# Land Acquisition and Sectoral Composition: Evidence from India*

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**Abstract**

Fragmented land ownership and ill-defined property rights are key obstacles to large-scale industrial development in emerging economies. To facilitate private investment, governments increasingly rely on compulsory land acquisition. I study the effects of this practice on industrial composition and local employment by leveraging a natural experiment: an unexpected reform that prohibited compulsory acquisition for Indian Special Economic Zones (SEZs). Using a novel dataset of the universe of SEZ proposals, I find that the ban significantly altered industrial composition: the share of manufacturing SEZ proposals decreases by almost 50 percent in states with prior compulsory acquisition mandates. This decline was driven primarily by areas with low land concentration, where private assembly is most difficult. Finally, I show that manufacturing SEZs after the reform are associated with significantly higher local employment than SEZs before the reform. Despite the reduction in the number of operational manufacturing SEZs, this cohort together contributed four times more to aggregate employment than those established under the compulsory acquisition regime.

**JEL classification:** L16, O14, Q15
**Keywords:** Land Reform, Industrial Policy, Industry Mix, India.

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## I. INTRODUCTION

In most developing countries, land institutions and policies significantly hinder the transition from agriculture to manufacturing (Adamopoulos et al., 2022). One mechanism through which these institutions hinder industrial development is the difficulty of land acquisition; when property rights are ill-defined, private land acquisition comes with risk of litigation from both landowners and nonowners over land titles and compensation (Chakravorty, 2013; Haghpanah et al., 2024). Even when property rights are secure, investors face high bargaining costs and the risk of holdout if land is split across many landowners (Miceli and Sirmans, 2007). To overcome these barriers to private investment, policymakers have increasingly adopted compulsory acquisition, or eminent domain, to facilitate the implementation of industrial policy (Lindsay et al., 2017).[1] An example is the Chinese government, which acquired five percent of all arable land for the purposes of non-agricultural activity between 1998 and 2004 (Kahn, 2006). However, there is no systematic evaluation of how compulsory acquisition impacts industrial development.

There are two primary empirical challenges to answering this question. First, land reforms are often endogenous and anticipated, allowing investors to adjust their investment decisions ahead of changes in policy. Second, there is a lack of suitable data as intentions around entry, especially in connection to land reform, are generally unobserved. In this paper, I overcome these challenges by leveraging the institutional context of Special Economic Zones (SEZs) in India. SEZs – large, demarcated areas with more attractive business and trade laws – are a key industrial policy in India: in 2021-2022, the 268 operational SEZs were responsible for almost 20 percent of India's total exports.[2]

To overcome concerns of reverse causality and anticipation of land reform, I exploit an unexpected reform that prohibited compulsory acquisition for Indian SEZs. The Indian SEZ Act, enacted in 2005, allows both public and private developers to set up SEZs. To attract private investment in frictional land markets, seven out of 39 State Governments mandated expropriating land for SEZ developers (Levien, 2012). Then, in 2007, a large protest against an SEZ in West Bengal was violently suppressed by the state police, resulting in at least fourteen casualties (Patra, 2019). In direct response to this tragedy, the Central Government abruptly prohibited compulsory acquisition for SEZs (SEZ Board of Approval, 2007). This ban significantly increased land acquisition costs, and especially in those states with a compulsory acquisition mandate for

[1] According to Land Matrix (2023), land deals to facilitate non-agricultural activity more than quintupled in the last twenty years, amounting to 552,566.99 square kilometers – almost equivalent to France's total land area. At least 25 percent of these deals involved a governmental body.

[2] Retrieved from SEZ India (2022); data on overall exports is from Press Information Bureau (2022).

SEZ developers.

To capture whether the reform changed the extensive margin of developers participating in industrial policy, I construct a unique dataset comprising the universe of SEZ proposals between 2006 and 2022. I scraped the minutes from 112 SEZ Board of Approval meetings, and used text analysis to compile a dataset of 1,435 proposals; for each, I identify the developer, proposed location, land area, and planned sector of operation. I supplement these records with publicly available data on SEZ notification and operation to track SEZ development over time. Finally, I merge the geocoded SEZ locations with SHRUG village-level data, allowing me to control for baseline local characteristics and measure the ban's impact on employment (Asher et al., 2021).

To guide the empirical analysis, I set up a stylized conceptual framework. I analyse the problem of the SEZ developer in light of existing theories on firm dynamics such as Hopenhayn (1992). Land acquisition costs increase convexly in plot size due to bargaining costs and the risk of holdout; under a compulsory acquisition regime, these costs are internalized by the state government. The model predicts the ban on eminent domain shifts these costs to the developer, thereby increasing the entry barrier for potential developers of SEZs. This effect is particularly pronounced for larger-scale or manufacturing SEZs and SEZs in areas with low land concentration. The model further predicts that the increased entry costs induces less productive developers of land-intensive SEZs to forego entry, thus affecting selection.

I first examine whether the reform affected the sectoral composition of SEZ developers. Methodologically, I employ a difference-in-differences (DiD) design in which I compare the reform's effects on SEZs across Indian states depending on their adoption of compulsory acquisition policy. To ensure these SEZ proposals and their locations are comparable, I include both region and date fixed effects alongsine local trends. To address the potential endogeneity of state-level compulsory acquisition mandates, I implement a triple-difference analysis that exploits within-state variation: specifically, I test whether the effect of the reform in compulsory acquisition states varies according to local land ownership concentration, as predicted by the conceptual framework. Finally, note that the reform's effect on industrial composition is properly identified insofar as treated states are not also subject to concurrent manufacturing (or services) shocks.

I find that the share of manufacturing SEZ proposals in states with compulsory acquisition mandates fell by 17.2 percentage points after the reform. With a baseline manufacturing share of 35.7 percent, this represents a almost 50 percent reduction in intentions to enter. This effect persists across all stages of SEZ development: the share of operational manufacturing SEZs, conditional on entry, is 24.5 percentage points lower. Consistent with the conceptual framework, I document that this decline is driven by areas with lower land concentration where land transaction costs are plausibly highest. My results thus show that an increase in land acquisition costs significantly affect

industrial composition by reducing the share of manufacturing in both proposed and ultimately realized SEZs.

Next, I study whether the increase in land acquisition costs following the reform altered the selection of SEZ developers. While data limitations preclude a direct productivity analysis of developers or even firms in pre- and post-reform SEZs, I provide indirect evidence that the average productivity of developers has increased after the reform. First, I find that operational SEZs that were proposed after the reform are significantly larger than their pre-reform counterparts within the same industry. Second, following Gallé et al. (2024), I analyse whether the compulsory acquisition reform impacted employment in and surrounding SEZs. Using two rounds of the Economic Census (2005-2013), I construct a sample of all villages within 50 kilometers of the 139 SEZs that became operational during this period. To account for potential spillovers that might contaminate the natural control group of villages close to the SEZ, I employ a spatial difference-in-differences strategy. Effectively, I estimate employment changes in directly treated, potentially indirectly treated and unaffected villages, and then compare whether these employment changes are different for post-reform SEZs.

I find an insignificant effect on local employment for the full sample; however, manufacturing SEZs proposed after the reform are associated with significant employment increases in their host municipality compared to pre-reform manufacturing SEZs. Specifically, nonagricultural employment in SEZ-hosting municipalities increases by 181 percent from a baseline average of 4,749 workers. This result cannot be explained by a reallocation from more land-intensive to more labor-intensive industries; rather, it appears to be driven by selection within industries. My findings suggest that compulsory acquisition policies do not only dictate the scale of participation in industrial policy, but also its impact on local labor markets. Finally, a back-of-the-envelope calculation suggests that despite the decline in the number of porjects, the contribution of post-reform manufacturing SEZs to aggregate employment is four times greater than that of their pre-reform counterparts.

My paper contributes to the literature in three distinct ways. First, it provides the first causal estimates of the impact of compulsory acquisition policy on industrial and labor market outcomes. The existing literature mainly consists of careful case studies, highlighting the negative welfare effects of households who are expropriated from, as in e.g. Cernea and Mathur (2007); Gironde and Senties Portilla (2016), or theoretical models specifying the conditions under which compulsory acquisition could foster economic development (Miceli and Sirmans, 2007; Ghatak and Mookherjee, 2014). My paper bridges this gap by empirically evaluating whether the economic consequences of the use of compulsory acquisition could justifies the welfare losses. The closest related paper to my study Blakeslee et al. (2021), finds that a land-rezoning program in Karnataka increased firm entry

and employment. I add to this by showing that while the practice of compulsory acquisition may facilitate private investment in large-scale industries, it is significantly less effective at generating the local employment growth that may be used to justify these interventions.

Second, as compulsory acquisition policy is used to overcome barriers to land acquisition, my paper contributes to the literature on land market frictions. There is extensive evidence that these frictions have a negative effect on agricultural productivity and output (e.g. Adamopoulos and Restuccia (2014); Britos et al. (2022); Foster and Rosenzweig (2022); Dasgupta and Rao (2022)), and more recently, also on manufacturing output and employment (Duranton et al., 2016; Pal et al., 2022; Sood, 2022). The closest related study is Mehta (2022), which finds that manufacturing, but not services, firms experience lower productivity and output in areas with low land concentration because manufacturing is significantly more land-intensive than services. My main contribution is to show that land market frictions also shape entry decisions, potentially impacting aggregate productivity by filtering which projects are proposed and realized, beyond merely limiting the physical expansion of existing firms.

Third, this study speaks to the optimal design of industrial policy, spefically Special Economic Zones. Despite their widespread popularity, the specific ingredients that determine their success remain poorly understood (Juhász et al., 2024). Because all other incentives provided by the Indian SEZ Act remained unchanged with the land reform, my results isolate he causal impact of land provision on policy participation and employment outcomes. This understanding is especially relevant for the Indian SEZ experience. given that the existing literature finds mixed effects on economic growth and employment (Hyun and Ravi, 2018; Görg and Mulyukova, 2022; Gallé et al., 2024), and negligible effect on broader development outcomes such as education and infrastructure (Aggarwal, 2007; Alkon, 2018). My findings suggest that land provision is an instrument that meaningfully affects whether industrial policy achieves its intended labor market goals.

The next section discusses the institutional background and the reform. Section III presents the conceptual framework and Section IV describes the data. Section V examines the impact of the compulsory acquisition reform on SEZ entry and development, and Section VI estimates the local labor market effects of this reform. Finally, section VII will conclude.

## II. Institutional framework

### *I. The Indian SEZ Act*

Since 2000, inspired by the success of Chinese SEZs, several states have introduced specific SEZ legislation to foster industrial development. Nevertheless, the popularity and prevalence of SEZs did not fully materialize until after the ratification of the Special Economic Zone Act on 23 June

2005 (Tewari, 2020). The Act centralized the development of SEZs and the benefits for firms locating inside an SEZ – those being duty-free imports, less stringent regulations around doing business and 15-year income tax benefits.[3] They can also set up a joint venture with up to 100% FDI with automatic approval, instead of the 49% threshold applicable for other Indian companies. Notably, the Indian Act differs from most other SEZ policies by allowing both public and private developers to set up an SEZ (Central Government of India). Public developers are generally state-owned investment companies, responsible for building infrastructure, managing land banks and providing incentives to local firms. Private developers are large Indian firms or, less frequently, foreign firms wishing to establish themselves in the Indian market.

The development of an SEZ proceeds through four stages. First, the developer submits a proposal to the State Government of the proposed location. The proposal must indicate the developer, the location, the sector it will be operating in, land ownership and the proposed investment and development activities, including construction of buildings and infrastructure. The developer also submits a Project Report detailing the economic and commercial feasibility of the proposed SEZ (Central Government of India, 2006).

In the second stage, the proposal is forwarded to the SEZ Board of Approval (BoA). The Board of Approval is appointed *ex officio* by the Central Government, meaning that they are nominated for the BoA by virtue of the office they hold (Central Government of India).[4] The BoA meets multiple times a year to judge whether proposals are of sufficient quality.[5] There are three elements to this judgement: the State Government must approve the proposal; the plan for the SEZ must meet the requirements and, importantly, the developer should own the land (Central Government of India).[6] All decisions on proposals are to be made with general consensus. If the developer (be it private or public) does not own the land, only in-principle approval can be granted; if there are multiple unfulfilled requirements the proposal is deferred. In the latter two cases, the proposal can be resubmitted to the BoA and discussed at a later time.

After the formal approval is received, the proposal moves to the notification stage. If the

[3] It is important to note that SEZs are not allowed to sell goods imported from the DTA *as is*, and can thus not function as nothing more but a warehouse for within-India sales (Central Government of India).

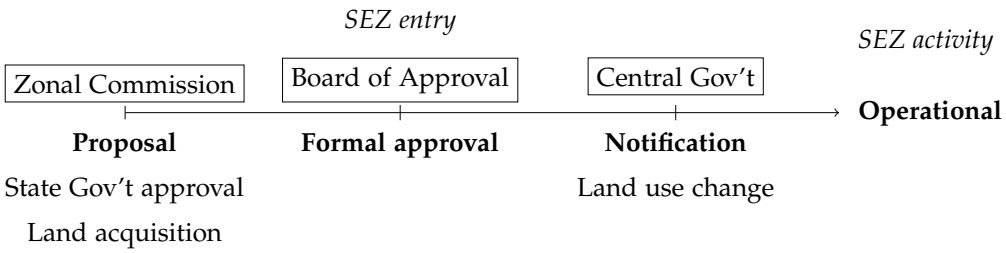
[4] The BoA always consists of four high-level officers from the Ministry of Commerce and Industry, the Ministry of Home Affairs and the Ministry of Finance; the Director General of Foreign Trade; at most ten officers from relevant ministries including the Ministry of Law and Justice and the Ministry of Science and Technology; and a Professor in the Indian Institute of Management or the Indian Institute of Foreign Trade (Central Government of India). This board is, based on the proposals to be discussed, then supplemented with a representative from each relevant State Government and the local Special Economic Zone Development Commissioner.

[5] These proposals are, based on the subsample of proposals for which I have the actual submission date, discussed in the order in which they were submitted.

[6] Alternatively, the developer must have a twenty-year lease on the land.

Central Government believes that the SEZ will bring about economic development and will be for the greater good, it changes the land use designation on the SEZ plot to industrial land (Central Government of India). Finally, after notification, the first unit can be constructed and finally start operating. The developer then sets up their own SEZ Board of Approval, which decides which firms to allow into the SEZ. This process is summarized in Figure 1. Within three years after the

**Figure 1:** *Schematic overview of the development stages of an SEZ*




law was instated, more than 500 SEZs in a variety of sectoral specializations in manufacturing and services were approved. In 2012, the total exports from the then operational SEZs equalled 87.45 billion dollars, which was a growth of 31 percent compared to the previous year and amounted to almost 20 percent of India's total imports.[7] In total, as of 29 October 2022, the Board of Approval has considered 1,435 proposals, of which more than 700 were approved.

