ÖREBRO UNIVERSITY
SCHOOL OF BUSINESS

WORKING PAPER

12/2018

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Economics

ISSN 1403-0586

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Mitigating Information Frictions in Trade: Evidence from Export Credit Guarantees*

Natasha Agarwal Magnus Lodefalk† Aili Tang Sofia Tano Zheng Wang

November 19, 2020

Abstract

Information frictions make foreign trade risky. Therefore, many countries offer export credit guarantees that insure export transactions against buyers' default. We investigate the causal effects of guarantees on firm performance. To overcome selection bias, we employ a quasi-experimental design and extraordinarily rich Swedish register data on guarantees, firms and trade. We arrive at three major findings. First, guarantees increase firm exports to the foreign market for which they were issued and elsewhere. The effect on exports elsewhere diminishes with the distance from the intended market. Second, guarantees do not generally impact jobs, value added or productivity. Third, guarantees affect firms heterogeneously. Exports increase the most for small and service firms and in the exports to small foreign firms. These firms are expected to be particularly disadvantaged by information frictions in trade. Guarantees also increase value added and jobs but only for inexperienced users and low-scale exports, respectively. Overall, these patterns indicate that guarantees mitigate information frictions in trade. In terms of the detailed mechanisms, the results suggest that guarantees primarily address the default risk in exports and secondarily ease liquidity constraints.

Keywords: Information frictions; Buyers' default; Liquidity constraints; Export credit guarantees; Trade; Firm performance

JEL Codes: D22, D82, F14, G28, G32, H81, L25.

*We thank Holger Breinlich, Holger Görg, Daniel Halvarsson, Dan Johansson, Henrik Jordahl, Anders Kärnä, Hildegunn Kyvik-Nordås, Patrick Nimander, Håkan Nordström, Maria Persson, Natália Pimenta Monteiro, Johanna Rickne, Fredrik Sjöholm and Maurizio Zanardi for their insightful feedback. We also thank the numerous participants in reference groups/seminars/workshops/conferences for their helpful comments and the Swedish Export Credit Agency for supplying export credit guarantee data. We are responsible for all views expressed and any remaining errors. Lodefalk and Tang acknowledge financial support from Growth Analysis and the Torsten Söderberg Research Foundation (grant no. E38/16).

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1. Introduction

Information frictions constitute substantial barriers for foreign trade that are on par with transport costs or tariffs (Rauch and Trindade, 2002, Head and Mayer, 2013, Allen, 2014, Steinwender, 2018). Exporters need to search for information about foreign markets and buyers. Acquiring this information is costly but likely insufficient for gauging if a foreign buyer will default on the payment.[1] With such frictions, exporters may also find it difficult to obtain the external financing that may be needed for exports.[2] Accordingly, exporters may hesitate to extend credit to foreign buyers, which reduces the competitiveness of exporting firms abroad. To enable firms to overcome these information frictions, governments may offer export credit guarantees to firms. This paper investigates if guarantees reduce information frictions in trade by estimating the causal effects of guarantees on several aspects of firm performance (trade, jobs, value added and labour productivity).

Government-backed export credit guarantees are prevalent but relatively underexplored academically. After World War I, several countries independently established an export credit agency (Dietrich, 1935). By acting as a "guarantor of last resort", the institution's purpose was to facilitate trade, and the ultimate aim was to promote exports and jobs. Currently, such agencies exist in many countries. They offer guarantees to firms for a fee. In 2017, they provided approximately 1 trillion USD of new guarantees (Berne Union, 2018). Despite the substantial amounts and the risks of distorting markets, research is relatively scarce on the effects of these institutions and the underlying mechanisms.[3] In fact, less than a dozen

[1] Locally, communities can reduce the risk of strategic default by monitoring and punishing opportunistic behaviour (Ostrom, 1990, Dixit, 2003), while nationally, legal systems discourage such behaviour. Internationally, however, contract enforcement mechanisms are weaker, and political risks add to the default risk. Therefore, throughout most of human history, traders themselves have often travelled to trade (Greif, 2006).

[2] Exporters face binding liquidity constraints in funding exports (Manova, 2013). They may therefore need assistance from financial intermediaries (e.g., Amiti and Weinstein, 2011, Paravisini *et al.*, 2014).

[3] Funatsu (1986) and Heiland and Yalcin (2020) provide theoretical models with guarantees that focus on foreign market risks and financial market constraints, respectively. Dixit (2003) provide a more general framework, while James (2011) provide a critical perspective that examines the US Export-Import (EXIM) Bank.

countries have been studied.[4]

Most empirical studies on export credit guarantees have used country- or industry-level data on guarantees and trade, in dearth of corresponding micro-level data, which raises the issue of omitted variable bias (e.g., Abraham and Dewit, 2000, Egger and Url, 2006, Auboin and Engemann, 2014, Freund, 2016, Agarwal and Wang, 2018). Studies commonly find a positive association between guarantees and exports. The association is stronger for the industries that are more dependent on external financing and for trade with riskier or less financially developed countries. The few micro-level studies find a positive association between guarantees and firm exports (Badinger and Url, 2013, Heiland and Yalcin, 2020), sales and jobs (Felbermayr *et al.*, 2012). These studies have data on guarantees combined with cross-sectional Austrian export data (Badinger and Url, 2013), survey sample data on German exports (Heiland and Yalcin, 2020) or sales and job data for predominantly larger German firms (Felbermayr *et al.*, 2012).[5] However, the paramount challenge in the literature is how to identify the causal effects when the treatment is self-selected: firms seek guarantees, which are deal specific, only if they expect to close these deals.

Our paper makes several contributions to this literature. To investigate the causal effects of guarantees, we attempt to cut the Gordian knot of self-selection regarding the use of guarantees. Accordingly, we introduce a novelty into this literature – a quasi-natural experiment. In 2012, the Swedish governmental export credit agency began to regularly approach firms *en masse* through surface mail. These direct marketing campaigns were designed to increase awareness about the guarantees of the agency. A specific feature of the campaigns was that

[4]Empirical studies have covered, e.g., Australia, Austria, Belgium, The Czech Republic, Germany, Japan, Turkey and the US. In addition, there are a few multi-country studies on various aspects of export financing, e.g., on the guarantees of OECD countries (Baltensperger and Herger, 2009) and on the export financing of four eastern European countries (Janda *et al.*, 2013).

[5]Badinger and Url (2013) study the export effects for a small cross-section of firms in Austria in 2008 by using an instrumental variable approach, Heiland and Yalcin (2020) study the within-firm impact on firms' assessment of having a "larger than usual" stock of foreign orders by using German guarantee panel data (2000-2010), commercially available data for predominantly larger firms and survey data, and Felbermayr *et al.* (2012) study sales and jobs effects by using a matching and difference-in-difference estimator after merging German guarantee panel data with commercially available data for predominantly larger firms (2000-2010).

they targeted exporting firms with a workforce below a threshold (250 workers). Therefore, firms' right below the threshold were more likely to acquire guarantees (a 44-percentage-point increase). We exploit this feature to estimate the causal effects of guarantees on firm performance, and we employ a fuzzy regression discontinuity design (FRDD).[6] For causal inference, it is important that our quasi-natural experimental design eliminates concerns about particular firms being selected to receive this mail. Fortunately, the Swedish direct marketing campaigns were directed at all exporting firms with less than 250 workers. Thus, the campaigns were general rather than targeting the most likely or desirable prospects. In effect, the campaigns approached 70 percent of all exporting small and medium-sized enterprises (SMEs) and virtually all of the goods-exporting SMEs. Most of these firms are only marginal exporters. To our knowledge, this is the first study to directly address the challenging issue of self-selection in the estimation of the causal effects of guarantees.

Second, we use a fuzzy regression discontinuity design while employing the most detailed and comprehensive data thus far in this literature. We combine granular and longitudinal transaction-level data on the universe of government-backed guarantees in Sweden, from 2000-2015, with exhaustive administrative panel data on firms and their bilateral trade and with data on their foreign buyers. The use of these data outperforms previously used micro-, meso- or macro-level data in identifying the causal effects of guarantees. For the first time in this literature, we can study the detailed linkages among guarantees, firm export destinations and firm performance while controlling for confounders at multiple levels. Moreover, guarantees are provided at the transaction level – for a particular firm's export of a certain product to a specific buyer in a particular country – and their direct effects can be expected to be captured fully at this level. In contrast, estimation at the country, industry or even firm level may attenuate the effects of guarantees.

Third, we corroborate a causal interpretation and improve our understanding of the underly-

[6]This research design mimics a randomised controlled trial and has recently been applied in, e.g., Asher and Novosad (2020) and Cohodes (2020).

ing mechanisms by exploring several novel dimensions. Guided by our conceptual framework, we start by analysing potentially differential impacts of guarantees across several response variables (firms' bilateral trade margins, jobs, value added and productivity) and key dimensions (firms' size, experience, foreign markets and time).[7] In this way, we investigate whether or not the overall effects can be explained by detailed evidence that is consistent with the role of guarantees in mitigating information frictions in trade. We then examine whether guarantees mainly address the buyers' default risk or ease firms' liquidity constraints. If a guarantee primarily addresses the risk of buyer's default, then the main effect would be expected to be on exports to the destination for which the guarantee was issued, while a homogeneous effect across destinations would indicate a primary effect of guarantees on firm liquidity. To analyse the destination market specificity of the effects, we meticulously match firm and export-destination dyads that acquire guarantees (treated) with very similar firm-destinations dyads (controls) that do not acquire guarantees. Then, we compare the changes – difference-in-differences (DD) – in the firm-destination- and firm-level performance of the treated and control firms while saturating the regressions with a full set of fixed effects.

Fourth, we contribute to the literature by studying the microeconomic impacts of guarantees not only on trade but also on firms' jobs, value added and labour productivity.[8] In this way, we shed light on whether or not the ultimate public goals – the promotion of jobs and value added – are attained. Another reason for studying performance measures beyond exports is that guarantees hypothetically could induce firms to redirect trade from safer to riskier markets, which may offer higher expected profits. Diverting trade could leave overall firm exports and jobs at home unaffected while benefiting buyers in riskier foreign markets.

[7]The role of firm characteristics for the perception and handling of trade barriers is emphasized in the trade literature (e.g., Melitz, 2003, Riding *et al.*, 2012, Minetti and Zhu, 2011, Chaney, 2014, Breinlich *et al.*, 2017). Regarding guarantees, Heiland and Yalcin (2020) find the link to expected exports to be positively associated with firm size, liquidity constraints and interbank interest rates, while Felbermayr *et al.* (2012) find a larger impact on sales and jobs during the financial crisis.

[8]Only one previous study has investigated the impacts on performance measures other than exports. Felbermayr *et al.* (2012) estimated the impacts on sales and jobs by using guarantee data and commercially available data for German firms. The study found a positive impact on both sales and jobs.

