	Residential care activities
88	Social work activities without accommodation
90	Creative, arts and entertainment activities
91	Libraries, archives, museums and other cultural activities
92	Gambling and betting activities
93	Sports activities and amusement and recreation activities
94	Activities of membership organisations
95	Repair of computers and personal and household goods
96	Other personal service activities
97	Activities of households as employers of domestic personnel
98	Undifferentiated goods- and services-producing activities of private households for own use
99	Activities of extraterritorial organisations and bodies

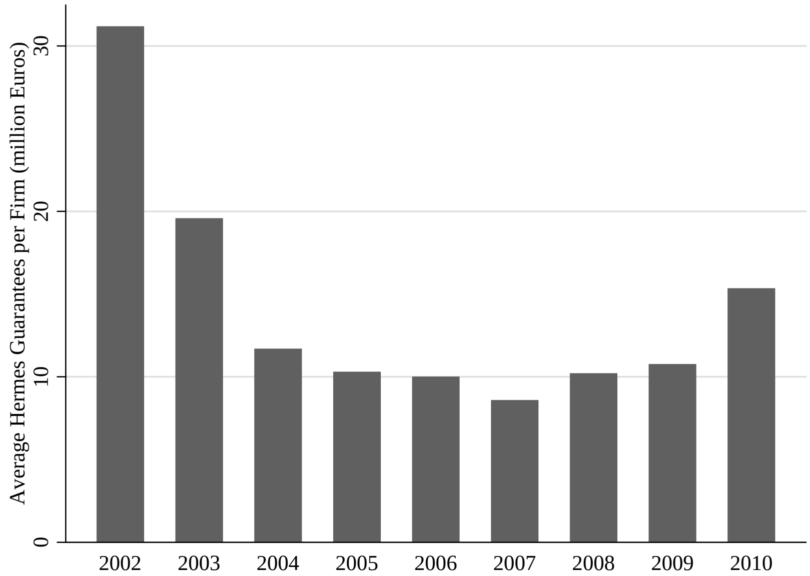
Notes: Based on correspondence tables from Eurostat (RAMON-Database) it is possible to transform export data from HS-6 into a NACE Rev. 2 sectoral classification. Euler Hermes' internal classification of EZDs permits the allocation of export guarantees to the listed 99 sectors. Although our data is restricted to manufacturing the transformation of our data based on official correspondence tables leads to some observations in the service sector. n.e.c. stands for not elsewhere classified.

Table A.3: Summary Statistics of Exporter Sample

Variable	Description	Obs	Mean	Std. Dev.	Min	Max
Age (yrs.)		35141	34	32.46	0	164
Sales (Euro, th.)		30020	196718	2241608	0	1.50e+08
Employment	*number of workers*	19608	9963124	8961	1	370684
Total assets (Euro, th.)		35551	144243	2396518	0	1.87e+08
Tangibles (Euro, th.)	*tangible assets*	28559	39689	805709	0	6.45e+07
Wage bill (Euro, th.)		26430	39037	400795	0	2.43e+07
Average wage (Euro, th.)	*wage bill/workers*	18171	52.76	51.99	.04	4468085
ln TFP	*ln of total factor productivity*	9055	5.14	.80	1.51	10.88
Liquidity	*current liabilities/current assets*	34705	.57	11.60	0	1909368
Current Assets (Euro, th.)		35551	79852	1362905	0	1.10e+08
Current Liabilities (Euro, th.)		34708	40155	764302	0	7.13e+07
APG (Euro)	*Guarantees of type "Ausfuhrpauschalgewährleistungen"*	35551	697991	8980487	-1.38e+07	6.45e+08
REV (Euro)	*Guarantees of type "Revolvierende Deckungen"*	35551	38998	1260691	0	1.46e+08
EZD (Euro)	*Guarantees of type "Einzeldeckungen"*	35551	740862	1.91e+07	0	1.92e+09
Hermes	*Hermes Status (0,1)*	35551	.14	.35	0	1
Year		35551	2006.42	2.32	2000	2010