## *II. Land in SEZs*

Land acquisition in India is a costly and complex process, as reflected by the inactivity of the land market (Chakravorty, 2013). First, land in India is extremely fragmented, with an average parcel size of 2.9 acres compared to 19.8 acres in rural China or 234 acres in the United States (Zheng et al., 2023; Sood, 2022). Second, land ownership in India is presumptive, meaning that it is characterized by possession and that titles are subject to challenge (Mishra and Suhag, 2017). Indeed, land conflicts in India constitute 66 percent of civil cases and 25 percent of cases at the Supreme Court, with land disputes in general threatening investments worth 200 billion dollars (Wahi, 2020). On average, resolving a land court case takes 20 years. Third, establishing rightful ownership is complicated further by the poor maintenance of land records and inaccuracies in land registration (Prabhakar et al., 2020). These factors generate huge land transaction costs; Sood (2022) estimated these to be on average 119 percent of the parcel market value.

Land in India is a state subject, so it or its high acquisition costs were not addressed in the 2005 SEZ Act (Public Accounts Committee, 2018). To help overcome these land transaction costs, state

[7] Retrieved from http://sezindia.nic.in/cms/export-performances.php.

governments independently decided to engage in land provision for both public and private SEZ developers through a variety of strategies – often before their proposal was to be discussed in the Board of Approval meetings.[8] The most direct of these is compulsory acquisition, mainly through the colonial Land Acquisition Act (LAA, 1894). This law allows government to forcibly acquire land for "public purposes", and was amended in 1984 to also include expropriation on behalf of private investors (Singh, 2020). Landowners received a compensation for their land proportional to the local circle rate, which is updated every five years based on local land transactions in the previous five years (Singh, 2013).[9] The state then transfers this land to the developer at said value, rather than the value of the commercial or industrial land to which it will soon be converted (Levien, 2012). The State Governments thereby shield the industrialist from the potential for holdout and the need for individual negotiations (Chakravorty, 2013).

Actual data on the use and intensity of compulsory acquisition is not public; I instead rely on state-specific SEZ policies. Seven states have instated policy that declares the government can provide land to both public and private SEZ developers. Of these states – Chandigarh (2005), Gujarat (2004), Haryana (2006), Madhya Pradesh (2003), Maharashtra (2001), Tamil Nadu (2005) and West Bengal (2003) – the first four explicitly name the Land Acquisition Act as the appropriate method to do so.

## *III. Protests and land acquisition reforms*

To understand the effect of land costs on sectoral composition, I exploit an unexpected policy change to land acquisition for SEZs. In 2006, a joint venture of the West Bengal Industrial Development Corporation and the Salim Group, a private enterprise, proposed to set up a chemical SEZ in Nandigram, West Bengal. The State Government agreed to engage in compulsory acquisition to obtain the required 4,047 hectares of land, directly affecting 29 villages and more than 100,000 people in Nandigram (Patra, 2019). When the land acquisition program started in January 2007, farmers and other locals began to barricade the area in protest. On March 14, 2007, the West Bengal State Government intervened by sending 3,000 police officers to suppress the 5,000 villagers participating in the protest. In the ensuing violence, 14 farmers were killed and more than a hundred farmers went, and remained, missing (Levien, 2012).

[8] For example, Adani Group requested 13,000 hectares of land from the Gujarat State Government to set up a conglomerate of SEZs in Mundra. By October 2007, 3,868 hectares of land, including grazing land in use by local communities, was allotted; the Government also facilitated the acquisition of private land in 14 villages (Kapoor and Upadhyay).

[9] This includes a solatium equal to 30% of the appropriate rate to compensate for the compulsory nature by which this sale is completed; note also that the infrequent updating of the circle rate implies the market value generally exceeds the circle rate (Singh, 2013).

Protests against large-scale land acquisition are not uncommon, but this violent repression was exceptional.[10] First, the Nandigram SEZ was moved to an empty strip of land in Nayachar, and ultimately scrapped. After three months, during which the BoA meetings were suspended, the Central Government announced that effective immediately, the Board of Approval would not approve any proposal for which the State Government had provided land using compulsory acquisition (SEZ Board of Approval, 2007). In other words, State Governments could no longer invoke the "public purpose" clause in the LAA to expropriate land for SEZ developers (Public Accounts Committee, 2018). It furthermore promised to revise the rules on land acquisition and resettlement and rehabilitation more formally, but new acts were not instated until 2013.[11]

After the reform, the toolkit of state governments to facilitate land acquisition was signficantly reduced but not eliminated; these remaining strategies were however significantly more cumbersome and expensive to execute. First, governments could still acquire land on behalf of the private investors provided that the landowners agreed with the deal (SEZ Board of Approval, 2007). Another strategy is for SEZ developers to draw upon land banks, which manage and distribute land plots for industrial development. These plots are however not always suitable for large projects such as SEZs (Singala et al., 2011). Finally, there is some evidence that states diverted assigned land, which is land legally distributed to marginalized communities, for industrial purposes (Singala et al., 2011; Kapoor and Upadhyay).

## III. Conceptual framework

Land assembly models such as Miceli (2011) show how low land concentration increases land acquisition costs. Private investors requiring a large plot of land will have to negotiate with a large number of landowners, and thereby run the risk of holdout. The holdout problem arises when a owner of a valuable resource decides to "hold out" on selling to potentially obtain a larger payoff later, despite the existence of a positive surplus between buyer and seller (Menezes and Pitchford, 2004).[12] Besides this risk, low land concentration generally implies larger bargaining costs, as there are many sellers to negotiate with. In the face of land market frictions, the land acquisition costs

[10] Another notable protest, albeit significantly less violent, in 2007 was against the Tata Nano car factory in Singur, West Bengal (Pal et al., 2022).

[11] A first step was the National Policy on Rehabilitation and Resettlement in October 2007, which advocates for land-for-land compensation, and preference to the landlosers for employment. An extended version of this bill, the Right to Fair Compensation and Transparency in Land Acquisition Resettlement and Rehabilitation Act, was finally ratified on 27 September 2013, coming into effect on 1 January 2014 (Ministry of Law and Justice, 2013)

[12] Empirical studies of speculative hold-out, such as Kitchens (2014) in the US or Singh (2013) in India, suggest that landowners that went to court during the land assembly process did obtain significantly larger compensations. This is mainly driven by the government's use of often old local circle rates to determine market value.

can be sufficiently high to preclude productive projects from realizing, thus acting as a barrier to entry (Miceli and Sirmans, 2007). To understand how the reform changed barriers to entry for SEZ developers, I expand on Sood (2022) and model the land acquisition cost of project $i$ in location $l$ and state $s$ as:

$$LAC_{ils} = \xi_s + m_i^{1+\gamma_{ls}}, \tag{1}$$

where $m_i$ is the total cost, or price times acreage, of the land. Then, $\xi_s$ captures the fixed costs associated with land acquisition such as collecting land records and designating a suitable location. Finally, $\gamma_{ls} \geq 0$ governs the convexity of the cost function, capturing the effect of land concentration on the risk of holdout and increased bargaining costs. Note that this parameter captures both variation *across* states, such as in the quality of land records, as well as variation *within* states in local land concentration.

Consider briefly the land acquisition costs in states with compulsory acquisition policies before the reform. First, the government's involvement implies lower fixed costs of acquisition, or $\xi_s^{CA} < \xi_s$ (Sood, 2022). Second, since compulsory acquisition by definition precludes any bargaining and therefore the holdout problem, we would see $\gamma_{ls}^{CA} << \gamma_{ls}$. This together implies that land acquisition costs are (1) on average lower and (2) increase less quickly in acreage in states that expropriate land for SEZs, provided they have similar land frictions.[13,14]

The reform moved several states from a compulsory acquisition to a non-compulsory regime, increasing $\xi_s$ and $\gamma_{ls}$ for all projects. However, to what extent the increase in $\gamma_{ls}$ increases land acquisition costs depends on the project. First, land acquisition costs increase more for larger projects, as

$$\frac{\partial^2 LAC_{ils}}{\partial m_i \partial \gamma_{ls}} > 0.$$

Connecting this hypothesis to industrialisation is straightforward, as industrial sectors are generally more land-intensive than services. As mentioned before, commercial services need on average 27.6 times less land than industry to produce one monetary unit of gross value added in the Netherlands and Spain (Batista e Silva et al., 2014). In the Indian context, Mehta (2022) documents that on average, the land requirement of a manufacturing sector is five times higher than that

[13] Note that this does not necessarily mean that there are no SEZs in states without compulsory acquisition policy, as the expected value of the project also differs across states (Miceli and Sirmans, 2007). In India, a substantial share of SEZs in states without compulsory acquisition is located in states that are more developed and otherwise industrially progressive (Tewari, 2020).

[14] One important remark here is that because land ownership in India is presumptive, there is likely not a significant difference in the probability of legal proceedings or the land owners' bargaining power for the same plot with and without compulsory acquisition, especially before the Resettlement and Rehabilitation Act (2013).

for services. Moreover, as in Behrens et al. (2022), he finds that land and labor are relatively bad substitutes for manufacturing firms. Then, as manufacturing industries are relatively more constrained by land than services industries, there will be relatively fewer manufacturing SEZ proposals after the reform. More generally, there will be a reallocation from more land-intensive to less land-intensive industries.

Second, land acquisition costs increase more in areas with lower local land concentration, as:

$$\frac{\partial^2 LAC_{ils}}{\partial \gamma_{ls}^2} > 0.$$

Note that these effects *cannot* be explained by movements in land prices or generally increased compensation after the compulsory acquisition reform, as those would not automatically decrease or increase in the degree of land concentration.

This increased barrier to entry might instigate a reallocation from more to less land-intensive industries, but can also affect selection *within* industries. If private developers are heterogeneous in ability or productivity, studying changes in SEZ entry decisions is not sufficient to understand the impact of land acquisition costs on industrialisation. To set ideas, Hopenhayn (1992) and Melitz (2003) show that the marginal entrant is the one for which the present discounted profits, as a function of their productivity, equals the entry costs. An increase in entry costs then acts as a higher barrier to entry, protecting incumbents and increasing selection (Hopenhayn, 1992). We would thus expect that the reform ensured the average entrant, especially for larger-scale industries, is more productive. Note that the goal of the Indian SEZ Act was to generate nonagricultural employment and transition out of agriculture; if the reform changed the composition of SEZ developers, this might have consequences for the effectiveness of the policy.

This hypothesis calls for an analysis on whether post-reform SEZs are established by more productive developers and therefore attract a more productive set of firms to enter into the SEZ. Ideally, one would compare the productivity of pre- and post-reform SEZ firms using an establishment-level dataset with detailed location information. This is however complicated: not only are the exact boundaries of SEZs not publicly known, most commonly used Indian firm-level datasets are not optimally suited for this exercise. For instance, the PROWESS database – used by Görg and Mulyukova (2022) – reports only headquarter addresses, leading to potential misclassification on establishment-level SEZ exposure and activity. The Annual Survey of Industries (ASI) provides richer establishment-level data but its sampling design implies insufficient coverage of SEZ-based firms for a meaningful pre- and post-reform comparison. The Economic Census (as used in Gallé et al., 2024) addresses these coverage issues but only includes employment data, thereby precluding direct productivity analysis.

Because of these data limitations, I focus on providing indirect evidence that the increased

developer after the reform. First, I consider the size of proposed SEZs: insofar productivity and size are positively correlated, I would expect the within-industry size differential between developers to widen as the reform filters out less productive developers. Second, I use the Economic Census to study whether post-reform SEZs generate more employment within the SEZ. I argue that any effects of the reform on employment are mainly driven by developers for three reasons. First, firms are not directly affected by the reform since they lease rather than purchase land. Second, even if developers' location and size choices change, firms could still opt to locate in pre-reform SEZs as almost all zones have vacancies.[15] Finally, note that the developer has complete control over which firms are allowed to set up a subsidiary in their SEZ (Central Government of India). Higher SEZ employment after the reform can then be indicative of higher developer productivity for a variety of reasons: be it an ability to invest in high-quality SEZ infrastructure, to select the most productive firms or to facilitate local agglomeration economies.

This framework establishes four testable hypotheses regarding the impact of increased land acquisition costs after the reform:

H1: A reallocation from manufacturing to services in terms of SEZ development;
H2: Despite the reallocation from large- to smaller-scale industries, SEZs become larger conditional on industrial classification;
H3: A more pronounced effect in areas with lower land concentration;
H4: A larger increase in local employment for post-reform SEZs, especially for manufacturing.

## IV. Data and descriptives

### *I. Proposal data*

The main dataset contains the universe of official Indian SEZ proposals. As mentioned before, any proposal that has obtained approval from the relevant State Government is sent to the Board of Approval. My principal data source are their meeting minutes, which are scanned documents publicly available from SEZ India (2022b). Specifically, I scraped the minutes for the 112 BoA meetings between 17 March 2006 and 29 October 2022. I then used text analysis to extract information about all SEZ proposals. The constructed dataset contains 1,435 proposals for 1,119 proposed SEZs, with information on the SEZ developer, the proposed location, the size in hectares, the sector in which they plan to operate and the decision of the Board, including, if applicable, the reason for deferral.[16] If an SEZ proposal did not provide a disaggregated enough location, I used

[15] In 2014, SEZ India documents that more than 50 percent of processing area in SEZs is vacant.

[16] The number of proposals exceeds the number of SEZs as some proposals were initially deferred and later resubmitted.

data from Land Matrix (2023), OpenStreetMap or newspaper articles to georeference the SEZs at their exact location. Furthermore, I use the COSIDICI website, which lists all State Industrial Development Corporations, to identify public developers. Finally, I obtain information on the date of notification and whether the SEZ is operational from the Ministry of Commerce. Further details on the data collection and other summary statistics are relegated to Section A.A.1 of the Appendix.

It is important to note that while I know the exact date on which a proposal was discussed, I only have information on when the proposal was submitted for proposals discussed after the protest up until 18 September. This complicates matters because proposals could be submitted since the SEZ Act was ratified on 23 June 2005, but the first BoA meeting did not take place until 17 March 2006. This generated an unusually large number of proposals to be discussed in the first few meetings. This is illustrated in Figure 2, which shows a substantial decline in the number of proposals discussed per meeting after the protest. However, because of censoring of proposal dates, this is partly a mechanical effect as the backlog of proposals is slowly cleared. I will elaborate on how this informs my empirical strategy in the next section.

