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Export Subsidies in Emerging Markets During the Great Trade Collapse

Jorge F. Chávez
Universidad del Pacífico

Antonio Cusato Novelli
Universidad del Pacífico

Max Perez Leon
NERA Economic Consulting Washington DC

Abstract

We estimate the impact of an export subsidy program in Peru during the Great Trade Collapse. Historically, Peruvian firms have accessed a five percent subsidy on the FOB value of exports. The rate increased up to eight percent during the crisis. For the years 2009-2010, we find that supported firms experienced a lower decline in export values and a lower exit probability of product-destination markets.

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Contact: Jorge F. Chávez - jf.chavezco@up.edu.pe, Antonio Cusato Novelli - a.cusaton@up.edu.pe, Max Perez Leon - max.perezleon@nera.com.

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1. Introduction

During the Great Recession of 2008-2009, the contraction of global trade was far greater than the contraction of economic activity. Naturally, the literature has paid a lot of attention to the sources of the Great Trade Collapse (Alessandria et al. 2010; Bussière et al. 2013; Eaton et al. 2016; among others). In terms of policy, there is little published evidence on how governments helped firms weather the crisis.[1] To the best of our knowledge, no studies look at policy responses that involve large amounts of direct financial support to exporters. This should come as no surprise, considering that the World Trade Organization has formally prohibited export subsidies (linked to the value of exports) since 2003 (except for least-developed countries). Defever et al. (2020) is the only paper that studies a small-scale export subsidy program in a least-developed country (Nepal), using detailed firm and transaction level information, for the period after the Great Trade Collapse (2012-2014). The Peruvian subsidy program, formally a large-scale drawback program, provides a laboratory in which to study the effects of export subsidies in a middle-income country during the crisis.

To analyze the relationship between subsidies and export performance—measured as the value of exports and the likelihood of exiting international markets—we use a matching difference-in-differences (DiD) approach, as in the export promotion literature (Van Biesebroeck et al. 2016); or, in general, the program evaluation literature (Imbens and Wooldridge 2009). This approach aims to overcome program endogeneity and rely on observable firm characteristics to identify the effects of the subsidy.

Our results show that subsidies have a positive impact on the intensive and extensive margins for the crisis window (2009-2010); in a context where the historical subsidy rate of 5 percent on the FOB value of exports was raised to 8 percent in 2009, and went back to its initial level in 2011. In terms of the intensive margin, treated firms, compared to non-treated firms, posted a higher export growth rate of 22 percentage points (from a baseline of -20 percent growth). This effect was calculated for those transactions in which firms keep exporting. Regarding the extensive margin, the exit probability was reduced by 0.09 percentage points for benefited firms (from a baseline rate of 0.30). As noted above, the crisis window overlaps with the policy reaction, and our estimations cannot disentangle the effect from accessing the subsidy and the subsidy rate increase.

Interestingly, when we break down the analysis per year, we find effects on the yearly change in export values in 2009 and 2010, but not in 2008 or 2011. Impact on the exit probability is observed for 2008, 2009 and 2010, but not for 2011. The fact that we only find effects on the extensive margin in 2008 is unsurprising. Most of the literature on export promotion has only found effects on the extensive margin (Makioka 2019). Moreover, the lack of effects during 2011 can be explained by program changes. We document that in this year (i) there were significant delays in accessing the funds by those firms that finally received the subsidy; (ii) the number of benefited firms remained stable; (iii) the subsidy rate returned to its historical level of five percent; and (iv) the program budget was significantly reduced.

[1] During the crisis, there were concerns about the potential implementation of protectionist measures that did not fully materialize (Bown and Crowley 2013). In terms of policy responses, Van Biesebroeck et al. (2016) analyze the case of programs implemented by export promotion agencies aimed at reducing information problems (were funds are limited).

With regard to our contribution, ours is the first paper to evaluate a large-scale export subsidy program in a middle-income country, and also the first to analyze this type of program in the context of a crisis. As such, we complement the study of Defever et al. (2020) for Nepal, as well as a large literature that focuses on the impact of 'soft' interventions of export promotion agencies in middle or advanced economies (Van Biesebroeck et al. 2016, Makioka 2019).[2] The provision of evidence about export subsidies is important given the lack of empirical studies (Defever et al. 2020). For example, the lastest Handbook of Commercial Policy (Bagwell and Staiger 2016) is silent about empirical studies on this topic (see the chapters of Bown and Crowley (2016) and Lee (2016)). Finally, unlike other historical export subsidy schemes (Helmers and Trofimenko 2013), the Peruvian scheme is relatively simple, since the benefit comes from the flat rate on exports and the main requirement is the payment of a positive amount in import duties.

