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Place-based policies and firm performance: Evidence from Special Economic Zones in India[☆]

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ABSTRACT

This paper exploits time and geographic variation in the adoption of Special Economic Zones in India to assess the direct effects of the program on firm performance. We combine geocoded firm-level data and geocoded SEZs. Our analysis yields that conditional on controlling for initial selection based on observables, the establishment of new SEZs did not induce any discernible positive effect on the productivity growth of firms in the SEZs. To explain this, we focus on the possibility of distortions through non-profitable activities on the part of managers. We find that firms especially in publicly-owned SEZs decreased their productivity growth, while firms located in privately-owned SEZs experience productivity increases. We also show that directors of firms located inside the publicly-owned zones experienced a significant increase in their salary growth, which is not the case in privately-owned SEZs. Our findings are in line with the idea that the possibility of rent-seeking by managers leads to distortions in program implementation.

1. Introduction

Place-based policies – a governmental tool used to enhance the economic growth of a particular area – have become increasingly popular among many policy-makers worldwide in the past few decades. Much of the research has focused on analyzing the effectiveness of these programs in developed countries, where the public resources target predominantly distressed regions (Busso et al., 2013; Kline and Moretti, 2014). There are only a few studies that evaluate such policies in developing countries, possibly due to lack of data. However, insights gained from programs in lagging regions in developed countries may not hold when examining programs in emerging economies since policies there generally target the most advantageous areas.

In this paper, we evaluate one of the most popular industrial policy tools used in the last two decades: Special Economic Zones (SEZs). SEZs constitute geographically delineated areas where fiscal incentives and regulatory frameworks are provided with the main goal to attract investments and generate additional economic activity in the region. A World Bank report states that within the zones, governments aim to create new firms and jobs and facilitate skills and technology transfers. This can generate synergies, networks and knowledge spillovers to stimulate the economic growth of the region (Group World Bank, 2017). However, such

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benefits may not materialize if SEZs lead to a misallocation of resources, in particular, if this is due to political interference (Alkon, 2018).

This paper provides novel evidence on the effects of SEZs in one of the fastest growing emerging economies — India. Partly in response to the apparent success of China's SEZs, the government of India introduced the 2005 SEZs Act with the view to attract investments, generate a big push for infrastructure development and thus facilitate economic growth. Over fifteen years since the launch of the program, 354 SEZs have been notified hosting over 5,600 units that provide employment to 2.5 million people (Factsheet on SEZs, Department of Commerce).¹

We use firm-level data to estimate the impact of the establishment of an SEZ on the productivity growth of firms. While there is a small but growing literature evaluating the impact of SEZs in various countries, most studies use data at some aggregated administrative unit level (e.g., Wang (2013) and Alkon (2018)).² Only very few papers use firm-level data (Brooks et al., 2021a; Steenbergen and Javorcik, 2017; Nazarczuk, 2018) and, to the best of our knowledge, none does so for India. Zooming in on the firm level arguably allows a more precise estimation of the impact of SEZs, taking into account firm heterogeneity.

While governments clearly expect SEZs to yield positive effects on development, whether such effects in fact materialize is an open question. Conceptually, firms within an SEZ may be expected to be able to boost their growth performance. They are able to benefit from certain incentives (e.g., tax reductions) which allow them to generate surplus vis-a-vis non-SEZ firms, which can be invested in innovation or other productivity-enhancing improvements. However, positive direct benefits may not necessarily materialize, if the establishment of an SEZ leads to a misallocation of productive resources or excessive rent-seeking on the part of the firm's owners or managers. This indicates the need for careful economic analysis of the potential benefits of SEZs at the level of the firm.

To fix ideas and motivate our empirical analysis, we begin with providing a simple theoretical framework on how the possibility of easy engagement with policy makers on the part of firms' managers in the context of an SEZ may affect firm performance. Our model follows closely (Dal Bó and Rossi, 2007) and Hao et al. (2020), where a representative firm produces output using capital and managerial effort. Managers can allocate their time between productive activity, which improves production efficiency of a firm, and a non-productive activity, such as rent-seeking or lobbying. Engaging in a non-productive activity yields some benefits to the firm or a manager, but comes at the expense of lower productivity. The easier it is for a manager to engage with a policy maker or a bureaucrat, the greater the marginal return to a non-productive activity.

We show that in such a framework, managerial effort may be diverted away from productive to a non-productive activity, which, consequently, decreases firm's productivity, and increases the benefits from a non-productive activity. Intuitively, in an environment where political engagement is easy, such as in firms in SEZs where managers are in frequent contact with policy makers and bureaucrats, managers may be willing to allocate more time to a non-productive activity because they expect to get higher rewards. But exerting such effort is costly, hence managers will decrease their time spent on a productivity-improving activity which will negatively affect measured firm performance.

Guided by our theoretical framework, we proceed with the empirical implementation. An important challenge confronting research aiming to assess the SEZs impact on firm performance is the unavailability of data on firms operating inside the zones. For India, detailed information on the actual SEZ (e.g. location, size, establishment year, etc.) is publicly available, though there is no information on which firms are located within the SEZs. We overcome this issue by first, geocoding the SEZs, and the firms in our dataset. We then combine these two data sources based on the geocoding using a concentric ring approach. Thus, spatial rings around the centroid of SEZs are created using the information on the size of the zone. This allows us to approximate firms inside an SEZ. The use of these fine-grained spatial data thus allows us to identify any potential effect on firms inside SEZs.

To the best of our knowledge, this study is the first to assemble a representative geocoded firm-level dataset with an assigned SEZ status for India. The final dataset consists of an unbalanced panel of firms which includes information on firm characteristics, a firm's SEZ status, the industry in which SEZ specializes and the date of notification of the SEZ. Firm-level data are obtained from Prowess - a database on the financial performance of Indian companies, collected by the Centre for Monitoring the Indian Economy (CMIE). The data cover periods before the implementation of the program, starting from 1988, and after the SEZs creation, up until 2020.

We exploit the longitudinal structure of the data and compare the performance of firms before and after the introduction of the SEZ program. To do so we employ a difference-in-differences methodology combined with an inverse probability weighting technique in an attempt to identify an effect of establishing an SEZ on firm performance.³ Since SEZs are established in more developed regions, a simple mean comparison of treated and untreated firms would lead to biased estimates due to a positive selection bias. Another estimation issue is that, because applications for developing an SEZ are reviewed on a rolling basis, firms are treated in different years over the period 2006–2020, resulting in a staggered treatment introduction. Thus, to correct for selection bias, we utilize a recently developed methodology for time-varying treatments, employing an inverse propensity score re-weighting approach, where weights are created at each point in time conditional on the development of the outcome variable as well as other time-varying variables. In this way, we create a pseudo-population where the treatment assignment at each point in time is orthogonal to the potential outcomes conditional on the pre-treatment observable covariates (Thoemmes and Ong, 2016; Girma and Görg, 2022). The control group is restricted to the matched sub-sample of firms located further than 40 kilometers away from the zones, to alleviate concerns that the outcomes of the untreated control group are affected by the treatment.

¹ Available under: http://sezindia.nic.in/upload/634908b5af04cImage_002.pdf Accessed on 29.02.2020.

² With data aggregated at such a geographical level, a distinction between inside and outside SEZs is not possible, of course.

³ Such an identification approach is also employed in, e.g., Guadalupe et al. (2012), Girma et al. (2015) and Brucal et al. (2019).

The analysis yields the following results. Conditional on controlling for initial selection based on observables, we do not find that the establishment of an SEZ yields any productivity growth effects on firms inside an SEZ. This result is robust to a more stringent definition of the treated group and alternative control groups. It also does not change if we use the Annual Survey of Industry as an alternative data source, or if we look at informal firms. In order to provide potential explanations for the estimated effect, we first distinguish SEZs into those developed by the state and those by private developers. We find that firms located in privately-owned SEZs are affected differently from those in publicly-owned SEZs: While the former experience productivity increases after the establishment of the SEZs, firms in publicly-owned SEZs experience decreases in productivity. Our findings thus suggest that the possibility for non-productive activities on the part of managers, and possibly government interference may play a role in determining productivity effects of SEZs.

To zoom in more on the role of such non-productive activities, we look at whether directors increased their salaries even in the absence of productivity growth effects. We find that this is indeed the case: directors of firms located inside newly established zones experienced a significant increase in their total remuneration growth, depending on the specification of around 10%–20%. This is, however, only the case in firms located in publicly-owned SEZs, not when firms are operating in privately-owned zones. This points towards rent-seeking on the part of managers in SEZs where there is easy contact between managers and bureaucrats or policy makers. This inhibits firms' growth potential.

Our paper contributes to a broader literature examining the effects of place-based policies in the presence of agglomeration economies, which focuses mostly on developed countries (Ham et al., 2011; Busso et al., 2013; Kline and Moretti, 2014; Criscuolo et al., 2019). The focus of this paper is on the SEZs program, a popular policy tool in developing countries used to attract investments and stimulate economic activity in the region. A number of related studies have evaluated SEZs in China (Wang, 2013; Lu et al., 2019), showing that the SEZs establishment increased capital investment, employment, wages and productivity of firms by achieving agglomeration economies. These papers identify SEZs at a more aggregated level than is done in our paper.

In the context of India, there are only few studies evaluating the impact of place-based policies. A paper that relates to our work is (Alkon, 2018) who also examines SEZs in India and finds no evidence for positive developmental spillovers.⁴ One difference between our work and his paper is that we use detailed firm-level data and identify treatment at the firm level, which allows us to pin-point firms within an SEZ. Additionally, we also consider and provide evidence for a possible explanation for the absence of positive development effects, namely, rent-seeking behavior on the part of managers. Other related work is (Gallé et al., 2022) who look at the employment effects of SEZs established between 2005–2013. Blakeslee et al. (2022) examine the effects of the Industrial Areas program in one of the Indian states and find a significant increase in firm creation and employment in the affected villages. Hasan et al. (2021) focus on industrial backward districts and find a short-run effect of a tax-exemption program in the better-off backward districts. Shenoy (2018) also evaluates the developmental effect of investment subsidies to a newly created Indian state and finds improvements in nightlight activity and household welfare. Chaurey (2017) studies the federally financed New Industrial Policy for two states and finds large increases in employment, number of factories, total output and wage bill in treated states. While these studies bring important insights to understanding the effects of place-based policies in India, they focus primarily on one targeted state, whereas the SEZ program was open to all states. Moreover, these studies evaluate more aggregate developmental aspect of the program, whereas we are interested in firm-level outcomes.

We also contribute to the literature on the role of infrastructure for firm-level performance as SEZ developers are required to provide adequate infrastructure in order to attract firms. Another policy implemented by the Indian government to boost firm performance by reducing transportation costs and increased connectivity is the Golden Quadrilateral Project (GQ) - the largest highway project which aims to connect four biggest metropolitan cities. Datta (2012) shows that firms in cities affected by the GQ project reduced their average stock of input inventories and have switched their suppliers, indicating that decreased transportation costs lead to the re-organization of production for manufacturing firms. Ghani et al. (2016) document that districts closer to the highway experienced an increase in manufacturing output with the effect being primarily driven by the new entrant firms. Asturias et al. (2019) find that GQ increased income and decreased misallocation among manufacturing firms. Decreased intra-national trade costs induced by the construction of highway also contributed to an increase in firm productivity (Abeberese and Chen, 2022). Chatterjee et al. (2021) show that the effect of public investment depends on whether a firm is in the formal or informal sector. While proximity to the highway increases productivity for formal firms, there are no benefits for firms in the informal sector. Donaldson (2018) finds similar effects from railway infrastructure, which led to lower trade costs and higher real income levels. Chandra and Thompson (2000) show that infrastructure investments affect the spatial allocation of economic activity. Counties that are targeted by these policies increase the level of economic activity at the expense of adjacent counties, which experience a decline in economic activity. Finally, Brooks et al. (2021a,b) show that industrial agglomeration policies limit competition and thus lead to a lower labor share of income. While the GQ project resulted in decreased transportation costs for manufacturing firms, SEZs provide primarily tax incentives which could potentially have differential effects on firms.

The rest of the paper is structured as follows. Section 2 describes the background information on the SEZs program in India. Section 3 provides a simple theoretical framework to guide our empirical analysis. In Section 4 the data are introduced. Section 5 proceeds with describing the methodology used. Results are presented in Section 6 and Section 7 concludes.

⁴ Several other papers provide descriptive evidence evaluating the efficiency of the SEZ program in India, e.g., (Aggarwal, 2007, 2012).

2. Background on Indian SEZs

India was one of the first countries in Asia to recognize the importance of Export Processing Zones (EPZs) for promoting exports, with Asia's first EPZ being established in the port city of Kandla, Gujarat state in 1965. The absence of modern infrastructure, an unstable fiscal regime as well as the complexities related to customs controls and clearance led to the reorganization of export-promoting policies. Motivated by the success of the SEZs in China, the Indian Government announced the launch of the "SEZs policy" in April 2000. The policy aims at enabling the establishment of SEZs in the private sector and making them an engine for economic growth by offering high-quality infrastructure, attractive fiscal incentives and minimum regulations. SEZs provide multiple new features as compared to the existing EPZs, which, among others, are no minimum export performance requirement and provision of social infrastructure in SEZs, whereas EPZs comprised only industrial activity (Aggarwal, 2012). While EPZs were predominantly viewed as export-promoting tools, SEZs' focus was shifted to the generation of additional economic activity and the advancement of infrastructure.