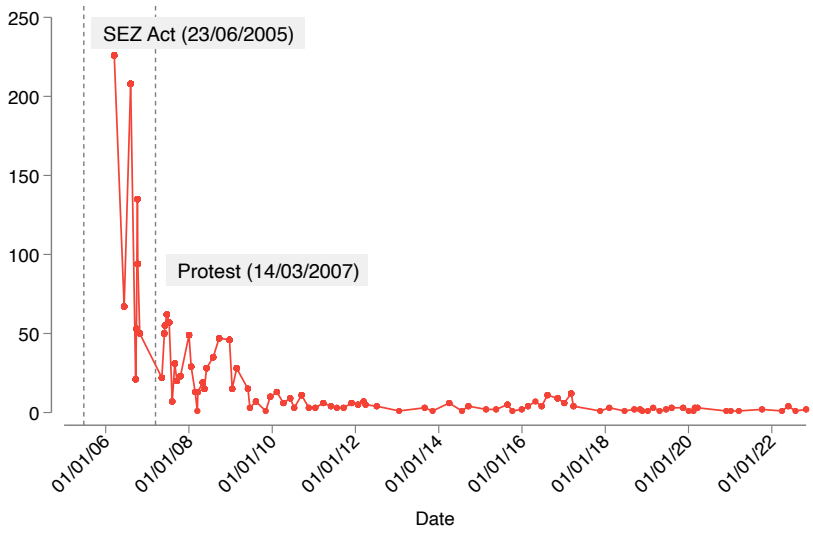



**Figure 2:** *Number of SEZ proposals per meeting*

I then spatially join the georeferenced SEZ data with the Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG) (Asher et al., 2021). SHRUG provides consistent administrative boundaries for over 500,000 villages and 8,000 towns between 1991-2021. The unit of aggregation in the SHRUG is a shrid; this is a location-based identifier that contains at least one village or town.[17] Using their rich open source data, I add economic and socio-economic variables

[17] A certain level of aggregation is unavoidable for panel data, as Indian villages are often subjected to changing

at the shrid-level. I use the 2001 round of the *Primary Census Abstract* (PCA, 2001) which contains information on municipality population, the labor force and agricultural employment. I also use the *Economic Census* (EC, 2005), which is a complete count of all non-agricultural economic units, to control for pre-SEZ Act manufacturing and service employment. To proxy for credit frictions, I compute the number of bank branches at the municipality level using Garg and Gupta (2020). Finally, I collect data on local land concentration, as this directly relates to both cost parameters in the land acquisition cost function. I computed the subdistrict-, district- and state-level land Theil-T index based on detailed plot distribution information from the 2000 Agricultural Census.[18] A subdistrict is the third-level administrative boundary in India; India consists of more than 6,000 subdistricts. Table 1 summarizes the variables in the proposal dataset; the characteristics of SEZ locations can be found in Table A.1 in Section A.A.1 in the Appendix. The average SEZ is 308.7 hectares, or approximately 1.8 by 1.8 kilometers, and 12 percent of SEZs are proposed by a public developer. In terms of the sectoral composition of the proposed SEZs, around two-thirds of SEZs are in services, such as IT, Research and Development, or warehousing. Manufacturing SEZs, including those for chemicals, pharmaceuticals and apparel, amount to 30 percent; around 2 percent of SEZs are in utilities, concerned with power generation or oil and gas.

**Table 1:** *Characteristics of SEZ proposals*

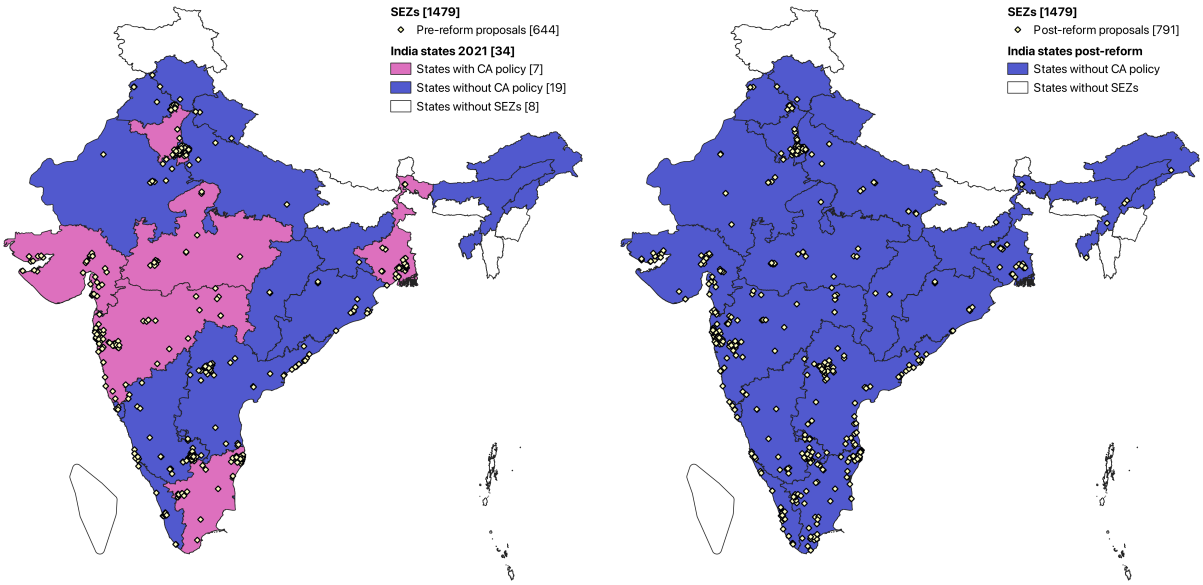
| | N | Mean | Median | SD |
|---|---|---|---|---|
| SEZ size (ha.) | 1441 | 308.7 | 37.5 | 1226.5 |
| Public | 1445 | 0.12 | 0 | 0.32 |
| Manufacturing | 1444 | 0.30 | 0 | 0.46 |
| Services | 1444 | 0.68 | 1 | 0.47 |
| Utilities | 1444 | 0.021 | 0 | 0.14 |

The unit of observation is a proposal-meeting-subdistrict combination. SEZ characteristics are obtained from the proposal dataset.

Finally, I obtain information on state propensity to engage in compulsory acquisition by parsing through all state-specific SEZ acts, rules and policies, as there is no publicly available information on actual land acquisitions by state governments, especially for private SEZs. Concretely, I classify

boundaries. If a village does not experience any boundary changes, the shrid, which I will interchangably call municipality or village, is at the village-level; otherwise it includes multiple villages.

[18] The Theil index is a measure of inequality. Its advantage over the more standard Gini coefficient is that it is sensitive to the number of landholders, thereby allowing me to capture land concentration as well as inequality. Results based on the Gini coefficient are quantitatively similar and available upon request. Section A.A.2 in the Appendix provides more information on this measure, including Figure A.3 that shows the country-wide variation of this index.




**(a)** *SEZ proposals before the reform* **(b)** *SEZ proposals after the reform*

**Figure 3:** *Location of proposed SEZs and states' CA policy*

a state as treated if they have an official pre-reform policy or act that specifies that the government can use the Land Acquisition Act (1894) or any other expropriation strategy to provide the land to SEZ developers. For example, clause 7.1 of the Haryana Special Economic Zone Act 2005 states: "The Government may transfer land owned, acquired or controlled by it to the Developer as per provisions of the Land Acquisition Act, 1894 (1 of 1894), and the rules made thereunder and as per State Government policy."[19] Figure 3 shows the spatial distribution of SEZ proposals across states by their commitment to eminent domain, before and after the reform.[20] Reassuringly, the location choices by SEZ developers did not seem to drastically change after the protest, as evidenced by the similarity of subfigures (a) and (b).

Based on the conceptual framework, one would, because of the convexity in the cost function, expect relatively larger SEZs and relatively more SEZs in large-scale industries in states with a compulsory acquisition policy. Table 2 shows how SEZs discussed before the protest differ between states with and states without a compulsory acquisition (CA) policy. First of all, SEZs in CA states are on average 64 percent larger than their counterparts in other states. All other SEZ characteristics in the first panel show no significant differences between treated and control states; the exception here is that states that engaged in compulsory acquisition see significantly

[19] Table A.5 in the Appendix shows all relevant clauses that inform the treatment assignment.

[20] The discrepancy in the amount of SEZs and the amount of SEZ proposals in Figure 3 reflects the five proposed SEZs (all by Rajasthan Explosives & Chemicals Ltd.) for which I could not determine a specific location.

**Table 2:** *Characteristics of pre-reform proposals by state's compulsory acquisition policy*

| | No CA policy | | CA policy | | Difference | |
|---|---|---|---|---|---|---|
| **SEZ Characteristics** | | | | | | |
| Log size (ha.) | 3.975 | (1.660) | 4.474 | (1.830) | 0.500 | (0.306) |
| Public developer (share) | 0.111 | (0.315) | 0.143 | (0.351) | 0.032 | (0.073) |
| Manufacturing SEZ (share) | 0.326 | (0.469) | 0.368 | (0.483) | 0.043 | (0.088) |
| Services SEZ (share) | 0.671 | (0.471) | 0.596 | (0.491) | -0.075 | (0.094) |
| Utilities SEZ (share) | 0.003 | (0.057) | 0.035 | (0.184) | 0.032* | (0.017) |
| **Location Characteristics** | | | | | | |
| Population (2001) | 13.066 | (1.354) | 13.522 | (1.250) | 0.456 | (0.466) |
| Labor force (2001) | 12.083 | (1.305) | 12.527 | (1.199) | 0.443 | (0.476) |
| Agricultural emp. (2001) | 9.982 | (0.949) | 10.314 | (0.763) | 0.332 | (0.358) |
| Manufacturing emp. (2005) | 9.652 | (1.853) | 10.084 | (1.684) | 0.432 | (0.617) |
| Services emp. (2005) | 10.663 | (1.712) | 11.056 | (1.620) | 0.393 | (0.526) |
| Distance to airport (km) | 2.897 | (0.926) | 3.052 | (0.826) | 0.155 | (0.174) |
| Distance to port (km) | 5.240 | (1.139) | 4.475 | (1.364) | -0.765* | (0.414) |
| Distance to power plant (km) | 2.458 | (0.793) | 2.618 | (0.804) | 0.160 | (0.136) |
| Distance to city (km) | 3.219 | (0.959) | 3.114 | (0.909) | -0.106 | (0.151) |
| Distance to highway (km) | 0.791 | (0.578) | 0.668 | (0.488) | -0.123 | (0.126) |
| Distance to railway (km) | 1.504 | (0.786) | 1.563 | (0.889) | 0.060 | (0.165) |
| At least one bank (2005) | 0.330 | (0.471) | 0.418 | (0.494) | 0.088 | (0.089) |
| Number of banks (2005) | 2.335 | (1.382) | 3.508 | (1.546) | 1.174** | (0.454) |
| Land concentration (Theil) | 0.649 | (0.394) | 0.537 | (0.190) | -0.112 | (0.121) |
| Observations | 306 | | 342 | | 648 | |

The unit of observation is a proposal-meeting-subdistrict combination; the sample is restricted to proposals discussed before the protest. SEZ characteristics are obtained from the proposal dataset. All location characteristics are aggregated up to the subdistrict level and are presented as they enter the regression – in logs. Data on agricultural employment, labor force, subdistrict size and population are from the 2001 PCA; manufacturing and services employment from the 2005 EC, bank data is retrieved from the RBI dataset and the Theil index on land concentration is computed using the 2000 Agricultural Census. Distances are based on comparing the centroid of each georeferenced SEZ to all places of interest as entered in OpenStreetMap. Standard errors are clustered at the state level.

more proposals for oil and gas and power generation SEZs. However, these types of SEZs are a minority, with only 30 proposals in the whole sample. Turning to location characteristics of SEZs discussed before the protest, we see that they are balanced across the two treatment groups except for distance to the port and presence of financial banks. All in all, this shows the need to control for

certain location characteristics that would otherwise threaten our identification, but is reassuring because the main outcomes – manufacturing and services – are not significantly different across treatment and control states. Even at a more disaggregate industry level, as in Table A.3 in Section A of the Appendix, I confirm that sectoral composition is generally not significantly different across states with and without compulsory acquisition.

### *II. Village-level employment data*

The fourth hypothesis predicts that post-reform SEZs generate more employment. I investigate this using two rounds of the Economic Census (2005-2013), which as mentioned before contains the universe of firms in the nonagricultural sector.I restrict my attention to the SEZs that became operational before the last available wave of the EC in 2013. As the exact dates of operation are not publicly available, I obtained an official list of all operational SEZs at 12 October 2012 that was published by the Ministry of Commerce and Industry.[21] I crossreference this list with an updated version published on 18 March 2013 to confirm that no other SEZs became operational in the last three months of 2012. Excluding the 19 SEZs that were established before the SEZ Act, I am left with 139 operational SEZs. These are displayed in Figure 4 separately for pre- and post-reform SEZs, subdivided into the three broad sectors of manufacturing, services and utilities.

To identify the local labor market effects of SEZs before and after the reform, I first need to classify which municipalities are sufficiently close to the SEZ to experience any spillovers. In constructing this dataset, I adopt a similar GIS strategy as Gallé et al. (2024) and Görg and Mulyukova (2022). In the absence of data on exact SEZ boundaries, I assume each SEZ is a circle and draw a buffer around the precisely georeferenced location. After I checked that the villages covered by the buffer were indeed SEZ-hosting municipalities, I drew 10 distance bins of five kilometers each around each SEZ and restricted the municipality sample to those that were within these bins. To ensure that I can pin down the effect of *only* one nearby SEZ, I exclude SEZs that share a municipality with another SEZ.[22] The final sample contains 43,673 distinct municipalities, which I classify into distance bins based on their nearest SEZ. I merge these villages with three rounds of the Economic Census (1998, 2005, 2013), the Population Census Abstract (2001) and the land concentration data. Table 3 reports the mean and standard deviation of several location characteristics for municipalities at different distance bins to their nearest SEZ, with the last column showing the difference between the preferred control, which is 20-25 kilometers from an SEZ, and the directly treated villages. The balancing table clearly shows that SEZ-hosting villages are closer

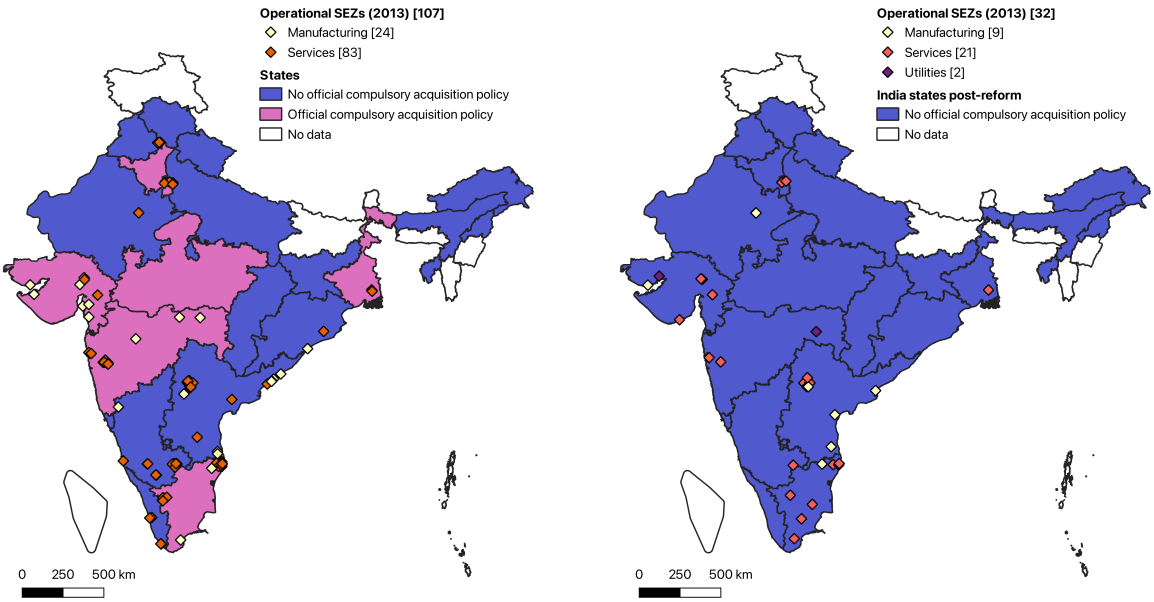
[21] This was retrieved from http://www.sezindia.nic.in.

