Review of Economic Analysis 11 (2019) 145–174 1973-3909/2019145

The Impact of Special Economic Zones on Exporting Behavior

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Using firm level data from Africa and Asia, we estimate the impact of being in a special economic zone (SEZ) on a firm's probability of exporting, export intensity, and value of exports. At the extensive margin, we find that SEZ firms in open economies are 25% more likely to export than their non-SEZ counterparts, with a large negative effect in closed economies. At the intensive margin, we find that SEZs increase the value of exports, but only in countries with barriers to imports where the estimate increase is 3.6%. Thus, the estimated effect of introducing an SEZ can be meaningful, but is heavily contingent on the local economic environment.

Keywords: Exporting, Trade Barriers, Special Economic Zones.

JEL Classifications: F14, F13

1 Introduction

With the link between exports and economic growth well established, numerous government policies have sought to encourage exports as a method of increasing productivity and growth. One such policy that has been widely utilized is the special economic zone (SEZ).[1] According

* We thank the editor, three referees, and participants at the 2016 European Trade Study Group, 2016 International Institute of Public Finance, 2016 Society for International Trade Theory, and 2017 Irish Economics Association conferences. This project has received funding from the European Union's Seventh Framework Programme for research, technological development and demonstration under grant agreement no. 613504. All errors are our own.

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[1] In the literature, several types of SEZs are discussed, including freeports, free trade zones, export promotion zones and industrial parks. Nevertheless, there is no clear-cut distinction between these with the definitions depending on the study at hand (see Akinci and Farole (2011) for discussion). Since our data do not distinguish among types of SEZs, we combine all of these under this single heading.

to the World Bank (2008), as of 2008 there were over 3500 SEZs which amounted to 68 million jobs and over $500 billion in trade-related value added. As of 2015, the number of SEZs stood at more than 4000 (The Economist, 2015; Nazarczuk, 2017). As described in Farole (2011), an SEZ is a defined geographic area in which special incentives and/or policies apply that are not available elsewhere in the country. Zeng (2015) notes that common SEZ features include streamlined processing of goods ready for export, lower export fees, and reductions in taxes and import tariffs on intermediates, all of which aim to make SEZ firms more competitive on world markets. As such, they are intended to be areas that encourage development via increased exporting, innovation, and investment. That said, setting up a production in an SEZ involves high fixed costs followed by uncertainty over the stability of the benefits SEZ residents will receive.[2] Furthermore, the usefulness of SEZ production is obviously contingent on the economic conditions a firm operates in and therefore changes in investment climate, institutional quality, or trade policy in the SEZ-granting country or overseas can impact their usefulness to firms (Aggarwal, 2012). As such, even if the polices announced in an SEZ are attractive on the surface, this may not be enough to induce changes to firm behavior.

Although there is a large body of case studies that provide often contradictory findings about the impact of SEZs on firm choices, there is little rigorous evidence on their economic impacts, particularly with respect to their main goal of promoting exporting.[3] With this in mind, this paper uses data on 11,161 firms across 21 Asian and African countries to test whether SEZs affect exports at either the extensive margin (i.e. whether to export at all) and/or the intensive margin (that is, how much to export conditional on exporting at all).[4]

Specifically, we use an exporter dummy variable for our extensive margin analysis and both the logged share of exports in total sales the logged value of total exports as measure of the intensive margin.[5] We find that the estimated impact is conditional on the local economic environment. In open economies, SEZs increase the probability of exporting by 25% but have no marked effect on the intensive margin of trade. In closed economies, SEZs appear to lower the probability of exporting, potentially due to increased scrutiny by trade officials. That said, they do appear to increase the value of trade by as much as 42%. Thus, in order to anticipate the potential effects of an SEZ, it is necessary to consider them in context of the local economic

[2] See Yang, et al. (2011) for a discussion of the costs of operating in an SEZ. Nazarczuk (2015) and Madani (1999) describe the uncertainty over the stability and longevity of SEZ provisions.

[3] See Zeng (2015), Farole and Akinci (2011), and Farole (2011) for examples and surveys of the literature.

[4] In particular, Zeng (2015) notes the lack of analysis of African SEZs. Aggarwal (2005) provides a review the main EPZs (Export Processing Zones) of India, finding that exports from EPZs have grown dramatically and now account for 50% of Indian manufacturing exports and 80% of their exports of electrical products. Note that Indian firms make up roughly half of our data.

[5] Intensive margin changes would result from SEZ effects on marginal costs (such as export duties or VAT rates) whereas changes at the extensive margin would also include changes in the fixed cost of exporting (such as red tape costs).

environment.

Alongside the rise of SEZs, an economic literature has grown to examine the link between SEZs, trade, and economic growth. On the theory side of this discussion, the focus has been on describing when and how to best use SEZs to improve exports and growth.[6] On the empirical side, the large majority of the literature is descriptive, discussing the experience of areas with SEZs via aggregated data. Examples here include Bräutigam and Tang (2014), Aggarwal (2005), Ge (1999), Amirahmadi and Wu (1995), and the contributions collected by Farole and Akinci (2011) and Farole (2011). On the whole, the indications from this literature are best described as mixed, with some suggesting that SEZs have sizable impacts on trade, investment, and welfare while others find the opposite. In any case, this literature does not employ regression analysis, instead relying on summary statistics for evaluating the impact of SEZs on exports.

There are, however, exceptions to this rule.[7] Leong (2013), in a regression estimating the impact of trade and foreign direct investment (FDI) on growth in Chinese and Indian regions, uses SEZs as an instrument for these endogenous variables.[8] However, he does not report the first stage results, and thus the impact of SEZs on exports, from his estimation. Also using Chinese regional data, Wang (2013) estimates the impact of factors such as FDI and exports on regional capital investment and productivity growth, finding that after the introduction of an SEZ, both variables have larger effects than before the SEZ was instituted. Likewise, Jensen and Winiarczyk (2014) consider the impact of SEZs on the development of Polish regions. They find that although SEZs there have attracted FDI, they have contributed little to employment or wage improvements. Closer to our level of analysis, Ebenstein (2012) utilizes firm-level information for China to examine the impact of SEZs on firm employment, productivity, and wages, finding positive effects on the first two. However, despite the stated SEZ goal of export promotion, none of these studies estimate the effect of SEZs on exports themselves.[9] [10]

To our knowledge, only a handful of studies specifically examine SEZs and exports using

[6] Examples include Klein (2010), Chaudhuri and Yabuuchi (2010), Schweinberger (2003), Yabuuchi (2000), Devereux and Chen (1995), Din (1994), Miyagawa (1992, 1986), and Hamilton and Svennson (1982).

[7] Beyond the studies discussed here specifically related to SEZs, Busso, Gregory, and Klien (2013) estimate the effect of empowerment zones in the US (a place specific policy comparable to a SEZ without the SEZ's international focus) on local employment and wage growth.

[8] When not using an instrumental variables estimator but including SEZs as a control variable, Leong (2013) found that SEZs had no clear-cut effect on growth, with the coefficient ranging from significantly positive to insignificant or even significantly negative depending on the controls and sample used.

[9] Although not a regression based analysis, Defever and Riaño (2015) calibrate Chinese data to a model with SEZs, inferring that SEZs have a sizable impact on exports.

[10] Yang, et al. (2011) focus on Cross-Border SEZs (CB-SEZs) around Chinese border which promotes cross-border trade with neighbors. However, they do not analyze exporting but rather various measures of firm performance.

regression analysis. Johansson and Nilsson (1997) estimate the impact of SEZs on aggregate exports for eleven developing countries over 13 years. While they tend to find a positive effect, the country-specific results indicate a great deal of heterogeneity, leading them to conclude that the export promotion effects are potentially positive only for generally export-oriented economies something which, due to the exclusion of fixed effects, they cannot control for. In contrast, by using firm-level data we can do precisely that. In particular, by doing so, we are able to illustrate that the conditionality hinted at by Johansson and Nilsson (1997) is a driving factor in the effect of SEZs. Most similar to our analysis are the various single country, firm-level studies that examine SEZ influence on exporting at the extensive and intensive margins. Examples here include Nazarczuk and Uminski (2018) (Poland) and Defever, et al. (2017) (Dominican Republic). One issue issue for these studies is that they do not address the potential endogeneity of SEZs (i.e. that they may be established in areas where FDI or productive firms are already present). As found in the single country studies of Steenbergen and Javorcik (2017) (Rwanda) and Nazarczuk and Uminski (2018) (Poland) matching across firms is the standard method of doing so, a practice we also use.[11] Our results complement all of these by using cross-country data as opposed to that for a single country. In particular, it is worth noting that some find positive effects on exporting (e.g. Nazarczuk and Uminski (2018)) whereas others find no effect (including Steenbergen and Javorcik (2017)). One possible reason for this is that, as suggested by Johansson and Nilsson (1997), underlying country heterogeneity may have an important effect. By using cross-country data, we are able to examine how the SEZ effect depends on trade policy, offering a potential rationale for the different effects in the literature.

