Institute for Economic Studies, Keio University

Keio-IES Discussion Paper Series

Do enterprise zones promote local business development? Evidence from Vietnam

Tien Manh Vu、山田浩之

2019 年 3 月 5 日
DP2019-008
https://ies.keio.ac.jp/publications/11150

Institute for Economic Studies, Keio University
2-15-45 Mita, Minato-ku, Tokyo 108-8345, Japan
ies-office@adst.keio.ac.jp
5 March, 2019

Do enterprise zones promote local business development? Evidence from Vietnam

Tien Manh Vu、山田浩之

IES Keio DP2019-008
2019 年 3 月 5 日
JEL Classification: O14, O25, L53
キーワード: Enterprise zone; Place-based policy; Infrastructure; Private sector; Local business development

【要旨】

We examined the effects of Vietnamese enterprise zones on local businesses based on different patterns of place-based policies as well as the ownership structure of the zone infrastructure developers (ZIDs). We constructed a panel of communes during 2000–2007 using a census survey of firms having more than nine employees and a census of zones and zone-based firms. We found that place-based policies led to growth in the number of jobs and firms in the communes where enterprise zones were located, even after excluding zone-based firms. Our findings also suggest that privately owned ZIDs worked best under corporate-tax incentives, while zones with a designated central government agency as the ZID had adverse spillover effects on business development in neighboring communes of the same district.

Tien Manh Vu
アジア成長研究所
〒803–0814
福岡県北九州市小倉北区大手町11-4
vu@agi.or.jp

山田浩之
慶應義塾大学経済学部
〒108–8345
東京都港区三田2–15–45
hyamada@econ.keio.ac.jp

謝辞：本研究は科研費（18K01580）2018-2020，（18K12784）2018-2021による研究成果である。ここに記して謝意を表したい。

Do enterprise zones promote local business development? Evidence from Vietnam

Tien Manh Vu[†] Hiroyuki Yamada[††]

Abstract

We examined the effects of Vietnamese enterprise zones on local businesses based on different patterns of place-based policies as well as the ownership structure of the zone infrastructure developers (ZIDs). We constructed a panel of communes during 2000–2007 using a census survey of firms having more than nine employees and a census of zones and zone-based firms. We found that place-based policies led to growth in the number of jobs and firms in the communes where enterprise zones were located, even after excluding zone-based firms. Our findings also suggest that privately owned ZIDs worked best under corporate-tax incentives, while zones with a designated central government agency as the ZID had adverse spillover effects on business development in neighboring communes of the same district.

JEL codes: O14, O18, O25, L53, L52

Keywords: Enterprise zone, Place-based policy, Infrastructure, Private sector, Local business development, Vietnam

[†] Corresponding author. Affiliation: Asian Growth Research Institute and Osaka University. 11–4 Otemachi, Kokura-kita, Kitakyushu, Fukuoka 803–0814, Japan. E-mail: vu@agi.or.jp

[††]Affiliation: Faculty of Economics, Keio University. 2–15–45 Mita, Minato–ku, Tokyo 108–8345, Japan. E-mail: hyamada@econ.keio.ac.jp

1. Introduction

Many countries have been using enterprise zones[1] to achieve economies of agglomeration, attract (foreign) investment, alleviate unemployment, and implement special policy experiments[2]. Regardless of the form of the zone, the establishment of such zones indicates an interference from the government in the allocation decisions of firms via tax and other incentive policies or subsidies. Thus, it is debatable whether place-based policies for enterprise zones are effective (Neumark and Kolko, 2010; Givord et al, 2013; Hanson and Rohlin, 2013) and reach pareto efficiency (Kline and Moretti, 2014; Fishback, 2017), especially if they entail public investment.

Until 2008, Vietnam had a unique dual, but non-overlapping, place-based policy system: a homogenous policy system of the central government (with corporate tax and export incentives) and a heterogeneous policy system of local governments (with land rent holidays). Each zone was subject to only one system. Interestingly, developer ownership of zone infrastructure has varied since the first zone was established in Vietnam in the early 1990s, with ownership residing with private firms, state-owned firms, and/or partnerships, including foreign investors. In addition, the central / local governments sometimes opted to become directly involved in developing and operating zone infrastructure. As a result, various patterns of place-based policies[3] have evolved in Vietnam.

In this paper, we build on this unique feature to estimate the direct and spillover effects of enterprise zones with different policy patterns on local businesses. To the best of our knowledge, this study is the first to consider two separate incentives: tax and land rent holidays, from the central government and local government, respectively. We are not aware of any previous study comparing the potentially different impacts of enterprise zones on local businesses, depending on the nature of ownership of the zone infrastructure developers (ZID). In addition, previous studies often focused on policies and incentives, but not on how the zone was developed and operated, especially if the developer was owned by a public or private

[1] See Akinci et al. (2008) for a detailed zone classification and its history of development worldwide.
[2] We acknowledge that "race-to-the-bottom" zones (such as lowering requirements in environment and labor regulations) also exist.
[3] This is different from the definition of Special Economic Zones (SEZ) in China. According to Wang (2013), the central government did not directly interfere in SEZs. A designated administrative committee, set up by and on behalf of the local government, was allowed to build and improve infrastructure.

enterprise. Moreover, rather than just the one-to-one spatial spillover effect (the spillover effect of a single enterprise zone at a time), this study considered the spatial spillover effects of various zone policies on every area not covered under any specific zone. This approach differs from previous concentric ring analyses such as Zheng et al. (2017) and Lu et al. (2018). Moreover, while our study is closely related to Lu et al. (2018), it differed not only in terms of the above contributions but also because we were able to identify zone-based firms, which Lu et al. (2018) were not. Our study is also among the few studies in the relevant literature that used the census data of a country.

More specifically, we focused on whether a zone's economic activities influenced local businesses. We defined a treated commune/district as one that hosted a zone in 2007. Conversely, a control or untreated commune/district had no zone in 2007. Thus, a commune in a treated district could either be a control commune or a treated one (see Figure 1). In terms of Vietnamese administrative divisions, the commune is the third level of administration, just below that of the district[4]. The place-based policies were applied only to zone-based firms. Local businesses were firms located outside the zone boundary. They operated in either treatment or control communes, and as such, were not eligible for the place-based policies.

To construct a panel of communes, we used a census of Vietnamese formal firms having more than nine employees during 2000–2007 and a census survey on zones, which contained detailed characteristics of the zones and the firms located within the zones. We first selected treated districts only, limiting the selection to those that did not have any zone until the end of 2002 to be able to compare the treated communes before and after the policy became effective and to carry out a pre-treatment parallel trend check. Then, we added control districts to the initial selection for comparing control communes in treated districts with control communes in control districts. Figure 1 illustrates our conceptual framework.

[Insert Figure 1 here]

We applied differences-in-differences (DD) in the panel analysis with a commune fixed effect, district-year fixed effect, and fixed effects of the year–commune characteristics in 2000. We also used clustered standard errors at the commune level. Our method was similar to a combination of geographic discontinuity design and DD, and allowed us to distinguish between

[4] The formal division of Vietnamese administrative units spans three levels: there were approximately 63 provinces, 700 districts, and over 10,000 communes during 2000–2007.

zone-based firms and other local businesses. Finally, we added a concentric ring analysis to estimate the spatial spillover effects from any enterprise zone to untreated communes in various data selections.

In general, we found that place-based policies led to a growth in both employment and the number of firms in the communes where the enterprise zones were located, even after excluding zone-based firms. Especially, we found that a private ZID under the regulation of the central government would perform best. However, when control districts were included, we found that enterprise zones developed and operated directly by the designated central government agency would have an adverse effect on the development of firms located in the control communes of the treated districts. Using concentric ring analysis, we found the spatial spillover effects to be limited to cases in which the treated commune was located within 0–14 km from the center of the control commune.

Our study is organized as the follows. Section 2 describes the heterogeneous policy patterns on enterprise zones in Vietnam during 2000–2007 and their unique features. Section 3 reviews related literature, followed by description of the data used in Section 4. Section 5 presents our identification strategies and methods. We report the results in Section 6 and provide additional discussion and concluding remarks in Section 7.

2. Vietnamese enterprise zones and heterogeneous policy patterns during 2000–2007

Vietnamese enterprise zones are broadly classified into two types: one under the central government and the other under provincial governments. The details are as follows.

The first enterprise zone was established in Vietnam as early as 1991; however, it was Vietnamese Government Decree 36 (dated April 24, 1997)[5] that first officially defined and regulated three types of enterprise zones: industrial zones, export processing zones, and high-

[5] The decree was valid until March 2008 and replaced by Government Decree 29/2008. This replacement granted the provincial governments the right to set up enterprise zones eligible for national place-based policies. All Boards were also moved under provincial governments. The prime minister only issued approvals on national and regional master plans of industrial zone development. However, an enterprise zone was required to already be included in the master plan before it could officially be established.

technology zones. Only zone-based firms were eligible for the corresponding place-based policies[6].

According to Government Decree 36, industrial zones are dedicated to industrial manufacturing and related services, while export processing zones are for exporting goods and supporting services. In contrast, high-technology zones are a centralized area for technology-intense firms. However, regardless of type, each zone must have a defined boundary and be isolated from residential areas. Additionally, only the prime minister possesses the authority to establish these zones (hereafter, IZ[7]). The prime minister also established Provincial Boards of Industrial Zone Management directly under his authority. The Boards functioned as a "zone government" on behalf of the prime minister to carry out governmental administrative work, but they operated independently from provincial presidents. Moreover, the decree did not restrict the Boards from becoming ZIDs[8]. The Boards enjoyed direct budget resources from the central government. The provincial government of the area in which the IZ was located had limited right to interfere with the functioning of the IZ, acting mainly to clear land for developing the IZ and nominating board personnel. Firms wishing to located in IZs to take advantage of the policy benefits were required to register and undergo a board approval process.

All IZ-based firms received the same set of benefits from the central government. For example, firms in high-technology zones can enjoy complete corporate-tax exemption for the initial eight years, and then pay only a flat 10% rate of corporate income tax, if profitable. Manufacturing (service) firms in export processing zones can pay just a 10% (15%) flat rate of corporate income tax from the fifth (second) year, if profitable. Firms in industrial zones 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.

According to the decree, each IZ must have a ZID. The ZID could be any kind of firm (private, state-owned, partnership), as long as it was not entirely foreign owned. The ZID first contracts with the Boards for a land grant. It can then call for investors/firms to locate in the zone. The ZID builds, maintains the infrastructure, and then leases it to firms who later locate their operations in the zone. The ZID is also required to provide environmental protection

[6] A majority of the zones were within a commune boundary; however, a district might host several zones.
[7] We used "IZ" because among zones under the central government, industrial zones were the largest in number.
[8] Vietnamese legal documents often referred to ZID as "infrastructure investors" or sometimes, "infrastructure operators."

measures and other services for zone-based firms. In turn, the ZID also receives some incentives, which are even more generous than those for the potential zone-based firms.

However, the government decree did not limit the freedom of provincial governments to set up their own enterprise zones. In fact, provincial governments established their own zones (industrial cluster, hereafter, IC), with the functions and organizational structures of ICs similar to those of IZs. ICs also have a definite boundary and are separated from ordinary residential areas. However, firms in ICs are not eligible for the generous benefits available to those in IZs during the years we study (2000–2007)[9]. ICs were also smaller in terms of area and scale, as seen in Table 1. Firms in ICs enjoy favorable policies from the local government, which must comply with national rules and regulations. Provincial governments also sought to name these zones in such a way as to reflect their smaller scale, using terms such as industrial cluster and industrial village (in Vietnamese: "cụm công nghiệp" and "làng nghề").

[Insert Tables 1 here]

To the best of our knowledge, there were no legal documents from the central government to regulate ICs until Government Decree 105/2009 in 2009. This decree officially defined ICs as we have described above and set a maximum land area of 50 hectares (expandable up to 75 hectares). Eight years later, Government Decree 68/2017 in May 2017 officially recognized a set of benefit policies for IC-based firms. The main benefit was land rent holidays for 7 (11) years for industrial clusters (industrial villages) and up to 11 (15) years for infrastructure developers of industrial clusters (industrial villages). However, there were no tax incentives for firms in ICs. Local incumbent firms were encouraged to locate in ICs. We acknowledge potential variations in place-based policies among ICs.

In short, the main differences between IZ and IC design lay in three major areas. First, IZs were established by the central government, while ICs were set up by local governments. Second, IZs had generous corporate tax incentives, while ICs had a land rent incentive package. Third, IZs promoted export activities, while ICs were more for local business activities. We note that ICs were not "losers" compared to IZs, as proposed in Hanson and Rohlin (2013); however, it might be considered that ICs were created with the aim to support IZs.

[9] However, after 2007, the presidents of each province were eligible to set up all types of zones based on the master plan approved by the central government. Firms in such zones could enjoy tax incentive packages from the central government. Several amendments and new regulations added more types of zones in 2008, 2009, 2013, 2015, and 2017.

3. Related literature

There is a wealth of literature in economics that debates whether enterprise zones have benefited the corresponding areas; whether their cost has been recovered through their gain, if they were constructed and operated by public investment; and whether enterprise zones have accelerated local business activities immediately outside the zone and in other parts of the region.

Identifying a valid control group is always the first challenge when evaluating a place-based policy. The choice of control group can significantly influence the findings and conclusions (Neumark and Kolko, 2010). Neumark and Kolko (2010) also suggested that the ideal control group should be similar to the zone, but without the policy design.

Geography boundary discontinuity design and its combination with differences-in-differences analysis have been widely used in the literature (Dell, 2010; Lee and Lemieux, 2010; Neumark and Kolko, 2010; Duranton et al, 2011; Gibbons et al, 2013; Givord et al, 2013; Hanson and Rohlin, 2013; Keele and Titinuik, 2015; Zheng et al, 2017; and Lu et al, 2018). The rationale was to be able to compare an inner zone with a tiny outer zone close to the zone boundary. The former was eligible for place-based policy, while the latter, as the control group, was not (Holmes, 1998). Even up to this point, identifying the exact geographical boundary has always been a challenge (Neumark and Kolko, 2010) because zone boundaries did not follow a standard postal code/geographic tract system.

