Do Special Economic Zones Boost Employment? Evidence from a Meta-Analysis

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ABSTRACT

This paper investigates whether Special Economic Zones (SEZs) effectively foster employment through a comprehensive meta-analysis. While numerous country-level studies have examined SEZ policies, their results remain fragmented and potentially biased by methodological and publication-related factors. To address this gap, we compile a meta-regression dataset of 851 estimates from 33 empirical studies that quantify the impact of SEZs on employment. Once corrected for publication bias, the average effect of SEZ programs on employment approaches zero. However, heterogeneous results emerge across different development contexts, with developing countries recording significant employment gains compared to their developed counterparts.

Keywords: Employment; Place-Based Policies; Special Economic Zones; Meta-Analysis; Publication Bias

1 Introduction

Special Economic Zones (SEZs) are geographically delimited areas where governments implement special rules for businesses and investors, usually in the form of tax incentives and simplified administrative procedures (Bost, 2019). Among the expected outcomes of this policy measure, employment growth plays a central role (UNCTAD, 2019). Indeed, SEZs are typically established in less developed regions to reduce economic disparities within a country. In this regard, several studies have evaluated the effectiveness of SEZ programs in generating employment over time. Busso et al. (2013) analyzed American Empowerment Zones using a Difference-in-Differences (DiD) approach with parametric reweighting on business data, finding that these zones increased local employment and wages without significantly raising living costs. In Poland, Ciżkowicz et al. (2017) employed panel and Spatial Durbin models to reveal significant employment growth and investment gains in SEZ powiats, while Jensen (2018) used an adjusted DiD at the gmina level to confirm improved employment after the 1990s. In developing countries, Chinese SEZs are often highlighted as a prominent example of place-based policies that effectively promote economic development (Alder et al., 2016). For example, Lu et al. (2019) analyzed Chinese SEZs by applying DiD estimation, boundary discontinuity techniques and spillover analysis on manufacturing firms from the National Bureau of Statistics, showing SEZs improved both employment and productivity. In Cambodia, Brussevich (2024) employed event-study methods with propensity score weighting to show that SEZs increased employment—especially for women—and reduced income inequality.

From this brief overview of recent literature on the impact of SEZ policies on employment, we can note that all empirical studies have relied on national-level data, with no cross-country analyses conducted. This is mainly due to the inherent difficulty of accessing data on various place-based policies implemented across different countries. Analyzing SEZ policies often requires consulting local data, which may differ substantially from country to country, making it difficult to use in cross-country analyses, or may simply be unavailable. Moreover, linguistic barriers may arise when accessing local data or legislation on SEZs, as these materials might be available only in local languages.

If conducting a cross-country analysis is difficult, a quantitative synthesis of findings from previous national-level empirical studies becomes particularly valuable. This is precisely what meta-analysis offers. To the best of our knowledge, no meta-analyses have yet been conducted to synthesize the impact of SEZ programs on employment. This paper aims to fill this gap by performing a meta-analysis based on a new meta-regression dataset of empirical studies that measured the employment effects of SEZ policies. In particular, section 2 presents a review of both theoretical and empirical studies on SEZs, with a primary focus on employment outcomes. Section 3 details the data and methods used, while section 4 discusses the main results.

2 Literature Review

Subsections 2.1 and 2.2 provide an overview of the theoretical and empirical literature on SEZs and their relationship with employment.

2.1 Theoretical Studies

Early theoretical arguments on place-based policies, such as SEZs, were skeptical about their ability to increase aggregate employment, viewing them instead as mere tools that redistribute job opportunities while generating unnecessary costs (Winnick, 1966). However, it has been argued that even if place-based policies were limited to redistributing jobs, they could still generate broader economic effects. This is because there would be a nonlinear relationship between the density of skilled workers and productivity, such that moving skilled people from one location to another would increase productivity in the target area more than it would decrease productivity in the origin area (Neumark & Simpson, 2015).

Nowadays such policies are justified from both social and economic perspectives. From a social point of view, Glaeser and Gottlieb (2008) argue that a primary justification for place-based policies lies in equity and egalitarian considerations, as they support residents of disadvantaged areas. From an economic perspective, these policies are considered necessary because private investors alone are often unable to revitalize certain regions, creating a need for coordinated public and private investment to stimulate local development. More generally, place-based policies are justified to address market imperfections, including labor market frictions and rigidities. In particular, the theoretical framework proposed by Kline and Moretti (2014) provides a rationale for such policies, as labor market institutions and bargaining behavior may lead to persistently high unemployment in certain communities.

SEZ policies can also be interpreted as potential catalysts for the transition from a planned to a market economy, as discussed by Litwack and Qian (1998) in their theoretical model. A similar argument is made in the SEZ development framework by Tang (2023), where newly industrialized economies launch SEZ programs to experiment with economic reforms in a gradualist manner. However, for such policies to be effective, they must be explicitly designed as place-based, moving the decision making about SEZs closer to the people with the relevant local knowledge; otherwise SEZs tend to be poorly designed and misplaced (Moberg, 2015). For example, the United Nations Conference on Trade and Development (UNCTAD, 2019) proposed the SEZ development ladder, according to which SEZ programs should exploit the comparative advantages of countries associated with a certain level of economic development to achieve the corresponding policy objectives. Duranton and Venables (2018) highlight that successful SEZs need to be located in places consistent with their objectives (e.g., export- or import-oriented zones need good access to ports), exploiting their comparative advantage (e.g., manufacturing-oriented areas should promote the development of that sector). Moreover, tax incentives alone are insufficient, as they should be accompanied by other benefits, such as streamlined bureaucratic procedures and services supporting investors operating in SEZs. Finally, policy implementation is crucial, requiring a coordinated action plan across agencies, with the SEZ authority responsive to firms' concerns. Similar policy recommendations were provided by Zeng (2021), who suggested that SEZs, after a rigorous assessment of the local context, should become an integral part of a long-term development strategy, supported by both physical and non-physical infrastructure, as well as appropriate legal and institutional frameworks to make the zones attractive to businesses and investors. Overall, these factors contribute to a true place-based policy capable of exploiting Marshall-Jacobs externalities and knowledge spillovers, increasing employment and fostering broader economic development (Moretti, 2012).