[22] There are 70 municipalities that host multiple SEZs; these are mainly large (both economically and in terms of actual size) metropolitan areas, such as the urban center of Bangalore which occupies an area of 741 kilometers.

**Table 3:** *Pre-reform location characteristics*

| Variable | (1) 0km | (2) 0-5km | (3) 5-10km | (4) 10-15km | (5) 15-20km | (6) 20-25km | (7) 25-30km | (8) 30-35km | (9) 35-40km | (10) 40-45km | (11) 45-50km | (12) (6)-(1) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Log population | 1.610 | 1.579 | 1.409 | 1.338 | 1.284 | 1.285 | 1.212 | 1.197 | 1.213 | 1.220 | 1.172 | 0.325 |
| density | (2.137) | (1.802) | (1.434) | (1.397) | (1.273) | (1.257) | (1.163) | (1.227) | (1.192) | (1.185) | (1.181) | (0.221) |
| Log agricultural | 5.661 | 5.343 | 5.290 | 5.394 | 5.477 | 5.538 | 5.482 | 5.421 | 5.435 | 5.360 | 5.397 | 0.123 |
| employment (2001) | (1.305) | (1.279) | (1.332) | (1.315) | (1.264) | (1.239) | (1.257) | (1.315) | (1.289) | (1.378) | (1.382) | (0.161) |
| Log nonagricultural | 4.934 | 4.481 | 4.206 | 4.117 | 4.039 | 4.077 | 3.973 | 3.931 | 3.923 | 3.832 | 3.794 | 0.858*** |
| employment | (2.099) | (1.817) | (1.591) | (1.538) | (1.505) | (1.488) | (1.473) | (1.505) | (1.470) | (1.450) | (1.465) | (0.224) |
| Number of banks | 0.278 | 0.106 | 0.065 | 0.068 | 0.061 | 0.054 | 0.046 | 0.054 | 0.051 | 0.044 | 0.045 | 0.223*** |
| (2005) | (0.920) | (0.428) | (0.319) | (0.281) | (0.277) | (0.232) | (0.202) | (0.239) | (0.231) | (0.211) | (0.201) | (0.073) |
| Land concentration | 0.498 | 0.490 | 0.486 | 0.485 | 0.469 | 0.456 | 0.461 | 0.469 | 0.451 | 0.442 | 0.438 | 0.042* |
| (Theil index) | (0.171) | (0.156) | (0.158) | (0.168) | (0.162) | (0.152) | (0.170) | (0.182) | (0.178) | (0.166) | (0.186) | (0.025) |
| Log distance to | 3.082 | 3.165 | 3.276 | 3.393 | 3.527 | 3.622 | 3.698 | 3.803 | 3.894 | 3.967 | 4.027 | -0.540*** |
| airport (km) | (0.838) | (0.788) | (0.704) | (0.643) | (0.572) | (0.533) | (0.508) | (0.467) | (0.443) | (0.432) | (0.408) | (0.128) |
| Log distance to | 4.546 | 4.653 | 4.683 | 4.718 | 4.761 | 4.858 | 4.952 | 4.969 | 4.984 | 4.959 | 5.026 | -0.312* |
| port (km) | (1.312) | (1.245) | (1.176) | (1.127) | (1.122) | (1.034) | (0.979) | (0.944) | (0.936) | (0.919) | (0.901) | (0.183) |
| Log distance to | 3.595 | 3.553 | 3.607 | 3.724 | 3.828 | 3.872 | 3.923 | 3.983 | 4.083 | 4.143 | 4.187 | -0.278** |
| city (>500K,km) | (0.907) | (0.839) | (0.792) | (0.754) | (0.721) | (0.678) | (0.654) | (0.604) | (0.558) | (0.523) | (0.513) | (0.121) |
| Log distance to | 0.852 | 1.012 | 1.126 | 1.163 | 1.190 | 1.206 | 1.232 | 1.268 | 1.274 | 1.277 | 1.339 | -0.353*** |
| highway (km) | (0.568) | (0.612) | (0.633) | (0.640) | (0.648) | (0.649) | (0.644) | (0.658) | (0.674) | (0.694) | (0.688) | (0.071) |
| Log distance to | 1.598 | 1.627 | 1.817 | 1.913 | 1.978 | 2.057 | 2.157 | 2.248 | 2.350 | 2.370 | 2.465 | -0.459*** |
| railway (km) | (0.788) | (0.776) | (0.754) | (0.773) | (0.826) | (0.869) | (0.866) | (0.877) | (0.878) | (0.907) | (0.905) | (0.126) |
| Log distance to | 2.362 | 2.596 | 2.718 | 2.810 | 2.869 | 2.932 | 3.049 | 3.129 | 3.202 | 3.267 | 3.288 | -0.570*** |
| power plant (km) | (1.039) | (0.813) | (0.789) | (0.784) | (0.785) | (0.795) | (0.758) | (0.735) | (0.733) | (0.741) | (0.725) | (0.168) |
| Observations | 157 | 1,251 | 2,378 | 3,096 | 3,802 | 4,388 | 5,128 | 5,517 | 5,802 | 6,169 | 5,985 | 43,673 |

This table reports the mean and standard deviation of several location characteristics for municipalities at different distance bins to their nearest SEZ, excluding SEZs in villages with multiple SEZs. Column (12) shows the difference-in-means between villages 20-25 kilometers from an SEZ (column (6)) and SEZ-hosting villages (column (1)). Distances are based on comparing the centroid of each village to all places of interest as entered in OpenStreetMap, population, area and agricultural employment are retrieved from the 2001 Primary Census Abstract, nonagricultural employment is taken from the 2005 Economic Census and the Theil index is computed using the 2000 Agricultural Census. Standard errors are clustered at the district level.




**(a)** *Proposed before the reform* **(b)** *Proposed after the reform*

**Figure 4:** *Operational SEZs up to 2013*

to virtually all amenities listed, and are characterised by higher nonagricultural employment and access to banks.

## V. IMPACT ON SEZ DEVELOPMENT

### *I. Empirical strategy*

**H1, H2: Sector choice and size** The first hypothesis predicts the reform causes a reallocation from manufacturing to services in terms of SEZ development, and more to less land-intensive industries more broadly. Because only two percent of all proposals are for Utilities sectors, I exclude these proposals from my sample, such that services sectors are the reference category.[23] I estimate variations of the following equation to understand the reform's impact on SEZ entry decisions:

$$Y_{it} = \beta Post_{it} \cdot CA_{s(i)} + \sum_{n \in N} t \cdot X_{sd(i),2005} + \alpha_{r(i)} + \alpha_t + \epsilon_{it}, \tag{2}$$

where $Y_{it}$ refers to the sector choice or the size in SEZ proposal $i$ discussed at a meeting at date $t$. $Post_{it}$ is a dummy designating whether the proposal whether the proposal $i$ was discussed before or after the protest and $CA_{s(i)}$ equals 1 if the proposed SEZ is located in a state $s$ which

[23] Including Utilities proposals does not change this coefficient or its associated standard errors by much.

ex ante committed to providing land to SEZ developers. I also always include meeting fixed effects $\alpha_t$ and economic region fixed effects $\alpha_{r(i)}$.[24] The latter refer to India's subdivision into 77 regions that are economically similar. This allows me to control for any state-level regulation beyond compulsory acquisition policy that might affect the sectoral choice of SEZs locating in that state, as well as local commonalities at a more disaggregate level. To ensure fair comparisons, I also control for all location characteristics described earlier by incorporating a linear trend at the subdistrict level across meeting dates $\sum_{n \in N} t \cdot X_{sd(i),2005}$.[25] Finally, $\epsilon_{it}$ captures any remaining unobserved region-year specific variables that also affect in which sectors SEZs are proposed. Since treatment, or at least the treatment intensity, is assigned at the state level, that is how I cluster my standard errors. The coefficient of interest is $\beta$: together with its estimated standard error, it shows whether states that engaged in compulsory acquisition policy observe significantly different types of proposals after the protest. For example, if the outcome variable is whether a proposal is for a manufacturing SEZ, I would expect $\beta < 0$, or that a relatively large increase in land acquisition costs is associated with a larger reduction in the more land-intensive manufacturing proposals. If the outcome variable is a dummy variable, I use a Linear Probability Model.

A natural follow-up question is how this *relative* change in sectoral composition after the reform translates into *absolute* changes in participation of manufacturing-oriented developers into the SEZ policy. As mentioned before, this analysis is complicated because of the mechanical decrease in the number of proposals after the reform regardless of compulsory acquisition mandate, as shown in Figure A.5. Still, I analyse how the number of proposals in every state in each quarter evolves differently for treated and control states after the reform, estimating Equation 2 with state-level controls and state and year fixed effects. This strategy allows me to difference out the mechanical decrease in the number of proposals after the reform – since proposals are discussed on a first-come-first serve basis, clearing the backlog of proposals should not significantly differ between states with and without compulsory acquisition. The results, shown in Supplemental Appendix Table A.7, show no evidence that compulsory acquisition states saw fewer proposals in general after the reform, but find a borderline (10.9%) significant decrease in the number of manufacturing proposals. While a lack of power does not allow me to make a stronger claim here, it is consistent with a reallocation from manufacturing to services SEZs.

[24] The results are robust to including subdistrict or state fixed effects instead of region fixed effects and including quarterly fixed effects instead of meeting fixed effects.

[25] A more natural approach would be to interact the pre-reform controls with a full set of year dummies. I do not have sufficient degrees of freedom to estimate standard errors, although the coefficient is of similar size. I also test how robust the results are to other strategies of including control variables in Figure A.12 in the Supplemental Appendix.

**H3: Stronger effect for areas with lower land concentration** To test the third hypothesis, I analyse whether the reform's effect is stronger in areas with lower land concentration.[26] I set up a triple difference-in-differences, interacting the treatment from Equation 2 with a subdistrict's land concentration prior to the reform:

$$\begin{aligned} Y_{it} =& \beta_1 Post_{it} \cdot CA_{s(i)} \cdot T_{sd(i)} + \beta_2 Post_{it} \cdot T_{sd(i)} + \beta_3 Post_{it} \cdot CA_{s(i)} + \beta_5 CA_{s(i)} \cdot T_{sd(i)} \\ &+ \beta_6 T_{sd(i)} + \sum_{n \in N} t \cdot \Gamma_{sd(i),2005} + \alpha_{s(i)} + \alpha_t + \epsilon_{it}, \end{aligned} \tag{3}$$

where $T_{sd(i)}$ is the subdistrict-level Theil index and $\alpha_{s(i)}$ are state fixed effects. I include state rather than region fixed effects as to leverage variation in land ownership patterns across regions; using region fixed effects instead does not change the size of the coefficient of interest but increases the standard errors. Also, note that the controls $\Gamma_{sd(i),2005}$ now no longer include land concentration as in Equation 2.

### *II. Identification concerns and robustness*

First, the unexpected nature of the protest and the subsequent reform generate experimental variation in the treatment variable and thereby helps subside concerns of reverse causality. The main threat to identification is that $\epsilon_{it}$ contains omitted variables that are correlated with the decision of a state to allow for compulsory acquisition of land for SEZs. Specifically, if a state's initial conditions both affect its likelihood to adopt compulsory acquisition policy and the change in *sectoral composition* of SEZs proposed to locate in that state before and after the reform, I cannot disentangle this from the impact of an increase in land acquisition costs. An obvious potential confounder is economic activity: assume for example that states whose manufacturing activity is on a downward trajectory before the reform are more likely to adopt compulsory acquisition policy. Then, as I assign a larger treatment exposure to states with such policies, I obtain a negative correlation between the reform and the incidence of manufacturing proposals. However, this coefficient is biased downward as these states were already becoming less attractive for manufacturing SEZ developers, and I cannot differentiate between the increase in land acquisition costs and the decrease in the expected profits from a manufacturing SEZ.[27] More broadly, states

[26] For this analysis, it is very important that land concentration does not also influence manufacturing employment directly. Reassuringly, the pre-reform correlation between the Theil index and nonagricultural employment is -0.0116, with a p-value of 0.6786.

[27] Table A.6 in the Appendix shows how the propensity of a state to introduce compulsory acquisition policy is indeed related to economic characteristics, with states with higher access to finance and larger labor force being more likely to adopt this policy. This reaffirms the need to both include controls at the state – or more restrictively, economic region – level, as well as to capture local economic characteristics that reflect the potentially differing preferences of manufacturing or services SEZ developers.

that are industrially more backward or otherwise characterised as less desirable for (manufacturing) SEZs might be more inclined to facilitate compulsory acquisition, biasing the coefficient after the reform downward.

To enable a fair comparison, and account for potential differences in outcome trends, I follow McCaig (2011) and control for time trends for pre-SEZ local characteristics in the broad industries of pre-SEZ agriculture, manufacturing and services employment. Beyond industry employment, I control for several location characteristics that plausibly affect expected SEZ profitability: I include linear trends for pre-SEZ subdistrict-level population, labor force, the number of banks, the size of the subdistrict and distances to the nearest port, airport, highway, railway, power plant and large city (Gallé et al., 2024). I also control for subdistrict-level land concentration, as that is directly related to land acquisition costs. As mentioned before, controlling for these trends is highly important as adoption of CA policy seems non-random.

