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Do enterprise zones promote local business development? Evidence from Vietnam

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Abstract

We examined the effects of Vietnamese enterprise zones, under the initiative of the central government, on local businesses across different types of ownership of zone infrastructure developers (ZIDs). We constructed a panel of communes during 2000–2007 using data on zone-based firms, local firms, and zones. We found that zones led to an increased number of firms and increased employment in the communes hosting the zones, even after excluding zone-based firms. Our findings also suggest that private partnerships in ZIDs worked best in attracting firms to the zones. The spatial spillover effects of the zones occurred up to a distance of 14 km.

KEYWORDS

enterprise zone, local business, private sector, Vietnam

1 | INTRODUCTION

Developing countries use enterprise zones for development, and private partnerships with zone infrastructure developers (ZIDs) have become common. Developing countries account for 76% of all enterprise zones worldwide (Akinci et al., 2008). Developing countries are using enterprise zones to promote economic activity (exports) and to attract foreign direct investment (FDI). The zones are not necessarily located in the distressed areas of developing countries. In contrast, in developed countries, enterprise zones are usually established in distressed areas[1] to increase employment and income (Neumark & Simpson, 2015).

Zones are characterized by public infrastructure investment, subsidies, and tax incentives. Typical examples in developed countries are the Tennessee Valley Authority in the United States, EU Structural

[1]See Bartik (2020) for place-based policies in distressed areas.

Funds, the US Federal Zone Program, California State Enterprise Zones, and French Enterprise Zones. Although developing countries also use similar forms, the private sector has developed and is operating 62% of all zones in developing countries (Akinci et al., 2008, pp. 10, 20). Therefore, developing enterprise zones to attract (foreign) firms to non-distressed areas and allowing the involvement of the private sector are new practices.

However, these new practices are not without critics. The involvement of the private sector in ZID may create conflicts of interests and compromise the original zone policies. If zones pursue export activities, then zone-based firms would tend to produce low value-added products made possible by low-skill assembly and import-dependent FDI. Footloose FDI might not result in spillover effects—technological or otherwise—on local businesses because the FDI firms might not use any inputs from local businesses. Firms might simply relocate business activities into the zone from elsewhere such that the stock of firms would remain the same. Firms might also open a tiny office in the zone to reap the policy benefits—so-called "mailbox effects" (Briant et al., 2015).

These new practices raise both existing questions that have been explored in developed countries and a new question pertaining specifically to developing countries. The existing questions are whether zone policies can promote local economic development and whether the policies are Pareto efficient (Fishback, 2017; Givord et al., 2013; Hanson & Rohlin, 2013; Kline & Moretti, 2014; Neumark & Kolko, 2010). The new question is whether the involvement of the private sector in zone development compromises the effectiveness of the policies. Economics researchers may be wary of spillover effects in the sense that the effects are evidence of bias in impact evaluations. However, spillover effects are what policymakers in developing countries want to measure. Positive spillover effects imply that vitalization of local businesses is an externality.

To answer these questions, we focus on enterprise zones in Vietnam because they have some unique features. First, the Vietnamese private sector is heavily involved in zone development. A private firm developed the first Vietnamese enterprise zone in the early 1990s. Second, the selection process for zone permits lacks transparency, especially before 2008. Specifically, there were no publicly available lists of candidate areas for establishing zone and no roadmaps for the selection process. Thus, local residents and businesses were not aware of the zone until the official issuance of the zone permit. Third, the concept of enterprise zones under the initiative of the central government differs from existing concepts in the United States, Europe, and even China. A Vietnamese enterprise zone is an area that is dedicated solely for firms (a camp with fences) and is isolated from local residents. Firms do not locate to the zone before issuance of the zone permit. Therefore, this difference in the concept of enterprise zones raises new questions. Does the zone have spatial spillover effects on local businesses located outside the zone boundaries, and, if so, how far do the effects go? These questions are more important than asking whether the zone policies are effective for zone-based firms within the limited area of the zone itself.

Focusing on enterprise zones under the initiative of the central government in Vietnam, we pose four main questions. First, have enterprise zones increased economic activity in the commune?[2] Using microdata from firms to answer this question, we count the number of firms[3] as well as the number of employees in the firms in the communes as proxies for economic activity. Second, do the zones have spillover effects outside the zone in the same commune? Third, how far do the spillover effects extend? Fourth, does the involvement of the private sector compromise zone development?

Using panel data aggregated at the commune level, we define communes with enterprise zones as treated communes. We define communes (districts) having no zone as of the end of 2007 as control communes (districts). Each treated commune is located inside a treated district (Figure 1). It should be noted that only those firms located within the zone (zone-based firms) are subject to zone policy, which does not apply to other firms located in the treated commune. We define all other firms as local businesses.

[2]In 2007, Vietnam consisted of 64 provinces in level 1, 679 districts in level 2, and 10,993 communes in level 3. On average, a commune is 30.1 km^2 in area with a population of 7770 people.

[3]We count firms with 10 or more employees because such firms are covered by Vietnamese Enterprise Surveys as census during 2000–2007.

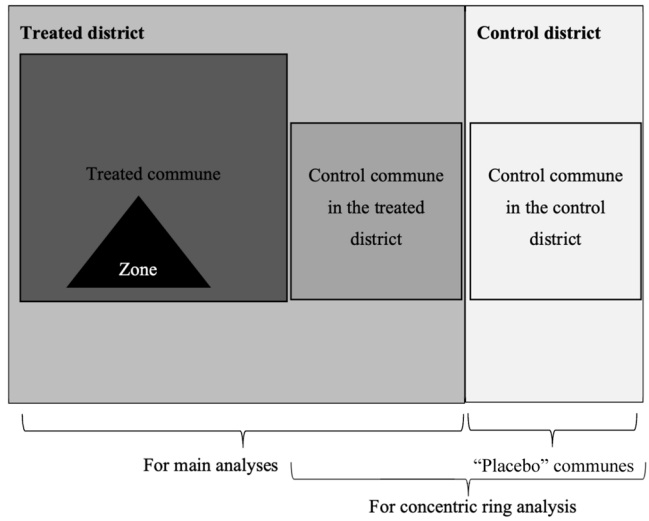

FIGURE 1 Conceptual framework for treated and control communes

The data cover the period from 2000 to 2007 and were obtained from Vietnamese Enterprise Surveys and the 2007 Establishment Census. The Establishment Census provides information on all enterprise zones and the communes in which they are located. The census also specifies zone-based firms. We used the information on zone-based firms to identify local businesses in each commune from the enterprise survey. The census also provides information to identify the ownership type of ZIDs.