Despite this, SEZs may fail to generate employment and economic growth in the treated areas, as their effectiveness depends not only on how they are implemented but also on contextual factors exogenous to this policy tool, such as the quality of institutions. In this regard, Alkon (2018) developed a theoretical framework to explain why SEZs did not generate developmental spillovers in India. According to this framework, SEZs can foster local development only when local politicians and bureaucrats are incentivized, through elections or career advancements, to support such development and when opportunities for rent extraction are sufficiently constrained. A similar argument has been made by Moberg (2015), who argued that a solution to the incentive problem, according to which SEZs create opportunities for rent-seeking and hence the incentives for policymakers to use them for personal gains, is a democratic system, as this links a politician's policy decisions closely to their chances of reelection.

If rent extraction by policymakers and bureaucrats undermines the effectiveness of SEZ policies, the opposite may occur—at least in terms of job creation and preservation—when firms in agglomerating industries extract rents, as argued by Brülhart and Simpson (2018) in their theoretical model. In this context, firms in highly agglomerated industries may have greater opportunities to extract rents from policymakers in the regions where they are concentrated, attracting higher per-job subsidies. This is particularly likely when place-based policies are implemented at the local level, as voters may be less informed by the media about local government actions than about those of the national government. Additionally, incumbent firms in mature or declining industries may lobby more aggressively in response to negative shocks, resulting in higher subsidies per job.

Dhingra et al. (2009) argue that locating firms in SEZs can enhance their performance through both endogenous factors (such as firms' investment in competitive resources and capabilities, linkages with other firms, and entrepreneurial ability) and exogenous factors (such as government and institutional support, and access to factors of production). However, not all firms find it convenient to locate in an SEZ, as this decision is influenced by their initial performance, according to the theoretical framework proposed by Liu and Jin (2022). In this context, the least productive firms are unable to bear the high fixed costs of entering SEZs and becoming exporters. Conversely, more productive firms facing high intermediate input tariffs are more likely to operate within SEZs, since the benefits gained from the zones outweigh the initial costs and allow them to avoid high production tariffs.

2.2 Empirical Evidence

Notable SEZ policies introduced in the United States include the Enterprise Zone (EZ) programs, initiated autonomously by several states with the stated aim of boosting employment in distressed community areas. Bondonio and Engberg (2000) assessed EZ programs from five states employing econometric methods on data at the US Postal ZIP code level for the period 1981–1994, and found that these programs did not significantly affect local employment. In the meantime, the US Federal government introduced the Empowerment Zone program, whose effects on employment were assessed by Busso et al. (2013). Using an adjusted Difference-in-Differences (DiD) approach on census data from 1980, 1990, and 2000, they found that these zones increased employment and wages for local residents without significantly affecting the cost of living.

The most well-known SEZ policy in Europe is the Polish program, which produced positive effects according to several studies. Ciżkowicz et al. (2017), using panel data from 2003 to 2012 at the powiat level, found a strong positive effect of SEZs on employment, along with a comparatively weaker impact on investment. Ambroziak and Hartwell (2018), through a counterfactual evaluation of statistically comparable powiats between 2005 and 2013, confirmed that SEZs enhanced investment attractiveness and job creation. Applying a panel-adjusted DiD approach to gmina-level data from 1995 to 2014, Jensen (2018) documented that SEZs generated employment gains, particularly in the subsequent transition period of the 1990s. Another example of SEZ-related policies in Europe is the *Zones Franches Urbaines* program in France, which produced mixed results, as shown by Givord et al. (2018) in a long-term evaluation. Using a DiD strategy on administrative data from 1995 to 2012, they observed a strong positive impact of the SEZ initiative on economic activity in the short run, but long-run estimates suggest that the program fails to foster self-sustaining economic development. In terms of employment gains, the policy produced only small effects, suggesting that it lacked precise targeting.

Similar positive employment effects have been observed in China. Lu et al. (2019) examined the impact of China's SEZ program using a DiD approach on firm-level data from the 2004 and 2008 economic censuses, showing that SEZs increased employment and productivity. By exploiting the diverse types of SEZs across more than 2,400 counties in China over 30 years, Lu et al. (2023) documented a positive relationship between increased job opportunities and wage premiums inside SEZs and local educational outcomes, especially in technology-oriented zones.

Brussevich (2024) performed an event-study analysis using a novel geo-tagged database of Cambodian SEZs at the district level between 2007 and 2017, and found that these zones contributed to employment gains, particularly for women, and reduced income inequality. In contrast, the evidence from Africa is more mixed: Rodríguez-Pose et al. (2022) noted that while some SEZs with well-targeted strategies perform better, the overall capacity of African SEZs to attract firms and generate employment remains limited. Similar outcomes have been documented in India by Alkon (2018), who examined spillover effects of the SEZ policy using the Covariate Balancing Propensity Score methodology on the 2001 and 2011 Indian census data, showing that Indian SEZs have failed to promote local socioeconomic development. However, some sectors benefited from the Indian SEZ policy, as shown by Gallé et al. (2024). In particular, their analysis indicates that gains in manufacturing and service employment were accompanied by a decline in agricultural labor, especially for women, suggesting that the policy contributed to structural change. Focusing on developing economies more broadly, Frick and Rodríguez-Pose (2019) used nightlight data as a proxy for SEZ performance between 2007 and 2012. Their analysis showed that sustaining SEZ growth over time is challenging and that zone size is a key determinant of long-term growth.

3 Data and Methods

The data and methods used in this study are detailed in subsections 3.1 and 3.2, respectively.