The fact that I have grouped data, and – especially in the pre-reform period – bunching due to the large amount of proposals submitted before the protest makes it difficult to verify parallel trends. I group discussed proposals by quarter to partially alleviate the variation in number of proposals by meeting over time and estimate a variation of Equation 2, in which I interact the compulsory acquisition dummy with quarter dummies and include quarter fixed effects. The coefficients in Figure A.6 thus reflect the difference in the share of manufacturing proposals for states with and without compulsory acquisition mandates in each quarter, relative to the baseline meeting just before the protest. This exercise asks a lot of the data, and while it suggests that the sectoral composition changes differently over time for states with compulsory acquisition mandates compared to those without it is not very conclusive. In Supplemental Appendix Figure **??** I show, in a less demanding exercise, that there are no significant pre-trends in general for

While the pretrends look good, the fact that I have grouped data, and – especially in the pre-reform period – a lot of bunching due to the large amount of proposals submitted before the protest makes it difficult to verify parallel trends. Instead, I conduct two placebo tests, each around halfway through the pre-reform period; the results of which are in Supplemental Appendix Table A.11. In both cases, also without controlling for local characteristics or trends, I obtain a small insignificant effect on the share of manufacturing. I further exclude the 164 proposals from the first meeting, which yields a slightly smaller but significant coefficient, and the first and the second meeting or the first 221 proposals, resulting in qualitatively similar results, although the coefficient is smaller and, for the strictest specification, not significant any more.

There are two other factors that strengthen the identification strategy. First, note that identifying the effect of the reform relies on a differential response between manufacturing and services: this means that it is properly identified insofar as treated states are not also subject to concurrent

manufacturing (or services) shocks. And second, since the land concentration analysis in Equation 3 relies on *within-state* variation, unobserved state-level trends are not a threat to identification for those results.

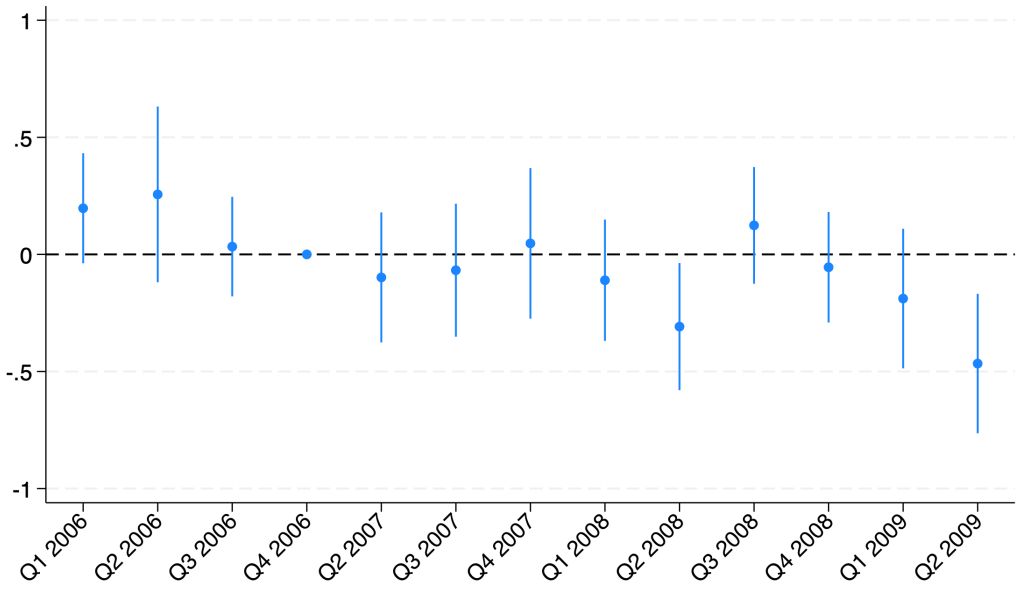



**Figure 5:** *Trends in SEZ sectoral composition across states depending on CA policy*

There are several other potential threats to identification, which I now discuss in turn. One concern is that the BoA treats manufacturing proposals, and especially those in compulsory acquisition states, differently after the reform. Because its members are chosen by virtue of the office they hold, it is unlikely that a change in board composition drives the results. Moreover, as the Board contains a variety of members from different political parties – and even some without official political affiliation – and all decisions need to be reached with general consensus, it is unlikely that members collectively start favoring certain states after the reform. In Table A.8 in the Appendix, where I directly estimate the impact of the reform on the probability of any proposal reaching the next stage, I show that this passthrough is not significantly different after the reform for both the full sample and for only manufacturing SEZs. Alternatively, the effect I find might not be driven by the reform, but instead an increase in uncertainty for developers now that protests from landowners have had major consequences. This is implicitly addressed with the land concentration analysis, as that only leverages within-state variation, but it is informative to address this concern more directly. In Supplemental Appendix Figure A.10, I verify that the effect is not driven by states where protests had especially salient impacts for developers or by economic

prowess in non-compulsory acquisition states.[28] Finally, because the Nandigram SEZ protest also had a strong environmental angle, I show the effect cannot be explained by developers moving away from polluting industries to prevent backlash (Levien, 2012).

Another concern is measurement error in the treatment variable. Because I cannot observe directly to what degree State Governments engage in eminent domain, I have to proxy with officially declared intentions of compulsory acquisition, complemented with anecdotal evidence on eminent domain use for SEZs. There are two main concerns: (1) a state might, despite the policy, only infrequently use compulsory acquisition for SEZs and (2) a state without an official policy might engage in compulsory acquisition regardless. Because I cannot directly rule out either concern, I instead focus on testing for their presence indirectly. First, I analyze how robust the result is to excluding each compulsory acquisition state in Supplemental Appendix Figure A.11. I find the same negative effect, albeit insignificant at the 5% level for excluding Gujarat and Maharashtra. Note that for Maharashtra, this is at least partly driven by a large widening of the standard errors, as almost 20 percent of the sample is removed. Excluding Gujarat mainly shows that the effect of the reform in that state was substantial, which is unsurprising given its economic performance and, according to anecdotal evidence, the particularly strong involvement of the State Government in compulsory acquisition.[29] To assuage the second concern, I restrict the control group to the two states that explicitly prohibited land provision for private SEZs: Uttar Pradesh and Kerala.[30] In Figure A.10, I find that this results in a coefficient that is larger in absolute size than in the baseline; note however that this is only suggestive due to a lack of power.

Another worry is that the effect I estimate is not because of the 2007 reform, but instead is a reflection of later legislative efforts to reform state land acquisition, especially the Right to Fair Compensation and Transparency in Land Acquisition, Rehabilitation and Resettlement Act in 2013. To rule out this alternative explanation, I rerun Equation 2, constructing different windows around the protest. Specifically, I first only include post-reform proposals one year after the reform, and expand with an additional year until I obtain the full sample again. Supplemental Appendix

[28] I first exclude West Bengal, a state that saw especially many protests against compulsory acquisition and eventually abolished land provision for industrialisation altogether (Sarkar, 2007; Patra, 2019). I also exclude Goa, where protests against land grabs pushed the Goa State Government to eventually abolish all SEZ projects in 2009. Finally, I exclude Andhra Pradesh, which has no compulsory acquisition mandate but was the third most popular state for SEZ developers.

[29] Because these compulsory acquisition mandates are adopted precisely to attract private investment, one would expect the reform's effect to be largest for private SEZ developers (Levien, 2012). In Supplemental Appendix Figure A.13, I show that the effect of the reform on manufacturing is indeed five times larger for private SEZs than public SEZs, albeit insignificantly so – note that only around 12% of the SEZs in my sample are proposed by a public developer.

[30] Uttar Pradesh published an amendment to their SEZ policy on July 17, 2007, stating that the government will not engage in compulsory acquisition for private SEZ developers. Kerala confirmed their stance against land provision for private SEZ developers in their 2008 SEZ policy.

Figure A.9 – consistent with Figure A.6 shows that the reform's effect on sector choice materialises immediately after the reform and is persistent until the end of the sample in 2022. This suggests that the estimated effect is indeed driven by the reform in 2007 rather than more recent land legislation.

A final concern is SUTVA violations, which in this context would imply that entry decisions are affected by previous entrants. More specifically, I might find a decrease in manufacturing entry in treated states if those states faced high pre-reform entry and thus are relatively more constrained in terms of resources such as land for new SEZs. On the other hand, manufacturing developers might find these states especially attractive if earlier entrants have formed (or added to existing) industrial belts or clusters in a certain state. Indeed, comparing the left-hand and right-hand figure in Figure 3 provides suggestive evidence that location choice of SEZ developers seems relatively similar before and after the reform. To understand whether there is a certain path dependence in SEZ entry, I rerun the first estimating equation and control for the number of (manufacturing) SEZs that are notified at a state-year level.[31] In Supplemental Appendix Table A.12, I show that the estimated effect of the on manufacturing is similar, and even slightly larger, than in the baseline results. Second, I show in Figure A.14 that the estimated effect is not significantly different in locations close to the port, which are both especially desirable for manufacturing and thus export-oriented SEZs and especially limited in terms of land availability.

### *III. Results*

In this section, I provide evidence that the reform affected entry of SEZ developers as well as characteristics of their proposals.

**H1: Sector choice** The first hypothesis predicts a reallocation from manufacturing to services. Table 4 displays the results of estimating Equation 2 with the dependent variable indicating whether the proposed SEZ is in manufacturing. The first two columns show the effect of the reform on the probability that a proposal discussed by the BoA is for a manufacturing SEZ, where column 1 includes region and meeting fixed effects without any controls, and column 2 contains the most stringent specification controlling for linear location trends.[32] In column 2, the reduction in the share of manufacturing proposals is 17.2 percentage points, or an almost 50 percent decrease compared to the baseline share of 35 percent.

[31] Notification implies that developers are allowed to start constructing in the zone, and would thus be a reasonable proxy for either potential competition or collaboration.

[32] Figure A.12 in Section B of the Appendix shows that the result is robust to alternative strategies to reduce omitted variable bias, including controls interacted with a post-reform dummy.

**Table 4:** *Relatively fewer manufacturing SEZs are proposed after the reform*

| | All proposals | | Formally approved | | Notified | | Operational | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| After protest | -0.190** | -0.172** | -0.257** | -0.213** | -0.215** | -0.169* | -0.293* | -0.245* |
| × State CA | (0.0713) | (0.0611) | (0.0916) | (0.0906) | (0.0736) | (0.0859) | (0.139) | (0.130) |
| Controls | No | No | No | No | No | No | No | No |
| Location trends | No | Yes | No | Yes | No | Yes | No | Yes |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Meeting FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1210 | 1210 | 600 | 600 | 384 | 384 | 183 | 183 |
| R-squared | 0.297 | 0.354 | 0.375 | 0.408 | 0.453 | 0.485 | 0.606 | 0.646 |

The dependent variable is a dummy that equals one if the proposed SEZ is in *Manufacturing*. *PostProtest* is a dummy that takes the value 1 if the meeting in which the proposal is discussed happens after 17 March 2007. Each observation is a SEZ-meeting-subdistrict combination, excluding all Utilities and resubmitted proposals. After the first column, the sample is restricted to include only proposals for SEZs that were formally approved, notified and operational respectively. The controls include measures of subdistrict-level log population density, labor force and agricultural employment from the 2001 PCA, and data on log nonagricultural employment from the 2005 EC. Log distances between the SEZ and the nearest airport, port, power plant, highway, railway and large city (>500K inhabitants) are also included. Finally, the controls include the Theil index on land concentration computed using the 2000 Agricultural Census and the log number of banks in the subdistrict from Asher et al. (2021). Standard errors, clustered at the state level, are in parentheses.
$^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

This raises the question of how this affects the sectoral composition of proposals at later development stages. As mentioned before, the probability of a (manufacturing) proposal moving to the next stage of SEZ development is not significantly affected by the shock. This means that the effects that I find in the remaining columns, describing the sectoral composition of SEZs moving beyond the initial proposal stage, are unlikely to be driven by the BoA being stricter in their judgment of manufacturing proposals. To ensure proper classification of SEZs before and after the reform, I drop all resubmitted proposals.[33] First, columns 3 and 4 display the result of estimating Equation 2 without and with controls respectively for those proposals that are formally approved. Column 4 shows there is a persistent decrease in the share of manufacturing proposals by 21.3 percentage points. Per columns 5 and 6, the share of manufacturing proposals that are notified and thus approved by the Central Government, decreases by 16.9 percentage points. Finally, columns 7 and 8 show that this shift in sectoral composition materializes also for operational SEZs. This

[33] The rationale for this is that resubmitting a pre-reform proposal after the reform is arguably also treated, since the developer could have decided to not resubmit the proposal. Including all proposals reduces the effect size slightly but otherwise does not change the results.

result is robust to a logit specification: Table A.13 in the Supplemental Appendix shows – in the preferred specification – that the odds of a proposal being for a manufacturing SEZ were 53% lower after the reform.

If the reform represents an increase in land acquisition costs, this would affect the more land-intensive industries regardless of the broad sector being manufacturing or services. While some services industries are persistently small-scale, such as IT and related services, other services industries, such as SEZs for supporting transport activities built around trade hubs, do not differ much in size from relatively smaller-scale manufacturing industries, such as textiles.[34] Table A.10 in the Supplemental Appendix documents a more general reallocation as SEZs in smaller-scale industries are relatively more likely to be proposed after the reform.

**Table 5:** *Effect on log SEZ size suggestive evidence for reallocation across and within industries*

| | All proposals | | Formal approval | | Notification | | Operational | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| After protest | -0.170 | 0.113 | -0.326 | 0.102 | -0.284 | 0.102 | 0.648* | 0.661* |
| × State CA | (0.200) | (0.124) | (0.208) | (0.139) | (0.274) | (0.153) | (0.343) | (0.332) |
| Location trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | No | Yes | No | Yes | No | Yes | No | Yes |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Meeting FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1217 | 1216 | 608 | 607 | 390 | 388 | 184 | 180 |
| R-squared | 0.484 | 0.812 | 0.546 | 0.764 | 0.603 | 0.799 | 0.779 | 0.876 |

The dependent variable is the approved log SEZ size in hectares. Each observation is a SEZ-meeting-subdistrict combination, excluding all Utilities proposals. Across model groups, the sample is restricted to include all proposals, then only proposals for SEZs that were formally approved, notified and operational respectively. Each second column is further restricted to only include manufacturing proposals. See the notes under Table 4 for details on the sample specification and the included location trends. Standard errors, clustered at the state level, are in parentheses.
$^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

**H2: Size** Per the second hypothesis we would expect the sector choice for post-reform SEZs to shift from more to less land intensive industries, but that conditional on industrial classification SEZs become larger as the average developer becomes more productive. To verify this, I estimate Equation 2 with the log proposed SEZ size as the outcome variable. The first column in Table 5 shows that on average, proposed SEZs became 17% smaller after the reform in CA states; however,

[34] See also Table A.2 in Section A of the Supplemental Appendix.

once I include industry fixed effects in column 2, the average SEZ size increases by around 11.3%. While both results are insignificant, the sign switch is suggestive and consistent with a reallocation from more to less land intensive industries but an increase in SEZ size conditional on the industry. This pattern is similar, and remains insignificant, for formally approved (column 3 and 4) and notified (column 5 and 6) SEZs. For operational SEZs, we see that the reform's effect on SEZ size is positive and significant at the 10% level, both with and without industry fixed effects. Thus, SEZs that became operational post-reform are significantly larger than their pre-reform counterparts.