The "SEZs Act" was passed by Parliament in May 2005, receiving Presidential assent on the 23rd of June 2005. The Act came into effect on February 10th, 2006 with the main objectives of: (i) generating additional economic activity, (ii) promoting exports of goods and services, (iii) promoting investment from domestic and foreign sources, (iv) creating employment opportunities, and (v) developing the infrastructure facilities. The incentives and facilities provided to the units in SEZs include:

- Duty free import/domestic procurement of goods for the development, operation and maintenance of SEZ units.
- 100% income tax exemption on export income for the first 5 years, 50% for the next 5 years and 50% of the ploughed back export profit for the next 5 years.
- Exemption from Minimum Alternate Tax,⁵ Central Sales Tax, Service Tax and State Sales Tax.
- Single window clearance for central and state level approvals.

EPZs established prior to the 2005 Act were notified and converted into SEZs, continuing their operation under the new policy.⁶ Any individual, cooperative society, company or partnership firm, including foreign firms, can submit a proposal for setting up an SEZ. They are referred to as developers of SEZs. Compared to SEZs in other countries, SEZs in India are not spatial units designated by the government. Rather, firms must apply for permission to develop an SEZ and customs boundaries are redrawn around the existing location. Therefore, the policy provides equal opportunities to establish an SEZ for government, private or joint developers. Another particular feature of Indian SEZs is that an SEZ status can be assigned even to a single firm.

The establishment of an SEZ proceeds in three steps: approval, notification and operation. The most crucial criterion for approval is the possession of land. When a developer is in the process of acquiring land, only in-principal approval can be granted. Furthermore, the formal approval can be issued only after (i) the state government has signed the project, (ii) the developer can prove the possession of land, and (iii) the state government has provided exemptions from taxes, ensured adequate infrastructure and issued clearance from the state regulatory bodies. After approval, the board provides notification for the authorization to begin the operation, at which point the investment and construction can be initiated (Alkon, 2018). However, not all approved or notified SEZs become finally operational.

We obtained a list of notified SEZs under the 2005 Act from the Ministry of Commerce and Industry, Department of Commerce.⁷ The dataset contains information on the name of the developer (which is a private or public company or organization that received notification of approval for developing an SEZ), the village and state names where an SEZ is located, the industry in which the SEZ specializes, the area, and the date of notification. There is no information on the number of units operating in each SEZ nor the amount of attracted investment or people employed in each SEZ. Overall, there are 354 notified SEZs reported by 2020 with the first zone being notified in 2006.⁸ Though the type of ownership is not indicated in the list of notified SEZs, 35 out of 354 zones are classified as state-owned according to the list of the Council of State Industrial Development and Investment Corporations of India.⁹

Regarding the location choice, the SEZs Act provides no limitation on the geographic location of the zones. However, it is not surprising to observe the concentration of zones in areas with developed infrastructure, targeting primarily big cities in the most industrialized regions (Kennedy and Rundell, 2014; Palit, 2009; Jenkins et al., 2015). Our data show that 84% of notified SEZs are located in India's eight most industrialized states (Andhra Pradesh, Gujarat, Karnataka, Kerala, Maharashtra, Tamil Nadu, Telangana and Uttar Pradesh). Furthermore, Fig. 1 illustrates that there is great heterogeneity with respect to the number of established zones across districts, with some districts receiving up to 44 zones compared to no SEZs in the northern and eastern parts of India. The non-random assignment of zones poses a potential threat to the causal identification of the effect of zones due to a positive selection bias, and we attempt to alleviate such a concern by using inverse probability weighting.

⁵ This exemption was withdrawn on 01.04.2012, however, other incentives remain in place.

⁶ In the analysis, only SEZs notified under the 2005 Act are used. That is, we exclude 19 converted SEZs to eliminate the concern that the initial incentives and goals of converted and newly notified SEZs are different. Table A.1 in Appendix provides summary statistics for SEZs notified under the 2005 Act and converted SEZs established before the 2005 Act. On average, converted SEZs have a bigger area compared to newly established SEZs which can be explained by the export-oriented policy of initially designed EPZs.

⁷ The list is available under: <http://sezindia.nic.in/upload/uploadfiles/files/notify.pdf>. Last update 29/02/2020.

⁸ Table A.2 in the Appendix shows the number of notified SEZs over time.

⁹ Available under: <https://www.cosidici.com/>.

SEZs by district

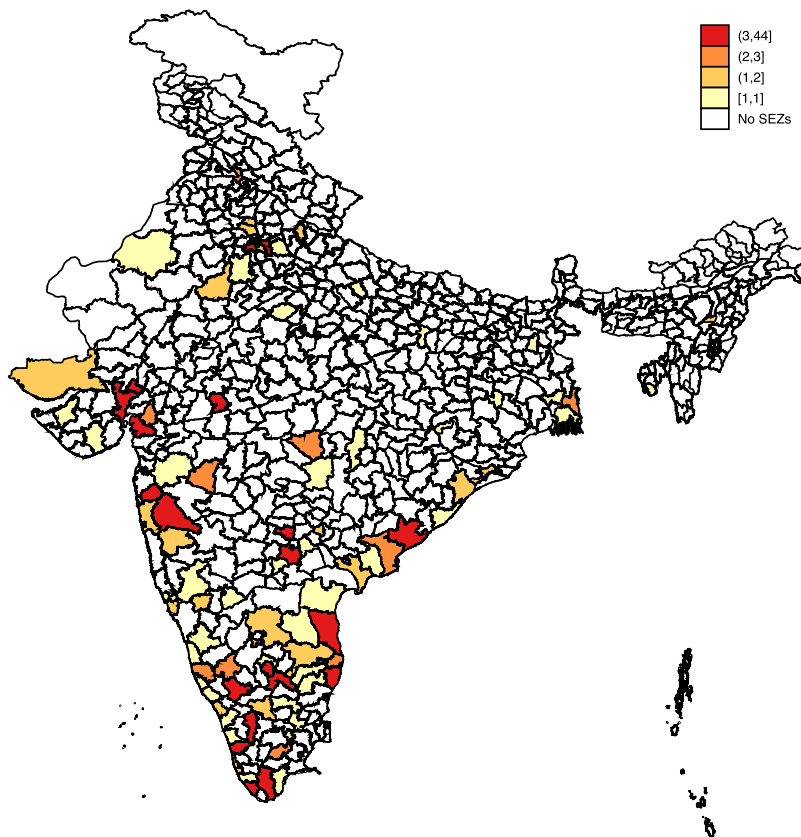


Fig. 1. Number of SEZs by district.

There is also substantial heterogeneity in terms of the area size of the SEZs.¹⁰ Fig. 2 depicts the histogram of the area, while Table 1 presents summary statistics of the area by SEZ-sector, showing substantial differences in average area size across sectors. Additionally, Fig. A.1 in the Appendix illustrates the distribution of SEZs by sector and time.

3. Theoretical framework

How can the establishment of a new SEZ impact firm performance? In this section, we present a simple theoretical framework to fix ideas and motivate our empirical analysis. This theoretical model follows closely (Hao et al., 2020), who look at how the presence of political connections impacts firm performance, and which in turn is based on (Dal Bó and Rossi, 2007).

We assume that there is a representative firm that produces an output Y using capital k and managerial effort e as inputs. Managers can allocate their time between productive activity, e_p , which enhances the production efficiency of a firm, or a non-productive activity, e_{np} , such as rent-seeking or lobbying the government. The total managerial effort e is the sum of two activities: productive and non-productive activities undertaken by the manager.

The production function of a firm is as follows:

$$Y = A(e_p)f(k) \quad (1)$$

where the productivity of a firm increases in the effort managers devote to productive activities. We assume that both managerial effort and capital increase the output at diminishing marginal rates, i.e. $A_{e_p} > 0$, $f_k > 0$, $A_{e_p e_p} < 0$, and $f_{kk} < 0$.

¹⁰ To facilitate the expansion of large-sized SEZs, the Indian Government introduced a sector-wise minimum land area requirement for establishing a zone. SEZs in sectors other than IT, Biotech and health services have a minimum requirement of land area of 50 hectares, whereas for the latter there is no minimum land area requirement. Given that 67% of SEZs are in the IT sector, the distribution of the area is right-skewed with the median area being 19.55 hectares, mean area - 107.8 ha and standard deviation of 411.82 ha. All of the outliers are multi-product SEZs with the largest being Adani Port and SEZs (6.456 ha) and Andhra Pradesh Industrial Infrastructural Corporation Ltd. (2.206 ha).

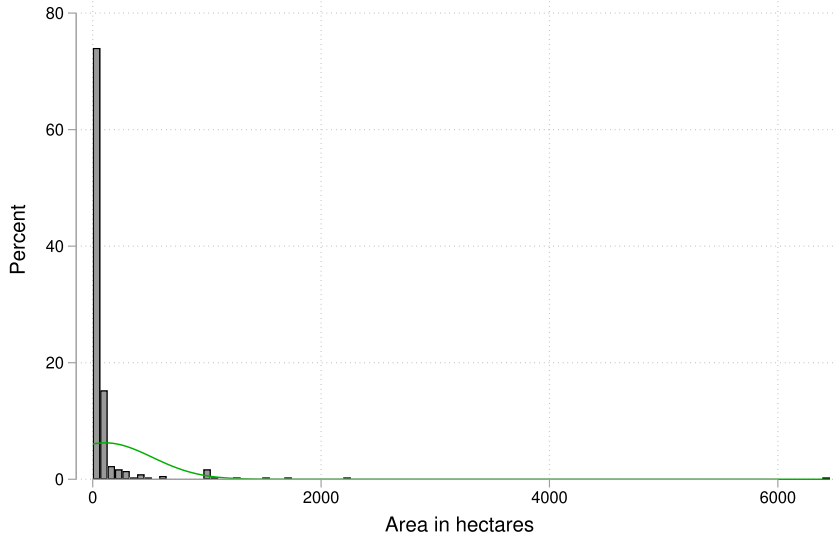


Fig. 2. Histogram of the area of SEZs.

Table 1
Summary statistics of area by sector.

	Area in hectares				
	Mean	SD	Min	Max	N
Aviation	101.69	0.45	101.17	101.98	3
Biotech	18.97	9.79	10.00	40.47	15
Construction	106.46	.	106.46	106.46	1
Energy	76.49	84.15	10.00	222.67	6
Engineering	124.23	69.24	36.42	317.71	16
Food processing	48.20	44.63	11.88	119.14	7
Free Trade and Warehousing Zones	109.55	144.91	40.63	434.86	7
Gems and Jewellery	68.80	.	68.80	68.80	1
Handicrafts	10.49	.	10.49	10.49	1
IT	24.12	33.39	1.05	223.00	237
Minerals	119.86	41.17	50.75	166.91	6
Multi-product	1,165.60	1,355.21	105.44	6,456.33	20
Paper products	109.81	.	109.81	109.81	1
Pharmaceuticals	94.47	54.50	11.47	247.39	18
Port	224.57	98.90	110.47	285.84	3
Textile	133.69	107.25	20.41	404.70	12
Total	107.80	411.82	1.05	6,456.33	354

Engaging in a non-productive activity yields benefits to the firm which are defined as $\xi(e_{np}, c)$. These may be pecuniary or non-pecuniary benefits to the firm as a result of the non-productive activity (such as lobbying). The greater the effort dedicated to the non-productive activity, the higher the benefits, i.e. $\xi_{e_{np}} > 0$. Benefits also depend on the ease of engagement with policy makers, c . A higher level of c implies an environment where engagement of the manager with the government becomes easier, due to $e.g.$, closer contacts to decision makers. Easier political engagement thus increases the marginal return to non-productive activity, i.e. $\xi_{e_{np}c} > 0$. We assume that managers care about their benefits and the total effort they exert, e :

$$\xi(e_{np}, c) - \theta(e) \tag{2}$$

where $\theta(e)$ is the cost of effort which is increasing and convex, i.e. $\theta_e > 0$, $\theta_{ee} > 0$. Managers may not be the owners of the firm, but it is expected that they care about the total firm's profits if higher profits lead into higher managerial compensation. Thus, the problem faced by the manager, which includes dividing effort between productive and non-productive activities, is as follows:

$$\max_{e_p, k} A(e_p)f(k) - rk + \xi(1 - e_p, c) - \theta(e) \tag{3}$$

The first-order conditions to the maximization problem above are:

$$A_{e_p}f(k) + \xi_{e_p} - \theta_e = 0 \tag{4}$$

$$A(e_p)f_k - r = 0 \tag{5}$$

These two conditions characterize the optimal solution e_p^* and k^* to the problem above. That allows us to look at how managerial effort and productivity change with the degree of a firm's political engagement.

Proposition 1. *With easier political engagement (i.e., higher c), the managerial effort is diverted away from productive to non-productive activity, $\frac{\partial e_{np}^*}{\partial c} > 0$. This decreases a firm's productivity, $\frac{\partial A^*}{\partial c} < 0$ and increases the benefits from non-productive activity, $\frac{\partial \xi^*}{\partial c} > 0$.*

Proofs of the proposition can be found in [Appendix A.3](#).