2. Data and The Program

We have compiled a new comprehensive database in relation to universe of Peruvian export transactions that were subject to the subsidy for the period of analysis. This information is combined with customs transaction level data disaggregated at the year-firm-product(HS6)-destination level. We analyze the effects of the program using transaction level data at the firm-product-destination-year (ipd,t) level. We follow the literature and define treatment at the firm-year level (i,t), even though the subsidy is requested per transaction (ipd,t). In our estimations, a firm is considered treated if it received the subsidy in at least one transaction during the year, conditional on not having accessed the benefit the previous year. We choose to define treatment at the firm level because funds received for a particular transaction are fungible, and ultimately benefit the firm.

The subsidy program is technically a drawback system intended to refund import tariffs to exporting firms. The imported inputs (that paid import tariffs) must be incorporated in the export products. Peru follows a simplified rule that does not link the import tariffs paid in a particular import transaction with the refund. Instead, the exporting firm recovers a percentage of its export FOB value (five percent up to 2008). Peru's subsidy rate increased to eight percent in 2009 and returned to five percent in January 2011 (with an intermediate drop to 6.5% in July 2010). To receive the subsidy, a firm has to show that it (i) pays a positive amount in import duties associated with imported inputs used in export products; and (ii) the exported goods are not part of an exclusion list.[3] The program is a subsidy because it provides exporters with more funds than what they paid in tariffs. Under a very conservative back-of-the-envelope calculation for 2009, we find that half (10 percent)

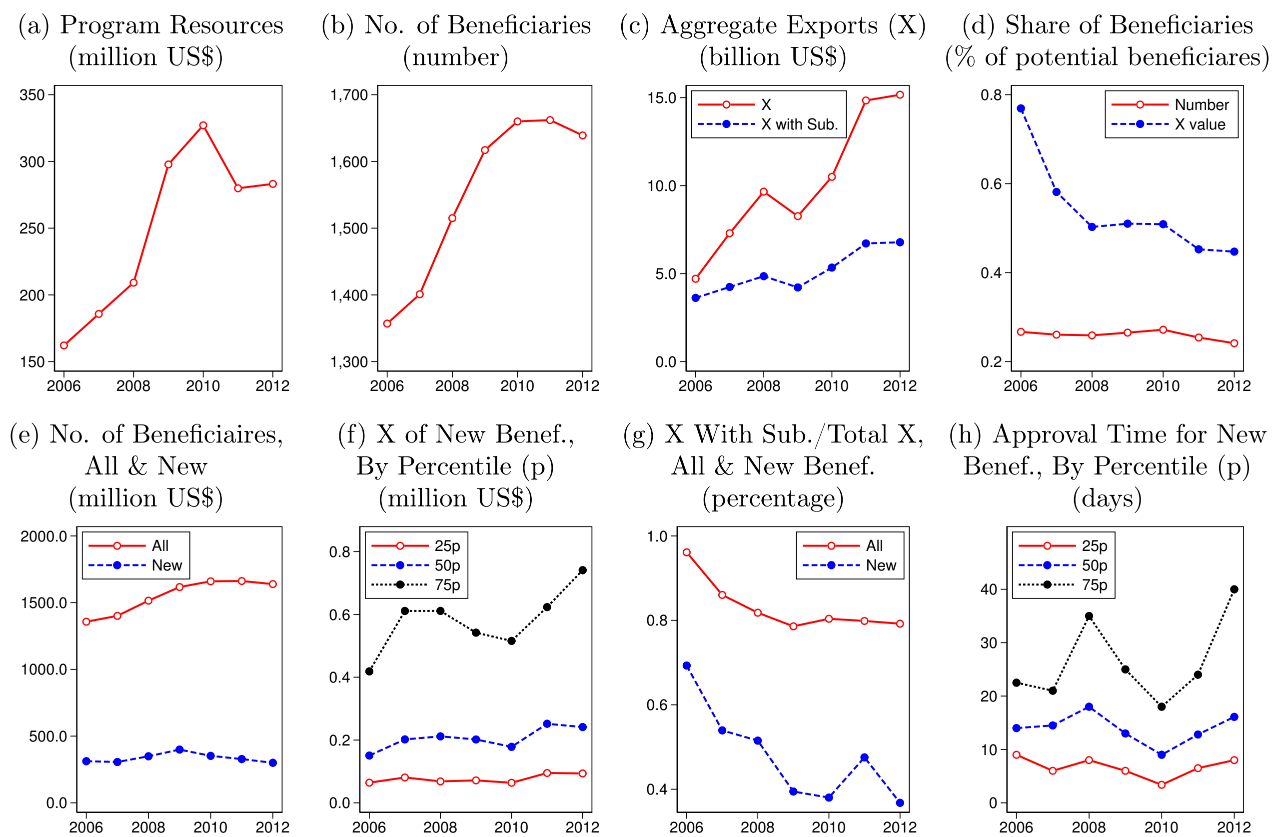
[2] Previous studies using less-detailed information about export subsidies include Das et al. 2007, Helmers and Trofimenko (2013) and Defever and Riaño (2017).