We propose a novel sample selection strategy. For the main analyses, we select only treated districts in which its first zone was permitted in 2003 or later (Figure 1). Within a treated district, the treated communes and control communes are geographically close to each other. The sample selection is important because the first aim of the zone policy is to attract (foreign) firms. The geographical characteristics of a location are often unique because of the narrow "S" shape of Vietnamese territory. The location also determines firms' operating costs, which are often a primary concern. Geographical proximity is often associated with similar characteristics among communes. Our sample selection captures 43% of all zones in Vietnam as of 2007. This selection also enables tests for pretreatment parallel trends. For concentric ring analysis, we add all control districts to the initial selection (Figure 1) but exclude treated communes.

We use a modified panel-event study, which combines difference-in-differences (DID), a geographical discontinuity design, and an event study investigating commune fixed effects, district-year fixed effects, and fixed effects of the year–commune characteristics (the characteristics in 2000 multiplied with year dummies). We analyze the heterogeneity of zone types and ZID ownership. We also perform Bacon DID decomposition (Goodman-Bacon, 2021) and a procedure suggested by de Chaisemartin and D'Haultfœuille (2020) to examine differences in the timing of treatments. Then, we estimate the spatial spillover effects from a zone to local businesses in the commune as well as nearby communes (by using a concentric ring analysis).

We find that zone policies led to increased economic activity; that is, there were more firms in the communes that have enterprise zones. ZIDs configured as private partnerships were associated with higher growth in the number of businesses. The effects also spilled over to local businesses; that is, there were more local businesses in communes that have enterprise zones. However, the spatial spillover effects were limited to 14 km.

Our study contributes to the literature in three key ways. First, we show that zone policy promotes local business development in Vietnam. Second, we are not aware of any previous study, particularly among developing and transitional economies, investigating whether the type of ZID ownership affects the success of zone policies. Third, we improve upon

the concentric ring analyses of Zheng et al. (2017) and Lu et al. (2019), the latter of which estimated the spatial spillover effects from the zone center to non-zone areas outside. However, non-zone areas can be affected by more than one zone at the same time. Therefore, we consider non-zone areas to be the center influenced by all the surrounding zone areas. Moreover, unlike in Lu et al. (2019), we were able to identify zone-based firms.

The remainder of our paper is organized as follows. Section 2 describes enterprise zone policies in Vietnam during 2000–2007. Section 3 reviews the methodological challenges. The data used are then described in Section 4. Section 5 presents our identification strategies and methods. We report the results in Section 6 and conclude the paper in Section 7.

2 | VIETNAMESE ENTERPRISE ZONES BETWEEN 2000 and 2007

Vietnamese enterprise zones during 2000–2007 were of two types: one under the initiative of the central government, and the other under provincial governments (see Table 1). We denote zones under the initiative of the central government as industrial zones (IZs)[4] and under local governments as industrial clusters (ICs).[5] We focus on IZs but also consider ICs for possible impacts.

IZs are areas dedicated for firms only and are often completely inside a commune. IZs are separated from local residents, usually by fences, in accordance with Vietnamese IZ regulations. Before zone construction, the government relocates any pre-existing residences out of the dedicated area for the zone. The government creates Boards of Industrial Zone Management directly under the control of the central government, specifically, the Prime Minister. Each board examines and approves applications from businesses that wish to locate in the zone, become zone-based firms, and be subject to zone policies.

The government also requires each board to set up a ZID. The ZID designs, builds, and operates the zone. The ZID and the board can be two independent identities. A firm can become a ZID,[6] but it cannot be owned entirely by foreigners. The government provides the ZID with a special incentive that is even more favorable than the policies for other zone-based firms.

IZ-based firms are eligible for tax incentives from the central government. For example, the firms can enjoy two years of full corporate tax exemption and then pay a flat corporate tax rate as low as 10%, depending on the actual proportion of export sales over total sales. Manufacturing (service) firms in export processing zones, a type of IZ, can pay just a 10% (15%) flat rate of the corporate income tax from the fifth (second) year, if profitable. Circular 134/2007/TT-BTC of the Ministry of Finance imposes a regular corporate income tax rate of 28% for firms located elsewhere.

Therefore, Vietnamese IZs are different from American, European, and Chinese enterprise zones. Vietnamese IZs are brand new infrastructure investments, but not necessarily from the government budget. The standardized infrastructure and the incentive policies are to attract investors and thus promote economic development. Specifically, Vietnamese IZs tend to promote manufacturing with an emphasis on export activities.

Vietnamese regulations on enterprise zones often come after their establishment. The first zone became operational in 1991 (as in Figure 2). Six years later, the first regulation on zones came into force with Government Decree 36 (dated April 24, 1997), which officially defined and classified enterprise zones into industrial zones, export processing zones, and high-technology zones. The decree stated that only the central government can permit the establishment of IZs. The central government consigned the permit rights to local governments in 2008 (Decree 29/2008).[7] However, the local

[4]We use "IZ" because among zones under the central government, industrial zones were the largest in number.

[5]In Vietnamese, provincial governments call IC "cụm công nghiệp" (industrial cluster) and "làng nghề" (industrial village). The meaning in Vietnamese implies a smaller scale zone which avoids being regulated by Government Decree 36.

[6]The firm first enters into a contract with the Board for a land grant. The firm builds and maintains the zone facilities and services, and then leases them to other zone-based firms.

[7]All zone administrative bodies are accountable to provincial governments. The central government only approves national and regional master plans for zone development.

TABLE 1 IZs and ICs as of July 2007

Variables	IZ			IC		
	Mean	Min	Max	Mean	Min	Max
Year of permit	2001.40	1991	2007	2003.69	1996	2007
Year started/expected in operation	2002.68	1992	2013	2005.56	1997	2012
Area in the masterplan (ha)	336.29	3.82	10,000	69.43	1.1	2111.29
Area for lease (ha)	194.34	0	2816.26	33.40	0	2111.29
Number of zones by	**IZ**			**IC**		
Status						
In operation	144			189		
Under construction	35			76		
Types						
Industrial (manufacturing) zone	173			17		
Export processing zone	4			2		
High-technology zone	0			1		
Economic zone	2			3		
Industrial cluster/industrial village	0			242		
Ownership of ZID						
POE	78			33		
POE (with foreign partnership)	25			3		
SOE	62			23		
Governmental agency	18			196		
Total	179			265		

Note: ZIDs can be structured as a partnership. The exclusions are as follows: all IZs/ICs that were permitted but not yet under construction in July 2007, based on the Vietnamese Establishment Census; 18 units that appeared with the term "industrial park" in their names but did not have a permit from either the central or provincial governments; and one unit that ceased operations.