**H3: Stronger effects for areas with lower land concentration** The third hypothesis predicts that the increase in land acquisition costs after the reform is more pronounced in areas with lower land concentration. First, I analyse the effect of the reform on the share of manufacturing SEZs. I estimate Equation 3 with a manufacturing dummy as the dependent variable, and compute the marginal effects of the reform across the land concentration distribution in compulsory acquisition states. Figure 6 shows these marginal effects, with the light blue area showing the density plot of local land concentration. We see that the effect of the reform in compulsory acquisition states on the share of manufacturing SEZs is largest in areas with lower land concentration, and significantly negative for areas with a Theil index below 0.5. To put this into perspective: the average (median) land concentration is 0.59 (0.53).

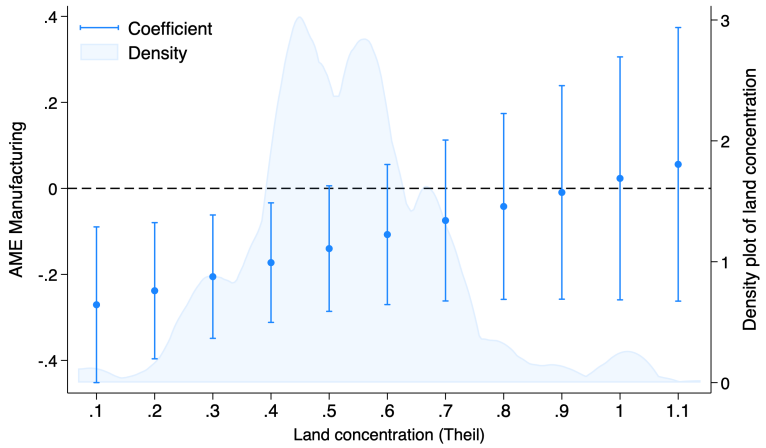


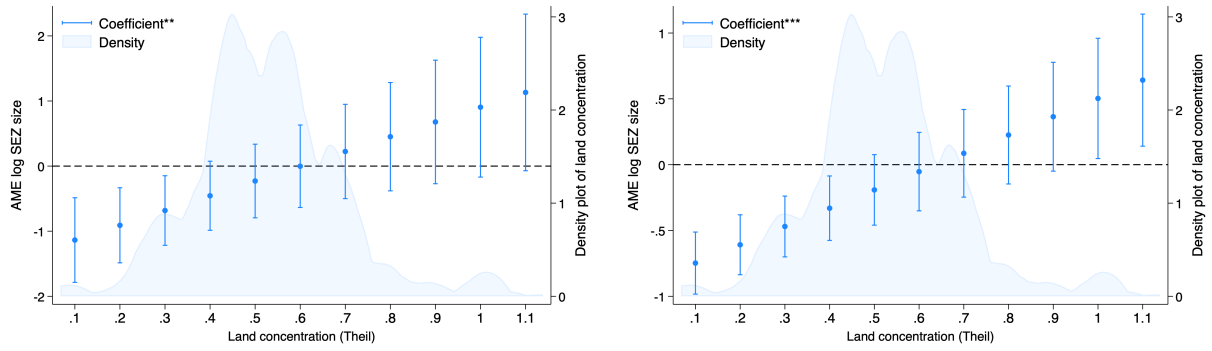

**Figure 6:** *Average marginal effects on share manufacturing SEZs by local land concentration*

Next, I estimate Equation 3 with log SEZ size as outcome variable, both without (a) and with (b) industry fixed effects; the estimated marginal effects of this regression are presented in Figure 7. The results show that the insignificant coefficient on log SEZ size in Table 5 obscures substantial heterogeneity across areas with different land concentration. Proposed SEZs are significantly

smaller on average in subdistricts with a Theil index below 0.4, and almost significantly larger in areas with relatively high land concentration; this becomes significant when I include industry fixed effects. It is in this exercise that we can more directly see that after the reform, relatively fewer large-scale manufacturing SEZs were developed in areas where the land is held by many landowners.

Further evidence is provided in the Supplementary Appendix. Figure A.7 and Figure A.8 show the results of allowing for land concentration to have a nonlinear effect on how the reform changes sector choice and size respectively. Furthermore, Table A.9 estimates Equation 2 with land concentration in the proposed SEZ location as the outcome variable, and shows that after the reform, locations for proposed SEZs are characterised by significantly higher land concentration.

In conclusion, SEZ developers tend to opt out of developing manufacturing or larger-scale SEZs after the reform, but significantly more so in areas with low land concentration. This is consistent with SEZ developers trying to mitigate the bargaining costs now that compulsory acquisition is no longer available to them.




**(a)** *Without industry FE* **(b)** *With industry FE*

**Figure 7:** *Average marginal effects on log SEZ size by local land concentration*

## VI. Impact on local employment

### *I. Empirical strategy*

The fourth hypothesis states that post-reform SEZs generate more employment than their pre-reform counterparts, and that this effect is especially pronounced for manufacturing SEZs. As mentioned before, I use two rounds of the Economic Census (2005-2013) to study changes in nonagricultural employment in villages within 50 kilometers of at most one SEZ; descriptive statistics for the full baseline sample are provided in Section A.A.3 of the Appendix. A standard difference-in-differences strategy, where one compares the employment growth in SEZ-hosting

villages to a control group of similar villages located (just) outside the SEZ between pre- and post-reform SEZs, is likely to lead to biased estimates. While treatment is assigned to the villages in which the SEZ is actually located, the shock to the labor market can affect the neighborhood beyond the SEZ boundaries (Butts, 2023). Indeed, Gallé et al. (2024) estimated that establishing an SEZ generates new employment up to ten kilometers away. A standard difference-in-differences design might also lead to bias if the SEZ boundaries are misspecified, especially if firms in "control villages" are actually located inside the SEZ.

To overcome this concern, I use a spatial difference-in-differences method. The underlying idea is that instead of using the nearest – most similar – control units, one uses control units further away, which are potentially less comparable but are not affected by spillovers. The identifying assumptions here are first that spillovers are local, such that there is a distance after which units are no longer exposed to spillovers, and that conditional on covariates, parallel outcome trends between these true control units and directly treated units hold (Butts, 2023). Then, the total treatment effect, or the sum of the direct effect and the average spillovers on treated units, can be recovered using a standard DiD that excludes all indirectly treated units. I implement this using a concentric ring approach. This divides the control units – both those that are indirectly treated and true controls – into different distance bins based on their *nearest SEZ* and estimates an average spillover effect on all control units for each distance bin.[35]

To implement the spatial difference-in-differences, I follow Gallé et al. (2024) and divide the villages in 50 kilometers around each SEZ into five kilometer distance bins. I also assume that spillovers do not extend beyond 20-25 kilometers from the nearest SEZ; this is thus the reference category. My baseline regression equation is:

$$\mathrm{d}\ln Y_{mt} = \sum_{d=0, d\neq 5}^{10} \beta_d (D_{[d_m=d]} \cdot Post_{mt}) + \gamma'(\mathbb{X}_m \cdot Post_{mt}) + Post_{mt} + \alpha_{mt}^{Ind} + \alpha_d + \alpha_t + \epsilon_{mt}, \quad (4)$$

where $\mathrm{d}\ln Y_{mt}$ is the first-differenced outcome at municipality $m$ which observes entry of an SEZ at time $t$. $D_{[d_m=d]}$ indicates whether municipality $m$ is in distance bin $d$ to its nearest operational SEZ in the post-treatment year; $d_m = 0$ indicates municipalities that host an SEZ, $d_m = 1$ reflects municipalities up to five kilometers from an SEZ, until $d_m = 10$ which contains municipalities 50 kilometers away from an SEZ. This is multiplied with the treatment dummy $Post_{mt}$, which equals one if municipality $m$ is near to an operational SEZ that was proposed after the reform. The model further includes ring and year ($\alpha_d$ and $\alpha_t$), and, to capture any outcome trends that are correlated to baseline characteristics, the controls listed in Table 3 at the municipality level $\mathbb{X}_m$ interacted

[35] This might lead to misleading estimates of the spillover effects if villages are exposed to more than one SEZ within 50 kilometers and spillovers are additive. As I am mostly interested in the direct treatment effect, an alternative empirical specification where I estimate the spillover effects separately is relegated to Section D of the Appendix.

with the treatment dummy. Moreover, to ensure the results are not driven by a reallocation from more to less land intensive industries, I include SEZ industry fixed effects $\alpha_{mt}^{Ind}$. Finally, following Butts (2023), I estimate the standard errors $\epsilon_{mt}$ following Conley (1991); this means that I allow for a common error term component for villages within 25 kilometers of each other. In Supplemental Appendix Figure A.16, I show that the results are robust to clustering at the district level or at the closest SEZ level.

As my theoretical predictions suggest manufacturing industries are impacted the most, I expect that the differential impact of pre- and post-reform SEZs is largest for those sectors. To that end, I estimate a variant of Equation 4:

$$\begin{aligned} \mathrm{d}\ln Y_{mt} = &\sum_{d=0,d\neq 5}^{10} \beta_d (D_{[d_m=d]} \cdot Post_{mt} \cdot Man_{mt}) + \sum_{d=0,d\neq 5}^{10} \delta_d^M (D_{[d_m=d]} \cdot Man_{mt})) + Man_{mt} \\ &+ \delta_1^P (Post_{mt} \cdot Man_{mt}) + \gamma' (\mathbb{X}_m \cdot Post_{mt}) + Post_{mt} + \alpha_{mt}^{Ind} + \alpha_d + \alpha_t + \epsilon_{mt}, \end{aligned} \tag{5}$$

where $Man_{mt}$ is a dummy equal to one if the nearest SEZ to municipality $m$ is in manufacturing.[36] The coefficients of interest, $\beta_d$, indicate how nonagricultural employment changed differently in villages close to a post-reform manufacturing SEZ compared to those near a pre-reform manufacturing SEZ.

The main threat to identification in this analysis is violation of the conditional mean independence assumption. If, after the reform, SEZ developers consistently began locating in places where outcome trends differ from where developers located before, the parallel trends assumption for the treated and control group is violated. This is especially important with the spatial DiD, now that the effective control units are no longer the immediately surrounding villages. The inclusion of village-level fixed effects will absorb any difference in village-level baseline characteristics, but will not account for differing trends in these characteristics. To that end, I interact each of these controls with a treatment dummy to reduce the potential bias coming from time-varying heterogeneity. Moreover, I show in Supplemental Appendix Figure A.15 that the results are similar when estimated without controls, which helps to subside concerns that differences across distance bins cause a bias (Altonji et al., 2005). Finally, I undertake a placebo test, by estimating Equation 4 using pre-reform waves from the Economic Census in 1998 and 2005. This supports the parallel

[36] The goal of this equation is to estimate the differential effect of a manufacturing SEZ in a certain ring to be proposed before or after the reform, and is equivalent to estimating the full set of interactions and then predicting the average marginal effect on employment with respect to the reform. More specifically, $Post_{mt}$ captures the effect of a post-reform non-manufacturing SEZ at 25 kilometers away, $Man_{mt}$ captures the effect of a pre-reform manufacturing SEZ at 25 kilometers away and $Post_{mt} \cdot Man_{mt}$ represents the effect of a post-reform manufacturing SEZ at 25 kilometers away. The corresponding interactions with $D_{[d_m=d]}$ summed over the different distance bins quantify the employment effects of these SEZs at any other distance than 25 kilometers away.

trends assumption when the placebo estimates do not show any significant pattern.

## *II. Results*

### II.i. Main results

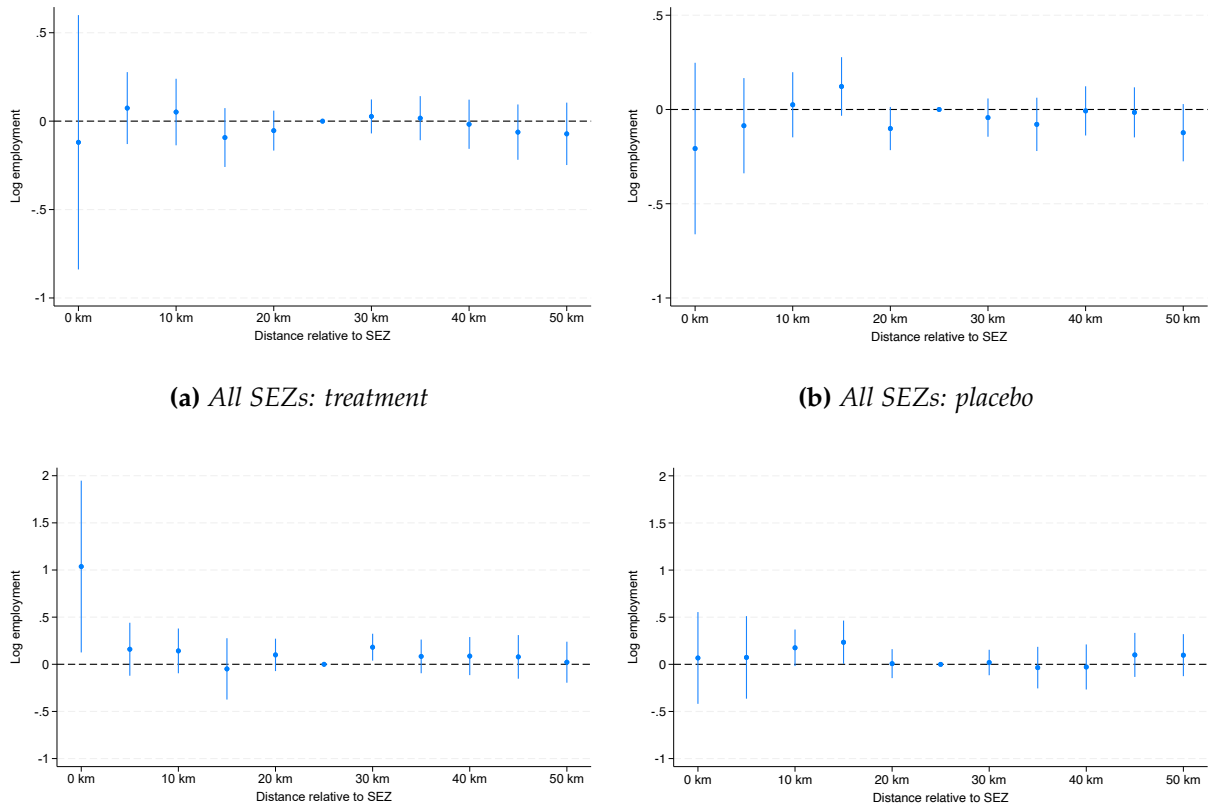
This section describes how the effect of SEZs on local labor market outcomes differs for pre- and post-reform SEZs. It should be noted that I find that, in line with Gallé et al. (2024), I find a positive effect of pre-reform SEZs on local employment up to 10 kilometers from the SEZ. These results will simply test whether post-reform SEZs generate employment beyond these pre-reform SEZs.

