American Economic Journal: Economic Policy 2019, 11(3): 325–360
https://doi.org/10.1257/pol.20160272

Place-Based Policies, Creation, and Agglomeration Economies: Evidence from China's Economic Zone Program†

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Combining rich firm and administrative data, this paper examines the incidence and effectiveness of a prominent place-based policy in China: special economic zones. Establishing zones is found to have had a positive effect on capital investment, employment, output, productivity, and wages, and to have increased the number of firms in the designated areas. Net entry plays a larger role in generating those effects than incumbents. The special zone program's net benefits over three years are estimated to amount to about US$15.62 billion. Capital-intensive industries benefit more than labor-intensive ones from the zone programs. (*JEL* O16, O18, O25, P25, R23, R32, R58)

Place-based programs—economic development policies aimed at fostering economic growth in a specific area within a larger jurisdiction—have grown popular and been pursued by many governments around the world over the past several decades. By design, place-based policies can potentially influence the location of economic activity, as well as the wages, employment, and industry mix in the targeted area (Kline and Moretti 2014b). Some economists are skeptical about the efficiency of such programs (Glaeser and Gottlieb 2008; Glaeser, Rosenthal, and Strange 2010). Firms may move from other regions to the targeted area and arbitrage away the benefits associated with the program without improving the welfare of local residents (Kline 2010, Hanson and Rohlin 2013). Still, agglomeration economies are considered an important rationale for policies that encourage new investment in a specific area (Kline and Moretti 2014a, Combes and Gobillon 2015).

Although there has been much research focused on such programs in the United States and Europe (see Neumark and Simpson 2014 for a comprehensive

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†Go to https://doi.org/10.1257/pol.20160272 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

review),[1] there have been few attempts to evaluate interventions in developing countries. Several questions loom especially large: who benefits and who loses from place-based programs? Do the economic gains substantially outweigh the costs? Which factors determine the effectiveness of such interventions? Since developing countries usually suffer from poorly developed institutions and markets, would the assumptions and conceptual approaches of the place-based policies in the United States and Europe still hold for them? Very little progress has been made in addressing these issues, largely because of a lack of longitudinal studies in developing countries, in particular research that traces a place-based program's effects on microlevel units such as firms and workers.

This study constitutes a novel step in that direction. Specifically, it documents microlevel evidence about the incidence and effectiveness of place-based policies in China's special economic zones (SEZs). SEZs are a prominent development strategy implemented worldwide (Akinci and Crittle 2008). They attempt to foster agglomeration economies by building clusters, increasing employment, and attracting technologically advanced industrial facilities. China provides an ideal setting for exploring the effects of SEZs on regions and firms, which is of great policy relevance. In 1979, China launched its first four SEZs as an experiment in pragmatic and innovative policies. After their early success, China's horizon for SEZs has gradually expanded from the coastal areas to the center and west. This study focuses on the wave of SEZs established between 2005 and 2008. In 2006, for example, 663 provincial-level SEZs were established in China, among which 323 were in coastal areas, 267 were in central areas, and 73 were in the west. That sample is more representative of the eventual spatial distribution than earlier waves, as it includes 42 percent of China's SEZs. Hence, estimates based on that sample have large-scale implications.

The analyses proceed in three stages. The first examines the local effects of an SEZ on the targeted area using a reduced-form approach. Welfare analysis is then applied to evaluate the SEZs' overall costs and benefits. Finally, the extent to which the effects of the zones depend on program features and the characteristics of the targeted localities is analyzed.

A key innovation has been the construction of a novel dataset that merges comprehensive data on China's economic zones with two geocoded economic censuses in 2004 and 2008 covering all manufacturing firms. The zone data include the year in which each zone was established, the type of zone, and the villages located within its boundaries. The merged dataset contains information on firms' ages, sectors, addresses, investment, employment, and output. That is supplemented, more importantly, with information on the firms' dynamics (entries, exits, and continuing operations) and their geographic proximity to a zone. In all, 3,143,445 firms are covered. The individual firm data are aggregated to construct a panel dataset by area and year. The data series cover two periods—two years before a zone's establishment and two years after—allowing an assessment of any effects of the SEZs on the targeted areas and providing interesting evidence about how various margins contribute to

[1] Prominent examples include initiatives that target lagging areas, such as enterprise zones in the United States and regional development aid within the European Union.

the impacts. Other important outcomes such as productivity and wages are analyzed using data from China's Annual Surveys of Industrial Firms (ASIFs) from 2004 to 2008. They cover all state-owned enterprises (SOEs) and non-SOEs with annual sales of more than five million yuan. This is the first time that the outcomes of interest for SEZs have been precisely measured on such a disaggregated level. It is also the first time that comprehensive geocoded information on Chinese firms has been compiled and analyzed in relation to SEZs, something which has not previously been possible in such studies. Only with very fine-grained geographic data can empirical analyses detect whether an SEZ has had a positive incremental effect on economic activity or simply displaced activity from an untreated area to an adjacent treated one.

The key challenge in identifying any causal effects of zone programs is selecting appropriate control groups, given the possible presence of spillovers. This study starts with a difference-in-differences (DD) analysis at the village level (the most disaggregated geographic unit and smaller than an SEZ) and then the county level. It compares the changes in performance among SEZ villages and counties with the changes among non-SEZ counterparts during the same period, conditional on a rich set of control variables. Beyond that, it investigates the robustness of the findings by checking parallel pre-trends between targeted and control areas using the ASIF data. As an alternative approach, the techniques of Neumark and Kolko (2010) and Briant, Lafourcade, and Schmutz (2015) are applied making use of the detailed information on firm location and zone boundaries. The discontinuity in treatment at the zone boundaries is exploited to combine a boundary discontinuity (BD) design with the DD setting (constituting a BD-DD analysis). Moreover, to examine any potential bias due to spillovers from nearby SEZ villages, we apply the unified estimation framework used by Miguel and Kremer (2004) and conduct concentric ring analyses following the lead of Kline and Moretti (2014a).

Moving beyond local effects, we compare the program's costs to the estimated magnitude of its impacts using a flexible estimation approach developed by Busso, Gregory, and Kline (2013) and Chaurey (2017). A back-of-the-envelope cost-benefit analysis is conducted. The distributional effects of the SEZ programs are considered, among which the main benefits include potential increases in firms' profits, workers' wages, and landlords' rental income. The corporate tax concessions that firms in SEZs typically enjoy are regarded as the main costs of the program.

The analyses yield three classes of results. First, the evidence shows that the SEZ program has had a significant and positive impact on the areas targeted. After two years, the SEZ areas have 58 percent more capital invested, 35 percent greater employment, and 49 percent larger output than the non-SEZ areas. The number of firms in the SEZs has increased by 29 percent. Productivity has increased by 1.5 percent on average within one year, and wage rates by 2.9 percent within two years, indicating agglomeration economies. There is relatively limited spillover in industrial activity between SEZs and the surrounding areas. The effects of SEZs mostly come from firm entering and exiting, with very limited effects from the previously existing firms.

Second, using the estimates from the back-of-the-envelope approach, the net present value of the benefits of the SEZ program during the 2006–2008 period is roughly US$22.60 billion. Comparing it with the total tax cost of US$6.98 billion, there are net benefits of US$15.62 billion from the zone program.

Finally, operating in a zone is most beneficial for firms in capital-intensive industries. Zones with better market potential or better access do not demonstrate significantly larger benefits. These findings are in line with the features of the SEZ programs, which typically subsidize capital investment.

This study fits into a large literature that explores quasi-natural experiments to quantify the impact of place-based programs. Criscuolo et al. (2019) investigates the causal impact of the UK's Regional Selective Assistance (RSA) program on employment, investment, productivity, and plant numbers. Givord, Rathelot, and Sillard (2013) examines the impact of Zones Franches Urbaines and their place-based tax exemptions on business entry and exit rates, economic activity, employment, as well as on the financial strength of the companies. Devereux, Griffith, and Simpson (2007), like Briant, Lafourcade, and Schmutz (2015), uncover geographic factors that can account for heterogeneities in programs' effects, though in their study they find a significant impact of better market access. Along these lines, Rothenberg (2011), too, emphasizes the role of transportation infrastructure in firm location choices, and hence the spatial distribution of economic activity. Chaurey (2017) has reported the only other study in a developing country (in India) that examines the impact of a location-based tax incentive scheme. That study finds that the program's heavy impact is driven by both the growth of existing firms and by the entry of new ones. There is no evidence of relocation or other spillovers between the treated and control areas. These Chinese findings are generally comparable with those of Chaurey, presumably due to the similar states of market development.

This study relates to a number of studies that have evaluated the aggregate effects of place-based policies in the presence of agglomeration externalities and inferred the implications for productivity and welfare, such as those of Busso, Gregory, and Kline (2013), and Kline and Moretti (2014a). But this study has been among the first to attempt a cost-benefit analysis of SEZs in the context of a developing economy. The significant net benefits estimated here could be closely linked to the institutional improvement brought by the SEZs in China.

This study also adds to industrial policy literature with special applications to Chinese SEZs. Rodrik (2008) has highlighted SEZs' utility as vehicles for China's integration into the global economy. Alder, Shao, and Zilibotti (2016) and Wang (2013) investigate the local (city-level) impact of SEZs on growth, capital formation, and factor prices, while Cheng (2014) estimates both the local (county-level) and aggregate impacts. The firm-level evidence developed in this study resonates particularly well with Wang's finding that the majority of the foreign direct investment attracted by the SEZs has been new activity rather than simply a reallocation from other non-SEZ areas (Wang 2013). Alder, Shao, and Zilibotti (2016), too, detects no evidence of beggar-thy-neighbor effects on GDP. There have been a few other attempts to extend the studies of China's SEZs to micro-domains. Schminke and Van Biesebroecke (2013) investigates the extensive margin effect of national-level zones on firms' exporting performance. More recently, Zheng et al. (2017) examines 110 national- and provincial-level industrial parks in eight major cities and any production and consumption spillovers they triggered.

Methodologically, this study relates to much previous work applying the geographic regression discontinuity designs (Holmes 1998; Black 1999; Bayer, Ferreira, and McMillan 2007; Dell 2010; and Keele and Titiunik 2015). It also relates broadly to a set of studies examining the impact of taxation on firm-level outcomes such as location decisions, entry, and employment (Duranton, Gobillon, and Overman 2011; Brülhart, Jametti, and Schmidheiny 2012).

The remainder of the paper is organized as follows. Section I lays out the SEZ reform background. Section II describes the identification strategies. Section III presents data in detail. Section IV reports the baseline local SEZ effect estimates and addresses various econometric concerns, followed by evidence on the mechanisms in Section V. Section VI provides an analysis on the cost and benefit of this program. Section VII investigates whether these effects are heterogeneous across industries, zones, and firms of different size. The last section offers concluding remarks.