Abbreviations: IC, industrial cluster; IZ, industrial zone; POE, private-owned enterprise; SOE, state-owned enterprise; ZID, zone infrastructure developer.

governments had permitted similar zones before 2008, namely, ICs. The central government did not issue any regulations on ICs until Government Decree No. 105/2009 in 2009.

ICs and IZs are similar in terms of structure and operations. The ICs seek to attract firms with the aim of benefiting in terms of economic agglomeration and efficient operations such as lower costs for processing harmful industrial waste. ICs are smaller in scale (see Table 1). However, IC-based firms were not eligible for tax incentives during 2000–2007 but are subject to different policies, such as land rent incentives, from local governments. The incentives can be land rent holidays for 7 (11) years for ICs (industrial villages) and up to 11 (15) years for a ZID of industrial clusters (industrial villages) according to the first regulation, namely, Government Decree 68/2017, issued in 2017.

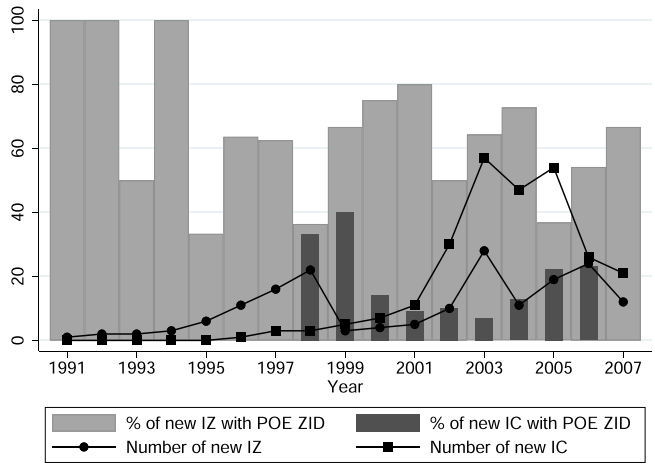

FIGURE 2 Newly permitted zones and involvement of the private sector in ZID. IC, industrial cluster; IZ, industrial zone; POE, private-owned enterprise; ZID, zone infrastructure developer

3 | EVALUATING THE IMPACTS OF ZONES: CHALLENGES AND DEVELOPMENTS

Although impact evaluations of enterprise zones are in high demand, in practice they are challenging to realize. Evaluations take place after the zones are established. Meanwhile, the selection of zones is not random. Policymakers often consider the existing characteristics of areas when deciding where to establish enterprise zones, and these characteristics are associated with zone outcomes. This issue might diminish the reliability of the impact evaluation's results but does not stop researchers from searching for better measures.

Identifying a valid control group (counterfactual) is always the first challenge. The choice of control group can significantly influence the findings and conclusions. Indeed, Neumark and Kolko (2010) suggested that the ideal control group should be similar to the zone but without the policy design. Previous studies have employed several measures to justify the selection of the control group. First, Neumark and Kolko (2010) suggested using counterfactual areas established from propensity score matching (PSM) methods based on the pretreatment characteristics of the areas. However, PSM overlooks unobservable characteristics (Neumark & Kolko, 2010). Second, in the case that policymakers recorded the procedure used for zone selection, Greenstone et al. (2010), Busso et al. (2013), Kline and Moretti (2014), and Zheng et al. (2017) suggested using "loser" and "winner" comparisons. The comparison is between the actual zone and the runner-up candidate. The comparison assumes the runner-up and the winner would have had similar characteristics. Busso et al. (2013) used a "placebo" area in the zone counties as the control group and then compared the "loser" and "winner." If the procedure has a criterion for selection, such as a threshold for assigning candidates to the treatment, Freedman (2013) suggested using a regression discontinuity design. However, such an assignment policy is rare in practice.

The second challenge is that differences in characteristics in the baseline between the treated and control areas are not trivial, even after controlling for the area's fixed effects. Area characteristics such as productivity, transportation development, and climate might have influenced the location choice of firms. Furthermore, the characteristics may also be chosen by the government to implement other economic policies in addition to establishing enterprise zones to alleviate poverty (Briant et al., 2015; Neumark & Kolko, 2010). For example, special economic zones, federal empowerment zones, and federal enterprise community programs co-exist in the United States (Ham et al., 2011).

To overcome the second challenge, various studies (Dell, 2010; Duranton et al., 2011; Gibbons et al., 2013; Givord et al., 2013; Hanson & Rohlin, 2013; Keele & Titiunik, 2015; Lee & Lemieux, 2010; Lu et al., 2019; Neumark & Kolko, 2010; Zheng et al., 2017) use a geographical boundary discontinuity design in combination with DID analysis. The method compares an inner zone with a tiny outer zone close to the zone boundary that acts as the control group. The outer zone has characteristics similar to the inner zone but is not subject to zone policies.

However, zone boundaries do not follow a standard postal code/geographic tract system (Neumark & Kolko, 2010), thereby leading to some complications. Furthermore, if zone policies have spatial spillover effects, the estimated results might be biased (Miguel & Kremer, 2004) because the tiny area near the zone is very likely to be within the impact scope of the spillover effects. Firms might also relocate from the outer zone to the inner zone (Chaurey, 2017; Hanson & Rohlin, 2013). Competition between zone-based firms and firms near zones might produce negative effects (Hanson & Rohlin, 2013).

Therefore, additional checks should be put in place. Specifically, Gibbons et al. (2013) suggest relaxing the assumption of spillover effects by accepting boundary effects and spatial trends. Zheng et al. (2017) and Lu et al. (2019) use concentric ring analysis, which assumes spatial spillover effects and uses a constant step distance (every 2 km) from the zone to explore the potential spillover area. The size of the spillover effects is inversely related to distance. The analysis can determine the step where the spillover effects disappear. The analysis may also discover steps with negative effects (i.e., displacement effects).