Using our firm-level data covering 21 African and Asian countries, we begin by comparing firms in SEZs to non-SEZ firms. We find that SEZ firms are generally more export oriented at the extensive and intensive margins, being both more likely to export and exporting greater values, although the share of revenue generated from exports is somewhat smaller. This mirrors the data of Johansson and Nilsson (1997). However, we also find that, among other differences, SEZ firms are more productive, larger, and more likely to be foreign-owned, all things found in the literature to be positively associated with exporting. Nazarczuk (2017) finds the same pattern in Polish data by looking at Kernel densities of firm characteristics of SEZ and non-SEZ firms. Turning to regression analysis where we can control for fixed country, sector, and year effects, we find that it indeed these other firm-specific factors that explain the greater export activity of SEZ firms. This result, however, is an average effect. We proceed by allowing

[11] Note that Steenbergen and Javorcik (2017) performs the propensity score matching with difference-in-difference option and find that if a firm chooses to move to a SEZ that fact doubles its output and its imports but no real impact of exporting. Nazarczuk (2017) uses a matching estimator on Polish firms but exporting status is a control, not a dependent variable in the analysis of firm productivity. It is also worth noting the contribution of Wang (2013) who uses a matching estimator across Chinese regions rather than firms. Alder, et al. (2013) do not use matching, but follow Wang (2013) by using Chinese cities.

the impact of the SEZ to vary with local country-level characteristics which are intended to reflect the types of barriers SEZs supposedly mitigate, namely export costs, taxes, regulatory burdens, weak institutions, and barriers to imports. Here, we find two results. First, when exporting and/or importing is relatively easy, firms in SEZs do indeed seem more likely to export. In contrast, when a country is closed, we find a negative impact of SEZs on the extensive margin. This may be due to closed countries' trade authorities heavily monitoring activities with SEZs, reflective of the possibilities raised by Johansson and Nilsson (1997). Both of these effects are large; the first suggests a 25% increase in the probability of exporting whereas the second implies a nearly 100% decrease. Second, for firms that do export, SEZs lead to higher export values when importing is difficult, with export sales rising approximately 42%. This is consistent with the notion that SEZs often permit importing at lower cost. Thus, although throughout our analysis we find no significant effect at the mean, we do find important effects depending on the country's openness to trade. Although our data do not allow us to distinguish whether these differences are due to cross-country differences in the SEZs themselves or arise from their interactions with other policies that vary across countries, it does point to a strong conditionality of their effects.

The rest of the paper is organized as follows. In the next section, we provide an overview of our data, including a discussion of its overarching features. Section 4 describes our econometric approach and provides our results. Section 5 concludes.

2 Data and Summary Statistics

2.1 Data Sources and Construction

Our firm-level data come from the World Bank's Enterprise Surveys.[12] Note that our data come from the more recent, unstandardized surveys as only these included a question on whether or not a firm was in an SEZ.[13] This also limits the country coverage relative to the standardized surveys, leaving us with 21 African and South Asian countries, with their surveys being carried out between 2007 and 2014. The data are cross-sectional, with surveys taking place once in each country.[14] Although the data include observations on services and retail/wholesale firms, as these firms do not face the same types of export barriers manufacturers do, we restrict the data to manufacturing.[15] After cleaning and harmonizing across the countries, the surveys have a similar layout and were conducted using a common methodology of random stratified sam-

[12] These can be found at `http://www.enterprisesurveys.org/`

[13] To our knowledge, ours is the first analysis of these more recent data.

[14] A handful of countries have been surveyed twice, however, as we cannot tell which firms were surveyed more than once, we cannot use this aspect of the data and therefore only use the largest survey round for each country. Similar approach has been used by Davies and Jeppesen (2015).

[15] Specifically, we use firms in industries 15 to 37 using the ISIC 3.1 Rev. Classification.

pling.[16] In all surveys, the World Bank defines the survey universe as "commercial, service or industrial business establishments with at least five fulltime-employees". The list of countries in our sample, the year of their survey, the number of observations, and the number of observations within an SEZ is provided in Table 1. In total, the sample contains 11,161 firms, 58% of which are in SEZs.[17]

Table 1: Countries in the Sample

Country	Num. of Firms	Num. of SEZ Firms	Year
Angola	111	22	2010
Bangladesh	1138	172	2013
Botswana	88	49	2010
Burkina Faso	61	28	2009
Cameroon	65	18	2009
Chad	57	16	2009
Ethiopia	177	61	2011
India	6834	4523	2014
Lesotho	43	27	2009
Madagascar	116	30	2009
Mali	283	283	2007
Mauritius	126	29	2009
Mozambique	253	253	2007
Nepal	243	162	2013
Nigeria	45	15	2009
South Africa	506	506	2007
Sri Lanka	310	12	2011
Tanzania	229		2013
Togo	13		2009
Uganda	233		2013
Zambia	243	243	2007
Total	11161	6449	

[16] Specifically, it uses strata on firm size (with three categories: <20 employees, 20-99 employees, and 100+ employees).

[17] This sample is the one for which all of our country-level controls were available. In unreported results, depending on the country level controls included, we were able to increase the number of firms to 12,279 over 31 countries. This, however, did not affect the nature of the estimates. These are available on request.

During the preparation of the unstandardized surveys we extracted several firm-specific variables. In particular, we have three measures of firm exporting behaviour: a exporter dummy variable indicating whether or not the firm exports, the log of the share of sales generated by exporting (referred to as export intensity), and the log of the value of exports. These variables are standard ones used by the literature to describe a firm's exporting behaviour at the extensive and intensive margins.[18] In addition, we collected several control variables identified by the literature as correlated with exporting. First, we include labour productivity, measured as the log of sales relative to employment.[19] Note that, although this measure does not control for other inputs, and is therefore not productivity itself, it is commonly employed as such in the literature (see Pavnick, 2002). Second, as a measure of firm size, we use the logged value of employment. In addition, we use the log of the firm's age. Third, we include five dummy variables respectively indicating whether or not a firm is foreign-owned, has an internationally recognized quality certificate, is a multi-product firm, licenses foreign technology, or imports intermediate inputs. Previous work using the standardized surveys finds that all of these are positively correlated both with the probability of exporting and the volume of exports, thus our priors are that the same holds true in our data.[20] Finally, and most importantly for our purposes, we have information on whether or not the firm self-identifies as being located in an SEZ.[21] If, as is generally believed, firms in SEZs find exporting both easier (due to lowered export barriers) and more profitable (due to lower taxes and barriers to imported intermediates), we expect that firms in SEZs would be more likely to export, have greater export sales, and have a higher export intensity.[22]

To explore this notion further, we introduce five country-level variables which represent measures of the types of barriers SEZs supposedly overcome. First, we create a measure of policy-driven exporting costs, using the Trading Across Border data from the World Bank Doing Business database (World Bank 2014).[23] More specifically, we combine three variables, the

[18] Further, these have the best cross-country coverage in the surveys.

[19] All monetary values are reported in local currencies, which we deflate using the annual consumer price index from the World Bank Development Indicators (World Bank, 2006-2014) and thereafter convert to US dollars using the annual average exchange rate from the same source.

[20] Examples include Davies and Jeppesen (2015) and Davies and Mazhikeyev (2015).

[21] The earlier surveys in our data only ask whether or not a firm is in an SEZ; some later ones further break this down into whether the firm is located in an export processing zone or an industrial park. We do not make use of this distinction here for two reasons. First, the World Bank do not provide any information in the surveys or the implementation notes detailing the difference between the two, thus, it is not clear whether or not this distinction is comparable across surveys. Furthermore, the existing literature is itself at odds over the difference (if any) between the two (see Madani (1999) for discussion). Second, using this information severely limits the sample size.

[22] For a discussion of the tax exemptions in African SEZs, see Bräutigam and Tang (2014).

[23] Note that as we do not have data on the export destination, we cannot control for destination-varying trade costs, only for origin export costs.

number of documents needed to export, the average number of days before a container is cleared for export, and the average cost of containerized export. We use these three measures precisely because the reflect the types of export barriers SEZs are intended to reduce. Across all three, there is a relatively high cross-country variation. The cost of exporting ranges from $560 in Sri Lanka to $6615 in Chad, while the number of documents required range from 4 in Mauritius to 11 in Cameroon, the Congo, and Nepal. Mauritius is also the country where it takes the least time to clear cargo for exporting, with an average of 10 days. At the other end of the distribution is Afghanistan, with an average of 86 days. That said, within a country, all three measures are relatively highly correlated. Because of this, we follow Davies and Jeppesen (2015) use principal component analysis to construct a source-specific export cost index. Details from this construction are found in Table 2. If SEZs help firms by lowering export barriers, we expect a positive coefficient from an interaction between the firm's SEZ variable and the country's export cost variable since it is in those countries with the greatest barriers that SEZs might provide the greatest benefits.