3.1 Data

We constructed the dataset following the guidelines reported by Havránek et al. (2020). In particular, the search strategy that led to the inclusion of primary studies from which we collected data is detailed in the Appendix A. In summary, we collected 851 estimates and their standard errors, along with additional information on the estimated models and study characteristics from 33 primary studies. To be included in our analysis, a study must include econometric estimates in which the dependent variable is a measure of employment and the independent variable captures the SEZ policy. Consequently, the general econometric model can be written as follows (we omit subscripts for simplicity):

$$emp = \beta_0 + \beta_1 SEZ + \beta_x Z_x + \epsilon$$

where emp denotes the measure of employment, explained as a function of the SEZ regressor, a vector Z of additional explanatory variables (if included), and the usual error term ϵ. In the context of a meta-analysis, the estimate of β_1 represents the effect size, which we use as the dependent variable in meta-regression models to estimate the average effect size corrected for both publication bias and systematic heterogeneity across primary studies.

While all primary studies represented the SEZ variable as an indicator equal to one in the presence of the policy and zero otherwise, employment was measured in different ways (e.g., employment growth, number of employees, etc.). Additionally, model specifications differed across primary studies, with some adopting a level-level specification and others a log-level one. Consequently, we standardized the effect size and its standard error, obtaining unitless measures that neither depend on the choice of the particular measure of the dependent variable nor on the model specification. We focus on the semielasticity, with details on the transformation provided in the Appendix B.

Semielasticities represent one of the most common standardized effect sizes in meta-analyses of economic research (Ioannidis et al., 2017), offering several advantages from both practical and economic perspectives. In the first case, obtaining semielasticities is relatively straightforward, as it requires only the effect size from a log-level model, or both the effect size and the mean of the dependent variable in the case of a level-level model. From an economic perspective, semielasticities offer an intuitive interpretation, as they represent the expected percentage change in employment resulting from the introduction of the SEZ policy.

As a robustness check, we also employed the partial correlation coefficient (PCC) for the effect size standardization. Together with semielasticity (and elasticity), it constitutes the most commonly used standardized effect size in economic meta-analyses (Ioannidis et al., 2017). The PCC is also relatively easy to compute, as it requires only the t-statistic associated with the effect size and the degrees of freedom of the estimated model.[1] However, it lacks a direct economic interpretation (Heimberger, 2023). While the sign of the PCC can be interpreted similarly to that of the semielasticity, its size cannot be translated into expected percentage changes. For this reason, our meta-analysis primarily relies on the semielasticity. To reduce the influence of potential outliers, we excluded semielasticities and their standard errors at or below the 2nd percentile and at or above the 98th percentile (Gechert & Heimberger, 2022).

3.1.1 Variables in the meta-regression dataset

We collected a range of variables for our meta-regression dataset, which can be grouped into the following categories: country attributes, time horizon, data and estimation de-

[1] Further details on the computation of the PCC are presented in Appendix B.

tails, and publication characteristics.

Country attributes. Several contributions to the SEZ literature have highlighted that country characteristics can influence the success of SEZ policies. For example, Frick and Rodríguez-Pose (2023) underscore the role of infrastructure as a key precondition for successful SEZs, while Alkon (2018) highlights that institutional quality significantly influences the effectiveness of SEZs in promoting socioeconomic development. Therefore, accounting for country attributes can help explain the heterogeneity observed in the results of primary studies. In particular, we coded a variable identifying the country in which the SEZ policy was implemented, along with additional variables capturing whether the country is classified as advanced or developing economy (International Monetary Fund, 2023), its infrastructure endowment (Schwab, 2019), and its institutional quality (World Bank, 2024).[2]

Time horizon. The time horizon may also contribute to explaining the heterogeneity observed across primary studies, as the effects of SEZ policies can vary substantially depending on the duration of their implementation. For example, rigidity in the labor market can lead to slower adjustments, with significant employment gains emerging only in the long run (Nickell, 1997). For this reason, we collected a variable capturing the time span covered by each study, as well as a categorical variable indicating whether the study refers to short-run effects of the SEZ policy on employment, long-run effects, or does not specify the time frame (Heimberger, 2023).

Data and estimation details. Additional sources of heterogeneity across primary studies may stem from data and estimation characteristics. For instance, studies employing simple estimators such as pooled OLS tend to produce biased coefficient estimates compared to more advanced methods that account for staggered policy adoption and use weighted or synthetic control groups (Arkhangelsky et al., 2021; Callaway & Sant'Anna, 2021). Similarly, conventional standard errors tend to be less conservative than robust alternatives, such as clustered standard errors. To account for methodological rigor and attention to data structure across primary studies, we coded variables indicating the estimator applied and the type of standard errors used in the primary studies, along with the number of observations included in the estimated models and other relevant descriptive statistics related to the dependent variable. Moreover, we coded a dummy variable indicating whether the primary study focused on macrodata. Specifically, we classified as macrodata those studies that did not consider firms or smaller entities as the unit of observation (for example, studies using municipalities, provinces or regions).

Publication characteristics. The final group of collected variables refers to the publication characteristics of the primary studies. First, we distinguish between published and

[2] The World Bank provides six composite indicators that capture different dimensions of governance: Voice and Accountability, Political Stability and Absence of Violence/Terrorism, Government Effectiveness, Regulatory Quality, Rule of Law, and Control of Corruption. We averaged these into a single composite indicator, which we used in our analysis.

unpublished studies. Unlike published studies, unpublished ones may report less reliable estimates, as they have not been peer reviewed. Among published studies, publication quality may vary depending on the reputation of the journal. Articles published in top-tier journals typically undergo a more rigorous and lengthy peer review process, which should lead to more reliable estimates. By contrast, studies published in lower-ranked journals may be subject to less stringent peer review standards, potentially allowing for the inclusion of less reliable findings. Accordingly, we created a dummy variable indicating whether a study was published in a peer-reviewed journal, and we included the Clarivate Impact Factor for the most recent year available in our sample (2023) to capture the quality of the journal in which the study appeared.[3] Additionally, we collected the publication year of each paper, enabling us to examine whether older or more recent studies systematically report different effect sizes (Havranek & Irsova, 2011). We also recorded the average number of citations per year as a proxy for the scientific community's attention to the article over time (Heimberger, 2023).