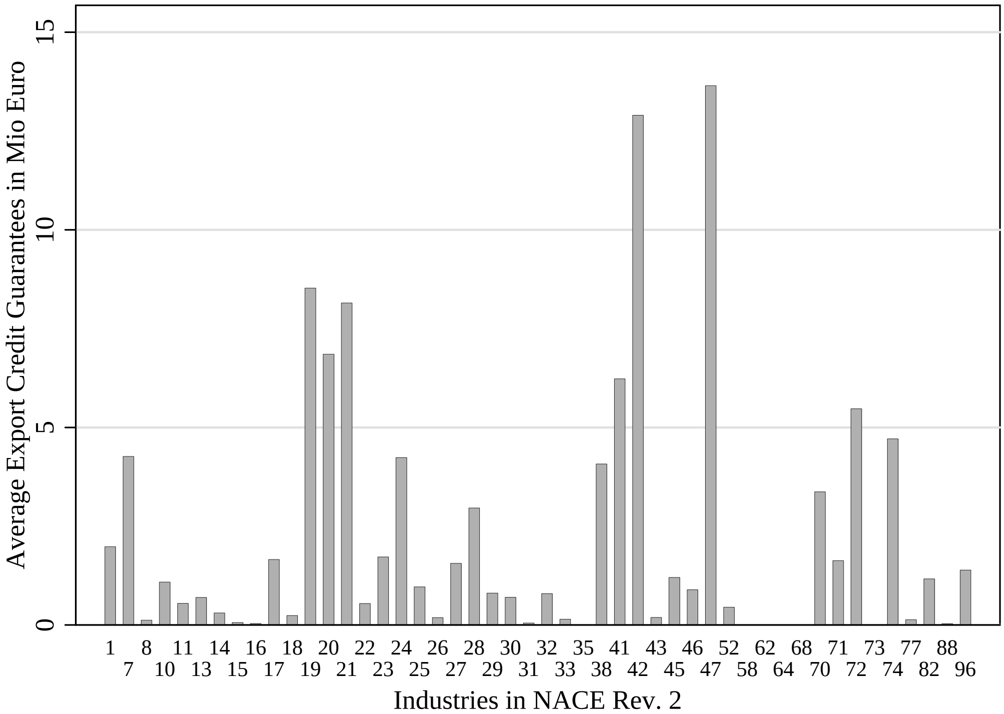
Notes: Firm specific accounting data stems from the Amadeus database provided by Bureau von Dijk Electronic Publishing GmbH. Hermes information is provided by Euler Hermes.

Figure A.1: Average Hermes Guarantee claims of firms across years



Notes: Columns depict average Hermes guarantees claims of firms per year in our estimation sample. Guarantees include all three major export guarantee instruments which are Einzeldeckungen (EZD), Ausfuhrpauschalgewährleistungen (APG), and revolving guarantees (REV).

Figure A.2: Average Hermes guarantees of firms across sectors - over all years



Notes: Columns depict average Hermes guarantees of firms in different industries in our estimation sample. Industry classification is in NACE rev. 2. Guarantees include all three major export guarantee instruments which are Einzeldeckungen (EZD), Ausfuhrpauschalgewährleistungen (APG), and revolving guarantees (REV). In contrast to aggregate data, export credit guarantees appear in more sectors compared to figure II, since through our firm level data we can allocate APG and REV to the respective sectors.

Table A.4: Association of Matching Variables with Treatment Status (Probit Estimations)

Dep. variable	Hermes (1)	Hermes (2)	Δ Hermes (3)	Δ Hermes (4)
ln Employment	0.0777*	0.0188	0.403***	0.307***
	(.0448)	(.0489)	(.0752)	(.0859)
ln sales	-0.160**	-0.0643	-0.509***	-0.381***
	(.0701)	(.0764)	(.103)	(.122)
ln Age	-0.0887***	-0.107***	-0.116***	-0.126***
	(.0143)	(.0151)	(.0264)	(.0295)
ln Liquidity	0.0124	0.00695	0.0541*	0.0641**
	(.0132)	(.0138)	(.0287)	(.032)
ln Skill	0.159***	0.0438	0.426***	0.223*
	(.0546)	(.0544)	(.0991)	(.122)
ln Tangibles	-0.0430***	-0.0395***	-0.00472	0.00154
	(.0138)	(.0147)	(.0227)	(.0261)
ln TFP	0.185***	0.146**	0.406***	0.377***
	(.0583)	(.0628)	(.0923)	(.106)
ln Total assets	0.321***	0.307***	0.108**	0.0902
	(.0352)	(.0384)	(.0544)	(.0657)
Constant	-3.282***	-2.430**	-2.887***	-0.656
	(.19)	(1.08)	(.328)	(1.04)
Sector/Year dummies	no	yes	no	yes
N	8731	8565	8731	5718
Pseudo R^2	0.0620	0.109	0.0314	0.0722
χ^2	625.0	1124.6	80.68	160.7