Second, we use a cross-country index that identifies the extent to which local business owners find the level of taxes to be a barrier to work and investment. This was rescaled so that higher numbers indicate more burdensome taxes.[24] Third, we include an index on the local perception of the quality of government institutions, with higher numbers meaning lower institutional quality. Both of these were obtained from the World Economic Forum (2014). From the Fraser Institute (2014), we obtained two additional indices: one measuring the burden of government regulation and one indicating the extent to which non-tariff barriers (NTBs) reduce the ability of imported goods to compete in local markets. Both of these were scaled so that higher numbers indicated greater restrictions. As with the export cost variable, we expect the interactions between firm *i*'s SEZ dummy and the local index to be positive, i.e. SEZ do more to promote exports when local barriers are large. Summary statistics for all variables are in Table 3.

2.2 SEZ vs. Non-SEZ firms

Before proceeding to regression analysis, it is useful to make some simple comparisons between SEZ and non-SEZ firms. Table 4 presents the means of our firm-level variables for SEZ and non-SEZ firms. The third column presents the coefficient from the SEZ dummy when regressing the variable in question on the SEZ dummy and a set of industry, country, and year dummies. Beginning with the exporter dummy variable, 20.8% of SEZ firms export, whereas 20.1% of non-SEZ firms do. After controlling for country, industry, and year effects in what amounts to a linear probability model, we find that SEZ firms are roughly 0.7% more likely to export with

[24] Specifically, in all the indices described here, we use the closest year available to the year of a given country's survey and when needed rescaled the variable so that higher numbers mean greater burdens. See the relevant source for discussion on the construction of the particular index.

Table 2: Construction of Export Costs

Panel A:	1	2
Number of obs.		11161
Retained factors		1
No. parameters		3
Panel B:	Eigenvalue	Proportion
Factor1	1.9578	0.6526
Factor2	0.8639	0.288
Factor3	0.1781	0.0594
Panel C:		
Variables	Factor1 Loadings	Uniqueness
Documents to export	0.5221	0.7274
Time to export	0.9416	0.1134
Cost to export	0.8937	0.2013

this difference highly significant. Likewise, SEZ firms export a greater value, where the result in column 3 indicates that SEZ firms export values are 37% higher than comparable firms.[25] The mean of the export intensity, however, is 35.4% lower for SEZ firms. Thus, these results suggest that SEZs may well increase exporting, if not the export intensity. However, it must be remembered that other factors also influence export activity and, as the rest of the table indicates, these differences are also significant.

In particular, SEZ firms are markedly more productive and larger, two variables that are typically positively correlated with exporting.[26] On a top of that, we find that SEZ firms are 11.2% younger than their non-SEZ counterparts which would generally makes them less export-oriented. Beyond these differences, we find that SEZ firms are slightly more likely to be foreign-owned, import intermediates, and license a foreign technology. The are also 21.4% more likely to have a quality certification. Finally, we find that they are slightly less likely to be multi-product firms. Thus, just as we find SEZ firms are more export oriented, we find that many of their characteristics also predispose them to exporting. In order to simultaneously control for all of these differences, we now turn to our regression analysis.

[25] Recall that when interpreting a coefficient β on a dummy variable in a log-linear equation, the percentage impact of going from 0 to 1 is $100 * (e^{\beta} - 1)$.

[26] Using data on Polish firms, Nazarczuk (2017) also finds that SEZ firms are more productive than non-SEZ firms on average and that SEZ firms have greater value added per employee.

Table 3: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Exporter	11161	0.205	0.404	0.000	1.000
Export Share	2291	-1.126	1.162	-5.298	0.000
Sales	2291	13.848	2.423	4.541	23.250
Productivity	11161	9.868	1.735	1.902	20.280
Employment	11161	3.699	1.335	0.000	11.074
Age	11161	2.680	0.803	0.000	5.242
Foreign Owned	11161	0.052	0.222	0.000	1.000
Quality Cert.	11161	0.377	0.485	0.000	1.000
Multi-product	11161	0.380	0.485	0.000	1.000
Import	11161	0.146	0.354	0.000	1.000
License	11161	0.130	0.336	0.000	1.000
Export Cost	11161	0.000	1.000	-1.883	5.958
Taxes	11161	-3.943	0.605	-4.800	0.000
Regulations	11161	-5.603	0.561	-6.598	-3.136
Institutions	11161	5.317	0.911	0.000	5.900
NTBs	11161	-5.991	0.637	-6.913	-3.529

3 Empirical Strategy

Our empirical strategy makes use of both regression analysis and propensity score matching. In Section 2, we found significant differences in the exporting behavior of SEZ and non-SEZ firms. However, before attributing the differences to being in an SEZ, it must be remembered that there were other significant differences as well which are often found to be correlated with exporting choices. Therefore, we turn to regression analysis that begins with a baseline specification (as specified below) to examine the relationship between SEZ status and exporting at the extensive and intensive margins while controlling for other factors. Following this, because the presumption is that SEZs impact exporting via lower trade barriers, we extend our regression specification by taking into account a number of important obstacles firms face, namely export costs, non-tariff barriers, taxes, institutional quality, and regulatory burden. We do so to investigate the potential conditionality suggested by Johansson and Nilsson (1997). Finally, we address the concern over the potential endogeneity in the SEZ variable, i.e. firms located in SEZs are there precisely because they intend to export (or the opposite). Ebenstein (2012), for instance, finds that in China, foreign-owned firms (many of which export) are indeed more likely to open in SEZs than elsewhere (with no impact on the location of domestic firms). Fol-

Table 4: SEZ Versus non-SEZ Firms

Variable	SEZ	non-SEZ	Difference	% Change
Exporter	0.208	0.201	0.007***	0.7%
Export Share	-1.307	-0.869	-0.437***	-35.4%
Export Sales	13.979	13.663	0.315***	37.0%
Productivity	10.210	9.401	0.809***	124.6%
Employment	3.779	3.589	0.190***	20.9%
Age	2.633	2.744	-0.112***	-10.6%
Foreign Owned	0.058	0.044	0.014***	1.4%
Quality Cert.	0.467	0.253	0.213***	23.7%
Multi-product	0.352	0.418	-0.066**	-6.4%
Import	0.146	0.147	0.000***	0.0%
License	0.149	0.104	0.044***	4.5%
Obs.	6449	4712		

Notes: SEZ coefficient comes from a regression using SEZ,country, sector, and year dummies. ***, **, and * on difference denote significance at the 1%, 5%, and 10% levels respectively. Percent change is $100(e^{\beta} - 1)$ where β is the SEZ coefficient. The export intensity and export value results only use exporting firms.

lowing, e.g. Nazarczuk and Uminski (2018), we therefore apply a propensity score matching methodology.

3.1 The Regression Model

In our baseline specification, we estimate for firm i in country j in sector s surveyed in year t:

$$EXP_i = \beta_0 + \beta_1 SEZ_i + \beta_2 X_i + \theta_j + \theta_s + \theta_t + \varepsilon_i \tag{1}$$

where EXP_i is one of three measures of firm i's export behavior (i.e. the exporter dummy, logged export intensity, or logged export value), SEZ_i is a dummy equal to 1 if the firm is in an SEZ, X_i is a vector of controls as discussed above, and the θs are a set of country, sector, and year dummy variables. These latter then control for unobservables common across firms in a given country (which are all observed for the same year), common across firms in a given sector, and common to all firms surveyed in a particular year. Because the data come from a stratified survey, we weight the observations according to the strata in the survey, specifically employment in three categories (under 20, 20-99, and 100+) and country.[27] Further, we cluster

[27] See `http://www.enterprisesurveys.org/methodology` for discussion on the survey stratification.

the standard errors by country.

The firm level controls come from the same firm-level surveys. As noted above, we take the log of continuous control variables (i.e. firms' productivity level, the number of full-time employees and age) before including them in regression/matching work. The other firm characteristics such as ownership, quality certificates and importing status are binary dummy variables. The choice of this set of control variables is based on their common use in the literature (e.g. Nazarczuk and Uminski (2018), Davies and Jeppesen (2015), McCann (2013), Nesterenko (2003), and Pavnic (2002)) where those studies were in turn motivated by the heterogeneous firms theory popularized by Melitz (2003) and Ottaviano and Melitz (2008). Overall, the literature suggests that the firms engage in export/import activities more if they are found to be more productive, bigger in size, older, and have a certificate or license. As differences were found in these between SEZ and non-SEZ firms (Table 4) it is therefore important to control for them in our regression.