In addition, if a spillover effect existed and reshaped the control group, the estimated difference would understate the direct effect (Miguel and Kremer, 2004), or even overstate it in the case of a negative effect. Re-allocations of firms to the zone (Hanson and Rohlin, 2013; Chaurey, 2017) and competition between zone-based and local firms located near the boundaries (Hanson and Rohlin, 2013) were typical examples of negative effects. In addition, characteristic differences in the baseline between the treated and control areas were not small, even after controlling for the area's fixed effects. Area characteristics such as productivity, transportation development, and climate could have influenced firms' decisions regarding their location. These characteristics were difficult to distinguish from the effect of government policies (Holmes, 1998). Similarly, if a zone policy was aimed at poverty alleviation, target areas might have simultaneously received several favorable (place-based) policies (Briant et al., 2015; Neumark and Kolko, 2010).

Previous studies recorded several measures used to justify the selection of the control group. Neumark and Kolko (2010) indicated that counterfactual areas might work if using propensity score matching (PSM) methods and that one should not use post treatment characteristics for PSM. However, PSM overlooked unobservable characteristics (Neumark and Kolko, 2010). Gibbons et al. (2013) suggested relaxing the assumption of the spillover effect, by accepting boundary effects and spatial trends. In addition, Allcott (2015) showed a "site selection bias" in estimating the program impact when comparing ordinary program evaluation and randomized control trials. The bias was probably caused by a high correlation between the selected treatment area and its later impact.

Some other works used "loser" and "winner" comparisons, such as Greenstone et al. (2010), Busso et al. (2013), Kline and Moretti (2014), and Zheng at al. (2017). They compared the actual zone with the runner-up candidate. This was because at the time of deciding a location for zone, the runner-up and the winner would have had similar characteristics. Busso et al. (2013) used a "placebo" area in the IZ counties as the control group and then compared the "loser" and "winner" after re-weighting based on the control group. However, Neumark and Kolko (2010) noted that other central or local government policies might affect the areas at the same time. For example, State Enterprise Zones, Federal Empowerment Zones, and Federal Enterprise Community programs co-exist in the US (Ham et al., 2011). Wang (2013) indicated another potential issue, namely, that the timing of the policies was 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) suggested using area and area-year fixed effects to solve this potential issue.

With recent advances in geocoding addresses and boundaries, some studies (Zheng et al., 2017; Lu et al., 2018) have suggested using concentric ring analysis. This method involves establishing a set of rings with a constant small step (i.e., every +2 km) around the zone. Each concentric ring is considered an impact area. This method can identify a critical distance at which the spillover effect is modest. Thus, while previous studies have considered the effects of a zone on the nearest untreated areas, they have not yet investigated cases in which various zones simultaneously influence an untreated area.

4. Data

We used two main sources of data: the Vietnam Enterprise Survey (VES) 2000–2007 and the Vietnamese Establishment Census Survey (ECS) 2007 to create a set of panel data of communes during 2000–2007 for analysis. We selected this date range for three important reasons. First, 2000 was when the VES was initiated. Second, until the end of 2007, IZs were homogenously controlled and monitored by the Provincial Boards of Industrial Zone Management directly under the Prime Minister of Vietnam. Third, the government of Vietnam had almost the same cabinet form 1997 until June 26, 2006, which secured stability and consistency in other general policies and arrangements of the central and local governments.

Since 2001, the VES has been conducted annually by the General Statistics Office of Vietnam (GSO) to obtain information regarding formal firms located in Vietnam, including their performance in the previous year. The sampling methods were altered year by year. The main difference across years is the cut-off points based on the number of employees of private firms for random sampling. The values of the cut-off points also depended on the provinces and varied by year. The GSO aimed to conduct a census of firms above the cut-off. The sampling guidelines issued by the GSO between 2000 and 2007 point to a census survey of firms with more than nine workers regardless of province and firm ownership[10]. We aggregated the data within all communes during 2000–2007 to construct the panel data for the communes. VES provided detailed information about firms' employment, capital, sales, tax codes, and location (at commune level) annually.

However, during 2000–2007, the VES did not include information on whether the firm was located in an IZ/IC. Thus, we had to rely on the ECS conducted by the GSO. The ECS aimed to collect information on all establishments, such as firms, plants, factories, firm branches, and governmental offices in July 2007. ECS included two important questionnaires regarding IZs/ICs. For IZs, the Provincial Boards of Industrial Zone Management responded to the questionnaires, while for ICs, a government agency on behalf of the provincial president, provided the responses.

[10] Since 2008, the GSO has frequently changed this threshold in terms of the number of workers. For example, in 2008, GSO used a 19-worker threshold for Hanoi and a 29-worker threshold for Ho Chi Minh City.

In the first form of these questionnaires, all establishments located in IZs/ICs in July 2007 were listed, along with their detailed information, including registered official name and tax code. We used this information to identify the corresponding firm in the VES. However, we did not consider establishments that were a branch or a plant/factory of a larger firm[11]. We used a combination of the tax code and province code as a unique identifier in both the VES and ECS. We preferred to use firms having tax codes because those firms were the most important source of revenue for the local and/or central government. We were able to identify 3,300 firms in the VES that were located in IZs/ICs and that satisfied all the above conditions in 2007.

The second form was specifically related to the IZs/ICs. The questionnaires recorded detailed information regarding the exact date of establishment (based on the official decision to establish the zone) as well as the exact start date of operations. If the IZ/IC was still under construction, the expected operation date was used instead. There were 566 zones in the ECS; however, we used the ones established by the central government (179 IZs) or provincial government (265 ICs), both of which were expected to be either in operation or under construction.

We also obtained other important information regarding the ZID of the IZ/IC. The ZID designated by the boards of industrial zone management designed, built, operated, and invested in the infrastructure of the IZs/ICs based on either their own standards or certain standards and requirements from firms who intended to locate in these zones. The ZID then made the infrastructure ready for lease, retaining responsibility for maintenance and factory modification within the IZs/ICs. The ECS contained detailed information on the ownership of the ZIDs. Their ownership mainly resided with "governmental agencies" (which means the zone management boards directly acted as ZIDs), state-owned enterprises (SOE), private-owned enterprises (POE), or foreign-owned enterprises (FOE). Furthermore, the ECS showed that the ZID was sometimes a combination of two or more of these aforementioned entities, based on partnerships. The ECS included detailed information about the ZID, such as name, tax codes, and ownership (as well as the country of origin, if any). From the ECS, we could distinguish IZs from ICs based on who granted the zone establishment permission.

[11] This information was available in the ECS.

5. Sample selection, variables, and identification strategies

In this section, we explain the empirical strategies used to answer our two primary questions, namely, the direct effects and the spillover effects of enterprise zones on local businesses.

5.1 Direct effect analysis

5.1.1. Sample selection criteria, outcome variables, and control variables

We used each commune in each year as an observation unit. Our sample selection strategy was to secure the validity of conducting a difference-in-difference analysis. All communes in the selected sample should have at least some years during 2000–2007 without any IZ/IC. We selected 2003 as the earliest year any selected commune could host an IZ/IC to be able to compare the treated communes before and after the policy became effective and for a pre-treatment parallel trend check[12]. Further, the analysis was restricted to "treated" communes, which are defined as communes with an IZ or IC that started their operation between 2003 and 2007, and "control" communes, which are defined as communes with no IZ or IC between 2000 and 2007, but belonging to the same district as the treated communes. We acknowledged that the location choice to build IZs/ICs was endogenous. For example, most IZs/ICs found on the website of Vietnamese industrial zones (www.industrialzone.vn) are conveniently located with respect to international hubs (international airports, seaports, and railroads) and major cities. Therefore, it was difficult to select a counterfactual location/commune whose baseline characteristics were equivalent to those of the treated location/commune. This was simply because there were not many choices left.

Therefore, to the best of our knowledge, selecting control communes from the same district as the treated commune(s) would be the best approach, given the information available[13]. We argue that control communes in the districts with treated commune(s) would have the most similar baseline characteristics (year 2000)[14], at least in terms of location. In addition, when choosing the candidates for setting up an IZ/IC, policymakers may have placed

[12] The difference between treated and control communes might be significant; however, we can only test a parallel trend before the treatment time.
[13] We did not have luxury of transparent information about the history of IZ/EC establishment and detailed information on how policymakers chose the zone, as found in Zheng et al. (2017).
[14] We tested the differences in characteristics in the baseline year, as shown in Appendix 4. They were similar in terms of distance to international airports and seaports in Vietnam.

some of the control communes in the same district as treated communes located in the shortlist. This sample selection criteria yielded 62 communes with IZs and 122 with ICs, and the resulting sample comprised 1,971 communes in 124 treated districts. We were able to construct a balanced panel of 1,971 communes for the eight-year period. The descriptive statistics can be found in Appendix 1.

From VES, we calculated two important outcome variables for each commune in each year: the logarithm of the total number of workers ($Ln(L)$) and the logarithm of the total number of formal firms ($Ln(Firms)$) for any formal firm having more than nine employees located in each commune in each year.

For control variables, we deployed four different baseline characteristics calculated for each commune in 2000 from VES, including the logarithm of total capital and sales per worker in formal firms located in the commune, distance to nearest seaport, and distance to nearest international airport. We compared these characteristics in Appendix 4. The distances to seaports and international airports were not statistically significant; however, the others were significantly different at various statistical confidence levels[15]. We controlled the differences in characteristics in the baseline by interacting these characteristics with year dummies (notated as $baseline_{i,2000} \times year_t$) and district-year dummies ($district_{kit} \times year_t$) as suggested by Wang (2013).

5.1.2. Empirical specification and identification strategy for direct effect analysis

First, to check for parallel trends, we estimated the following equation:

$$Outcomes_{it} = \theta_{1t} \times IZ_i \times year_t + \theta_{2t} \times IC_i \times year_t + \theta_{3kt} \times district_{kit} \times year_t + \theta_{4it} \times baseline_{i,2000} \times year_t + commune_i + \zeta_{it}, \quad (1)$$

where i notates commune and t notates year (2000–2007). $Outcomes_{it}$ are the two outcome variables mentioned in Section 5.1.1 for commune i in year t. IZ_i is the IZ-specific treatment commune dummy, and IC_i is the IC-specific treatment dummy. Variable $year_t$ represents the year fixed effect, $commune_i$ is the commune fixed effect, and $district_{kit}$ is the fixed effect of district k where commune i is located. Variable $baseline_{i,2000} \times year_t$ is the baseline

[15] Therefore, at least among the selected districts, the decision to choose a commune to host a zone would be based on the advantages of the location rather than on social programs (such as unemployment elimination). Thus, this is different from the Regional Selective Assistance in the UK (Criscuolo et al., 2019), the Indiana Enterprise Zone program (Papke, 1994), and the New Deal in the US (Fishback, 2017).

characteristics-year interaction term. Standard errors are clustered at the commune level. The statistical significance of θ_{1t} and θ_{2t}, for the years of 2000 and 2001 (considering 2002 as a base category) would be a validation test of DD estimations. They ideally should be statistically insignificant.

Then we modified Equation (1) to estimate the following main specification:

$$Outcomes_{it} = \alpha_1 \times IZ_i \times treated_{it} + \alpha_2 \times IC_i \times treated_{it} + \alpha_{3kt} \times district_{kit} \times year_t + \alpha_{4it} \times baseline_{i,2000} \times year_t + commune_i + \varepsilon_{it} \quad , \tag{2}$$

where $treated_{it}$ takes 1 if an IZ (or IC) in commune i in year t is in operation. α_1 and α_2 acted as difference-in-differences estimators and identified how much the policy on IZs/ICs influenced the outcomes.

In addition, distinguished from previous literature, we can identify (A) the time when the zone was under construction, and (B) the composition of the ZID. Therefore, for our main estimations, we were able to add two sets of detailed dummies. We assumed that the time of the establishment decision coincided with the starting point of constructing the zone. We denoted any time in and after the year of zone establishment by a dummy $established_{it}$ for the hosting commune. We also assumed that the construction duration lasted until the zone was ready for renting out for the first time. Similarly, during this construction period, we used dummy $under_{it}$ to indicate this condition. We added another dummy $treated_{it}$ to denote any time when and after the zone was first ready for renting out. Thus, $established_{it}$ covered both $under_{it}$ and $treated_{it}$.

For (A), we estimated:

$$Outcomes_{it} = \beta_1 \times IZ_i \times treated_{it} + \beta_2 \times IZ_i \times under_{it} + \beta_3 \times IC_i \times treated_{it} + \beta_4 \times IC_i \times under_{it} + \beta_{5kt} \times district_{kit} \times year_t + \beta_{6it} \times baseline_{i,2000} \times year_t + commune_i + \epsilon_{it} \quad , \tag{3}$$

where $under_{it}$ takes 1 if an IZ (or IC) in commune i in year t is still under construction. All other notations are the same as those in Equation (2).

For (B), we estimated:

$$Outcomes_{it} = \gamma_{1l} \times IZ_i \times established_{it} \times ownership_{il}$$

$$+\gamma_{2m} \times IC_i \times established_{it} \times ownership_{im}$$

$$+\gamma_{3kt} \times district_{kit} \times year_t + \gamma_{4it} \times baseline_{i,2000} \times year_t + commune_i + \omega_{it}, \quad (4).$$

where $established_{it}$ takes 1 as IZ (or IC) in commune i in year t was established[16]. $ownership_{il}$ ($ownership_{im}$) was ownership of the ZID of an IZ (IC) in commune i. It could be a $POE, SOE, FOE,$ or direct designated governmental agency ($governmental\ agency$). They correspond to the four types of ZID for all zones in Vietnam. As place-based policies were homogenous among IZs during 2000–2007, $ownership_{il}$ identified the difference in infrastructure operation.

5.2 Spillover effects and ring analysis

5.2.1. Sample selection criteria and variables

In the previous subsection (5.1), we used only the sample of districts having commune(s) with IZs or ICs between 2003 and 2007. We called those "treated" districts (but having both "treated" and "control" communes). In this subsection, we pooled communes in the "treated" districts and communes in "control" districts. Control districts are districts in which communes did not have any IZs or ICs between 2000 and 2007. Thus, all communes in these control districts are control communes. These control districts acted as "placebo" areas, similar to the approach applied in Busso et al. (2013).

For the outcome variables, we calculate "net" variables for each commune in each year: the logarithm of the total net number of workers ($Ln(Net\ L)$) and the logarithm of the total net number of formal firms ($Ln(Net\ Firms)$). These are obtained by deducting the number in each zone (IZ or IC) from the total number in each commune each year. So, $Ln(Net\ L)$ and $Ln(Net\ Firms)$ in control communes are equal to the logarithms of the total number of workers and firms, respectively. For treated communes, these values indicate the net outcomes for local businesses located outside the IZ/IC border but within the treated commune. In addition, we followed Vu and Yamada (2017) to impute zero values. For control variables, we use the same set of variables described in section 5.1.1. The descriptive statistics can be found in Appendix 1.