I. Background

In China, SEZs have been widely adopted as a key industrial policy aimed at increasing foreign and domestic investment, promoting international trade, and stimulating technological cooperation and innovation in a specific geographic area. China has two main categories of SEZs: national-level and province-level economic zones. The former have been approved by the central government and are more privileged, while the latter are provincial government initiatives. Geographically, national and provincial SEZs are mutually exclusive—a location cannot be both a provincial and a national SEZ at the same time. Each zone has an administration committee, which administers the zone on behalf of the sponsoring government, handling project approval, local taxation, land management, finance, personnel, environmental protection, and security. Because SEZs are considered engines for economic growth, their success is linked to the political careers of regional government officials. That encourages their supervisors to strive for the best possible performance (Xu 2011). SEZs enjoy a certain degree of independence and have authority to define, within limits, their own preferential policies. The most important preferential policies usually include the following (Wang 2013; Alder, Shao, and Zilibotti 2016):

Tax Deductions and Customs Duty Exemptions.—Corporate income tax rates of 15 percent–24 percent as opposed to the 33 percent firms normally pay in China are available to foreign enterprises, technologically advanced enterprises, and export-oriented enterprises. Customs duty exemption is given for equipment and machinery employed in the production of exports.

Discounted Land-Use Fees.—Under Chinese law, all land is state-owned. Investors may lawfully obtain land development and business use rights through a contractual agreement, often after an auction or bidding process. To attract more industrial capital, SEZs set low land transfer fees (Wei and Zhao 2009).[2] The duration of the

[2] Development zones were mostly created by city governments on rural land expropriated at below-market prices. Within the zone boundaries, municipalities have acquired large tracts of collectively owned land following

agreement, the fees, and the method of payment depend on the type of business. For example, in Guizhou province, for enterprises certified as high-tech, the discounted rate could be only 25 percent to 35 percent of the regular fee. Export-oriented enterprises may receive a 10 percent to 20 percent discount on the normal fee. For infrastructure projects such as those improving transportation, telecommunications, water supply, energy supply, or environmental protection, the discount could be 20 percent to 30 percent.

Special Treatment in Securing Bank Loans.—Foreign-invested enterprises are encouraged to make use of domestic finance for their investments. The banks prioritize their loan applications.

Compared to the place-based programs in developed countries, China's SEZs have several distinctive features. First, China as a developing nation faces more governance and financing constraints than its more-advanced counterparts. Before the SEZ program, China's business environment was typified by weak institutions, including poor protection of private property rights, limited financial resources, and weak infrastructure. None of this stimulated entrepreneurship. Small-scale regional SEZs were established, aimed at policy experimentation and innovation. Within the zones, better institutions were provided, aimed at reducing preexisting distortions and improving economic efficiency (Alder, Shao, and Zilibotti 2016). The early SEZs represented the Communist Party's commitment to the market economy and property rights protection, at least within those areas. Financial resources were directed to the targeted areas. The SEZs worked constantly to improve their utilities, telecommunications, transport, storage, and other basic installations and service facilities. At the same time, policies in the non-SEZ regions remained basically unchanged (Rodrik 2008). That reduced resistance to the reform by substantially reducing the number of losers (Xu 2011). The SEZs constituted a new reform path implementing transitional and heterodox institutional reforms. They managed to provide efficient incentives while maintaining the rents of those who were politically powerful (Qian 2003). Taken together, those features of China's institutional landscape imply that the welfare consequences of place-based policy interventions may be different from those in a more developed setting such as the United States or Europe where the policy environment is closer to a first-best world, and more delicate equity-efficiency trade-offs may be involved.

Figure 1 shows the geographic distribution of the SEZs established in five waves over three decades: the 1979–1983 wave, the 1984–1991 wave, the 1992–1999 wave, the 2000–2004 wave, and the 2005–2008 wave. In the first two waves, there were few SEZs established and they were mostly located in coastal regions and provincial capital cities. After Deng Xiaoping's famous southern tour in 1992, there was a surge in zone establishment (93 national and 466 provincial SEZs), and a multi-level and diversified pattern of opening coastal areas and integrating

a formal requisitioning procedure. The administration committee of the SEZ then develops the now state-owned land by resettling the residents, paying compensation, destroying old construction, and installing new infrastructure. Plots developed in this way were eventually transferred to the zone's enterprises. See Wei and Zhao (2009).

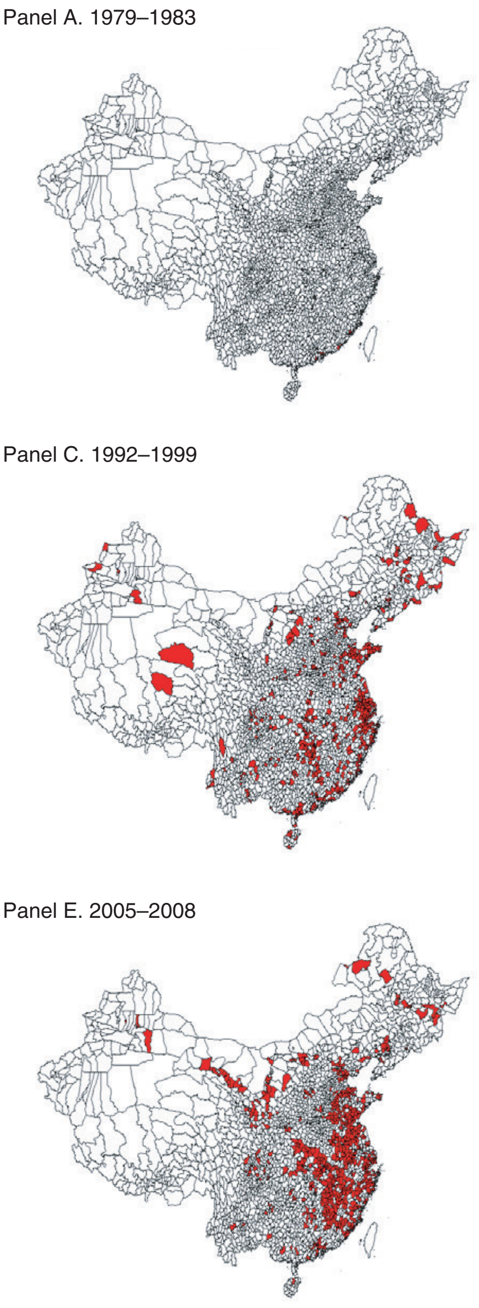

Figure 1. Special Economic Zones by Wave

Notes: There have been five granting waves of SEZs: 1979–1983, 1984–1991, 1992–1999, 2000–2004, and 2005–2008. In each wave, counties where SEZs were newly established are indicated by the shaded areas.

them with river, border, and inland areas took shape. From 2000, aiming at reducing regional disparity, China's first comprehensive regional development plan (the Western Development Strategy) was launched. As a result, zone establishment was for a few years concentrated in inland cities. More recently, zone establishment has been more balanced geographically. Between 2005 and 2008, 338 SEZs were established in coastal areas, 269 in the central area, and 75 in the west. For detailed descriptions of these waves, see online Appendix A.

There are several types of SEZs, in which the preferential policies have different focuses (Alder, Shao, and Zilibotti 2016; Zeng 2010). Economic and technological development zones (ETDZs) are broadly defined zones with a wide spectrum of investors. High-tech industrial development zones (HIDZs) are intended to promote high-tech industrialization (such as software writing, integrated circuit, and communications equipment manufacturing, biotechnology research, and so on) and to foster technological enterprise-based innovation. HIDZs and ETDZs have some similar functions, and the line between the two types of zone is blurred. Specialized industrial zones (SIZs) are cluster-type industrial parks aiming to develop particular industries, which should be consistent with local comparative advantages. Bonded zones were set up with three objectives: export processing, foreign trade, and logistics supported by bonded warehousing. Although they are physically inside China's borders, they function outside the country's customs regulations. Export processing zones (EPZs) are similar to bonded zones but are solely for export processing (to develop export-oriented industries).

Table 1 shows the number of each type of zone established in the five waves. National-level SEZs are more diverse, with EPZs being the dominant type in the recent waves. Provincial zones are usually ETDZs, HIDZs, or SIZs. In the earlier years, SEZ status was not granted randomly. According to State Council documents, the central government authorized municipalities to establish SEZs based on a favorable geographic location, favorable industrial conditions, and good human capital (Wang 2013). Such site selection may have systematically biased the results because any positive effects could primarily reflect successful initial targeting of better endowed areas, which would be more responsive to treatment (Allcott 2015). However, as the SEZ program was later expanded to other areas, it tended to be more representative of the eventual spatial distribution and less subject to such biases. This study therefore focuses on the latest granting wave. To further alleviate estimation biases arising from interactions between the old and new zones, areas covering zones from earlier waves are excluded from the analyses in constructing the comparison group (see Section II for details).

Even within a given city, economic zones were not randomly located—another dimension of the site selection bias. For example, zones tends to be located far from central business districts where farmland is more available and the opportunity cost is lower. That nonrandom siting of SEZs presents challenges in identifying their effects. How to choose a comparable control group? In this study, several estimation strategies are applied to address the identification issue (see Section II for details).

Table 1—SEZ Wave Breakdown

Wave	1979–1983	1984–1991	1992–1999	2000–2004	2005–2008
Number of zones newly established	4	66	559	261	682
Comprehensive SEZs	4				
National-level economic zones		46	93	64	19
By type					
1. Economic and technological development zones		12	20	17	
2. High-tech and industrial development zones		26	27		
3. Export processing zones			1	39	18
4. Bonded zones		4	11		
5. Border economic cooperation zones			14		
6. Other		4	20	8	1
By region					
1. Coastal region		36	60	39	15
2. Central region		6	18	12	2
3. Western region		4	15	13	2
Province-level economic zones		20	466	197	663
By type					
1. Economic and technological development zones		18	430	169	615
2. High-tech and industrial development zones		2	29	14	20
3. Specialized industrial zones			7	14	28
By region					
1. Coastal region		7	277	76	323
2. Central region		7	138	71	267
3. Western region		6	51	50	73

Notes: For each period, the number of development zones newly established is provided, and comprehensive SEZs, national-level development zones, and province-level economic zones are distinguished. The comprehensive SEZs are the zones established in Shenzhen, Shantou, Zhuhai, and Xiamen. National-level development zones are granted by the central government. Province-level development zones are granted by the provincial governments. The coastal region includes Liaoning, Beijing, Tianjin, Hebei, Shandong, Jiangsu, Shanghai, Zhejiang, Fujian, Guangdong, Guangxi, and Hainan. The central region includes Heilongjiang, Jilin, Inner Mongolia, Shanxi, Henan, Anhui, Hubei, Hunan, and Jiangxi. The western region includes Shaanxi, Gansu, Ningxia, Qinghai, Xinjiang, Guizhou, Yunnan, Chongqing, Sichuan, and Tibet.