To this baseline specification, we introduce additional controls intended to proxy for the differential impact of export costs, taxes, NTBs, regulation and institutional quality attributes across SEZ and non-SEZ firms, where for country measure Y_j we estimate:

$$EXP_i = \beta_0 + \beta_1 SEZ_i + \alpha_1 SEZ_i * Y_j + \beta_2 X_i + \theta_j + \theta_s + \theta_t + \varepsilon_i. \quad (2)$$

The standard presumption is that SEZs promote exporting as firms located in SEZs face lower tariffs (or none in imported intermediates), lower non-tariff barriers to exporting, less intrusive regulations, and/or pay lower taxes. Therefore we include these in our expanded regression.[28] Thus, these are the variables we turn to to examine the conditionality of SEZ effects. To our best knowledge, there is no study which looks specifically at trade costs/barrier variables and interacts them with SEZ variable.[29] Note that with the inclusion of the additional variables, the marginal effect of being in an SEZ is a function of $\beta_1 + \alpha_1 * Y_j$. As our country controls are negative at the mean in the data with a maximum value of zero (with the exception of export costs which are mean zero by construction), if α_1 is estimated to be negative, this means that $\alpha_1 * Y_j$ is positive, i.e. being in an SEZ increases exporting with an impact that approaches zero as the barrier rises.

3.2 Propensity Score Matching

As an alternative estimation strategy, we employ propensity score matching approach. The general idea behind this approach is to find a match for SEZ firms (treatment group) from non-SEZ firms (control group) based on similarities in characteristics (productivity, employment size, age, ownership etc.). This ensures that the firms in our compared groups are alike and by

[28] Summary statistics for these are in Table 3.

[29] Yang, et al. (2011) include controls such as financial services, tax incentives, and land price variables in their export performance analysis, but do not condition the SEZ effect on them.

that it reduces any selection bias which may be effecting the results from regression. With this matching approach, the goal is to estimate the following:

$$\tau_{ATT} = E_{SEZ=1,p(X)}(E(EXP(1)|_{SEZ=1,p(X)}) - E(EXP(0)|_{SEZ=1,p(X)})) \quad (3)$$

which is the difference in the exporting variable E (here, the exporter dummy, export intensity, or export value) when the firm is in an SEZ (i.e. is treated) versus when it is not, holding the probability of the firm being in the SEZ constant (see Caliendo and Kopeinig, 2008).[30] As any remaining differences in the productivities of the matched sample of SEZ and non-SEZ firms is attributed to the treatment, it is paramount to ensure that all observable factors influencing the firm's selection into a given treatment as well as the firm's exporting behaviour, are controlled for. Although several matching approaches are available, using a caliper of .0001 worked best with respect to the tests of appropriateness (see Panel B of Table 6, discussed momentarily). This, however, comes at the cost of the number of firms for which a match could be found, resulting in only 4250 non-SEZ firms and 2645 SEZ firms for which there was common support (i.e. slightly over half the sample). Nazarczuk and Uminski (2018) also use the same matching approach like ours as a robustness check.[31]

4 The Results

4.1 Regression Analyses: The Extensive Margin of Trade

In Table 5 we present our estimates for the probability of exporting, i.e. on the extensive margin. Here, we use a logit estimator due to the binary nature of the dependent variable.[32] Column 1 presents the results using only the standard set of controls, all of which are positive and significant as expected with the exceptions of the multi-product and license dummies which are insignificant.[33] Note that we are not claiming causation, but merely correlation due to the lack of time series data needed for improved identification. In column 2, we introduce the SEZ dummy variable. As can be seen, after controlling for the other differences across firms, we find no significant impact of the SEZ variable. This is comparable to results Nazarczuk and Uminski (2018) obtain from Polish data and Steenbergen and Javorcik's (2017) estimates from Rwanda data. Note that these two papers are also able to use time-series information, and therefore have

[30] Note that we continue to control for country, sector, and year dummies in this.

[31] In addition, they also employ the propensity score matching using a difference in difference method which compares changes over time after a firm becomes located in an SEZ (see Guo and Fraser (2014), Heckman, et al. (1997)). This was also done by Steenbergen and Javorcik (2017). This is not possible in our case due to our reliance on cross-sectional data.

[32] Note that as a firm either exports or does not, we do not suffer from violations of the Independence of Irrelevant Alternatives assumption. Further, as we need to control for country, sector, and year dummies, we cannot use a probit estimator.

[33] Elliott and Virakul (2010) find a similar result for multi-product firms when using developing countries.

more robust identification. Thus, the finding in Table 4 indicating a difference in the probability of exporting seems to be the result of other differences across firms, not whether or not they are in an SEZ.

Table 5: Probability of Exporting

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Productivity	0.185***	0.184***	0.188***	0.184***	0.185***	0.184***	0.187***	0.191***
	(0.0227)	(0.0227)	(0.0228)	(0.0227)	(0.0227)	(0.0227)	(0.0227)	(0.0228)
Employment	0.601***	0.601***	0.604***	0.601***	0.601***	0.601***	0.602***	0.603***
	(0.0250)	(0.0250)	(0.0251)	(0.0250)	(0.0250)	(0.0250)	(0.0250)	(0.0251)
Age	0.191***	0.192***	0.196***	0.191***	0.192***	0.190***	0.194***	0.194***
	(0.0396)	(0.0399)	(0.0401)	(0.0399)	(0.0399)	(0.0399)	(0.0400)	(0.0402)
Foreign Owned	0.467***	0.466***	0.480***	0.460***	0.470***	0.455***	0.484***	0.450***
	(0.135)	(0.135)	(0.135)	(0.135)	(0.135)	(0.135)	(0.135)	(0.137)
Quality Cert.	0.752***	0.751***	0.744***	0.752***	0.750***	0.752***	0.750***	0.748***
	(0.0690)	(0.0694)	(0.0695)	(0.0695)	(0.0694)	(0.0694)	(0.0696)	(0.0695)
Multi-product	0.0392	0.0397	0.0454	0.0394	0.0397	0.0389	0.0410	0.0461
	(0.0649)	(0.0651)	(0.0651)	(0.0651)	(0.0651)	(0.0651)	(0.0651)	(0.0651)
License	0.0262	0.0254	0.0147	0.0265	0.0245	0.0271	0.0187	0.0125
	(0.0809)	(0.0812)	(0.0814)	(0.0812)	(0.0812)	(0.0813)	(0.0813)	(0.0817)
Import	1.139***	1.139***	1.150***	1.137***	1.140***	1.134***	1.148***	1.144***
	(0.0781)	(0.0781)	(0.0781)	(0.0781)	(0.0781)	(0.0781)	(0.0782)	(0.0785)
SEZ		0.0115	-0.0155	0.516	-0.280	0.639	-1.979**	2.058
		(0.0757)	(0.0778)	(0.621)	(0.783)	(0.538)	(0.964)	(1.575)
Export costs*SEZ			-0.317***					-0.543***
			(0.108)					(0.160)
Taxes*SEZ				0.124				0.212
				(0.151)				(0.379)
Regulation*SEZ					-0.0517			-0.102
					(0.138)			(0.384)
Institutions*SEZ						0.113		0.470**
						(0.0958)		(0.187)
NTBs*SEZ							-0.326**	-0.130
							(0.156)	(0.377)
Constant	-8.320***	-8.321***	-8.223***	-8.460***	-8.298***	-8.493***	-8.404***	-9.196***
	(0.509)	(0.509)	(0.524)	(0.548)	(0.513)	(0.542)	(0.507)	(0.796)
Net SEZ effect=0			0.84	0.72	0.89	0.64	0.72	0.43
Observations	11,161	11,161	11,161	11,161	11,161	11,161	11,161	11,161

Notes: ***, **, and * on difference denote significance at the 1%, 5%, and 10% levels respectively. All specifications include country, sector, and year dummies. Net SEZ Effect = 0 reports the p value at the sample mean.

One feature of this result, however, is that it assumes that the impact of SEZs is the same everywhere. As discussed in the introduction, SEZs are often intended to aid firms in overcoming trade barriers. Thus, it may be that the positive effect of an SEZ is found in a country where exporting is expensive. With this in mind, column 3 introduces an interaction between the SEZ dummy and the export cost variable (recall that since the export cost is a country-level variable and each country is surveyed in a single year, the country dummy absorbs the non-interacted

export cost variable).[34] If SEZs aid in overcoming export costs and therefore play a role mostly in high export cost countries, we expect this coefficient to be positive. In contrast, we find that it is significantly negative, i.e. in a high export cost country an SEZ firm is less likely to export. This may reflect the findings of Johansson and Nilsson (1997), where they argue that SEZs encourage exports in primarily export-oriented (i.e. low export cost) countries. As reported at the bottom of the table, at the sample mean for export costs, the estimated marginal effect is insignificant.