[16] We did not divide $established_{it}$ into $under_{it}$ and $treated_{it}$ as Equation (3) because the set of dummies became fragmented.

5.2.2. Empirical specification and identification strategy for spillover effects and ring analysis

For the first spillover analysis, we estimated the following equation:

$$Net\ Outcomes_{it} = \beta_1 \times IZ_i \times treated_{it} + \beta_2 \times IZ_i \times under_{it} + \beta_3 \times IC_i \times treated_{it} + \beta_4 \times IC_i \times under_{it} + \beta_5 \times IZ.treated.district_{nt} + \beta_6 \times IC.treated.district_{nt} + \beta_7 \times IZ.IC.treated.district_{nt} + \beta_{8qt} \times district_{qit} \times year_t + \beta_{9it} \times baseline_{i,2000} \times year_t + commune_i + \epsilon_{it}, \quad (5)$$

where $Net\ Outcomes_{it}$ is each of the two net variables ($Ln(Net\ L)$and $Ln(Net\ Firms)$), discussed above. $IZ.treated.district_{nt}$ takes 1 at year *t* if any IZ is in operation in a district *n* without an IC. District *n* contains commune *i*. Similarly, $IC.treated.district_{nt}$ takes 1 at year *t* if there is any IC in operation in a district *n* without an IZ. However, if both IC and IZ are in operation at year *t* in the same district *n*, $IZ.IC.treated.district_{nt}$ takes 1. β_1-β_4 shows the difference between treated communes and control communes of the same treated district. Meanwhile, β_5-β_6 indicates the spillover effects into the control communes of treated districts compared with other control communes in control districts.

Next, we repeated the exercise in Equation (4) in this new sample selection in the following equation:

$$Net\ Outcomes_{it} = \gamma_{1l} \times IZ_i \times established_{it} \times ownership_{il} + \gamma_{2m} \times IC_i \times established_{it} \times ownership_{im} + \beta_{3k} \times IZ.established_{nt} \times treated.district_n \times ownership_{nk} + \beta_{4o} \times IC.established_{nt} \times treated.district_n \times ownership_{no} + \beta_{5np} \times IZ.IC.established_{nt} \times treated.district_n \times ownership_{np} + \beta_{6qt} \times district_{qt} \times year_t + \beta_{7it} \times baseline_{i,2000} \times year_t + commune_i + \omega_{it} \quad (6)$$

where $IZ.IC.established_{nt} \times treated.district_n \times ownership_{np}$ takes 1 when it corresponds to one out of four types[17] of ZID ownership if: (a) treated district *n* contains two or more treated communes; and (b) both IZ and IC were established and represented in the district *n* in year *t*. $IZ.established_{nt} \times treated.district_n \times ownership_{nk}$ takes 1 in similar conditions except that district *n* had only IZ(s) at year *t*.

Our previous estimations using Equations (2)–(4) and the "net value" of the outcomes show whether the spillover effects of the policy patterns in treated communes could influence firms located outside the zone border but still in the treated commune. The estimation results should be of interest to policymakers because they can identify whether the impacts of their policies extended across their area of administration.

However, these estimations still neglect the spatial spillover effect of the policy. For example, a commune in a control district might receive some effects from a nearby treated commune of a treated district. Similarly, a control commune of a treated district might be located too far away from the treated commune in the district and thus, might not be influenced. In addition, a control commune might be influenced by several treated communes located in the surrounding area. To the best of our knowledge, few studies have considered these spatial spillover effects from several zones to an untreated area.

Therefore, we have proposed a new strategy. We first selected all control communes during 2000–2007. We developed a set of concentric rings from the center of each control commune. The first ring radius was 2 km, and we increased the radius of the consecutive ring with a constant step of 2 km. We repeated this *s* times (*s* = [1, 49]) until reaching the largest ring with a 100 km radius. We counted any IZ/IC located in a commune whose center location fell into each ring interval and combined with the earliest treatment time of the IZ/IC (from the earliest IZ/IC's establishment year among those located in the same ring interval) to construct a set of 49 ring dummies. Then, we estimated the following equation:

$$Outcomes_{it} = \mu_{1s} \times IZ.Ring.step_{2\times s,i} \times first.IZ.established_{2\times s,i,t} + \mu_{2s} \times IC.Ring.step_{2\times s,i} \times first.IC.established_{2\times s,i,t} + \mu_{3s} \times IZ.IC.Ring.step_{2\times s,i} \times established_{2\times s,i,t}$$

[17] We used a dummy of specific ownership regardless of the number of IZs/ICs falling into one type of the four.

$$+\mu_{4kt} \times district_{kit} \times year_t + \gamma_{5it} \times baseline_{i,2000} \times year_t + commune_i + \omega_{it} \quad , \tag{7}$$

where $IZ.Ring.step_{2\times s,i} \times first.IZ.established_{2\times s,i,t}$ takes 1 if at least one IZ, but no IC, is present in any commune whose center is between 2×(*s*–1) and 2×*s* kilometers from the center of commune *i* at year *t*. μ_{1s} (μ_{2s}) showed the spillover effects on the control commune during time *t* when IZ (IC) was the first and only zone located in the ring numbered *s*. If the ring was host to a mixture of IZs and ICs, μ_{3s} was the corresponding coefficient. Figure 2 illustrates our conceptual framework on concentric ring analysis.

[Insert Figure 2 here]

5.3 Heterogeneity and Nickell bias

One of the challenges is that the establishment of an IC might be correlated with an existing IZ. This is because the IZ was already established and has benefited from the central government's policy. The IZ might need some suppliers located nearby or that are industrially clustered. The IZ might not be able to accommodate all kinds of suppliers inside the zone due to zone-entry conditions. Thus, the local government might have an incentive to install ICs next to IZs to serve the needs of the IZ occupants.

Therefore, we carefully separated the selected sample into five cases: a) treated districts with IZs or ICs, which we used for the main reports; b) treated districts with IZs but excluding districts having communes with only ICs; c) treated districts with ICs but excluding districts having communes with only IZs; d) treated districts hosting only IZs; and e) treated districts hosting only ICs. We repeated all estimations from Equations (1)–(5) for b)–e) and have reported the results in our appendices. In the case of Equation (7), we repeated all estimations with control communes in treated districts and with control communes in control districts.

One concern when using a short time range panel and several observations is the Nickell bias (Nickell, 1981). We followed the suggestions from Angrist and Pischke (2009) and conducted additional estimations using a one-year-lagged outcome as a control variable but excluding commune fixed effects in the standard ordinary least squared (OLS) method with robust commune-clustered standard errors. All other control variables and outcomes were the same as Equations (1)–(7). Angrist and Pischke (2009) implied that the real effects lay between the coefficients of the OLS and the coefficients of the previous estimations using the commune fixed effect. We reported estimations of both specifications in the main results.

6. Results

6.1 The effectiveness of zone policies: direct effects

First, we find the zone policies to be effective for all formal firms located in the administrative unit (commune) as seen in Columns (1) to (4) of Table 2. The zone policies helped increase the number of formal jobs and firms in the commune hosting the zone. The effectiveness emerged immediately following the construction of the zone and was magnified once the zone became fully operational. However, we acknowledge that limited information on internal labor migration caused us to refrain from further deducting a decrease in unemployment in the treated commune.

[Insert Table 2 here]

Second, the zone policies by the central government had different levels of influence in formal firms located in the commune compared with zone policies by provincial governments. The influence of the central government policies tended to be larger. This could be because of either more generous benefits from the central government, economies of scale (the magnitude of the coefficient of IZ is often bigger than that of IC), or that IZ-based firms under export incentives are more likely to do business with firms located in various areas rather than with only those around the zone boundary.

Third, we found different ownership types of ZID could have different associations with local business development in the treated commune (Columns (5) and (7) in Table 2). For IZs with private or state-owned ZIDs, the positive association with the number of workers and firms tended to become statistically significant. The coefficients for foreign ZIDs are also positive and some are statistically significant. By contrast, the coefficients of central government agencies are not as pronounced. Perhaps, as a ZID, the private sector, in particular, would have more advantages in connecting with local businesses than the government itself. Private-sector participants might also be more efficient in developing and managing zones. This argument matched with and provided empirical evidence for lessons learnt from the history of developing modern SEZs as provided by Akinci et al. (2008). In addition, our findings suggest that the influence of private ownership was apparent, regardless of the concern

in Akinci et al. (2008)[18]. However, we acknowledge that we did not consider the endogenous decision to grant ZID right to a firm. Besides, a governmental agency might take over the role of infrastructure developer if the expected profit does not surpass the cost, which would prove unattractive for any potential profit-based infrastructure developer. As a result, limited information on the cost of zone development and operation deterred us from conducting further cost-benefit analyses.

Meanwhile, provincial government agencies acting as ZID statistically worked best with ICs. However, place-based policies could be very heterogeneous across ICs at a provincial level. In addition, we noted that approximately 79% of ICs were under provincial administration. One possibility was that the provincial government might be powerful enough to drive local firms into one location. Another possibility was related to rent exemption incentives. If the specifics of the land rent holiday legalized in 2017 had already been applied during 2000–2007, IC-based firms would have been free from land rental costs for 11 years. A profit-run ZID cannot wait for 11 years to collect this revenue. Thus, the local government was probably the entity best able to afford developing ICs.

Figure 3 is a graphical visualization on the parallel trend test (base year: 2002). Zero falls between the upper and lower confidence intervals in 2001, implying there is no systematic difference between treatment and control communes. This confirms the parallel trend condition, at least in 2001.

[Insert Figure 3 here]

6.2 Spillover effects of zone policies on local businesses located outside the zone boundary

First, we use only the sample of treatment districts to estimate Equation (5), but the outcome variables are net ones ($Ln(Net\ L)$ and $Ln(Net\ Firms)$). That is, we exclude the corresponding zone-based firms from outcome calculations. Table 3 reports the results. We find evidence of positive spillover effects of zone policies on local businesses outside the zone boundary but within the treated communes. The results for IZs matched findings from Wang (2013) for Chinese SEZs. Perhaps, IZ-based firms were more likely to be export firms and did

[18] The concern was that the reluctance of local governments to get involved in private zones would have caused inadequate investment in infrastructure to connect the zone to the outside, causing the Vietnamese private zone to remain vacant.

not compete with local firms in the output market. At the same time, the positive spillover effects could imply that the positive direct effects are underestimated.

However, compared with the previous cases, we barely found statistically significant effects of IZ policies during the zone construction period. In contrast, they were apparent for ICs under construction. Perhaps, ICs were established by provincial governments for the benefit of local people. Thus, ICs would have less incentive to prioritize outside partners to construct the zone.

[Insert Table 3 here]

In addition, we found IZs and ICs had different effects on local businesses (using the outcomes of "net" values), but they were consistent with previous results using the outcomes as a "total" value (the ones in Subsection 6.1), especially in eight specific cases of ownership as shown in Table 3. IZ communes during the treated period had a higher impact on not only local businesses located in the commune, but also those outside the zone boundary, compared with IC communes in the corresponding period.

Next, communes in the "treated" districts and "control" districts are pooled to estimate Equation (5). Table 4 reports the results. We found the spillover effect was not limited to local businesses located in the treated communes during the treatment period. The effect existed even in other communes of the same treated districts when we added communes from control districts to the selected sample. Especially, the effect was more pronounced in districts hosting both IZs and ICs.

[Insert Table 4 here]

Further, breaking down the ownership of the ZID, we found the spillover effects were consistent with all our previous estimations. Private ZIDs worked best among communes hosting IZs, as shown in Table 5. In contrast, provincial government agencies offered the best alternative among communes with ICs.

[Insert Table 5 here]

However, we found evidence suggesting an adverse effect of IZ policies on formal firms located in control communes of the treated districts (compared with those located in other control communes of the "control" districts). As seen in Table 5, coefficients of $IZ.established \times treated\ district \times governmental\ agency$ became statistically significant and negative for all outcomes. This showed that formal firms in control communes

of treated districts where IZs were under stricter control from the central government would have an adverse effect compared with other firms in communes of "control" districts. The results may imply the central government policy might have allocated resources to IZs at the expense of firms located in control communes. However, it could also be that the central government's benefits attracted firms to relocate from control communes to or close to the zone or treated commune.

Finally, we showed that the spatial spillover effects worked best within a 0–10 km (4-14 km) distance from the IZ (IC) hosting communes, as shown in Figures 4–5. The spatial spillover effects were muted outside the range.

[Insert Figure 4 and 5 here]

6.3 Checks on heterogeneity and true estimates considering Nickell bias

After repeating all estimations in Equation (4) with additional data selections, including districts with all IZs, with all ICs, with IZ but no IC, and with IC but no IZ, we found our estimation results to be robust and consistent, as seen in Appendices 2–3, 6–7, and 10–13. Policies on IZs/ICs had a positive effect on local businesses located in the same commune but outside the zone boundary regardless of the data selection. Similarly, the ownership type of ZID mattered for the development of local businesses, which was found to be consistently true regardless of data selection.

Finally, as explained in Subsection 5.3, we conducted estimations using a one-year-lagged outcome as a control variable but excluding commune fixed effects in standard OLS for exploring the issue of Nickell bias (we call this "alternative" specification). These estimation results are shown in Tables 2-5, respectively. Broadly speaking, our preferred specifications, which included commune fixed effects but not lagged outcome variables as control variables, yielded coefficients with a larger magnitude in absolute value terms, compared with the corresponding "alternative" specifications. Thus, the estimated coefficients using our preferred specifications were on the "upper side" while those using the "alternative" specifications were rather conservative. However, the qualitative results were very similar between the two specifications, and the main findings remain unchanged.

7. Conclusions

We estimated the impact of enterprise zones on local businesses from different aspects, such as place-based policies (either corporate tax or land rent exemptions), construction and operating periods, and ownership of ZIDs. We found significant (positive) relationships among these aspects, along with the growth in employment opportunities and number of formal firms both in the treated and control communes of treated districts. Private ZIDs worked best in enterprise zones under the regulation of the central government, providing empirical evidence of a sustainable model for zone development. Spatial spillover effects from treated communes to control communes were found within a range of 0–14 km.