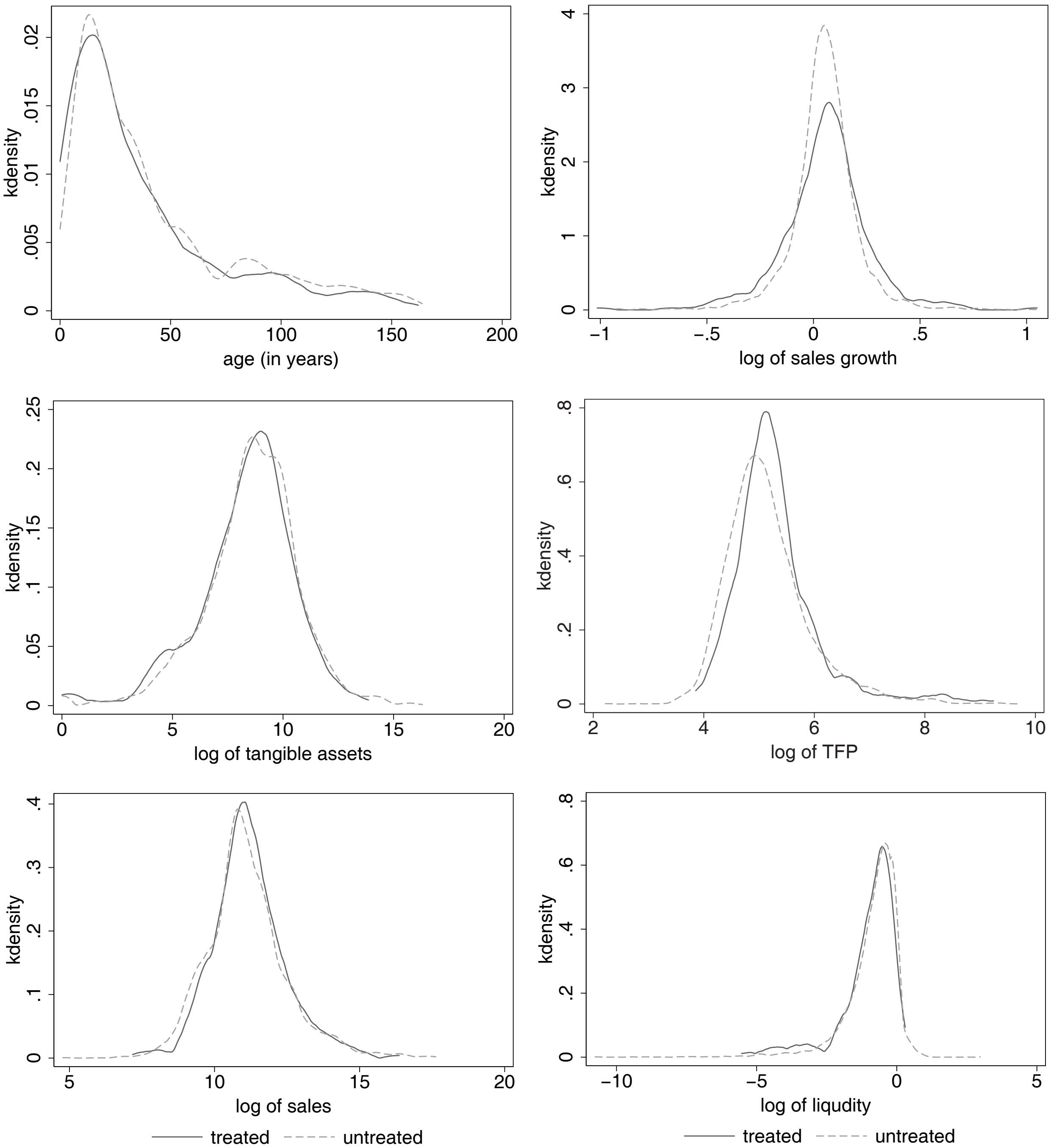
Notes: Standard errors in parentheses. Significance levels denoted by: * p<0.1, ** p<0.05, *** p<0.01. The first dependent variable *Hermes* is an indicator for whether a firm received Hermes guarantees at least once between 2000 and 2010. The second dependent variable Δ *Hermes* indicates a change in the treatment status. It equals one in t if the firm changes from no treatment in $t-1$ to treatment in period t and zero otherwise.