The third challenge is differences in timing. First, Wang (2013) indicates that the timing of zone policies is not random. Thus, the impact might vary by time and depend on the designated area's characteristics and conditions at the starting point. Wang (2013) suggests using area and area–year fixed effects to address this issue. A time-varying effect can be also observed in panel-event studies. Second, differences in when the zones were established might cause biased two-way fixed effects estimators (de Chaisemartin & D'Haultfœuille, 2020). This problem arises because some later-treated observations were already present in the control group before treatment. This problem can be serious if the treated observations outweigh the control observations, or if all observations are treated by the end of the time window. de Chaisemartin & D'Haultfœuille (2020) proposed a ratio test and a new estimator as a solution to this issue. However, if there are abundant control observations by the end of the time window, a simple decomposition of the two-way DID helps to estimate the bias. Goodman-Bacon (2021) proposed a solution in which the two-way DID estimator is decomposed into three components in four different timing-comparison groups.

4 | DATA

We combined two data sets from the General Statistics Office of Vietnam (GSO) having the same commune identity, namely, the Vietnam Enterprise Survey (VES) 2000–2007 and the 2007 Establishment Census, to construct the commune panel data. Within the VES, we aggregated two outcomes: number of firms and employment per commune. The Establishment Census helped to identify treated (control) communes, the timing of treatment, and ownership type of the ZIDs.

The VES is conducted annually using census and random sampling methods, based on firm size in terms of number of employees. Firms with less than 10 employees are randomly sampled, and firms with 10 or more employees are included for the period 2000–2007.

Detailed information on zones is recorded in the 2007 Establishment Census, including the history of establishment, who permitted the zone, the ZID, a list of zone-based firms, and the commune in which the zone is located. The commune location and the year of permit for each zone were used to identify treated (control) communes and districts as well as the timing of policy treatment. The information on who permitted the zone helped to classify zones as IZ or IC. A total of 566 zones were included in the 2007 Establishment Census; however, we used only those zones initiated by the central government (179 IZs) or by provincial governments (265 ICs).

Information on ownership was available for each ZID. Ownership was mainly by "governmental agencies" (which means the zone management boards directly acted as ZIDs), state-owned enterprises (SOEs), and private-owned enterprises (POEs)[8]. The ZID could also be a combination (i.e., partnership) of two or more of these entities.

We selected the period 2000–2007 for several reasons. First, 2000 marks the first year of the VES. Second, until the end of 2007, the Boards of Industrial Zone Management were directly under the control of the central government, led by the Prime Minister. Third, the central government had nearly the same cabinet from 1997 until June 2006, which created a certain level of stability and consistency in matters of policy at the central- and local-government levels. Fourth, the threshold for the VES random sampling method increased for large cities and varied by province each year starting from 2008.

Using data from the VES, we counted the number of firms with 10 or more employees (under the census) for each commune as well as the corresponding number of people employed each year. We combined the tax code of the firm and the province code to form a unique identifier. We also used the identifier to trace the zone-based firms recorded in the 2007 Establishment Census each year from 2006 back to 2000 in the VES. We were able to identify 3300 such firms (in 2007) and whether those firms appeared in the VES in other years. We subtracted the firms from the VES firms in each year. The remaining firms were the stock of local businesses in the commune. Although we were unable to examine whether firms relocated to zones and became zone-based firms because such information was not available, our analyses of spillover effects outside the zone in the same commune complement the limitations due to the lack of data.

5 | SAMPLE SELECTION, MAIN OUTCOMES, AND METHODS

5.1 | Selecting the sample and candidates for counterfactuals

It is possible that the locations of Vietnamese zones might have an endogenous issue, but the process for deciding zone locations is rarely discussed or revealed to the public. The selection of communes (location) for zones might be associated with the characteristics of those communes. That is, the location may be associated with firms' operational costs, proximity to transportation hubs, and access to markets. These factors are also the key points listed on the Vietnam Industrial Zone website, which serves as a forum to connect firms and zones (www.iz.com.vn). In addition, the locations of zones might also have unique characteristics resulting from Vietnam's "S" shaped geography. Therefore, a matching method using observable characteristics of areas to find counterfactuals might fail if the algorithm assigns too little weight to location.

To mitigate this issue, we selected for our main estimations only those districts in which the first zone was permitted in 2003 or later. We call these districts "treated districts." In treated districts, there are two types of communes: control-group communes having no zone and treated-group communes having a zone in 2003 or later (see Figure 1). Because control and treated groups are located in the same districts, the distances to the main seaports and international airports were similar (see Supporting Information Appendix 2). The selection enabled us to test the DID parallel-trend assumption because all the communes in the selected sample did not have a zone before 2003. Also, the issuance of an IZ permit within a commune that has never had a zone before can come as a surprise to local residents.

However, we acknowledge the possibility of selection issues as a trade-off for using more reliable statistical tests and an identification strategy. As a result, 145 zones established before 2003 were omitted. Excluding districts that had a zone before 2003 further reduced the sample size. This is because some of the zones established in 2003 and after were in districts that had pre-existing zones.

[8]We regarded foreign-affiliated firms as POEs.

Following this selection procedure, we obtained a panel of 1971 communes across 124 districts for the main analysis. The communes had 184 zones in the treated group (62 IZs and 122 ICs). The selection captured about 42% of all Vietnamese zones in 2007. The data are shown in Supporting Information Appendix 1.

5.2 | Main outcomes

From the VES, we calculated two important outcomes for each commune in each year: the logarithm of the total number of workers (ln(*L*)) and the logarithm of the total number of firms (ln(*Firms*)). We used an inverse hyperbolic sine transformation similar to a logarithm transformation, which allows zero-valued observations to be retained. This was necessary because some communes had no firms above the threshold of 10 employees. Hence, we used the equation $\ln(value_{in\ use}) = \ln(value_{raw} + (value_{raw}^2 + 1)^{(1/2)})$. The descriptive statistics of the outcomes are presented in Supporting Information Appendix 1.

5.3 | Methods

5.3.1 | Overall impacts

We used a panel-event study design. A basic DID estimates average treatment β_1, that is, the impact of the zone policy on the outcome (Y_{it}) in the panel of commune *i*:

$$Y_{it} = \beta_1 \times zone \times treated + \beta_2 \times zone + \beta_3 \times treated + \zeta_{it}, \quad (1)$$

zone is a binary variable for the treated commune, that is, the commune hosting the zone. For the timing of the policy treatment, the dummy *treated* changes from 0 to 1 if *treated* is during or after the year of the zone permit. Y_{it} was from both zone-based firms and local businesses.