This should not, however, be interpreted as no significant effect since at the sample mean export costs are zero (by construction). Instead, this should be interpreted as in Figure 1 which plots the difference in the estimated probability of exporting for an SEZ firm relative to a non-SEZ firm (vertical axis), all else equal, across the spectrum of export cost values (horizontal axis). The figure indicates that if in a country export costs are low (i.e. the left upper corner) SEZ firms are more likely to export with the reverse found when exporting is expensive (i.e. right bottom corner). At the minimum of the export cost measure, the estimated marginal effect is positive and highly significant (with a probability value of 0.004). Likewise, for the maximum export costs, the impact is significantly negative (with a probability value of 0.004). This seemingly paradoxical result may be driven by the constrained optimization of trade authorities. When an economy is closed, relatively little funding may be available to the officials regulating exports. Therefore they would have an incentive to focus their efforts in locations where the values of production, productivity and exports are particularly high, i.e. SEZs.[35] This greater scrutiny within an SEZ may then increase the probability of inspection, increasing the expected need for the appropriate export permits which, particularly in these countries, are costly. As such, while some aspects of exporting may be reduced by the SEZ, the fixed cost of doing so may rise. In more open and better funded countries, however, this effect would be smaller as the trade authority casts a wider inspection net, allowing the export promoting aspects of SEZs to dominate. Furthermore, these effects are economically large. Approximately 40% of firms in low export cost countries export. As such, the nearly 0.1 increase for low export cost countries in Figure 1 is a 25% increase in the probability of exporting. At the other end, in high export cost countries, only about 20% of firms export. Therefore the roughly 0.2 reduction would reduce the probability of exporting by nearly 100%.

In columns (4), (5), and (6), we repeat this exercise, replacing the export cost interaction with an interaction using the tax, regulation, and institution indices. In each case, neither the SEZ variable nor its interaction is significant. In column (7), we utilize the NTB interaction and find a negative coefficient on this interaction. At the sample mean (where the NTB value is

[34] Although the surveys contain some firm-level information on exporting, as this is available reported only by exporters, we cannot make use of these data as they are missing for non-exporting firms.

[35] A comparable effect is found by Gómez-Guillamón and Sanchez-Val (2012) who find that tax auditing is more effective in more dense areas.

Figure 1: Change in the Probability of Exporting - Export Costs

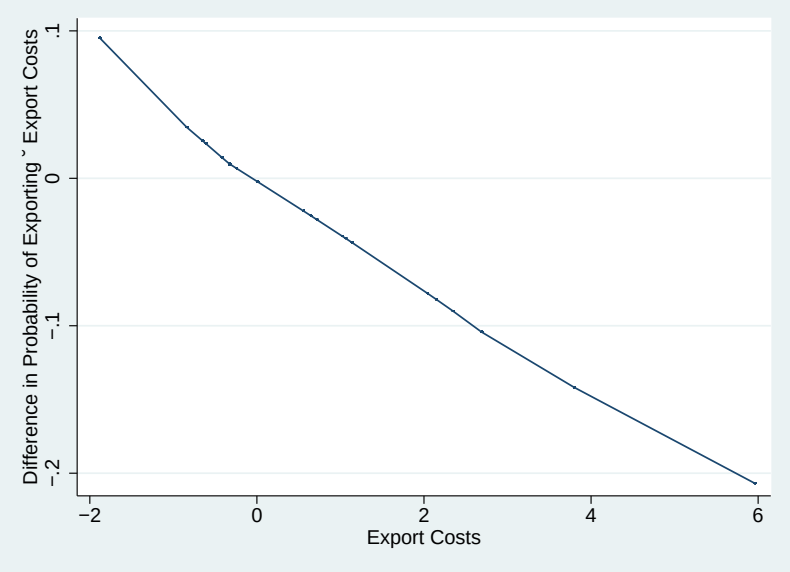
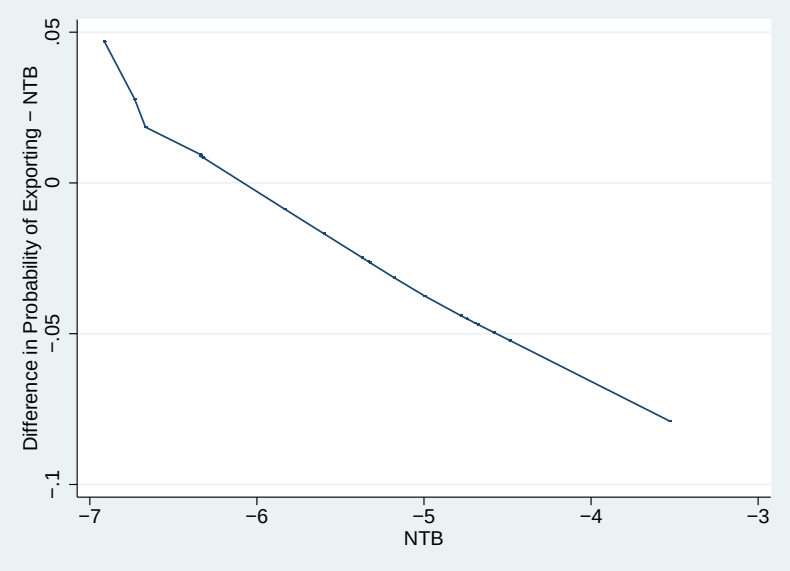


Figure 2: Change in the Probability of Exporting - NTBs



-5.991), the net effect of an SEZ is $-1.979 + (-0.326) * (-5.991) = -0.026$, which as indicated at the bottom of the table we cannot reject as different from zero. However, as with the export cost, this masks variation across countries that is revealed when plotting the difference in export probabilities across the different NTB levels in Figure 2. The figure shows that whenever NTB

level is low (i.e. upper left corner) the higher the probability that firms in SEZs would export as oppose to firms in non-SEZ areas. For countries with minimal NTBs, as with the export cost measure, the net effect is positive and significant. For high NTB countries, the impact is negative and significant with the effect in very high NTB countries estimated to equate to roughly a 50% reduction in the probability of exporting. Thus, again we see that closed economies are those where NTBs seem to lower the probability of exporting. Finally, column (8) includes all five interactions where only the export cost and institution coefficients are significant. Here, we find that SEZs increase the export probability in countries with weak institutions. In addition, we again find that they reduce the export probability in countries with high export costs. Finally, as in column (3), we find a significantly positive net effect for low export cost countries (with a probability value of 0.001) and a significantly negative effect for high export cost nations (with a probability value of 0.0007).

4.2 Propensity Score Matching: Probability of Exporting

Table 6 displays the results of propensity score matching. In Panel A of the table results with the unmatched sample indicates that SEZ firms are significantly more likely to export (as in Table 4). However, after matching, i.e. ensuring that probability of treatment is controlled for, the difference between SEZ and non-SEZ firms is insignificantly negative with a value of $\tau_{ATT} = -0.0159$. This is in line with the results of Steenbergen and Javorcik (2017) but in contradiction to the matching results from Nazarczuk and Uminski (2018) who report $\tau_{ATT} = 0.184$ which is statistically significant at 1% level. Beyond the obvious differences in data sets, it should be noted that export barriers in Rwanda are likely large in comparison to the countries in our sample whereas Polish barriers are likely relatively low. Coming back to our result, the negative differences in the probability of exporting are driven not by a firm being in an SEZ, but by the characteristics of firms in SEZs. In order to support the validity of this test, Panel B presents three post-estimation checks, discussed in Caliendo and Koeinig (2008). The first of these is a two-sample t-test, which works by comparing the means of the covariates between the SEZ and non-SEZ firms, before and after matching. If the matching is of a high quality (i.e. the distribution of treated and control groups are quite similar), no significant differences should be found after matching. As the table indicates, it is indeed the case. The second test involves re-estimating the propensity score using the matched sample and comparing the Pseudo R-squared obtained from the probit estimation before and after matching. Again, if the matching is of a high quality, the distribution of the covariates should be similar across treated and untreated firms, resulting in a relatively low pseudo-R2 after matching has taken place. Again, this holds. Finally, we perform a likelihood test on the joint significance of all the variables included in the probit model before and after matching. Following the same logic, we should expect to reject this test on the matched sample only (Caliendo and Kopeinig, 2008) which is again the case. Thus, these tests support the validity of the matching.

Combining these results, we see that the impact of SEZs on the probability of exporting is a nuanced one. In open economies, particularly those generally open to exports, SEZs seem to increase exporting at the extensive margin. For those that are closed to exports and/or imports, however, the opposite effect is found. This is consistent with Johansson and Nilsson (1997) and may be reflective of differences between open and closed economies with respect to the effectiveness of trade authorities.