Appendix C includes a table with the description and main descriptive statistics of the variables used in our meta-regression analysis.

3.2 Methods

Our meta-analysis relies on both graphical and quantitative methods to reveal the average effect size corrected for publication bias and systematic heterogeneity across primary studies.

Publication bias refers to the intentional or unintentional tendency to publish results based on their statistical significance or their consistency with prevailing theoretical expectations (Card & Krueger, 1995; Havranek & Kokes, 2015). Several factors contribute to publication bias. On the one hand, editors of academic journals are more likely to accept papers that report statistically significant findings or that align with prevailing theories. On the other hand, researchers may choose to report only those results that they believe will increase the likelihood of their papers being accepted, leading to the so-called file drawer problem (Rosenthal, 1979). Consequently, reported effect sizes tend to be systematically inflated relative to the underlying true effect size, typically at the expense of precision, as studies with larger standard errors are more likely to yield extreme and statistically significant estimates.

A first step in detecting publication bias is to examine the funnel plot, in which standardized coefficients (or effect sizes) are plotted against their precision, typically represented by the inverse of their standard errors. At the bottom of the funnel plot we observe less precise estimates, often associated with larger standardized coefficients,

[3] Following Heimberger (2023), we coded a value of 0.01 for studies that were not published in a peer-reviewed journal. Additionally, we assigned this value to published studies not covered by the Impact Factor, as they likely correspond to publications in low-quality or even predatory journals.

whereas at the top we find more precise estimates that typically correspond to smaller and more realistic standardized coefficients, which are closer to the true effect size. In the absence of publication bias, the funnel plot should appear symmetric, because studies with different precisions are equally likely to report effect sizes that deviate in either direction (positive or negative) from the true effect size. However, it is important to note that publication bias is not the only potential source of funnel plot asymmetry. Heterogeneity across primary studies–such as differences in methodologies, study contexts and other design characteristics–may also contribute to this asymmetry. Consequently, once systematic heterogeneity is taken into account, the evidence for publication bias may be substantially attenuated or even disappear. Accounting for these sources of variability requires conducting a multivariate meta-regression analysis, which will be presented at the end of this section.

A formal approach to detect publication bias is the Funnel Asymmetry Test–Precision Effect Test (FAT-PET), which can be implemented by estimating the following linear model:

$$SC_{ij} = \delta_0 + \delta_1 SE_{ij} + \epsilon_{ij}$$

where SC_{ij} is the standardized coefficient i from study j, SE_{ij} is the corresponding standard error and ϵ_{ij} is the error term. The term $\delta_1 SE_{ij}$ accounts for potential publication bias. Specifically, the null hypothesis $\delta_1 = 0$ constitutes the FAT. If this hypothesis is rejected, funnel plot asymmetry is present. However, this does not necessarily imply the presence of publication bias, as such asymmetry may also arise from systematic heterogeneity across primary studies. The null hypothesis $\delta_0 = 0$ corresponds to the PET and is used to assess whether the average effect size differs significantly from zero after accounting for potential publication bias. To check the robustness of the FAT-PET, we also applied the nonlinear methods proposed by Ioannidis et al. (2017), Andrews and Kasy (2019), and Furukawa (2019) (see Appendix D for further details on these methods).

Multivariate meta-regression analysis extends the linear model discussed above by also accounting for systematic heterogeneity. Specifically, the model to be estimated becomes:

$$SC_{ij} = \delta_0 + \delta_1 SE_{ij} + \delta_x X_{ij} + \epsilon_{ij}$$

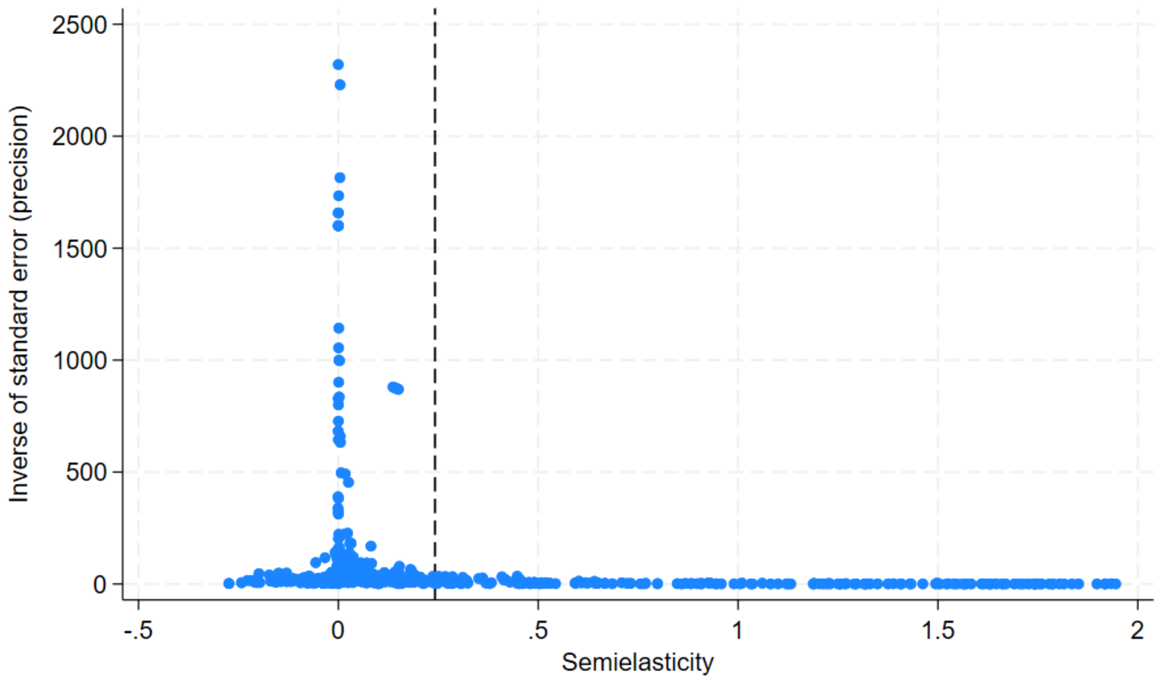
where X_{ij} is a vector of additional covariates that capture systematic heterogeneity across primary studies. Examples of such sources of heterogeneity are discussed in Section 3.1.1. In this model, the estimate of δ_0 represents the estimated average effect size adjusted for both publication bias and included sources of heterogeneity, and it is expected to approximate the true but unknown effect size.

