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, share of revenues from exports, and value of exports. On average, we find that SEZs have no impact on exporting behaviour; this however masks cross-country heterogeneity. At the extensive margin, we find that SEZ firms in low-export cost economies and those in countries with weak institutions are more likely to export than their non-SEZ counterparts. Similarly, a firm in an SEZ tends to export a greater amount and earn a higher share of its sales from exports when it resides in a low-exporting cost country. These results suggest that the impact of SEZs on exporting may be highly contingent on local factors.

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1 Introduction

With the link between exports and economic growth well established, numerous government policies have sought to encourage exporting 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), in 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 cross-country 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 9171 firms across 13 Asian and African countries to test whether SEZs affect exports at either the extensive or intensive margin.[3] On average, we find little average impact on either margin of trade, a result found in both regression and matching estimation. This, however, hides cross-country variation in the impact of SEZs. When local institutions are weak, our results find that firms in SEZs are more likely to export. In addition, when export costs are initially low, being in an SEZ tends in increase the probability of exporting, the share of sales generated by exporting, and the value of exports. This suggests that for an SEZ to be successful, it may be necessary to consider it in the context of the local economic

[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.

environment and that they may be particularly effective when combined with other trade liberalization efforts.

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)

[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.

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.[7]

To our knowledge, there are only two studies to do so. First, 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) may be a driving factor in the effect of SEZs. Yücer and Sroën (2017) also consider conditionality when examining aggregate bilateral exports, finding that the impact of an SEZ on bilateral export values is contingent on MFN import tariffs. Their aggregate data, however, does not permit them to examine the extensive margin of trade or the role of firm characteristics. 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 aggregate findings 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

[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.

literature to be positively associated with exporting and factors that cannot be considered in the aggregate analyses of Johansson and Nilsson (1997) or Yücer and Sroën (2017). Turning to regression analysis, where we control for firm characteristics as well as 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 on average. That said, following the lead of Yücer and Sroën (2017), we then 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. Here, we find that firms in SEZs are more likely to export, particularly when the local invitational quality is weak. At the sample mean, this would indicate that a firm in an SEZ for the average institutional quality would have a 5.6% higher probability of exporting. When allowing the impact to vary according to export costs, we find that SEZs in low-exporting countries are associated with a higher probability of exporting, a greater share of income earned via exporting, and a larger value of exports.

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

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

whether or not a firm was in an SEZ.[9] 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. For eight of these countries, there was no variation in SEZ status across firms (i.e. all were in or out of SEZs). As there was no within country variation for these countries, they were omitted, leaving us with 13 countries in total.[10] The data are cross-sectional, with surveys taking place once in each country.[11] 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.[12] After cleaning and harmonizing across the countries, the surveys have a similar layout and were conducted using a common methodology of random stratified sampling.[13] 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 9,171 firms, 54.5% of which are in SEZs.[14]

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 the export share, where the non-logged value is between 0 and 1), and the log of the value of exports. In addition, we collected several control

[9]To our knowledge, ours is the first analysis of these more recent data. The difference between the standardized and unstandardized surveys is that the coding of variables across surveys has been standardized in the first whereas we were forced to do this ourselves for the surveys we use.

[10]There were 1990 firms with the firm-level controls in these nations. When we include them, significance rises along with the number of observations, but there was no qualitative impact.

[11]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.

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

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

[14]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.

variables identified by the literature as correlated with exporting. First, we include labour productivity, measured as the log of sales relative to employment.[15] 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.[16] Finally, and most importantly for our purposes, we have information on whether or not the firm self-identifies as being located in an SEZ.[17] 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.[18]

To explore this potential conditionality, 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). Note that as we do not have data on the export destination, unlike Yücer and Siroën (2017) we cannot control for destination-

[15] 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.

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

[17] 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. That said, we acknowledge this limitation of our data, especially in light of Yücer and Siroën (2017) who find differences across type of SEZ.

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

varying trade costs. 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. 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 Chad, with an average of 70 days. That said, within a country, all three measures are relatively highly correlated. Because of this, we follow Davies and Jeppesen (2015) and use principal component analysis to construct a source-specific export cost index. Details from this construction are found in Table 2. In light of Johansson and Nilsson (1997), we expect a negative coefficient here, i.e. SEZs have export promoting effects in open economies.