Table 6: Propensity Score Matching: Probability of Exporting

Panel A: Selection					
Sample	Treated	Controls	Difference	S.E.	T-stat
Unmatched	0.207978311	0.193411765	0.014566	0.00830	1.75
ATT	0.2	0.215879017	-0.015879	0.01457	-1.09
Panel B: Sensitivity Test					
Variable	Matched	Treated	Control	T stat	Prob. Val.
Productivity	Unmatched	10.151	9.5113	22.3	0.000
	Matched	10.058	10.063	-0.15	0.884
Employment	Unmatched	3.9051	3.6355	9.92	0.000
	Matched	3.7916	3.8166	-0.7	0.483
Age	Unmatched	2.7018	2.7577	-3.47	0.001
	Matched	2.7471	2.7555	-0.4	0.691
Foreign Owned	Unmatched	0.03428	0.03576	-0.39	0.695
	Matched	0.02987	0.02949	0.08	0.935
Quality Cert.	Unmatched	0.52401	0.25765	27.22	0.000
	Matched	0.46578	0.46994	-0.3	0.762
Multi-product	Unmatched	0.26569	0.39294	-13.26	0.000
	Matched	0.27713	0.29452	-1.4	0.162
License	Unmatched	0.13865	0.09106	7.16	0.000
	Matched	0.10851	0.09981	1.04	0.301
Import	Unmatched	0.12393	0.14024	-2.33	0.020
	Matched	0.11682	0.1293	-1.38	0.167

Sample	Pseudo R^2	LR χ^2	$p > \chi^2$
Raw	0.229	2966.48	0
Matched	0.005	34.08	0.833

4.3 Regression Estimates: The Intensive Margin of Trade

The above results indicate that SEZs have an impact on the extensive margin of trade; however in closed economies, this effect is negative suggesting that SEZs there may increase inspections and the fixed cost of exporting. This does not, however, mean that they must also reduce trade for firms that choose to export since they may simultaneously work to lower the marginal cost of exporting. In this section, we use two measures of the intensive margin, the logged share of sales generated via exports (export intensity) and the logged value of exports (export value). Note that in this analysis, we restrict ourselves to the set of exporting firms and thus face no problems with zero exports.

4.3.1 Regression Estimates and Matching: Export Intensity

Table 7 begins by estimating the effect of SEZs and the other controls on the export intensity using the same approach as in Table 5. Because the export intensity cannot exceed zero (the log of 1), we use a Tobit estimator. As can be seen, SEZs have limited effects. In column (7), we find a marginally significant coefficient both for the SEZ variable and the interaction. Figure 3 plots the estimated difference between an SEZ firm's export intensity a comparable non-SEZ firm across the different NTB levels. The figure indicates that when the NTB level is high in a country (upper right corner) the higher the relative export intensity of an SEZ firm (note that in this and subsequent figures, confidence intervals are included but that they are difficult to see due to the preciseness of the estimates). For open economies (left-hand side of the graph), the point estimate of this difference is negative but economically small. For high NTB countries, however, the effect is significantly positive (with a probability value of 0.049 at the maximum NTB). However, when we also control for export costs in column (8), this effect disappears to be replaced by a marginally negative coefficient on the interaction between SEZ status and trade costs. This results in a pattern similar to Figure 1; however it is only for high export cost countries that we find a significant net effect. That said, as the significance of the coefficients is not particularly strong, we do not wish to make too much of these results, preferring to instead say that the evidence of an SEZ effect on export intensity is at best limited. Other controls do, however, have a strong impact on the export intensity. In particular, younger, single-product, non-importers earn a greater share of sales from exporting.

As with the extensive margin, one might worry about the endogeneity of the SEZ variable, thus in Table 8 we employ the same matching technique described above (but replacing the exporter dummy with the export intensity variable). Here, as we have fewer exporting firms we are forced to rely on a set of 821 non-SEZ firms and 158 SEZ firms for which we had common support. As in the extensive margin results, after matching we estimate an insignificant $\tau_{ATT} = 0.1433$ with the post-estimation tests supporting the quality of the matches. That is in line with Nazarczuk and Uminski (2018). Thus, after other important firm characteristics are

Table 7: Export Intensity

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Productivity	-0.0349	-0.0368	-0.0364	-0.0369	-0.0377*	-0.0369	-0.0392*	-0.0396*
	(0.0224)	(0.0225)	(0.0225)	(0.0225)	(0.0225)	(0.0225)	(0.0225)	(0.0227)
Employment	0.0311	0.0314	0.0314	0.0314	0.0322	0.0312	0.0322	0.0342
	(0.0250)	(0.0250)	(0.0250)	(0.0250)	(0.0249)	(0.0250)	(0.0249)	(0.0249)
Age	-0.166***	-0.161***	-0.160***	-0.161***	-0.163***	-0.162***	-0.163***	-0.161***
	(0.0383)	(0.0384)	(0.0384)	(0.0384)	(0.0384)	(0.0384)	(0.0383)	(0.0382)
Foreign Owned	0.0858	0.0819	0.0811	0.0836	0.0659	0.0795	0.0748	0.0622
	(0.116)	(0.116)	(0.116)	(0.117)	(0.116)	(0.117)	(0.116)	(0.118)
Quality Cert.	-0.0883	-0.0943	-0.0947	-0.0944	-0.0961	-0.0946	-0.0959	-0.102
	(0.0674)	(0.0675)	(0.0675)	(0.0675)	(0.0673)	(0.0674)	(0.0673)	(0.0674)
Multi-product	-0.216***	-0.212***	-0.212***	-0.213***	-0.211***	-0.212***	-0.213***	-0.209***
	(0.0639)	(0.0637)	(0.0638)	(0.0637)	(0.0637)	(0.0637)	(0.0637)	(0.0641)
License	0.0769	0.0736	0.0728	0.0733	0.0772	0.0737	0.0768	0.0760
	(0.0780)	(0.0779)	(0.0779)	(0.0780)	(0.0781)	(0.0779)	(0.0780)	(0.0779)
Import	-0.121*	-0.123*	-0.122*	-0.122*	-0.127*	-0.124*	-0.128*	-0.124*
	(0.0669)	(0.0667)	(0.0668)	(0.0668)	(0.0666)	(0.0667)	(0.0666)	(0.0664)
SEZ		0.0940	0.0904	0.00687	1.026	0.226	1.454*	2.895*
		(0.0730)	(0.0728)	(0.477)	(0.652)	(0.497)	(0.782)	(1.515)
Export costs*SEZ			-0.0339					-0.224*
			(0.0691)					(0.131)
Taxes*SEZ				-0.0214				-0.0765
				(0.117)				(0.373)
Regulation*SEZ					0.165			0.134
					(0.114)			(0.360)
Institutions*SEZ						0.0240		-0.0615
						(0.0884)		(0.155)
NTBs*SEZ							0.222*	0.445
							(0.126)	(0.317)
Constant	-0.822**	-0.852**	-0.821**	-0.811**	-1.006**	-0.911**	-0.764**	-0.295
	(0.363)	(0.367)	(0.365)	(0.390)	(0.391)	(0.413)	(0.365)	(0.420)
Net SEZ effect=0			0.21	0.21	0.17	0.19	0.11	0.23
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291

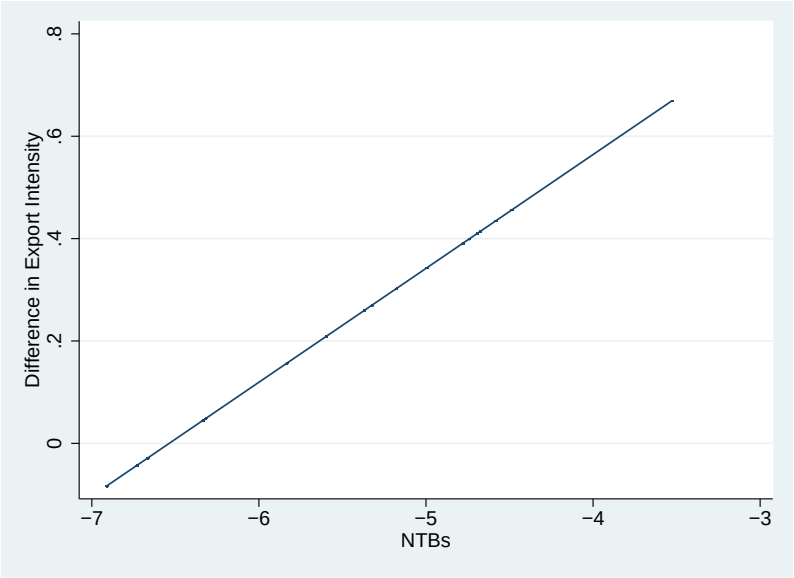
Notes: ***, **, and * on difference denote significance at the 1%, 5%, and 10% levels respectively. All specifications include country, sector, and year dummies. Net SEZ Effect = 0 reports the p value at the sample mean.

matched, the export intensity of SEZ and non-SEZ firms are statistically the same.

4.3.2 Regression Estimates and Matching: Export Value

Table 9 turns to the Export Value (again for the set of exporting firms). As with the export intensity results, we find limited impact of SEZs. That said, we do find a relatively robust impact from the NTB interaction which is significantly positive, both on its own in column (7) and when used alongside the other interactions in column (8). Figure 4 illustrates the estimated impact. The figure indicates that whenever NTB level is high (i.e. closed economies) the higher the difference in export sales level between firms in SEZs,and those in non-SEZ parts

Figure 3: Change in Intensity of Exporting - NTBs



Note: 95% Confidence Intervals are extremely narrow and hence indistinguishable from the line.