Turning to our results, we have three major findings. First, there is a causal effect of guarantees on firm exports. Guarantees increase the probability that a firm enters into a foreign market and increase the value of exports to this market. Exports also increase to other destinations than the destination for which the guarantees were issued. This additional impact diminishes with the distance from the intended market. Second, we find no general causal effects on performance in terms of jobs, value added and labour productivity. This finding contrasts with the commonly advanced aims of government-backed guarantees to promote not only exports but also jobs and overall economic growth. Third, we find that guarantees affect firms heterogeneously. Large manufacturing firms dominate the use of guarantees, but in terms of value, other firms experience the largest impacts of using guarantees. Exports increase the most for small firms, service firms and when exporting to small foreign firms. We expect these firms to be particularly disadvantaged by information frictions in trade. In addition, we find that the firms that start to use guarantees for small-scale exports hire more workers and that the firms that start to use guarantees for the first time in exports to a market experience an increase in value added. Overall, we interpret these patterns to indicate that guarantees mitigate information frictions in trade, primarily by addressing the default risk and secondarily by easing liquidity constraints.

This paper also relates to a broader literature on information frictions in trade and their mitigation. This includes both theoretical trade models with imperfect information (e.g., Rauch and Casella, 2003, Chaney, 2014, Allen, 2014, Steinwender, 2018) and the empirical studies on countermeasures beyond guarantees, such as technology (e.g., Steinwender, 2018), networks (e.g., Rauch and Trindade, 2002, Parsons and Vézina, 2018), and export promotion/subsidies (e.g., Alvarez, 2004, Görg *et al.*, 2008, Ferguson and Forslid, 2019). Export credit guarantees are similar to export promotion and grants as they are a public measure for exports. Still, guarantees are distinct. Guarantees are to be fully paid by firms, are transaction specific, and focus on the buyers' default risk. Our paper adds to this literature by providing evidence on guarantees and firm performance that is robust to self-selection

and based on comprehensive administrative data.[9]

Finally, our paper relates to the trade finance literature. Exporters face binding liquidity constraints with respect to the funding of both fixed and variable export costs (Manova, 2013). Financial intermediaries may assist with, for example, financing or payment arrangements (Amiti and Weinstein, 2011, Scmidt-Eisenlohr, 2013, Paravisini *et al.*, 2014). We analyse a prevalent government-backed instrument to address firms' export risk that may also improve liquidity. Our research design helps us study the effects while addressing the issue of reverse causality discussed in the trade finance literature (e.g., Amiti and Weinstein, 2011, Niepmann and Schmidt-Eisenlohr, 2017).

The remainder of this paper is organised as follows. In Section 2., we describe our conceptual framework. In Section 3., we introduce our data and portray the exporters and their foreign buyers. Next, in Section 4., we elaborate on our identification strategy and explain the quasi-natural experiment. In Section 5., we present our econometric results on the effects of guarantees on firm performance. In Section 6., we conclude. (We provide additional results and details in the Online Appendix.)

2. Conceptual Framework

Our conceptual framework discusses the default and liquidity problems in foreign trade (Section 2.1.) and analyses the rationales for and effects of government intervention to arrive at our conjectures for the empirical analysis (Section 2.2.).

2.1. The Default and Liquidity Problems in Foreign Trade

In foreign trade, heterogeneous agents typically agree that one of them will extend credit to the other in the form of trade credit (in-kind) via either an open account for the exporter or cash-in-advance for the importer. Information frictions expose agents not only to the hazard

[9]Recently, a few studies have emerged on export promotion that also directly address self-selection, e.g., by adopting a randomised controlled trial design for a sample of apparel and textile firms in Vietnam (Kim *et al.*, 2018) or for a sample of SMEs in UK manufacturing (Breinlich *et al.*, 2017).

that the other will default – the default problem – but also to the opportunity cost of having scarce resources committed to a risky deal that restricts the agent's alternative use of these resources – the liquidity problem.[10] The time value of money adds to the liquidity problem since long-distance and cross-border trade is time consuming.

To address the default problem, agents can themselves search for information about one another and the foreign market or involve an intermediary, but in both cases, they will incur fixed costs.[11] To address the liquidity problem, agents can tie up working capital or borrow from a financial intermediary, but they will incur variable costs, which increase in the size of the deal and potentially also in its duration.

If agents incur these costs, then not every agent can retrieve the information and afford financing, at least not for every potential other agent. Generally, the costs may be bearable only for the most productive agents, which typically are larger firms.[12] Therefore, not every firm engages in trade, and the firms that do primarily trade with neighbouring markets or markets where they already have established a foothold (Melitz, 2003, Chaney, 2014, Morales *et al.*, 2019). Some firms, e.g., small-sized firms, may be disadvantaged, even if they are highly productive, either because of the potentially non-existent/small scale of their current trade, which can result in high or even prohibitive average trade costs, or because of their inability to access external financing (Berman and Héricourt, 2010, Muûls, 2015).[13]

The most productive agents, who can bear the involved costs, can either extend credit to signal the superiority of their offer (Lee and Stowe, 1993, Giannetti *et al.*, 2011) or use financial intermediaries to extend credit. More liquid firms can themselves more easily extend

[10]The incomplete nature of contracts, more cumbersome contract enforcement internationally and political risk, e.g., currency transfer restrictions, exacerbate the default risk (Ellingsen and Vlachos, 2009).

[11]The acquired information is likely to be incomplete, which continues to make it less safe to extend credit for foreign trade than for domestic transactions. Assuming managers are risk-averse (Lovallo *et al.*, 2020), they may thus still hesitate to trade. Potentially, the acquired information may even make the firms more hesitant to export, as they realise the risks associated with the potential market and customer (e.g., Breinlich *et al.*, 2017).

[12]There may be trade deals with positive expected value that, due to high levels of risk, not even the most productive firms are willing to take on.

[13]Consider two firms, A and B, that have identical productivity, but A already exports extensively, while B has just begun. Then, B may find the fixed costs of entering into trade to be prohibitive.

credit. However, even using intermediaries to extend credit requires a strong balance sheet or particular collateral.[14] Therefore, the firms in industries with less collateral and smaller firms in general – which are less productive, more liquidity constrained and face difficulties in accessing external financing – may be particularly disadvantaged (OECD, 2013, Manova, 2013, Carpenter and Petersen, 2002, Riding *et al.*, 2012). Thus, such firms may be restricted either to trade with "safe" counterparties or to not trade at all, which limits firm growth (Quadrini and Qi, 2018).

2.2. Export Credit Guarantees

To address the default and liquidity problems that may depress foreign trade below the level associated with optimal resource allocation, governments may intervene (Dixit, 2003). The government can act as the "guarantor of last resort" through government-backed export credit guarantees for a fee. These non-marketable guarantees address the default risks involved for firms and thereby also their liquidity problem. Thus, more exporters can trade and can more easily secure trade financing (Funatsu, 1986, Heiland and Yalcin, 2020).

There are a number of reasons why a government institution can insure foreign transactions that financial intermediaries hardly underwrite and do so at competitive fees. If the government provides the guarantees, then it takes on the fixed costs of information collection, including the acquisition of specialised knowledge about political risks and channels for assessing commercial risks in foreign trade; the government can also diversify risks beyond the scope of many firms and utilise its taxation authority and endowments as collateral to ensure contract fulfilment (within the coverage ratio), even for highly risky or large transactions. The government may also easily reinsure export credit risks. It may also employ its public and diplomatic channels to pursue claims against defaulting parties. Government-backed trade finance via guarantees may be especially important during macroeconomic crises, as

[14] A strong balance sheet may be insufficient, however. Information asymmetries between exporters and intermediaries about importers may make the intermediaries non-competitive in underwriting the risks (Smith, 1987, Brennan *et al.*, 1988). This may lead to adverse selection into using underwriters, which in turn limits the latters' underwriting to "safer" deals with a short maturity or to entire customer portfolios.

trade finance is more important in foreign than in domestic trade, and trade finance tends to dry up during such crises (Ahn *et al.*, 2011). Finally, in practice, the government seems to be advantaged vis-a-vis banks in providing competitively priced guarantees, since only banks were subject to new regulations in the aftermath of the financial crisis, such as increased capital requirements.

An institution that mitigates information frictions in trade can promote firm exports at both the extensive and intensive margins in a Melitz (2003)-type model. Lowering the fixed costs causes new firms to start exporting, which expands their sales. Likewise, lowering the variable costs spurs firm exports both by new export entry and by the expansion of existing exporters. Assuming heterogeneous export costs across countries, we expect similar pro-export effects at the firm-destination margins (Conjecture 1).[15] If we abstract from Melitz (2003) to also allow for dynamic gains from trade, then export guarantees could lead to within-firm productivity growth (Conjecture 2), which improves welfare. For example, producing for the foreign market may lead to more investment in innovation (see, e.g., Schmookler, 1954, Lileeva and Trefler, 2010, Aghion *et al.*, 2018) or learning-by-exporting effects (e.g., Loecker, 2013) or provide a foothold to more easily enter into adjacent foreign markets, which lowers firm-country entry costs (e.g., Chaney, 2014, Morales *et al.*, 2019). Related to our discussion and Conjecture 1, we expect that smaller, less well-endowed and less internationalised firms may be particularly advantaged in exports by institutional support. In addition, we expect stronger export effects in times of macroeconomic shock.

In expecting guarantees to promote exports, from Conjecture 1, we have two corollaries. The first corollary is that guarantees increase firm exports; they do not simply divert exports to high-risk markets (Corollary 1). Therefore, our empirical investigation should also analyse the effects of guarantees on total firm performance, such as employment and value added.

[15] If the new exporters are at the right-tail of the productivity distribution of the non-exporters (Lileeva and Trefler, 2010), and assuming that the expanding existing exporters also are above par in their distribution, then this generates between-firm productivity growth via competition for scarce resources and a subsequent exit of the least productive firms (Melitz, 2003). Therefore, exports, sales and productivity will rise, which improves welfare.

The second corollary holds that we expect guarantees to have a stronger impact on firm exports to the given destination for which the guarantee is provided if the guarantee primarily addresses the default risk (Corollary 2a). Acquiring a guarantee for a specific transaction addresses the risk of buyer's default in this transaction but not in the other transactions of the firm. Conversely, we expect guarantees to have as large an impact on exports to destinations other than the one for which the guarantee is provided if the guarantee primarily addresses the liquidity problem (Corollary 2b). A guarantee could generally facilitate access to external funding for trade, which eases the liquidity constraints of the firm. The guarantee may also be discounted by the firm's bank. In this way, the guarantee enables the firm to export to other customers across the world or to reduce liabilities on its balance sheet, and it improves free cash flow and, therefore, firm valuation by reducing the net working capital needed.

3. Data and a Portrait of the Firms Involved

We begin this section with a primer on the source of our data on export credit guarantees – the Swedish Export Credit Agency. Next, we present our data on guarantees, firms and foreign trade, including the response variables and confounding factors. We conclude by portraying the firms that use the guarantees and these firms' foreign buyers.