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 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.[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.

[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.

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, 21.1% of SEZ firms export, whereas 19.2% 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 4.9% 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 48.8% higher than comparable firms.[20] The mean of the export intensity, however, is only 8.86% higher for SEZ firms with this difference insignificant. 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 10.4% 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. They are also 16% 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.

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

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:

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

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 firm-level controls as discussed above, and the θs are a set of country, sector, and year dummy variables.[21] 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.[22] 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

[21] In unreported results, rather than using sector dummies, we used dummies for the firm's main product. This did not affect the results in any major fashion.

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

(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. Finally note that as the country controls are the same across firms within a nation, that their impact is captured by the country dummies.

3.1 The Extensive Margin of Trade

Table 5 presents our estimates for the probability of exporting. Here, we use a logit estimator due to the binary nature of the dependent variable.[23] 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.[24] 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, or, in with Johansson and Nilsson (1997) in mind, the other way around. 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).[25] In any case, we find no significant impact in column (3) (but do in (8) as discussed below).

[23]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 due to the potential bias; see Green (2004).

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

[25]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.

In columns (4) through (7), we repeat this exercise, replacing the export cost interaction with an interaction using the tax, regulation, institution, and NTB indices. Only for column (6), where we allow the impact of an SEZ to vary with invitational quality, do we find a significant impact. Here, the net effect of the SEZ at the sample mean is estimated to be $-1.228 + .206 * -5.51 = .0919$, which would result in a 5.6% increase in the probability of exporting. That said, we fail to reject the equality of this to zero. Nevertheless, as institutional quality worsens (gets closer to zero), this impact rises and at the worse end of the institutional quality distribution, we cannot reject the null of a net positive impact of an SEZ on the probability of exporting. Finally, in column (8) we include all of these interactions where we again find a positive impact of the SEZ when institutional quality is weak. Here, we also find a negative impact on the interaction with exporting costs.[26] 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.[27]

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). Further, the above results are correlations and not causation, something that one would need panel data to determine. As an attempt to deal with these issues, we proceed by using 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

[26]The correlation between institutions and export costs is .6182, so given their opposite signs, omitting the institution interaction may explain why we find no effect from the export cost interaction in (3) but we do in (8).

[27]Note that since by construction the mean export cost is zero, that at the sample mean the SEZ effect would not include the coefficient from that interaction.

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).[28] 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 .00004 worked best with respect to the tests of appropriateness (see Panel B of Table 6, discussed momentarily).[29] This, however, comes at the cost of the number of firms for which a match could be found, resulting in only 1257 SEZ and 4169 non-SEZ firms for which there was common support (i.e. 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 supports the above finding of no average difference in the probability of exporting between SEZ and non-SEZ firms after controlling for other firm characteristics. 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.

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

[29] With higher calipers, we continued to find differences between matched firms for some characteristics. Smaller calipers than this, however, resulted in few matched SEZ firms. Thus, this choice is an attempt to balance sufficient numbers with quality matching.

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 suggesting that while on average their impact may be negligible, variation in local characteristics are important for determining their effect. In particular, we find evidence suggesting that they may help overcome the trade barriers generated by weak insinuations. Further, they may support exporting in countries where exporting is relatively low cost.

3.2 The Intensive Margin of Trade

In this section, we use two measures of the intensive margin, the logged share of sales generated via exports (export share) 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 share using the same approach as in Table 5. Because the export share cannot exceed zero (the log of 1), we use a Tobit estimator. As before, SEZs have effects conditional on export costs in columns (3) and (8) that suggest that SEZs encourage exporting but only in relatively open economies. Recalling that at the sample mean export costs are zero, consider instead the average "open" economy (with a negative export cost) with a mean export cost of -.88. Here, using the estimates of column (3), an SEZ firm would have an estimated log export share that is 46.8% higher. 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 944 matched firms for which we had common support. However, this included only 149 of the 904 SEZ exporters. With

that caveat in mind, the post-estimation tests suggest quality matching and in line with the regression results, indicate no average impact of SEZ status.

Table 9 turns to the export value (again for the set of exporting firms). As with the export share results, we find an impact only for the export cost interaction where as before the indication is that SEZs may encourage exports, but then primarily for countries where it is relatively easy to export. As with the share estimates, considering an open economy where trade costs are -0.88, this would suggest that a firm's export value rises by 3.6% if it is in an SEZ. Finally, Table 10 shows the statistics from the matching for export value.