[3] As regards condition (i), imported inputs (for which tariffs were paid) can be imported directly by the firm producing the good; can be acquired from third parties (local importers of the input); or, as a final option, the exporter can buy locally produced intermediate goods that contain imported inputs (for which tariffs were paid). Condition (ii) implies that beneficiaries are producers of non-commodity exports. The exclusion list includes 275 different types of products. For 2008, exports included on the list represent US$ 19.6 billion. Mineral products, metals, stone and glass account for 97.9 percent of the value of the products excluded. Products not included in the list, which are our universe of analysis, totaled US$ 9.7 billion in exports.

of the beneficiary firms received funds of at least 3.5 (55) times the import duties paid (see Appendix 1).

The program is relatively large. Panel (a) of Figure 1 shows that during 2009-2011, total subsidies represented around US$ 300 million per year. This number represents 0.13 percent of Peruvian GDP and one percent of total tax collection. Also, it is equivalent to the funding of the large scale conditional cash transfer program implemented in Peru to fight poverty (*Juntos*). In the case of Nepal, Defever et al. (2020) report that the subsidy program represents less than US$ 4 million per year. Panel (b) shows a smooth increase in the number of firms that access the benefit, even though in 2009 the subsidy rate was increased. Between 2009 and 2010, there were approximately 1,600 firms that accessed the subsidy. By contrast, Defever et al. (2020) report 151 firms benefiting in the best year, from a universe of around 900 eligible exporters. Panel (c) shows the value of aggregate exports for products eligible for the benefit. For 2008, exports outside the exclusion list totaled US$ 9.7 billion, while US$ 4.9 billion in exports received the subsidy. Importantly, aggregate exports did not recover their previous trend until 2011. Hence, we define our crisis window as 2009 and 2010. For the crisis window, benefited firms represent 25% and 50% of the total number of firms and export value that could potentially request the subsidy, as shown in panel (d).

Figure 1. Program Characteristics, 2006-2012.



Notes: 'New Beneficiaries' denote those firms that access the subsidy, conditional on not having received funds in the previous period. This is the definition of treated firms in our estimations.

As described in detail in the next section, our focus of analysis is the firms that access the benefit, conditional on not having received the subsidy in the previous period. We denote this group of firms as 'new' firms. Panel (e) shows that the number of 'new' firms fluctuated between 300 and 400 for the period 2006-2012, following a U shaped pattern, with the highest number recorded in 2009. Panel (d) shows the evolution of exports for three cases of 'new' firms: those in the 25th, 50th and 75th percentiles of the distribution of newly benefited firms per year. Since we compare different firms per year, the figure indicates that new entrants were smaller (in terms of export values) during the crisis window (2009-2010). A second consideration for treatment in our analysis, is that our definition is at the firm level (i,t), whereas the subsidy is requested per transaction (ipd,t). Hence, a treated firm receives subsidies for some transactions and not for others. Panel (g) shows that all the firms received the subsidy for more than 80 percent of their transactions (in terms of value) in each of the years analyzed, while 'new' firms benefitted across approximately 50% of their transactions during 2006-2012.

The last panel (h) in Figure 1 shows the approval time of the application process for 'new' firms, again for the 25th, 50th, and 75th percentiles of the entrant firms per year. The figure indicates that the time to access the benefit decreased during 2009-2010, but since 2011 the numbers have reversed. The increasing difficulties in accessing the benefit since 2011 are consistent with the policymakers' decision to reduce the subsidy rate, which returned to its 5% historical level that year. Moreover, the number of beneficiaries stopped growing in 2011, and in 2012 there was a small reduction, as shown in panel (b). Hence, its seems that the policymakers' intention was to help firms, in terms of program access and resources, only during the crisis window. The policymakers' decision to discourage the access to the program—by reducing the subsidy rate and increasing the approval times—can be explained by the fact that the subsidy system is highly regressive. Ranking the firms according to funds received, the first 200 firms (out of 1,500-1,600) represent 83 percent of total subsidy funds in 2001, and 69 percent in 2013.

3. Empirical Strategy

The matching DiD analysis assumes that the selection-on-observables assumption holds. We use propensity score matching, as in most of the literature on export promotion (for example see Volpe Martincus and Carballo 2008 or Munch and Schaur 2018), and in addition we implement a doubly-robust matching estimator (Wooldridge 2007), as in Van Biesebroeck et al. (2016) or Defever et al. (2020).

Our estimates are based on transaction level data (firm-product-destination or ipd), but the definition of treatment is at the firm level (i). Moreover, there are different degrees of subsidy intensity, since not all the firms benefit for all of their export transactions. The indicator function $\mathbb{1}\left[sub_{ipd,t} > 0\right]$ takes the value of one if the transaction for firm i to market pd receives the subsidy (zero otherwise). On this basis, we can define the subsidy intensity as the ratio $\omega_{i,t} = \sum\left(\mathbb{1}\left[sub_{ipd,t} > 0\right] X_{ipd,t} + \left(1 - \mathbb{1}\left[sub_{ipd,t} > 0\right]\right) X_{ipd,t}\right) / \sum X_{ipd,t}$. In our estimations, we classify firms as treated if $\omega_{i,t} > 0$, as a first requirement for being treated, while the control firms simply did not receive anything $(\omega_{i,t} = 0)$.