However, we acknowledge several drawbacks in our study, which should be addressed by future research. First, we considered only the stock of firms in a defined area (commune) and neglected the entry and exit of firms, which might offer better insight into the impact of place-based policies (Chaurey, 2017). However, as the VES sampling method was changed from year to year with a lower bound that excluded firms with less than ten employees, we were unable to identify whether the disappearance of firms in the data was due to firm exit or due to a firm's reduction in the number of employees. Second, we were unable to consider the effects of zones in different set-up periods, such as zones established in the early 1990s, due to data limitations. Third, we were aware of, but unable to address, the issues raised by Bertrand et al. (2004), indicating that used outcomes would exhibit positive serial autocorrelation, while the treated dummies changed very little within the commune. Fourth, we had limited information to conduct cost-benefit analyses among zones fully controlled and developed by the local (central) government agencies. However, the majority of ZIDs of IZs were private (see Table 1), and the larger impacts from private ZIDs suggest that place-based policies were probably successful, in general.

Acknowledgements

This work was supported by JSPS (Japan Society for the Promotion of Science) KAKENHI Grant Number 18K12784 and 18K01580, and project grants from the Asian Growth Research Institute.

References

Akinci, G., Crittle, J., Akinci, G., & Crittle, J. (2008). *Special Economic Zone: Performance, Lessons Learned, and Implication for Zone Development (English)*. Foreign Investment Advisory Service (FIAS) occasional paper. Washington, DC: World Bank. Link: http://documents.worldbank.org/curated/en/343901468330977533/Special-economic-zone-performance-lessons-learned-and-implication-for-zone-development. Last accessed January 29, 2019.

Allcott, H. (2015). Site Selection Bias in Program Evaluation. *The Quarterly Journal of Economics*, *130*(3), 1117–1165. http://doi.org/10.1093/qje/qjv015

Angrist, J. D., & Pischke, J-S. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton, NJ: Princeton University Press.

Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How Much Should We Trust Differences-In-Differences Estimates? *The Quarterly Journal of Economics*, *119*(1), 249–275. http://doi.org/10.1162/003355304772839588

Briant, A., Lafourcade, M., & Schmutz, B. (2015). Can Tax Breaks Beat Geography? Lessons from the French Enterprise Zone Experience. *American Economic Journal: Economic Policy*, *7*(2), 88–124. http://doi.org/10.1257/pol.20120137

Busso, M., Gregory, J., & Kline, P. (2013). Assessing the Incidence and Efficiency of a Prominent Place-based Policy. *American Economic Review*, *103*(2), 897–947. http://doi.org/10.1257/aer.103.2.897

Chaurey, R. (2017). Location-based Tax Incentives: Evidence from India. *Journal of Public Economics*, *156*, 101–120. http://doi.org/10.1016/j.jpubeco.2016.08.013

Criscuolo, C., Martin, R., Overman, H. G., & Van Reenen, J. (2019). Some Causal Effects of an Industrial Policy. *American Economic Review*, *109*(1), 48–85. http://doi.org/10.1257/aer.20160034

Dell, M. (2010). The Persistent Effects of Peru's Mining Mita. *Econometrica*, *78*(6), 1863–1903. http://doi.org/10.3982/ECTA8121

Duranton, G., Gobillon, L., & Overman, H. G. (2011). Assessing the Effects of Local Taxation using Microgeographic Data. *Economic Journal*, *121*(555), 1017–1046. http://doi.org/10.1111/j.1468-0297.2011.02439.x

Fishback, P. (2017). How Successful Was the New Deal? The Microeconomic Impact of New Deal Spending and Lending Policies in the 1930s. *Journal of Economic Literature*, *55*(4), 1435–1485. http://doi.org/10.1257/jel.20161054

Gibbons, S., Machin, S., & Silva, O. (2013). Valuing School Quality using Boundary Discontinuities. *Journal of Urban Economics*, *75*(1), 15–28. http://doi.org/10.1016/j.jue.2012.11.001

Givord, P., Rathelot, R., & Sillard, P. (2013). Place-based Tax Exemptions and Displacement Effects: An Evaluation of the Zones Franches Urbaines Program. *Regional Science and Urban Economics*, *43*(1), 151–163. http://doi.org/10.1016/j.regsciurbeco.2012.06.006

Greenstone, M., Hornbeck, R., & Moretti, E. (2010). Identifying Agglomeration Spillovers: Evidence from Winners and Losers of Large Plant Openings. *Journal of Political Economy*, *118*(3), 536–598. http://doi.org/10.1086/653714

Hanson, A., & Rohlin, S. (2013). Do Spatially Targeted Redevelopment Programs Spillover? *Regional Science and Urban Economics*, *43*(1), 86–100. http://doi.org/10.1016/j.regsciurbeco.2012.05.002

Ham, J. C., Swenson, C., İmrohoroğlu, A., & Song, H. (2011). Government Programs Can Improve Local Labor Markets: Evidence from State Enterprise Zones, Federal Empowerment Zones and Federal Enterprise Community. *Journal of Public Economics*, *95*(7–8), 779–797. http://doi.org/10.1016/j.jpubeco.2010.11.027

Holmes, T. J. (1998). The Effect of State Policies on the Location of Manufacturing: Evidence from State Borders. *Journal of Political Economy*, *106*(4), 667–705. http://doi.org/10.1086/250026

Keele, L. J., & Titiunik, R. (2015). Geographic Boundaries as Regression Discontinuities. *Political Analysis*, *23*(1), 127–155. http://doi.org/10.1093/pan/mpu014

Kline, P., & Moretti, E. (2014). Local Economic Development, Agglomeration Economies, and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority. *The Quarterly Journal of Economics*, *129*(1), 275–331. http://doi.org/10.1093/qje/qjt034

Lee, D. S., & Lemieux, T. (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature*, *48*(2), 281–355. http://doi.org/10.1257/jel.48.2.281

Lu, Y., Wang, J., & Zhu, L. (forthcoming). Place-Based Policies, Creation, and Agglomeration Economies: Evidence from China's Economic Zone Program. *AEJ: Economic Policy*. https://www.aeaweb.org/articles/pdf/doi/10.1257/pol.20160272

Miguel, E., & Kremer, M. (2004). Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities. *Econometrica*, *72*(1), 159–217. http://doi.org/10.1111/j.1468-0262.2004.00481.x

Neumark, D., & Kolko, J. (2010). Do Enterprise Zones Create Jobs? Evidence from California's Enterprise Zone Program. *Journal of Urban Economics*, *68*(1), 1–19. http://doi.org/10.1016/j.jue.2010.01.002

Nickell, S. (1981). Biases in Dynamic-Models with Fixed Effects. *Econometrica*, *49*(6), 1417–1426. http://doi.org/10.2307/1911408

Papke, L. E. (1994). Tax Policy and Urban Development. Evidence from the Indiana Enterprise Zone Program. *Journal of Public Economics*, *54*(1), 37–49. http://doi.org/10.1016/0047-2727(94)90069-8

Vu, T.M., & Yamada, H. (2017). Firms and Regional Favoritism. AGI Working Paper Series 2017-16. Link: http://id.nii.ac.jp/1270/00000136/. Last accessed January 24, 2019.

Wang, J. (2013). The Economic Impact of Special Economic Zones: Evidence from Chinese Municipalities. *Journal of Development Economics*, *101*(1), 133–147. http://doi.org/10.1016/j.jdeveco.2012.10.009

Zheng, S., Sun, W., Wu, J., & Kahn, M. E. (2017). The Birth of Edge Cities in China: Measuring the Effects of Industrial Parks Policy. *Journal of Urban Economics*, *100*, 80–103. http://doi.org/10.1016/j.jue.2017.05.002

Appendix 1 Descriptive statistics of treated districts

Variable	Mean	Std. Dev.	Min	Max
Ln(L)	2.603	3.073	0	10.985
Ln(Firms)	1.366	0.898	0.881	6.246
Ln(Net L)	2.566	3.059	0	10.985
Ln(Net firms)	1.355	0.898	0	6.246
Year	2003.500	2.291	2000	2007
IZ × treated	0.007	0.086	0	1
IC × treated	0.012	0.110	0	1
IZ × under	0.005	0.070	0	1
IC × under	0.013	0.113	0	1
IZ × established × POE	0.007	0.086	0	1
IZ × established × SOE	0.004	0.059	0	1
IZ × established × FOE	0.001	0.024	0	1
IZ × established × governmental agency	0.001	0.031	0	1
IC × established × POE	0.003	0.053	0	1
IC × established × SOE	0.002	0.040	0	1
IC × established × FOE	0.000	0.018	0	1
IC × established × governmental agency	0.019	0.137	0	1
Baseline 1 (ln(total capital) in 2000 of the commune)	1.536	2.326	0	8.483
Baseline 2 (ln(sales per worker) in 2000 of the commune)	2.958	4.416	0	13.894
Baseline 3 (Distance to the nearest international airport in 2000)	98,023	72,199	2,974	314,909
Baseline 4 (Distance to the nearest seaport in 2000)	113,333	70,639	1,162	336,088
Number of identical districts	124			
Number of identical communes	1,971			

Appendix 2 Direct effects of IZs and ICs on local formal businesses in treated communes, by treated district selection

Treated district selection	IZ		IC		IZ only		IC only	
VARIABLES	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IZ × treated	1.5393***	0.6347***	1.4496**	0.6452***	1.5942***	0.6422***		
	(0.3704)	(0.1052)	(0.5819)	(0.1600)	(0.4649)	(0.1378)		
IC × treated	0.0439	0.1681	0.8669***	0.2938***			1.0372***	0.3288***
	(0.4963)	(0.2054)	(0.1919)	(0.0637)			(0.2005)	(0.0660)
IZ × under	0.7723**	0.1887**	1.0558*	0.1221	0.6660	0.2177**		
	(0.3797)	(0.0820)	(0.5781)	(0.1110)	(0.4702)	(0.1066)		
IC × under	–0.1848	–0.0604	0.4991***	0.1053**			0.6647***	0.1471***
	(0.3712)	(0.1078)	(0.1789)	(0.0465)			(0.1941)	(0.0492)
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,744	4,744	12,760	12,760	3,008	3,008	11,024	11,024
R–squared	0.397	0.486	0.358	0.486	0.365	0.422	0.343	0.461
Number of communes	593	593	1,595	1,595	376	376	1,378	1,378

Notes:
Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1).
All districts were treated districts sometime between 2003–2007.
All districts did not have any IZs or ICs until the end of 2002.
Data selection in column (5) to (8) did not have communes simultaneously hosting IZs and ICs.

Appendix 3 Direct effects of IZs and ICs by ZID ownership on treated communes, by treated district selection

Treated district selection	IZ		IC		IZ only		IC only	
VARIABLES	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IZ × established × POE	1.4253***	0.5053***	1.7546**	0.5467***	1.2597**	0.4868***		
	(0.4243)	(0.1219)	(0.6927)	(0.1899)	(0.5100)	(0.1545)		
IZ × established × SOE	0.8216**	0.3757***	0.2851	0.2908	0.9809**	0.4052***		
	(0.3594)	(0.1174)	(0.5792)	(0.1956)	(0.4610)	(0.1476)		
IZ × established × FOE	0.6983	0.6370***	0.7345	0.7652***	0.6024*	0.2817***		
	(0.6528)	(0.1120)	(0.9272)	(0.1393)	(0.3425)	(0.0963)		
IZ × established × governmental agency	0.9956*	0.1002	0.9519	0.1011	0.9435	0.0801		
	(0.5349)	(0.1361)	(0.5819)	(0.1968)	(1.0416)	(0.0897)		
IC × established × POE	–0.7271	–0.2180	0.0988	0.1195			0.5564	0.2894**
	(0.6411)	(0.1622)	(0.4656)	(0.1293)			(0.5432)	(0.1464)
IC × established × SOE	–1.0246*	–0.1669*	0.3010	0.0383			1.3297*	0.1628
	(0.5894)	(0.0926)	(0.5397)	(0.1235)			(0.6876)	(0.1969)
IC × established × FOE	0.8132	–0.0147	0.4289	–0.0477			0.6328*	0.0248
	(0.5317)	(0.2432)	(0.3079)	(0.0921)			(0.3412)	(0.0931)
IC × established × governmental agency	0.3949	0.2823	0.6963***	0.2230***			0.7450***	0.2226***
	(0.4500)	(0.2118)	(0.1732)	(0.0541)			(0.1841)	(0.0556)
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,744	4,744	12,760	12,760	3,008	3,008	11,024	11,024
R–squared	0.397	0.484	0.357	0.485	0.364	0.417	0.342	0.460
Number of communes	593	593	1,595	1,595	376	376	1,378	1,378

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1).

Appendix 4 Mean differences in characteristics in year 2000 between treated and control communes among treated districts

	Treated communes	Control communes	Difference	P–value
Total capital	30,567.8	17,896.5	12,671.3	0.076
Ln(sales per worker)	4.4	2.8	1.5	0.000
Distance to seaport	106,044	114,070	8,025	0.145
Distance to international airport	92,742	98,557	5,815	0.302
Number of communes	181	1,790		

Appendix 5 Parallel trend check among communes located in treated districts

Treated district selection	IZ or IC		IZ only		IC only	
VARIABLES	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)
	(1)	(2)	(3)	(4)	(5)	(6)
IZ commune × year 2000	–0.4715*	–0.0574	–0.5900*	–0.1101		
	(0.2584)	(0.0581)	(0.3281)	(0.0731)		
IZ commune × year 2001	–0.3474	–0.2292**	–0.3198	–0.2725**		
	(0.4561)	(0.1153)	(0.5448)	(0.1346)		
IZ commune × year 2003	0.3998*	0.1390**	0.6243*	0.1314*		
	(0.2263)	(0.0543)	(0.3353)	(0.0741)		
IZ commune × year 2004	0.6528**	0.2481***	0.7344*	0.2461**		
	(0.3128)	(0.0713)	(0.4409)	(0.0964)		
IZ commune × year 2005	0.8353**	0.3308***	0.9178**	0.3182***		
	(0.3342)	(0.0852)	(0.4620)	(0.1164)		
IZ commune × year 2006	0.9126**	0.3302***	1.0024**	0.3212**		
	(0.4003)	(0.0966)	(0.4970)	(0.1271)		
IZ commune × year 2007	1.4528***	0.5045***	1.5790***	0.4937***		
	(0.3913)	(0.1055)	(0.5058)	(0.1415)		
IC commune × year 2000	–0.7240***	–0.1375***			–0.9201***	–0.1361***
	(0.2037)	(0.0431)			(0.2298)	(0.0465)
IC commune × year 2001	–0.5413*	–0.0839			–0.5258	–0.0684
	(0.3103)	(0.0670)			(0.3520)	(0.0731)
IC commune × year 2003	0.2074	0.0086			0.2296	0.0317
	(0.1523)	(0.0322)			(0.1697)	(0.0358)
IC commune × year 2004	0.4394***	0.0991***			0.5695***	0.1347***
	(0.1658)	(0.0372)			(0.1921)	(0.0407)
IC commune × year 2005	0.3271*	0.1196***			0.4700**	0.1710***
	(0.1855)	(0.0450)			(0.2146)	(0.0496)
IC commune × year 2006	0.3329*	0.1778***			0.4336**	0.2277***
	(0.1855)	(0.0496)			(0.2095)	(0.0541)
IC commune × year 2007	0.6735***	0.2314***			0.8628***	0.3035***
	(0.2113)	(0.0596)			(0.2317)	(0.0635)
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,768	15,768	3,008	3,008	11,024	11,024
R–squared	0.359	0.467	0.368	0.422	0.346	0.464
Number of communes	1,971	1,971	376	376	1,378	1,378

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1).