Combining these results, we find that at the intensive margin, SEZs may encourage additional exporting when export costs are relatively low. This, combined with the intensive results, supports the findings of Johansson and Nilsson (1997).

3.3 Additional Regressions

To explore the data further, we examined several alternative samples. First, 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 for India-only and India-omitting samples (which represents a large share of the sample). In each case, comparable signs for the coefficients were found but significance was much weaker, again possibly due to smaller sample sizes.[30] All of these additional results are available on

[30]Note that in the India-only sample, we could not include interactions with the country variables.

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 limited econometric evidence testing the notion, especially across countries. We contribute to the debate by providing the first cross-country, firm-level econometric study examining the relationship between SEZs and exporting at both the extensive and intensive margins. The resulting pattern is a nuanced one. At the extensive margin, SEZs increase the likelihood of exporting but primarily when institutions are weak or exporting is already fairly easy. At the intensive margin, open economies may again see exporting rise both in value and the share of income earned via exporting. 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 exporting policies.

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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
Mauritius	126	29	2009
Nigeria	45	15	2009
Sri Lanka	310	12	2011
Total	9117	5002	

Table 2: Construction of Export Costs

Panel A:	1	2
Number of obs.		9171
Retained factors		1
No. parameters		3
Panel B:	Eigenvalue	Proportion
Factor1	1.8546	0.6182
Factor2	0.95274	0.3176
Factor3	0.1926	0.0642
Panel C:		
Variables	Factor1 Loadings	Uniqueness
Documents to export	0.3356	0.8874
Time to export	0.9444	0.1082
Cost to export	0.9220	0.1498

Table 3: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Exporter	9,171	0.2022	0.4016	0	1
Export Share	1,854	-1.029	1.1541	-4.6052	0
Export Sales	1,854	14.0389	2.1273	6.4813	20.2086
Productivity	9,171	9.877	1.4193	1.9025	20.2663
Employment	9,171	3.7919	1.3197	0	11.0744
Age	9,171	2.7257	0.7849	0	5.2417
Foreign Owned	9,171	0.0351	0.1841	0	1
Quality Cert.	9,171	0.4104	0.4919	0	1
Multi-product	9,171	0.3137	0.464	0	1
Import	9,171	0.1252	0.3309	0	1
License	9,171	0.1187	0.3235	0	1
Export Cost	9,171	-0.1244	0.7885	-1.8146	6.5496
Taxes	9,171	-4.0497	0.5397	-4.8	0
Regulations	9,171	-5.6173	0.5555	-6.598	-3.136
Institutions	9,171	-5.5105	0.8512	-5.9	0
NTBs	9,171	-6.119	0.4836	-6.913	-3.529

Table 4: SEZ Versus non-SEZ Firms

Variable	SEZ	non-SEZ	Percent Change
Exporter	0.2105	0.1921	4.9***
Export Share	-1.1881	-0.8199	8.86
Export Sales	14.0804	13.9843	48.83***
Productivity	10.1651	9.5314	16.41***
Employment	3.9126	3.6471	35.92***
Age	2.6955	2.7619	-10.41***
Foreign Owned	0.0346	0.0357	2.64***
Quality Cert.	0.5346	0.2615	15.98***
Multi-product	0.2539	0.3855	-1.91*
Import	0.1134	0.1394	3.2***
License	0.1415	0.0914	8.11***
Obs.	5002	4169	