Firms can also be classified into three groups according to total annual sales (local and exports). Small firms are those with sales below US$ 118,000 per year; medium-sized firms present sales between US$ 118,000 and US$ 1,003,000; and large firms record sales above

US$ 1,003,000 (these thresholds are based on the classifications set by the Peruvian tax authority).

We use a matching DiD approach (Heckman et al. 1997) to compare the change in exports before and after firms access the subsidy with that of the matched control firms, using propensity score matching (Psm).[4] Matching accounts for the imbalances in the distribution of covariates between the beneficiary and non-beneficiary firms. This approach means that time-invariant effects are eliminated. The identifying assumption is that we can control for all variables that affect firm selection and export performance (selection-on-observables assumption), such that differences in the comparison are due to treatment and not to endogenous selection (Heckman et al. 1999). Formally, the estimator is given by

$$\delta = \sum_{i \in \{I^t, Z\}} \left[\Delta x_{ipd,t} - \sum_{j \in \{I^c, Z\}} \hat{v}_{ij} \Delta x_{jpd,t} \right] v_{ij} \tag{1}$$

Where t denotes time, Z is the common support, I^t is the set of treated firms, I^c is the control group, i are those treated firms that belong to the common support, j refers to the control firms that belong to the common support, $\hat{v}$ is the weight associated with comparison of the observation of firm j (which applies to the corresponding product-destination pd markets) with an observation corresponding to firm i ($\hat{v}$ depends on the matching estimator), v is the weight for the re-weighting that allows reconstruction of the outcome distribution for the treated, and $\Delta x_{ipd,t}$ is the change in the export performance indicator for firm i (or alternatively j) for a market pd during period t. Note that we define treatment at the firm level, and fort consistence the match is also carried out at the firm level.

To make sure that our results are correct in the presence of misspecification, we also use the inverse-probability-weighted regression-adjustment estimator ($Ipwra$), a doubly-robust estimator. The advantage of following this approach is that even though one of the two models (the treatment model or the outcome model) can be misspecified, we still obtain correct results for the treatment effect.

To study the intensive margin, we analyze $\ln\left(X_{ipd,t}\right) - \ln\left(X_{ipd,t-1}\right)$, or the change in exports between years t and $t-1$. In terms of the extensive margin, we focus on the change in export status, or the probability of market exit, constructing an exit indicator $\mathbb{1}\left(X_{ipd,t+1} = 0 | X_{ipd,t} > 0\right)$ that takes the value of one if firm i did not export to the market pd in $t+1$, conditional on positive exports in t for the same ipd. We follow Van Biesebroeck et al. (2016) by estimating (1) on a cross-section of observations. For the intensive margin, in our baseline estimation the cross-section refers to changes in transactions carried out during two years, 2009 and 2010 (or the sample includes $\ln\left(X_{ipd,2009}\right) - \ln\left(X_{ipd,2008}\right)$ and $\ln\left(X_{ipd,2010}\right) - \ln\left(X_{ipd,2009}\right)$ observations). As shown in Figure 1, this period is our crisis window (Van Biesebroeck et al. (2016) also consider 2010 as part of the crisis). For the extensive margin, the cross-section analyzes the pd transactions of firms that exported in 2008 and not in 2009 ($\mathbb{1}\left(X_{ipd,2009} = 0 | X_{ipd,2008} > 0\right)$), and the same for a positive value in 2009 and zero in 2010 ($\mathbb{1}\left(X_{ipd,2010} = 0 | X_{ipd,2009} > 0\right)$).

[4] The estimator is the nearest-neighbor (NN) with five neighbors. The results are robust to carrying out the same estimation with a different number of neighbors, as well as the kernel matching estimator (results are available upon request).

As regards the definition of treatment, we focus on those firms treated in t that were not treated in $t-1$, as in, for example, Munch and Schaur (2018). This definition is more strict than the one used in Van Biesebroeck et al. (2016), which considers treatment as access to the program for a window of time around the crisis (for example, to analyze the impact during 2010, a firm is considered treated if it received support in at least one year between 2007 and 2010). In like manner, Defever et al. (2020) define treatment if a firm receives the subsidy any year after the beginning of the program. To be more specific about our definition, in the case of the intensive margin we consider treated firms in 2009 if $\omega_{i,2009} > 0$ and $\omega_{i,2008} = 0$, which we denote by $\mathbb{T}(\omega_{i,2009} > 0 | \omega_{i,2008} = 0)$, and treated in 2010 when $\mathbb{T}(\omega_{i,2010} > 0 | \omega_{i,2009} = 0)$. For the extensive margin we analyze transaction exits in 2009 and 2010, with positive exports and treatment in 2008 and 2009 respectively. The corresponding definitions are $\mathbb{T}(\omega_{i,2008} > 0 | \omega_{i,2007} = 0)$ in the first case, and $\mathbb{T}(\omega_{i,2009} > 0 | \omega_{i,2008} = 0)$ in the second.