This theoretical framework can guide our empirical analysis on the effect of SEZs on firm performance. Firstly, the establishment of an SEZ enhances the benefit derived from managerial effort going into productive activities through lower taxes, duties and less red tape (through the establishment of single window clearance as discussed above). This should imply higher productivity of the firms. Secondly, however, an SEZ where managers may have close contact to policy makers and bureaucrats constitutes an environment where easy political engagement facilitates the possibility of managers engaging more in non-productive activities. They may divert their efforts to non-productive activity because they expect to get higher rewards. But given that effort is costly, increasing time spent on non-productive activities, such as lobbying or rent-seeking, decreases managerial effort dedicated to production-enhancing tasks, thus negatively affecting the productivity of firms. Which of these two outcomes — productivity increases because of higher productive-activities, or decreases due to more non-productive activities, is an empirical issue that we now turn to.

4. Compiling the data set

One of the main challenges in assessing the impact of SEZs, particularly in developing countries, is the unavailability of data on firms operating inside SEZs. Therefore, in an effort to overcome this limitation, we merge firm-level data from Prowess with a list of notified SEZs obtained from the Ministry of Commerce and Industry using a spatial approach.

Prowess is a database of financial performance of Indian companies, collected by the Centre for Monitoring the Indian Economy (CMIE). This data has been used extensively in other strands of research, see, e.g., [Goldberg et al. \(2010\)](#), [De Loecker et al. \(2016\)](#), [Herkenhoff et al. \(2024\)](#). Prowess includes relatively large firms and accounts for 60%–70% of the economic activity in the industrial sector ([Goldberg et al., 2010](#)). Our dataset is an unbalanced panel of firms covering the period from 1988 to 2020. The dataset provides information on the financial statements of firms, including sales, assets, raw materials, energy costs, compensation to employees, exports, industry, and most importantly, the address of the registered office of the firm.

Another dataset, the Annual Survey of Industries (ASI) also collects annual data on firms in the formal sector (used in, e.g., [Chaurey \(2017\)](#), [Martin et al. \(2017\)](#)). We prefer Prowess data in our context for a number of reasons. Firstly, a crucial advantage of using Prowess over the ASI is that the precise location information is available in Prowess, whereas only the administrative territorial unit such as state or district is reported in ASI. This allows us to focus on firms as treatment units and determine whether a firm is inside, in the vicinity, or far away from an SEZ. Using district-level information in ASI would only allow to classify a firm as within a district that has an SEZ, resulting in potential aggregation bias. Second, Prowess is panel data, whereas publicly available ASI is a cross-section. Recently, the Government of India has revised its policy and released firm identifiers going back to 1999. However, district information is not available in panel ASI. [Martin et al. \(2017\)](#) merge panel ASI with publicly available cross-sectional ASI which contains district information, though this information is only available until 2010.¹¹ Third, Prowess contains firms operating in both the manufacturing and service sectors, whereas ASI incorporates solely the manufacturing sector.¹² While recognizing the advantages of using Prowess for our analysis, we also carry out robustness checks using a constructed panel ASI with district information (as in [Martin et al. \(2017\)](#) from 1999 to 2010) to verify our results.

There are, however, also potential downsides to the Prowess data. Firstly, it is not well suited to studying firm entry and exit because firms are not legally obliged to report to the data collecting agency. However, because Prowess contains mostly large firms, entry and exit is not necessarily an important margin of adjustment for these firms. In what follows, we therefore look at the impact of SEZs on established firms. Another limitation of this dataset is that it captures firms and not plants and it provides no information on the number of plants operated by each firm. Nevertheless, we believe that this is not an important concern because to be able to benefit from SEZ incentives, physical presence inside the area is required. If headquarters are strategically placed inside the zone, if anything, it would lead to an overestimation of the effect.

Since Prowess does not directly report information on the SEZ status of the firm, the address is used to identify the geographic coordinates of the firm. Geocoding is done using ArcGIS Online Geocoding Service.¹³ The latitude and longitude of each firm, together with spatial rings of different radii around the centroid of SEZs, are plotted on a map using ArcGIS to identify the location of a firm in relation to SEZs. We manually check the random sample of firms using Google Maps to verify the accuracy of geocoordinates.

¹¹ The merge is done based on a number of variables, such as industry, number of plants, ownership, opening and closing value of fixed capital, total number of employees, and income from services.

¹² The majority of firms (23%) in Prowess operate in financial service activities, followed by wholesale and retail trade (12%) and chemicals (5%) as presented in [Table A.3](#). However, because firms are under no legal obligation to report the data, only less than 10% of firms (mostly public sector and large IT companies) disclose employment information, which makes Prowess unsuitable to analyze the labor market implications of SEZs. Prowess does provide wage bill information which is used later on for the TFP estimation.

¹³ We exclude from the analysis the following states due to the small number of observations: Andaman and Nicobar, Arunachal Pradesh, Chandigarh, Goa, Nagaland and Manipur.

The primary difficulty in pinning down the exact location of the SEZ is imprecise location information, which is available at the village level in the most disaggregated form. Thus, to pinpoint the accurate address of the SEZ, we manually identify the latitude and longitude of the zone using the name of the developer combined with the village and state names. Since SEZs are not points on a map but rather geographic zones, we use the information on the area of the SEZ (presented in Table 1) to create spatial rings around the centroid of the zones.

We assume that all firms within this circle are inside the SEZ and we therefore refer to this circle as *inside*. Because we do not know the actual boundaries of SEZs, we assume that they have a circular shape. This assumption could lead to some firms that are actually located in SEZs being classified as non-SEZ firms and vice versa. Classifying non-SEZ firms as treated may induce attenuation bias. To alleviate concerns about such measurement error, we probe into a more stringent definition of treated firms and decrease the original radius by 5% and 15%, respectively.¹⁴

Our control group consists of matched firms further than 40 km away from the center of an SEZ. This distance is chosen to make sure that control group firms are not themselves affected by the treatment. However, given that the actual size of SEZs is rather small, we provide robustness checks where we change the control group to firms located between 20–30 km, more than 20 km or more than 30 km away from an SEZ.

Additionally, we calculate the distance from all firms to the nearest airport and port to look at heterogeneity in the treatment effects. The list of domestic and international airports is taken from the statistics on total traffic in 2000 provided by the Directorate General of Civil Aviation.¹⁵ Data on ports are sourced from the Indian Ports Association.¹⁶ We geolocate ports and airports and use ArcGIS to calculate the nearest distance.

An important point to be made is that the list of notified SEZs is used for the analysis. As described above, the establishment process consists of three stages: approval, notification and operational stage. Not all approved SEZs become eventually notified or operational. As of 2020, there are 421 formal approvals, 84% of which are notified and only 57% are operational. However, at the time of formal notification, investments and construction can begin, which may already affect the performance of firms. Following this reasoning, we chose the notification stage as our treatment. Moreover, we do not consider in our analysis SEZs notified prior to the enactment of the 2005 Act. These are 19 EPZs that were established before the SEZs policy and were converted into SEZs with the enforcement of the 2005 Act. Since the initial goal of EPZs was primarily to promote exports, whereas SEZs' focus is turned into developmental effects, the provided incentives may be different, which leads us to focus solely on SEZs notified under the SEZs Act.¹⁷

As our main interest is in the effect of an establishment of an SEZ on firm performance, the analysis focuses on TFP growth as the main variable of interest. All variables are deflated using industry-specific Wholesale Price Index (WPI) for manufacturing firms and yearly WPI for service firms and transformed into logarithms. Total factor productivity is estimated using (Akerberg et al., 2015) approach (a detailed explanation on the estimation is presented in Appendix A.4). Given the caveats of TFP estimation, we use alternative measures of firm performance, such as sales growth or wage growth as robustness checks. Other variables used as baseline controls include age, a foreign ownership dummy, dummy variables for manufacturing and service sectors measured in 2005 and time-invariant state dummies.¹⁸ We classify a firm as foreign-owned if the percentage of equity shares held by foreign individuals, corporate bodies or institutions exceeds 25%. Time-varying covariates include, depending on the specification, total assets, sales, TFP and exporter dummy.

Table 2 presents summary statistics for treated firms and the control group. Panel A depicts the mean of the variables for all years, Panel B presents the summary statistics for the pre-treatment 2005 year. Looking at the initial level of productivity for inside SEZ firms and the control group, we observe that the TFP level is slightly higher for the treated group compared to the control group, which indicates that initially more productive firms self-selected into SEZs. Those firms are also more likely to export. However, productivity growth was significantly higher for firms in the control group before SEZs took off, whereas inside-SEZ firms experienced even negative TFP growth. Further, treated firms are initially younger and have lower assets compared to the control group which may indicate that they are new entrants in the market. They are also less likely to be foreign-owned and operate predominantly in the services sector. Overall, this pre-treatment mean comparison indicates the potential for a positive selection bias in terms of initial productivity level. To assuage such concerns, we control for the pre-treatment observable variables so that the results can be interpreted accounting for this type of selection.

Further examining trends for the whole sample, we observe that SEZ firms are catching up and have higher TFP growth compared to the control group. Moreover, their sales growth and assets growth exhibit a higher value than the one observed for the control group. Regarding the industry distribution, SEZ firms are more likely to operate in the services sector relative to firms in the control group.

Prowess data contain registered and large firms. Whereas it is unlikely that informal firms may be present inside SEZs, it is plausible that the development of SEZs may lead to the establishment of small informal firms surrounding the zone that provide services to SEZ firms or employees. Given the presence of a sizable informal sector in India, we additionally check whether the

¹⁴ Table A.4 in the Appendix presents the number of treated firms in each spatial ring.

¹⁵ The list is publicly available under: https://www.dgca.gov.in/digigov-portal/?page=jsp/dgca/InventoryList/dataReports/aviationDataStatistics/airTransport/domestic/yearly/4_5/5-35.pdf&main4267/4210/serviceName. Accessed on 07.01.2024.

¹⁶ Available under: <http://ipa.nic.in/index.cshtml>. Accessed on 07.01.2024.

¹⁷ None of the firms in the control group falls inside SEZs established prior to the 2005 Act. We further provide a robustness check excluding firms in the treated group that are located in those converted SEZs.

¹⁸ We do not observe the change in the registered address of the firms, hence the location information is time-invariant as of the latest financial report.

Table 2
Summary statistics of firms.

	(1) Inside Mean	(2) Control Mean
<i>Panel A: Whole sample</i>		
TFP	1.143	1.031
TFP growth	0.008	0.004
Exporter dummy	0.529	0.441
Sales growth	0.052	0.023
Log of total assets	5.569	5.420
Log asset growth	0.071	0.043
Age	40.583	42.758
Foreign dummy	0.043	0.024
Manufacturing dummy	0.478	0.734
Services dummy	0.346	0.137
Distance to port, log	8.904	11.505
Distance to airport, log	9.436	11.902
<i>Panel B: Pre-treatment variables in 2005</i>		
TFP	1.250	1.003
TFP growth	-0.013	0.047
Exporter dummy	0.265	0.211
Sales growth	-0.014	0.060
Log of total assets	3.498	3.712
Log asset growth	0.022	0.038
Age	36.000	38.943
Foreign dummy	0.009	0.013
Manufacturing dummy	0.341	0.576
Services dummy	0.538	0.325
Distance to port, log	8.838	11.871
Distance to airport, log	9.335	11.980

establishment of SEZs has an effect on the number of informal firms and their sales and employment. The data on informal sector are sourced from two waves of Informal Non-Agricultural Enterprises Survey conducted by the National Sample Survey Organisation (NSSO). We use the 55th round conducted between 1999 and 2000 and the 67th round implemented between 2010 and 2011. Because the least disaggregated geographical unit is a district, we conduct this analysis at the district level.

5. Empirical strategy

Recall that the aim of the paper is to estimate the effects of the establishment of an SEZ on firm performance. As pointed out above, the main purpose of establishing SEZs was to improve the economic development of the regions — and not, as *e.g.*, in China, to boost exports. We, therefore, focus in our analysis on firm productivity and compare the productivity growth of firms before and after the establishment of SEZs relative to the firms that are not exposed to the program. We consider growth rather than levels as this differences out differences in productivity levels across firms. The key assumption then is that treated and control groups would have evolved in the same way in the absence of treatment, in other words, the conditional mean independence (CIA) assumption should be satisfied.¹⁹

The identification relies primarily on the assumption that the treatment is exogenous. This may not necessarily hold given that firms self-select into treatment. To assuage concerns about selection, and ensure that treated and control groups are comparable, we determine the average treatment effect of SEZs on firm performance using a combined difference-in-differences and propensity-score re-weighting approach (an identification strategy used frequently in the literature, *e.g.*, [Guadalupe et al. \(2012\)](#), [Girma et al. \(2015\)](#) and [Brucal et al. \(2019\)](#)). Hence, our identification relies on the assumption of selection on observables. It should be kept in mind, of course, that possible unobserved variables that might also drive selection are, by their very nature, not accounted for.

We have a further complication in that the treatment and the outcome we observe is time varying, as SEZs are established at different points in time. In such a case, standard propensity score methods applied to longitudinal data may be misleading when the treatment and the variable of interest are observed at multiple points in time ([Fitzmaurice et al., 2008](#)). Pre-treatment covariates used for deriving conditional probabilities vary over time in a way that is possibly influenced by previous outcome variables. Therefore, the longitudinal structure of panel data and the rolling introduction of the treatment make it difficult to use the standard inverse probability weighting technique, which may lead to biased estimates.