We used a dummy $zone \times treated_{i,j}$ in the place of *zone* × *treat* to set up the panel-event study. Specifically, *j* indicated the time difference from the year the zone commenced operations[9] (*n*). Thus, $j = t - n$. For example, the dummy $treated_{i,-1}$ equals 1 if the treated commune was in a year before the treatment (i.e., a year before the zone started to host zone-based firms). We used $zone \times treated_{i,0}$ as the baseline; this dummy indicated the treated commune on the starting year of operations.

$$Y_{it} = \theta_{1,j} \times zone \times treated_{i,j} + \theta_2 \times i + \theta_3 \times t + \zeta_{it}. \quad (2)$$

We added the DID average policy treatment estimators similar to *zone* × *treated* for IZ and IC. We used four different commune baseline characteristics in 2000: the logarithm of total capital and sales per worker in firms located in the commune, distance to the nearest seaport, and distance to the nearest international airport. We describe four characteristics of the communes in 2000 ($baseline_{i,n,2000}$) in Supporting Information Appendix 1 and compare treated and control communes in Supporting Information Appendix 2. We interacted these characteristics with year dummies (notated as $baseline_{i,2000} \times t$), as suggested by Wang (2013). The set of baseline-year-commune-time effects ($baseline_{i,n,2000} \times t$) replaced the time fixed effect (*t*). We also controlled for *k* district time trends λ_{kt} (λ_{kt} = *district dummy* × *t*):

[9]Zones generally began operating about a year after the permits were granted (see Table 1). The year in operation precisely dictated the timing for possible association between zone-based firms and local businesses.

$$Y_{it} = \theta_{1,j} \times zone \times treated_{i,j} + \theta_{2i} \times IZ \times treated + \theta_{3i} \times IC \times treated + \theta_{4n} \times baseline_{i,n,2000} \times t + \theta_{5k} \times \lambda_{kt} + \theta_6 \times i + \zeta_{it}. \quad (3)$$

We tested for the condition of DID parallel trend (H0: coefficients $\theta_{1,-7}$, $\theta_{1,-6}$, $\theta_{1,-5}$, ..., $\theta_{1,-1}$ were equal). If the test statistics do not reject the null hypothesis, then the condition is met. It is not necessary for all coefficients to equal zero; pretreatment years equal the first year of the treatment. In the first year of zone operations, some zone-based firms had already commenced business inside the zone.

Furthermore, we aimed to estimate the impact of ZID ownership by using a set of ownership dummies ($ownership_{il}$):

$$Y_{it} = \gamma_{1,j} \times zone \times treated_{i,j} + \gamma_{2l} \times IZ \times treated \times ownership_{il} + \gamma_{3l} \times IC \times treated \times ownership_{il} + \gamma_{4n} \times baseline_{i,n,2000} \times t + \gamma_{5k} \times \lambda_{kt} + \gamma_6 \times i + \sigma_{it}, \quad (4)$$

where the dummy $ownership_{il}$ indicates the ZID ownership type. ZID ownership is one or a combination of *POE*, *SOE*, and direct designated governmental agency (*governmental agency*). $Ownership_{il}$ identifies differences in infrastructure operation[10] among IZs because the IZ policies issued by the central government were the same during 2000–2007. We also tested whether all ownership coefficients equaled zero. If the test hypothesis can be rejected, then the difference in ZID ownership should be associated with the outcomes.

5.3.2 | Spillover effects from the zone to the commune hosting the zone

We examined whether the zones might impact local businesses located just outside the zone boundaries. The spillover effects demonstrate whether zone-based firms attracted and promoted the stock of local businesses (i.e., the local economy) by using local services and inputs. Thus, we re-calculated the outcomes of local businesses ($\ln(NY_{it})$). NY_{it} is a net value, namely, the result of subtracting the corresponding outcomes of zone-based firms from Y_{it}.

We modified Equations (3) and (4) by replacing Y_{it} with NY_{it}:

$$NY_{it} = \gamma_{1,j} \times zone \times treated_{i,j} + \gamma_{2l} \times IZ \times treated + \gamma_{3l} \times IC \times treated + \gamma_{4n} \times baseline_{i,n,2000} \times t + \gamma_{5k} \times \lambda_{kt} + \gamma_6 \times i + \omega_{it}, \quad (5)$$

and

$$NY_{it} = \gamma_{1,j} \times zone \times treated_{i,j} + \gamma_{2l} \times IZ \times treated \times ownership_{il} + \gamma_{3l} \times IC \times treated \times ownership_{il} + \gamma_{4n} \times baseline_{i,n,2000} \times t + \gamma_{5k} \times \lambda_{kt} + \gamma_6 \times i + \omega_{it}, \quad (6)$$

5.3.3 | Spatial spillover effects to other communes

We examined spatial spillover effects to an extended sample of control communes. Our estimations allowed us to explore how far the effects could extend; if the relocation of firms near the zones led to a vacuum

[10]We used ownership variables to predict the size of IZs; however, all ownership-type coefficients were statistically insignificant. Therefore, it is less likely that ownership type is associated with zone size.

(negative) in nearby areas; and whether our previous estimations were biased. We removed treated communes and added all districts lacking a zone in 2007 (i.e., the control districts) to the existing sample (illustrated in Figure 1). The selected sample included 522 control communes (described in Supporting Information Appendix 7).

Effects were estimated by concentric ring analysis. Figure 3 illustrates the conceptual framework of the analysis. We developed a set of concentric rings from the center of each control commune. The radius of the first ring was 2 km. We increased the radius of the consecutive rings with a constant step of 2 km. We repeated this s times ($s = [1, 50]$) until we reached the largest ring with a 100-km radius. We counted any IZs/ICs located in a commune whose central location fell within each ring interval and combined it with the earliest treatment time of the IZ/IC (from the permit year of the first IZ/IC located in the same ring interval) to construct a set of 49 ring dummies.

We performed the estimation by using the following equation:

$$Y_{it} = \mu_{1s} \times IZ_{2\times s,i} \times IZ.\ treated_{2\times s,i,t} + \mu_{2s} \times IC_{2\times s,i} \times IC.\ treated_{2\times s,i,t} + \mu_{3s} \times IZ.\ IC_{2\times s,i} \times IZ.\ IC.\ treated_{2\times s,i,t} + \gamma_{4n} \times baseline_{i,n,2000} \times t + \gamma_{5k} \times \lambda_{kt} + \gamma_6 \times i + \omega_{it}, \quad (7)$$

$IZ_{2\times s,i}$ equals 1 if there was at least one IZ, but no IC, by 2007, the center of which is between $2 \times (s - 1)$ and $2 \times s$ km from the center of the control commune i. $IZ.\ treated_{2\times s,i,t}$ takes 1 if $IZ_{2\times s,i} = 1$ and if year t is during or after the permit year of the corresponding oldest IZ. μ_{1s} (μ_{2s}) showed spillover effects on the control commune during time t when IZ (IC) was the first and only zone located in ring s. If the ring was host to a mixture of IZs and ICs, μ_{3s} was the corresponding coefficient.