As to the covariates that explain program participation, there is evidence that points out that different degrees of internationalization tend to generate different needs in terms of government support (see Volpe Martincus and Carballo 2008 for a discussion). Hence, all of the papers in the export promotion literature have used pre-treatment export performance indicators to predict program participation. We include two measures of export/firm size two years before treatment, which are (i) the log of exports in $t-2$ and (ii) the number of workers in $t-2$, as well as one proxy for a common trend in export performance one year before treatment, which is (iii) the change in the number of products the firm exports between $t-2$ and $t-1$.[5] In addition, we include a dummy that indicates whether the firm is a direct importer in $t-2$.

A potential pitfall of the approach is not being able to control for crucial firm-level time varying variables, such as productivity (the selection-on-observables assumption is untestable). The literature has been clear in recognizing that this may lead to upward biases (Görg et al. 2008, Volpe Martincus and Carballo 2008, Volpe Martincus and Carballo 2010, Van Biesebroeck et al. 2016). In the specific case of the subsidy program, the exporter had to actively apply for the subsidy and there are associated costs (including the compliance costs to prove that the product was eligible and to avoid potential sanctions). More productive firms can easily absorb these costs and, naturally, productivity is an important characteristic. However, since our window of analysis is very short, we assume that firm productivity changes slowly over time, as in the export promotion literature (Volpe Martincus and Carballo 2010). Since time-invariant effects are eliminated in the matching DiD analysis, we feel comfortable about following the assumption typically imposed in the literature.

4. Results

The first subsection shows the estimations, while the second present the robustness checks.

4.1 Baseline Results

The matching estimator aims to compare treated firms with firms that have similar characteristics, but which did not receive the subsidy. Table 1 shows the estimates of the selection

[5] Van Biesebroeck et al. (2016) emphasizes the importance of controlling for pre-treatment trends for the crisis.

model and summarizes statistics to analyze the difference between treated and control firms after the matching process.

Table 1. Selection Model and Indicators of Matching Quality

INTENSIVE MARGIN										
	Dependent Variable: $\mathbb{T}(\omega_{i,2009} > 0 \vert \omega_{i,2008} = 0) \mid \mathbb{T}(\omega_{i,2010} > 0 \vert \omega_{i,2009} = 0)$									
			Mean treat	Mean control	Standardized differences			Variance ratio		
	Coef.	SE	Raw	Raw	Raw	Weig. *Psm*	Weig. *Ipwra*	Raw	Weig. *Psm*	Weig. *Ipwra*
$ln(X_{t-2})$	0.134***	0.014	10.144	8.851	0.465	0.000	0.000	1.027	0.947	0.978
$ln(\#workers_{t-2})$	0.121***	0.026	3.867	3.017	0.383	0.002	0.003	1.030	0.996	1.011
$\Delta ln(\#prod_{t-1})$	0.028	0.080	0.037	0.027	0.022	-0.015	-0.002	1.115	0.942	0.991
$Importer_{t-2}$	-0.053	0.120	0.670	0.533	0.282	0.012	0.002	0.889	0.992	1.000
Observations	11,519		770	10,749						
Pseudo R^2 (before/after): 0.035 / 0.000										
χ^2 test (before/after): 228.85 / 0.20. | p-value (before/after): 0.000 / 0.995										
Imai-Ratkovic overidentification test p-value (H_0: covariates are balanced): 0.2248										
EXTENSIVE MARGIN										
	Dependent Variable: $\mathbb{T}(\omega_{i,2008} > 0 \vert \omega_{i,2007} = 0) \mid \mathbb{T}(\omega_{i,2009} > 0 \vert \omega_{i,2008} = 0)$									
			Mean treat	Mean control	Standardized differences			Variance ratio		
	Coef.	SE	Raw	Raw	Raw	Weig. *Psm*	Weig. *Ipwra*	Raw	Weig. *Psm*	Weig. *Ipwra*
$ln(X_{t-2})$	0.066***	0.013	9.328	8.446	0.457	-0.017	0.006	1.260	1.220	1.091
$ln(\#workers_{t-2})$	0.200***	0.024	3.928	2.981	0.189	0.014	0.007	0.895	0.888	1.070
$\Delta ln(\#prod_{t-1})$	0.358***	0.070	0.080	-0.009	0.534	-0.006	0.003	0.918	0.817	1.189
$Importer_{t-2}$	-0.160	0.107	0.678	0.534	0.000	0.018	0.007	0.877	0.987	0.997
Observations	12,720		967	11,753						
Pseudo R^2 (before/after): 0.034 / 0.000										
χ^2 test (before/after): 230.18 / 0.50. | p-value (before/after): 0.000 / 0.974										
Imai-Ratkovic overidentification test p-value (H_0: covariates are balanced): 0.0										