4 Results and Discussion

4.1 Distribution of semielasticities and their standard errors

Figure 1 shows the semielasticities plotted against their precision. As expected, less precise estimates at the bottom display larger semielasticities, since studies with larger standard errors are more likely to produce extreme and statistically significant estimates. We can see that the funnel plot is clearly asymmetric, as semielasticities from less precise studies are predominantly positive. In particular, the average semielasticity is 0.242. This may be indicative of a publication bias that favors studies in which SEZ policies have a positive effect on employment, which needs to be formally investigated using quantitative methods.

Figure 1: Funnel plot of semielasticity



The dashed vertical line indicates the average of the standardized coefficients.

Similar findings are observed when constructing the funnel plot using the PCCs, although the asymmetry is less pronounced (see Appendix D).

4.2 Publication Bias

The results from the Funnel Asymmetry Test–Precision Effect Test are reported in Table 1. As we reported standardized effect sizes from primary studies using different data and specification choices, these are expected to have different variances. Thus, the FAT-PET results may be affected by heteroscedasticity. To address this concern, we conducted the funnel asymmetry and precision effect tests using Weighted Least Squares (WLS), weighting each standardized coefficient by the inverse of its variance (Heimberger, 2023). Consequently, WLS estimates assign more weight to those estimates that are more precise. Additionally, since our meta-regression dataset includes standardized effect sizes that may originate from the same primary studies, we addressed potential within-study dependence in the FAT-PET results by clustering the standard errors at the study level.

Table 1: FAT-PET results

	PET only	WLS	WLS with median values	WLS with alternative weights	IV	PCC
δ_1 (Publication bias)	–	1.271*** (0.367)	1.980*** (0.640)	0.866*** (0.113)	0.721*** (0.071)	1.758* (0.887)
δ_0 (Corrected effect)	0.242* (0.127)	0.012 (0.013)	0.002 (0.001)	0.052* (0.027)	0.054** (0.026)	0.004 (0.004)
Observations	779	779	33	779	779	779

***, ** and * denote coefficients significant at 1%, 5% and 10% respectively. Study-level clustered standard errors are reported in parentheses. All estimates except for the first column were obtained using WLS.

The first column reports the PET results only, with δ_0 corresponding to the unweighted estimate of the average effect size, unadjusted for potential publication bias. The estimated average effect size is significantly different from zero, implying that employment is expected to increase by 24.2% following the introduction of the SEZ policy. The FAT-PET estimates using WLS are shown in the second column. δ_1 is statistically significant and positive, suggesting potential selective reporting of estimates that show the positive effects of SEZ policies on employment. As expected, the weighted estimate of the average effect size corrected for potential publication bias is substantially lower than the unweighted raw estimate of the average effect size, not being significantly different from zero. The other columns report robustness checks in which we conducted the FAT-PET using alternative samples, weights, estimators, and standardized coefficients: the third column includes only the median values from primary studies; the fourth column uses the inverse of the number of estimates per study as weights to account for potential within-study dependence; the fifth coulumn applies the instrumental variable estimator;[4] and the sixth column includes the PCC as the standardized effect size. All robustness checks are consistent with the baseline FAT-PET results: the funnel asymmetry test indicates potential publication bias, while the precision effect test shows that the corrected estimate of the average effect size is substantially lower than the raw estimate.

In Appendix D we report the estimates of the average effect size obtained using non-

[4] Standard errors may be endogenous in meta-regression because they vary with study characteristics and methodological choices. Following Heimberger (2023), we instrument the standard error using the inverse of the square root of the number of degrees of freedom.

linear tests. These results are consistent with the FAT-PET findings, as the estimated average effect is not significantly different from zero across different methods.

4.3 Multivariate Meta-Regression Analysis

So far, the average effect size has been adjusted only for potential publication bias. To obtain a closer approximation of the true effect size, we also need to account for systematic heterogeneity across primary studies. In Table 2, we extend the FAT-PET analysis by incorporating moderators that may capture potential sources of this heterogeneity.

Table 2: Multivariate meta-regression results

	Country + Time	Data + Publication	Full model	PCC
Publication bias	0.978** (0.430)	-0.066 (0.457)	0.203 (0.461)	1.202 (0.931)
Advanced country	-0.060* (0.031)	–	-0.076*** (0.018)	-0.064 (0.060)
Short-run estimate	-0.032 (0.025)	–	0.003 (0.003)	0.006 (0.020)
Long-run estimate	0.021 (0.014)	–	0.009 (0.009)	0.083 (0.067)
Bootstrap std. error	–	0.025 (0.050)	0.055 (0.051)	0.048 (0.083)
Clustered std. error	–	0.004 (0.029)	0.026 (0.023)	-0.056 (0.053)
Robust std. error	–	(0.094) (0.100)	0.107 (0.096)	-0.005 (0.049)
Adjusted DiD estimator	–	-0.009 (0.013)	0.011 (0.013)	0.014 (0.019)
Other estimator	–	-0.048*** (0.015)	-0.059*** (0.016)	0.007 (0.023)
Macrodata	–	0.011 (0.012)	-0.016 (0.014)	-0.026 (0.019)
Published	–	0.070 (0.064)	0.058 (0.061)	0.059 (0.063)
Citations per year	–	-0.002*** (0.001)	-0.001* (0.001)	0.001 (0.001)
Publication year	–	0.005*** (0.001)	–	–
Constant	0.058* (0.030)	-0.003 (0.064)	0.010 (0.063)	0.005 (0.044)
Observations	779	779	779	779
Adjusted R^2	0.499	0.877	0.879	0.115

***, ** and * denote coefficients significant at 1%, 5% and 10% respectively. Study-level clustered standard errors are reported in parentheses. All estimates were obtained using WLS.