3.1. The Swedish Export Credit Agency

In Sweden, export credit guarantees are provided by the Export Credit Agency, *Exportkreditnämnden* (EKN). The EKN is an independent governmental agency under the Foreign Ministry that was established in 1933. In recent years, the agency has annually provided new guarantees worth approximately five billion USD, but it has the authority to provide substantially larger amounts, and it benefits from unlimited credit from the Swedish National Debt Office. In 2017, the agency had approximately 400 customers and covered between 1,500 and 2,000 business transactions to over 130 foreign countries. In 2017, the value of all

guarantees outstanding was USD 21,256 million.[16]

The Export Credit Guarantee Ordinance states that the agency may issue export credit guarantees to promote Swedish exports, internationalisation and competitiveness *"if the operation that is to be guaranteed is of Swedish public interest, or otherwise beneficial for the financial development in Sweden."*. The agency should also increase knowledge of its services among SMEs and reduce their export thresholds.

The EKN guarantees should complement privately available – so-called marketable – guarantees. That is, the agency is the guarantor of last resort.[17] Moreover, the agency should break even in the long run. Therefore, the fees paid by its customers – the so-called premiums – should finance the business of the agency to cover both expected losses and overhead costs. If it fails to cover losses, which occurred only once in the 1980s, then it can access the necessary funding from the Swedish National Debt Office. A final restriction is that the agency may not issue short-term guarantees ($<$ 24 months) for exports to countries considered to be very safe in terms of credit risk, such as other high-income OECD countries.

Applying for a guarantee is free of charge, open to any firm, and may be accomplished online, while usage is associated with a fee (the premium). When applying, the exporting firm, its foreign buyer and the export transaction are screened. The agency assesses if the parties to the deal can fulfil their contractual obligations. It also assesses the risk of losses so that the guarantee would be needed. Denials occur but are very rare.[18] If *offered* a guarantee, then the firm may choose to use it or not. Most of the firms that receive an offer accept it, which results in the agency *issuing* the guarantee.

The premium is transaction specific and expressed as a percentage of the guaranteed export

[16] As in many EU countries, but unlike in, for example, the US, the Swedish export financing system is divided into two parts, with the EKN providing guarantees to firms and the Swedish Export Credit Corporation (SEK AB) providing mid-sized to large firms with export credit for medium- to long-term and large export contracts.

[17] In Sweden, there are some private insurers. However, they mainly cover all turnover or customer portfolios, which ensures that risks are spread out and thereby lowers premiums and overhead costs.

[18] The agency generally attempts to facilitate firm efforts to acquire a guarantee, if the firm so wishes.

value. It is based on the agency's calculation of the probability of default, the risk duration, and the guaranteed amount. The premium is higher when the default probability is higher, the risk duration is longer and the guaranteed amount is larger. The default risk, in turn, is based not only on the country credit risk, which is endogenously and annually set cooperatively by the OECD, but also on the commercial risk of the buyer.[19] The guaranteed amount cannot exceed a set limit in terms of the share of the export transaction; this is the so-called coverage ratio.[20] The remaining transaction share – the deductible – is retained by the firm for its own account.

3.2. Data

Data on Export Credit Guarantees

We have accessed novel and exhaustive panel data from the EKN on offered and issued guarantees that insure export transactions against buyers' default, which are so-called loss on claim guarantees (LOCGs; henceforth, export credit guarantees or guarantees.).[21] The loss on claim guarantees include guarantees for exporters, letters of credit and guarantees for lenders, with the guarantees for exporters accounting for approximately 80 percent of the offered LOCGs. Our highly detailed data on guarantees from the EKN enable us to explore seller-contract-destination-buyer variation over almost two decades. (More information on the data construction is provided in the Online Appendix, part B.)

The picture that emerges is that a limited number of firms account for a large number of guarantees and/or highly valued export contracts (see Table A1 in the Online Appendix). At

[19]The agency classifies the commercial risk of the buyer on an A-F scale, with an A corresponding to a government buyer and an F representing a newly established firm/weak firm/highly uncertain project.

[20]This ratio is the guaranteed transaction value over the potential loss on the claim as stipulated by the firm. The potential loss on the claim is the sum of the contract value and the firm's associated foreign market costs.

[21]LOCGs are typical products of ECAs, see, e.g., the survey of Growth Analysis (2015). LOCGs account for approximately 80% of the number of guarantees offered by the EKN. The remaining 20% of the offered guarantees concern (a) bills of exchange, (b) claims against exporters, (c) investment credits, and (d) working capital. Although (a) is not directly tied to an export event, (b) is, but it concerns exporters' rather than buyers' default. The latter two guarantees (c and d) are not directly tied to an export contract.

least half of the firms have only one guarantee per year, while the mean number of guarantees per firm and year is six. The average value of an export contract is USD 3.8 million, and the median value is USD 259,100. The number of unique firms that receive guarantees in the years that we cover is 957, with 84.6 percent of these firms being registered in Sweden. The regional allocation of guarantees is skewed towards exports to the Middle East and Latin America (see Table A2). In total, guarantees are issued to 168 destination countries.[22]

In Figure 1, we display the trends in the number of guarantees (Panel A) and in the firms that use them (Panel B) in the years from 2000-2016. The number of guarantees increased substantially, while the number of firms using them rose more moderately. The upward trends began in the years before the financial crisis, but following the crisis, the increase was more rapid. In the aftermath, firms temporarily decreased and then increased their usage, as measured by the number of guarantees. However, in the post-crisis period, the number of firms using the agency's guarantees gravitated toward the lower pre-crisis level. Overall, these patterns indicate an increase over time in the number of guarantees per firm.

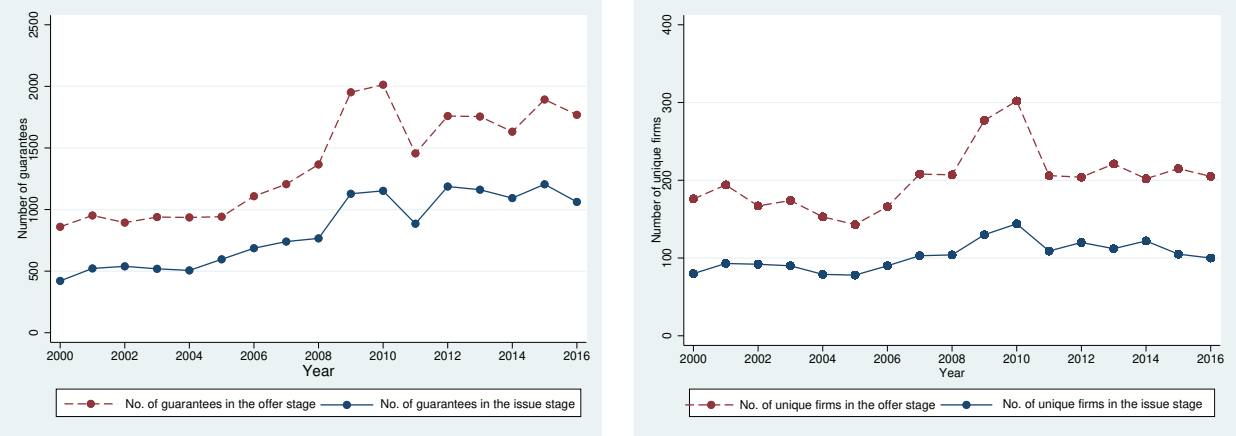

FIGURE 1

Number of guarantees and the firms using guarantees, per year.

Notes: The figure plots guarantees and the firms with guarantees in the years from 2000-2016. To the left are the total number of guarantees per year, and to the right are the total number of unique firms that used guarantees per year. The red (dotted) line is for the guarantees that firms have applied for and have subsequently been offered. The blue (continuous) line is for the guarantees that firms have been offered and have subsequently acquired, which are so-called issued guarantees.

[22] According to discussions with industry members, the share of bilateral exports covered by the guarantees of the EKN varies substantially, with the agency, in effect, being almost the sole provider of guarantees for exports to some risky countries.

Data on Firms and Foreign Trade

To identify the causal effects of guarantees, we need to carefully consider the selection into the use of guarantees and control for confounding factors in the estimation of the effects on firm performance. Therefore, we must complement the previously mentioned EKN data with detailed information on firm performance and characteristics. We have access to such information in the administrative registers from Statistics Sweden. Because of the presence of unique identifiers of all residents, establishments, firms and enterprise groups in Sweden, we can easily merge the EKN data with these registers. Additionally, we include macro-level data on, for example, production, access to foreign markets, financial development and trade flows. (We summarise our variables and sources in Table A3 and provide more information on the dataset construction in Part B of the Online Appendix.)

Specifically, we use five key registers from Statistics Sweden. For information about firms' worker composition, we use the Longitudinal Integrated Database for Health Insurance and Labour Market Studies (LISA). LISA includes very detailed information about the characteristics of all individuals (from 15 years of age). The Structural Business Statistics (SBS) provides granular data on the input and output of all active firms, including information from income statements, balance sheets, business and earnings statistics, and register-based labour market statistics. Foreign Trade Statistics (FTS) contains firm-country-product level longitudinal data that cover firms' trade. Comprehensive data for non-EU trade in goods are from the Swedish Customs. Data for intra-EU trade in goods are from Statistics Sweden and include transactions from firms with trade above a certain threshold, which cover 96 percent of all intra-EU trade. Service trade data are from a survey that captures 80 percent of total trade. Enterprise group affiliation is from the Enterprise Group Register, and industry affiliation is from the Business Register.

Accordingly, we have access to information on the population of workers, establishments, firms and enterprise groups in Sweden and these firms' bilateral trade and their transaction-

level use of guarantees. Most of the information is provided on an annual basis. Henceforth, the period of study is from 2000-2015 since we lack business statistics for 2016.

To construct our dataset for analysis, we focus on the users of guarantees and aggregate the transaction-level data to the levels of our subsequent analyses, namely, the firm-destination and firm levels. (In the subsequent analyses, we compare these dyads and firms with similar ones out of the same population.) By aggregating the data, for example, in 2015, we arrive at 462 firm-destination observations with guarantees held by 136 unique firms. These firms' exports amounted to two billion USD and almost half of the exports of these firms were guaranteed by the agency (see Table A4 in the Online Appendix). The exports covered by the agency went to 99 countries and consisted of approximately 1,397 unique products at the 8-digit CN level. The exports covered by the agency's export credit guarantees constituted a small share of the total Swedish exports of goods and services in 2015, as captured by our merged dataset, which is approximately 0.7 percent and is down from 7.1 percent during the financial crisis.[23]

Subsequently, we focus our interest on firm-destination dyads and the firms that received agency guarantees this year but not in the previous year. We regard these as potentially subject to treatment. We compare these dyads and firms with a control group of very similar dyads and firms, as is later explained. In 2015, there were 53 unique first-time buyers of guarantees – potentially treated firms – compared to the total of 136 firms that received guarantees this year.

Response Variables and Confounding Factors

By using our matched longitudinal dataset, we test whether guarantees subsequently promote bilateral exports and ultimately contribute to jobs and value added. The response variables are therefore the probability of exporting, (log) value of exports (goods and services),[24] (log)

[23]Most trade is financed via an open account, a minority is financed via financial intermediaries, and another smaller minority is financed via cash-in-advance payments (Chauffour and Malouche, 2011).