Notes: The selection model analysis corresponds to the propensity score estimation with a logit specification. Weig. refers to the weighted standardized differences. *Psm* denotes propensity score matching and *Ipwra* denotes inverse-probability-weighted regression-adjustment. $\Delta ln(\#prod_{t-1}) = ln(\#prod_{t-1}) - ln(\#prod_{t-2})$. Statistical significance $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

First, the objective of the estimation is simply to account for characteristics that can help to predict program participation. Predictable patterns emerge. The larger the firm size, in terms of the lag value of exports, the higher the chances of accessing the subsidy. As mentioned before, the biggest firms are those that receive most of the funds. The same holds for the size of the firm, in terms of the lag value of the number of workers. Moreover, a higher growth rate in the number of products before treatment, or a firm that is performing better, provides higher chances of access. Finally, being a direct importer lowers the chances of accessing the benefit. Even though beneficiaries require to use imported goods, most of them access the subsidy by claiming input goods acquired by third-party importers.[6]

Importantly, the results of Table 1 indicate that there are no systematic differences between the two groups. The standardized differences are significantly reduced after the matching procedure and are very small for all covariates, both for the intensive and extensive margins. These fall well below the 20% criteria typically used in the literature.[7] Similarly, the variance ratios get very close to one for the intensive margin case, and significantly improve for the extensive margin in the case of the *Ipwra* estimator. In addition, we calculate the pseudo R^2 and the likelihood ratio test of the joint insignificance for all covariates (selection model) before and after matching. We find that the pseudo R^2 is reduced after matching and the test changes from rejection to non-rejection. These results indicate that the balancing procedure was successful (Caliendo and Kopeinig 2008). Finally, the Imai and Ratkovic (2014) test of overidentification does not reject the null hypothesis that covariates are balanced only in the intensive margin case. Still, the other matching quality results for the extensive margin are as expected.

Table 2. Treatment Effects for the Intensive and Extensive Margins

INTENSIVE MARGIN: Growth Rates for 2009 and 2010
Dependent Variable: $\ln\left(X_{ipd,t}\right)$ - $\ln\left(X_{ipd,t-1}\right)$

	OLS			*Psm*				*Ipwra*			
	Coef.	SE	#obs	ATET	SE	#obs	Stdif	ATET	SE	#obs	Stdif
All	0.244***	0.06	31,257	0.200**	0.084	11,519	0.007	0.222**	0.088	11,519	0.002
S&M	0.260***	0.073	21,744	0.332***	0.080	5,446	0.029	0.323***	0.074	5,446	0.005
L	0.289***	0.094	9,513	0.139	0.118	6,073	0.015	0.124	0.113	6,073	0.004

EXTENSIVE MARGIN: Exit Probability for 2009 and 2010
Dependent Variable: Pr $\left(X_{ipd,t+1} = 0 | X_{ipd,t} > 0\right)$

	OLS			*Psm*				*Ipwra*			
	Coef.	SE	#obs	ATET	SE	#obs	Stdif	ATET	SE	#obs	Stdif
All	-0.151***	0.017	90,396	-0.088***	0.027	12,720	0.014	-0.091***	0.022	12,720	0.003
S&M	-0.158***	0.021	66,439	-0.188***	0.053	5,751	0.074	-0.124***	0.041	5,751	0.038
L	-0.124***	0.029	23,957	-0.058*	0.032	6,969	0.009	-0.063**	0.026	6,969	0.006

Notes: OLS denotes ordinary least squares, *Psm* denotes propensity score matching and *Ipwra* denotes inverse-probability-weighted regression-adjustment. Stdif refers to the mean standardized differences for the covariates of the selection model. # obs denotes the number of observations. S&M denotes small and medium size firms, while L denotes large firms.
Standard errors clustered at the firm level. Statistical significance $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2 reports the Average Treatment Effect on the Treated (ATET) for the intensive and extensive margins. As in the previous table, there are two panels (intensive/extensive margin results). In each case we present our baseline results, which takes into account the whole sample (all firms), and in the two subsequent rows we split the sample into two groups: first we add small and medium-sized firms, and then we have large firms. There are three

[6] As discussed in the Appendix, most of the imported inputs used to access the program are labels, tapes, packaging cartons, paper, varnishes, among others. These items are typically acquired from third-party importers, which represent more than 50% of the approved applications.