To avoid multicollinearity in our multivariate meta-regression analysis, we included only covariates with correlations lower than 0.65. Furthermore, since our analysis focuses on the corrected estimate of the average effect size, we centered the continuous moderators at their mean values so that the constant represents the estimated average effect size at the mean values of these covariates.

The first column shows estimates using only the country attribute and time horizon variables as moderators. The second column includes only the data and estimator details as well as publication characteristics as moderators. The third column presents the full model, combining the sets of moderators from the first two columns; publication year was removed from the full model because it was highly correlated with the advanced country indicator. Finally, the fourth column repeats the estimation of the full model, but uses the PCC as the standardized effect size. Except for this model, which does not yield significant estimates, all models have adjusted R^2 values between 0.499 and 0.879, indicating that the included covariates account for a substantial portion of the variance in the standardized effect size.

After introducing the set of moderators for data details and publication characteristics, evidence of publication bias is no longer statistically significant, suggesting that the variation previously attributed to publication bias may be explained by these moderators.

The statistically significant negative coefficient of the Advanced country indicator admits two possible interpretations. One plausible explanation is that baseline employment levels and labour-market structures differ systematically between advanced and developing economies, so that SEZ policies produce smaller employment responses in advanced settings. Another possible explanation is that SEZ studies are often conducted by institutions in the countries implementing the policies. Consequently, studies on advanced countries tend to use more reliable local data and stricter identification strategies, which typically lead to more conservative estimates.

The moderator capturing the use of other estimators (mainly instrumental variables and non-parametric approaches) is statistically significant with a negative sign, suggesting that robust estimation techniques usually yield smaller SEZ employment effects. Similarly, the coefficient of Citations per year is significant and negative, indicating that highly cited studies tend to report slightly smaller employment effects of SEZs. Taken together, this pattern suggests that widely cited studies are typically methodologically rigorous, using robust estimation strategies that produce more conservative effect sizes.

The positive and statistically significant coefficient of Publication year suggests that SEZ policies have become slightly more effective at generating employment over time. This outcome is consistent with the tendency of new SEZ programs to emulate those previously implemented in other countries that were successful in creating employment (Arbolino et al., 2024), fostering a virtuous cycle that progressively enhances SEZ policy design over time.

The constant represents the average effect size when all dummy variables are zero. Thus, the estimate of 0.010 in the full model indicates the average effect size of unpublished studies from developing countries with an unspecified time horizon, utilizing microdata and the OLS estimator with non-robust standard errors. However, our primary interest lies in studies adhering to "best practices". Specifically, we considered published studies that employ adjusted DiD estimators with clustered standard errors, regardless of the time horizon and data aggregation level (micro or macro). Given this, the average effect size for advanced economies is 0.024 (SE: 0.018), which is not statistically different from zero. In contrast, for developing economies, the average effect size is 0.100 (SE: 0.013), which is statistically significant at the 1% level. We re-estimated the average effect size for "best practices", but this time regardless of the development setting and data aggregation level. In both the short and long run, the estimated constant ranges between 0.06 and 0.07 and is statistically significant at the 1% level.

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Appendix

A Search Strategy

We conducted a systematic review to identify primary studies that measured the impact of Special Economic Zones (SEZs) on employment. For this purpose, we searched the Google Scholar and Scopus databases using the following query: ("Special Economic Zone" OR "Special Economic Zones" OR SEZ OR SEZs) AND employment. Additionally, we screened reference lists of potentially eligible primary studies to identify further relevant records. To be included in our meta-analysis, a study had to meet the following inclusion criteria:

Papers analyzing the impact of SEZs on employment. We included studies assessing the effectiveness of SEZ policies in generating employment.

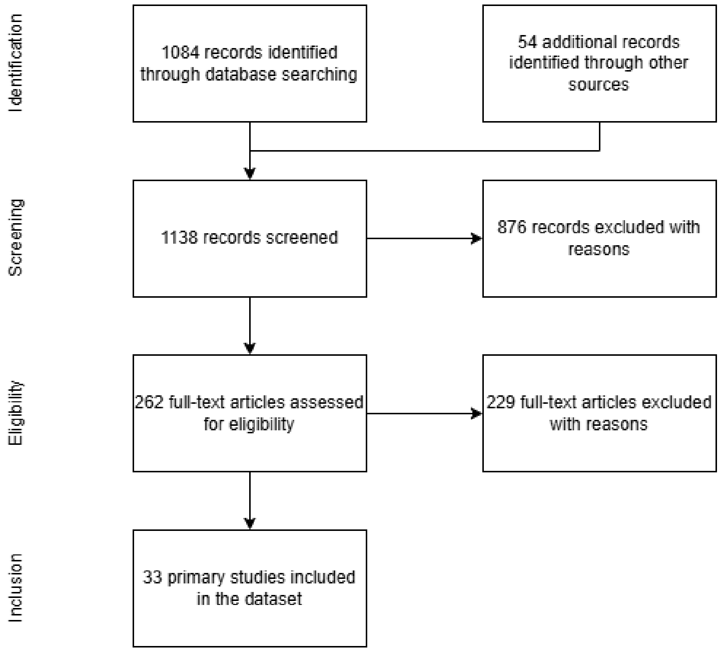
Papers reporting econometric estimates. We considered only quantitative studies providing econometric estimates where employment is the dependent variable and the SEZ policy indicator is the key predictor. Qualitative studies and quantitative papers using alternative methods (e.g., simulations) were excluded.

Language and time restrictions. Only studies written in English and published prior to January 2025 were included.