[24]A small constant ($1e-7$) is added to avoid truncation.

value added, (log) employment, and (log) labour productivity.

To avoid confounding the effects of export guarantees, we need to meticulously control for the influence of a range of other factors. Fortunately, our dataset is uniquely suited to this task because of its comprehensiveness and fine level of detail.

First, we include variables related to trade, output and input. To ensure similar initial conditions, we typically lag these variables by one period, and we include the pre-trend of the response variables.[25] All continuous variables are in log format. We then begin by adding trade intensities and firm exporter and importer status. Next, we include firm output in the form of turnover and value added. Then, firm input is included in the form of intermediate goods (raw materials, goods and services), workforce size (number of full-time employees), human capital stock (proxied by the share of post-secondary educated employees and by wages paid to workers), and physical capital stock. Additionally, we include the firm age, which has been associated with both firm growth and exports.

Next, we control for affiliations that may confound the results, by adding indicators for multinational status, foreign ownership and industry. We also add year-specific effects to control for, for example, macroeconomic shocks such as the global financial crisis.

At this stage, we have considered key trade, output and input characteristics, and firms' affiliations. In this way, we have in effect controlled for firm productivity while avoiding a range of assumptions and pitfalls associated with measuring total factor productivity (see, e.g., Hummels *et al.*, 2014). Moreover, our control variables are likely to capture the financial constraints of the firm, which relate to the aspects of a firm's operations.[26]

Third, we use information related to sectors and destination countries. To capture the financial vulnerability of the firm's sector, we compute the two-digit sector average external financial dependence and the asset tangibility of the sector by closely following Braun and

[25] Using an alternative lag structure does not affect our results.

[26] For example, the output and size of a firm are proxies for the firm's financial risk, while physical capital is related to the availability of collateral, which affects a firms' ability to receive loans.

Larrain (2005) and Manova (2013). External financial dependence is the share of capital expenditures not financed with cash flows from operations.[27] Asset tangibility measures the share of net property, plant, and equipment in the total book value of assets.[28]

At the country level, we add the usual gravity variables (GDP, bilateral distance, and membership in the WTO and free trade agreements), financial development indicators from the Fraser Institute and country credit risk from the EKN. The financial development indicators are used as proxies for the capacity of the country to provide external financing, for example, for trade. The country credit risk is included to reflect both the risk of a government imposing barriers to transfer funds abroad (in local or foreign currencies) and the risk of force majeure, such as natural or political disasters, for example, war. In the estimations of the export effects, these variables are included at the firm-destination-year level, while in the firm-level estimations, they are included as the weighted mean of the country-level variables, with the weights being the firm's bilateral share of exports in its total global exports.

Fourth, we add a number of first-differenced variables (turnover, human and physical capital and wages) and a measure of the foreign demand shocks for the firm's existing export portfolio. Our underlying conjecture is that firms' intention to export, their realised exports and interest in backing from the EKN are all related to both foreign demand shocks and the firm's trajectory, with growing (shrinking) firms being more (less) prone to expand abroad, while the expected relation to seeking EKN backing is not as clear cut.[29]

Our foreign demand and supply shock variables draw on Hummels *et al.* (2014) and Munch and Schaur (2018). The idea is to create a firm-specific measure of foreign demand (supply) shocks by combining firm-product-level export (import) data from Statistics Sweden and bi-

[27] Specifically, we compute financial dependence (F_s) as $F_s = \frac{L_s - C_s}{L_s}$, where L_s represents the funds used to add property, plants, and equipment; C_s denotes the funds used for operating activities; and C_s is equal to the adjusted funds from operations (A_s) plus the change in inventory (I_s), where A_s is the net income less equipment depreciation.

[28] Constructing the measures at the sector level attenuates our concern that the measures could be endogenous to the firm's financial development.

[29] A growing firm may be receptive to means to assist its expansion abroad, but it is also feasible that shrinking firms are under even stronger pressure to find such means.

lateral product-level import (export) data from the United Nations COMTRADE database. Both sources of data provide trade values at the six-digit harmonized system level. Specifically, we compute the firm shock variable FS_{it} as follows for demand (and analogously for supply):

$$FS_{it} = \sum_{k} ex_{ikt-1} \frac{IM_{kt} - IM_{kt-1}}{IM_{kt-1}} \tag{1}$$

where ex_{ikt-1} is firm i's export share of product k in the pre-treatment year, k belongs to the firm's set of export products, and IM_{kt} measures the imports of all countries except Sweden of this product in year t.[30]

Finally, we use information on foreign buyers to assist in identification and sub-analyses. We can retrieve foreign buyer information by exploiting the fact that the EKN dataset contains identification numbers for the foreign buyers of the exporting Swedish firms. Specifically, we have the worldwide unique Dun & Bradstreet (DNB) identification number (DUNS) or the organisational number of the foreign buyer. Through these numbers, we add information on foreign buyers' most updated financial composition and corporate structure, which are all from the DNB Global Reference Solution (GRS) database.

3.3. A Portrait of Guaranteed Firms and their Foreign Buyers

Finally, in this section, we describe both the firms with export credit guarantees in our matched longitudinal dataset and these firms' foreign buyers (Tables A5-A9 in the Online Appendix). For comparison with the universe of Swedish firms, which are denoted "U" below, we provide their statistics in parentheses.

There are 671 unique exporting firms in the dataset (Table A5). Two-thirds of the firms are SMEs, with a median of 58 employees (U:1). Over time, the share of SMEs increased, from 60 percent in 2000 to 69 percent in 2015.[31] However, large firms clearly dominate in terms of

[30] If a firm's export information is missing in $t-1$, we replace it with information from the previous year.

[31] Industry representatives report that a likely contributor to this trend is the increased costs that SMEs have faced for using private financial intermediaries since the late 2000s, which have induced them to consider the agency instead. New laws and regulations to prevent money laundering and terrorist financing have meant

employment and turnover, with the average firm having 734 employees (U:6) and a turnover of USD 499 million (U:2). Most firms that use guarantees are multinational, contrary to most Swedish firms, but are not foreign owned, similar to Swedish firms in general. Most firms export but not intensively. The average *total* export intensity is 8.7 percent, and the bilateral export intensity is 3 percent (U:8 and 0.1, respectively). Regarding the sectoral distribution of the firms that use the guarantees, manufacturing and wholesale jointly dominate to capture an 81-percent share of the firms (Table A6). These are also major industries in Sweden, in terms of employment, and they account for approximately one-fifth of the firms in private business (excluding primary industries).

With respect to the foreign buyers, they are rather similar in size to the exporting firms in Sweden (Table A7). Most of the buyers are medium-sized in terms of employment, although large buyers account for a substantial share of overall employment. In terms of commercial activity, the foreign buyers are somewhat smaller than the Swedish firms. The largest share of foreign buyers is found in Latin America, which account for 31 percent (Table A8). Interestingly, the second most important region of foreign buyers is Europe. The sectoral distribution of the foreign firms mimics the sectoral distribution of the exporting firms in Sweden (Table A9).

4. Identification Strategy

In this paper, we estimate the effects of export credit guarantees on firms' performance. In identifying the causal effects of such a voluntary act, we encounter two problems. First, we have self-selection in the "treatment" of guarantees. Firms are likely to seek guarantees for exports only if they expect to close these deals. Moreover, guarantee usage may be non-random across firms, which confounds the effects. For example, large firms are traditionally more established users of guarantees. Such differences in usage across firms may be correlated with the outcomes in which we are interested, for example, large firms are also known to

higher compliance costs for financial intermediaries, and these costs have trickled down to their customers.

be more strongly associated with exports. Second, the outcomes for using and not using a guarantee cannot simultaneously be observed for the same firm; that is, we are missing the counterfactual, the so-called fundamental problem of causal inference (Holland, 1986).

Our key strategy to address the two problems is to exploit a quasi-natural experimental setting. We utilise specific information about occurrences when the Swedish agency has directly approached firms – in large-scale marketing campaigns – rather than firms approaching the agency. The research design directly addresses the issue of self-selection while paying attention to the issue of the missing counterfactual. We estimate the local effects from the exogenous variation in the likelihood of treatment. Because of the cut-off rule adopted to assign firms to the treatment, it is possible to assess the effects of the guarantees by using a fuzzy regression discontinuity design (FRDD) where the assigned treatment through the cut-off is an instrument for the effective use of the guarantees (Hahn *et al.*, 2001, Imbens and Lemieux, 2008). This approach is closer to a randomised controlled trial than most other non-experimental designs and therefore provides for cleaner identification (Cook and Wang, 2008). We now turn to the details of our quasi-natural experimental setting.

Between 2012 and 2016 the agency ran several large-scale direct marketing campaigns that regularly targeted approximately 15,000 firms. The aim was to increase awareness about its guarantees and to attract customers among exporting SMEs. SMEs had earlier been identified as having relatively poor knowledge about the agency, particularly about its offers. The government increasingly emphasized this in communication with the agency. The adversarial situation of exporting SMEs in the wake of the financial crisis likely brought this issue of guarantees even more to the fore. The resulting campaigns had therefore as the key criterion for inclusion that the exporting firms had to have less than 250 employees.

The marketing campaigns were designed as follows. The agency approached a firm's chief executive, financial officer, marketing manager or export manager through surface mail. This mail contained a signed cover letter from the regional contact person of the agency and a

brochure with both real-world examples of Swedish companies using the guarantees and some information about the guarantees. The letter would typically start with some statistic on the development of the exports of SMEs or a quote from an SME using the guarantees and then discuss the role and importance of the EKN. It concluded with an offer of more information via e-mail, phone or a personal visit by the agency.

For identification purposes, it is encouraging that these marketing campaigns were general in nature, not specifically targeting certain firms.[32] The design virtually eliminates concerns about certain firms being selected into receiving this mail. In fact, the approached firms constituted not only approximately 5 percent of all SMEs but also the overall majority (70 percent) of the firms that exported and virtually all of the goods-exporting firms. However, most of the approached firms were only marginal exporters. Their median export intensity was 2.4 percent, and their mean (and median) number of foreign destinations was one.

In this setting, we employ the FRDD. We can use the key selection criterion of firms having less than 250 employees to control for the unobservables that may affect the selection into using guarantees. Then, we can compare the development of firms just below and just above the firm size threshold. The threshold makes the probability of treatment random around the value of 250 employees. The firms with at least 250 employees were as likely to get the information had they only had slightly fewer employees. In Figure 2, we display how the cut-off works in practice among the Swedish firms. The use of guarantees jumps (+44 percentage points) at the threshold, but as expected, not all firms assigned the treatment respond by starting to use guarantees. The firms size threshold is also advantageous in fulfilling a fundamental criterion in regression discontinuity designs, namely, that the agents should not be able to influence the threshold to be able to benefit from the treatment.

[32] A potentially more effective marketing design could have targeted the likeliest prospects, such as the firms that were most likely to become new customers based on, e.g., their industry, size and export profile or the firms that had the largest export potential based on, e.g., the foreign market demand growth in their specific industry.