To overcome this issue, we follow a growing literature on time-varying treatments and calculate weights at each point in time ([Fitzmaurice et al., 2008](#); [Thoemmes and Ong, 2016](#); [Girma and Görg, 2022](#)). To illustrate, at the first treatment occurrence,

¹⁹ Another important assumption is that of parallel trends. In [Appendix A.5](#) we report results from event studies which show no evidence of pre-treatment differences in trends, supporting this assumption.

we predict treatment assignment given the observed history of the covariates. At the next time point, a different set of weights is constructed that makes the treatment selection at time two orthogonal of all observed covariates prior to this treatment selection. Repeating this procedure for each year following the first treatment introduction results in a set of weights, which are eventually cross-multiplied to form a unique final weight for firm i at time t .²⁰

Taking all together the stabilized weight is estimated as follows:

$$SW_{it} = \prod_{s=1}^T \frac{\Pr(SEZ_{s_{it}} = 1 | X_i^0)}{\Pr(SEZ_{s_{it}} = 1 | \bar{X}_{it-1}, X_i^0)}, \quad (6)$$

where $SEZ_{s_{it}}$ is an indicator for a post-SEZ period for firm i at time t . It is always zero for never-treated firms. X_i^0 are time-invariant covariates which include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. \bar{X}_{it-1} are time-varying covariates up until $t - 1$, including the log of total assets, log of sales, exporter dummy and the history of the outcome variable. To incorporate information on the values of time-varying covariates before the start of the treatment, the value for 2006 is replaced by the mean value for 2004–2006. Thus, the stabilized weights are defined for each firm i at time t .

The intuitive interpretation is similar to standard propensity score methods. Firms that exhibit a high propensity to be treated and are ultimately treated are down-weighted in the pseudo-population because they are over-represented relative to the control group, which exhibits high treatment probability but is not treated. It is worth reiterating that propensity score weighting helps get the treatment independent of observable covariates but there may still be some unobserved factors inducing the selection decision, which cannot be controlled for.

The propensity scores are estimated using covariate balancing propensity score (CBPS) following (Imai and Ratkovic, 2014). This methodology makes use of the dual characteristics of the propensity score as covariate balancing score and the conditional likelihood of treatment assignment. Thus, CBPS models treatment probability while at the same time optimizing the covariate balance, which eliminates the need for separate covariance balancing checks. Still, for illustrative purposes, we report some balancing tests in Table A.5 in the Appendix.

Once the weights are formed, they can be included in the final regression. The estimated weighted difference-in-differences regression equation takes the following form:

$$\Delta Y_{it} = \alpha + \beta SEZ_{s_{it}} + \theta X_i^0 + \lambda_t + \mu_s + \epsilon_{it} \quad (7)$$

where ΔY_{it} is the dependent variable (defined as productivity growth), $SEZ_{s_{it}} = 1$ for SEZ firm in post-SEZ period and zero otherwise, X_i^0 include baseline controls such as age, a dummy variable for foreign ownership and dummies for manufacturing and service sectors in 2005. Besides baseline controls, each regression controls for treatment selection based on observable covariates (log of assets and the history of the dependent variable) up until $t - 1$ by including probability weights. λ_t are year fixed effects that control for time trends common to all firms. μ_s are state fixed effects which absorb differences in the geographic location of the zones. Standard errors are clustered at the SEZs level. To eliminate time-invariant unobserved firm-specific effects, the variables are log differenced. β is the coefficient of interest that shows whether the expected change in the outcome from pre-SEZs to post-SEZs is different in the treated group relative to the control group. In the choice of the control group as firms located more than 40 kilometers away from the zones, we relied on two primary factors: the control group should not be affected by the treatment and it should be comparable to the treated group. In robustness checks we also use different definitions of the control group as firms within 20–30 km distance from the SEZ, or firms farther away than 20 or 30 km.²¹

6. Results

6.1. Baseline

The results of the time-varying treatment estimation approach are presented in this section. We start by estimating the effect of SEZs on TFP growth (calculated as in Ackerberg et al. (2015)), in Table 3. Results show that conditional on controlling for initial selection based on observables, the establishment of SEZs did not have any statistically significant effect on the productivity growth of firms inside the zone. Following our previous discussion on potential measurement error stemming from including non-SEZ firms in the treated group, in Column (2) we reduce the original radius by 5% and keep the control group as before. The result shows a significant decrease in productivity growth, however the statistical significance is only at 10%. In Column (3) we decrease the original radius by 15%. Results are similar in magnitude and have a negative sign, alleviating concerns about potential measurement error.²²

²⁰ For instance, if firm i is treated in 2006 and observed throughout the whole sample period up until 2020, a set of 14 weights will be estimated. Taking the product of all the weights results in a unique weight for firm i and time t .

²¹ Another possibility would be to form a control group consisting of firms that applied for SEZs but were rejected, in line with (Kline and Moretti, 2014) and Helmers and Overman (2017). Examining the minutes of the meeting of the Board of Approval reveals that the majority of applications are approved, and, when the required documents are lacking, “in-principal” approval is granted or the application is deferred until the developer is able to present the required clearances or satisfies the minimum land requirement. Thus, due to the limited number of observations, this approach is not possible here.

²² One may also be interested in whether SEZs affect firms in the vicinity of the zone, so-called spillover effects. However, given that there is no discernible effect of SEZs on the productivity growth of firms located inside the zones, it does not seem plausible for spillovers to exist. Nevertheless, we look at the impact of the opening of an SEZ on firms in distance bands around the zone. We run our baseline model on the sample of firms located between 0–5 km, 5–10 km, and 10–15 km around the SEZ and report the results in Appendix. As expected, Table A.6 shows that there is no indication of significant spillovers for neighboring firms.

Table 3
Time-varying treatment effect of SEZs on the growth rate of TFP.

	(inside) TFP growth	($r - 5\%$) TFP growth	($r - 15\%$) TFP growth
SEZs	-0.0586 (0.0600)	-0.0419* (0.0207)	-0.0360 (0.0534)
Age 2005	-0.0103*** (0.00202)	-0.00130 (0.000784)	-0.00375** (0.00176)
Service 2005	-0.128** (0.0569)	0.0568*** (0.00801)	-0.0108 (0.0378)
Manufacturing 2005	-0.0659 (0.0534)	0.0193 (0.0388)	-0.0161 (0.0109)
Foreign ownership 2005	-0.0585 (0.0707)	-0.0796*** (0.0208)	0.0297 (0.0250)
Constant	0.529*** (0.0556)	0.0790*** (0.0202)	0.116* (0.0624)
N	2333	2291	2205
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The outcome variable is measured as the log difference of TFP. TFP is measured using (Ackerberg et al., 2015) approach. Control group are matched firms located further than 40 km away from an SEZ. Time-varying covariates for creating the propensity scores include log of assets, log of sales, and the history of the outcome variable. Time-invariant covariates include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. The weights are derived using CBPS and Eq. (6). Standard errors are clustered at the SEZs level.

6.2. Robustness

Given these somewhat surprising results (certainly from a policy perspective), we conduct a number of tests to see whether our findings are robust to changes in the specification.

We firstly look at the control group, which so far is defined as firms located further than 40 km away. However, this might be a broad definition given that the average size of SEZs is rather small. Thus, in Table 4, we change the control group to firms located (i) between 20 and 30 km of an SEZ, (ii) farther than 20 km or (iii) farther than 30 km away from the SEZ. We keep the treated group as those firms located inside an SEZ. We still do not find any significant positive effect of SEZ establishment on firm performance, supporting our baseline findings. In the subsequent analysis we therefore stick with our initial control group of firms farther away than 40 km from the SEZ.

Secondly, we consider different measures of firm performance. Thus far, our main outcome of interest is productivity growth. Because firms are more likely to report sales and total wage bill with greater coverage compared to material and energy inputs which are needed to calculate TFP, we also look at sales growth, wage growth and wage intensity growth as alternative measures of firm performance. Table 5 reports the results for the alternative outcome variables. We observe no significant effects of SEZs on the sales or wage growth of SEZ firms, and even negative effects on wage intensity growth. These results thus support our baseline findings of no positive effects of the establishment of an SEZ on firm performance.

In our analysis thus far, we do not consider Export Processing Zones (EPZs) established prior to the SEZs Act and later converted to SEZs with the enactment of the 2005 Act. However, firms that are located in converted SEZs and appear in our treated or control groups may be affected by a different type of incentives and therefore may lead to biased results. Geocoding EPZs and identifying firms located inside them shows that none of the firms in the control group is located in converted SEZs. However, some treated firms are indeed located in those converted SEZs. In Table 6 we exclude those firms and observe that the results are not affected.

In order to see how dependent our results are on our time-varying treatment approach, we also contrast our main results with a more conventional PSM technique, using nearest-neighbor matching. In this case the probability of being treated is derived based on a single set of pre-treatment 2005 covariates for all firms equally. Table A.7 in the Appendix shows that the results are also robust to this alternative PSM measure.

We also verify the robustness of our results using panel data from the Annual Survey of Industry (ASI). Because we do not have precise address information, we assign treatment to all firms that are located in a district that has an SEZ. Even though the analysis is done at the firm level, treatment is defined at the district level and given the relatively small number of treated and control districts, it is impractical to implement our preferred time-varying treatment approach. Hence, we kept only those SEZs established in 2006 and performed a standard PSM. We predict the probability of a district being treated based on pre-treatment district-level covariates, such as log distance to port, employment rate in 2001, average district-level GDP between 2000 and 2004, log population in 2001, and time-invariant state dummies. These covariates are taken from publicly available statistics. Hence, all firms within the same district will have the same propensity score. Then, treatment probabilities are transformed into weights and we run a weighted difference-in-differences regression.

Table 4
Time-varying treatment effect of SEZs on the growth rate of TFP using alternative control groups.

	(control 20–30 km) TFP growth	(control >20 km) TFP growth	(control >25 km) TFP growth
SEZs	−0.208*** (0.0686)	0.0834 (0.0565)	−0.00533 (0.0578)
Age 2005	0.00108 (0.00352)	−0.00104** (0.000450)	0.00306*** (0.000124)
Service 2005	0.114 (0.0848)	0.144*** (0.00579)	0.309*** (0.00239)
Manufacturing 2005	0.306*** (0.0687)	0.0463 (0.0585)	0.0518 (0.0574)
Foreign ownership 2005	−0.234*** (0.0515)	−0.0903* (0.0473)	−0.0727* (0.0415)
Constant	0.0321 (0.0987)	0.0503 (0.0703)	−0.237*** (0.0800)
N	1382	3648	3250
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The outcome variable is measured as the log difference of TFP. TFP is measured using (Akerberg et al., 2015) approach. Treated are firms located inside the original radius of SEZs. Time-varying covariates for creating the propensity scores include log of assets, log of sales, and the history of the outcome variable. Time-invariant covariates include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. The weights are derived using CBPS and Eq. (6). Standard errors are clustered at the SEZs level.

Table 5
Time-varying treatment effect of SEZs using alternative variables.

	(inside) Sales growth	(inside) Wage growth	(inside) Wage intensity growth
SEZs	−0.143 (0.109)	0.208 (0.200)	−0.227** (0.0865)
Age 2005	−0.00350** (0.00138)	−0.00916*** (0.00199)	0.0028** (0.00104)
Service 2005	0.0261 (0.0868)	−0.417** (0.181)	0.554*** (0.114)
Manufacturing 2005	0.373*** (0.0921)	0.198* (0.107)	0.211*** (0.0195)
Foreign ownership 2005	0.105** (0.0504)	−0.490* (0.268)	−0.112 (0.120)
Constant	−0.169 (0.106)	0.680*** (0.229)	−0.129 (0.0901)
N	2849	2575	2434
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The outcome variable is measured as the log difference of sales, wages and scaled wages to sales. Time-varying covariates for creating the propensity scores include log of assets, and the history of the outcome variable. Time-invariant covariates include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. The weights are derived using CBPS and Eq. (6). Standard errors are clustered at the SEZs level.

Results are presented in Table A.8 in the Appendix. Similar to our baseline specification, we observe no significant effect on labor productivity growth, defined as growth of output per worker, and a significant negative effect on sales growth of treated establishments. We also do not find any significant effects on wages, total assets or employment. Hence, overall, we observe a similar picture when using the ASI data to look at all manufacturing establishments in India — the establishment of SEZs did not bring any discernible positive effect on firm performance among manufacturing firms.

6.3. Extensions

As we increase the radius of the spatial circles around SEZs, one firm may fall within multiple SEZs in case they are located close to each other. One of the rationales for setting up an SEZ is to provide infrastructure facilities. Reliable and accessible infrastructure helps attract firms to SEZs. Thus, if SEZs are clustered around one area and build the necessary infrastructure, then the more SEZs

Table 6
Robustness check excluding firms located in EPZs established prior to the enactment of the SEZs Act and later converted to SEZs.

	(inside) TFP growth	($r - 5\%$) TFP growth	($r - 15\%$) TFP growth
SEZs	-0.0586 (0.0600)	-0.0419* (0.0207)	-0.0360 (0.0534)
Age 2005	-0.0103*** (0.00202)	-0.00130 (0.000784)	-0.00375** (0.00176)
Service 2005	-0.128** (0.0569)	0.0568*** (0.00801)	-0.0108 (0.0378)
Manufacturing 2005	-0.0659 (0.0534)	0.0193 (0.0388)	-0.0161 (0.0109)
Foreign ownership 2005	-0.0585 (0.0707)	-0.0796*** (0.0208)	0.0297 (0.0250)
Constant	0.529*** (0.0556)	0.0790*** (0.0202)	0.116* (0.0624)
N	2333	2291	2205
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: Time-varying covariates for creating the propensity scores include log of assets, log of sales and the history of the outcome variable. Time-invariant covariates include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. The weights are derived using CBPS and Eq. (6). Standard errors are clustered at the SEZs level.

there are in the vicinity, the more developed the infrastructure may be. Thus, firms inside the zones may benefit not only from own-SEZ incentives but also from the number of other SEZs in the vicinity. To check this, we now allow for such differential *treatment intensity*. We define a new variable “Other SEZs” that counts the number of other SEZs within a 5, 10, and 15 kilometer radius. We re-estimate our baseline specification adding this additional explanatory variable which captures the average additional effect from each of the other SEZs.