Papers reporting relevant statistics. A study had to report all relevant statistics necessary to compute standardized effect sizes either directly or indirectly. Specifically, the paper needed to provide at least the SEZ coefficients and their standard errors, which are required to measure the semielasticity in log-level models, as well as the mean of the dependent variable, necessary to approximate the semielasticity in level-level models. Additionally, the study had to report the number of observations in the estimated models, which can serve as a proxy for the degrees of freedom needed to compute the partial correlation coefficient.

Thirty-three primary studies met these inclusion criteria, yielding a total of 851 estimates included in our meta-regression dataset. Figure A.1 presents the PRISMA flowchart (Moher et al., 2009), which outlines the different steps of our search strategy that led to the inclusion of primary studies from which we collected data.

Figure A.1: The PRISMA flowchart



B Effect Size Standardization

We standardized the effect sizes to obtain a unitless measure of the impact of SEZ policy on employment, thereby allowing comparisons across primary studies. Our standardization coefficient is based on the semielasticity,[5] defined as the expected percentage change in employment resulting from the introduction of the SEZ policy. Semielasticities can be computed directly from a log-level model as follows:

$$semielasticity = e^{\hat{\beta}} - 1$$

where $\hat{\beta}$ denotes the estimated coefficient of the effect size.

Instead, the standard errors of the semielasticities can be derived using the delta method (Cazachevici et al., 2020):

$$Var(semielasticity) \approx (e^{\hat{\beta}} SE(\hat{\beta}))^2 \rightarrow SE(semielasticity) = \sqrt{Var(semielasticity)}$$

where $\hat{\beta}$ still denotes the estimated coefficient of the effect size in a log-level model and $SE(\hat{\beta})$ its standard error.

For level-level models, the semielasticity is equal to $\hat{\beta}/\bar{y}$, where $\hat{\beta}$ and $\bar{y}$ are the estimated coefficient of the effect size and the sample average of the employment measure, respectively. Additionally, the delta method can be used to derive the standard error of the semielasticity:

$$Var(semielasticity) \approx \left(\frac{SE(\hat{\beta})}{\bar{y}}\right)^2 \rightarrow SE(semielasticity) = \sqrt{Var(semielasticity)}$$

where $SE(\hat{\beta})$ is the standard error of the estimated effect size in a level-level model.

To check the robustness of our results, we used the partial correlation coefficient (PCC) as an alternative standardized measure of the impact of the SEZ policy on employment. The PCC is a unitless measure that ranges between –1 and 1 (Stanley & Doucouliagos, 2012). Unlike semielasticities, whose size reflects the expected percentage change in employment resulting from the introduction of the SEZ policy, the PCC lacks a straightforward economic interpretation, although its sign carries a similar meaning. In particular, PCCs can be readily computed as follows:

$$PCC = \frac{t}{\sqrt{t^2 + df}}$$

where t is the t-statistic associated with the estimate, defined as the ratio of the estimated effect size to its standard error, and df denotes the degrees of freedom of the estimate.[6] On the other hand, the standard error of the PCC is given by the following formula

[5] We used semielasticities because all primary studies represented the SEZ regressor as a binary indicator (1 if the policy was in place, 0 otherwise).

[6] Since primary studies rarely report the degrees of freedom of their estimates, we followed standard meta-analytic practice and approximated df as $n - 2$, where n is the sample size (Stanley et al., 2025).

$$SE(PCC) = \sqrt{\frac{(1 - PCC^2)}{df}}$$

Our dataset also includes estimates from models with interactions involving the SEZ indicator. In these cases, before computing the semielasticities and PCCs, we derived the average marginal effects of the SEZ policy on employment. Suppose we are analyzing a model that includes the SEZ indicator and its interaction with the regressor z:

$$emp = \beta_0 + \beta_1 SEZ + \beta_2 (SEZ * z) + \epsilon$$

The average marginal effect (AME) of SEZ on emp can be derived as follows:

$$AME = \hat{\beta}_1 + \hat{\beta}_2 \bar{z}$$

where $\hat{\beta}$ and $\bar{z}$ denote the estimated effect size and the sample mean of z, respectively.

Using the delta method, one can derive the standard error of the AME:

$$Var(AME) \approx SE(\hat{\beta}_1)^2 + (SE(\hat{\beta}_2)\bar{z})^2 \rightarrow SE(AME) = \sqrt{Var(AME)}$$

where $SE(\hat{\beta})$ is the standard error of the estimated effect size.

C Descriptive Statistics

Table C.1 includes the mean and standard deviation, as well as a description of the variables included in our meta-regression analysis.

Table C.1: Descriptive statistics

Variable	Description	Mean	S.D.	Observations
Semielasticity	Semielasticities obtained by standardizing the effect sizes reported in primary studies	0.242	0.467	779
SE(Semielasticity)	Standard error of the semielasticity	0.260	0.536	779
PCC	PCCs obtained by standardizing the effect sizes reported in primary studies	0.029	0.079	779
SE(PCC)	Standard error of the PCC	0.021	0.020	779
Bootstrap std. error	Dummy variable indicating if the estimate was computed using bootstrap std. error	0.060	0.238	779
Clustered std. error	Dummy variable indicating if the estimate was computed using clustered std. error	0.637	0.481	779
Robust std. error	Dummy variable indicating if the estimate was computed using robust std. error	0.051	0.221	779
Non-robust std. error	Dummy variable indicating if the estimate was computed using non-robust std. error	0.252	0.434	779
OLS estimator	Dummy variable indicating if the estimate was obtained using the OLS estimator	0.398	0.490	779
Adjusted DiD estimator	Dummy variable indicating if the estimate was obtained using adjusted DiD estimators (mainly based on propensity score methods)	0.546	0.498	779
Other estimator	Dummy variable indicating if the estimate was obtained using other estimators (mainly IV and non-parametric approaches)	0.056	0.231	779
Macrodata	Dummy variable indicating if the primary study focused on macrodata	0.503	0.500	779
Advanced country	Dummy variable indicating if the primary study focused on an advanced economy, as classified by the IMF	0.502	0.500	779
Infrastructure endowment	Infrastructure endowment score of the country, based on the Global Competitiveness Index 4.0 provided by the WEF	79.505	10.807	779