II. Estimation Strategy

The village is used as the unit of analysis in the baseline analysis, as it is the most disaggregated geographic unit in the data, and smaller than an SEZ. Difference-in-differences estimation compares one village's performance before and after the founding of an SEZ (the first difference) with the changes among non-SEZ counterparts during the same period (the second difference). Specifically, we use the following specification:

$$Y_{vt} = \lambda_v + \gamma D_v \times Post_t + \lambda_{ct} + \left(\mathbf{X}_v \times \lambda_t\right)'\boldsymbol{\eta} + \varepsilon_{vt}, \tag{1}$$

where Y_{vt} is an outcome (the logarithm of capital, employment, output, number of firms, total factor productivity, the wage rate) of village v in year t; D_v denotes the program status with 1 if village v had an SEZ program and 0 otherwise; $Post_t$ is the indicator of a post-SEZ period; λ_v is a village fixed effects term capturing time-invariant village characteristics such as geographic location; λ_{ct} is a county-year fixed effects term capturing macro shocks common to all villages within the same county in year t; X_v is a vector of baseline village characteristics (to be discussed later); λ_t is a year fixed effect; and ε_{vt} is the error term. To control for potential heteroscedasticity and serial correlation, the standard errors are clustered at the county level. To check for any sensitivity to using the village as the unit of analysis, we conduct the DD estimations using counties. They comprise many villages and are bigger than an SEZ.

The unbiased estimation of γ hinges on two conditions. First, conditional on the controls (i.e., λ_v, λ_{ct}, $X_v \times \lambda_t$), that the regressor of interest $D_v \times Post_t$ is uncorrelated with the error term ε_{vt}—the conditional mean independence assumption. The second condition is that there is no spillover from the treatment villages to the control villages. In the next two subsections, we discuss several strategies to examine these two conditions in our research setting.

A. *Conditional Mean Independence*

Conditional mean independence condition is verified using three approaches.

Conditional on Baseline Characteristics.—SEZ villages (or counties) are first compared with the non-SEZ areas in terms of a wide range of baseline characteristics. The covariates can shed light on how SEZ and non-SEZ areas differed at the inception of the SEZ program. All of the baseline characteristics are then included in the DD estimations to rule out the influence of the pre-program differences between the treatment and control groups. If the results with and without baseline controls are largely similar, then that is taken as indicating that the DD estimates are not severely biased by incomparability between the treatment and control groups (Altonji, Elder, and Taber 2005).

Pretreatment Parallel Trends.—A valid identification strategy requires that the treatment and control groups follow similar pre-program parallel trends. To confirm this, additional years of village and county outcome data were collected, and any temporal trends are analyzed comparing the treatment and control groups at both the village and county levels.

BD-DD Estimation.—Despite the various specifications used, the DD estimations could still have suffered from the nonrandom selection of the SEZ sites. As an alternative estimation approach, a boundary discontinuity framework is employed based on physical distance, an approach pioneered by Holmes (1998) and Black (1999) and widely applied in previous studies (e.g., Bayer, Ferreira, and McMillan 2007; Dell 2010; Duranton, Gobillon, and Overman 2011; and Gibbons, Machin, and Silva 2013). Compared with the standard regression discontinuity design, a BD design involves a discontinuity threshold, which in this case is an SEZ boundary (Lee and Lemieux 2010). The premise of the BD framework is that close to the zone boundary, treatment and control areas should have very similar underlying characteristics except for the zone policies (the regressor of interest).[3] Any discontinuity in the outcomes at a zone boundary is assumed to be attributable to the zone's effects.

The traditional BD framework is imbedded in DD analyses and BD-DD analyses are conducted, which compare areas close to the zone boundary before and after the zone's establishment. That provides a further control for any time-invariant differences in location characteristics among areas close to the zone boundary.

The BD-DD estimation equation is

$$(2) \qquad Y_{azt} = \lambda_a + \gamma^z D_{az} \times Post_t + \lambda_{zt} + \varepsilon_{azt},$$

where Y_{azt} measures performance in area a within 1,000 meters of the boundary of zone z in year t. The variable D_{az} is an indicator set equal to 1 if area a is inside zone z with zone policies, and 0 otherwise; λ_a is an area fixed effect capturing all time-invariant area characteristics; λ_{zt} is a neighborhood-year fixed effect capturing unobserved shocks common to both sides of the zone z boundary in year t. Including neighborhood-year fixed effects allows for flexible time trends across different zones. Again, ε_{azt} is an error term. To ensure conservative statistical inference, the standard errors are clustered at the zone level.

B. *Spillovers*

The aforementioned framework does not allow for any spillover of the SEZ program from SEZ villages to non-SEZ villages, but such spillovers may in fact take place. For example, SEZ villages may attract industrial activity from non-SEZ villages (e.g., through relocation). That would constitute a negative spillover, but firms in non-SEZ villages might at the same time learn from their competitors in SEZ villages, an example of a positive spillover. Two techniques are applied to check on the magnitude of any spillovers and the associated biases in the DD estimates of the SEZs' effects.

Direct Estimation of the Spillover Effect.—To directly test for any spillover between villages, we apply the approach used by Miguel and Kremer (2004). Assume that spillovers between villages mostly take place within a broadly-defined region and are less important between such regions. On that assumption, the data for aggregated geographic units, specifically counties, are used.[4] The analyses take advantage of the rich variations in the proportion of counties that are treated as well as the percentage of villages within a county that are treated. Specifically, we use the following augmented specification:

$$(3) \qquad Y_{vct} = \lambda_v + \gamma^v \cdot (D_{ct} \times D_v) + \sigma \cdot D_{ct} + \lambda_t + (\mathbf{X}_v \times \lambda_t)'\boldsymbol{\eta} + \varepsilon_{vct},$$

[3] Additionally, areas close to a zone boundary were required never to have been within any old zone (established before 2006). That rules out any concern that the estimates could have been complicated by interactions between old and new zones.

[4] We do not consider cross-county spillovers because subsequent ring analyses suggest very little program impact beyond 20 kilometers (km) from a treated village, and the average distance across Chinese counties is about 1,200 km.

where $D_{ct} = D_c \times Post_t$; and D_c denotes the treatment status as 1 if county c had one or more villages in an SEZ and 0 if it had none.

In this regression, σ captures the spillover effect on the untreated villages within a treatment county, whereas γ^v is the additional direct effect on a treated village. Their sum, $\gamma^v + \sigma$, is the overall treatment effect on a treated village. The equation (3) shows that if possible spillover σ is not accounted for, bias arises in the program effect estimates from the DD specification (1).

Concentric Ring Analysis.—Another approach to estimating the magnitude of any spillovers and the importance of any biases is to exclude non-SEZ villages adjacent to the SEZ from the control group (Kline and Moretti 2014a; see also Neumark and Kolko 2010 for a discussion of using adjacent areas as controls). Without sufficiently detailed information about village boundaries, that approach involves first calculating the distance between any two villages, and then excluding from the control group first the non-SEZ villages located within 2 km of an SEZ village, then those within 4 km, continuing step-wise out to 20 km. This results in a series of estimates that show how sensitive the estimates of an SEZ's effects are to the use of the adjacent area in the control group, and how important any spillover effects are.

To further explore the robustness of the DD estimates to any spillover effects, an alternative framework used by Zheng et al. (2017) is applied. It permits separately identifying the treatment effect and spillovers on a set of non-SEZ rings around the SEZ villages. Specifically, the specification is

$$(4) \qquad Y_{vt} = \lambda_v + \gamma D_v \times Post_t + \sum_{n=1}^{10} \sigma_n Ring(2(n-1), 2n)_v \times Post_t + \lambda_{ct} + (\mathbf{X}_v \times \lambda_t)'\boldsymbol{\eta} + \varepsilon_{vt},$$

where $Ring(2(n-1), 2n)_v$ are dummy variables indicating whether or not village v is located in the nth ring that is between $2(n-1)$ and $2n$ km from its nearest SEZ village, $n = 1, 2, \ldots, 10$. In this regression, γ is the treatment effect on the SEZ village, and σ_n is the spillover externality effect on the nearby nth ring.

III. Data

Firm Data.—The data used in this study came primarily from the economic censuses conducted by China's National Bureau of Statistics at the end of 2004 and 2008. The advantage of census data over the ASIF data often used in similar studies (e.g., Hsieh and Klenow 2009) is that it is more comprehensive, covering all manufacturing firms in China, while the latter includes all SOEs and non-SOEs with annual sales of more than five million yuan. Table A1 in the online Appendix compares those two data sources for 2004 and 2008. The census data, which represent the entire population of manufacturing firms, clearly show smaller and more dispersed sales, employment, and total assets figures than the ASIF data.

The census data contain firms' full basic information, such as address, location code (a 12-digit code corresponding to a village or community), industry affiliation, and ownership. The address and the location code are used to locate a firm geographically and identify whether or not it is in a zone. The census data report employment, output, and capital for each firm.

Although the economic census achieves complete coverage of manufacturing firms, it has two shortcomings. First, the data were collected in two waves, only one of them in the pretreatment period. That prevents comparing pretreatment trends between the targeted and control areas. Also, the dataset includes only three firm-level outcomes, from which the productivity and price impacts of SEZs cannot be directly inferred. To overcome these issues and provide comprehensive assessments of any SEZ effects, the analyses are augmented using the ASIF data to check for parallel pre-trends, investigate other important outcomes (such as changes in TFP and wages), and conduct cost-benefit analyses. In particular, the ASIF figures from 1998 (the first year to have the data) to 2008 are used for county-level parallel trend tests and those from 2004 (the first year to have detailed village administrative codes) to 2008 are used for village-level common-trend tests. The ASIF figures from 2004 to 2007–2008 are used to examine TFP and wages at both the village and county levels.[5] ASIF figures are also used to conduct the cost-benefit analyses at the county-level along the lines pioneered by Busso, Gregory, and Kline (2013) and by Chaurey (2017).

Firm SEZ Status.—The census data did not directly report information about each firm's SEZ status. To identify whether or not a firm is located within an SEZ, a comprehensive SEZ dataset from the China's Ministry of Land and Resources is consulted. It defines SEZ boundaries in terms of villages, communities, and sometimes roads. Based on that information, maps are consulted to determine whether or not a village or community is within the boundary. The SEZs' official websites often report detailed information about the villages and communities within their administrative boundaries, and they, too, are consulted. The National Bureau of Statistics and the Ministry of Civil Affairs also report administrative divisions and codes at the village and community level on their websites. For some economic zones, this includes information on the villages and communities under their administration.

A list of villages and communities within each zone is thus created. Matching the list with the census data, the firms' addresses as well as their 12-digit location codes are used. (See online Appendix B for a detailed discussion.) To verify that approach, the results are checked by matching them against the SEZ names, which some firms include in their addresses.[6]

Coordinates Data.—In the BD-DD analyses, the outcomes of individual firms are aggregated into areas close to the zone boundaries. This requires precise geographical information about firms' locations (specific coordinates) to determine each firm's distance from a zone boundary. The firms' addresses are used with Google's geocoding

[5] In the 2008 ASIF data, no information on materials or value added is available. That makes calculating TFP impossible for 2008, so the TFP analyses use data from 2004 to 2007.

[6] If an SEZ boundary bisected a village or community, only part of it would be in the zone. But this is not a concern in China where the local governments survey and appraise land and outline plans for future development based on village and community units.

API to obtain their geographic coordinates.[7] Each firm's detailed Chinese address (for example, "157 Nandan Road, Xuhui District, Shanghai, China") is first used with Google maps to obtain a map with the specific location of the address indicated (see online Appendix Figure A1). After confirming the correctness of the marked location, the firm's latitude and longitude are extracted from the Google map. This process allows determining the coordinates of approximately half of the firms.