Results are presented in Table 7. There is a strong and statistically significant increase in the firm’s productivity the more other SEZs there are. The coefficient indicates that the establishment of other SEZs within 5 km increases the productivity growth of firms by 3%. The effect dissipates with distance both in terms of the magnitude and statistical power. Other SEZs within 10 km increased the productivity growth of firms by 2%. Still, the effect of the own SEZ is still negative, albeit reduced in statistical significance.

The aggregate effect estimated thus far may mask substantial heterogeneities. To look at whether the ownership, age, size, sector of the firm or of the SEZ, or proximity to infrastructure matters for absorbing the SEZ benefits, we perform heterogeneity analysis. The results are presented in Table 8. Foreign firms, large firms defined as firms with above median sales in 2005, young firms defined as those firms with age smaller than the median in 2005, or firms located in SEZs focusing on high-tech sectors, do not seem to be differentially affected. Looking at the sector of the firm, manufacturing firms experience a significant drop in TFP growth compared to firms in the services sector. This can potentially be explained by the fact that all goods and service supplied by SEZs to the Domestic Tariff Area (the whole of India excluding SEZs) are treated as imports and are subject to import tariffs which may discourage manufacturing firms to sell domestically. Lastly, the distance to port seems to matter, whereas the proximity to an airport does not play any role.

Lastly, we examine whether the establishment of SEZs contributed to the growth of the informal sector in the surrounding area. As discussed above, we use data on the informal sector and construct a panel of districts with two data points, before and after treatment. Treatment is defined at the district level and is equal to one if a district has an SEZ in 2006. To deal with the selection bias, we implement propensity score matching based on pre-treatment district-level covariates, such as log distance to port, employment rate in 2001, average district-level GDP between 2000 and 2004, log population in 2001, and time-invariant state dummies. We transform estimated probabilities into weights and run a weighted difference-in-differences regression with district and year fixed effects. Results presented in Table A.9 in the Appendix indicate that SEZs did not induce the creation of new informal firms in the district. Further, we do not observe neither an increase in sales nor increased employment of incumbent informal firms at the district level.

6.4. How to explain our results?

It appears that a robust result thus far is that there are no discernible direct effects on the performance of the average firm in a newly established SEZ. How can we explain this somewhat unexpected result? In the context of our theoretical framework presented in Section 3, we conjecture that managers of firms in newly established SEZs may find it advantageous to engage in non-productive activities such as lobbying and rent-seeking, implying decreasing firm productivity as a result. Furthermore, there is the possibility of excessive governmental involvement as potential mechanisms underlying the inefficiency of Indian SEZs, as hypothesized by Alkon

Table 7
Treatment intensity effect of SEZs on TFP growth.

	(inside) TFP growth	($r - 5\%$) TFP growth	($r - 15\%$) TFP growth
<i>5 km distance</i>			
SEZs	-0.113* (0.0579)	-0.0630*** (0.0186)	-0.0235 (0.0497)
Other SEZs within 5 km	0.0338*** (0.0102)	0.0148*** (0.00395)	-0.0120 (0.00820)
<i>10 km distance</i>			
SEZs	-0.131* (0.0694)	-0.0749*** (0.0214)	-0.0544 (0.0640)
Other SEZs within 10 km	0.0192** (0.00707)	0.00899*** (0.00205)	0.00417 (0.00747)
<i>15 km distance</i>			
SEZs	-0.115 (0.0691)	-0.0739*** (0.0201)	-0.0361 (0.0538)
Other SEZs within 15 km	0.00794* (0.00459)	0.00446** (0.00163)	-0.000332 (0.00503)
N	2333	2291	2205
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: Control group are firms located further than 40 km from an SEZ. Time-varying covariates for creating the propensity scores include log of assets, log of sales, and the history of the outcome variable. Time-invariant covariates include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. The weights are derived using CBPS and Eq. (6). Standard errors are clustered at the SEZs level.

(2018). Many SEZs are developed by state-owned entities. These state development corporations facilitate land acquisition; however, government intervention may fail to account for market conditions, infrastructure, labor availability, and other necessary inputs, thus turning SEZs into projects with little productivity gains.

To take a first look at these potential explanations, we make use of the fact that not all SEZs are run by the state, but some are also developed by private owners. We use this information in the data to estimate our empirical model separately for privately-owned and publicly-owned SEZs to check whether there is a differential effect of opening a private SEZ compared to a public SEZ.²³

Results reported in Table 9 indicate that firms located in privately-owned SEZs are indeed affected differently from those in publicly-owned developments. We find that firms in newly established privately-owned SEZs do experience positive productivity growth effects, though this estimate is only statistically significant in columns (1) and (2), not in (3). Nevertheless, this points to positive growth effects in privately-owned SEZs. This is not the case in publicly-owned SEZs, where we find consistently negative productivity growth effects. This clearly supports the conjecture that firms in state-owned SEZs perform worse. This could be due to a higher level of non-productive activities on the part of the manager (as hypothesized in our theoretical framework), or excessive government interference.

In order to focus more on the possibility of managers engaging in non-productive activities for rent-seeking, we now turn to look at their income. If non-productive activities are important, we may conjecture that managers maximize their income, rather than maximizing firm performance. Hence, we may expect that we see increases in salaries for managers, even if productivity does not increase. We would not expect that if the lack of productivity growth in SEZs is purely due to excessive government interference.

The Prowess database provides information on directors' pay, which we propose to use to probe further into this issue. Is it the case that directors increase their salaries even if, as we have shown, there are no positive average productivity effects for firms in SEZs? To investigate this, we use the growth of total remuneration to a company's directors as the outcome variable in Table 10.²⁴ Results are in line with our conjecture. From column (1), we see that the total remuneration of directors in SEZ-firms increased, on average, significantly by around 10% after the SEZ was established. Based on the alternative definitions in columns (2) and (3), we observe an even higher coefficient in terms of magnitude, which translates into an increase of around 20%.

The potential for non-productive activities on the part of managers may be higher in publicly-owned SEZs, where engagement with policy makers is easier compared to private-owned SEZs. When distinguishing these two types of SEZs, we find that, indeed,

²³ Because the information on the ownership type is not available in the list of SEZs, we identified 35 state-owned industrial development corporations based on the names listed on the website of the Council of State Industrial Development and Investment Corporations of India available at: <https://www.cosidici.com>.

²⁴ Unfortunately, the reporting of directors' remuneration is not obligatory, which leads to a substantial drop in observations. To ensure that the smaller sample does not introduce a bias, we re-estimate the TFP growth model in Table 3 also on this smaller sample used here. We find a negative coefficient on TFP growth which is similar in magnitude to the baseline estimate. Results are available on request.

Table 8
Heterogeneous effects of SEZs on TFP growth.

	(inside) TFP growth	($r - 5\%$) TFP growth	($r - 15\%$) TFP growth
<i>Panel A: Ownership type</i>			
SEZs = 1	-0.0589 (0.0604)	-0.0446** (0.0210)	-0.0402 (0.0563)
Foreign = 1	0.0740 (0.0670)	0.0259 (0.107)	0.0963 (0.118)
SEZs = 1 × Foreign = 1	0.0838 (0.0657)	0.118** (0.0533)	0.0478 (0.0922)
<i>Panel B: Firm size</i>			
SEZs = 1	-0.0565 (0.113)	0.0527 (0.0379)	0.0323 (0.0642)
Large = 1	-0.0308 (0.0188)	0.117*** (0.0204)	0.109*** (0.0128)
SEZs = 1 × Large = 1	-0.0101 (0.137)	-0.0956 (0.0565)	-0.0535 (0.0528)
<i>Panel C: Age</i>			
SEZs = 1	-0.118*** (0.0364)	-0.0850*** (0.0222)	-0.0693* (0.0346)
Young = 1	-0.220*** (0.0402)	-0.0840*** (0.0171)	-0.196*** (0.00787)
SEZs = 1 × Young = 1	0.116 (0.0964)	0.0798 (0.0771)	0.0486 (0.0601)
<i>Panel D: Sector</i>			
SEZs = 1	0.0711 (0.0417)	0.0655** (0.0280)	0.154*** (0.0352)
Manufacturing = 1	-0.175*** (0.0161)	-0.155*** (0.00914)	-0.196*** (0.0187)
SEZs = 1 × Manufacturing = 1	-0.217** (0.0824)	-0.222*** (0.0665)	-0.387*** (0.0687)
<i>Panel E: Hi-Tech SEZs</i>			
SEZs = 1	-0.0339 (0.0418)	-0.0504*** (0.0113)	0.0224 (0.0179)
Hi-tech = 1	0.0282 (0.0443)	0.0817*** (0.0156)	-0.0302 (0.0482)
SEZs = 1 × Hi-tech = 1	-0.0887 (0.0753)	-0.0637 (0.0608)	-0.0944 (0.0872)
<i>Panel F: Distance to port</i>			
SEZs = 1	-0.381*** (0.0888)	-0.0329 (0.0313)	0.106*** (0.0305)
Close to port = 1	0.0363 (0.0219)	-0.0270 (0.0281)	0.00368 (0.0160)
SEZs = 1 × Close to port = 1	0.302*** (0.0728)	0.0117 (0.0413)	-0.169** (0.0654)
<i>Panel G: Distance to airport</i>			
SEZs = 1	0.285 (0.305)	-0.0894* (0.0444)	-0.0459 (0.0558)
Close to airport = 1	-0.0848 (0.0624)	0.00670 (0.0468)	0.0862*** (0.0137)
SEZs = 1 × Close to airport = 1	-0.271 (0.306)	0.0404 (0.0298)	-0.0341 (0.0398)

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: Time-varying covariates for creating the propensity scores include log of asset, log of sales and the history of the outcome variable. Time-invariant covariates include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. The weights are derived using CBPS and Eq. (6). Standard errors are clustered at the SEZs level. After the weights are estimated, the Eq. (7) is re-estimated for different samples. Large-size firms are firms with log of sales above the sample median in 2005. Young takes the value of one if firm's age is younger than the median in 2005. Close to port and airport are dummy variables taking the value of 1 if the distance is smaller than the median distance in the sample. All specifications control for year and state fixed effects.

Table 9
Time-varying treatment effect of SEZs using private and public SEZs.

	Private SEZs			Public SEZs		
	(inside) TFP growth	($r - 5\%$) TFP growth	($r - 15\%$) TFP growth	(inside) TFP growth	($r - 5\%$) TFP growth	($r - 15\%$) TFP growth
SEZs	0.194** (0.0743)	0.266*** (0.0791)	0.110 (0.138)	-0.299 (0.0525)	-0.275* (0.0240)	-0.282*** (0.00190)
Age 2005	-0.00255 (0.00279)	-0.00460 (0.00299)	-0.00302 (0.00245)	-0.0104 (0.00247)	-0.00517 (0.00110)	-0.00440** (0.000136)
Service 2005	-0.200*** (0.0440)	-0.0550 (0.0879)	-0.161* (0.0919)	0.0722 (0.0166)	0.0632* (0.00653)	0.0566*** (0.000259)
Manufacturing 2005	-0.151*** (0.0226)	-0.0276 (0.0448)	-0.0867* (0.0455)	0.0945 (0.0484)	0.0984 (0.0186)	0.0809* (0.00643)
Foreign ownership 2005	-0.0910*** (0.0299)	-0.193*** (0.0500)	-0.252*** (0.0671)	-0.159 (0.0560)	0.168** (0.00294)	0.0637** (0.00190)
Constant	0.167 (0.167)	-0.000160 (0.163)	0.245 (0.160)	0.425 (0.0686)	-0.0758 (0.0565)	-0.0538 (0.0141)
N	503	461	438	1830	1830	1767
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The outcome variable is measured as the log difference of TFP. TFP is measured using (Akerberg et al., 2015) approach. Control group are matched firms located further than 40 km away from an SEZ. Time-varying covariates for creating the propensity scores include log of assets, log of sales, and the history of the outcome variable. Time-invariant covariates include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. The weights are derived using CBPS and Eq. (6). Standard errors are clustered at the SEZs level.

Table 10
Time-varying treatment effect of SEZs on directors' salary growth.

	(inside) Dir salary growth	($r - 5\%$) Dir salary growth	($r - 15\%$) Dir salary growth
SEZs	0.107** (0.0460)	0.208*** (0.0153)	0.206*** (0.0230)
Age 2005	0.00288 (0.00279)	0.00181 (0.00227)	0.00146 (0.00190)
Service 2005	-0.185** (0.0725)	-0.449 (0.336)	-0.450 (0.353)
Manufacturing 2005	-0.238*** (0.0523)	-0.110** (0.0329)	-0.0993** (0.0363)
Foreign ownership 2005	0.529 (0.578)	0 (.)	0 (.)
Constant	0.150 (0.0987)	-0.136 (0.133)	-0.118 (0.129)
N	604	389	375
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: Control group are firms located further than 40 km from an SEZ. Time-varying covariates for creating the propensity scores include log of assets, log of sales and the history of the outcome variable. Time-invariant covariates include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. Foreign ownership dummy is omitted from the estimation using $r - 5\%$ and $r - 15\%$ because there are only domestic firms in the estimation sample. The weights are derived using CBPS and Eq. (6). Standard errors are clustered at the SEZs level.