Table A.5: Pairwise Correlation of Matching Variables and Treatment Status

	ln Age	ln Employment	ln Sales	ln Liquidity	ln Skill	ln Tangibles	ln TFP	ln Total assets	Hermes
ln Age	1.0000								
ln Employment	0.2259*	1.0000							
ln Sales	0.2277*	0.8459*	1.0000						
ln Liquidity	-0.0617*	-0.0769*	-0.0913*	1.0000					
ln Skill	0.0054	-0.0343*	0.2553*	-0.0559*	1.0000				
ln Tangibles	0.2539*	0.7743*	0.7645*	-0.0745*	0.0123	1.0000			
ln TFP	-0.1012*	-0.1483*	0.3648*	0.0314*	0.4296*	-0.2204*	1.0000		
ln Total assets	0.2198*	0.8572*	0.9417*	-0.1096*	0.2525*	0.8080*	0.1681*	1.0000	
Hermes	-0.0389*	0.1484*	0.2728*	-0.0203*	0.1258*	0.1500*	0.1313*	0.2589*	1.0000

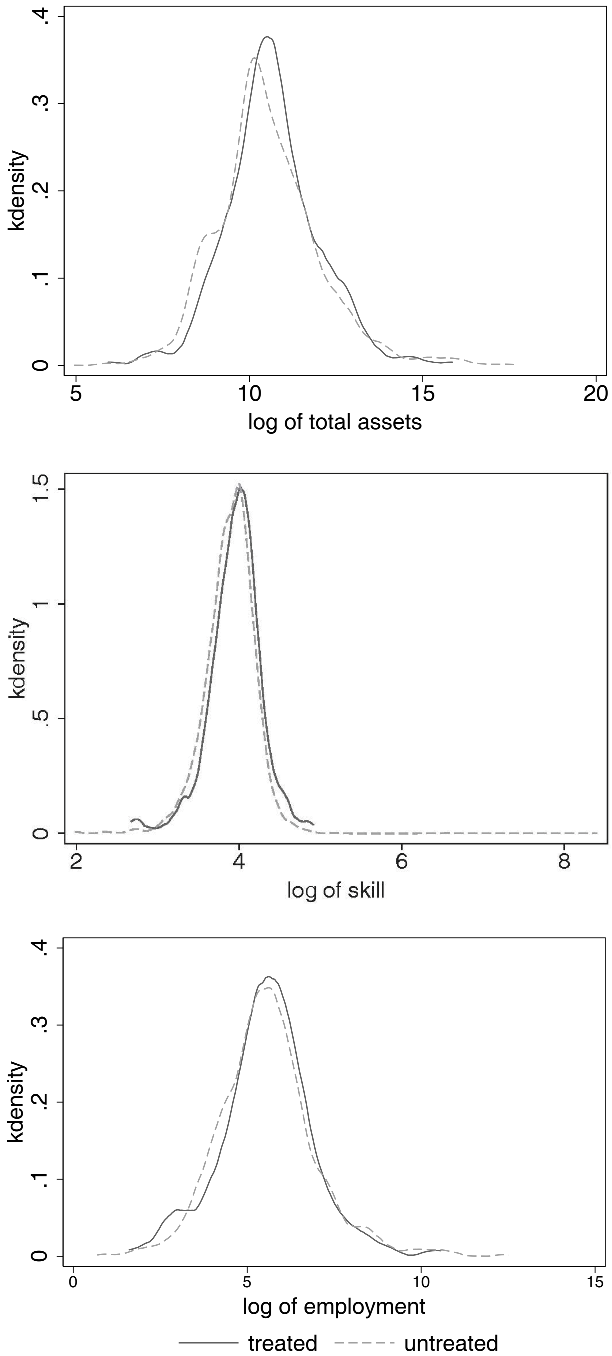
Notes: A * indicates significance at the level of 0.01. *Hermes* (0,1) indicates whether the firm received Hermes guarantees at least once between 2000 and 2010.