To understand the effect of an SEZ opening on SEZ-hosting villages, I estimate Equation 4 with log nonagricultural employment as outcome. The results are displayed in Figure 8, together with their 90% confidence intervals. Panel (a) shows the estimated treatment effect at each five-kilometer distance bin within 50 kilometers of a post-reform SEZ. More specifically, the coefficient at 0 km shows that villages that host a post-reform SEZ saw 11 percentage point lower employment growth between 2005 and 2013 than villages that host a pre-reform SEZ.[37] This effect is however insignificant, as are the estimates at further distances from the SEZ. Thus, I do not find evidence that in the aggregate, post-reform SEZs generated more local employment than pre-reform SEZs. Panel (b) shows the result of a placebo exercise, where I run the same regression with employment growth between 1998 and 2005 as an outcome; I also find no significant effects there.

Since the land reform mainly impacted manufacturing entry, it is instructive to test whether these SEZs led to significantly different local labor market outcomes post-reform. Panel (c) of Figure 8 shows the results from estimating Equation 5. I find a positive and significant effect for SEZ-hosting villages: on average, employment in villages with a post-reform manufacturing SEZ increases by 181% more than villages with a pre-reform manufacturing SEZ in the same industry. The results without industry fixed effects are similar, confirming that the employment increase is mainly driven by within-industry reallocation. To contextualize this estimate, note that the average SEZ-hosting municipality has 4,749 nonagricultural employees in 2005. iI find no significant effects beyond the villages SEZs are located in, expect for – curiously – a positive and significant effect on employment in villages 30 kilometers away from an SEZ. The corresponding placebo test in panel (d), and especially the lack of a significant effect at SEZ-hosting villages, provides suggestive evidence that the parallel trends assumption is satisfied.

One concern is that this increase in employment reflects relocation rather than creation of new jobs, as is often the case with place-based policies in Western countries (Criscuolo et al., 2022). This is however not consistent with these results, as municipalities up to 50 kilometers outside the SEZ

[37] As $\exp^{-.1196} = 0.887$.




**(a)** *All SEZs: treatment*

**(b)** *All SEZs: placebo*

**(c)** *Manufacturing SEZs: treatment*

**(d)** *Manufacturing SEZs: placebo*

**Figure 8:** *Employment effects on nearby villages from operational SEZs*

generally see no significant change in employment. Further, it would be improbable that relocation effects only materialize in villages very far away from the SEZs; given the barriers to migration or travel it is more reasonable to expect relocation from villages relatively close to the SEZ (Topalova, 2010; Munshi and Rosenzweig, 2016). My results do not reflect large-scale migration across the distance bins, and thus suggest that this is new employment.

Finally, I consider the impact of the reform on aggregate SEZ employment. Gallé et al. (2024) estimate that the SEZ policy increased nonagricultural employment by about 1.25 million people in villages within 10 kilometers of an SEZ. To understand whether the contribution of manufacturing SEZs to this figure was in any way influenced by the reform, I build on the results from estimating Equation 5. I focus on employment in SEZ-hosting villages as it is there that I found a significant difference between SEZs before and after the shock. A quick back-of-the-envelope calculation shows that the 24 pre-reform manufacturing SEZs were responsible for approximately 21,924 additional workers in SEZ-hosting municipalities, and that the 9 post-reform manufacturing SEZs

generated 92,526 additional employees in SEZ-hosting municipalities.[38] This together suggests that compulsory acquisition reduces entry into manufacturing, but is associated with both higher local and higher aggregate industrial employment.

## VII. CONCLUSION

Compulsory land acquisition, or eminent domain, has been a long-standing practice for governments across the world and levels of development. To facilitate private investment, governments increasingly rely on compulsory land acquisition policies. There is however no systematic evaluation of how compulsory acquisition impacts the formation and scale of industrial development.

In this paper, I leverage the institutional context of Indian Special Economic Zones to answer this question. I study the effects of an unexpected ban on compulsory acquisition on SEZ development and economic outcomes. To pin down the effect on SEZ entry, I construct a novel dataset containing the universe of SEZ proposals between 2006-2022. I set up a simple conceptual framework that interprets land acquisition costs as an entry barrier. The model then predicts that prohibiting compulsory acquisition would induce sectoral reallocation from more land intensive to less land intensive sectors. Moreover, this effect would be strongest in areas with low land concentration. Finally, the model predicts that the higher entry barrier would disincentive less productive developers to enter, thereby affecting selection.

I find that the compulsory acquisition reform strongly impacted entry of developers of manufacturing SEZs: on average, treated states saw two fewer manufacturing proposals per quarter, and the share of manufacturing in the SEZ sectoral composition decreased by 50%. This decrease is persistent over the development process, as also the share of manufacturing zones that are operational falls by 24.5 percentage points. I further show that the effect is strongest in the areas with the lowest land concentration. The main result is robust to exclusion of specific anti- or pro-SEZ states, excluding polluting industries which are similar to the contested Nandigram SEZ, accounting for potential spatial spillovers and a variety of different model specifications. Finally, placebo tests shifting the reform to an earlier date or excluding the first meeting show no effect, suggesting that the result is not driven by any pre-trends.

Then, I study whether this change in entry also affected employment outcomes within and around SEZs. I find no significant effects for the full sample, but – in accordance with manu-

[38] The coefficient on SEZ-hosting villages pre-reform equals 0.072955, implying that these manufacturing SEZs on average increased nonagricultural employment by 7.6%. The 24 pre-reform manufacturing SEZs are spread over 61 villages, such that the approximated additional employment equals $61 \times (\exp^{0.72955} - 1) \times 4,749 = 21,924.28$. The 9 post-reform SEZs are located on 11 municipalities in total, such that the approximated additional employment equals $11 \times (\exp^{1.019281} - 1) \times 4,749 = 92,525.8$.

facturing being more land intensive – find that post-reform manufacturing SEZs generate 181% more employment in SEZ-hosting villages than their pre-reform counterparts. This result seems to be driven by selection within industries. Finally, I show that in the aggregate, post-reform manufacturing SEZs generated more than four times more nonagricultural employment than their pre-reform counterparts. This provides evidence that compulsory acquisition in the context of industrial policy not only affects participation in the policy, but also its consequences for local and total industrial employment.

This research is but a first step into understanding the effect of compulsory acquisition on industrial development, welfare and the effects of industrial policy. First, further investigation into why post-reform SEZ developers generate more employment than their pre-reform counterparts would be fruitful. This paper abstracts from the role of the firms in SEZs due to data constraints; however, learning more about the firms could help illuminate how exactly the proposed selection mechanism for developers manifests itself.Second, since SEZs are also a trade policy, another interesting avenue for future research would be to complement these employment results with other measures of developer quality, such as contributions to international trade, capital and agglomeration effects, to further pinpoint what elements determine a successful SEZ. Finally, a first-order follow-up question to this project is to directly contrast the welfare gains and losses from compulsory acquisition in a quantitative spatial general equilibrium model. This is necessary to fully understand whether the common adage -– that the welfare losses of those who are dispossessed of their land do not come close to the aggregate gains from industrialisation – holds true. Besides adding to our understanding of the welfare implications of compulsory acquisition, this can shed light on the implications of local industrial policy for national structural transformation.

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## SUPPLEMENTAL APPENDIX FOR ONLINE PUBLICATION

## A. DATA SOURCES AND SAMPLE CONSTRUCTION

This appendix complements Section IV, and discusses how I obtained the data on SEZ proposals, the control variables and the municipality dataset.

### *A.1. SEZ proposal data*

This dataset is based on the agendas and minutes of the 119 meetings held by the SEZ Board of Approval between 17 March 2006 and 29 October 2022, which were retrieved from the SEZ India website.[39] I used OCR to transform the scanned meeting minutes (around 2,700 pages) into searchable text, and then used text analysis techniques to create the dataset. Using the structure of these minutes, and specifically how information about the proposal was relayed, allowed me to extract the features listed of each proposal. This resulted in 1,435 unique proposals, with information on date on which the proposal is discussed, the developer and location of the proposed SEZ, the sector of the zone, the proposed size and the final decision, highlighted in Figure A.1 (a). I then merged this with information from the meeting agendas, which provided an overview of all proposals and whether they were new or had been deferred in an earlier meeting. This also listed, for proposals discussed in 2007, when the proposal was submitted, whether the land was already acquired and whether the State Government approved of the proposal (SEZ India, 2022a). To find out which SEZs were developed by public entities, I extracted a list of all State Industrial Development Corporations from the website of Council of State Industrial Development and Investment Corporations of India (COSIDICI), supplemented with ownership data from SEZ India (2014). Finally, I used GIS techniques to create a spatial layer containing the exact location, and specifically villages, that contain an SEZ. I relied on a personal map on Google Maps and OpenCage to obtain coordinates for each proposed SEZ. This left around 500 SEZs to be assigned coordinates by hand, which I did using OpenOSM, Google Maps and newspaper articles. I also manually corrected the geometries that represented large SEZs to accurately capture their size in relation to the villages they are located in. Table A.1 summarizes the location characteristics of the SEZs in my sample.

The second source of data are lists of all notified and operational SEZs compiled and published published by SEZ India; these are updated approximately twice a year. For the main dataset, I used

[39] The minutes for the 5th and 11th meeting were never uploaded, but the notes from the fifth meeting were obtained from Yumpu.com. I used the agenda for the 11th meeting, combined with information on resubmitted proposals and SEZs in further stages of development to infer the decisions made in this meeting.

**1. Request for setting up of a sector specific SEZ for agro processing sector at MIDC Industrial area, Akola District, Akola, Maharashtra by Maharashtra Industrial Development Corporation (MIDC) – 100 hectares (Sl. No. 2):**

The Board noted the Developer was in possession of the land. The State Government had also recommended the proposal. Accordingly, the Board decided to grant **formal approval** for setting up of a **sector specific SEZ for agro processing sector at MIDC Industrial area, Akola District, Akola, Maharashtra by Maharashtra Industrial Development Corporation (MIDC) over an area of 100 hectares.**

**(a)** *Excerpt from BoA meeting on 05/06/2007*

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| 238 | 27 | Navi Mumbai SEZ Pvt. Ltd. | Village Ulwe, Taluka, Panvelo, District Raigad, Maharashtra | MH | IT/ITES-A | 21.13 | 1236(E), Dt.27th May, 2008 | |
| 239 | 28 | Gitanjali Gems Limited | Village Chirvat and Sangurli, Taluka Panvel, District Raigad, Maharashtra | MH | Gems & Jewellery | 10.035 | 1409(E), Dt. 9th June, 2008 | |
| 240 | 29 | Maharashtra Industrial Development Corporation (MIDC) | Akola Industrial Area, Village Mouje Yevata and Shivapur, Taluka & District-Akola | MH | Agro processing | 100 | 1480(E), Dt.17th June 08 | |

**(b)** *Excerpt from List of Notified SEZs on 01/02/2012*

**Figure A.1:** *Building the proposal dataset: MIDC's Akolan SEZ*

three lists of notified SEZs (published on 1st January 2012, 1st December 2017 and 31st December 2022) and the list of operational SEZs on 31st December 2023. With some new uploads, older versions are removed from the site; I therefore used the Wayback Machine to access the earlier documents. The list of notified SEZs provides information on the developer, the location, the sector, the size of the SEZ and the date of notification. The list with operational SEZs only provides the developer, location and sector of the SEZ. Note that these lists also contain the SEZs that were developed before the 2005 SEZ Act; I remove these from the dataset. I again use OCR to transform the tables (around 90 pages) to readable text, and extract the aforementioned characteristics as in Figure A.1 (b). To merge this data with the proposal dataset, I restrict my sample to those proposals that were formally approved. Then, I employed a fuzzy string match algorithm to match proposals to their date of notification and an indicator of operations and verified all matches by hand.

Figure A.2 shows the breakdown of number of proposals by sector and timing. Both before and after the protest, most proposals were for IT SEZs. The second most popular category is multi-product and n.e.c., which refers to SEZs that allow firms in multiple different sectors and SEZs that are not elsewhere classified, such as SEZs for gems and jewellery. Strikingly, one observes more proposals in the services sector after the protest, whereas for manufacturing and

**Table A.1:** *Location characteristics of SEZ proposals*

| | N | Mean | Median | SD |
|---|---|---|---|---|
| Size of subdistrict (ha.) | 1446 | 56114.2 | 52973.2 | 46205.9 |
| Population (2001) | 1446 | 1442781.9 | 430856 | 2603509.9 |
| Population density (2001) | 1446 | 591.7 | 11.1 | 3085.2 |
| Labour force (2001) | 1446 | 509116.2 | 173446 | 895426.7 |
| Agricultural employment (2001) | 1446 | 34877.5 | 33271.5 | 23690.7 |
| Manufacturing employment (2005) | 1446 | 103776.0 | 12958 | 377923.0 |
| Srvices employment (2005) | 1446 | 244488.2 | 33247 | 805341.6 |
| Distance to nearest airport (km) | 1446 | 28.4 | 18.7 | 26.7 |
| Distance to nearest port (km) | 1446 | 212.3 | 216.4 | 176.8 |
| Distance to nearest highway (km) | 1446 | 1.46 | 0.90 | 1.70 |
| Distance to nearest railway (km) | 1446 | 6.04 | 4 | 7.14 |
| Distance to nearest power plant (km) | 1446 | 15.9 | 12.4 | 14.7 |
| Distance to nearest city (>500K, km) | 1446 | 41.3 | 20.4 | 56.2 |
| At least one bank (2005) | 1446 | 0.35 | 0 | 0.48 |
| Number of banks (2005) | 1429 | 73.0 | 16 | 169.7 |
| Land concentration (Theil) | 1270 | 0.59 | 0.54 | 0.31 |

The unit of observation is a proposal-meeting-subdistrict combination. All location characteristics are aggregated up to the subdistrict level. Data on agricultural employment, labour force, subdistrict size and population are from the 2001 Primary Census Abstract; nonagricultural employment from the 2005 Economic Census, bank data is retrieved from the RBI dataset and the Theil coefficient on land concentration is computed using the 2000 Agricultural Census. Distances based on comparing the centroid of each georeferenced SEZ to all places of interest as entered in OpenStreetMap.

utilities there is no consistent pattern. Regarding land intensity, Table A.2 shows how the proposed SEZ size before the reform varies by broad sector and industry. While small-scale manufacturing and larger-scale services sectors exist, these are relatively rare in my sample. In general, the data confirms Mehta (2022) in that manufacturing is significantly more land intensive than services.

Table A.3 shows the industrial composition, subdivided across manufacturing, services and utilities, of SEZs before the protest across these states. First, the average SEZ is larger in compulsory acquisition states for almost all industries, with the exception of apparel, footwear and metals. As the standard errors are quite large, these differences are not statistically significant. Moreover, while the number of proposals is only slightly larger in states that engage in land provision, the sectoral breakdown is quite different. Specifically, these states tend to have relatively more proposals for large-scale industries, such as multi-product SEZs, chemicals, and large-scale power generation.