Table 11
Time-varying treatment effect of SEZs on directors' salary growth in private and public SEZs.

	Private SEZs			Public SEZs		
	(inside) Dir salary growth	($r - 5\%$) Dir salary growth	($r - 15\%$) Dir salary growth	(inside) Dir salary growth	($r - 5\%$) Dir salary growth	($r - 15\%$) Dir salary growth
SEZs	-0.198 (0.264)	-0.580 (0.569)	-0.652 (0.578)	0.288*** (0.00143)	0.196 (0.0332)	0.184* (0.0146)
Age 2005	0.00928** (0.00328)	0.00875 (0.00486)	0.0102* (0.00482)	-0.0000691 (0.00160)	-0.000171 (0.00151)	-0.0000268 (0.000770)
Service 2005	-0.110 (0.164)	0.276 (0.264)	0.275 (0.249)	-0.255 (0.110)	-0.936*** (0.000242)	-0.987*** (0.0111)
Manufacturing 2005	0.192 (0.141)	0.371 (0.365)	0.357 (0.328)	-0.227 (0.0613)	-0.117 (0.0467)	-0.115 (0.0264)
Foreign ownership 2005	-0.233* (0.119)	0 (.)	0 (.)	1.978*** (0.0191)	0 (.)	0 (.)
Constant	-0.171 (0.257)	-0.284 (0.264)	-0.405 (0.243)	0.294* (0.0444)	-0.0296 (0.00956)	-0.0124* (0.00104)
N	136	102	102	468	287	273
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: Control group are firms located further than 40 km from an SEZ. Time-varying covariates for creating the propensity scores include log of assets, log of sales and the history of the outcome variable. Time-invariant covariates include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. Foreign ownership dummy is omitted from the estimation using $r - 5\%$ and $r - 15\%$ because there are only domestic firms in the estimation sample. The weights are derived using CBPS and Eq. (6). Standard errors are clustered at the SEZs level.

managers in firms in newly established public-owned SEZs are able to increase their salaries (the last three columns of Table 11), which is in line with the idea that managers engage in non-productive activities in these zones. Furthermore, we do not find such effects for firms in privately-owned SEZs (the first three columns of Table 11). This supports our interpretation as managers running firms in privately-owned SEZs may not have the opportunities to engage easily with bureaucrats or public officials.

7. Conclusion

Incentives brought by the SEZs Act pose the natural question of whether firms directly affected by the program experience significant improvements in their performance. Our analysis is based on a unique representative geocoded dataset of firms and their assigned SEZ status covering all of India.

Our findings demonstrate that conditional on controlling for initial selection based on observables, India's SEZs program did not generate any discernible effect on average productivity growth for firms located inside the zones, on average. This goes against what policy makers generally expect. Our results also differ from results found for SEZs in China, though they are in line with earlier findings by Alkon (2018) based on a more aggregate analysis for India.

In an attempt to explain this, we focus on the possibility of distortions through engagement in non-productive activities by managers of firms in SEZs. We show that firms in newly established SEZs that are run by private owners, experience productivity growth effects. This is not the case for firms in publicly-owned SEZs. This, in the context of our theoretical framework, possibly points to negative implications from the potential for managers to engage in non-productive activities in publicly owned SEZs.

We further substantiate this interpretation by looking at the impact of the establishment of an SEZ on directors' pay. We provide evidence which is in line with the idea that rent-seeking on the part of companies' directors may contribute to the negative productivity effects, as we find that directors' pay growth increases as a result of establishing an SEZ. Moreover, we establish that this is only the case in SEZs that are publicly-owned, but not in zones that are run by private owners. Overall, our findings are in line with the idea that the apparent inefficiency of the SEZ program may be driven by political interference and rent-seeking by firms' managers. This, from a policy perspective, may suggest that the design of the SEZ policy in India may need to be re-evaluated.

Due to the growing popularity of SEZs as policy tools in developing countries, further efforts should be carried out to analyze the effectiveness of the program in India as well as other countries. For example, our focus is on the productivity growth of existing firms, leaving aside considerations about labor market implications at the firm level or SEZs' implications in terms of attracting new firm formation. As our analysis shows, it is important to provide constructive advice for policy makers on the local developmental implications of SEZ programs.

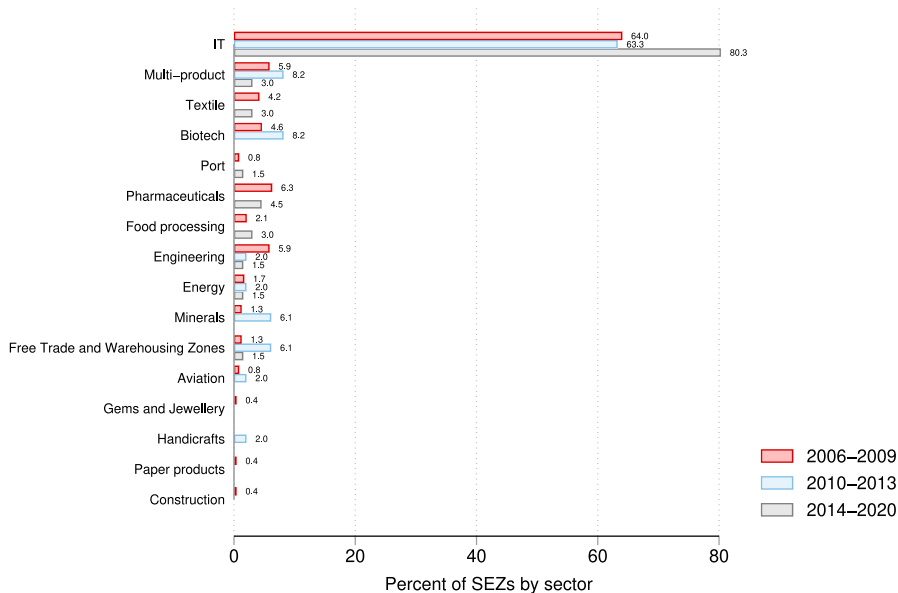


Fig. A.1. Sector-wise distribution of SEZs over time.

Table A.1

Comparison of SEZs notified under the 2005 Act and converted SEZs established prior to the 2005 Act.

	(1)					(2)				
	SEZs					SEZs established before 2005 Act				
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
Notification date	354	2009.69	3.92	2006	2020	19	2002.89	1.70	2000	2005
Area in ha	354	107.80	411.82	1.05	6456.33	19	150.58	239.12	2.02	1052.18
Radius in meters	354	396.68	431.65	57.82	4533.34	19	576.64	393.63	80.19	1830.08
Commencement of operation						19	1996.05	12.19	1965	2006

Data availability

The authors do not have permission to share data.

Appendix A

A.1. Figures

See Fig. A.1.

A.2. Tables

See Tables A.1–A.9.

A.3. Proof of the proposition

The sufficient condition for the maximization problem states that principal minors should alternate in sign, starting from the negative. Thus, $|H_{11}| < 0$ and $|H| > 0$. In our model,

$$|H_{11}| = A_{e_p e_p} f(k) + \xi_{e_p e_p} - \theta_{ee} < 0 \tag{8}$$

$$|H| = (A_{e_p e_p} f(k) + \xi_{e_p e_p} - \theta_{ee})(A(e_p)) f_{kk} - (A_{e_p} f_k)^2 > 0 \tag{9}$$

Table A.2
The establishment of SEZs over the 2006–2020 period.

	Frequency	Percent	Cum. percent
2006	54	15.25	15.25
2007	89	25.14	40.40
2008	50	14.12	54.52
2009	45	12.71	67.23
2010	20	5.65	72.88
2011	14	3.95	76.84
2012	6	1.69	78.53
2013	10	2.82	81.36
2014	5	1.41	82.77
2015	3	0.85	83.62
2016	11	3.11	86.72
2017	30	8.47	95.20
2018	4	1.13	96.33
2019	9	2.54	98.87
2020	4	1.13	100.00
Total	354	100.00	

For the proof of Proposition 1, we use the implicit function theorem to derive the comparative statics of how optimal values change in response to c .

$$\frac{\partial e_p^*}{\partial c} = \frac{b_1 a_{22} - b_2 a_{12}}{|H|} \tag{10}$$

$$\frac{\partial k^*}{\partial c} = \frac{-b_1 a_{21} + b_2 a_{11}}{|H|} \tag{11}$$

where $b_1 = -\xi_{e_p c}$ and $b_2 = 0$ is the direct effect of political connections on the FOC for e_p and k . Then,

$$\frac{\partial e_p^*}{\partial c} = \frac{(-\xi_{e_p c})(A(e_p)f_{kk})}{|H|} < 0 \tag{12}$$

$$\frac{\partial k^*}{\partial c} = \frac{(-\xi_{e_p c})A(e_p)f_k}{|H|} > 0 \tag{13}$$

which implies that $\frac{\partial e_{np}^*}{\partial c} > 0$ and $\frac{\partial A^*}{\partial c} < 0$.

A.4. TFP estimation

Consider the following Cobb–Douglas production technology with Hicks-neutral productivity in logarithmic form:

$$q_{it} = \beta_l l_{it} + \beta_k k_{it} + \omega_{it} + \epsilon_{it}, \tag{14}$$

where q_{it} is the logarithm of value added, l_{it} and k_{it} denote the log of labor and capital inputs, respectively, all of which are observed. There are two econometrically unobserved terms: ω_{it} and ϵ_{it} . The latter term represents shocks to the production that are not observed by the firm before making the input decision at time t . In contrast, ω_{it} represents productivity shocks that are potentially observed by the firm while making the input decision. To illustrate, the examples of ω_{it} might be the managerial ability of a firm, the expected delays and down-time due to a machine breakdown, the expected amount of rainfall at a farm, etc. On the other hand, ϵ_{it} represents the deviation from the predicted rainfall or the expected delay time, a sudden breakage of a machinery and other unexpected shocks or a measurement error.

The challenge in obtaining consistent production function estimates lies in the correlation between the unobserved productivity shocks and the input decision. The decision of a firm on the production inputs (l_{it} , k_{it}) will most likely depend on the observed by the firm ω_{it} , which makes OLS estimates of β_l and β_k inconsistent.

The control function approach proposed by Olley and Pakes (1996) and extended by Levinsohn and Petrin (2003) and the (Ackerberg et al., 2015) technique are applied. The unobserved productivity shocks are proxied by the following material demand function:

$$m_{it} = m_t(l_{it}, k_{it}, \omega_{it}) \tag{15}$$

By inverting (15), productivity is expressed as:

$$\omega_{it} = h_t(l_{it}, k_{it}, m_{it}) \tag{16}$$

The estimation then proceeds in two steps. In the first stage, Eq. (14) is estimated, where ω_{it} is substituted with its proxy from Eq. (16). Thus, the estimation equation is as follows:

$$q_{it} = \Phi_t(l_{it}, k_{it}, m_{it}) + \epsilon_{it}, \tag{17}$$

where $\Phi_i(l_{it}, k_{it}, m_{it}) = \beta_l l_{it} + \beta_k k_{it} + h_i(l_{it}, k_{it}, m_{it})$. Important to notice that none of the coefficients $\beta = (\beta_l, \beta_k)$ are estimated in the first stage due to perfect collinearity, however, the predicted output is used to express the productivity:

$$\omega_{it}(\beta) = \hat{\Phi}_{it} - \beta_l l_{it} - \beta_k k_{it}. \tag{18}$$

In the second stage, moment conditions are formed to identify the production function coefficients. Thus, the law of motion for productivity explains the current level productivity as a function of productivity in the previous period and the innovation term ξ_{it} in the productivity shock ω_{it} :

$$\omega_{it} = g_i(\omega_{it-1}) + \xi_{it}. \tag{19}$$

Non-parametrically regressing $\omega_{it}(\beta)$ on $g(\omega_{it-1})$, the innovation term $\xi_{it}(\beta) = \omega_{it}(\beta) - E[\omega_{it}(\beta) | \omega_{it-1}(\beta)]$ is obtained from the residuals of the regression.

Given the timing assumptions that k_{it} and e_{it} were decided at $t - 1$ and that lagged labor, l_{it-1} , is chosen at $t - b - 1$, prior to m_{it} being chosen at t , where $0 < b < 1$, implies that the innovation term in productivity shocks is uncorrelated with all input choices prior to t . Thus, the moment conditions are:

$$E\left(\xi_{it}(\beta) \begin{pmatrix} l_{it-1} \\ k_{it} \end{pmatrix}\right) = 0 \tag{20}$$

Once the production function coefficients have been estimated, a firm-level total factor productivity is calculated as:

$$\hat{\omega}_{it} = \hat{\Phi}_{it} - \hat{\beta}_l l_{it} - \hat{\beta}_k k_{it}. \tag{21}$$

To account for industry differences in the production technology, the elasticities are estimated by industry. Some industries are combined to ensure enough observations in each group.

Value added is measured as firm revenue less expenditures on material inputs. Material inputs are defined as the sum of expenditures on raw material expenses and consumption of stores and spares plus energy costs. Labor input is measured by the total wage bill which comprises wages, social security contributions, bonuses, paid-leaves, etc. Capital input is represented by the gross fixed assets which include the movable, immovable and intangible assets of a firm.