Appendix 6 Effects of IZs/ICs by ZID ownership on local businesses located in the treated commune but outside the IZ/IC border, by treated district selection

Treated district selection	IZ		IC		IZ only		IC only	
VARIABLES	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IZ × treated	0.8607**	0.3789***	0.3845	0.3000*	1.1915**	0.4521***		
	(0.3439)	(0.1167)	(0.3301)	(0.1695)	(0.5224)	(0.1592)		
IC × treated	–0.0527	0.0538	0.6137***	0.2154***			0.7611***	0.2565***
	(0.4629)	(0.1483)	(0.1963)	(0.0665)			(0.2091)	(0.0722)
IZ × under	0.4848	0.1222	0.9345*	0.0979	0.3398	0.1403		
	(0.3430)	(0.0922)	(0.5008)	(0.1051)	(0.4209)	(0.1232)		
IC × under	–0.2121	–0.0715	0.4656***	0.0878*			0.6339***	0.1287***
	(0.3528)	(0.1036)	(0.1739)	(0.0460)			(0.1882)	(0.0489)
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,744	4,744	12,760	12,760	3,008	3,008	11,024	11,024
R–squared	0.387	0.464	0.352	0.472	0.358	0.399	0.339	0.450
Number of communes	593	593	1,595	1,595	376	376	1,378	1,378

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1).

Appendix 7 Effects of IZs/ICs on local businesses located in the treated commune but outside IZ/IC border, by treated district selection

Treated district selection	IZ		IC		IZ only		IC only	
VARIABLES	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IZ × established × POE	0.8062**	0.2886**	0.5869**	0.1477	0.9085	0.3617*		
	(0.4030)	(0.1376)	(0.2856)	(0.1837)	(0.5805)	(0.1845)		
IZ × established × SOE	0.3574	0.2186**	–0.1597	0.1970	0.5273	0.2259*		
	(0.3707)	(0.1103)	(0.8250)	(0.2427)	(0.4108)	(0.1248)		
IZ × established × FOE	0.7604	0.6803***	1.1519**	0.9282***	0.4365	0.2029*		
	(0.4962)	(0.1248)	(0.5408)	(0.1745)	(0.3835)	(0.1113)		
IZ × established × governmental agency	1.0139*	0.1100	0.9812*	0.1103	0.9563	0.0882		
	(0.5324)	(0.1297)	(0.5614)	(0.1925)	(1.0585)	(0.0914)		
IC × established × POE	–0.7462	–0.2250	0.1620	0.0682			0.6711	0.2210*
	(0.6423)	(0.1638)	(0.4727)	(0.1119)			(0.5362)	(0.1197)
IC × established × SOE	–0.8834*	–0.1273*	0.3638	0.0485			1.3369*	0.1638
	(0.5319)	(0.0751)	(0.5250)	(0.1213)			(0.6934)	(0.1943)
IC × established × FOE	0.8692*	0.1000	0.5469	0.0001			0.7951**	0.0666
	(0.5159)	(0.1818)	(0.3344)	(0.0971)			(0.3400)	(0.0942)
IC × established × governmental agency	0.2399	0.1572	0.5144***	0.1665***			0.5666***	0.1753***
	(0.4107)	(0.1467)	(0.1721)	(0.0541)			(0.1860)	(0.0582)
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,744	4,744	12,760	12,760	3,008	3,008	11,024	11,024
R–squared	0.388	0.464	0.352	0.472	0.356	0.397	0.339	0.449
Number of communes	593	593	1,595	1,595	376	376	1,378	1,378

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1).

Appendix 8 Descriptive statistics of sample containing treated districts and all non–treated districts

Variable	Mean	Std. Dev.	Min	Max
Ln(L)	2.176	2.987	0	12.227
Ln(Firms)	1.292	0.890	0.881	7.151
Ln(Net L)	2.164	2.980	0	12.227
Ln(Net firms)	1.289	0.889	0	7.151
Year	2003.500	2.291	2000	2007
IZ × treated	0.002	0.043	0	1
IC × treated	0.003	0.055	0	1
IZ × under	0.001	0.035	0	1
IC × under	0.003	0.056	0	1
IZ × established × POE	0.002	0.043	0	1
IZ × established × SOE	0.001	0.030	0	1
IZ × established × FOE	0.000	0.012	0	1
IZ × established × governmental agency	0.000	0.015	0	1
IC × established × POE	0.001	0.027	0	1
IC × established × SOE	0.000	0.020	0	1
IC × established × FOE	0.000	0.009	0	1
IC × established × governmental agency	0.005	0.069	0	1
IZ × treated district	0.025	0.155	0	1
IC × treated district	0.076	0.265	0	1
IZ–IC × treated district	0.008	0.091	0	1
IC × treated district × POE	–	–	–	–
IC × treated district × FOE	–	–	–	–
IC × treated district × SOE	–	–	–	–
IC × treated district × governmental agency	–	–	–	–
IZ × treated district × POE	0.018	0.132	0	1
IZ × treated district × FOE	0.000	0.016	0	1
IZ × treated district × SOE	0.006	0.078	0	1
IZ × treated district × governmental agency	0.001	0.032	0	1
IZ–IC × treated district × POE	0.005	0.070	0	1
IZ–IC × treated district × FOE	0.001	0.038	0	1
IZ–IC × treated district × SOE	0.003	0.050	0	1
IZ–IC × treated district × governmental agency	0.001	0.035	0	1
Number of identical districts	533			
Number of identical communes	7,998			

Appendix 9 Parallel trend check among communes located in treated districts and other control communes in non–treated districts

Treated district selection	IZ or IC		IZ only		IC only	
VARIABLES	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)
	(1)	(2)	(3)	(4)	(5)	(6)
IZ commune × year 2000	–0.6804***	–0.0716	–0.7043**	–0.1219*		
	(0.2251)	(0.0496)	(0.3061)	(0.0656)		
IZ commune × year 2001	–0.4266	–0.2424**	–0.6027	–0.2333		
	(0.4325)	(0.1161)	(0.5677)	(0.1450)		
IZ commune × year 2003	0.3901*	0.1448***	0.5882*	0.1343*		
	(0.2035)	(0.0494)	(0.3158)	(0.0698)		
IZ commune × year 2004	0.6036**	0.2460***	0.6689*	0.2402**		
	(0.2892)	(0.0698)	(0.3991)	(0.0942)		
IZ commune × year 2005	0.7542**	0.3090***	0.9031*	0.3184***		
	(0.3290)	(0.0847)	(0.4595)	(0.1151)		
IZ commune × year 2006	0.7847**	0.3209***	0.8994*	0.3286***		
	(0.3947)	(0.0979)	(0.4856)	(0.1257)		
IZ commune × year 2007	1.1854***	0.4719***	1.2801***	0.4679***		
	(0.3979)	(0.1092)	(0.4878)	(0.1390)		
IC commune × year 2000	–0.7893***	–0.1394***			–0.8743***	–0.1256***
	(0.1891)	(0.0410)			(0.2248)	(0.0464)
IC commune × year 2001	–0.7469***	–0.1213**			–0.4681	–0.0256
	(0.2881)	(0.0612)			(0.3400)	(0.0707)
IC commune × year 2003	0.1440	0.0099			0.1867	0.0267
	(0.1393)	(0.0294)			(0.1563)	(0.0331)
IC commune × year 2004	0.3251**	0.0884**			0.5089***	0.1270***
	(0.1515)	(0.0344)			(0.1828)	(0.0377)
IC commune × year 2005	0.1785	0.1056**			0.4099*	0.1626***
	(0.1759)	(0.0430)			(0.2091)	(0.0474)
IC commune × year 2006	0.1174	0.1553***			0.3236	0.2155***
	(0.1764)	(0.0485)			(0.2035)	(0.0543)
IC commune × year 2007	0.4253**	0.1951***			0.7079***	0.2725***
	(0.1989)	(0.0566)			(0.2250)	(0.0611)
IZ/IC district × year 2003	0.1929***	0.0163	0.0885	–0.0583*	0.2140**	0.0040
	(0.0646)	(0.0122)	(0.1627)	(0.0321)	(0.0924)	(0.0137)
IZ/IC district × year 2004	0.3292***	0.0300***	0.6388***	0.0489	0.2896***	–0.0041
	(0.0556)	(0.0105)	(0.1621)	(0.0348)	(0.0750)	(0.0120)
IZ/IC district × year 2005	0.4513***	0.0503***	0.8858***	0.1086***	0.3966***	0.0094
	(0.0534)	(0.0104)	(0.1798)	(0.0372)	(0.0844)	(0.0157)
IZ/IC district × year 2006	0.6110***	0.0883***	1.4288***	0.2184***	0.9557***	0.0827***
	(0.0552)	(0.0118)	(0.2147)	(0.0500)	(0.1097)	(0.0206)
IZ/IC district × year 2007	0.8453***	0.1339***	2.0782***	0.3306***	1.5311***	0.2058***
	(0.0572)	(0.0133)	(0.2304)	(0.0569)	(0.1186)	(0.0246)
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63,984	63,984	3,008	3,008	11,024	11,024
R–squared	0.184	0.287	0.234	0.283	0.255	0.349
Number of communes	7,998	7,998	376	376	1,378	1,378

Note: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1).

Appendix 10 Effects of IZs/ICs on formal businesses located in the treated communes compared with those in non–treated districts

Treated district selection	IZ or IC		IZ		IC		IZ only		IC only	
VARIABLES	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IZ × treated	1.5190***	0.6772***	1.5134***	0.6744***	1.5726**	0.7239***	1.4830***	0.6536***		
	(0.3997)	(0.1138)	(0.3958)	(0.1130)	(0.6511)	(0.1569)	(0.4943)	(0.1511)		
IC × treated	0.7500***	0.2804***	–0.0490	0.2335	0.7418***	0.2797***			0.8683***	0.2918***
	(0.1815)	(0.0584)	(0.4162)	(0.1695)	(0.1817)	(0.0584)			(0.1938)	(0.0622)
IZ × under	0.5171	0.1274	0.5274	0.1311	0.7770	0.0564	0.4053	0.1644		
	(0.3958)	(0.0869)	(0.3957)	(0.0867)	(0.5805)	(0.0957)	(0.5084)	(0.1187)		
IC × under	0.2884*	0.0739*	–0.2745	–0.1017	0.2876*	0.0757*			0.4077**	0.1129**
	(0.1667)	(0.0443)	(0.3178)	(0.1132)	(0.1670)	(0.0443)			(0.1893)	(0.0465)
IZ × treated district	0.4984***	0.0460*	0.4726***	0.0440*	0.7038***	0.1264***	0.4123***	0.0237		
	(0.0956)	(0.0238)	(0.0982)	(0.0246)	(0.1534)	(0.0333)	(0.1161)	(0.0291)		
IC × treated district	0.5639***	0.0783***	0.5456***	0.1170***	0.5495***	0.0761***			0.5431***	0.0735***
	(0.0497)	(0.0102)	(0.1404)	(0.0419)	(0.0499)	(0.0103)			(0.0522)	(0.0106)
IZ–IC × treated district	0.9558***	0.2034***	1.0072***	0.2217***	0.9501***	0.2085***				
	(0.1183)	(0.0318)	(0.1270)	(0.0346)	(0.1258)	(0.0334)				
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63,984	63,984	52,960	52,960	60,976	60,976	51,224	51,224	59,240	59,240
R–squared	0.182	0.287	0.176	0.279	0.183	0.292	0.169	0.263	0.178	0.279
Number of communes	7,998	7,998	6,620	6,620	7,622	7,622	6,403	6,403	7,405	7,405

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1).

Appendix 11 Effects of IZs/ICs by ZID ownership on formal businesses located in the treated communes compared with those in non–treated districts, by treated district selection

Treated district selection	IZ or IC		IZ		IC		IZ only		IC only	
VARIABLES	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)	Ln(L)	Ln(Firms)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IZ × established × POE	1.3847***	0.5167***	1.3569***	0.5135***	1.6621**	0.5323***	1.2436**	0.5066***		
	(0.4744)	(0.1370)	(0.4722)	(0.1365)	(0.8188)	(0.1913)	(0.5732)	(0.1793)		
IZ × established× SOE	0.5604	0.3966***	0.6148	0.4000***	0.0991	0.3241**	0.7658	0.4258***		
	(0.4022)	(0.1121)	(0.3942)	(0.1130)	(0.5231)	(0.1650)	(0.5065)	(0.1415)		
IZ × established × FOE	0.3618	0.5882***	0.3161	0.5656***	0.6276	0.7633***	0.6764**	0.2865***		
	(0.9941)	(0.2170)	(1.0128)	(0.2144)	(1.1498)	(0.1378)	(0.3276)	(0.1024)		
IZ × established × governmental agency	0.6805	0.0324	0.7405	0.0310	0.5936	0.0264	0.5421	–0.0533		
	(0.5802)	(0.1182)	(0.5509)	(0.1190)	(0.6756)	(0.1514)	(0.9588)	(0.0886)		
IC × established × POE	0.5626*	0.1332	–0.1632	–0.1366	0.5672*	0.1358			0.8641**	0.2488*
	(0.3282)	(0.1096)	(0.6110)	(0.1068)	(0.3292)	(0.1099)			(0.3549)	(0.1355)
IC × established × SOE	0.3886	0.0681	–0.6970	–0.0070	0.4317	0.0777			1.1206***	0.1183
	(0.4300)	(0.1152)	(0.4352)	(0.0988)	(0.3972)	(0.1176)			(0.4118)	(0.1826)
IC × established × FOE	0.0357	–0.1013	–0.0720	–0.3665**	0.0343	–0.1062			0.8731***	0.1928***
	(0.5945)	(0.1908)	(0.3948)	(0.1792)	(0.5989)	(0.1948)			(0.1820)	(0.0513)
IC × established × governmental agency	0.8382***	0.2457***	0.2127	0.2895	0.8233***	0.2444***			0.8919***	0.2420***
	(0.1690)	(0.0494)	(0.4025)	(0.1779)	(0.1694)	(0.0494)			(0.1811)	(0.0511)
IZ × treated district × POE	0.6300***	0.1111***	0.6573***	0.1173***	0.5503***	0.1256***	0.6654***	0.1144***		
	(0.1232)	(0.0292)	(0.1278)	(0.0307)	(0.1682)	(0.0404)	(0.1593)	(0.0380)		
IZ × treated district × SOE	0.2657	–0.0782*	0.3226*	–0.0646	1.0072*	–0.0336	0.2520	–0.0680		
	(0.1675)	(0.0431)	(0.1663)	(0.0430)	(0.5530)	(0.0418)	(0.1763)	(0.0475)		
IZ × treated district × FOE	0.8864	0.1193	0.8842	0.1245	1.1642*	0.1988*	–1.3140***	–0.3726***		
	(0.6065)	(0.1139)	(0.6059)	(0.1135)	(0.6270)	(0.1107)	(0.3268)	(0.1022)		
IZ × treated district × governmental agency	–0.8594***	–0.2876***	–0.8070***	–0.2721***	0.0365	0.0070	–1.0038***	–0.3379***		
	(0.1721)	(0.0703)	(0.1719)	(0.0696)	(0.2752)	(0.0620)	(0.1961)	(0.0833)		