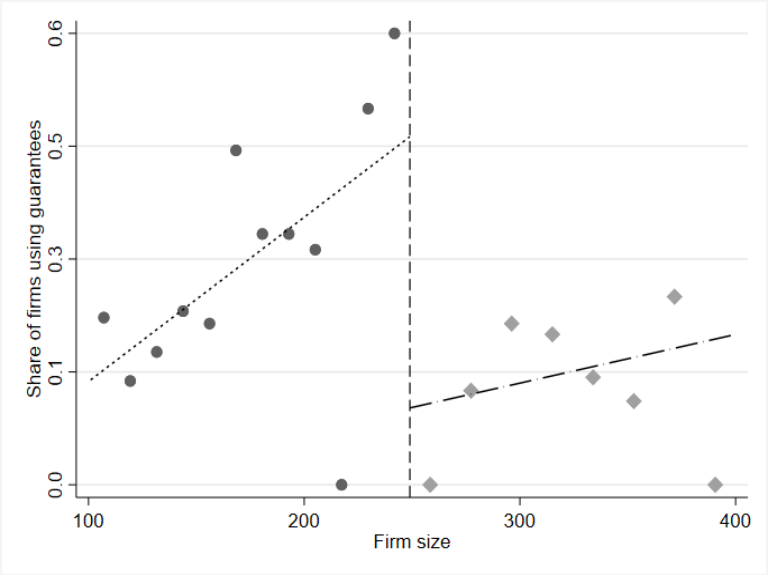

FIGURE 2

The use of guarantees around the firm size threshold.

Notes: The figure displays guarantee usage according to the assignment variable (firm size) for firms, with the discontinuity at the threshold being 0.51 (0.07) and within the bandwidth of 150. A linear fit is imposed on both sides of the cut-off. The dots correspond to the average use of the guarantees for a bin width of 10.

We argue that both the low awareness of the guarantees among SMEs and the relatively low pay-off of getting the information mail from the Swedish agency make it unlikely that firms would hire or fire employees to qualify for being contacted by the agency. Reassuringly, we also find the distribution of employment to be smooth around the threshold.

In our FRDD, we use the exogenous assignment to the treatment as an instrument for the usage of the guarantees to address the potential endogeneity problems that arise from partial compliance. Therefore, the treatment status is probabilistically determined as a discontinuous function of firm size (Lee and Lemieux, 2010). Following most of the papers in the literature, we use a parametric approach.[33]

More specifically, we measure the effects as the difference-in-difference (DD) in the response variables between the year before being approached by the agency $(t-1)$ and the following year (t). This is to compare the difference in the outcomes between the firms that are likely

[33]For robustness, we alternatively employ a non-parametric FRDD.

to use a guarantee vis-a-vis a destination in t but not in $t-1$ and the firms that were not likely to use a guarantee in t or in $t-1$. For each firm-country dyad, there are two potential outcomes, namely, the cases of likely treatment Y_1 and of non-treatment Y_0. We let D be an indicator variable that denotes treatment. The estimated local average treatment effect on the treated ($LATT$) is then

$$\hat{\delta}_{LATT} = E[Y_1 - Y_0 | D = 1, F = T^*, \mathbf{X} = x] = E(Y_1 | D = 1, F = T^*) - E(Y_0 | D = 1, F = T^*) \tag{2}$$

where $Y_1|D = 1$ is the outcome of interest for the treated firms and $Y_0|D = 1$ is the hypothetical outcome for them if they had not been approached by the agency. The selection into treatment depends on the value of the exogenously set threshold ($T^* = 250$) relative to the forcing variable F. $\mathbf{X}$ is the vector of the firm, industry and foreign market characteristics that is used to increase the precision of the estimates; these characteristics were discussed in Section 3.2.. Conditional on F, we assume that the joint distribution of Y_0 and Y_1 is orthogonal to the threshold value.

Formally, we estimate the model as:

$$D_i = \alpha_o + \alpha_1 L_i + \mathbf{X_i}\boldsymbol{\alpha_2} + \epsilon_i \tag{3}$$

$$Y_i = \beta_0 + \beta_1 \widehat{D_i} + \beta_2 L_i + \mathbf{X_i}\boldsymbol{\beta_3} + \eta_i, \tag{4}$$

where Y_i is the DD response variable of firm i (which is measured as the changes in export probability, export value, value added, employment and labour productivity),[34] D_i is the assigned treatment, L_i is the firm size, $\mathbf{X_i}$ is the vector of the characteristics that we include to avoid any bias in the case of limited balancing, and ϵ_i and η_i are random error terms.

In the first stage, our equation 3 relates firms' participation in the guarantees to firm size,

[34]The first two variables are at the firm-country level, and the following variables are at the firm level.

with parameter α_1 capturing the effect of firm size on the participation in the guarantees. In the second stage, equation 4 relates firm performance to firm size by using a smooth function in the estimation. Assuming that the relation is continuous around the cut-off, any jump in the response variable in proximity to the cut-off can be interpreted as a causal treatment effect, which is captured by parameter β_1.

For the validity of the FRDD, the predetermined covariates should balance for firms just above and below the cut-off point. This means that around the threshold of the firm size, firms should not significantly differ in terms of characteristics. To investigate this issue, we ran a number of discontinuity regressions by using our firm, sector and foreign market characteristics, in turn, as a response variable; that is, we regressed the characteristics on the assigned treatment dummy while controlling for the firm size. Being assigned the treatment is not statistically associated with an overall majority of the 36 characteristics, which reassures us about the random assignment around the discontinuity (Imbens and Lemieux, 2008) (Table A10). However, since 4 of the 36 variables are associated with the assignment of treatment,[35] we choose to carefully control for firm, sector and foreign market characteristics in our estimations.

5. Econometric Results

5.1. Main Results

In Table 1, we present our results from employing the FRDD to the Swedish quasi-natural experiment. Starting with the results for equation (3), we encouragingly find that exposure to the assigned treatment ($<$ 250 employees) strongly determines the effective treatment (using guarantees). Around the threshold, employing one less worker is associated with a 43-percent increase in the probability of usage.

Next, we turn to the results for firm performance, that is, the estimation of equation (4).

[35] The variables are import intensity, the cost of raw materials, FTA membership and private sector credit.

TABLE 1
Effects of guarantees on firm performance.

Panel A:	Change in the probability of exporting (1)	Change in export values (2)	
Intention-to-treat effects	**0.055**	**0.458**	
	(0.018)	(0.122)	
1st. stage estimates	**-0.425**	**-0.434**	
	(0.129)	(0.133)	
1st. stage F-statistics	20.16	32.16	
Obs.	296,226	296,226	
BW Loc. Poly. (h)	137	130	
Intention-to-treat effects (200% h)	**0.032**	**0.469**	
	(0.011)	(0.136)	
Intention-to-treat effects (50% h)	**0.101**	**0.916**	
	(0.312)	(0.450)	
Panel B:	Change in value added (1)	Change in employment (2)	Change in value added/worker (3)
Intention-to-treat effects	0.264	0.303	0.001
	(0.237)	(0.214)	(0.001)
1st. stage estimates	**-0.441**	**-0.432**	**-0.435**
	(0.124)	(0.132)	(0.118)
1st. stage F-statistics	10.78	20.46	26.16
Obs.	296,226	296,226	296,226
BW Loc. Poly. (h)	157	132	174

Notes: The table displays our main fuzzy regression discontinuity estimates of the local average treatment effects on the treated ($LATT$). First, we estimate the effects of being below the treatment cut-off on the probability to be treated. Then, we estimate the $LATT$ in terms of the differences in the outcomes between $t-1$ (the year before being approached by the agency) and t for the following performance measures: firm-destination export margins; firm value added; firm employment; and firm value added per worker. All continuous variables are in log form, with 1e-7 added to export values to avoid truncation. Throughout, we control for firm, sector and market characteristics and include firm-/industry-year and destination-year fixed effects (see 3.2. for details). In these estimations, an optimal bandwidth was determined by using the data-driven technique proposed by Imbens and Kalyanaraman (2012), with a triangle kernel weight. The specification uses local linear regression. Additionally, in Panel A, we provide the estimates when using half or twice the optimally chosen bandwidths. The first-stage Kleibergen-Paap rk LM and rk Wald statistics strongly support instrument validity (not displayed for brevity). The robust standard errors are in parentheses and clustered at the firm-destination level. For the coefficients in bold, $p < 0.05$.

First, in Panel A, we analyse the effects on firms' bilateral exports. We find that the probability of exporting increases relative to the controls (0.055) and that the value of exports increases substantially (0.458). The estimated coefficients represent the $LATT$ for the firms that received the treatment because their firm sizes are below the cut-off. These results are in line with our main Conjecture 1 and are economically non-trivial.

In Panel B, we analyse the effects of the treatment on other measures of firm performance. We find no statistically significant effects on firms' value added, jobs and labour productivity, which refutes Conjecture 2. Our insignificant finding on jobs contrasts with the results of a German study on a sample of predominantly larger firms that finds a positive and statistically significant firm-level job impact of guarantees aggregated to the firm level (Felbermayr *et al.*, 2012).

We interpret these fuzzy regression discontinuity results as demonstrating the causal effects of guarantees on firm export performance. As discussed, firms just above the threshold were as likely to get the information if they had only had a few less workers. Finally, we argue that the issue of manipulation to qualify for the treatment hardly applies to this quasi-experimental setting, which promotes our confidence in the results.

Robustness of the main results

In this subsection, we carefully test the robustness of our main results to the choices of estimator and bandwidth. Reassuringly, these additional results are both qualitatively and statistically consistent with our main results that use a parametric FRDD estimator together with a data-driven method for deciding the optimal bandwidth.

Starting with our parametric FRDD estimator, we recognise that it has a potential weakness in that the results could depend on the underlying distributional assumptions. To test whether our results are robust to such assumptions, we adopt an alternative non-parametric FRDD estimator. As before, the aim is to estimate the $LATT$ around the discontinuity of

250 employees. To address potential covariate imbalance in this particular design, we follow Linden and Yarnold (2016) by combining the approaches of propensity score matching and of constructing a weight based on the conditional probability of using the guarantees (that is, the "inverse probability of the treatment weight" (IPTW)).

In the first step of applying the non-parametric FRDD estimator, we estimate the propensity score (p) by using a probit regression to predict the use of guarantees while conditioning on the covariates that prior to the treatment were observed to affect the selection into treatment.[36] Each firm-country observation then receives an IPTW based on its actual treatment status and the estimated propensity score. Treated units get a weight equal to $\frac{1}{p}$, while control units get a weight equal to $\frac{1}{1-p}$. Using the IPTW not only ascertains that the distribution of covariates is equal between the treated and control units but also removes any existing association between the baseline covariates and treatment to replicate a randomised controlled trial. In the second step, we measure the effects as the difference-in-difference (DD) in the outcome variables between the year before being approached by the agency ($t-1$) and the following year (t). Lastly, we multiply the IPTW weight by the kernel weight and use this new weight in the FRDD regression to estimate the $LATT$.