Wages, value added, capital, energy expenses and intermediate materials are deflated by the 2-digit NIC-Industry Wholesale Price Index. Variables of firms in the service sector are deflated by the yearly WPI. All variables are monotonically transformed using the inverse hyperbolic sine (asinh). The inverse hyperbolic sine closely parallels log transformation but is defined at zero.²⁵ The interpretation of the regression coefficients is similar to log-transformed variables (Card and DellaVigna, 2020; Bahar et al., 2019).

Additionally, alternative measures of TFP, namely the approach of Levinsohn and Petrin (2003) is calculated and presented in the correlation Table A.10.

A.5. Event studies

We present an event-study design based on a within-firm estimator. This illustrates the development of the variables of interest in the years preceding and following the establishment of SEZs for each firm i . Accounting for differential timing of treatment, the approach thus handles pre-trends and post-treatment dynamics.²⁶

As suggested by De Chaisemartin and d'Haultfoeuille (2020), we employ a newly proposed estimator which estimates the average treatment effect in the groups that switch treatment at the time of switching. Thus, at each time a difference-in-differences is estimated based on groups that change their treatment status at time t relative to groups with stable treatment. Subsequently, these difference-in-differences are averaged over the whole observation period with weights depending on the number of switchers at each time. As a robustness check, we further present results using an alternative estimator proposed by Borusyak et al. (2021) and a two-way fixed effects specification.

We restrict the choice of the control group to a sub-sample of firms located further than 40 kilometers away from the zones to alleviate the concern that the control group is affected by the treatment. The event window is restricted to 10 years before and after treatment. This requires assumptions about the nature of the effect outside of the window. The model which excludes all periods outside of the event window makes an implicit assumption that treatment effects drop to zero outside of the event window. Following the suggestion of Schmidheiny and Siegloch (2019), we bin the endpoints assuming constant treatment effects before and after the event. Binning introduces important parameter restrictions which ensure that the model is identified econometrically.

²⁵ The inverse hyperbolic sine (asinh) is defined as $\ln(\alpha + \sqrt{\alpha^2 + 1})$. For $\alpha \geq 2$, $\text{asinh}(\alpha) = \ln(2) + \ln(\alpha)$ and $\text{asinh}(0) = 0$.

²⁶ Recent econometric literature has raised concerns regarding the unbiasedness of the two-way fixed effects estimator in the presence of treatment heterogeneity and staggered treatment adoption (Borusyak et al., 2021; Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). When the treatment effects are heterogeneous, the average treatment effect in the two-way fixed effects estimator is a weighted average of all the heterogeneous treatment effects. In the presence of both the variation in treatment timing and treatment heterogeneity, the average treatment effect is identified in part through the changes over time within already treated units. Consequently, some of the weights on the heterogeneous treatment effect underlying the average treatment effect can be negative, particularly for groups treated for many periods. Specifically, De Chaisemartin and d'Haultfoeuille (2020) show that even though all the average treatment effects are positive, the linear regression coefficient may be negative. This leads to difficulties in interpreting a two-way fixed effects estimator.

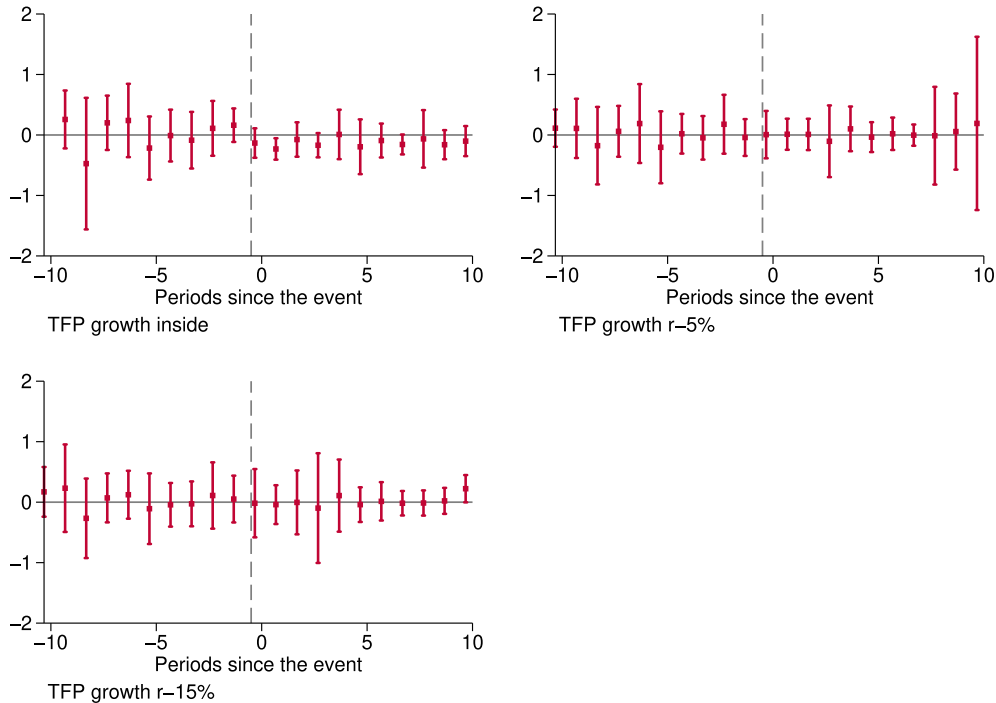


Fig. A.2. Event study graph for TFP growth using (De Chaisemartin and d’Haultfoeuille, 2020). 95% confidence interval is reported. Standard errors are clustered at the SEZs level.

We estimate the following regression equation:

$$\Delta Y_{it} = \alpha + \sum_{k \geq -10}^{10} \beta_k \times D_{it}^k + \gamma X_{it} + \phi_i + \lambda_t + \epsilon_{it}, \tag{22}$$

where event dummies for the window $-10 \leq k \leq 10$ are created. D_{it}^k represents the SEZ program establishment event. $D_{it}^k = 1$ if the observations’ period of firm i at time t relative to the first period when firm i is treated by an SEZ equals the value of k . D_{it}^k is always 0 for never-treated firms. ΔY_{it} is the outcome variable defined as TFP growth for firm i at time t . ϕ_i are firm fixed effects that control for time-invariant differences between firms. λ_t represents year fixed effects that control for business cycle trends common across all firms in India. Standard errors are clustered at the SEZs level to account for spatial correlation in the error term. X_{it} is a vector of controls including an exporter dummy, manufacturing and services dummy and foreign ownership dummy which change over time. The coefficient of interest, β_k , identifies the effect of SEZs program k years following its implementation.

To visualize the dynamic effects, the point estimates together with 95% confidence intervals are plotted in Fig. A.2. Importantly, looking at pre-treatment trends, we cannot reject the null hypothesis of no significant differences between treated and control groups prior to treatment.

We also provide a robustness check using two-way fixed effects and Borusyak et al. (2021) estimators in Fig. A.3. The point estimates of a newly proposed estimator closely parallel two-way fixed effects results, with the latter having wider confidence intervals. The broader picture, however, remains the same.

Since the sample used for the analysis is unbalanced, it may create a concern that the attrition of firms is non-random. As an additional robustness check, we keep only those treated firms that are observed for consecutive ten years before and after the treatment and re-estimate Eq. (22) for the sample of balanced treated firms. We keep the control firms as before not to lose observations. Fig. A.4 depicts the results.

Appendix B. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.euroecorev.2024.104752>.

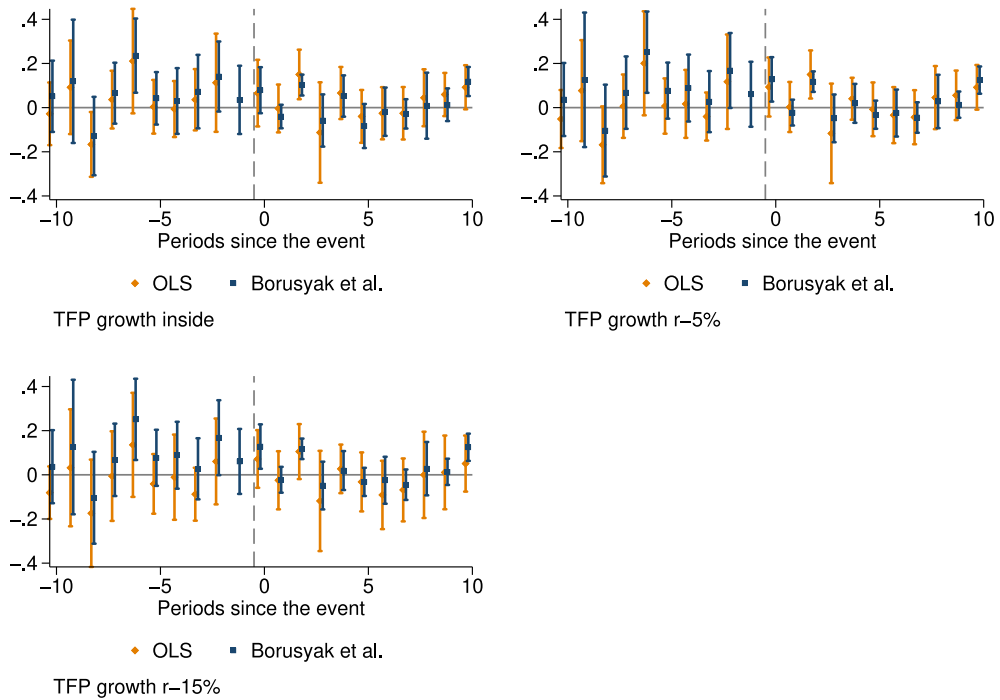


Fig. A.3. Robustness check using two-way fixed effects and Borusyak et al. (2021). 95% confidence interval is reported. Standard errors are clustered at the SEZs level.

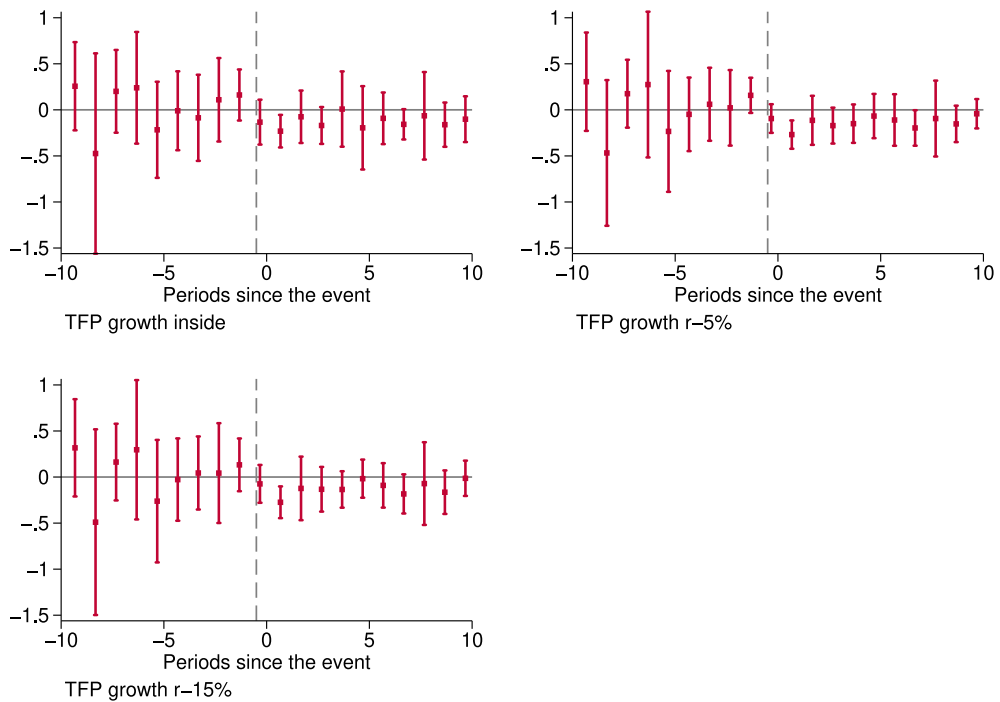


Fig. A.4. Event study graph for TFP growth. 95% confidence interval is reported. The sample of treated firms is balanced for the event window [-10, 10]. Standard errors are clustered at the SEZs level.

Table A.3
Summary statistics of firms by industry. Pooled sample for 1988–2020.