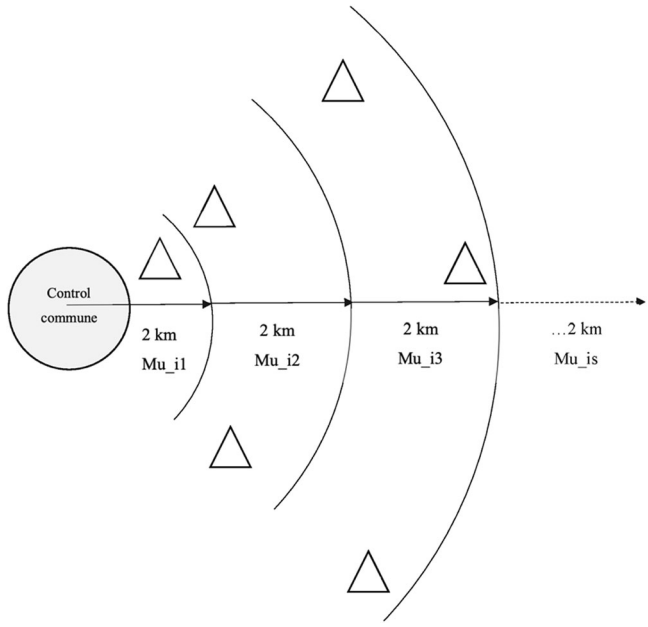

FIGURE 3 Conceptual framework of the concentric ring analysis. Mu_is is μ_{is} in Equation (7) where $i = [1, 3]$. Δ indicates the center of the treated commune having zones

5.4 | Heterogeneity and robustness check

First, given that IZs differ from ICs, we altered the sample selection approach in several ways to deal with the heterogeneity of the two zone types for a robustness check. Specifically, we re-estimated Equations (4)–(7) for each of the following sub-samples: (Panel A) districts having IZ, (Panel B) districts having IC, (Panel C) districts having only IZ, and (Panel D) districts having only IC. We also used Panel A in a purely event-study specification with NY_{it}.

Second, we are aware of biasness due to timing differences in two-way DID which are thoroughly discussed in de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021), Callaway and Sant'Anna (2021), and Sun and Abraham (2021). We examined the potential biasness by using two different applied methods from the recent literature. First, we follow Goodman-Bacon (2021) to consider differences in when the permits were issued.[11] This Bacon DID decomposed the total possible impact into three parts: timing groups (the commune having a zone later served as the control group for the earlier treated commune, and the commune having a zone earlier served as the control group for the later treated commune), within components (using previously treated communes as the base), and never treated (using never-treated communes as the base). The third component, never treated, is the impact of interest because it shows the difference between treated communes and the communes that have never had a zone during the study period. The other two components are not considered because of timing differences. Second, we applied the user-written Stata ado command "twowayfeweights" suggested by de Chaisemartin and D'Haultfœuille (2020) to calculate the negative weights that lead to biasness.

6 | RESULTS

6.1 | Overall impacts

We found that zone policies were associated with more businesses located within the administrative boundaries of the communes, including the zone itself. The number of businesses emerging within a year after the zone was formed is shown in coefficient *zone* × $treated_1$ in Columns (1)–(4) of Table 2.

The parallel trend tests supported our interpretation in which the outcome was the number of firms (see Supporting Information Appendix 5). The results showed that the number of firms cannot be rejected as being constant for 7 years before the year the zone permit was issued. However, similar tests rejected the null hypotheses when the outcomes were employment. Thus, our estimations on employment impacts may be biased. The treated commune should increase employment a year or two before the zone started to operate, as additional test results showed (e.g., H0: All *zone* × $treated_{-j}$ are equal; [j = 2; 7]). This increase in employment may be due to ZID activities such as zone construction in the period between the issuance of the zone permit and the starting year of zone operations.

Second, policies of the central government (*IZ* × *treated*) tended to have a larger impact compared with policies of the provincial governments (*IC* × *treated*) (see Table 2 and the hypothesis-testing results in Supporting Information Appendix 5). Some possible reasons might be generous tax incentives and economies of scale. Unfortunately, we did not have enough information to analyze this in depth.

Third, ZID ownership types were associated with business development as shown in columns 3 and 4 in Table 2. However, the associations existed only in IZ communes. ZID coefficients, except IZ × treated × Governmental agency, were positive and statistically significant. Among the coefficients, ZIDs with the involvement of the private sector (*IZ* × *treated* × *POE*) tend to have consistently positive effects.

[11]We applied the corresponding user-written Stata ado command "bacondecomp."

TABLE 2 Aggregate effects of zones on communes' business

Variables	(1) ln(L)	(2) ln(Firms)	(3) ln(L)	(4) ln(Firms)
zone × *treated*$_1$	0.3552**	0.1721***	0.4069***	0.1778***
	(0.1503)	(0.0457)	(0.1515)	(0.0465)
zone × *treated*$_2$	0.6579***	0.2589***	0.7058***	0.2636***
	(0.1992)	(0.0708)	(0.1985)	(0.0699)
zone × *treated*$_3$	0.7224***	0.3365***	0.7609***	0.3354***
	(0.2499)	(0.0969)	(0.2405)	(0.0944)
zone × *treated*$_4$	1.0082**	0.4277***	1.0152**	0.4165***
	(0.4442)	(0.1241)	(0.4454)	(0.1243)
IZ × treated	0.8195***	0.3078***		
	(0.2699)	(0.0792)		
IC × treated	0.2883*	0.0769		
	(0.1567)	(0.0475)		
IZ × treated × POE			1.0135***	0.3888***
			(0.3888)	(0.1107)
IZ × treated × SOE			0.4235	0.2409**
			(0.3640)	(0.1149)
IZ × treated × Governmental agency			−0.1595	−0.0093
			(0.4070)	(0.1160)
IC × treated × POE			−0.0845	−0.0547
			(0.4931)	(0.1402)
IC treated × SOE			0.2747	0.0992*
			(0.1711)	(0.0568)
IC × treated × Governmental agency			0.4235	0.2409**
			(0.3640)	(0.1149)
zone × *treated*$_{-j}$ (j = [1; 7])	Yes	Yes	Yes	Yes
District × year fixed effect	Yes	Yes	Yes	Yes
Baseline × year fixed effect	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes
Observations	15,768	15,768	15,768	15,768
R^2	0.358	0.468	0.358	0.468
Number of communes	1971	1971	1971	1971

Note: Standard errors clustered at the commune level are in parentheses.