Appendix 11 (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IZ–IC × treated district × POE	0.6995***	0.1860***	0.8070***	0.2031***	0.6484***	0.1823***				
	(0.1259)	(0.0371)	(0.1277)	(0.0374)	(0.1318)	(0.0381)				
IZ–IC × treated district × SOE	0.9258***	0.1064**	0.9907***	0.1143**	0.9478***	0.1077**				
	(0.2272)	(0.0472)	(0.2289)	(0.0477)	(0.2367)	(0.0480)				
IZ–IC × treated district × FOE	0.6326	0.1945*	0.6285	0.2143*	0.6581	0.1940				
	(0.4785)	(0.1162)	(0.4910)	(0.1180)	(0.4843)	(0.1183)				
IZ–IC × treated district × governmental agency	0.0339	0.0820	0.1606	0.0924	0.1520	0.1217				
	(0.1800)	(0.1025)	(0.1770)	(0.1039)	(0.1997)	(0.1095)				
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63,984	63,984	52,960	52,960	60,976	60,976	51,224	51,224	59,240	59,240
R–squared	0.179	0.286	0.177	0.280	0.179	0.291	0.170	0.264	0.173	0.277
Number of communes	7,998	7,998	6,620	6,620	7,622	7,622	6,403	6,403	7,405	7,405

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1).

Appendix 12 Spillover effects of IZs/ICs on formal businesses located in the treated communes but outside the IZ/IC compared with those in non–treated districts, by treated district selection

Treated district selection	IZ		IC		IZ only		IC only	
VARIABLES	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IZ × treated	0.9100**	0.4432***	0.6573*	0.4341***	1.0452**	0.4520***		
	(0.3601)	(0.1206)	(0.3563)	(0.1624)	(0.5252)	(0.1648)		
IC × treated	–0.0905	0.1696	0.5385***	0.2263***			0.6403***	0.2398***
	(0.4437)	(0.1175)	(0.1895)	(0.0617)			(0.2045)	(0.0687)
IZ × under	0.2971	0.0710	0.7869	0.0607	0.0783	0.0769		
	(0.3440)	(0.0938)	(0.5392)	(0.0971)	(0.4209)	(0.1297)		
IC × under	–0.1057	–0.0551	0.3211*	0.0832*			0.4171**	0.1134**
	(0.3521)	(0.1135)	(0.1665)	(0.0427)			(0.1866)	(0.0446)
IZ × treated district	0.3258***	0.0001	0.4492***	0.0473	0.2824**	–0.0122		
	(0.0948)	(0.0240)	(0.1525)	(0.0401)	(0.1113)	(0.0280)		
IC × treated district	0.3502**	0.0601	0.3872***	0.0320***			0.3811***	0.0296***
	(0.1398)	(0.0395)	(0.0495)	(0.0104)			(0.0516)	(0.0107)
IZ–IC × treated district	0.7768***	0.1580***	0.7158***	0.1425***				
	(0.1323)	(0.0360)	(0.1305)	(0.0341)				
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	52,960	52,960	60,976	60,976	51,224	51,224	59,240	59,240
R–squared	0.192	0.310	0.199	0.323	0.187	0.296	0.195	0.311
Number of communes	6,620	6,620	7,622	7,622	6,403	6,403	7,405	7,405

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1).

Appendix 13 Spillover effects of IZs/ICs by ZID ownership on formal businesses located in the treated communes but outside the IZ/IC border compared with those in non–treated districts, by treated district selection

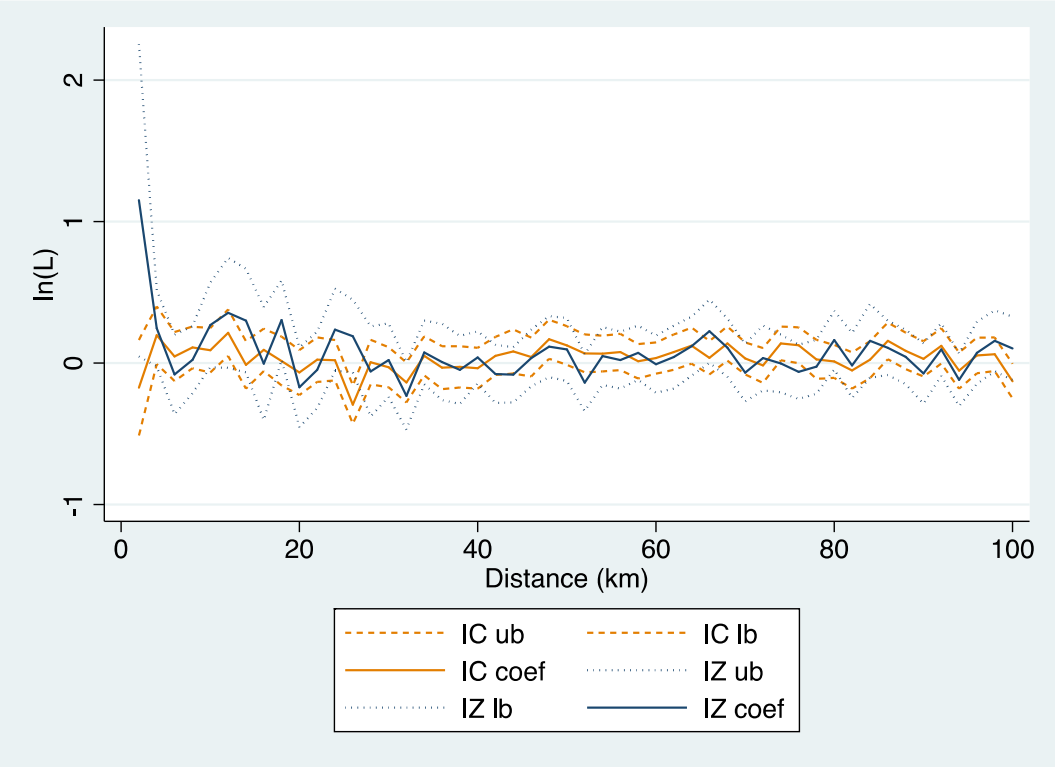
Treated district selection	IZ		IC		IZ only		IC only	
VARIABLES	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)	Ln(Net L)	Ln(Net firms)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IZ × established × POE	0.8661*	0.3319**	0.9377**	0.2778	0.8518	0.3548*		
	(0.4453)	(0.1545)	(0.3709)	(0.1707)	(0.6345)	(0.2133)		
IZ × established × SOE	0.1262	0.2109*	–0.5414	0.2255	0.3137	0.2065*		
	(0.3346)	(0.1109)	(0.7293)	(0.2484)	(0.3937)	(0.1217)		
IZ × established × FOE	0.2059	0.6251**	0.4972	0.8847***	0.4867	0.2027*		
	(0.6445)	(0.3023)	(0.4837)	(0.2724)	(0.3625)	(0.1219)		
IZ × established × governmental agency	0.7945	0.0715	0.7287	0.0025	0.6038	0.1427*		
	(0.4837)	(0.1254)	(0.6035)	(0.1892)	(0.7608)	(0.0829)		
IC × established × POE	–0.1758	–0.2313*	0.2816	–0.0057			0.4507	0.0894
	(0.5741)	(0.1332)	(0.2920)	(0.0933)			(0.3243)	(0.1059)
IC × established × SOE	–0.9059**	0.0162	0.2391	0.0259			0.9591**	0.0375
	(0.4406)	(0.1298)	(0.4426)	(0.1065)			(0.4873)	(0.1514)
IC × established × FOE	0.6656	–0.0114	0.8325*	0.1035*			1.4511***	0.1538***
	(0.4233)	(0.1554)	(0.4614)	(0.0619)			(0.1857)	(0.0556)
IC × established × governmental agency	0.1383	0.2852*	0.5809***	0.2029***			0.6255***	0.1954***
	(0.4255)	(0.1578)	(0.1739)	(0.0528)			(0.1863)	(0.0558)

Appendix 13 (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IZ × treated district × POE	0.5234***	0.0501	0.2665	0.0885	0.5528***	0.0429		
	(0.1220)	(0.0318)	(0.1746)	(0.0552)	(0.1500)	(0.0377)		
IZ × treated district × SOE	0.1839	–0.0465	0.9667*	–0.0798	0.0979	–0.0428		
	(0.1664)	(0.0417)	(0.5438)	(0.0494)	(0.1770)	(0.0458)		
IZ × treated district × FOE	0.8422	0.1537	1.0813*	0.2443**	–1.1460***	–0.3819***		
	(0.5966)	(0.1259)	(0.6314)	(0.1180)	(0.3582)	(0.1201)		
IZ × treated district × governmental agency	–0.8101***	–0.2839***	–0.2155	–0.0306	–0.9629***	–0.3481***		
	(0.1729)	(0.0602)	(0.2871)	(0.0583)	(0.2028)	(0.0710)		
IZ–IC × treated district × POE	0.5327***	0.1690***	0.3368**	0.1722***				
	(0.1386)	(0.0443)	(0.1452)	(0.0460)				
IZ–IC × treated district × SOE	0.8288***	0.0692	0.7626***	0.0607				
	(0.2373)	(0.0538)	(0.2471)	(0.0535)				
IZ–IC × treated district × FOE	0.5262	0.3104**	0.5645	0.2831**				
	(0.4841)	(0.1256)	(0.4766)	(0.1257)				
IZ–IC × treated district × governmental agency	–0.1016	0.0372	–0.1722	0.0807				
	(0.1964)	(0.1068)	(0.2226)	(0.1121)				
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	52,960	52,960	60,976	60,976	51,224	51,224	59,240	59,240
R–squared	0.148	0.265	0.148	0.269	0.142	0.252	0.143	0.258
Number of communes	6,620	6,620	7,622	7,622	6,403	6,403	7,405	7,405

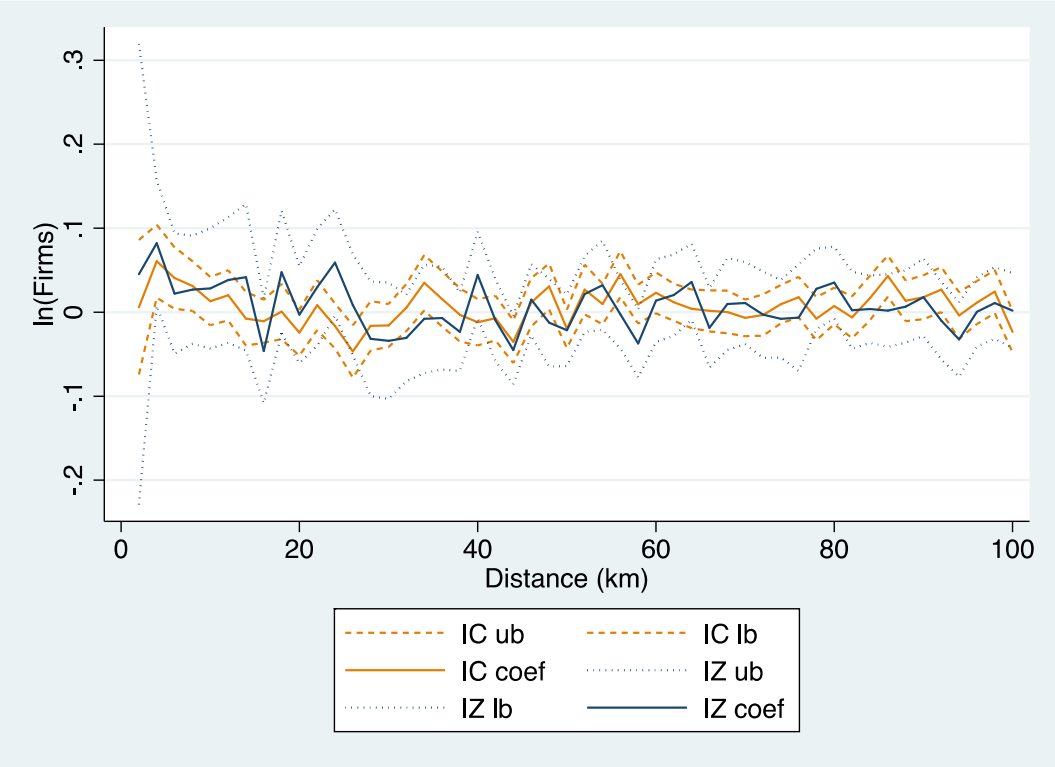
Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1).