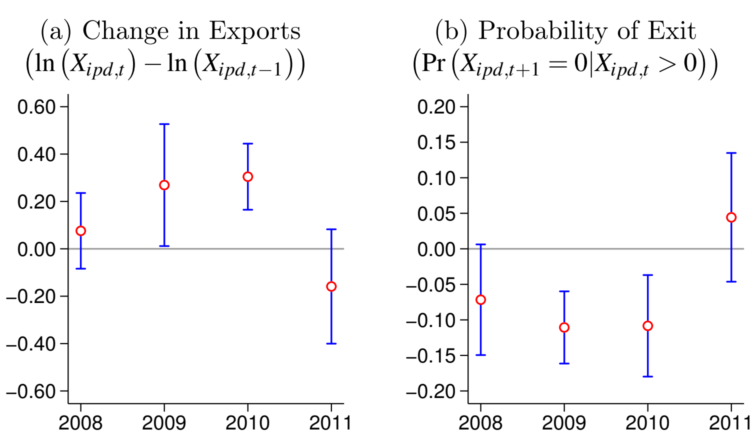
[7] The export promotion literature has followed the usual practice (Rosenbaum and Rubin 1985) of considering a bias above 20% as large (Smith and Todd 2005, Girma and Gorg 2007, Volpe Martincus and Carballo 2008, Lee 2013).

sets of columns. The first report the OLS estimations, the second the *Psm* results, and the third the *Ipwra* estimations. We report the coefficients of interest, as well as the standard errors clustered at the firm level (SE), the number of observations (#obs) and the mean standardized differences (Stdif).

We found a positive program effect on export performance in terms of intensive and extensive margins. Studies of export promotion policies aimed at reducing asymmetric information problems (Makioka 2019), as well as the study of Defever et al. (2020), have found positive effects on the extensive margin only (most studies have focused on the number of products exported and destinations served). An exception is Van Biesebroeck et al. (2016), which also finds a positive impact on the value of exports during the Great Trade Collapse.

In our baseline estimation for the intensive margin, firms that did not access the subsidy had an average growth between 2009 and 2010 of -0.204, while firms that benefit showed a positive growth of 0.018, yielding an ATET of 0.222 (*Ipwra* estimation). This finding should come as no surprise, because export promotion policies are 'soft' interventions that do not involve significant amounts of funds. Moreover, the subsidy program in Nepal provides a one or two percent ad valorem rates, while in Peru the rate increase to 8 percent in the context of the crisis. In addition, our intensive margin treatment effect is of the same magnitude as those estimated in Van Biesebroeck et al. 2016. They also look at the Peruvian case, but focus on the impact of the Peruvian export promotion agency (Promperu) during 2007-2010. Even though we should expect the subsidy scheme to have a greater impact, two important differences explain our relatively low or their relatively high estimates (besides the analysis of different samples). As pointed out earlier, Van Biesebroeck et al. 2016 use a more lax definition of treatment and analyze the log level of exports.[8]

Figure 2. ATET Per Year, $t \in \{2008, 2011\}$. Effects for Firms Treated in Year t on the Change in Exports in t and Exit Probability in Year $t+1$



An important point to highlight is that our intensive margin results are driven by small and medium-sized firms. Since our focus is on the crisis, it is consistent to find effects mainly on small and medium-sized firms, which are probably less prepared to face difficult times (for example, they may face tightened borrowing constraints). Although we find no effects for newly benefited large firms, we cannot rule out the possibility that other groups, like

[8] If we focus on $\ln\left(X_{ipd,t}\right)$ instead of $\ln\left(X_{ipd,t}\right)$ - $\ln\left(X_{ipd,t-1}\right)$, our *Ipwra* estimation increases from 0.22 to 0.42.

those large firms that access the subsidy every year, might have benefited from the increase in the subsidy rate from 5% to 8%. Closely related to this point, we should recognize our inability to disentangle the effects between the increase in the rate and the possibility that the subsidy makes firms more resilient (regardless of the rate).

Our extensive margin results show that the probability of exit during 2009-2010 for non treated firms was 0.308, while firms that received support recorded a chance of exit of 0.217. Hence, the ATET associated with exit is -0.091. Again, our results are of the same order of magnitude as Van Biesebroeck et al. (2016). However, now we do observe a positive impact for small and medium-sized firms, as well as for large firms. In keeping with the intensive margins results, the impact is higher for small and medium-sized firms.

Next, we break down the analysis to consider the ATET per year. The results of this analysis are reported in Figure 2, where we use the same model, the estimator is the *Ipwra* and we include all firms. For the intensive margin case, we only observe significant effects during 2009 and 2010, which are of the same order of magnitude. It should be recalled that during these two years the subsidy rate was increased and the program became more accessible. In the case of the extensive margin, the program reduced the chances of exit during 2008-2010. As in the case of the growth of exports, we find no effects for 2011. As pointed out in the introduction, this was a year of adjustment for the program.

4.2 Robustness

We carry out three robustness checks, given our treatment definition and data availability.[9] The first exercise considers the period of the crisis to be characterized by large and heterogenous real effects across sectors (p) and countries (d), as pointed out by Paravisini et al. (2015). Because we are using transaction level data, we can restrict the matching within the same (pd) market. In this way, we carry out a comparison between transactions that faced similar demand and transportation shocks during the crisis.[10] The second robustness check exploits the fact that treated firms carried out transactions for which the subsidy was and was not received. Firms treated in our sample during 2008-2010 accessed the benefit in 40 to 50 percent of their transactions (in terms of exported values). In a placebo test, we drop all the transactions that received the benefit $\left(\mathbb{1}\left[sub_{ipd,t} > 0\right] = 1\right)$. We expect our treatment effects to vanish or to be significantly reduced.