Table C.1 (continued)

Variable	Description	Mean	S.D.	Observations
Institutional quality	Institutional quality score of the country, based on the Worldwide Governance Indicators provided by the World Bank	61.974	19.015	779
Short-run estimate	Dummy variable indicating if the primary study refers to a short-run estimate	0.375	0.484	779
Long-run estimate	Dummy variable indicating if the primary study refers to a long-run estimate	0.334	0.472	779
Other time horizon	Dummy variable indicating if the primary study does not specify the time frame of the estimate	0.291	0.455	779
Published	Dummy variable indicating if the primary study was published in an academic journal	0.968	0.176	779
Publication year	Year in which the primary study was published	2016.748	7.051	779
Citations per year	Average number of citations per year received by the primary study	21.807	24.342	779
Impact factor	Clarivate Impact Factor (2023) of the academic journal that published the study	4.859	2.344	779

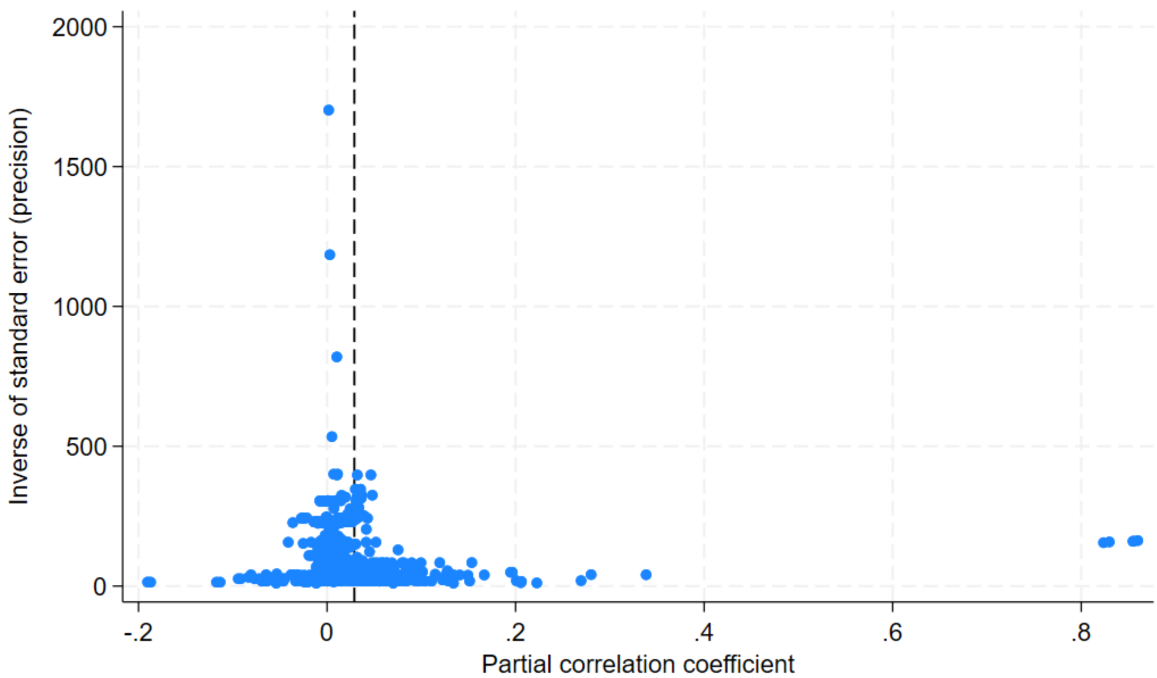
Note: The descriptive statistics were computed on the trimmed dataset, from which we excluded semielasticities and their standard errors at or below the 2nd percentile and at or above the 98th percentile to mitigate the influence of potential outliers.

D Robustness Checks

D.1 Distribution of PCCs and their standard errors

The funnel plot for the PCC is presented in Figure D.2. Less precise PCCs exhibit a tendency toward positive values, although this asymmetry is less pronounced compared to what was observed for the semielasticity. In particular, the average value of the PCC is 0.029.

Figure D.2: Funnel plot of PCC



The dashed vertical line indicates the average of the standardized coefficients.

D.2 Non-Linear Tests of Publication Bias

We also estimated the true effect size corrected for publication bias using the non-linear methods proposed by Ioannidis et al. (2017), Andrews and Kasy (2019), and Furukawa (2019). In particular, Ioannidis et al. (2017) introduced the weighted average of the adequately powered (WAAP), an approach to correcting bias in empirical economics research. In particular, their approach estimates the true effect size by considering only adequately powered estimates, defined as those with standard errors less than the absolute value of the fixed-effect weighted average divided by 2.8. Andrews and Kasy (2019) proposed a method to identify in meta-studies the conditional probability of publication as a function of study results, allowing for bias-corrected estimation of the true effect size. Furukawa (2019) presented a new stem-based bias correction method that is robust to publication selection processes. This method extends the "top10" estimator that uses the most precise 10% of studies (Stanley et al., 2010) by providing a formal method to select the optimal number of studies for estimating the true effect size.

Table D.2 reports the estimates of the average effect size obtained from the non-linear tests discussed above, along with the baseline result based only on the PET for comparison.

Table D.2: Non-linear tests of publication bias

	PET only	Ioannidis et al. (2017)	Andrews and Kasy (2019)	Furukawa (2019)
Corrected effect	0.242* (0.127)	0.021 (0.019)	-0.001 (0.001)	0.001 (0.013)

***, ** and * denote coefficients significant at 1%, 5% and 10% respectively. Standard errors are reported in parentheses. For the Andrews and Kasy (2019) approach, we used cutoffs for p(.) at 1.65 and a Student-t model for the distribution of effects.

The results of the non-linear tests are consistent with the FAT-PET results discussed in section 4.2, as the estimates of the average effect size are not significantly different from zero.