In Table 2, on the next page, we display these alternative FRDD estimates. Reassuringly, we find that the first- and second-stage estimates are qualitatively similar to the main estimations. Being just below the threshold increases exports both at the extensive and intensive firm-country margins but does not have any statistically significant impact on the other response variables. Economically, these estimates are larger than our main estimates.

Finally, in the last rows of Panels A of both Tables 1 and 2, we test the robustness of the FRDD estimates to the choice of bandwidth by using bandwidths of half or twice the chosen ones (McCrary, 2008). Encouragingly, we find that in all of the cases, altering the bandwidths does not affect the statistical significance of our results. Moreover, the estimates

[36] In estimating propensity scores, we exclude only the assignment variable (workforce size) from the model. All other baseline covariates are included as before.

TABLE 2
Alternative estimates of the effects on firm performance.

Panel A:	Change in the probability of exporting (1)	Change in export values (2)	
Intention-to-treat effects	**0.124**	**1.159**	
	(0.060)	(0.380)	
1st. stage estimates	**-0.135**	**-0.137**	
	(0.049)	(0.058)	
1st. stage z-statistics	-3.310	-3.304	
Obs.	296,226	296,226	
BW Loc. Poly. (h)	92	89	
Intention-to-treat effects (200% h)	**0.094**	**0.309**	
	(0.032)	(0.148)	
Intention-to-treat effects (50% h)	**0.460**	**8.366**	
	(0.131)	(2.120)	
Panel B:	Change in value added (1)	Change in employment (2)	Change in value added/worker (3)
Intention-to-treat effects	0.201	0.648	0.001
	(0.322)	(0.504)	(0.003)
1st. stage estimates	**-0.130**	**-0.138**	**-0.129**
	(0.042)	(0.040)	(0.032)
1st. stage z-statistics	-3.226	-3.482	3.864
Obs.	296,226	296,226	296,226
BW Loc. Poly. (h)	90	119	110

Notes: The table displays our alternative non-parametric fuzzy regression discontinuity estimates of the local average treatment effects on the treated ($LATT$). First, we estimate the effects of being below the treatment cut-off on the probability to be treated while addressing the potential covariate imbalance by following Linden and Yarnold (2016). Then, we estimate the $LATT$ in terms of the differences in the outcomes between $t-1$ (the year before being approached by the agency) and t for the following performance measures: firm-destination export margins; firm value added; firm employment; and firm value added per worker. All continuous variables are in log form, with 1e-7 added to export values to avoid truncation. Throughout, we control for firm, sector and market characteristics and include firm-/industry-year and destination-year fixed effects (see 3.2. for details). In these estimations, the optimal bandwidth was determined by using the data-driven technique proposed by Imbens and Kalyanaraman (2012), with a triangle kernel weight. The specification uses local linear regression. Additionally, in Panel A, we provide the estimates when using half or twice the optimally chosen bandwidths. The robust standard errors are in parentheses and clustered at the firm-destination level. For the coefficients in bold, $p < 0.05$.

still remain economically important. However, as expected, changing the bandwidth affects the magnitudes of the estimates – with a narrower bandwidth resulting in substantially larger estimates and a wider bandwidth resulting in smaller estimates.

5.2. Heterogeneous Impacts and Mechanisms

To further investigate the causal effects of guarantees that we found in our quasi-natural experimental setting, we explore the detailed impacts across firms that differ, for example, in terms of size, experience and industry. Guided by our conceptual discussion, we expect firms to heterogeneously benefit from guarantees according to how disadvantaged they are by information frictions in foreign trade. We also apply our corollaries on the specific mechanisms by which guarantees impact exports to our data. However, we first revisit the identification strategy.

A Complementary Strategy

For our further analysis, our preferred identification strategy has two limitations. First, the FRDD captures the *LATT*s around the threshold value of 250 employees. Therefore, these effects apply to mid-sized firms but could be less valid for substantially smaller or larger firms. Second, applying the approach to firms around the threshold value implies that there are relatively few included observations, specifically, commonly less than 50 treated firms. These limitations inhibit our analysis of the potentially differential effects across, for example, firm size, experience and time. Meanwhile, such analyses are important for understanding the mechanisms at play from guarantees to firm performance.

Therefore, as a complementary strategy, we meticulously employ a DD propensity score matching estimator (e.g., Rosenbaum and Rubin, 1983, Heckman *et al.*, 1997). With this strategy, we both pay close attention to the selection into the treatment and to the time-invariant as well as time-varying heterogeneity in the unobserved firm characteristics. This DD matching estimator is especially suitable in our case because we have access to uniquely

rich population data (e.g., Heckman *et al.*, 1999, Smith and Todd, 2005). This access offers the possibility to control for common support, that is, that both firm-destinations that use guarantees (treated) and those that do not use guarantees (controls) have similar pre-treatment distributions for the selection variables. The DD matching estimator also offers flexibility in the form of fewer parametric assumptions than if we use, for example, an OLS regression as a complementary strategy.[37] Moreover, the alternative approach mimics a comparison of the de facto outcome with its counterfactual. We gauge the effect in the form of the average treatment effect on the treated (ATT) by comparing the treated with similar controls. The parameter of interest is δ_{ATT}, which measures the mean changes in the outcomes of the treated and untreated dyads and is estimated as

$$\hat{\delta}_{ATT} = [Y_1|D = 1, P(\mathbf{X})] - [Y_0|D = 0, P(\mathbf{X})] \tag{5}$$

where the treated and untreated dyads are matched on the conditional *ex ante* probability ($P(\mathbf{X})$) of using guarantees for exports to the foreign destination. $\mathbf{X}$ is a vector of the covariates observed prior to the treatment that are assumed to affect the selection into treatment.

The estimated parameter $\hat{\delta}_{ATT}$ can arguably be causally interpreted since we ensure a common support in the estimation, carefully heed the requirement of conditional independence between the treatment assignment and the outcome of the non-treated (see next paragraph), and since treatment is not mutually exclusive.[38] Furthermore, for identification, we select the variables for the propensity score estimation that affect both the selection into using export credit guarantees and the outcome (De Luna *et al.*, 2011).

The richness of the data that were presented in Section 3.2., allows us to condition the analysis on an unusually large number of observable pre-treatment characteristics of firms,

[37] An additional advantage relative to, e.g., within regressions is that we can abstain from assuming that past outcomes (e.g., exports and employment) do not affect the selection into treatment (Imai and Kim, 2017).

[38] One firm's treatment does not impede another one's.

industries and countries. In this way, we substantially limit the risk that unobserved heterogeneity between the treated and controls will affect the response variables.

The complementary estimator is unbiased even in the presence of systematic differences in the remaining unobserved time-invariant characteristics (between the treated and controls) that may affect the outcome, given that these characteristics will be differenced out. However, we also augment the estimator by adding firm-/industry-year and destination-year specific effects. In this way, we further control for any remaining unobserved *time-varying* heterogeneity that could bias the results and for multilateral resistance.

We implement this DD matching estimator by first estimating the propensity score with a probit model. Next, we employ a matching procedure with replacement, where the treated firm-destination dyad is matched with its three closest non-treated matches in the propensity score in the same year.[39] The matching performs well, with the percentage bias being low overall, individually across variables, and hardly ever close to statistically significant (see Table A11 in the Online Appendix).

Although our interest is in the effects of guarantees, we identify several patterns with respect to the determinants of using them. A firm's trade experience is the primary factor associated with usage. Export experience increases the likelihood of usage, while relying on a larger share of imported products decreases it. The factor second-most associated with usage is the share of workers with a post-secondary education. A more educated workforce is linked to a higher probability of using guarantees. We also note that firms are less likely to use guarantees with respect to a foreign market with which there is a free trade agreement. However, they are more likely to use guarantees for exports to distant, riskier and less financially developed foreign destinations.[40]

[39] After matching, for the treated, we are left with 1,032 observations, 863 firm-destinations and 307 firms, while for the matched controls, we have 2,610 observations, 2,551 firm-destinations and 1,270 firms. Note that the dyads are not allowed to be their own controls in the years when they did not acquire guarantees, since matching is at the firm-destination-year level.

[40] Note that the results for the non-displayed two-digit firm logs of input costs and two-digit financial vulnerability variables are not statistically significant, and there is no statistically significant bias in the matching.

Results Across Firm Size

In Table 3, we display the differential average treatment effects on the treated across firm size from equation (5), after matching the firm-destination dyads that have guarantees (treated) at time t with similar dyads without such guarantees (controls). As discussed in the conceptual framework, we expect the effects to be particularly strong for smaller firms.

TABLE 3
Effects on firm performance by firm size.

	ATT, t (1)	t-stat (2)	ATT, t+1 (3)	t-stat (4)	ATT, t+2 (5)	t-stat (6)	Observations (7)
(A) *Change in probability of exporting*							
All firms	0.179	12.93	0.179	11.42	0.124	7.99	3,636
Micro and small firms	0.458	9.11	0.410	8.28	0.339	6.39	967
Medium firms	0.197	6.78	0.222	6.35	0.206	5.95	1,017
Large firms	0.114	7.56	0.121	6.73	0.077	4.18	1,624
(B) *Change in export values*							
All firms	1.723	9.59	1.396	6.38	0.722	2.83	2,686
Micro and small firms	3.567	8.50	3.122	4.33	3.230	4.01	524
Medium firms	1.396	3.92	1.649	4.29	1.464	2.74	759
Large firms	0.907	4.15	0.975	3.50	0.394	1.24	1,390
(C) *Change in value added*							
All firms	0.041	1.31	0.009	0.19	0.179	2.35	3,550
Micro and small firms	-0.278	-1.38	-0.353	-1.78	-0.176	-1.41	957
Medium firms	0.100	1.15	0.217	0.92	1.032	2.11	999
Large firms	0.055	1.60	0.003	0.07	0.073	1.69	1,573
(D) *Change in employment*							
All firms	0.118	1.63	0.113	1.61	0.067	0.76	3,633
Micro and small firms	-0.013	-0.06	-0.013	-0.06	-0.107	-0.33	967
Medium firms	-0.035	-0.26	0.283	1.14	0.303	1.08	1,017
Large firms	0.015	1.10	-0.005	-0.19	0.029	0.92	1,624
(E) *Change in value added/worker*							
All firms	-0.014	-1.21	-0.012	-1.09	0.003	1.48	3,550
Micro and small firms	-0.251	-1.32	-0.255	-1.44	-0.010	-0.71	957
Medium firms	3.100e-04	0.34	0.001	0.76	0.006	1.93	999
Large firms	1.468e-05	0.36	-5.970e-05	-1.03	1.505e-05	0.26	1,573

Notes: The table displays the estimates of the average treatment effects on the treated (ATT) by firm size, using three nearest-neighbour matching with replacement and DD with firm-/industry-year and destination-year fixed effects. Response is measured as the difference in the outcomes between $t-1$ (the year before treatment) and t, $t+1$, or $t+2$. All continuous variables are in log form, with 1e-7 added to export values to avoid truncation. A common support restriction has been imposed, and firms are required to be present from $t-1$ to $t+2$. Robust standard errors are used.