The third exercise follows a similar logic. We restrict the sample to 'new' firms of 2009 and 2010, so that there are no selection issues. Using a classical DiD approach we estimate the program's effects on the intensive margin within treated firms. We run two regressions (one per year). We estimate: $\ln X_{ipd,t} = \alpha_0 + \alpha_1 \mathbb{1}[2009] + \sum_{t=2008}^{2009} \gamma_t \mathbb{1}\left[sub_{ipd,2009} > 0\right] \mathbb{1}[t] + \varepsilon_{ipd,t}$; using data for 2008 and 2009. α_0 measures the log of exports without subsidy in 2008, α_1 measures the change in exports in 2009 for non-benefited transactions ($\mathbb{1}[2009]$ is a dummy variable that takes the value of one in 2009), γ_{2008} measures the difference between transactions with and without subsidy in 2008, while γ_{2009} is our coefficient of interest. This latter coefficient measures the change in the log of exports for transactions that received the benefit in 2009 but not in 2008. A parallel regression with information from 2009 and 2010

[9] We thank the referee for suggesting the last two exercises.

[10] Note that our results would be upward biased if we compared (i) markets in which we only have treated observations and that were less affected by the crisis, with (ii) markets more severely affected and in which we only have control observations.

captures the effects for ‘new’ firms of 2010.

Table 3. Robustness

	Baseline		Matching Restricted to the Same *pd* Market		*Placebo Test*: Drop All Obs. With $\mathbb{1}\left[sub_{ipd,t} > 0\right] = 1$	
	ATET	SE	ATET	SE	ATET	SE
INTENSIVE MARGIN						
Psm	0.200**	0.084	0.302**	0.140	0.082	0.132
Ipwra	0.222**	0.088	n.f.	n.f.	-0.015	0.106
EXTENSIVE MARGIN						
Psm	-0.088***	0.027	-0.131***	0.040	-0.077**	0.038
Ipwra	-0.091***	0.022	n.f.	n.f.	-0.042	0.029

Notes: *Psm* denotes propensity score matching and *Ipwra* denotes inverse-probability-weighted regression-adjustment. n.f. denotes not feasible. The placebo sample includes ipd,t transactions of non-treated firms and ipd,t observations for treated firms that did not received the benefit $\left(\mathbb{1}\left[sub_{ipd,t} > 0\right] = 0\right)$. Standard errors clustered at the firm level. Statistical significance $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

Table 3 presents the first two robustness exercises. The three sets of columns refer to the baseline results, also reported in Table 2, the first robustness exercise (restricting the matching to the same *pd* market) and the second robustness exercise (the placebo test). The first exercise indicates that our treatment effects for the intensive and extensive margins holds, and that in the worst scenario, our treatment effects are greater. The placebo test results are as expected; in three of the four estimates our results became non-significant. Table 4 presents the last robustness check. The table includes four columns: two for the ‘new’ firms of 2009 (with and without fixed effects), and the same for 2010. Once we control for firm, product and destination fixed effects, we find that benefited transactions are higher on average one year before requesting the subsidy ($\gamma_{t-1} > 0$). Our coefficients of interest, γ_t are significant and higher than in our baseline estimations.

Table 4. Robustness, Analysis Within Treated Firms

	$t = 2009$	$t = 2009$	$t = 2010$	$t = 2010$
α_0	9.580***	9.241***	9.092***	8.834***
	[0.313]	[0.065]	[0.381]	[0.072]
α_1	-0.050	-0.040	0.147	0.257***
	[0.078]	[0.084]	[0.094]	[0.081]
γ_{t-1}	-0.006	1.030***	0.509	1.309***
	[0.431]	[0.172]	[0.359]	[0.234]
γ_t	0.429***	0.419***	0.455***	0.346**
	[0.124]	[0.134]	[0.157]	[0.156]
#obs	2,419	2,415	2,260	2,219
R^2	0.003	0.788	0.016	0.811
FE i		✓		✓
FE p		✓		✓
FE d		✓		✓

Notes: FE denotes fixed effects. Standard errors clustered at the firm level. Statistical significance $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

5. Conclusions

We estimate the effects of an export subsidy program in Peru during 2009-2010 (subsidy rate of eight percent on the FOB value). Our results point out that the subsidy has major effects on the intensive and extensive margins. In the case of firms that received the funds, the change in exports was approximately 20 percentage points greater than in the control group. In terms of the extensive margin, the exit probability for 2009 and 2010 was almost 10 percentage points smaller for firms that received the subsidy one year before.

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