Abbreviations: IC, industrial cluster; IZ, industrial zone; POE, private-owned enterprise; SOE, state-owned enterprise; ZID, zone infrastructure developer.

***$p < 0.01$.

**$p < 0.05$.

*$p < 0.1$.

In addition, we examined several concerns about ZID ownership type. First, we tested whether ZID selection was correlated with the characteristics (in 2000) of the communes hosting the 62 IZs. The selected sample showed that every ZID had only one ownership type. We regressed each of the four characteristics of the IZ communes in 2000 on the ZID ownership set (see Supporting Information Appendix 3). In every regression, we found that the test hypothesis H0 (POE = SOE = 0) could not be rejected. Second, we examined whether the type of ZID ownership in the 62 IZs in the selected sample was associated with the ZID ownership type of the nearest pre-existing IZ (established before 2003). We did not find any statistically significant relationship here either (see Supporting Information Appendix 4). In addition, the IZs in the selected sample tend to be located far away from the pre-existing IZs, with a mean nearest distance of 34 km. Taken together, all of these results suggested that the decision to grant ZID status to firms might have been made without consideration of ownership type.

6.2 | Spillover effects

We found evidence of positive spillover effects of the zone-based firms on the number of local businesses outside the zone boundary but within the treated communes, as per coefficients of $zone \times treated_1$ - $zone \times treated_4$ in Table 3. Hypothesis tests showed that the assumptions on the pretreatment parallel trend hold (see columns 2 and 4 of Supporting Information Appendix 6). The interpretations are similar to those in the previous subsection.

However, different ZID ownership types have no significant influence on local businesses located outside the zone boundaries. The test hypothesis H0 (*All ZID ownership* = 0) cannot be rejected (see column 3 and 4 of Supporting Information Appendix 6). The test results suggested that who owned and/or operated the ZID is not important for attracting firms to relocate into the zones.

Second, the spatial spillover effects of IZs on other communes were within 14 km when the outcome was the number of local businesses (employment) as in Figures 4 and 5, which display the estimated μ_{1s} (μ_{2s}) against the distance from the control communes. This 14-km distance implies that the results are an underestimate. We did not find any significant negative μ_{1s}, which is an IZ-related coefficient, associated with the number of local businesses in the additional 15–30 km shown in Figure 5. This result suggested that the relocation of local businesses close to zones, if any, were less likely to cause a significant negative impact on the stock of local businesses; in other words, there were no displacement effects.

6.3 | Heterogeneity and robustness checks

The effects were more robust among IZ districts with the appearance of ICs. We divided the sample into districts containing IZs and ICs, and further restricted them to districts with only IZs (ICs).[12] First, the overall and spillover effects were more pronounced in Panel A, that is, districts having IZs and ICs. The parallel trend conditions hold for all estimations in Panel A (see Supporting Information Appendix 8). It is possible that the decision to set up an IZ might come as a surprise to local residents. By law, the central government is the only legal entity that can grant permission to establish an IZ. In most cases, the central government is geographically distant from the commune. The impacts of zones were also statistically significant in Panels B and C. Second, ZID ownership type could not be used to explain the spillover effects. Ownership types were statistically significant only in Panel A and only for overall impacts. Ownership types were not significantly correlated with the overall effects in Panel C, that is, IZ districts lacking ICs.

[12]Detailed estimations (evidence of specific positive impacts) are available upon request.

TABLE 3 Spillover effects on local businesses within the administrative boundaries of the commune

Variables	(1) ln(Net L)	(2) ln(Net Firms)	(3) ln(Net L)	(4) ln(Net Firms)
zone × $treated_1$	0.2836*	0.0949*	0.3252**	0.1001*
	(0.1573)	(0.0517)	(0.1585)	(0.0529)
zone × $treated_2$	0.5322***	0.2004***	0.5695***	0.2051***
	(0.1927)	(0.0691)	(0.1932)	(0.0689)
zone × $treated_3$	0.5736*	0.3492***	0.6101**	0.3515***
	(0.3016)	(0.1264)	(0.2986)	(0.1247)
zone × $treated_4$	1.3097***	0.4670**	1.3306***	0.4617**
	(0.4957)	(0.1863)	(0.4955)	(0.1866)
IZ × treated	0.4181	0.1778**		
	(0.2694)	(0.0905)		
IC × treated	0.2373	0.0665		
	(0.1514)	(0.0469)		
IZ × treated × POE			0.5208	0.2233*
			(0.3857)	(0.1338)
IZ × treated × SOE			0.0719	0.1305
			(0.3727)	(0.1107)
IZ × treated × Governmental agency			−0.0103	−0.0191
			(0.4161)	(0.1011)
IC × treated × POE			0.0326	−0.0180
			(0.4884)	(0.1221)
IC × treated × SOE			0.1883	0.0831
			(0.1668)	(0.0570)
IC × treated × Governmental agency			0.0719	0.1305
			(0.3727)	(0.1107)
zone × $treated_{-j}$ (j = [1; 7])	Yes	Yes	Yes	Yes
District × year fixed effect	Yes	Yes	Yes	Yes
Baseline × year fixed effect	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes
Observations	15,768	15,768	15,768	15,768
R^2	0.352	0.452	0.352	0.453
Number of communes	1971	1971	1971	1971

Note: Standard errors clustered at the commune level are in parentheses.

Abbreviations: IC, industrial cluster; IZ, industrial zone; POE, private-owned enterprise; SOE, state-owned enterprise; ZID, zone infrastructure developer.

***$p < 0.01$.

**$p < 0.05$.

*$p < 0.1$.