To deal with incomplete addresses (those with only information on the village, building, or street name, but with no number or building name), road name changes, and reporting errors, the remaining firms are searched for using their 12-digit location codes.[8] For example, a firm with the inexplicit address "Liuhe Town, Taicang City, Suzhou, Jiangsu Province, China" also had a 12-digit location code of "320585102202" which corresponds to the more specific "Liunan Village, Liuhe Town, Taicang City, Suzhou, Jiangsu Province, China." The name of that village or community could then be used to collect the latitude and longitude information from Google maps (see online Appendix Figure A2).

Village and County Baseline Characteristics.—The village-level baseline characteristics include a village's distance from an airport and port, the capital-to-labor ratio and the number of firms in the village in 2004, all aggregated from the 2004 census data. The counties' baseline characteristics come from the provinces' 2004 statistical yearbooks and the China Population Census for 2000. Those sources report a rich set of variables including land area, total population, employment, GDP, the share of rural employment, export intensity, the ratio of government expenditure to revenue and much else, all in 2004. Cumulative GDP growth rate between 1998 and 2004 is also collected, along with the share of employment in agriculture, and that in manufacturing, the mortality rate, and the share of eliminated illiteracy population, all as of 2000.[9] The airport and port distances, capital-labor ratios, and number of firms in 2004 come from the 2004 census data.

Regression Data.—These analyses focus on SEZs established between 2004 and 2008. There were 682 SEZs established during that period (19 in 2005 and 663 in 2006), and there was substantial geographic variation. There were 338 SEZs established in the coastal area, 269 in the central area, and 75 in the west. Nineteen were national-level zones, most of which were EPZs. There were 615 province-level ETDZs, 20 province-level HIDZs, and 28 province-level SIZs. National-level zones are excluded from the analyses because of the concern that they might not be fully comparable with province-level zones and because they are mostly EPZs within pre-established ETDZs—an overlapping problem.[10]

[7] The robustness of these results is checked using Baidu's geocoding API service. Baidu is the Chinese version of Google. It provides a similar service, but uses a different coordinate system.

[8] There are approximately 700,000 villages and communities in China. The nation's habitable area is about 2.78 million square kilometers. On average, a village or community covers about 4 square kilometers. In the census data, the average number of firms in a village or community was 5.4 in 2004 and 6.7 in 2008. The statistics indicate the precision of using village or community coordinates when firms do not provide a detailed address.

[9] It would have been ideal to have data on all of the control variables for 2004, but data availability requires augmenting the analysis with data on some covariates in 2000.

[10] In 2005, 19 national-level zones were approved by the central government, of which 18 were EPZs. Such national-level zones have higher level administration committees than provincial-level SEZs, and their committees

For the DD analyses, individual firms are aggregated to construct panel datasets by county (or village) and by year. Thus, each county (or village) has two observations in 2004 and 2008 in the DD estimation, a year of data before and a year of data after the zone's establishment. For the county-level regressions, the sample consists of 1,582 counties: 362 SEZ counties and 1,220 without an SEZ. For the village-level regressions, the analysis is restricted to SEZ villages and non-SEZ villages in the same county. The resulting sample comprises 59,949 villages in 580 counties: 3,963 SEZ villages and 55,986 non-SEZ villages.

The BD-DD analysis involves calculating each firm's distance from the nearest SEZ boundary. The coordinates of each firm's location have been established, but accurate geocodes for each SEZ's boundaries are not available, which prevent calculating the distance to the boundary directly.[11] Instead, the approach used by Duranton, Gobillon, and Overman (2011) is applied to determine the distance indirectly. To determine whether a firm is located within 1,000 meters of a zone boundary, a search within a radius of 1,000 meters of the firm is conducted,[12] as illustrated in online Appendix Figure A3. If firm A is located outside a zone and within 1,000 meters of firm B inside the zone, A is designated as being within 1,000 meters of the zone boundary; otherwise, it is not. Similarly, if firm C is located inside a zone and another firm (firm D) is found to be located outside the zone but within 1,000 meters of C, it is designated as located within 1,000 meters of the zone boundary.

Repeating these steps for each firm in the census data yields a sample of 587 SEZs with 163,069 firms located within 1,000 meters of their boundaries: the 2008 sample contains 126,976 firms, approximately 43 percent of which are located inside an SEZ; the corresponding numbers for the 2004 sample are 81,739 and 41 percent.[13] Those firms are then aggregated to construct a panel dataset by area and by year. Each zone's 1,000 meter neighborhood comprises two areas—inside and outside the zone—and each has two observations in 2004 and 2008. The regression sample for estimation comprises 1,174 areas.

IV. Empirical Findings

A. *Comparing SEZ and Non-SEZ Areas*

Table 2 lists the treatment and control group means for a variety of county and village characteristics in the initial year (all measured before the onset of the program), including a village's distance from an airport and port, capital-labor ratio, and the number of firms. For counties, it reports their total population, employ-

enjoy more authority in managing the zones. Those EPZs were mostly in pre-established ETDZs by design. To take the Huizhou Export Processing Zone as an example, it is located within the Guangdong Huizhou ETDZ, which was established in 1997. The DD and BD-DD identification techniques would not be applicable in that situation, as the preexistence of the ETDZ confounds the effect of the newly approved EPZ. See Wang (2013) for more details.

[11] The most detailed Chinese GIS data are at the town level. The unavailability of village boundary data renders an accurate geocoding of the zone boundaries impossible.

[12] On average, a village or community in China covers about 4 square kilometers. Assuming villages and communities are circular allows calculating an average radius for a village or community of about 1,000 meters, which is why a range of 1,000 meters from a zone boundary is used in the analyses.

[13] The analysis is restricted to SEZs that had firms on each side of their boundary in both 2004 and 2008.

TABLE 2—SUMMARY STATISTICS

	SEZ (1)	Non-SEZ (2)	Difference (3)
Panel A. Village level			
log distance from an airport (km)	3.766	3.877	−0.111
	(0.013)	(0.003)	(0.013)
log distance from a port (km)	4.847	4.918	−0.072
	(0.022)	(0.005)	(0.021)
log capital-labor ratio in 2004	1.729	1.158	0.571
	(0.020)	(0.005)	(0.018)
log number of firms in 2004	4.630	4.167	0.463
	(0.015)	(0.005)	(0.018)
Panel B. County level			
log population in 2004	3.912	3.317	0.595
	(0.032)	(0.025)	(0.050)
log employment in 2004	0.883	0.291	0.592
	(0.034)	(0.023)	(0.047)
log GDP in 2004	3.529	2.693	0.836
	(0.037)	(0.029)	(0.057)
Share of rural employment in 2004	10.436	10.619	−0.183
	(0.279)	(0.173)	(0.352)
Export intensity in 2004	0.013	0.006	0.007
	(0.002)	(0.001)	(0.002)
Ratio of government expenditure to revenue in 2004	2.861	5.865	−3.004
	(0.062)	(0.182)	(0.337)
GDP growth rate from 1998–2004	0.542	0.583	−0.041
	(0.018)	(0.009)	(0.019)
Share of employment in agriculture in 2000	0.754	0.790	−0.037
	(0.007)	(0.004)	(0.008)
Share of employment in manufacturing in 2000	0.142	0.134	0.008
	(0.004)	(0.002)	(0.005)
Mortality rate in 2000	0.062	0.065	−0.003
	(0.0005)	(0.0004)	(0.0008)
Share of eliminated illiteracy population in 2000	0.021	0.022	−0.001
	(0.001)	(0.001)	(0.001)
log land area (10,000 square km)	−1.746	−1.419	−0.328
	(0.032)	(0.029)	(0.056)
log distance from an airport (km)	4.320	4.534	−0.214
	(0.032)	(0.020)	(0.041)
log distance from a port (km)	5.713	6.354	−0.641
	(0.049)	(0.026)	(0.055)
log capital-labor ratio in 2004	4.704	4.767	−0.063
	(0.029)	(0.019)	0.039
log number of firms in 2004	5.593	4.244	1.348
	(0.048)	(0.040)	(0.077)

Notes: This table reports the summary statistics of the treatment and control samples (SEZ and non-SEZ). Panel A shows initial village-level characteristics, and panel B reports county-level characteristics in the initial year. Columns 1 and 2 show means and standard deviations in parentheses. Column 3 reports the unconditional difference between the treatment and control group.

ment, GDP, the share of rural employment, export intensity, ratio of government expenditure to revenue, GDP growth rate, share of employment in agriculture and that in manufacturing, mortality rate, share of eliminated illiteracy population, land area, distance from an airport and port, capital-to-labor ratio, and the number of firms.

SEZ areas tend to be closer to an airport and to have more manufacturing firms and capital than non-SEZ areas whether analyzed on a county or village basis. But column 3 of Table 2 shows that on many dimensions there were significant differences between the SEZ and non-SEZ counties. The counties with an SEZ were on average more densely populated, economically better developed and had greater fiscal capacity. They were also more accessible from a port. All of those baseline characteristics are included in the empirical analyses, interacted with year dummies in the DD estimations to control for the presence of the pre-program differences between the treatment and control groups.

B. *Baseline Estimates*

Table 3 presents the DD estimates using the village as the unit in the regressions. All of the regressions control for county-year fixed effects and village fixed effects.

Panels A and B report the results with and without controlling for baseline village characteristics (as illustrated in Table 2). Four outcomes reported in the two economic censuses are considered in columns 1–4: capital, employment, output, and the number of firms. The logarithms of those outcome data are presented to highlight the magnitude of the effects. The estimated coefficients of all four outcomes are consistently positive and statistically significant, suggesting that after the zones' establishment the SEZ villages gained investment, employed more labor, and produced more output than the non-SEZ villages—as would be expected, since they attract more firms. Meanwhile, panels A and B exhibit the same estimation patterns, with the former having slightly small magnitudes. These results suggest that the DD estimates may not have been entirely driven by the pre-program differences between SEZ and non-SEZ villages.

Given the limited accounting information collected in the economic censuses, the analyses of the efficiency impacts of the SEZ policies use the supplementary ASIF data from 2004 to 2008 (the years for which the 12-digit location codes were available).[14] Specifically, the ASIF data are applied in examining the relationship between SEZ status and total factor productivity and wage rates. TFP is estimated for each firm using the approach of Ackerberg, Caves, and Frazer (2015). They are then averaged for each village weighted using employment (see online Appendix C for details of the firm productivity estimation). Firms' average wage rates are similarly aggregated to the village level, also weighted by employment. As is shown in columns 5 and 6, both estimates are both statistically and economically significant. These results suggest that after the establishment of the zones, the SEZ areas witness an increase in productivity. And firms in the zones pay higher wages than those outside.

[14] Table A2 in the online Appendix shows consistent SEZ program effects on capital, employment, output, and the number of firms using the ASIF data.