Notes: Percent change is $100(e^{\beta} - 1)$ where β is the SEZ coefficient from a regression using an SEZ, country, sector, and year dummies. ***, **, and * on difference denote significance of this SEZ coefficient at the 1%, 5%, and 10% levels respectively. 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	0.232***	0.230***	0.231***	0.230***	0.230***	0.230***	0.230***	0.231***
	(0.0264)	(0.0264)	(0.0264)	(0.0264)	(0.0264)	(0.0264)	(0.0264)	(0.0264)
Employment	0.621***	0.620***	0.621***	0.620***	0.620***	0.619***	0.620***	0.620***
	(0.0281)	(0.0281)	(0.0281)	(0.0281)	(0.0281)	(0.0281)	(0.0281)	(0.0281)
Age	0.208***	0.214***	0.215***	0.214***	0.214***	0.211***	0.214***	0.211***
	(0.0459)	(0.0464)	(0.0464)	(0.0463)	(0.0464)	(0.0463)	(0.0464)	(0.0465)
Foreign Owned	0.767***	0.760***	0.770***	0.743***	0.739***	0.725***	0.755***	0.731***
	(0.198)	(0.198)	(0.199)	(0.199)	(0.200)	(0.201)	(0.200)	(0.202)
Quality Cert.	0.777***	0.768***	0.766***	0.772***	0.769***	0.772***	0.768***	0.767***
	(0.0791)	(0.0795)	(0.0796)	(0.0797)	(0.0795)	(0.0795)	(0.0795)	(0.0794)
Multi-product	0.00967	0.0139	0.0157	0.0131	0.0138	0.0117	0.0136	0.0166
	(0.0753)	(0.0754)	(0.0754)	(0.0754)	(0.0754)	(0.0755)	(0.0754)	(0.0754)
License	-0.0623	-0.0698	-0.0718	-0.0678	-0.0669	-0.0674	-0.0691	-0.0724
	(0.0944)	(0.0949)	(0.0950)	(0.0949)	(0.0949)	(0.0950)	(0.0950)	(0.0952)
Import	1.247***	1.247***	1.249***	1.245***	1.245***	1.239***	1.247***	1.238***
	(0.0924)	(0.0923)	(0.0923)	(0.0924)	(0.0925)	(0.0925)	(0.0924)	(0.0928)
SEZ		0.0824	0.0729	0.915	1.064	1.228**	0.349	2.270
		(0.0808)	(0.0826)	(0.635)	(0.805)	(0.560)	(1.073)	(1.742)
Export Costs*SEZ			-0.0841					-0.323*
			(0.110)					(0.173)
Taxes*SEZ				0.204				0.0260
				(0.154)				(0.394)
Regulation*SEZ					0.173			-0.0445
					(0.141)			(0.410)
Institutions*SEZ						0.206**		0.410**
						(0.0996)		(0.186)
NTBs*SEZ							0.0434	0.0144
							(0.174)	(0.409)
Constant	-9.070***	-9.080***	-9.039***	-9.333***	-9.176***	-9.438***	-9.071***	-9.613***
	(0.566)	(0.564)	(0.569)	(0.630)	(0.568)	(0.633)	(0.566)	(0.751)
Observations	9,171	9,171	9,171	9,171	9,171	9,171	9,171	9,171

Notes: ***, **, and * denote significance at the 1%, 5%, and 10% levels respectively. All specifications include country, sector, and year dummies.

Table 6: Propensity Score Matching: Probability of Exporting

Panel A: Selection

Sample	Treated	Controls	Difference	S.E.	T-stat
Unmatched	0.2105	0.1921	0.0184	0.0084	2.18
ATT	0.183	0.2094	-0.0264	0.0179	-1.48

Panel B: Sensitivity Test

Variable		Treated	Control	T stat	Prob. Val.
Productivity	Unmatched	10.165	9.5314	21.83	0
	Matched	10.106	10.057	0.95	0.34
Employment	Unmatched	3.9126	3.6471	9.64	0
	Matched	3.8165	3.7453	1.39	0.163
Age	Unmatched	2.6955	2.7619	-4.03	0
	Matched	2.7562	2.7329	0.78	0.438
Foreign Owned	Unmatched	0.0346	0.0357	-0.3	0.765
	Matched	0.0278	0.0372	-1.32	0.187
Quality Cert.	Unmatched	0.5346	0.2615	27.55	0
	Matched	0.4877	0.4608	1.35	0.178
Multi	Unmatched	0.2539	0.3855	-13.66	0
	Matched	0.2792	0.2952	-0.89	0.376
License	Unmatched	0.1415	0.0914	7.41	0
	Matched	0.1193	0.1085	0.86	0.391
Import	Unmatched	0.1134	0.1394	-3.75	0
	Matched	0.1058	0.1131	-0.59	0.558

Sample	Pseudo R^2	LR χ^2	$p > \chi^2$
Raw	0.233	2947.76	0
Matched	0.008	28.52	0.93