	Frequency	Percent	Cum. percent
Crop & animal production	5795	2.080	2.080
Forestry & logging	6550	2.351	4.431
Fishing & aquaculture	83	0.0298	4.461
Mining of coal & lignite	485	0.174	4.635
Extraction of crude petroleum & natural gas	284	0.102	4.737
Mining of metal ores	505	0.181	4.919
Other Mining & quarrying	1937	0.695	5.614
Food	11 479	4.121	9.735
Beverages	2176	0.781	10.52
Tobacco	322	0.116	10.63
Textiles	10 792	3.874	14.51
Wearing apparel	646	0.232	14.74
Leather	1040	0.373	15.11
Wood	762	0.274	15.38
Paper	3309	1.188	16.57
Printing & reproduction of recorded media	191	0.0686	16.64
Coke & refined petroleum products	1182	0.424	17.06
Chemicals	14 919	5.355	22.42
Pharmaceuticals	5150	1.849	24.27
Rubber & plastics products	7548	2.710	26.98
Other non-metallic mineral	4447	1.596	28.57
Basic metals	10 423	3.742	32.32
Fabricated metal products	3849	1.382	33.70
Computer, electronic & optical products	4330	1.554	35.25
Electrical equipment	6139	2.204	37.46
Machinery & equipment	7518	2.699	40.15
Motor vehicles	4001	1.436	41.59
Other transport equipment	1018	0.365	41.96
Furniture	191	0.0686	42.02
Other manufacturing	8977	3.222	45.25
Electricity, gas etc. supply	2375	0.853	46.10
Water collection	63	0.0226	46.12
Construction of buildings	6040	2.168	48.29
Civil engineering	4675	1.678	49.97
Specialized construction activities	327	0.117	50.09
Wholesale & retail trade & repair of motor-vehicles and motorcycles	1114	0.400	50.49
Other wholesale & retail trade	33 816	12.14	62.63
Retail trade	1325	0.476	63.10
Land transport & transport via pipelines	988	0.355	63.46
Water transport	617	0.221	63.68
Air transport	353	0.127	63.80
Warehousing & support activities for transportation	2581	0.927	64.73
Postal and courier activities	182	0.0653	64.80
Accommodation	3554	1.276	66.07
Food and beverage service activities	21	0.00754	66.08
Publishing activities	965	0.346	66.43
Music publishing activities	1046	0.375	66.80
Programming and broadcasting activities	65	0.0233	66.82
Telecommunications	1481	0.532	67.36
Computer programming	6661	2.391	69.75
Information service activities	822	0.295	70.04
Financial service activities	62 925	22.59	92.63
Insurance, reinsurance and pension funding	25	0.00897	92.64
Other financial activities	4884	1.753	94.39
Real estate activities	24	0.00862	94.40
Legal & accounting activities	7	0.00251	94.40
Activities of head offices	2706	0.971	95.37
Architecture & engineering activities	1207	0.433	95.81
Scientific research & development	160	0.0574	95.87
Advertising & market research	801	0.288	96.15
Other scientific activities	136	0.0488	96.20
Rental and leasing activities	4592	1.648	97.85
Employment activities	185	0.0664	97.92
Travel agency etc. activities	481	0.173	98.09
Security & investigation activities	184	0.0661	98.16
Office administrative etc. activities	1316	0.472	98.63
Public administration & defence	87	0.0312	98.66
Education	549	0.197	98.86
Residential care activities	1725	0.619	99.48

(continued on next page)

Table A.3 (continued).

	Frequency	Percent	Cum. percent
Creative, arts & entertainment activities	356	0.128	99.60
Sports activities	335	0.120	99.72
Activities of membership organizations	562	0.202	99.92
Repair of computers	175	0.0628	99.99
Other personal service activities	34	0.0122	100
Total	278 575	100	

Table A.4
Number of treated firms.

	inside	$r - 5\%$	$r - 15\%$
Number of firms	365	342	306

Table A.5

Balancing tests for Table 3.

	Mean_T	Mean_C	Diff	Std_diff	SD_pool	Var_ratio
TFP growth 2007	-.0306041	.0371246	-.0677286	-.1964851	.3447011	.3638072
TFP growth 2008	.0498761	.0025674	.0473087	.1280457	.3694671	7.073546
TFP growth 2009	.1035154	-.0096373	.1131527	.3384436	.3343327	1.304905
TFP growth 2010	.0738164	.105203	-.0313866	-.1157659	.2711208	1.699172
TFP growth 2011	-.0490866	-.0244979	-.0245887	-.0747262	.3290501	1.855059
TFP growth 2012	-.0884978	-.0154257	-.0730721	-.2311986	.3160575	1.213906
TFP growth 2013	.0061014	-.0096164	.0157178	.0484615	.3243354	.5791143
TFP growth 2014	-.0981652	.0283833	-.1265485	-.3011092	.4202743	1.845388
TFP growth 2015	-.027415	-.0344038	.0069889	.0165275	.4228619	3.153742
TFP growth 2016	.0537141	-.0110184	.0647325	.2001206	.3234675	1.250471
TFP growth 2017	.0560137	-.0361295	.0921432	.243853	.3778639	.201018
TFP growth 2018	-.0317368	.0154634	-.0472002	-.1014127	.4654265	.2190282
TFP growth 2019	-.0730152	-.0647591	-.008256	-.0453632	.1819982	1.32999
Sales, log 2007	6.229466	5.84271	.3867565	.1779721	2.17313	.536271
Sales, log 2008	6.198675	5.769638	.4290375	.1951129	2.19892	.5639559
Sales, log 2009	6.295255	5.901783	.3934721	.1968921	1.998414	.8287457
Sales, log 2010	6.145384	5.896509	.2488745	.1212924	2.051856	.7533541
Sales, log 2011	6.094609	5.913083	.1815258	.0892621	2.033627	.7286819
Sales, log 2012	5.968552	5.78759	.1809623	.0874656	2.068953	.6975718
Sales, log 2013	6.146463	5.780265	.3661982	.1906571	1.920717	.613986
Sales, log 2014	6.092594	5.675578	.4170167	.2199246	1.896181	.6223785
Sales, log 2015	6.057592	5.621607	.4359853	.2329859	1.871294	.6371974
Sales, log 2016	5.93949	5.497107	.4423828	.237311	1.864148	.6855392
Sales, log 2017	5.909683	5.450284	.4593992	.2398691	1.915208	.7302015
Sales, log 2018	5.792835	5.354435	.4384006	.2310138	1.897724	.6557915
Sales, log 2019	5.698884	5.345936	.3529477	.1984941	1.778127	.5898571
Sales, log 2020	5.446124	5.215475	.2306493	.1418637	1.625851	.5580802
Assets, log 2007	6.553469	5.970023	.5834458	.2680736	2.176439	1.102208
Assets, log 2008	6.484842	6.020497	.4643449	.2296701	2.021791	1.198935
Assets, log 2009	6.452284	5.985213	.4670717	.2312575	2.019704	1.21575
Assets, log 2010	6.356445	5.936503	.4199423	.2068788	2.029896	1.145388
Assets, log 2011	6.312437	5.889947	.4224902	.2100584	2.011299	1.126895
Assets, log 2012	6.229611	5.81991	.4097009	.2060746	1.988119	1.110805
Assets, log 2013	6.311796	5.818331	.4934653	.2598761	1.898848	1.012797
Assets, log 2014	6.201204	5.768886	.4323175	.2338831	1.848434	1.112378
Assets, log 2015	6.216414	5.717884	.4985301	.272273	1.830993	1.125967
Assets, log 2016	6.158782	5.64803	.5107515	.2854919	1.789022	1.120922
Assets, log 2017	6.088455	5.59418	.4942748	.2799153	1.765801	1.145587
Assets, log 2018	5.885549	5.536142	.3494069	.2025026	1.725444	1.064753
Assets, log 2019	5.74165	5.476276	.2653737	.1602919	1.655566	1.056533
Assets, log 2020	5.499846	5.303417	.1964287	.1257027	1.562645	.9288343
Age 2005	44.19048	43.675	.5154762	.0275483	18.7117	1.083739
Services 2005	.2380952	.1	.1380952	.3861858	.3575875	2.063492
Manufacturing 2005	.5714286	.775	-.2035714	-.4426769	.4598646	1.437788
Foreign 2005	.047619	.05	-.002381	-.0109198	.2180389	.9774436
Gujarat	.1428571	.35	-.2071429	-.4582049	.4520748	.5510204
Maharashtra	.2857143	.525	-.2392857	-.4777888	.500819	.8378088
Tamil Nadu	.5238095	.075	.4488095	1.058497	.4240064	3.680824

Note: The table is based on estimation for the last year in the sample. As rules of thumb, the balancing is considered to be achieved if the variance ratio is between 0.5 and 2, and standardized difference < 0.2 for key variables.

Table A.6

Estimating spillovers: Time-varying treatment effect of SEZs on the growth rate of TFP of firms in 0–5 km, 5–10 km, 10–15 km distance bands.

	(0–5 km) TFP growth	(5–10 km) TFP growth	(10–15 km) TFP growth
SEZs	–0.0620 (0.101)	–0.0511 (0.0622)	–0.0768** (0.0354)
Age 2005	–0.0121*** (0.00116)	–0.0114*** (0.000532)	0.00253*** (0.000899)
Service 2005	0.121 (0.183)	–0.0348* (0.0194)	0.0489 (0.0560)
Manufacturing 2005	–0.273 (0.201)	0.0766 (0.0642)	0.0832 (0.0560)
Foreign ownership 2005	–0.0811 (0.0601)	0.136 (0.110)	–0.190** (0.0834)
Constant	0.759*** (0.131)	0.485*** (0.0240)	–0.240*** (0.0770)
N	16 577	10 076	6598
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: The outcome variable is measured as the log difference of TFP. TFP is measured using (Akerberg et al., 2015) approach. Time-varying covariates for creating the propensity scores include log of assets, log of sales, and the history of the outcome variable. Time-invariant covariates include age, a foreign ownership dummy, dummies for manufacturing and service industries in 2005 and state dummies. The weights are derived using CBPS and Eq. (6). Standard errors are clustered at the SEZs level.

Table A.7

Estimation results using standard propensity score re-weighting.

	(inside) TFP growth	($r - 5\%$) TFP growth	($r - 15\%$) TFP growth
SEZs	0.0141 (0.0155)	0.0102 (0.0120)	0.00929 (0.0150)
Age 2005	–0.00131 (0.00108)	0.000382 (0.000373)	–0.0000437 (0.000288)
Service 2005	0.107 (0.0648)	0.0325 (0.0308)	0.0435 (0.0380)
Manufacturing 2005	0.0578 (0.0409)	0.0105 (0.0106)	0.0107 (0.0109)
Foreign ownership 2005	–0.0227 (0.0362)	–0.0120 (0.0170)	–0.00401 (0.0160)
Assets log 2005	–0.0267* (0.0129)	–0.00882 (0.00539)	–0.0125** (0.00595)
Sales log 2005	0.0415** (0.0177)	0.0149*** (0.00522)	0.0171** (0.00657)
Constant	–0.0562 (0.0574)	–0.0257 (0.0448)	–0.000867 (0.0357)
Observations	3421	5979	5752
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: First, we estimate the propensity score matching using nearest-neighbor as follows: $P = Pr(D_i = 1 | X_i^0)$, where $D_i = 1$ if firm i is in an SEZ and zero for never-treated firms. X_i^0 is a vector of pre-treatment covariates including age, dummies for manufacturing and service sectors, a foreign ownership dummy all measured in 2005, a time-invariant state dummies, and mean of log of sales and mean of log of assets for 2004–2006. Once the probabilities are estimated, they are transformed into weights. The treatment group receives a weight of $\frac{1}{Pr(D_i=1|X_i^0)}$ and the control group is weighted by $\frac{1}{1-Pr(D_i=1|X_i^0)}$. Standard errors are clustered at the SEZs level.

Table A.8
Estimation results using weighted propensity score regression and Panel ASI.

	(1) Labor productivity growth	(2) Sales growth	(3) Wages growth	(4) Asset growth	(5) Employment growth
SEZs	-0.111 (0.169)	-0.260** (0.110)	0.132 (0.201)	0.0490 (0.0828)	-0.173 (0.187)
Age	-0.0192*** (0.00259)	-0.00783*** (0.00282)	0.0103*** (0.00289)	0.00171 (0.00192)	0.000732 (0.00437)
Private	0.413* (0.247)	-0.107 (0.309)	-0.725*** (0.217)	-0.0709 (0.422)	-0.0534 (0.324)
Assets log	0.193*** (0.0209)	0.676*** (0.0349)	0.590*** (0.0316)	0.971*** (0.0184)	0.363*** (0.0292)
Constant	8.831*** (0.520)	5.831*** (0.690)	4.845*** (0.631)	-1.274** (0.524)	-3.207*** (0.732)
Observations	3169	2664	2249	1526	1311
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: First, we estimate the propensity score matching using CBPS as follows: $P = Pr(D_d = 1 | X_d^0)$, where $D_d = 1$ if district d hosts an SEZ and zero for never-treated districts. X_d^0 is a vector of pre-treatment covariates including log distance to port, employment rate in 2001, log of average district-level GDP between 2000 and 2004, log population in 2001, and time-invariant state dummies. Once the probabilities are estimated, they are transformed into weights. The treatment group receives a weight of $\frac{1}{Pr(D_d=1|X_d^0)}$ and the control group is weighted by $\frac{1}{1-Pr(D_d=1|X_d^0)}$. Common sample is imposed. Standard errors are clustered at the district level.

Table A.9
Estimation results using weighted propensity score regression and informal non-agricultural enterprise survey data.

	(1) # firms, log	(2) Sales, log	(3) # employees, log
SEZ	0.313 (0.250)	-0.553 (0.652)	-0.219 (0.184)
Constant	5.947*** (0.0598)	11.93*** (0.161)	0.550*** (0.0455)
Observations	340	340	340
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Note: First, we estimate the propensity score matching using CBPS as follows: $P = Pr(D_d = 1 | X_d^0)$, where $D_d = 1$ if district d hosts an SEZ and zero for never-treated districts. X_d^0 is a vector of pre-treatment covariates including log distance to port, employment rate in 2001, log of average district-level GDP between 2000 and 2004, log population in 2001, and time-invariant state dummies. Once the probabilities are estimated, they are transformed into weights. The treatment group receives a weight of $\frac{1}{Pr(D_d=1|X_d^0)}$ and the control group is weighted by $\frac{1}{1-Pr(D_d=1|X_d^0)}$. Common sample is imposed. Standard errors are clustered at the district level.

Table A.10
Correlation table for different TFP measures.

	TFP ACF	TFP LP
TFP ACF	1	
TFP LP	0.816***	1

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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