Table 3—The SEZ Effects: Village-Level Analysis

Dependent variable	log capital (1)	log employment (2)	log output (3)	log number of firms (4)	log productivity (5)	log wage rate (6)
Panel A. Without controlling for covariates						
SEZ × post 2006	0.334	0.271	0.330	0.192	0.007	0.026
	(0.032)	(0.026)	(0.035)	(0.021)	(0.007)	(0.013)
Panel B. Controlling for covariates						
SEZ × post 2006	0.579	0.345	0.492	0.290	0.015	0.029
	(0.034)	(0.028)	(0.038)	(0.023)	(0.007)	(0.013)
Village FEs	Yes	Yes	Yes	Yes	Yes	Yes
County-year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	580	580	580	580	405	406
Observations	119,898	119,898	119,898	119,898	43,830	56,600

Notes: All observations are at the village-year level. In columns 1–4, census data from 2004 and 2008 are used for analysis. In column 5, ASIF data from 2004 to 2007 are used. In column 6, ASIF data from 2004 to 2008 are used. In columns 1–6, the dependent variable is the natural log of the measure of capital, employment, output, number of firms, productivity, and wage rate, respectively. Covariates include village-level characteristics listed in panel A of Table 2, interacted with the year dummy. The standard errors are reported in parentheses, clustered by county. All regressions control for village fixed effects and county-year fixed effects.

Table 4—The SEZ Effects: County-Level Analysis

Dependent variable	log capital (1)	log employment (2)	log output (3)	log number of firms (4)	log productivity (5)	log wage rate (6)
Panel A. Without controlling for covariates						
SEZ × post 2006	0.153	0.121	0.188	0.076	0.023	0.063
	(0.035)	(0.024)	(0.038)	(0.023)	(0.007)	(0.014)
Panel B. Controlling for covariates						
SEZ × post 2006	0.132	0.155	0.172	0.115	0.015	0.047
	(0.039)	(0.028)	(0.046)	(0.024)	(0.008)	(0.015)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	1,582	1,582	1,582	1,582	1,478	1,502
Observations	3,164	3,164	3,164	3,164	5,868	7,434

Notes: All observations are at the county-year level. In columns 1–4, census data from 2004 and 2008 are used for analysis. In column 5, ASIF data from 2004 to 2007 are used. In column 6, ASIF data from 2004 to 2008 are used. In columns 1–6, the dependent variable is the natural log of the measure of capital, employment, output, number of firms, productivity, and wage rate, respectively. Covariates include county-level characteristics listed in panel B of Table 2, interacted with the year dummy. The standard errors are reported in parentheses, clustered by county. All regressions control for county and year fixed effects.

To check the sensitivity of the results to the geographic unit used in the analysis, we report county-level DD estimates. The results are presented in Table 4. All of the regressions include year fixed effects and county fixed effects.

The estimated coefficients are consistently positive and statistically significant. They show consistent patterns in the two panels, suggesting that the DD estimates are not driven by the pre-program differences between SEZ and non-SEZ counties (i.e., baseline county characteristics as in Table 2). While the county-level DD estimates are smaller than the village-level ones, the differences reflect some muting due to the inclusion of non-SEZ villages in the SEZ counties. Without spillovers, the county-level DD estimator is simply the village-level version weighted by the outcome shares of the SEZ villages in the county.

To calculate the economic magnitude, we use the estimates from panel B of Table 3. Specifically, two years after the establishment of a zone, capital investment has increased by 57.9 percent on average, employment by 34.5 percent, output by 49.2 percent, and the number of firms by 29 percent. Those results compare well with those of previous studies. For example, Givord, Rathelot, and Sillard (2013) finds that the French Zones Franches Urbaines program has significant effects on both business creation and employment. Criscuolo et al. (2019) also finds a large and statistically significant average effect of the UK's employment and investment promotion program.

After one year of the SEZ program, productivity have improved by 1.5 percent on average. Wage rates have increased by an average of 2.9 percent after two years. Our findings are broadly consistent with other scholarly work on the agglomeration benefits of policies encouraging new business investment in a targeted area (Neumark and Simpson 2014). In particular, Zheng et al. (2017) analyzes the impact of 43 state-level and 67 provincial-level industrial parks in China and find that they generate TFP increases and wage premiums within the targeted area.[15] The state-level parks had a larger effect on TFP than the provincial-level ones, but there was little difference in their effect on wages. That is consistent with the findings here. But our findings do contrast with those of Alder, Shao, and Zilibotti (2016), whose city-level analyses show that only state-level SEZs have a large and positive effect on local GDP per capita. They find that the effect of provincial-level SEZs is not significant. However, in comparing results from multiple studies, one should bear in mind that they differ greatly in the period studied, their primary samples (zones), and the data used.

C. *Validity Checks*

This subsection provides two tests on the DD identifying assumption as discussed in Section IIA, that is, the check on the pre-trends, and the BD-DD estimation.

Check on the Pre-trends.—The ASIF data are used to analyze the pre-trends and verify the common trend assumption central to DD analysis. Specifically, the data from 2004 to 2008 are used for village-level DD analyses, and the regressor $D_v \times Post_t$ in the equation (1) is replaced by $D_v \times \lambda_t$. Ideally, the data series should be extended to earlier years to include a longer pretreatment period, but the ASIF only in 2004 started to report the 12-digit location codes essential for pinning down the firms' villages. The regression results are reported in online Appendix Table A3. Among all of the outcomes, the coefficients are small in magnitude and mostly insignificant in 2005. But they become statistically significant and of greater magnitude from 2006 on. These results confirm that SEZ and non-SEZ villages had comparable trends prior to the granting event (before 2006) and that remarkable differences started to emerge right after the zones were established.

[15] In that study, the plants (including incumbent plants and new entrants) in the parks are 25.7 percent more productive after the introduction of the park. The wages in the parks are 12.7 percent higher after a park's creation.

The analyses of the county-level pre-trends use data from 1998 (the first year in the ASIF data) to 2008. The regression results are displayed in Figure 2, in which 2005 (the year before the SEZs' establishment) is used as the reference year.

For capital, employment, and output, all the coefficients before 2006 are negative and largely insignificant, sharing similar small magnitude, but they become positive and gradually increase in magnitude after 2006. For the number of firms, TFP, and wage rates, the coefficients show a similar pattern. These results also support the assumption of similar trends in the SEZ and non-SEZ counties before the SEZ program.

BD-DD Estimates.—Table 5 shows the coefficients describing the impact of an SEZ program as estimated using the BD-DD framework with a sample of areas within 1,000 meters of a zone boundary. Consistent with the baseline estimates, the analyses show statistically and economically significant effects of the SEZ program. Moreover, the estimates are not sensitive to the inclusion of additional controls. The magnitudes of the coefficients are comparable to those from the baseline DD estimations, lending further support to the utility of this estimation framework.

D. *Spillovers*

One might be concerned that these DD estimates could be biased due to spillovers from SEZ villages to non-SEZ ones. The two sets of exercises laid out in Section IIB are used to address that possibility.

Direct Estimation of the Within-County Spillover Effect.—Equation (3) in Section IIB allows the direct estimation of any spillover. The results are reported in Table 6. For employment, output, and the number of firms, the spillovers are positive and significant, albeit small, suggesting a slight downward bias of the SEZ effects in the baseline estimations. There are no significant spillovers with respect to capital, productivity, or wages. After correcting for the minor spillovers, sizable and positive effects of the SEZ program remain. Overall, although spillovers are considered an important concern in the place-based policy literature, they do not appear to have been empirically first order in this Chinese setting.

Concentric Ring Analysis.—The concentric ring approach elaborated in Section IIB allows a further check on spillovers in the village-level DD estimations. It generates a series of estimates $\hat{\gamma}_j$ (where $j \in \{1, 2, \ldots, 10\}$) corresponding to the exclusion of non-SEZ villages located within $2j$ km of an SEZ village. Figure A4 in the online Appendix plots the estimated coefficients $\hat{\gamma}_j$ with their 95 percent confidence intervals. Overall, using different sets of control villages yields positive and significant estimates and some evidence that is consistent with small positive spillovers from SEZ villages to nearby ones. Excluding adjacent non-SEZ villages does not substantially affect the estimates. Since spillover diminishes with distance from an SEZ village, these results suggest limited impact of spillovers.

That pattern is corroborated by the estimates of equation (4). Figure A5 in the online Appendix shows that the estimated treatment effects of being an SEZ village

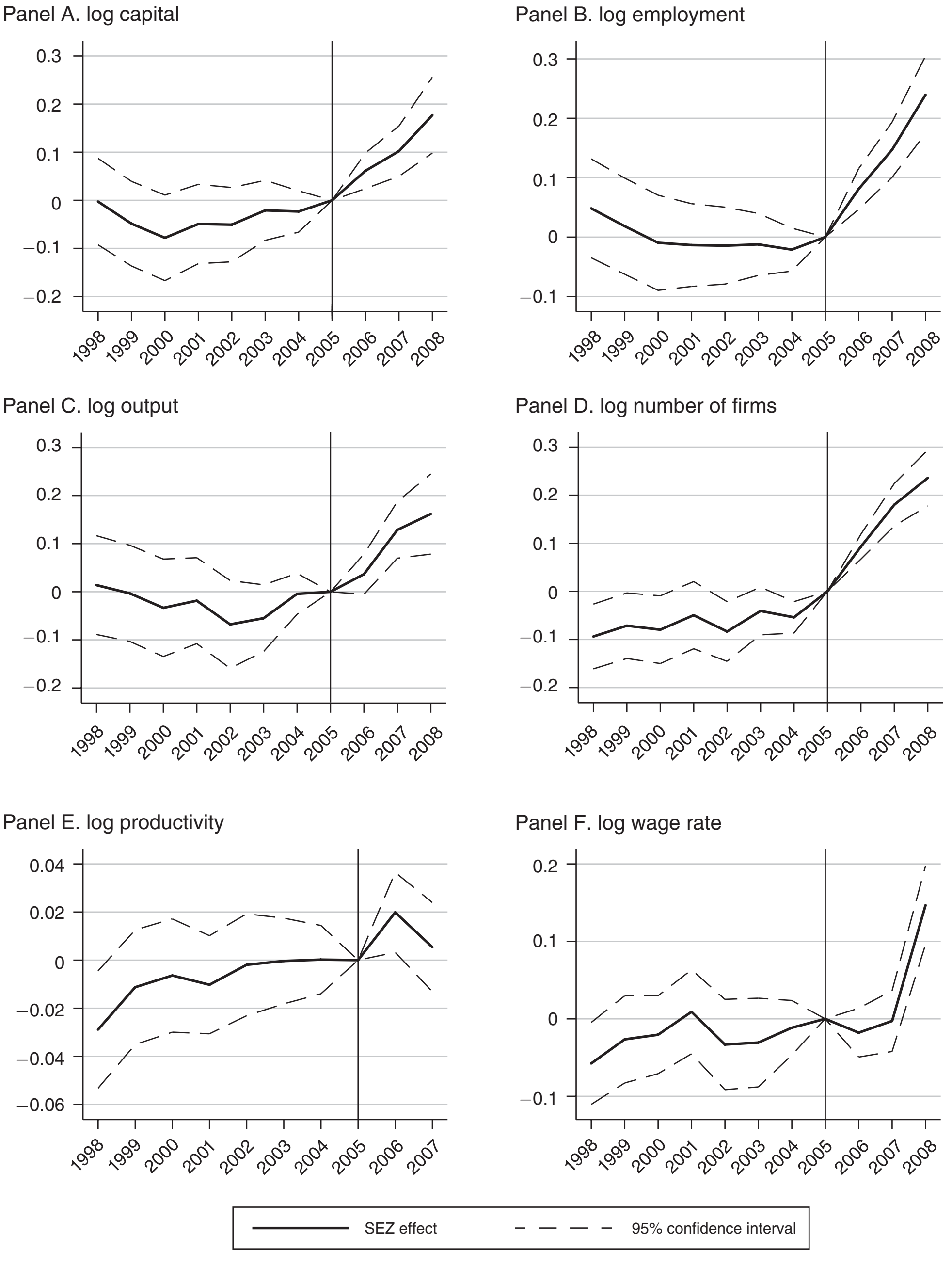

Figure 2. Test for Parallel Trends at the County-Level Analysis

Notes: Any temporal trends are analyzed comparing the treatment and control groups at the county level. ASIF data are used for the analysis. The plots connected by the solid line indicate changes in outcomes compared to 2005 (the period immediately before the SEZ treatment) conditional on baseline characteristics, county, and year fixed effects. The dashed lines indicate the 95 percent confidence intervals where standard errors are clustered at the county level.