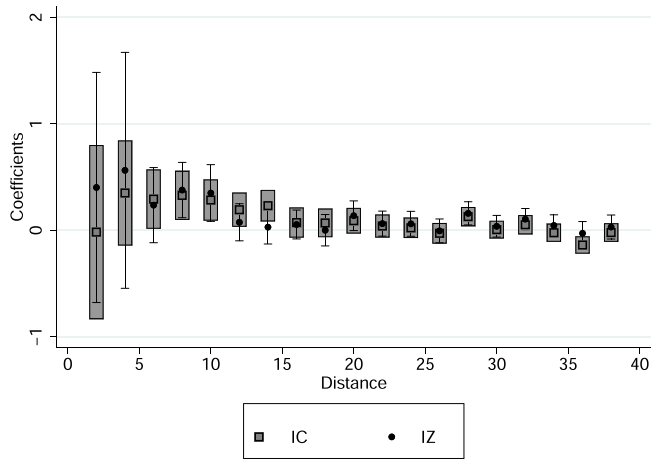

FIGURE 4 Spatial spillover effects of IZs/ICs on employment. IZ(IC) and corresponding two-standard deviation bounds are coefficients μ_{1s} (μ_{2s}). Effects are displayed between 0 and 40 km from the estimations accounting for 0–100 km. IC, industrial cluster; IZ, industrial zone

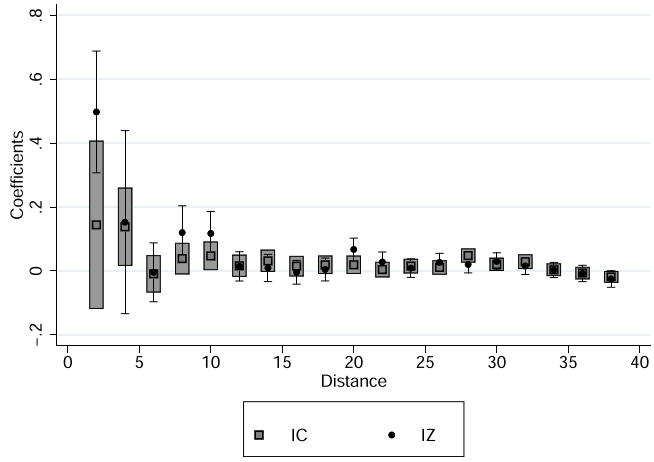

FIGURE 5 Spatial spillover effects of IZs/ICs on number of firms. Same as Figure 4. IC, industrial cluster; IZ, industrial zone

The IZs in Panel A, which are our main focus, had robust impacts in the purely panel-event study specification. We removed the DID average policy treatment estimators in Equation (3) and estimated the following using Panel A:

$$NY_{it} = \theta_{1,j} \times zone \times treated_{i,j} + \theta_{4n} \times baseline_{i,n,2000} \times t + \theta_{5k} \times \lambda_{kt} + \theta_6 \times i + \zeta_{it}, \quad (8)$$

We draw the estimated $\theta_{1,j}$ against the timeline as in Figure 6. The treatment starts when the zone began receiving zone-based firms ($j = 0$). All hypothesis-testing results supported the validity of the parallel trend assumption and significant positive impacts. The impacts were from the IZ-based firms to local businesses located outside the IZ but within the commune.

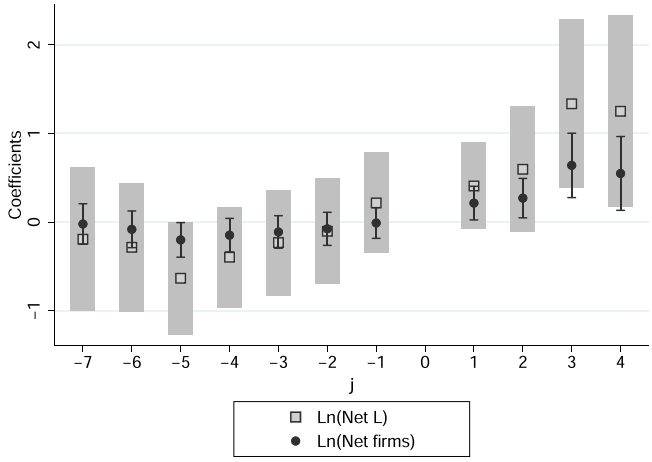

FIGURE 6 Impact of IZ-based firms on local businesses outside IZ boundaries among districts with IZs. Coefficients ($zone \times treated_j$) corresponding to ln (Net L) and ln (Net firms) as outcomes along with two-standard deviation bounds. p value = 0.1578 (0.2557) for H0: Parallel trend of DID is met where the outcome is ln (Net L) (ln (Net firms)). p value = 0.0375 (0.001) for H0: All $zone \times treated_j = 0 (j > 0)$ where the outcome is ln (Net L) (ln (Net firms)). IC, industrial cluster; IZ, industrial zone

Finally, we found that differences in treatment timing did not significantly affect our conclusions. Estimated by using procedure from de Chaisemartin and D'Haultfœuille (2020), the sum of the negative weights is equal to −0.03. Similarly, the results using Bacon DID (Supporting Information Appendix 9) supported our interpretations in the previous estimations. DID estimation and the never-treated component were positive for all main outcomes. The results were predictable because the control communes outweighed the treated communes. A total of 1790 control communes and 181 treated communes were included in our sample for 2007.

7 | CONCLUSIONS

We examined the impact of enterprise zones on local businesses in Vietnam and found significant (positive) relationships, that is, an increase in the number of firms and employment in communes and local businesses where IZs are located. The spatial spillover effects on the number of local businesses extended to a distance of 14 km. The involvement of the private sector in zone development significantly increased the number of zone-based firms. Therefore, the findings provide empirical evidence of a sustainable model for zone development.

We also must acknowledge several limitations to this study that should be addressed in future research. First, we were able to examine only 43% of the existing zones as of 2007. Second, the spillover effects may be greater than those suggested by the estimated results. Third, we considered only the stock of firms in a defined area (commune) and neglected the entry and exit of firms. An entry–exit analysis might offer better insights into the impact of place-based policies (Chaurey, 2017). This limitation was due to the VES sampling method in which the cut-off point (e.g., number of employees) for random sampling changed from year to year. Therefore, we were unable to identify whether the disappearance of firms in the data was due to the firm's exit or due to a reduction in the number of employees. Fourth, we lacked data on public investment in infrastructure for zones and communes. Therefore, we were unable to conduct a cost–benefit analysis of Vietnamese enterprise zones. However, at least the majority of ZIDs of IZs were private (see Table 1). The large proportion of IZs having private involvement in ZIDs suggests that zone policies were probably successful in general.

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CONFLICTS OF INTEREST

The authors declare no conflicts of interest.

DATA AVAILABILITY STATEMENT

Research data are not shared.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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