Appendix 14 Spatial spillover effect of IZs/ICs on non–treated communes' formal employment among treated districts



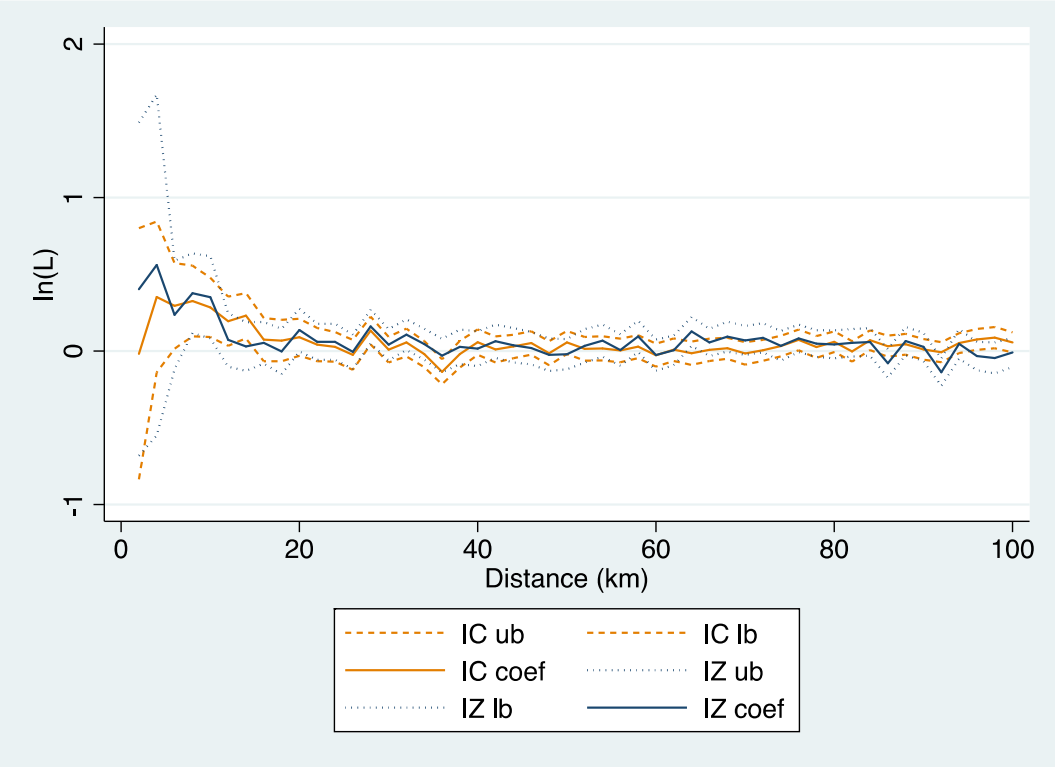
Note: lb (ub) is lower (upper) bound at 2–SD from the coefficient value.

Appendix 15 Spatial spillover effect of IZs/ICs on non–treated communes' number of firms among treated districts



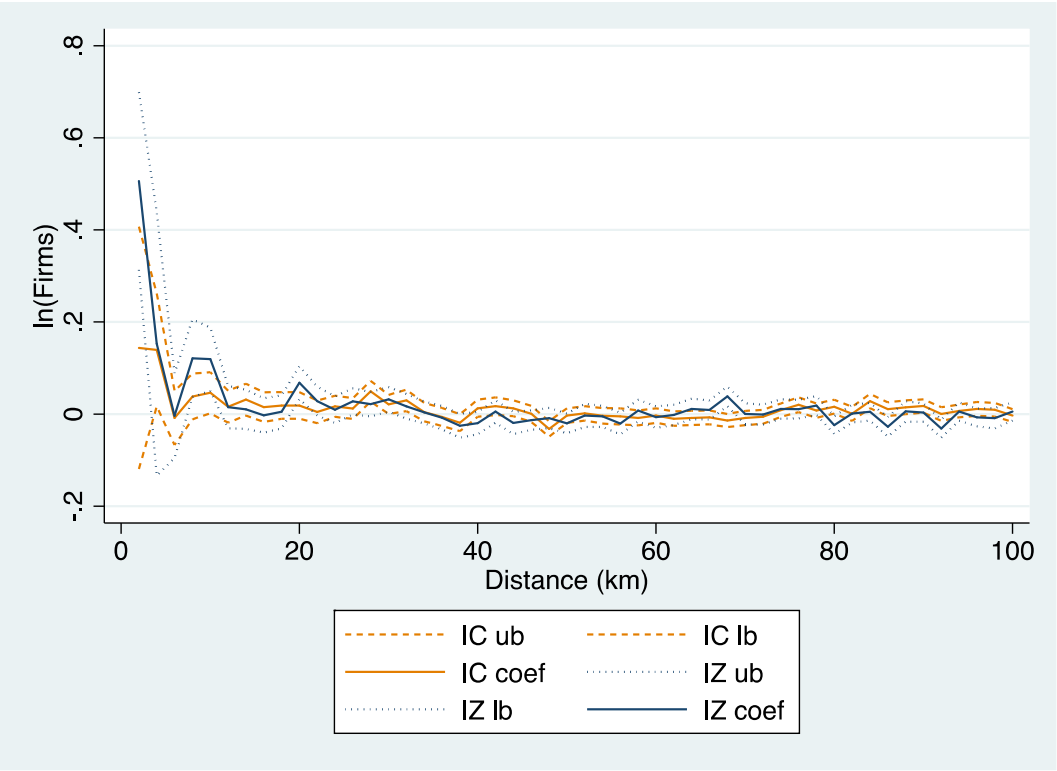
Note: lb (ub) is lower (upper) bound at 2–SD from the coefficient value.

Appendix 16 Spatial spillover effect of IZs/ICs on non–treated communes' formal employment among non–treated districts



Note: lb (ub) is lower (upper) bound at 2–SD from the coefficient value.

Appendix 17 Spatial spillover effect of IZs/ICs on non–treated communes' number of firms among non–treated districts



Note: lb (ub) is lower (upper) bound at 2–SD from the coefficient value.

Table 1 IZs and ICs as of July 2007

	IZ			IC		
	Mean	Min	Max	Mean	Min	Max
Year of establishment	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	2,111.29
Area for lease (ha)	194.34	0	2,816.26	33.40	0	2,111.29
	IZ			IC		
Status						
In operation	144			189		
Under construction	35			76		
Classification						
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		
SOE	62			23		
FOE	25			3		
Government agency	18			196		
Total	179			265		

Notes:

ZID can be structured as a partnership.
Exclusions:

- All IZs/ICs that were established but not yet under construction in July 2007, based on the Vietnamese Establishment Census Survey.
- Eighteen units appeared with the word “industrial park” in their names but were not established by either the central or provincial governments.
- One unit that ceased operation.

Table 2 Effects of IZs/ECs on the local formal businesses located in the treated communes

VARIABLES	Ln(L) (1)	Ln(L) (2)	Ln(Firms) (3)	Ln(Firms) (4)	Ln(L) (5)	Ln(L) (6)	Ln(Firms) (7)	Ln(Firms) (8)
IZ × treated	1.5587***	0.9104***	0.6522***	0.2957***				
	(0.3631)	(0.2189)	(0.1039)	(0.0475)				
IC × treated	0.8709***	0.7055***	0.2968***	0.1626***				
	(0.1913)	(0.1259)	(0.0637)	(0.0382)				
IZ × under	0.7921**	0.6278***	0.1908**	0.1030**				
	(0.3738)	(0.2263)	(0.0818)	(0.0438)				
IC × under	0.4985***	0.3608***	0.1059**	0.0463				
	(0.1781)	(0.1272)	(0.0464)	(0.0370)				
IZ × established × POE					1.4341***	0.8460***	0.5154***	0.2337***
					(0.4093)	(0.2064)	(0.1198)	(0.0480)
IZ × established × SOE					0.8263**	0.6476***	0.3829***	0.2030***
					(0.3738)	(0.1845)	(0.1190)	(0.0540)
IZ × established × FOE					0.8941	0.8141	0.7221***	0.2733**
					(0.6804)	(0.6543)	(0.1132)	(0.1113)
IZ × established × governmental agency					0.9280*	0.4786	0.0788	0.0301
					(0.5428)	(0.3856)	(0.1275)	(0.0654)
IC × established × POE					0.1053	0.5052	0.1164	0.1414*
					(0.4647)	(0.3279)	(0.1291)	(0.0753)
IC × established × SOE					0.2070	0.3445	0.0155	0.0213
					(0.5474)	(0.3001)	(0.1205)	(0.0715)
IC × established × FOE					0.4594	–0.2373	–0.0276	–0.2414***
					(0.2895)	(0.2493)	(0.0859)	(0.0600)
IC × established × governmental agency					0.6964***	0.5213***	0.2254***	0.1134***
					(0.1735)	(0.1091)	(0.0544)	(0.0332)
Previous year outcome	No	Yes	No	Yes	No	Yes	No	Yes
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	No	Yes	No	Yes	No	Yes	No
Observations	15,768	13,797	15,768	13,797	15,768	13,797	15,768	13,797
R–squared	0.357	0.724	0.467	0.845	0.356	0.724	0.465	0.845
Number of communes	1,971	1,971	1,971	1,971	1,971	1,971	1,971	1,971

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1). We used standard OLS for (3), (4), (7), and (8).

Table 3 Effects of IZs/ICs on the local formal businesses located in the treated communes but outside the IZ/IC border

VARIABLES	Ln(Net L)	Ln(Net L)	Ln(Net firms)	Ln(Net firms)	Ln(Net L)	Ln(Net L)	Ln(Net firms)	Ln(Net firms)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IZ × treated	0.8858***	0.5937***	0.3975***	0.2069***				
	(0.3399)	(0.1975)	(0.1160)	(0.0562)				
IC × treated	0.6137***	0.4939***	0.2169***	0.1240***				
	(0.1959)	(0.1376)	(0.0663)	(0.0433)				
IZ × under	0.5011	0.4138*	0.1224	0.0702				
	(0.3370)	(0.2167)	(0.0924)	(0.0532)				
IC × under	0.4650***	0.3628***	0.0881*	0.0360				
	(0.1733)	(0.1272)	(0.0459)	(0.0367)				
IZ × established × POE					0.8143**	0.4883**	0.2996**	0.1465**
					(0.3921)	(0.2041)	(0.1364)	(0.0589)
IZ × established × SOE					0.3629	0.4634**	0.2254**	0.1605***
					(0.3758)	(0.2094)	(0.1106)	(0.0590)
IZ × established × FOE					0.9865*	0.8629	0.7708***	0.2934***
					(0.5186)	(0.5494)	(0.1322)	(0.0931)
IZ × established × governmental agency					0.9533*	0.4919	0.0878	0.0335
					(0.5376)	(0.3803)	(0.1246)	(0.0638)
IC × established × POE					0.1687	0.5274	0.0652	0.1259*
					(0.4710)	(0.3319)	(0.1113)	(0.0727)
IC × established × SOE					0.2860	0.3921	0.0384	0.0314
					(0.5229)	(0.2905)	(0.1142)	(0.0708)
IC × established × FOE					0.5879*	–0.1191	0.0225	–0.2125***
					(0.3137)	(0.2513)	(0.0884)	(0.0626)
IC × established × governmental agency					0.5146***	0.4005***	0.1687***	0.0843**
					(0.1752)	(0.1138)	(0.0548)	(0.0347)
Previous year outcome	No	Yes	No	Yes	No	Yes	No	Yes
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	No	Yes	No	Yes	No	Yes	No
Observations	15,768	13,797	15,768	13,797	15,768	13,797	15,768	13,797
R–squared	0.351	0.723	0.451	0.839	0.350	0.723	0.451	0.839
Number of communes	1,971	1,971	1,971	1,971	1,971	1,971	1,971	1,971

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1). We used standard OLS for (3), (4), (7), and (8).

Table 4 Spillover effect of IZs/ICs on local businesses located in the treated communes but outside the IZ/IC border compared with non–treated communes

VARIABLES	Ln(Net L)	Ln(Net L)	Ln(Net firms)	Ln(Net firms)
	(1)	(2)	(3)	(4)
IZ × treated	0.9775***	0.6486***	0.4611***	0.2409***
	(0.3614)	(0.1909)	(0.1193)	(0.0508)
IC × treated	0.5421***	0.4222***	0.2259***	0.1109***
	(0.1815)	(0.1208)	(0.0621)	(0.0366)
IZ × under	0.2540	0.2328	0.0571	0.0214
	(0.3560)	(0.2129)	(0.0936)	(0.0502)
IC × under	0.2755*	0.2292**	0.0694	0.0249
	(0.1630)	(0.1096)	(0.0442)	(0.0310)
IZ × treated district	0.4657***	0.1392***	0.0364	0.0092
	(0.0935)	(0.0486)	(0.0239)	(0.0114)
IC × treated district	0.5507***	0.1092***	0.0746***	0.0069
	(0.0497)	(0.0277)	(0.0103)	(0.0056)
IZ–IC × treated district	0.9741***	0.2941***	0.2079***	0.0856***
	(0.1193)	(0.0706)	(0.0321)	(0.0179)
Previous year outcome	No	Yes	No	Yes
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	No	Yes	No
Observations	63,984	55,986	63,984	55,986
R–squared	0.180	0.705	0.283	0.822
Number of communes	7,998	7,998	7,998	7,998

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1). We used standard OLS for (3), (4), (7), and (8).

Table 5 Spillover effect of IZs/ICs by ZID ownership on local businesses located in the treated communes but outside the IZ/IC border compared with non–treated communes

VARIABLES	Ln(Net L)	Ln(Net L)	Ln(Net firms)	Ln(Net firms)
	(1)	(2)	(3)	(4)
IZ × established × POE	0.8497*	0.4841**	0.3203**	0.1551***
	(0.4629)	(0.2110)	(0.1483)	(0.0581)
IZ × established × SOE	0.1460	0.4120**	0.2478***	0.1649***
	(0.3196)	(0.2012)	(0.0939)	(0.0570)
IZ × established × FOE	0.5445	0.5862	0.6532***	0.2253***
	(0.8977)	(0.6010)	(0.2311)	(0.0809)
IZ × established × governmental agency	0.7001	0.3077	0.0374	0.0113
	(0.5733)	(0.3221)	(0.1168)	(0.0559)
IC × established × POE	0.5248*	0.4861**	0.0856	0.0961*
	(0.2868)	(0.2205)	(0.0895)	(0.0518)
IC × established× SOE	0.4395	0.1938	0.0848	0.0477
	(0.4071)	(0.2726)	(0.1181)	(0.0845)
IC × established × FOE	0.1932	–0.2310	–0.0626	–0.2368***
	(0.6024)	(0.4015)	(0.1927)	(0.0768)
IC × established × governmental agency	0.6720***	0.3477***	0.2072***	0.0732***
	(0.1671)	(0.0978)	(0.0521)	(0.0284)
IZ.established × treated district × POE	0.5850***	0.1814***	0.0997***	0.0248*
	(0.1202)	(0.0594)	(0.0294)	(0.0131)
IZ.established × treated district × SOE	0.2791*	0.1048	–0.0774*	–0.0148
	(0.1666)	(0.0868)	(0.0431)	(0.0240)
IZ.established × treated district × FOE	0.8995	0.5024	0.1224	0.0196
	(0.6015)	(0.5539)	(0.1128)	(0.0867)
IZ.established × treated district × governmental agency	–0.8629***	–0.7277***	–0.2888***	–0.1890***
	(0.1719)	(0.1080)	(0.0704)	(0.0394)
IZ–IC.established × treated district × POE	0.7051***	0.3124***	0.1828***	0.1050***
	(0.1294)	(0.0850)	(0.0376)	(0.0222)
IZ–IC.established × treated district × SOE	0.9076***	0.4032***	0.1086**	0.0536
	(0.2263)	(0.1358)	(0.0478)	(0.0338)
IZ–IC.established × treated district × FOE	0.6769	–0.1989	0.2139*	–0.0046
	(0.4776)	(0.2492)	(0.1171)	(0.0542)
IZ–IC.established × treated district × governmental agency	0.0413	0.1463	0.0823	0.0864*
	(0.1807)	(0.1412)	(0.1016)	(0.0455)
Previous year outcome	No	Yes	No	Yes
Baseline 1–4 × year dummies	Yes	Yes	Yes	Yes
District × year dummies	Yes	Yes	Yes	Yes
Commune fixed effect	Yes	No	Yes	No
Observations	63,984	55,986	63,984	55,986
R–squared	0.177	0.705	0.283	0.822
Number of communes	7,998	7,998	7,998	7,998

Notes: Standard errors clustered at commune-level in brackets. (*** p<0.01, ** p<0.05, * p<0.1). We used standard OLS for (3), (4), (7), and (8).