First, we note that the $LATT$ for exports from the fuzzy regression discontinuity around the threshold are qualitatively similar to the ATT from the DD matching estimator for all

firms.[41] The first two panels (A and B) of Table 3 display these average effects of the export credit guarantee treatment at time t on exports to the firm-destinations where guarantees are used.[42] Using guarantees is positively associated not only with contemporaneous (column 1) but also with subsequent (columns 3 and 5) export performance, relative to control firm-destinations, although the magnitude of the impact decreases somewhat over time.[43] We cautiously interpret the longevity of the impact as indicating that firms benefit from the guarantees in subsequent business deals with customers in the same foreign market (Lee and Stowe, 1993).

Importantly, we find that firm size matters for the treatment effects. Micro and small firms experience the largest increase in the probability of exporting to a foreign destination. For large firms, the effect on the probability of exporting is much smaller. Turning to the export values, the contemporaneous average treatment effect is more than 5 times larger in the change of percentage points for micro and small firms than for firms in general.[44] We interpret this inverse relation between the impacts on exports and the firm size as suggesting that guarantees facilitate the entry of less productive, less well-endowed and less internationalised firms into foreign destinations and substantially sustain their existing exports there.[45]

In terms of the characteristics of the foreign customers for which guarantees are provided, we find the strongest effects for exports to micro and small-sized foreign firms and only statistically significant effects for exports to the foreign customers that are domestically

[41] We also compared our results on export values at the firm-level with the findings of two other micro-level studies on the subject, with our estimates being in between them (Badinger and Url, 2013, Heiland and Yalcin, 2020). We note that our empirical setting is quite different from these studies, as it employs firm-destination-year panel data (16 years) for the population of firms, matches on a uniquely wide range of pre-treatment characteristics at several levels, and estimates the DD results of the guarantee treatment for all the treated firms.

[42] Remember that the treatment consists of a firm being a "first-time" user of a guarantee vis-a-vis a foreign destination, which means that the firm did not acquire any guarantee for this destination in the previous year.

[43] For the effects up to $t + 5$, see Table A12 in the Online Appendix.

[44] We use the exponentiation of the log difference in the fitted values when interpreting the sizeable effects on the export values in percentage points.

[45] Our finding of a negative link between the firm size and the firm-destination export values is qualitatively similar to the firm-level finding of Heiland and Yalcin (2020).

owned (see Tables A13-A14 in the Online Appendix).[46] Thus, we find the effects on exports to be inversely related not only to the size of the exporter but also to the size of the foreign buyer. These results suggest that guarantees are instrumental for both exporters and importers in mitigating information frictions in foreign trade.

In Panels C to E of Table 3, we revisit our Conjecture 2 on firm productivity and Corollary 1 on the effects on total firm performance by estimating the changes in value added, employment and labour productivity. Again, we find that the effects of guarantees on jobs and labour productivity are statistically insignificant, while the magnitudes are larger than from the FRDD. However, utilising guarantees leads to higher value added in the third year for medium-sized firms. Furthermore, an additional analysis demonstrates that guarantees contribute to total firm exports (see Table A15 in the Online Appendix).

Accordingly, by using our complementary identification strategy, we again find that guarantees assist firms in expanding along the extensive and intensive country margins of exports. The impacts on the firm-destination trade margins are inversely related to the size of exporters and foreign buyers. We also find an increase in the total firm-level exports but no such impact on other firm-level performance measures. We conclude that the effects of guarantees are heterogeneous both across performance measures and across firm size.

Results Across Firm Experience

As hypothesised in our conceptual framework, and as confirmed in Table 3, smaller firms experience relatively strong export effects from guarantees.[47] However, being a micro or small firm is no longer wholly synonymous with limited export experience, including experience using guarantees. Some start-ups begin exporting almost from their inception and may also export early to multiple countries – so-called born global firms– while other small firms

[46] These effects are from a rolling cross-section of exporting firm-destinations and their foreign buyers.

[47] As mentioned above, because they are less productive, smaller firms may generally be less able to bear the costs of trade, particularly when such costs are distributed over previously non-existent or small-scale trade. Smaller firms are also known to have difficulties in attracting external financing.

export much later, and still other small firms rarely or never export. Being small is therefore an imperfect proxy for limited export experience, the associated information issues, limited access to trade finance, a low diversification of the risk portfolio, and the use of export credit guarantees. Therefore, we re-examine the impact of firm size by focusing on *inexperience* in using guarantees. For this purpose, we exploit the panel dimension of our data; explore the effects when large export transactions are excluded; and modify the construction of the treatment and control groups.[48] In Table 4, we perform these additional estimations where we distinguish between firms without and with extensive experience using guarantees, and we perform estimations for smaller transactions.

First, below the results for all firms, we display the findings from limiting the analysis to the firms that have never used guarantees for exports to any country. The treated are the firm-destination dyads that acquired guarantees in year t, and the controls are the dyads that did not acquire guarantees. The average treatment effects are generally stronger in these estimations, where neither the treated nor the controls had ever used guarantees to any market. For example, the association between using guarantees for the first time to any country and the probability of exporting to a foreign destination is especially large; in fact, it is more than 80 percent larger in the change of percentage points than for all firms. The results for first-time users to a specific destination are on par with the results for all firms with respect to export probability but larger in terms of the export values and substantially larger in terms of value added, with the latter result being statistically significant in $t+2$. Additionally, we find that labour productivity in $t+1$ increases for the first-time users of guarantees for exports to a specific destination.

Second, we compare the firms that continuously use guarantees with the firms that cease using them in year t. Throughout, the treatment effects are substantially smaller in this setting. Whether or not to acquire a guarantee appears to be more important to the firms

[48] Recall that in the DD matching results above on firm size, the treated firms have acquired a guarantee to a destination in t but not in $t-1$, whereas the control firms neither had guarantees in t, nor in $t-1$.

TABLE 4

Effects across the guarantee experience and for smaller transactions.

	ATT, t (1)	t-stat (2)	ATT, t+1 (3)	t-stat (4)	ATT, t+2 (5)	t-stat (6)	Observations (7)
(A) Change in the probability of exporting							
All firms	0.179	12.93	0.179	11.42	0.124	7.99	3,636
Use guarantees for the first time	0.329	8.92	0.377	8.89	0.326	7.26	1,031
Use guarantees for the first time to a destination	0.186	12.26	0.174	10.29	0.146	8.03	3,161
Continually using guarantees	0.101	14.67	0.101	13.04	0.086	10.60	11,997
Excluding the top-10 largest transactions	0.178	12.49	0.167	10.64	0.124	7.86	3,611
Excluding transactions > SEK 500 mn	0.195	12.99	0.193	11.74	0.119	7.59	3,589
(B) *Change in export values*							
All firms	1.723	9.59	1.396	6.38	0.722	2.83	2,686
Use guarantees for the first time	3.162	6.74	3.138	4.99	2.550	3.60	658
Use guarantees for the first time to a destination	2.026	10.13	1.586	6.75	1.269	4.50	2,298
Continually using guarantees	0.655	7.39	0.395	3.41	0.218	1.60	9,297
Excluding the top-10 largest transactions	1.737	9.59	1.281	6.05	0.769	3.02	2,692
Excluding transactions > SEK 500 mn	1.871	10.44	1.644	7.39	0.870	3.38	2,666
(C) *Change in value added*							
All firms	0.041	1.31	0.009	0.19	0.179	2.35	3,550
Use guarantees for the first time	0.093	1.53	0.125	1.80	0.112	1.49	1,017
Use guarantees for the first time to a destination	0.071	2.05	0.324	2.13	0.520	2.90	3,074
Continually using guarantees	0.019	1.35	0.058	1.81	0.137	3.24	11,691
Excluding the top-10 largest transactions	0.062	2.05	0.053	0.92	0.217	2.52	3,528
Excluding transactions > SEK 500 mn	0.050	1.56	0.053	0.92	0.240	2.44	3,499
(D) *Change in employment*							
All firms	0.118	1.63	0.113	1.61	0.067	0.76	3,633
Use guarantees for the first time	0.366	1.37	0.509	1.78	0.176	0.91	1,028
Use guarantees for the first time to a destination	0.071	1.12	0.158	1.99	0.070	0.70	3,151
Continually using guarantees	0.040	1.59	0.077	2.45	0.013	0.33	11,978
Excluding the top-10 largest transactions	0.072	1.00	0.055	0.92	0.070	0.82	3,604
Excluding transactions > SEK 500 mn	0.138	2.10	0.196	2.90	0.172	1.83	3,585
(E) *Change in value added/worker*							
All firms	-0.014	-1.21	-0.012	-1.09	0.003	1.48	3,550
Use guarantees for the first time	0.016	1.80	0.021	1.91	0.017	1.35	1,017
Use guarantees for the first time to a destination	0.002	1.04	0.006	2.43	0.006	2.10	3,074
Continually using guarantees	7.424e-04	0.78	4.653e-04	0.49	0.002	1.77	11,691
Excluding the top-10 largest transactions	-0.004	-0.59	-0.002	-0.25	0.005	2.09	3,517
Excluding transactions > SEK 500 mn	9.026e-04	0.73	0.004	2.08	0.004	1.47	3,499

Notes: The table displays the estimates of the average treatment effects on the treated (ATT) across the guarantee experience and for smaller transactions by using three nearest-neighbour matching with replacement and DD with firm-/industry-year and destination-year fixed effects. The response is measured as the difference in the outcomes between $t-1$ (the year before treatment) and t, $t+1$, or $t+2$. All continuous variables are in log form, with 1e-7 added to export values to avoid truncation. A common support restriction has been imposed, and firms are required to be present from $t-1$ to $t+2$. Robust standard errors are used. The results when excluding transactions of > SEK 500 mn should be interpreted with some caution since the export contract value is not provided for all transactions.

that are less accustomed to using guarantees than to continuous users.

Finally, we exclude the largest transactions from the observations of the treatment group. The impacts are still similar to those in the main results.[49] The only difference is that the impact on jobs is larger and statistically significant when we limit the analysis to transactions below SEK 500 million.

We interpret these results as further confirming our expectation that typically smaller firms – which are less productive, more liquidity constrained, have limited access to trade finance, have little experience with exports and guarantees, and engage in low-scale exports if they export at all – benefit the most from export credit guarantees. Additionally, we note the positive impacts on value added for inexperienced users of guarantees in a foreign market and on jobs for low-scale exports. We cautiously interpret the impacts on value added and jobs as indicating the potentially instrumental role of guarantees in reducing the information frictions in trade for firms with little experience and low-scale exports. By using guarantees, such firms may be less hesitant not only to export but also to hire and increase their value added.

Results Across Firm Industry

Next, we analyse the impacts across the industries associated with different levels of tangible assets.[50] Manufacturing and wholesale represent a dominant share not only of the firms that use guarantees but also of the private sector in Sweden in general.[51] However, the micro-level impact of guarantees across industries is unknown.

In performing the first firm-destination-/firm-level estimation results across industries, we generally find the impacts on exports to be larger for firms in service industries than for firms in manufacturing (Table A17 of the Online Appendix). Firms in the extended business

[49] Excluding the 5, 10 or 20 largest transactions produces nearly identical results (the results are available upon request).