TABLE 5—THE SEZ EFFECTS: BD-DD ESTIMATIONS

Dependent variable	log capital (1)	log employment (2)	log output (3)	log number of firms (4)	log productivity (5)	log wage rate (6)
Panel A. Without controlling for covariates						
SEZ × post 2006	0.547	0.471	0.553	0.233	0.016	0.038
	(0.066)	(0.049)	(0.069)	(0.038)	(0.009)	(0.014)
Panel B. Controlling for covariates						
SEZ × post 2006	0.633	0.495	0.602	0.266	0.016	0.039
	(0.071)	(0.051)	(0.070)	(0.042)	(0.009)	(0.014)
Area FEs	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood-year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,348	2,348	2,348	2,348	3,714	4,710

Notes: Observations are at the area-year level within 1,000 meters of a zone boundary. In columns 1–6, the dependent variable is the natural log of the measure of capital, employment, output, number of firms, productivity, and wage rate, respectively. In columns 1–4, census data from 2004 and 2008 are used. In column 5, ASIF data from 2004 to 2007 are used. In column 6, ASIF data from 2004 to 2008 are used. Area fixed effects and neighborhood-year fixed effects are included in the specification. Covariates include area-level characteristics, which are averaged from the villages in the area, interacted with the year dummy. The standard errors are clustered at the zone level, reported in parentheses.

TABLE 6—SPILLOVER EFFECT

Dependent variable	log capital (1)	log employment (2)	log output (3)	log number of firms (4)	log productivity (5)	log wage rate (6)
Spillover effect	0.032	0.073	0.119	0.052	−0.001	0.025
	(0.046)	(0.034)	(0.054)	(0.023)	(0.005)	(0.020)
Additional direct effect	0.663	0.353	0.547	0.375	0.017	0.029
	(0.062)	(0.049)	(0.065)	(0.040)	(0.010)	(0.023)
Covariates × year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Village FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	1,779	1,779	1,779	1,779	1,577	1,587
Observations	142,202	142,202	142,202	142,202	82,716	109,088

Notes: All observations are at the village-year level. In columns 1–4, census data from 2004 and 2008 with full sample of 103,263 villages in 1,779 counties including both SEZ counties and non-SEZ ones are used for analysis. In column 5, ASIF data from 2004 to 2007 with full sample of 32,149 villages in 1,577 counties are used. In column 6, ASIF data from 2004 to 2008 with full sample of 33,826 villages in 1,587 counties are used. In columns 1–6, the dependent variable is the natural log of the measure of capital, employment, output, number of firms, productivity, and wage rate, respectively. Covariates include village-level characteristics listed in panel A of Table 2. The standard errors are reported in parentheses, clustered by county. All regressions control for village fixed effects and year fixed effects.

are quantitatively comparable to the benchmark estimates in panel B of Table 3 and to the spillover estimates in Table 6. The estimated impact of spillover externality is negligible in terms of capital, employment, output, and number of firms, though there are small spillover effects in terms of productivity and wages. The F-tests of the joint significance of the spillover effects show that they are statistically insignificant in all outcomes except for productivity.

Taken together, these exercises suggest that the baseline estimates of the SEZs' effects are not significantly biased by the relatively small spillovers from SEZ villages to non-SEZ villages nearby. In a closely related study, Zheng et al. (2017) shows that industrial parks generate net growth rather than a simple reshuffling of economic activity from the rest of the city. Our findings largely echo theirs but with notably less spillover.[16] In other related work, Chaurey (2017) examines the microlevel impact of a location-based tax incentive scheme in India. He reports finding large increases in employment, total output, fixed capital, and the number of firms, and finds no evidence of firms' relocating or of spillover of industrial activity between the treatment and control areas. His results are generally consistent with those of this study, probably because the two countries have similar levels of market development.

Using more aggregated city-level data, Wang (2013) has found that the majority of the direct investment attracted to China from overseas by the SEZs between 1978 and 2008 is new activity rather than simply relocation from non-SEZ areas. And Alder, Shao, and Zilibotti (2016) also finds no evidence of beggar-thy-neighbor effects on GDP using data from 1988 to 2010. They report positive spillover from the state-level zones to nearby cities, which become stronger during the first 10 years, but no significant effect of provincial-level zones. Overall, these city-level findings to some extent confirm the limited negative spatial interactions detected in this study, along with the fairly small spillovers from provincial-level SEZs to nearby areas, at least in the short run.

V. Decomposition

These findings establish that SEZ areas had more invested capital, employment, and output, attracted more firms, and showed increased productivity and higher wage rates than non-SEZ areas in the period studied. China's SEZ program significantly reduces the costs of capital and land and tax rates within the zone areas, which seems to have significantly influenced firms' location choices and investment decisions.

Firms could have responded to the SEZ policy initiatives by varying inputs and outputs (along the intensive margin) and by entering or exiting a zone (along the extensive margin). To further illuminate such decisions, the SEZ effects are decomposed into an extensive margin effect attributable to new entrants and exiters and an intensive margin effect generated by continuing firms.

A. *Framework*

The decomposition method developed by Foster, Haltiwanger, and Krizan (2001) and Foster, Haltiwanger, and Syverson (2008) is applied to the TFP and wage rates data. It separates the changes in the weighted average of firm-level productivity (or wages) into four margins. TFP will serve as an illustration. For each village v (the index is omitted), aggregate TFP in year t is

$$TFP_t = \sum_i \varphi_{it} tfp_{it},$$

[16] Zheng et al. (2017) finds that in an impact area extending 2 km from an industrial park's boundary, the spillovers in terms of TFP and wages are 12 percent and 6 percent, respectively.

where φ_{it} is the employment share of firm i in year t; and tfp_{it} is firm i's TFP in year t. The changes in aggregate TFP before and after the implementation of the SEZ program can then be decomposed as

$$(5) \qquad \Delta TFP_t = \underbrace{\sum_{i \in C} \varphi_{it-1} \Delta tfp_{it}}_{\text{within}} + \underbrace{\sum_{i \in C} (tfp_{it-1} - TFP_{t-1}) \Delta \varphi_{it}}_{\text{between}} + \underbrace{\sum_{i \in C} \Delta tfp_{it} \Delta \varphi_{it}}_{\text{cross}}$$

$$+ \underbrace{\sum_{i \in N} \varphi_{it}(tfp_{it} - TFP_{t-1}) - \sum_{i \in X} \varphi_{it-1}(tfp_{it-1} - TFP_{t-1})}_{\text{net entry}},$$

where C, N, and X, represent the sets of continuing, entering, and exiting firms, respectively. The terms on the right side of equation (5) represent, in the order of their inclusion, the within-firm effect, the between-firm effect, the cross effect, and the net entry effect.

The decomposition of outcomes like capital, employment, and output is a bit different, as it tracks changes in the logarithm of the total summation. The technique embodies the spirit of Foster, Haltiwanger, and Krizan's (2001) and Foster, Haltiwanger, and Syverson (2008)'s method. The treatment of output will serve as an illustration. For each village v,

$$\ln S_t = \ln\Big(\sum_i s_{it}\Big).$$

Hence, the decomposition becomes

$$(6) \qquad \Delta \ln S_t = \Delta \ln\Big(\sum_i s_{it}\Big)$$

$$= \Delta \ln\Big(\sum_{i \in C} s_{it} + \sum_{i \in N/X} s_{it}\Big) = \Delta \ln\big(S_t^C + S_t^{N/X}\big)$$

$$\simeq \underbrace{\varphi \Delta \ln S_t^C}_{\text{within}} + \underbrace{(1 - \varphi) \Delta \ln S_t^{N/X}}_{\text{net entry}},$$

where $\varphi = \big(S_{t-1}^C / S_{t-1} + S_t^C / S_t\big)/2$. The two terms on the third line of equation (6) represent the within-firm effect and the net entry effect, respectively.

With these decompositions, each term (ΔY_v) can then be regressed on the SEZ program indicator along with the baseline controls, i.e.,

$$(7) \qquad \Delta Y_v = \gamma^v D_v + (\lambda_{ct} - \lambda_{ct-1}) + \mathbf{X}_v' \boldsymbol{\eta} + \Delta \varepsilon_v,$$

which isolates how much each decomposition term contributes to the SEZ's total impact.

B. *Results*

The decomposition analyses require distinguishing the continuing firms, entrants, and exiters. Each group is traced from 2004 to 2007–2008 using the firm IDs, names, addresses, and other information to carefully pin down the firm dynamics.[17] The capital, employment, and output decompositions use the 2004 and 2008 census data. For productivity decomposition, the ASIF 2004 and 2007 data are exploited. For wage rate decomposition, the ASIF data for 2004 and 2008 are used.

The decomposition results for capital, employment, and output are presented in panel A of Table 7. Column 1 reports the total effects of the SEZ program. Columns 2 and 3 are the within-firm effects and the net entry effects, respectively. Most of the SEZ effects arise through firm births and deaths. They account for 80 percent of the changes in capital and employment, and 90 percent of the changes in output. The decomposition results for TFP and wages are reported in panel B, with column 1 for the total effects, and columns 2–5 for the within-firm effects, the between-firm effects, the cross effects, and the net entry effects, respectively. Within-firm effects and between-firm effects are negligible. The net entry effects are statistically and economically significant, consistent with the patterns of capital, employment, and output.

Overall, this decomposition indicates that the zones mostly promoted extensive margin effects. This result agrees with the findings of Givord, Rathelot, and Sillard (2013) who find no evidence of an employment effect on existing businesses. Employment growth in their sample comes mostly from new businesses and firms that relocated. Criscuolo et al. (2019) do, though, find a large and statistically significant average effect of the UK's RSA program on employment and investment, with about half of the effects arising from incumbent firms growing (the intensive margin) and half caused by net entry (the extensive margin). However, in interpreting our results about the extensive margin, we caution that some firm births could be considered relocations if the SEZs attracted newly born firms from other regions. Some investors may simply have changed their location choices in establishing a new firm in response to an SEZ.