Figure A.3: Overlap before matching



Notes: This graph illustrates the overlap properties of the main matching variables before matching. Each panel depicts Epanechnikov kernel density functions of one matching variable for treated firms (i.e. firms that received export credit guarantees) and untreated firms (i.e. firms that never received Hermes guarantees) for the period 2000-2010. For sales growth, TFP, liquidity, sales, and tangible assets we use two year averages before the treatment year.

Overlap before matching



Notes: This graph illustrates the overlap properties of the main matching variables before matching. Each panel depicts Epanechnikov kernel density functions of one matching variable for treated firms (i.e. firms that received export credit guarantees) and untreated firms (i.e. firms that never received Hermes guarantees) for the period 2000-2010. For employment, skill and total assets we use two year averages before the treatment year.

Table A.6: Robustness of the SATT estimates

	full sample				tfp, employment growth		lagged matching variables	
	Δ ln Sales		Δ ln Employment		Δ ln Sales	Δ ln Employment	Δ ln Sales	Δ ln Employment
# matches	NACE 2	NACE 4	NACE 2	NACE 4	NACE 2	NACE 2	NACE 2	NACE 2
1	0.0273	0.0203	0.0334***	0.0266**	0.0654***	0.0294***	0.0546**	0.0394***
	(0.0188)	(0.0188)	(0.0120)	(0.0107)	(0.0200)	(0.0108)	(0.0212)	(0.0107)
2	0.0352**	0.0288*	0.0302***	0.0215**	0.0668***	0.0327***	0.0442**	0.0335***
	(0.0169)	(0.0169)	(0.0099)	(0.0100)	(0.0179)	(0.0096)	(0.0177)	(0.0096)
3	0.0354**	0.0323**	0.0279***	0.0259***	0.0493***	0.0303***	0.0436***	0.0314***
	(0.0159)	(0.0158)	(0.0090)	(0.0089)	(0.0167)	(0.0089)	(0.0160)	(0.0089)
4	0.0350**	0.0361**	0.0303***	0.0287***	0.0459***	0.0313***	0.0389**	0.0322***
	(0.0150)	(0.0146)	(0.0086)	(0.0084)	(0.0158)	(0.0088)	(0.0153)	(0.0086)
5	0.0337**	0.0361**	0.0289***	0.0276***	0.0388**	0.0302***	0.0340**	0.0343***
	(0.0145)	(0.0143)	(0.0085)	(0.0083)	(0.0158)	(0.0086)	(0.0150)	(0.0085)
N	46374	46374	46258	46258	5109	5128	5987	5990
N treated	286	286	278	278	216	215	226	226

Notes: Treatment effects are estimated as changes in log outcomes between t and $t-1$. Treatment is defined as the change in treatment status from no treatment in $t-1$ to treatment in t. Matching variables are pre-treatment averages of *tfp, skill, tangible assets, liquidity, employment, sales, sales growth, age, total assets*. In columns 5 and 6 *tfp growth* and *employment growth* are added as additional matching variables. In columns 7 and 8 lagged matching is based on lagged values rather than on pre-treatment averages of the matching variables. Matching is performed within sector-year cells. *NACE 2* (*NACE 4*) indicates matching in sector cells defined on the 2- (4-) digit level of the Nace Rev 2. industry classification, respectively. *,** ,*** indicates significance on the 10,5, and 1% significance level, respectively.