[50] Our sectoral classification for this analysis is presented in Table A16 of the Online Appendix.

[51] The manufacturing industry is still overrepresented among the firms that use guarantees.

service industry category (professional, scientific and technical industries and other service industries) experience an especially strong increase in the bilateral probability of exporting and in export values in t and $t+1$. However, the positive impacts are not restricted to service industries. Across all four of our broad categories of industries, there are positive and significant associations between guarantees and export performance.

We find it interesting that guarantees have a relatively large impact on the firm-destination export values in service industries. These industries are relatively constrained in terms of collateral and not as experienced in trade. Therefore, we expect the impact of guarantees to be particularly strong.[52] However, on the one hand, this might be a somewhat unexpected result since export credit guarantees have traditionally been used for exports of goods. On the other hand, this finding may be less surprising when considering the role of business service firms for the manufacturing industries in Sweden and also considering the trend in manufacturing of outsourcing non-core service activities (Lodefalk, 2016). The relatively large impact in services industries may also be related to foreign business deals potentially consisting of bundles of goods and services, with services provided both in tandem with delivery of goods and in the process of maintenance.

Mechanisms

The patterns above are congruent with our conceptual framework, where the institution of export credit guarantees assists firms in overcoming information frictions in foreign trade and thereby promotes exports and potentially also overall firm performance. However, to understand the mechanisms behind these microeconomic effects, we carry out three additional sets of estimations.

If it is the case that guarantees address exporters' risk of foreign buyer default and/or ease liquidity constraints, then we expect the effects to be stronger in the presence of a negative

[52] Recently, Heiland and Yalcin (2020) found that the within-firm link from guarantees to reporting higher-than-expected exports is positively associated with liquidity constraints.

macroeconomic shock, which raises uncertainty, reduces demand and constricts external finance. Foreign customers are less likely to both order and be able to pay for ordered products. Exporters are more likely to face liquidity problems. Luckily, we can investigate this by exploiting the fact that the financial crisis in the late 2000s is included in our dataset. Additionally, such results could further corroborate a causal effect on firm performance since the crisis was an event exogenous to Swedish firms.[53]

We estimate the average treatment effect on the treated of using guarantees before (2000-2007), during (2008-2012) and after the financial crisis (2013-2015) (Table A18 of the Online Appendix). We find the effects on the intensive firm-destination margin to be 30 percent larger during the crisis, on average, than the effects in the non-crisis period, while the effects on the extensive margin hardly differ in magnitude across the time periods. Moreover, there is a positive, substantial and statistically significant contemporaneous impact on value added during the crisis. The value added impact during the crisis stands out compared with the much smaller positive pre-crisis and negative post-crisis impacts. Overall, the results seem to corroborate a stronger effect of guarantees on firm performance during negative macroeconomic shocks. We interpret this effect to mean that guarantees address the risks of default and/or financial constraints to promote firms' exports and value added.[54]

Next, we pay attention to the issue of whether guarantees increase trade either by creating new trade, as hypothesised in the first corollary of our first conjecture, or simply by diverting trade from one foreign country to another. This matters to the interpretation of how guarantees affect firm trade – by either mitigating information frictions or creating new market distortions. Basically, there are two potential sources of a trade redirecting effect of guarantees. First, hypothetically, government-backed guarantees could "crowd out" private insurers or foreign competitors, which results in firms choosing to redirect exports. However,

[53]For a similar test, see Felbermayr *et al.* (2012).

[54]The results corroborate the findings of Felbermayr *et al.* (2012) for a German sample of larger firms. They also relate to the finding of Heiland and Yalcin (2020) that the within-firm link from guarantees to reporting higher than expected exports is positively associated with the market-wide refinancing cost.

to some extent, intergovernmental rules in the WTO and an OECD-supported arrangement circumscribe such crowding out. Second, guarantees could divert exports from more to less productive exporters or from safer to riskier foreign markets. We have therefore analysed the export effect across firms' productivity distribution and across debtor and country risk in their foreign markets (Tables A19-A20 in the Online Appendix). Overall, our results do not support the notion that guarantees divert exports to low-productivity firms or to riskier foreign customers and markets.

Finally, we investigate the specific mechanisms at play from guarantees to exports. Do guarantees promote exports primarily by addressing the default risk (Corollary 2a) or by easing liquidity constraints (Corollary 2b)? To this end, we examine the firm-destination specificity of the effects. We compare the impact of a firm acquiring guarantees for a specific country on exports to the same country compared with the impact on exports to other countries in the same region and on exports to any other region in the world. Because guarantees insure specific transactions against foreign buyers' default, if there is a causal link from guarantees to exports, then we expect the impact to be the strongest for exports to the country for which the guarantee was acquired (Corollary 2a). In Table 5, we display the results. We find such a pattern. The impact on export performance vis-a-vis a country is positive but weaker for acquiring guarantees for neighbouring countries and still weaker regarding guarantees for the rest of the world.[55]

How should we interpret the finding that a firm that acquires a guarantee for a specific foreign market also to a certain extent seems to benefit from exports to other countries or regions? Building on our previous conceptual discussion, we view these results as indicating that a firm's acquisition of a guarantee assists the firm not only in handling the risk of buyer's default for this contract but also in handling liquidity constraints (Corollary 2b).

Overall, we regard the results in Table 5 as further evidence that guarantees have a causal

[55]The average impact on the export probability and the export value of the country from the treatment for the neighbouring and distant regions, across the estimates, is in the range of 9% and 72% of the guarantee-specific-country impacts, respectively.

TABLE 5
Effects on firm export performance by region.

	ATT, t (1)	t-stat (2)	ATT, T+1 (3)	t-stat (4)	ATT, t+2 (5)	t-stat (6)	Observations (7)
(A) *Change in probability of export*							
First time use of guarantees	0.329	8.92	0.377	8.89	0.326	7.26	1,031
First time use of guarantees in the same region	0.238	38.87	0.134	20.68	0.126	19.39	23,829
First time use of guarantees in other regions	0.031	16.83	0.053	25.24	0.056	26.60	185,427
(B) *Change in export values*							
First time use of guarantees	3.162	6.74	3.138	4.99	2.550	3.60	658
First time use of guarantees in the same region	1.868	26.51	0.974	11.13	1.113	11.16	18,032
First time use of guarantees in other regions	1.047	19.87	0.838	12.74	0.869	11.42	34,816

Notes: The table displays the estimates of the average treatment effects on the treated (ATT) in terms of exports, by region, by using three nearest-neighbour matching with replacement and DD with firm-/industry-year and destination-year fixed effects. The response is measured as the difference in the outcomes between $t-1$ (the year before treatment) and t, $t+1$, or $t+2$. Export values are in log form, with 1e-7 added to avoid truncation. A common support restriction has been imposed, and firms are required to be present from $t-1$ to $t+2$. Robust standard errors are used. We divide the regions into Europe, other OECD, Asia, Africa, Latin America, Middle East and Islands.

effect on firm exports, mainly by addressing the default risk, which thereby increases firm-destination exports; simultaneously, guarantees have a weak causal effect on firm exports because they address liquidity constraints.

5.3. Further Robustness Analysis

In this section, we perform a pseudo-treatment test to further demonstrate the robustness of the causal identification in this paper. We conclude by analysing the robustness of our complementary estimator to the issues of model specification and matching.

Pseudo Treatment Test

Although our results from the FRDD and from a complementary DD matching approach both suggest that guarantees contribute to firms' bilateral exports, it would be reassuring for identification if a "pseudo-treatment" did not affect firms' bilateral exports. In essence, this means testing the so-called unconfoundedness assumption – namely, that the treatment is exogenous (Imbens and Wooldridge, 2009). We construct the "pseudo-treatment" by re-coding the data such that the treatment appears to occur in destination-years when there was, in fact, no treatment. Then, we apply the DD matching estimator. If identification holds, then we should verify no impacts. Overall, the estimated outcomes of the pseudo-

treatment are not even weakly significantly different between the firms that are treated and the firms that are not treated (Table A21). In magnitudes, the insignificant results are much smaller than or even have the opposite sign to the results of the main estimations.

DD Matching Issues

Finally, we analyse the robustness of our complementary DD matching estimations by comparing their results with those from using a naive model, a first nearest-neighbour matching model and a kernel matching model. In terms of specification, the naive model only matches treated and control firm-destination dyads on employment, turnover, firm age, physical capital stock, export and import status, and their intensities. Comparing our preferred DD matching results with the findings of the naive model suggests that omitting a wide range of firm, industry and country characteristics may severely bias the results (Table A22 in the Online Appendix). The naive model overestimates the impact on export values, whereas it generally underestimates the subsequent impact on value added and employment, and it produces statistically significant results for labour productivity.[56] Turning to the matching issues, we compare our complementary DD matching results with the results when including only the first nearest neighbour in matching and when using a kernel matching estimator. Reassuringly, we find that the key conclusions from the DD matching specification are also robust to the matching issues.

6. Concluding Remarks

Trade has been risky throughout human history, and long-distance trade is particularly risky. Traders, kin or middlemen have carried out trade, and arrangements have been made for protection and contract enforcement. With the event of institutions such as the community responsibility system in European communes and subsequent national courts, domestic trade

[56] Compared to the naive model, both the AIC and BIC values are much smaller in the main DD matching specification, which indicates the preference for the main DD specification in terms of both goodness of fit and the avoidance of overfitting.

flourished (Greif, 2006). However, only a century ago, governments established an institution to facilitate foreign trade – specifically, government-backed export credit guarantees. Since the onset of the financial crisis, the provision of export credit guarantees has doubled. We investigate the role of this institution. We present novel and robust causal evidence from Sweden on the effects of guarantees on firm performance. Our contribution is enabled by employing a quasi-experimental design and the most granular and exhaustive longitudinal data to date in the literature.

Our results from the first quasi-natural experiment in the literature demonstrate that guarantees have a causal effect on exports at both the extensive and intensive firm-country margins. However, we find no general causal effects of guarantees on firm performance in terms of jobs, value added and labour productivity. To investigate the consistency of these results with the idea that guarantees mitigate information frictions in trade, we explore several dimensions of our extraordinarily detailed data. We find that guarantees affect firms heterogeneously. Guarantees disproportionately assist the exports of smaller firms and service firms and the exports to small foreign firms. We expect these firms to be particularly disadvantaged by information frictions in foreign trade. The export effect was also stronger during the financial crisis, when uncertainty abounded and demand and external financing were constricted. In addition, we find that the firms that start to use guarantees for low-scale exports hire more workers and that the firms that start to use guarantees for the first time in exports to a market experience an increase in value added. Finally, we examine the specific mechanisms at play from guarantees to exports by focusing on the destination market specificity of the effects. We find that exports mainly increase to the destinations for which the guarantees were given. However, exports also increase to other destinations, and this additional impact diminishes with the distance from the intended market. We interpret these results as indicating that guarantees primarily address the default risk that exporters face and secondarily ease liquidity constraints. Overall, our results suggest that export credit guarantees mitigate information frictions in trade.

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