(upper right corner). Comparable to Figure 3, we find no economically significant effect for low NTB countries but an economically and statistically significant positive effect in high NTB countries. At that end of the NTB distribution, the expected difference in exports is 0.5 which, relative to the mean export value of 13.7 in high NTB countries, is a 3.6% increase. This may be evidence of the fact that it is possible for SEZ firms to import intermediates under reduced duties, increasing production and therefore exports. In addition, column (5) provides some marginal evidence that SEZ increase export volumes in strong regulation countries, with the effect illustrated in Figure 5. The figure suggests that at highly regulated economies the difference in export volumes become more apparent between firms in SEZs and those in non-SEZ parts where SEZ firms making more export sales. Again, it is only for the heavily regulated countries where we estimate an economically significant net effect, one which indicates that SEZ firms in these nations export a greater value. Beyond the SEZ variable, unsurprisingly, more productive, larger, and foreign firms export higher values. Younger, single-product, and non-importing firms also export greater values.

Finally, Table 10 again explores the possibility that our results are driven by endogeneity of the SEZ variable. Nevertheless, we again find an insignificant effect after matching, with $\tau_{ATT} = 0.0212$. As with the extensive margin, this is consistent with export value results of Steenbergen and Javorcik (2017) but differs from those Nazarczuk and Uminski (2018). Note

that, as this is the same set of firms as in Table 8 with a different export outcome variable, the post-estimation tests from matching are the same as reported there.

Table 8: Propensity Score Matching: Export Intensity

Panel A: Selection					
Sample	Treated	Controls	Difference	S.E.	T-stat
Unmatched	-1.17866516	-0.823489372	-0.355175786	0.052534604	-6.76
ATT	-1.14432697	-1.28757787	0.143250898	0.141142051	1.01
Panel B: Sensitivity Test					
Variable		Treated	Control	T stat	Prob. Val.
Productivity	Unmatched	10.46	9.7555	10.68	0.000
	Matched	10.357	10.496	-0.89	0.375
Employment	Unmatched	4.7738	4.9919	-3.41	0.001
	Matched	4.884	4.6757	1.40	0.164
Age	Unmatched	2.8231	2.9316	-3.04	0.002
	Matched	2.8858	2.9854	-1.20	0.232
Foreign Owned	Unmatched	.10056	.07186	2.19	0.029
	Matched	.10759	.06329	1.41	0.160
Quality Cert.	Unmatched	.69646	.4933	9.17	0.000
	Matched	.72152	.64557	1.45	0.148
Multi-product	Unmatched	.26536	.42144	-7.24	0.000
	Matched	.25949	.24684	0.26	0.797
License	Unmatched	.19646	.19732	-0.05	0.963
	Matched	.23418	.20886	0.54	0.589
Import	Unmatched	.3473	.36784	-0.93	0.355
	Matched	.3481	.33544	0.24	0.813

Sample	Pseudo R^2	LR χ^2	$p > \chi^2$
Raw	0.232	601.61	0
Matched	0.092	39.18	0.179

4.4 Comparing SEZ Impact Estimates with Other Studies

Combining these results, we find that, while there is limited evidence of SEZs affecting the export share or the number of exporters, however, they do seem to encourage greater value of exports in countries with high NTBs/export costs, potentially due to reduced duties on imported intermediates. As we find no robust effect on the export intensity, this would suggest that

Table 9: Value of Exports

Variables								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Productivity	0.967***	0.966***	0.966***	0.966***	0.966***	0.966***	0.964***	0.964***
	(0.0167)	(0.0168)	(0.0168)	(0.0168)	(0.0167)	(0.0167)	(0.0167)	(0.0169)
Employment	1.011***	1.011***	1.011***	1.011***	1.011***	1.011***	1.011***	1.013***
	(0.0184)	(0.0184)	(0.0184)	(0.0184)	(0.0184)	(0.0184)	(0.0184)	(0.0184)
Age	-0.0852***	-0.0808***	-0.0808***	-0.0807***	-0.0820***	-0.0811***	-0.0825***	-0.0806***
	(0.0283)	(0.0283)	(0.0283)	(0.0284)	(0.0283)	(0.0283)	(0.0283)	(0.0283)
Foreign Owned	0.0716	0.0668	0.0667	0.0676	0.0530	0.0649	0.0581	0.0543
	(0.0873)	(0.0874)	(0.0874)	(0.0881)	(0.0877)	(0.0880)	(0.0875)	(0.0885)
Quality Cert.	-0.0304	-0.0351	-0.0351	-0.0351	-0.0368	-0.0352	-0.0368	-0.0391
	(0.0482)	(0.0482)	(0.0482)	(0.0482)	(0.0481)	(0.0482)	(0.0481)	(0.0481)
Multi-product	-0.134***	-0.131***	-0.131***	-0.131***	-0.129***	-0.131***	-0.130***	-0.128***
	(0.0465)	(0.0464)	(0.0464)	(0.0464)	(0.0464)	(0.0464)	(0.0464)	(0.0466)
License	0.0493	0.0464	0.0463	0.0462	0.0478	0.0463	0.0476	0.0466
	(0.0549)	(0.0549)	(0.0549)	(0.0549)	(0.0549)	(0.0549)	(0.0548)	(0.0547)
Import	-0.110**	-0.112**	-0.112**	-0.112**	-0.115**	-0.113**	-0.116**	-0.112**
	(0.0496)	(0.0496)	(0.0496)	(0.0496)	(0.0496)	(0.0495)	(0.0495)	(0.0495)
SEZ		0.0765	0.0760	0.0364	0.790*	0.167	1.189**	1.898**
		(0.0545)	(0.0543)	(0.381)	(0.409)	(0.349)	(0.506)	(0.963)
Export costs*SEZ			-0.00411					-0.130
			(0.0550)					(0.0887)
Taxes*SEZ				-0.00989				-0.0236
				(0.0948)				(0.239)
Regulation*SEZ					0.128*			0.0412
					(0.0744)			(0.222)
Institutions*SEZ						0.0167		-0.0962
						(0.0643)		(0.117)
NTBs*SEZ							0.183**	0.367*
							(0.0842)	(0.217)
Constant	-0.642**	-0.705**	-1.137***	-0.695**	-0.773***	-0.734**	-0.938***	-0.362
	(0.276)	(0.276)	(0.382)	(0.290)	(0.278)	(0.298)	(0.295)	(0.573)
Net SEZ effect=0			0.16	0.16	0.18	0.14	0.9	0.26
Observations	2,291	2,291	2,291	2,291	2,291	2,291	2,291	2,291
R-squared	0.839	0.839	0.839	0.839	0.839	0.839	0.839	0.839

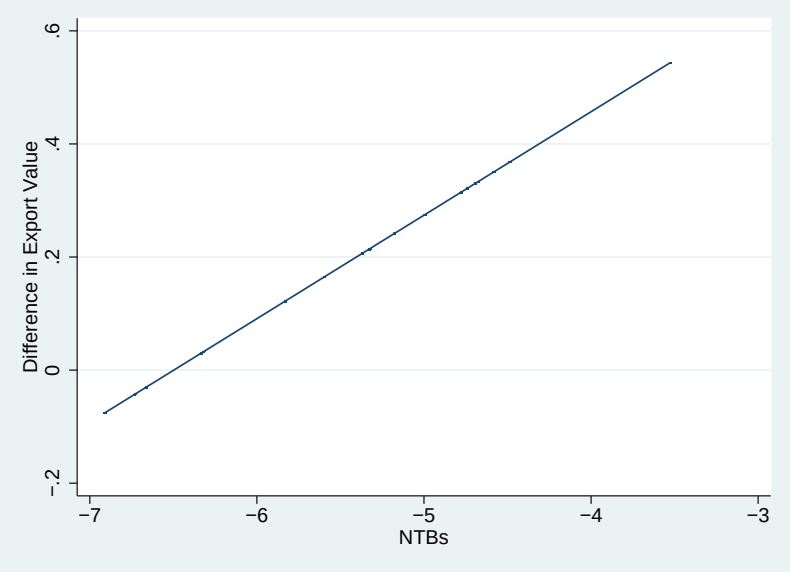
Notes: ***, **, and * on difference denote significance at the 1%, 5%, and 10% levels respectively. All specifications include country, sector and year dummies. Net SEZ Effect = 0 reports the p value at the sample mean.