VI. Cost-Benefit Analysis

The previous analyses document beneficial effects of the SEZ program, but the aggregate welfare implications of the program remain unclear, given its costs and possible redistribution of economic activity. We investigate that important issue using a flexible back-of-the-envelope cost-benefit estimation technique proposed by Busso, Gregory, and Kline (2013) and subsequently applied by Chaurey (2017). The SEZ program's main benefits have included increasing firms' profits, raising

[17] For firms that reported unique IDs (their legal person codes) in the census data, the tracing involves matching their firm ID in the 2004 and 2008 censuses. For firms with multiple IDs, the firm name is used to link observations over time. Firms may receive a new ID as a result of restructuring, merger, or privatization. For a firm for which no observation with the same ID could be identified, as much information as possible on the firm's name, location code, the name of its legal representative person, phone number, and so on is used to find a match. A similar approach is applied in tracing firms in the ASIF data for 2004 and 2007–2008.

Table 7—Decomposition

	Total growth (1)	Within (2)	Net entry (3)	(4)	(5)
Panel A					
log capital	0.579	0.129	0.485		
	(0.034)	(0.015)	(0.038)		
log employment	0.345	0.010	0.304		
	(0.028)	(0.011)	(0.030)		
log output	0.492	0.098	0.402		
	(0.038)	(0.018)	(0.040)		
	Total growth	Within	Between	Cross	Net entry
Panel B					
log productivity	0.015	0.001	0.001	−0.013	0.026
	(0.011)	(0.007)	(0.003)	(0.004)	(0.011)
log wage rate	0.048	0.004	0.002	−0.061	0.104
	(0.022)	(0.025)	(0.005)	(0.016)	(0.019)

Notes: In panel A, census data in 2004 and 2008 aggregated at the village level are used for analysis. In panel B, ASIF data in 2004 and 2007 aggregated at the village level are used for analysis on log productivity; ASIF data in 2004 and 2008 aggregated at the village level are used for analysis on log wage rate. The SEZ effects are decomposed into: new entrants and exiters, or the extensive margin effect; and continuing firms, or the intensive margin effect. SEZ effects are estimated using equation (7). Covariates listed in panel A, Table 2, are included in all specifications. The standard errors are clustered at the county level, reported in parentheses.

workers' wages, and generating rental income for landlords. It is shown that in this sample, the increase in housing rents is negligible,[18] so the analysis focuses on wages and profits.

The effects of a zone on corporate profits $(\pi^{corporate})$ and wage bills (π^{wage}) are estimated using the county-level DD estimation. Table 8 reports the results.

Based on the estimated zone effect $(\hat{\gamma}^{corporate}; \hat{\gamma}^{wage})$, the counterfactual corporate profits $(\tilde{\pi}^{corporate})$ and wage bills $(\tilde{\pi}^{wage})$ are calculated as: $\tilde{\pi}^{corporate} = \pi^{corporate}/(1+\hat{\gamma}^{corporate})$ and $\tilde{\pi}^{wage} = \pi^{wage}/(1+\hat{\gamma}^{wage})$. Here, $\hat{\gamma}^{corporate}$ and $\hat{\gamma}^{wage}$ are the estimated zone effects on corporate profits and wage bills. The program's benefits can then be expressed as the total difference between the actual and counterfactual figures: $\pi^{corporate} - \tilde{\pi}^{corporate}$ for profits and $\pi^{wage} - \tilde{\pi}^{wage}$ for wages.

Table 9 shows that the actual corporate profits are 168.67 billion RMB in 2006, 223.76 billion RMB in 2007, and 253.29 billion RMB in 2008. The estimated benefits increases in corporate profits linked to the zone program are then 23.20 billion RMB in 2006, 30.77 billion RMB in 2007, and 34.83 billion RMB in 2008. Similarly, the actual wage bills are 155.23 billion RMB in 2006, 193.52 billion RMB in 2007, and 261.69 billion RMB in 2008. So the calculated benefits in terms of wage bills are 24.96 billion RMB, 31.11 billion RMB, and 42.08 billion RMB. Adding the two categories of benefits and using a discount rate of 3 percent, the total gains from the zone program are roughly 180.73 billion RMB (or US$22.60 billion).

[18] Data on housing costs from 2004 to 2008 are only available on the city level (one administrative level above a county), so the analysis of any SEZ effect on housing costs is conducted on the city level. Table A4 in the online Appendix shows that there is no significant effect. That result is consistent with Wang's analyses of SEZs from 1978 to 2008, which find that any increase in housing rents is negligible (Wang 2013).

Table 8—SEZ Effects: Corporate Profits and Wage Bills

Dependent variable	log corporate profits (1)	log wage bills (2)
SEZ × post 2006	0.159	0.192
	(0.066)	(0.029)
Covariates × year dummies	Yes	Yes
County FEs	Yes	Yes
Year FEs	Yes	Yes
Number of clusters	1,428	1,502
Observations	6,363	7,440

Notes: All observations are at the county-year level. ASIF data from 2004 to 2008 are aggregated for the analysis. Covariates include initial county-level characteristics listed in panel B, Table 2. The standard errors are clustered at the county level, reported in parentheses. All regressions control for county and year fixed effects.

Table 9—Cost and Benefit Analysis: A Back-of-the-Envelope Approach

Year	Actual value (billion RMB) (1)	SEZ effect (2)	Counterfactual value (billion RMB) (3)	Benefits (billion RMB) (4)
Panel A. Benefits				
(1) Corporate profits				
2006	168.67	0.1595	145.47	23.20
2007	223.76	0.1595	192.99	30.77
2008	253.29	0.1595	218.45	34.83
(2) Wage bills				
2006	155.23	0.1916	130.27	24.96
2007	193.52	0.1916	162.41	31.11
2008	261.69	0.1916	219.62	42.08

Year	Counterfactual corporate profits (billion RMB)	Statutory tax rate	Actual taxes paid (billion RMB)	Costs (billion RMB)
Panel B. Costs (corporate tax)				
2006	145.47	33%	32.14	15.87
2007	192.99	33%	40.38	23.30
2008	218.45	25%	36.25	18.36

Notes: The calculations on benefits and costs of the SEZ program using a back-of-the-envelope approach are shown in the table. See Section VI for details.

The corporate tax forgone can be estimated as the difference between the counterfactual taxes and actual taxes paid. The counterfactual taxes can be estimated as counterfactual corporate profits ($\tilde{\pi}^{corporate}$) × the statutory tax rate (33 percent in 2006 and 2007; 25 percent in 2008). As Table 9 shows, the estimated corporate tax breaks are 15.87 billion RMB in 2006, 23.30 billion RMB in 2007, and 18.36 billion RMB in 2008. Using a discount rate of 3 percent produces a total cost of 55.80 billion RMB (US$6.98 billion). Comparing the costs and the benefits yields an estimated net benefit of 124.93 billion RMB (or US$15.62 billion) from the zone program.

Table 10—Heterogeneous Effects of the SEZ Program

Dependent variable	log capital (1)	log employment (2)	log output (3)	log number of firms (4)	log productivity (5)	log wage rate (6)
Panel A. Capital versus labor-intensive industries						
Differential treatment impact	0.106	0.067	0.109	0.025	0.010	0.002
	(0.060)	(0.049)	(0.066)	(0.034)	(0.014)	(0.026)
Panel B. SEZ counties with good versus poor infrastructure						
Differential treatment impact	−0.045	0.003	−0.012	−0.068	−0.017	0.021
	(0.072)	(0.058)	(0.084)	(0.042)	(0.014)	(0.026)
Panel C. Firms with large versus small size						
Differential treatment impact	0.065	0.055	0.029	0.049	0.004	−0.006
	(0.047)	(0.036)	(0.053)	(0.019)	(0.013)	(0.026)

Notes: All observations are at the village-year level. In columns 1–4, census data from 2004 and 2008 are used for analysis. In column 5, ASIF data from 2004 to 2007 are used. In column 6, ASIF data from 2004 to 2008 are used. Panels A, B, and C report the differences in the SEZ effects between capital-intensive and labor-intensive industries, between SEZs with good and poor infrastructure, and between large and small firms. Capital and labor intensity are defined at the four-digit level based on a capital-labor ratio above or below the median value in 2004. SEZ counties with good (poor) infrastructure index are those with infrastructure indices above (below) the median in 2004; a larger index indicates better infrastructure. Firms with large (small) size are those with sales above (below) the median in 2004.

VII. Heterogeneous Effects

Operating in an SEZ could have different impacts on firms with different characteristics, operating in different zones, and different industries. Industry variations in capital-labor ratios, zones' different access to transportation, and different firm sizes allow examining possible differences in the impact. All of these tests are conducted with the village-level data using the same set of controls as in the benchmark village-level DD analysis.

Capital-Intensive versus Labor-Intensive Industries.—Because of the capital cost reductions available, firms in capital-intensive sectors may have been more likely to benefit from a zone program and to have derived greater benefits. To investigate that possibility, the industries are categorized based on whether their average capital-labor ratios in 2004 were above or below the sample median. The results estimating the differential impact on the two groups are reported in panel A of Table 10 (see Table A5 in the online Appendix for the estimates from the two subsamples).

Most of the SEZ effects are indeed consistently stronger in the capital-intensive industries. In absolute terms, after the implementation of an SEZ, there is on average 10.6 percent more capital investment and 10.9 percent larger output among the capital-intensive industries compared to the labor-intensive ones. Those results are in line with the features designed into the SEZ programs, which typically subsidize capital investment.

Good versus Poor Infrastructure.—Most firms trade in multiple markets. The level of economic activity in a location depends in part on that location's access to markets for its goods (Hanson 2005). Good airports and highways help reduce firms' trade and communication costs, so proximity to markets and good infrastructure should make a zone more attractive (Graham, Gibbons, and Martin 2010; Combes and Gobillon 2015). To investigate whether there are such differential effects, an infrastructure index is constructed for each SEZ county. It includes the county's distance from the nearest airport and port, the highway density of the city in which the county resides, and the county's market potential. For each SEZ county, its distance from the nearest airport and port is measured, and those distances are ranked from greatest to least. That yielded the sub-indices (*rank_airport* and *rank_port*). The county's highway density is ranked from lowest to highest to obtain the subindex (*rank_highway*).[19]

A county's market potential is then quantified using a technique in the spirit of Harris (1954) and of Rogers (1997). The impact of trade and communication costs is assumed to increase with the inverse of a county's distance from all the other counties within the same province. The market potential MP_s of SEZ county s is therefore defined as

$$MP_s = \frac{\sum_{c \in PROV} GDP_c / dist_{cs}}{\sum_{c \in PROV} GDP_c},$$

where $PROV$ denotes a province, c denotes a county, GDP_c stands for county c's gross domestic product (GDP), and $dist_{cs}$ is the distance between SEZ county s's administrative headquarters and county c.[20] Following Briant, Lafourcade, and Schmutz (2015), the weighted sum of the markets accessible from an SEZ county is divided by the total size of all of the markets in the province to mitigate the impact of large counties. The SEZ counties' market potentials are then ranked from lowest to highest, resulting in the fourth subindex (*rank_mp*).

A zone's infrastructure index is then the average of the ranks associated with the four dimensions: $rank = (rank\_airport + rank\_port + rank\_highway + rank\_mp)/4$, so a larger index indicates a zone with better infrastructure. The SEZ counties are then divided into two groups based on whether their infrastructure index in 2004 was above or below the sample median.

