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The Impact of Special Economic Zones on Exporting Behavior*

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Abstract

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.

JEL classification: F14; J16.

Keywords: Exporting; Trade Barriers; Special Economic Zones.

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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 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). 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. Although there is a large body of case study analyses of SEZs, there is little rigorous evidence on their economic impacts, particularly with respect to their main goal of promoting exporting.[2] This paper fills that gap by using data on 11,161 firms across 21 Asian and African countries to test whether SEZs affect exports at either the extensive or intensive margin.[3] 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 environment.

[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.

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

[3] In particular, Zeng (2015) notes the lack of analysis of African SEZs.

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.[4] 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), 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 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. As such, they cannot establish a causal link between SEZs and their effects.

There are, however, exceptions to this rule.[5] 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.[6] 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

[4]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).

[5]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.

[6]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.

SEZ goal of export promotion, none of these studies estimate the effect of SEZs on exports themselves.[7]

To our knowledge, the only study to do so is Johansson and Nilsson (1997), who 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. An additional shortcoming of the existing literature is that none of them address the potential endogeneity of SEZs (i.e. that they may be established in areas where FDI or productive firms are already present). The exception to this is Wang (2013) who, as we do, uses a matching estimator (although whereas she matched across regions, we match across firms).

Using our firm-level data, we begin by comparing firm in SEZs to non-SEZ firms. We find that SEZ firms are generally more export oriented at the extensive and intensive margins, being 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. 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 the proceed by allowing 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.

[7]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.

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 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 3 describes our econometric approach and provides our results. Section 4 concludes.

2 Data and Summary Statistics

In this section, we introduce our data and compare the summary statistics between those firms in SEZs and those not.

2.1 Data Sources and Construction

Our firm-level data come from the World Bank's Enterprise Surveys.[8] 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.[9] This also limits the country coverage relative to

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

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

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.[10] 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.[11] After cleaning and harmonizing across the countries, the surveys have a similar layout and were conducted using a common methodology of random stratified sampling.[12] 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.[13]

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. 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.[14] 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 respec-

[10] 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.

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

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

[13] 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.

[14] 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.

tively 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.[15] Finally, and most importantly for our purposes, we have information on whether or not the firm self-identifies as being located in an SEZ.[16] 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.[17]

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).[18] More specifically, we combine three variables, the 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

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

[16]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.

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

[18]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.

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. 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 the extent to which NTBs reduce the ability of imported goods to compete in local markets. Both of these were scaled so that higher numbers indicated greater restrictions.his was was rescaled so that higher numbers indicate more burdensome taxes.[19] 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

[19]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.

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 .7% more likely to export with this difference highly significant. Likewise, SEZ firms export a greater value, where the result in column 3 indicates that SEZ firms export values are 31.6% nire than comparable firms.[20] The mean of the export intensity, however, is 43.6% 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. On the other hand, 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.

3 Regression Results

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. Therefore in this section, we turn to regression analysis. Specifically, we estimate for firm i in country j in sector s surveyed in year t:

[20] 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 $100 * (e^{\beta} - 1)$.

$$EXP_i = \beta_0 + \beta_1 SEZ_i + \beta_2 X_i + \theta_j + \theta_s + \theta_t + \varepsilon_i \quad (1)$$

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.[21] Further, we cluster the standard errors by country.

To this baseline, we introduce additional controls intended to proxy for the differential impact of export costs, taxes, and other country-specific attributes across SEZ and non-SEZ firms, where for country measure Y_c we estimate:

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

Note that from this, the marginal effect of being in an SEZ is a function of $\beta_1 + \alpha_1 * Y_c$. 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 $\alpha_1 * Y_c$ is positive, i.e. being in an SEZ increases exporting with an impact that approaches zero as the barrier rises.

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

3.1 The Extensive Margin of Trade

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.[22] 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.[23] 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. 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.

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).[24] 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.

[22] 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.

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

[24] 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.