Table 7: Export Share

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Productivity	-0.0490*	-0.0513*	-0.0498*	-0.0516*	-0.0515*	-0.0512*	-0.0519*	-0.0517*
	(0.0282)	(0.0284)	(0.0285)	(0.0284)	(0.0284)	(0.0284)	(0.0284)	(0.0285)
Employment	0.0152	0.0154	0.0170	0.0156	0.0157	0.0157	0.0154	0.0192
	(0.0305)	(0.0305)	(0.0305)	(0.0305)	(0.0305)	(0.0306)	(0.0305)	(0.0305)
Age	-0.215***	-0.209***	-0.206***	-0.208***	-0.210***	-0.208***	-0.209***	-0.208***
	(0.0470)	(0.0472)	(0.0471)	(0.0473)	(0.0472)	(0.0473)	(0.0472)	(0.0469)
Foreign Owned	0.0256	0.0159	0.0188	0.0306	0.000442	0.0216	0.00852	-0.0338
	(0.184)	(0.184)	(0.182)	(0.188)	(0.185)	(0.186)	(0.183)	(0.187)
Quality Cert.	-0.121	-0.128	-0.131	-0.128	-0.130	-0.127	-0.129	-0.140*
	(0.0812)	(0.0813)	(0.0814)	(0.0813)	(0.0810)	(0.0812)	(0.0810)	(0.0809)
Multi-product	-0.266***	-0.261***	-0.259***	-0.262***	-0.260***	-0.261***	-0.261***	-0.254***
	(0.0750)	(0.0748)	(0.0748)	(0.0748)	(0.0749)	(0.0748)	(0.0748)	(0.0752)
License	0.0661	0.0613	0.0575	0.0594	0.0632	0.0614	0.0623	0.0622
	(0.0947)	(0.0946)	(0.0944)	(0.0948)	(0.0950)	(0.0946)	(0.0949)	(0.0945)
Import	-0.0627	-0.0651	-0.0578	-0.0621	-0.0676	-0.0632	-0.0670	-0.0634
	(0.0800)	(0.0798)	(0.0799)	(0.0799)	(0.0797)	(0.0797)	(0.0796)	(0.0794)
SEZ		0.0923	0.0623	-0.284	0.529	-0.0542	0.569	2.568
		(0.0784)	(0.0799)	(0.523)	(0.749)	(0.545)	(1.078)	(1.675)
Export Costs*SEZ			-0.197**					-0.342**
			(0.0982)					(0.152)
Taxes*SEZ				-0.0922				-0.0215
				(0.128)				(0.408)
Regulation*SEZ					0.0769			0.176
					(0.130)			(0.388)
Institutions*SEZ						-0.0265		-0.0137
						(0.0964)		(0.162)
NTBs*SEZ							0.0772	0.274
							(0.173)	(0.359)
Sigma	1.257***	1.257***	1.255***	1.257***	1.257***	1.257***	1.257***	1.253***
	(0.0301)	(0.0302)	(0.0301)	(0.0302)	(0.0302)	(0.0302)	(0.0302)	(0.0301)
Constant	-0.309	-0.329	-0.132	-0.159	-0.398	-0.267	-0.301	0.0260
	(0.453)	(0.457)	(0.457)	(0.466)	(0.472)	(0.489)	(0.456)	(0.465)
Observations	1,854	1,854	1,854	1,854	1,854	1,854	1,854	1,854

Notes: ***, **, and * denote significance at the 1%, 5%, and 10% levels respectively. All specifications include country, sector, and year dummies.

Table 8: Propensity Score Matching: Export Intensity

Panel A: Selection

Sample	Treated	Controls	Difference	S.E.	T-stat
Unmatched	-1.1881	-0.8181	-0.37	0.0536	-6.9
ATT	-1.1391	-1.3714	0.2323	0.1494	1.56

Panel B: Sensitivity Test

Variable		Treated	Control	T stat	Prob. Val.
Productivity	Unmatched	10.476	9.7966	10.24	0
	Matched	10.5	10.743	-1.44	0.152
Employment	Unmatched	4.792	5.0061	-3.31	0.001
	Matched	4.9157	4.6946	1.43	0.155
Age	Unmatched	2.8197	2.9307	-3.06	0.002
	Matched	2.7884	2.949	-1.72	0.087
Foreign Owned	Unmatched	0.1016	0.0704	2.34	0.019
	Matched	0.0873	0.1007	-0.4	0.693
Quality Cert.	Unmatched	0.7056	0.5006	9.17	0
	Matched	0.6779	0.7248	-0.88	0.377
Multi	Unmatched	0.2574	0.4164	-7.32	0
	Matched	0.3222	0.2752	0.88	0.377
License	Unmatched	0.1985	0.1975	0.05	0.958
	Matched	0.2215	0.1678	1.17	0.243
Import	Unmatched	0.3466	0.3736	-1.2	0.232
	Matched	0.3423	0.3758	-0.6	0.548