Figure 1 Conceptual framework for treated and control communes

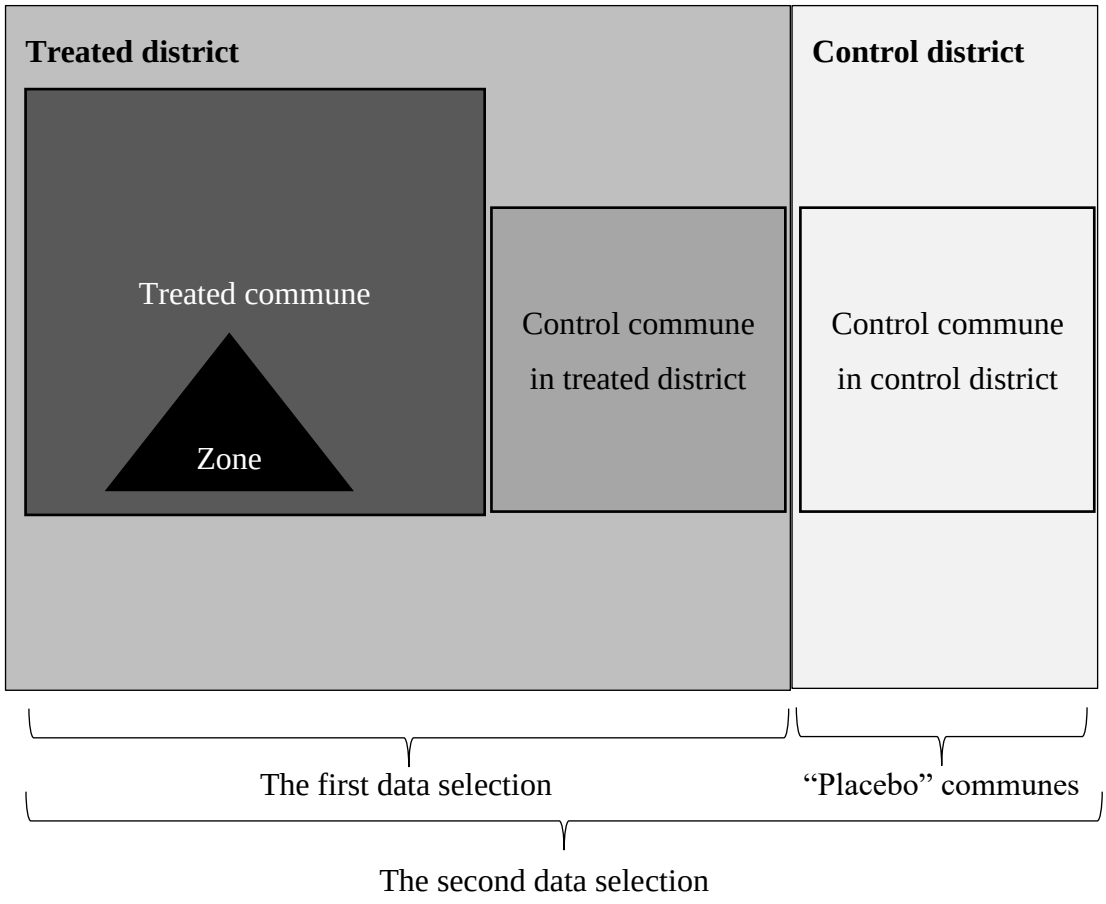
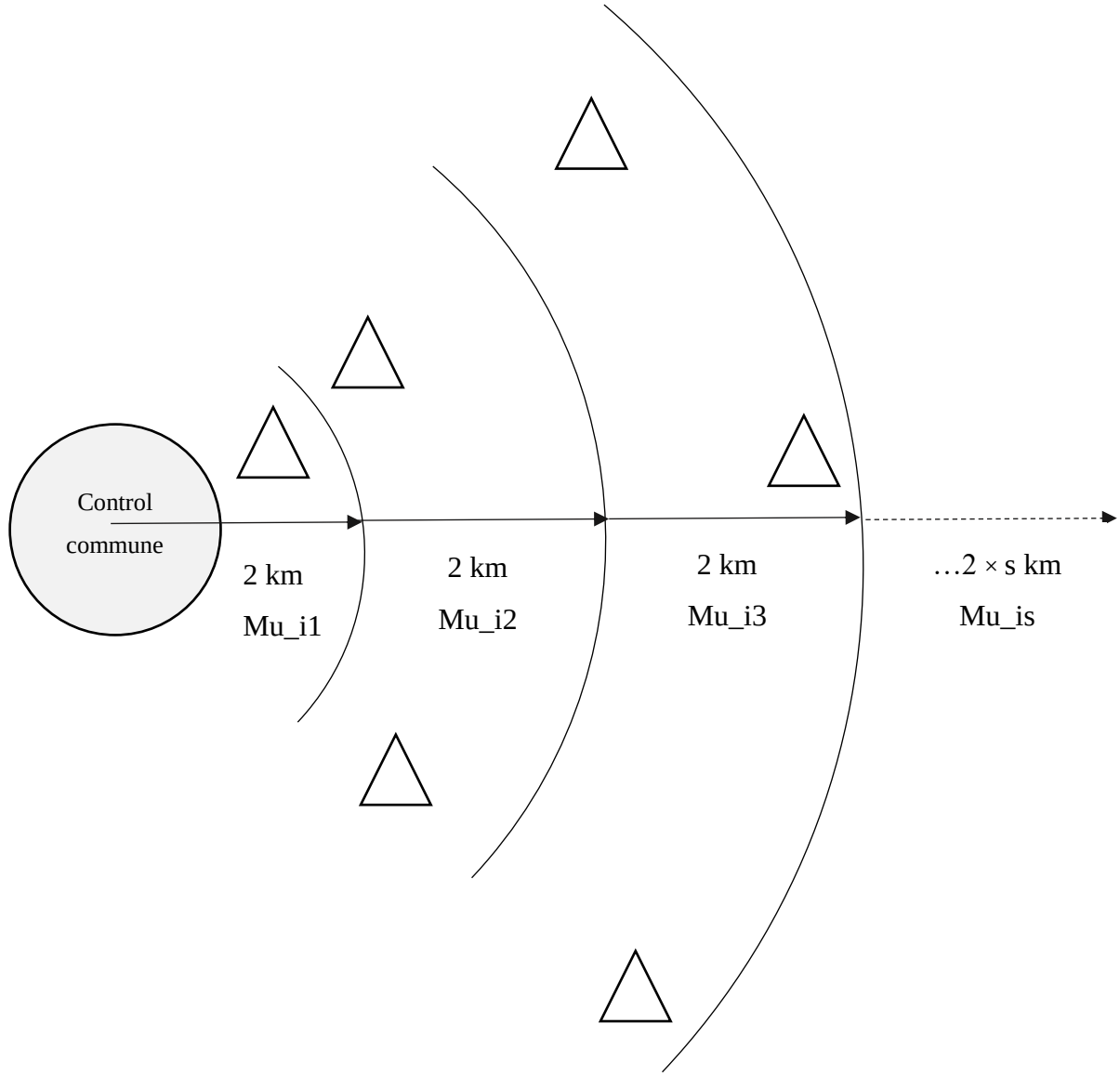


Figure 2 Conceptual framework of concentric ring analysis



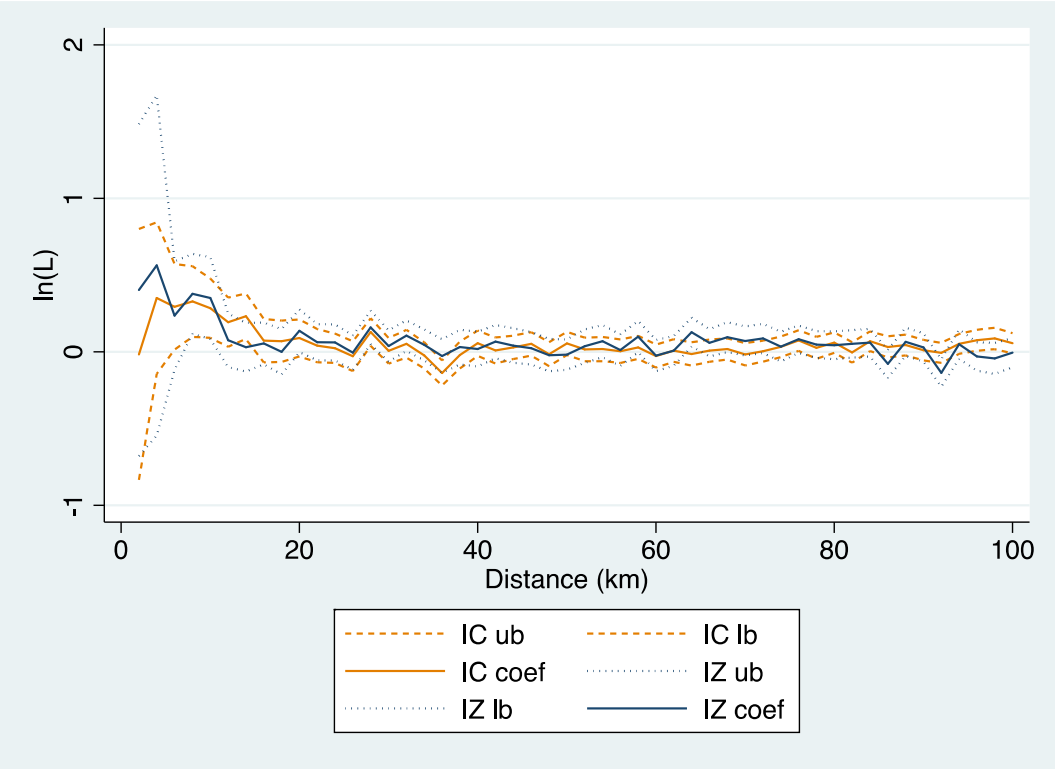
Notes: Mu_is is μ_{is} in equation (7) where i = [1, 3]

△ Center of treated commune having IZ/IC

Figure 3 Pre-trend checks

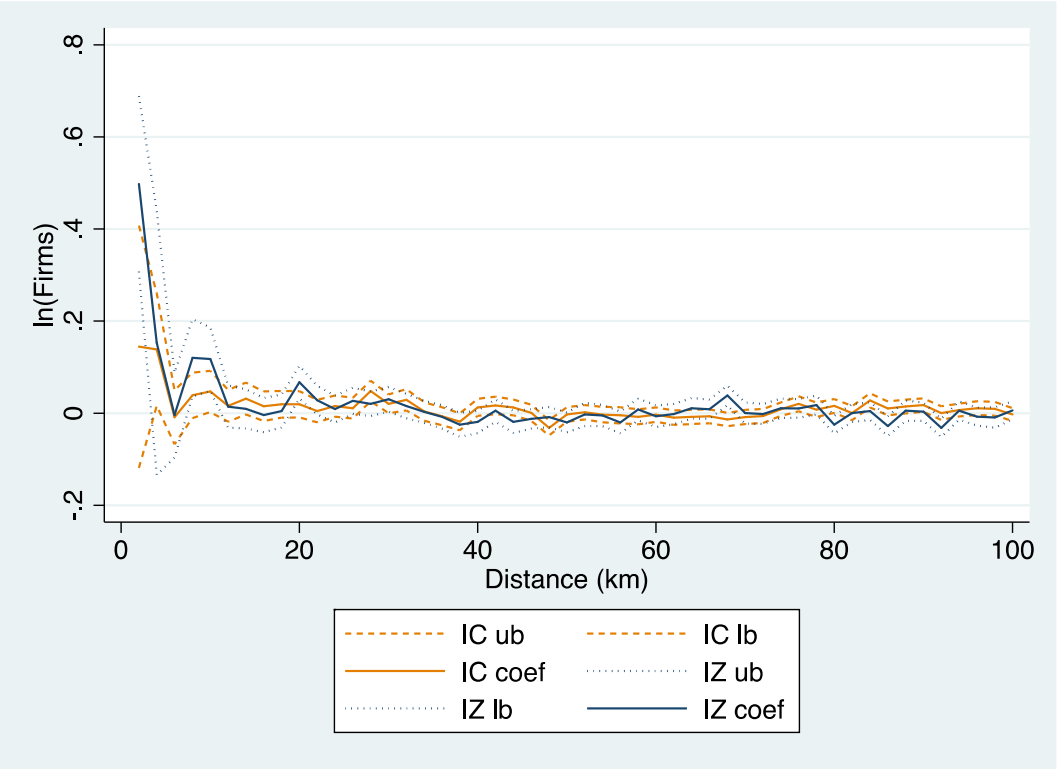
Note: Data were obtained from the corresponding coefficients in Columns (1) and (2) of Appendix 5.

Figure 4 Spatial spillover effects of IZs/ICs on non–treated communes' formal employment



Note: lb (ub) is lower (upper) bound at 2–SD from the coefficient value.

Figure 5 Spatial spillover effect of IZs/ICs on non–treated communes' number of firms



Note: lb (ub) is lower (upper) bound at 2–SD from the coefficient value.