The estimation results for the difference in policy impact on the two groups are reported in panel B of Table 10 (see online Appendix Table A6 for additional details). No statistically or economically significant differences in the SEZ effects are found between the zones with good and poor infrastructure. That implies that accessibility and the surrounding region's market potential are not critical factors in determining a zone's impact.

Firms of Large versus Small Size.—To investigate whether firm size is important in determining a zone's beneficial effects, the firms are sorted into two groups based

[19] The list of airports is compiled from China's 2005 Transportation Yearbook, while the data on highway density (miles of highways divided by the land area of the city) are from China's 2005 Regional Statistical Yearbook.

[20] Note that both Harris (1954) and Rogers (1997) use a city as the regression unit, and their market potential for a city is the weighted average of the GDPs of the other cities. In China, economic zones are smaller units than counties. The county where an economic zone resides is, therefore, also included in the calculation of market potential.

on whether their total sales in 2004 are above or below the sample median. They are then aggregated to the village level for estimation. The differential impact between large firms and smaller ones is reported in panel C of Table 10 (see online Appendix Table A7 for additional details). The SEZs tends to attract larger firms. That, too, echoes the program's design, in that larger firms are more likely to make large capital investments. But there are no statistically significant differences between small and large firms in terms of the other outcomes.

Taken together, these results indicate that capital-intensive firms benefit more from the zone program than labor-intensive ones, but the effects of an SEZ are quite similar regardless of an SEZ's accessibility and for firms of different size. That resonates with the findings of previous work, which has emphasized the characteristics of the industry in analyzing the effects of place-based policies (Criscuolo et al. 2019, Combes and Gobillon 2015). But these findings contrast with previous findings on the role of regional characteristics, for example, those of Briant, Lafourcade, and Schmutz (2015). They do, though, compare well with the work by Alder, Shao, and Zilibotti (2016), who find in their study of Chinese SEZs that market access had no significant relationship with a city's GDP. One possible explanation is that the later SEZs established in the wave of 2006 were less subject to selection compared to the earlier waves. The characteristics of their locations may not therefore have differed as much. Overall, these findings suggest that the complementary roles of regional and industry characteristics in place-based development programs may hinge on the specific context.

VIII. Conclusions

This study exploits a natural experiment involving the establishment of China's economic zones, which targeted firms rather than individuals. By focusing on a prominent place-based policy in China, the study has addressed whether or not zones work, for whom, and also what works and where (Neumark and Simpson 2014). It does so by constructing a dataset with geo-coded information about firms with relatively fine granularity. The findings constitute the first compelling evidence about the local economic effects of zones, their benefits and costs, and some determinants of program effectiveness, at least in China. Given the large number of developing countries implementing similar zone programs, the findings have important implications for policy (Akinci and Crittle 2008) and the design of more effective SEZs.

China's zone programs have demonstrated a large effect on the targeted areas in terms of extensive margins, especially via entries and exits. Existing firms have experienced limited improvement in their performance. There have also been productivity benefits and price impacts arising from locating in an SEZ, which indicate the presence of agglomeration economies. There are relatively limited spillovers in industrial activity between SEZs and non-SEZ areas.

In monetary terms, the program is estimated to have brought a net benefit of US$15.62 billion within three years of its implementation. These findings may help to dispel the general pessimism about zone programs in developing countries.

Another important finding is that a zone's effectiveness depends crucially on the design of its policies. China's economic zones offer various subsidies for capital investment, and operating in a zone is significantly more beneficial for capital-intensive firms than for more labor-intensive ones. Zones with better market potential or better access do not demonstrate significantly larger benefits. That finding serves as a reminder that formulating effective policy requires paying close attention to the circumstances of the agents to be influenced.

This study has been a first step toward understanding the micro-foundations of place-based policies in developing countries. Much remains to be done. This study evaluates only short-term effects (two years after the zones' establishment) due to data limitations. Further efforts should more precisely investigate the long-term impacts of the zones with a structural approach and better data. It would be interesting, in particular, to uncover any links between local political, economic, and social institutions and the effects of zones (Becker, Egger, and von Ehrlich 2013).[21] Such analyses would undoubtedly be of great benefit in defining how SEZ policy interventions should best be implemented in specific contexts.

Appendix

A. *Five Waves of Economic Zone Formation*

The waves of zone establishment shown in Figure 1 are as follows.

1979–1983.—In the late 1970s, China's State Council approved small-scale SEZ experiments in four remote southern cities: Shenzhen, Zhuhai, and Shantou in Guangdong Province, as well as in Xiamen in Fujian Province. China started with virtually no foreign direct investment and almost negligible foreign trade before 1978, so those zones were considered a test base for the liberalization of trade, tax, and other policies nationwide.

1984–1991.—Supported by the initial achievements of the first group of SEZs, the central government expanded the SEZ experiment in 1984. Fourteen other coastal cities were opened to foreign investment. From 1985 to 1988, the central government included even more coastal municipalities in the SEZ experiment. In 1990, the Pudong New Zone in Shanghai joined the experiment along with other cities in the Yangtze River valley. An important pattern of this economic zone granting wave is that cities with better geographical locations, industrial conditions, and human capital were selected. Forty-six national-level development zones and 20 province-level development zones were established from 1984 to 1991.

1992–1999.—After Deng Xiaoping's famous southern tour in 1992, the State Council opened several border cities and all the capital cities of the inland provinces and autonomous regions. This period witnessed a huge surge

[21] Becker, Egger, and von Ehrlich (2013) investigates the heterogeneity among EU member states in terms of their ability to utilize transfers from the European Commission. Only regions with sufficient human capital and good-enough institutions are able to turn transfers into faster per capita income growth and more per capita investment.

in the establishment of development zones. Ninety-three national-level development zones and 466 province-level development zones were created within municipalities to provide better infrastructure and achieve agglomeration of economic activity. As a result, a multi-level and diversified pattern of opening coastal areas and integrating them with river, border, and inland areas took shape in China.

2000–2004.—From 2000, aiming at reducing regional disparity, the State Council launched the Western Development Strategy, China's first comprehensive regional development plan to boost the economies of its western provinces. The success of the coastal development zones demonstrated their effectiveness in attracting investment and boosting employment. As a result, more development zones were granted by the central authorities and the provincial governments in inland cities. China's entry into the World Trade Organization in 2001 led to an increasing number of national-level export processing zones and bonded zones. In total, 64 national-level development zones and 197 province-level development zones were established between 2000 and 2004.

2005–2008.—From 2005, an additional 682 SEZs were established. In terms of their geographical distribution, 338 were in coastal areas, 269 in central areas, and 75 in western areas. In terms of granting authority, 19 national-level development zones and 663 province-level development zones were formed.

B. *Identifying Each 12-Digit Location Code within a Zone's Boundaries*

Each firm's administrative location code is used to locate it as either within an SEZ or not. These three cases summarize the process:

(i) Some SEZs have their own administrative codes. For example, Nanling Industrial Zone (zone code: S347063) in Anhui has an independent 12-digit administrative location code: 340223100400 (Anhui Nanling Industrial Zone Community).

(ii) Some zones are coterminous with a town or a Chinese administrative area termed a street. All villages or communities under the town or street will then be within the zone's boundaries. For example, Fei County Industrial Zone (zone code: S377099) in Shandong encompasses all of Tanxin town (administrative location code: 371325105). The nine-digit town code is enough to pin down its zone status.

(iii) Some zones take in several villages or communities. For example, Yunmeng Economic Development Zone (zone code: S427040) in Hubei administers eight villages and one community: Xinli Village, Heping Village, Qianhu Village, Hebian Village, Zhanqiao Village, Quhu Village, Zhaoxu Village, Sihe Village, and Qunli Community. An enterprise in any of them will be within the zone.

C. *Estimation of Firm TFP*

Consider the following Cobb-Douglas production function (in logarithmic form):

$$y_{it} = \beta_l l_{it} + \beta_k k_{it} + \beta_m m_{it} + \omega_{it} + \epsilon_{it}, \tag{A1}$$

where y_{it} is the logarithm of firm output, and l_{it}, k_{it}, and m_{it} are the logarithms of the employment, capital, and materials inputs. The variable ω_{it} is the firm's productivity, and ϵ_{it} takes in measurement error and any unanticipated shocks to output.

Obtaining consistent production function estimates $\boldsymbol{\beta} = (\beta_l, \beta_k, \beta_m)$ requires controlling for unobserved productivity shocks potentially leading to simultaneity and selection biases. A control function based on a static input demand function is used as a proxy for the unobserved productivity.

The control function approach initiated by Olley and Pakes (1996) and extended by Levinsohn and Petrin (2003) is applied. The following material demand function is used as a proxy for the unobserved productivity:

$$m_{it} = m_t(\omega_{it}, l_{it}, k_{it}). \tag{A2}$$

Inverting (A2) yields the control function for productivity:

$$\omega_{it} = h_t(l_{it}, k_{it}, m_{it}).$$

In the first stage, unanticipated shocks and measurement errors (ϵ_{it}) are purged by estimating the following equation:

$$y_{it} = \phi_t(l_{it}, k_{it}, m_{it}) + \epsilon_{it}, \tag{A3}$$

which yields a predicted output ($\hat{\phi}_{it}$).

Equations (A1) and (A3) from the first-stage estimation can then be used to express productivity:

$$\omega_{it}(\boldsymbol{\beta}) = \hat{\phi}_{it} - \beta_l l_{it} - \beta_k k_{it} - \beta_m m_{it}. \tag{A4}$$

To estimate the production function coefficients $\boldsymbol{\beta}$, the technique of Ackerberg, Caves, and Frazer (2015) is applied and moments are formed based on innovation in the productivity shock ξ_{it} in law of motion for productivity:

$$\omega_{it} = g(\omega_{it-1}) + \xi_{it}.$$

Using (A4), $\omega_{it}(\boldsymbol{\beta})$ is non-parametrically regressed against $g(\omega_{it-1})$ to obtain the innovation term $\xi_{it}(\boldsymbol{\beta}) = \omega_{it}(\boldsymbol{\beta}) - E(\omega_{it}(\boldsymbol{\beta}) | \omega_{it-1}(\boldsymbol{\beta}))$.

The moment conditions used to estimate the production function coefficients are then

$$E\left(\xi_{it}(\beta)\mathbf{Y}_{it}\right) = 0,$$

where $\mathbf{Y}_{it}$ contains lagged labor and materials, and current capital.[22]

Once the production function coefficients $\hat{\boldsymbol{\beta}} = \left(\hat{\beta}_l, \hat{\beta}_k, \hat{\beta}_m\right)$ have been estimated, a firm's total factor productivity can be computed as

$$\hat{\omega}_{it} = \hat{\phi}_{it} - \hat{\beta}_l l_{it} - \hat{\beta}_k k_{it} - \hat{\beta}_m m_{it}.$$

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[22] Following the lead of other scholars, labor and materials are treated as flexible inputs and their lagged values are used to construct the moments. Capital is considered a dynamic input with adjustment costs, so its current value is used in forming the moments.

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