Sample	Pseudo R^2	LR χ^2	$p > \chi^2$
Raw	0.233	587.9	0
Matched	0.072	28.57	0.488

Table 9: Level of Exports

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Productivity	0.959***	0.957***	0.958***	0.957***	0.957***	0.957***	0.957***	0.957***
	(0.0202)	(0.0203)	(0.0203)	(0.0203)	(0.0203)	(0.0203)	(0.0203)	(0.0204)
Employment	1.004***	1.004***	1.004***	1.004***	1.004***	1.004***	1.004***	1.006***
	(0.0212)	(0.0212)	(0.0212)	(0.0212)	(0.0212)	(0.0212)	(0.0212)	(0.0212)
Age	-0.103***	-0.0980***	-0.0966***	-0.0974***	-0.0988***	-0.0976***	-0.0988***	-0.0970***
	(0.0321)	(0.0321)	(0.0321)	(0.0322)	(0.0321)	(0.0322)	(0.0321)	(0.0321)
Foreign Owned	0.00456	-0.00495	-0.00378	0.00221	-0.0199	-0.00100	-0.0164	-0.0346
	(0.123)	(0.123)	(0.122)	(0.125)	(0.125)	(0.125)	(0.123)	(0.126)
Quality Cert.	-0.0658	-0.0712	-0.0721	-0.0710	-0.0729	-0.0708	-0.0729	-0.0774
	(0.0526)	(0.0527)	(0.0527)	(0.0527)	(0.0525)	(0.0526)	(0.0525)	(0.0524)
Multi-product	-0.152***	-0.148***	-0.148***	-0.149***	-0.147***	-0.149***	-0.148***	-0.143***
	(0.0505)	(0.0504)	(0.0504)	(0.0504)	(0.0505)	(0.0504)	(0.0504)	(0.0508)
License	0.0472	0.0434	0.0417	0.0428	0.0440	0.0436	0.0435	0.0425
	(0.0610)	(0.0609)	(0.0608)	(0.0609)	(0.0609)	(0.0609)	(0.0609)	(0.0606)
Import	-0.0732	-0.0763	-0.0730	-0.0750	-0.0783	-0.0753	-0.0785	-0.0743
	(0.0553)	(0.0553)	(0.0553)	(0.0553)	(0.0553)	(0.0552)	(0.0552)	(0.0552)
SEZ		0.0727	0.0567	-0.135	0.492	-0.0245	0.701	1.806*
		(0.0572)	(0.0578)	(0.408)	(0.445)	(0.373)	(0.627)	(1.044)
Export Costs*SEZ			-0.110					-0.212**
			(0.0722)					(0.104)
Taxes*SEZ				-0.0511				0.0291
				(0.101)				(0.251)
Regulation*SEZ					0.0747			0.0693
					(0.0804)			(0.235)
Institutions*SEZ						-0.0177		-0.0741
						(0.0682)		(0.119)
NTBs*SEZ							0.103	0.272
							(0.103)	(0.239)
Constant	0.262	0.240	0.384	0.258	0.219	0.253	0.179	0.378
	(0.595)	(0.601)	(0.622)	(0.604)	(0.596)	(0.607)	(0.599)	(0.623)
Observations	1,854	1,854	1,854	1,854	1,854	1,854	1,854	1,854
R-squared	0.796	0.796	0.796	0.796	0.796	0.796	0.796	0.797

Notes: ***, **, and * denote significance at the 1%, 5%, and 10% levels respectively. All specifications include country, sector, and year dummies.

Table 10: Propensity Score Matching: Export Value

Sample	Treated	Controls	Difference	S.E.	T-stat
Unmatched	13.9846	0.0959	0.0999	0.96	0
ATT	14.2762	14.0663	0.2099	0.2692	0.78