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, all else equal, across the spectrum of export cost values. At the minimum of the export cost measure, the estimated marginal effect is positive and highly significant (with a probability value of .004). Likewise, for the maximum export costs, the impact is significantly negative (with a probability value of .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. As such, 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.[25] 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 .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 .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 -5.991), the net effect of an SEZ is $-1.979 + (-.326) * (-5.991) = -.026$, which as

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

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.[26] For countries with minimal NTBs, as with the export cost measure, the net effect is positive (albeit insignificant with a probability value of .723). For high NTB countries, the impact is negative and significant (with a probability value of .046 at the maximum NTB value) and equates 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 .001) and a significantly negative effect for high export cost nations (with a probability value of .0007).

One obvious concern with this estimation is the potential for endogeneity in the SEZ variable, i.e. firms located in SEZs are there precisely because they intend to export (or the opposite). Additionally, Ebenstein (2012) 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). In order to explore this, we utilized a propensity score matching estimator. With this approach, the goal is to estimate:

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

which is the difference in the exporting variable E (here, the exporter dummy) 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).[27] As any remaining differences

[26]Note that the kink in the graph is due to changes in other firm characteristics at this level of NTBs.
[27]Note that we continue to control for country, sector, and year dummies in this.

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).

With this caveat in mind, the results in Panel A, when using 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 $\tau_{ATT} = -.0159$. Thus, again, 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, no significant differences should be found after matching. As the table indicates, 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. 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 intensive 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.

3.2 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 simultaneous 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.

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. For open economies, the point estimate of this effect is negative but insignificant (as is the case at the sample mean). For high NTB countries, however, the effect is significantly positive (with a probability value of .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 $\tau_{ATT} = .1433$ with the post-estimation tests supporting the quality of the matches.

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. Comparable to Figure 3, we find no significant effect for low NTB countries but a significantly positive one for high NTB countries. At that end of the NTB distribution, the expected difference in exports is .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. Again, it is only for the heavily regulated countries where we estimate a 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 $\tau_{ATT} = .0161$. 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.

Combining these results, we find that, while there is limited evidence of SEZs affecting the export intensity of their firms, they do seem to encourage greater value of exports in countries with high NTBs, potentially due to reduced duties on imported intermediates. As we find no robust effect on the export intensity, this would suggest that 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 .35 from 4 for these countries, this means an increase in (non-logged) sales of 41.9%.

3.3 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. Finally, 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. All of these additional results are available on request.

4 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 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, 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. In addition, it indicates that SEZs may play a particularly useful role in a general overhaul of a country's policies. In open economies, they may affect the extensive margin positively with little effect on the intensive margin. 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.