Table 10: Propensity Score Matching: Export Value

Sample	Treated	Controls	Difference	S.E.	T-stat
Unmatched	14.0554632	13.9238786	0.131584618	0.099936301	1.32
ATT	14.0969394	13.884578	0.212361387	0.260619058	0.81

cheaper imports increase both exports and domestic sales proportionally. Further, this is an economically sizable effect. In the high NTB countries, the mean (log) value of sales is 11.8. Pulling the estimated increase of 0.35 from 4 for these countries, this means an increase in

Figure 4: Change in Value of Exports - NTBs

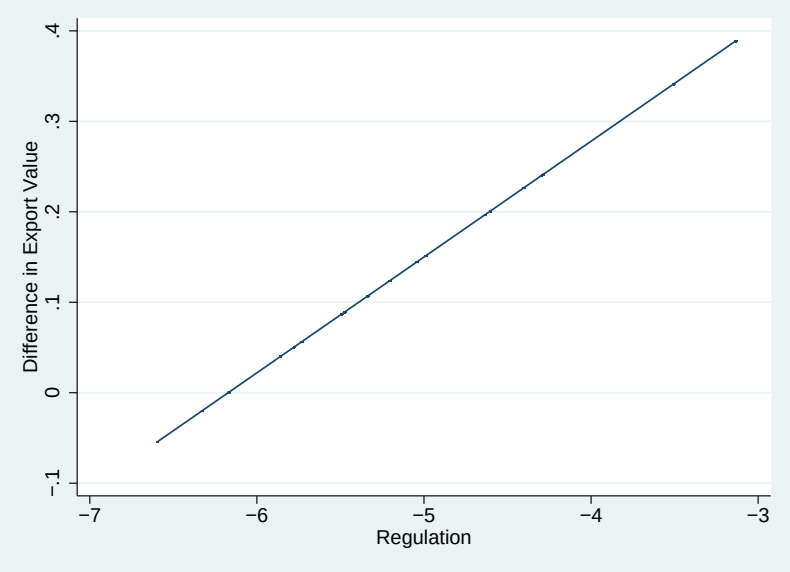


Note: 95% Confidence Intervals are extremely narrow and hence indistinguishable from the line.

(non-logged) sales of 41.9%.

Because the interaction with these trade barrier measures is our innovation, there are no comparable results in the literature for us to compare ourselves to. Nevertheless, it is instructive to compare what is comparable (the non-interacted SEZ dummy in column 2 of Tables 5 (probability of exporting) and 9 (export value)). In Table 11, we compare our estimates with those in Nazarczuk and Uminski (2018), Steenbergen and Javorcik (2017), and Nesterenko (2003). While there are differences across the studies, all, use firm-level data, employ and exporter dummy (using a Logit estimator) or logged export values (with OLS), and include an SEZ dummy as the variable of interest. However, there are differences in terms of time, country coverage and the size of samples as shown in the table. Despite these difference, as reported, the various studies all find positive coefficents for the SEZ dummy at both level extensive and intensive margins. Overall, the magnitude of our estimates fall somewhere in the middle with those reported by Nazarczuk and Uminski (2018) notably higher which might be due to the overall openness of Poland compared to the countries in the other three studies. In general, we find that our estimates are generally in line with those found elsewhere which suggests that, across the literature while SEZs do not negatively impact firms' exporting behavior, their value as an export-promoting policy tool can be questioned.

Figure 5: Change in Value of Exports - Regulation



Note: 95% Confidence Intervals are extremely narrow and hence indistinguishable from the line.

Table 11: Extensive and Intensive Margin Estimates Compared

	Nazarczuk & Uminski (2018)	Steenbergen[a] & Javorcik (2017)	Nesterenko[b] (2003)	Our[c] Results
Dep. Var. —	Exporter (dummy)	Exporter (dummy)	Exporter (dummy)	Exporter (dummy)
SEZ —	1.124	0.0079	0.48311***	0.0115
Dep. Var. —	Export Value (log)	Export Value (log)	Export Value (log)	Export Value (log)
SEZ —	1.740***	0.159	0.067***	0.0765
Data —	Firm-level	Firm-level	Firm-level	Firm-level
Obs. —	518	179,149	23,649	11,161/ 2,291
Period(s) —	2004-2014	2008-2016	1996-1999	2007-2014
Sector(s) —	Mnfc.	Mnfc & Srvs	Mnfc.	22 Mnfc.
Country(ies) —	Poland	Rwanda	Ukraine	21 African/Asian
Est. method —	Logit/OLS-FE	Logit/OLS-FE-DiD	Logit/OLS-FE	Logit/OLS-FE

Notes: ***, **, and * on difference denote significance at the 1%, 5%, and 10% levels.
a They use quarterly firm reports in and outside of SEZs in Kigali, Rwanda.
b Dataset covers 24 counties in Ukraine.
c Our dataset is a cross-sectional one consolidating surveys conducted at some point in 2007-14 period.

4.5 Additional Regressions

To explore the data further, we examined several alternative samples. First, rather than manufacturing, we considered agricultural products. There, as in manufacturing, we found only

occasionally significant impacts of SEZs and when this was the case, they were typically negative and then for the extensive margin. Second, we considered different subsamples of manufacturing, specifically food, transport equipment, and textiles. Although the significance of the coefficients was markedly weaker, potentially due to the smaller sample sizes, when the SEZ variables were significant, they were comparable to those found here. As a further test of the endogeneity of the SEZ variable, following the results of Ebenstein (2012), we split the sample between foreign-owned and domestically-owned firms since he found that the first group was more likely to locate in an SEZ than elsewhere. Nevertheless, we found the same results in these subsamples as in the combined sample, again suggesting that endogeneity is not driving the result. We estimated the effect of SEZs separately for Asian and African countries (the two groups in our data) and excluding India (which represents a large share of the sample). In both cases, neither the SEZ variable itself nor its interactions were significant. We also tried to interact importer dummy with the NTB and export cost variables.[36] When doing so, we find that importers in SEZs have a smaller impact from NTBs on their exporting behavior at both margins, suggestive that SEZs might help to mitigate some of the trade barriers felt by importers who also wish to export. Finally, in our intensive margin regressions, we explored the potential role of the WTO's Export Share Requirement (ERS) policy.[37]. This policy demands that, to be consistent with WTO rules, firms in SEZs should be required to achieve a minimum export intensity (Defever and Riano, 2017). In Table 4, we show that the export share of SEZ firms is instead smaller than for non-SEZ ones, suggesting a contradiction of the ERS policy. Delving deeper, this difference is driven by two countries, Zimbabwe and South Africa.[38] That said, after dropping these two countries, our results do not change significantly. All of these additional results are available on request.

5 Conclusion

Special economic zones have long been touted as a method of increasing exports and, as a result, improving the level of development in a region. While there are numerous case studies on the issue, there is scant econometric evidence testing the notion. We contribute to the debate by providing the first firm-level, cross-country econometric study testing whether SEZs do in fact increase exports at either the extensive or intensive margins. The resulting pattern is a nuanced one. At the extensive margin, SEZs increase the likelihood of exporting by as much as 25%, but only for firms in relatively open economies. In closed economies, we find the opposite effect,

[36] This was motivated in part by Yang, et al. (2011) who find that firms choose to move to SEZs in China because they import raw materials and intermediates. Thus, the import duty reductions in the SEZ significantly lower importers' production costs.

[37] We thank one of our reviewers for suggesting this.

[38] Those countries might have removed the ERS policy to attract more FDI to their SEZs as done by the Dominican Republic (Defever, et al. 2017).

something that might be consistent with differing patterns of enforcement across countries. At the intensive margin, we find little evidence suggesting that SEZs affect the share of sales earned from exporting. They do, however, seem to markedly increase the value of exports in countries with import barriers, something that suggests that SEZs may reduce the cost of intermediate inputs, encouraging both domestic and foreign sales. Combining these effects, if the goal is to increase exporting, it is likely that policy makers will need to consider SEZs in light of the local economic environment before choosing to use them. This is consistent with the other single-country studies on SEZs and, as indicated in Yang, et al. (2011), it may suggest that the additional costs of being in an SEZ (e.g. higher fixed setup costs) can outweigh their benefits. That said, our results do indicate a conditional effectiveness of SEZs and future research with access to panel data can help to fill out the policy environment for which they do (or do not) promote exporting. In particular, our estimates suggest that in open economies, SEZs affect the extensive margin positively with little effect on the intensive margin whereas for closed economies, introducing SEZs may mean greater exports spread across fewer firms. As these have distributional consequences across firms and regions, such factors should be considered when creating SEZs. As such, we hope that our results provide a stepping stone to the development of a framework under which SEZs play a useful role in a general overhaul of a country's policies.

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6 Appendix

Table 12: Countries in the Sample

Country	N	N*	Year
Angola	111	22	2010
Bangladesh	1138	172	2013
Botswana	88	49	2010
Burkina Faso	61	28	2009
Cameroon	65	18	2009
Chad	57	16	2009
Ethiopia	177	61	2011
India	6834	4523	2014
Lesotho	43	27	2009
Madagascar	116	30	2009
Mali	283	283	2007
Mauritius	126	29	2009
Mozambique	253	253	2007
Nepal	243	162	2013
Nigeria	45	15	2009
South Africa	506	506	2007
Sri Lanka	310	12	2011
Tanzania	229		2013
Togo	13		2009
Uganda	233		2013
Zambia	243	243	2007
Total	11161	6449	