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

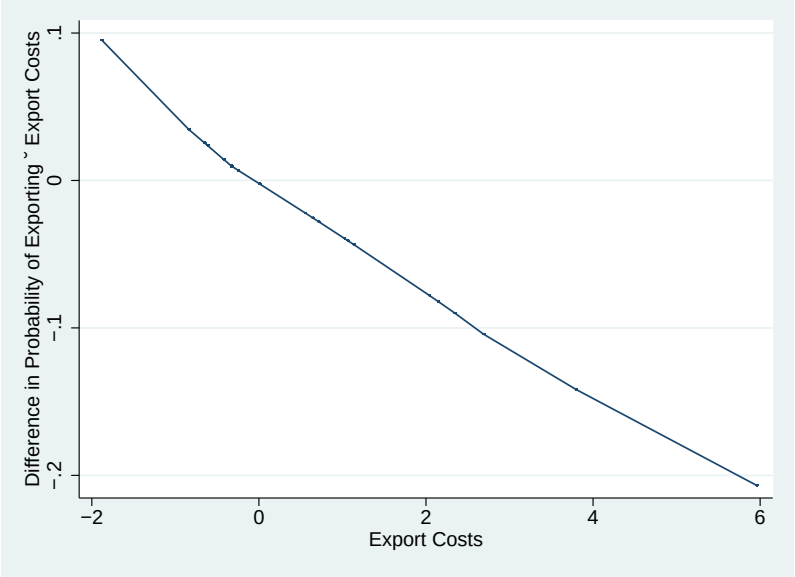

Figure 2: Change in the Probability of Exporting - NTBs

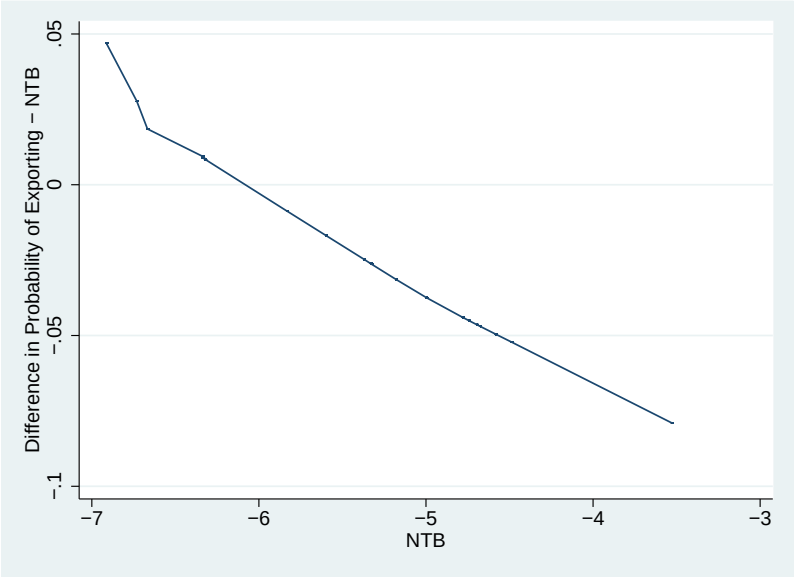

Figure 3: Change in Intensity of Exporting - NTBs

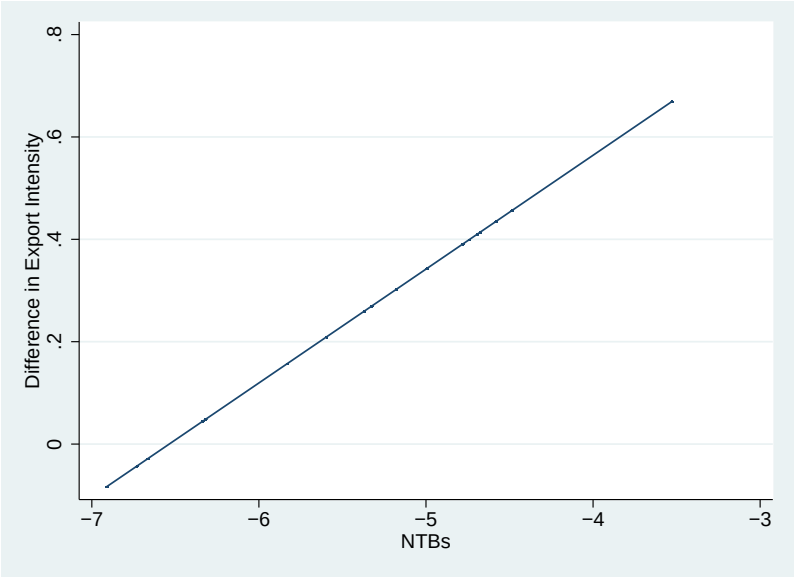

Figure 4: Change in Value of Exports - NTBs

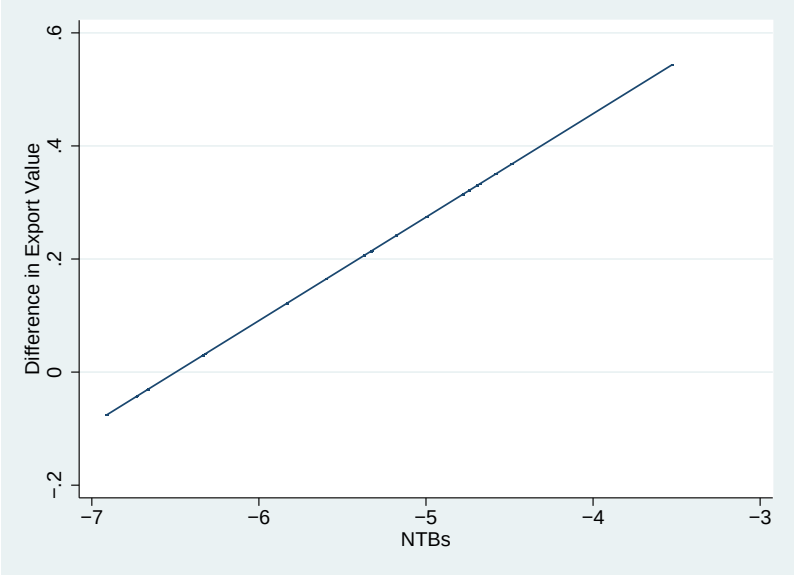

Figure 5: Change in Intensity of Exports - Regulation

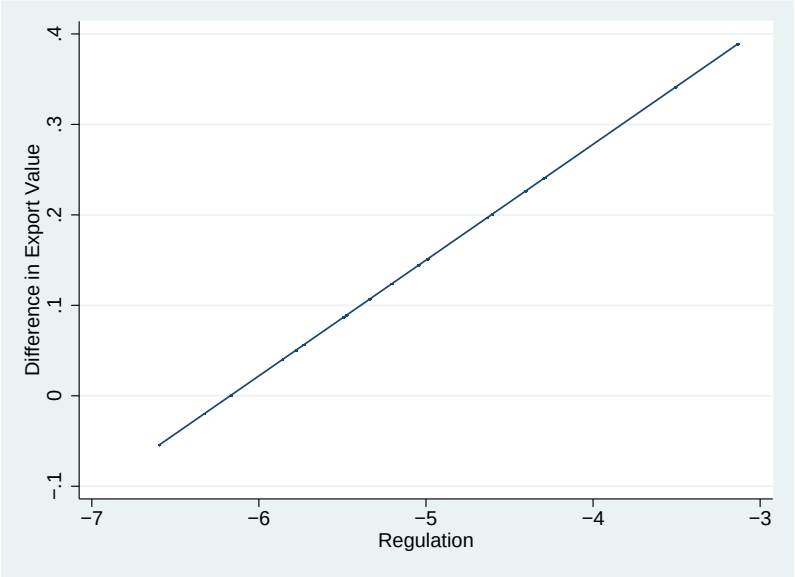

Table 1: Countries in the Sample

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	

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

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

Table 4: SEZ Versus non-SEZ Firms

Variable	SEZ	non-SEZ	Difference	Percent 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.

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.

Table 6: Propensity Score Matching: Probability of Exporting

Panel A: Selection

Sample	Treated	Controls	Difference	S.E.	T-stat
Unmatched	0.207978311	0.193411765	0.014566547	0.008305597	1.75
ATT	0.2	0.215879017	-0.015879017	0.014576247	-1.09

Panel B: Sensitivity Test

Variable	Matched	Treated	Control	T stat	Prob. Val.
Productivity	Unmatched	10.151	9.5113	22.3	0
	Matched	10.058	10.063	-0.15	0.884
Employment	Unmatched	3.9051	3.6355	9.92	0
	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
	Matched	0.46578	0.46994	-0.3	0.762
Multi-product	Unmatched	0.26569	0.39294	-13.26	0
	Matched	0.27713	0.29452	-1.4	0.162
License	Unmatched	0.13865	0.09106	7.16	0
	Matched	0.10851	0.09981	1.04	0.301
Import	Unmatched	0.12393	0.14024	-2.33	0.02
	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

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.

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

Table 9: Level 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

Selection					
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

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