**Table A.2:** *Mean and median sizes of proposed SEZs by industry before the reform*

| | N | Mean | 25th perc. | Median | 75th perc. |
|---|---|---|---|---|---|
| **Manufacturing** | | | | | |
| Apparel | 11 | 96.06 | 56.6 | 100 | 120 |
| Chemicals | 23 | 322.04 | 100 | 130 | 200 |
| Food processing | 8 | 102.68 | 62 | 103 | 139.7 |
| Footwear and leather | 10 | 106.11 | 60 | 100 | 101 |
| Gems | 6 | 45.94 | 10.2 | 37.3 | 80.9 |
| Machinery | 6 | 88.61 | 12.1 | 77.5 | 144 |
| Metals | 9 | 224.13 | 101 | 101.2 | 106 |
| Multi-product/nec | 101 | 2355.02 | 1000 | 1080 | 2000 |
| Non-metallic mineral products | 6 | 118.62 | 100 | 105.5 | 130 |
| Other transport equipment | 2 | 110.70 | 100 | 110.7 | 121.4 |
| Paper and printing | 1 | 121.40 | 121.4 | 121.4 | 121.4 |
| Petroleum and refineries | 2 | 1000.00 | 1000 | 1000 | 1000 |
| Textile | 30 | 156.13 | 100 | 102.8 | 202 |
| Vehicles | 8 | 107.17 | 68.2 | 100 | 150.5 |
| Total | 223 | 1164.23 | 101 | 283.28 | 1012 |
| **Services** | | | | | |
| IT/ITES | 324 | 35.12 | 10.9 | 16.0 | 30.4 |
| Other business activities | 15 | 147.79 | 100 | 108.9 | 180 |
| Research and Development | 48 | 137.52 | 20.9 | 100 | 142.5 |
| Supporting transport activities | 20 | 230.12 | 40 | 80.4 | 202.5 |
| Total | 407 | 60.93 | 11.25 | 20.23 | 48.48 |
| **Utilities** | | | | | |
| Power generation | 13 | 419.76 | 103 | 180 | 1100 |
| Total | 643 | 450.82 | 13.4 | 47.6 | 168 |

The sample is restricted to proposals discussed before the protest. The unit of observation is a proposal-meeting-subdistrict combination.

**Table A.3:** *Characteristics of SEZ proposals and their locations*

| **SEZ size (ha.)** | No CA policy | | | CA policy | | | Difference | | |
|---|---|---|---|---|---|---|---|---|---|
| By industry: | Mean | SD | N | Mean | SD | N | Diff. | SE | N |
| **Manufacturing** | | | | | | | | | |
| Apparel | 116.528 | (22.633) | 5 | 79.005 | (39.183) | 6 | -37.523* | (19.905) | 11 |
| Chemicals | 146.708 | (111.183) | 9 | 434.759 | (1,030.105) | 14 | 288.051 | (347.515) | 23 |
| Food processing | 92.478 | (60.197) | 4 | 112.875 | (72.137) | 4 | 20.397 | (46.977) | 8 |
| Footwear and leather | 130.880 | (74.662) | 6 | 68.945 | (29.200) | 4 | -61.935 | (39.811) | 10 |
| Machinery | 12.070 | (0.100) | 2 | 126.875 | (74.682) | 4 | 114.804 | (56.011) | 6 |
| Metals | 375.538 | (358.729) | 4 | 103.000 | (2.739) | 5 | -272.538 | (157.544) | 9 |
| Multi-product | 1,780.689 | (2,196.434) | 45 | 2,550.583 | (4,448.210) | 68 | 769.893 | (715.305) | 113 |
| Minerals | 110.000 | (14.142) | 4 | 135.850 | (49.285) | 2 | 25.850 | (23.832) | 6 |
| Transport equipment | 110.702 | (15.136) | 2 | | () | 0 | 0.000 | (0.000) | 2 |
| Paper and printing | 121.4 | () | 1 | | () | 0 | 0.000 | (0.000 | 1 |
| Refineries | | () | 0 | 1,000 | () | 2 | 0.000 | (0.000) | 2 |
| Textile | 138.842 | (84.706) | 15 | 172.589 | (102.804) | 16 | 33.747 | (33.963) | 31 |
| Vehicles | 61.590 | (45.639) | 4 | 152.750 | (60.472) | 4 | 91.160* | (37.881) | 8 |
| **Services** | | | | | | | | | |
| IT | 31.508 | (54.783) | 169 | 39.049 | (74.863) | 155 | 7.541 | (7.247) | 324 |
| Other services | 100.000 | () | 1 | 167.792 | (108.143) | 15 | 67.792 | (111.690) | 16 |
| R& D | 104.943 | (95.354) | 23 | 196.451 | (314.747) | 27 | 91.507 | (68.235) | 50 |
| Warehousing | 195.346 | (174.291) | 11 | 272.611 | (658.081) | 9 | 77.265 | (205.654) | 20 |
| **Utilities** | | | | | | | | | |
| Power generation | 11.900 | () | 1 | 453.750 | (481.082) | 12 | 441.850 | (500.726) | 13 |
| Observations | | | 309 | | | 349 | | | 658 |

The unit of observation is a proposal-meeting combination, excluding two proposals without a sector designation. Classification into manufacturing, services and utilities is based on India's National Industry Classification (NIC). Excluded are Education (services) and Oil and gas (utilities), as these sectors see no proposals before the reform. Standard errors in parentheses.

$^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

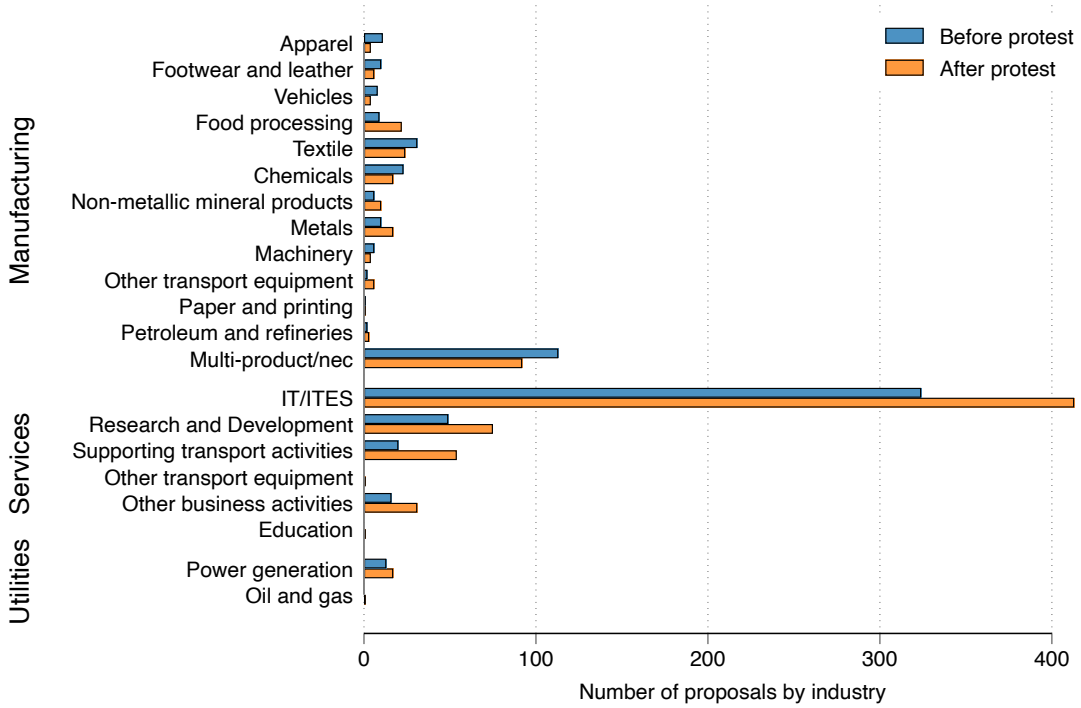



**Figure A.2:** *Proposals by industry, before and after reform*

## *A.2. Data on land concentration*

To obtain a measure of land concentration, I scraped the *Agricultural Census* (AC, 2000), which provides me with the exact plot size distribution, crop types and irrigation in India's subdistricts. This dataset is publicly available, but one needs to download the data separately for each subdistrict-year combination.[40] I follow the literature and use the detailed plot size distribution data to calculate land concentration at the subdistrict, district and state level. As a proxy for land transaction costs, I compute the Theil-T index on land concentration as:

$$T_T = \frac{1}{N}\sum_{i=1}^{N}\frac{x_i}{\mu}\ln\left(\frac{x_i}{\mu}\right)$$

Figure A.3 shows how the Theil index varies across India, with darker colors reflected a higher land concentration. Displayed in white are subdistricts for which I do not have data; states such as Bihar and Jammu and Kashmir were not sampled in 2000 – note that these do not see any new SEZs in my sample period – and some subdistricts could not be matched perfectly.

[40] I obtained these records from the Agricultural Census website, currently available at this link.

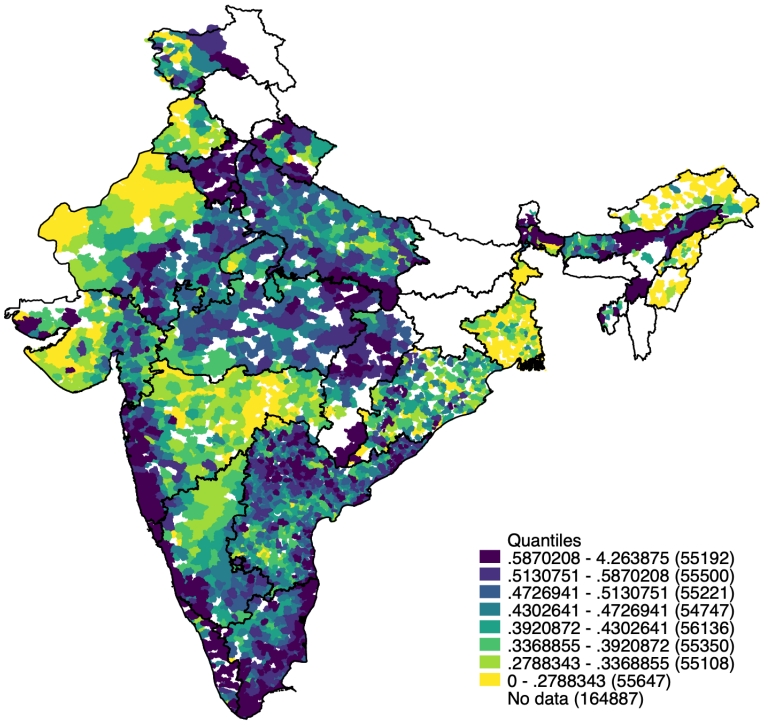



**Figure A.3:** *Theil-T index on land concentration*

## *A.3. Data on SEZ-neighboring municipalities*

Having compiled a dataset of geocoded SEZs that were operational before 2013, I loaded this into QGIS 3.29. Using GIS techniques, I reprojected them to Coordinate Reference System EPSG:7755 – WGS 84, as this projection measures distances in meters instead of degrees. Then, assuming all SEZs are circular, I used the field calculator on this layer to compute the radius based on the area of the SEZ. I then created a buffer around each SEZ point, where the distance equals the implied radius. I then added ten multi-ring buffers with a distance of 5 kilometers around each SEZ to create the distance bins. I then map villages to these buffers, using the shapefile of villages in 2001 from Asher et al. (2021). Discarding any village that is further than 50 kilometers from any SEZ, the sample reduces to 65,204 villages. Then, I assign each village to their closest SEZ based on whether their administrative boundaries overlap with the distance buffers. Figure A.4 displays the villages within 50 kilometers of the E. Complex SEZ in Amreli, next to Pipapav port, in Gujarat; villages in darker colors are further away from the SEZ.

As mentioned before, some villages are close to multiple SEZs. To model these spatial spillovers more precisely, I separately extract the full distance matrix of villages and SEZs within 50 kilometers of these villages, and match this data to my primary dataset. This allows me to

remove any municipalities that host multiple SEZs – mainly large metropolitan areas; to ensure no mismeasurement of the spatial spillovers, I remove these SEZs from the sample. Put differently: every SEZ in the sample enacts, for the purposes of this analysis, a separate effect on local labor markets. This leaves with the aforementioned number of 43,673 municipalities. Table A.4 describes the baseline sample.

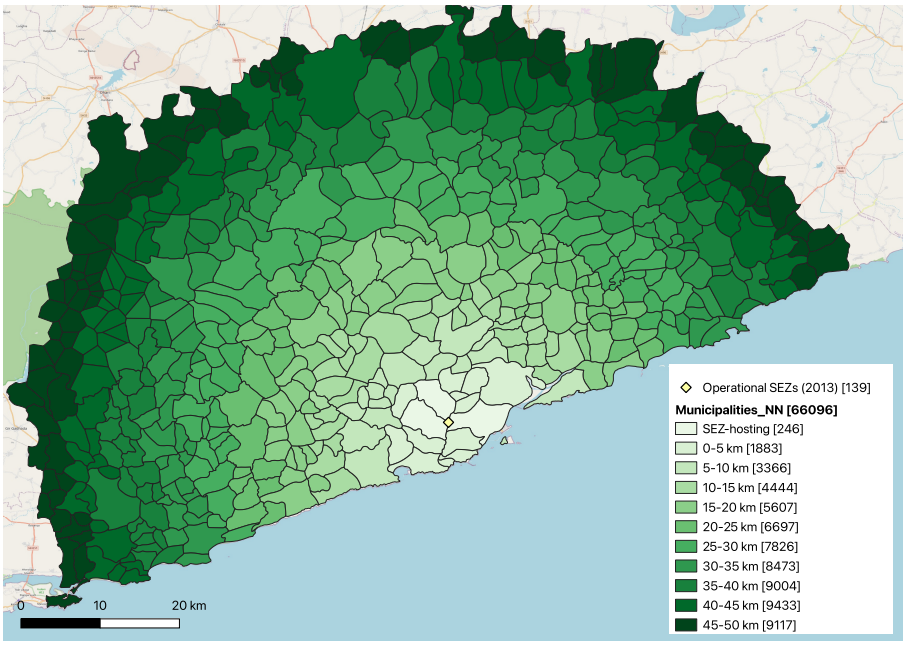



**Figure A.4:** *Villages neighbouring the E. Complex SEZ in Amreli, Gujarat*

## *A.4. Compulsory acquisition policy*

This section discusses the compulsory acquisition policies that inform the treatment assignment, and highlights the necessity for controlling for economic characteristics.

The treatment assignment reflects the content of all state-level SEZ acts, policies or rules. As mentioned before, several states drafted their own SEZ act before the national SEZ Act in 2005, while others set up such legislation after the national Act was published, extending the national Act with additional rules and privileges. Table A.5 shows the relevant clauses of the state SEZ policies and acts that allow State Governments to engage in compulsory acquisition to facilitate their development. It is important to note that for Maharashtra and West Bengal, the clauses do not directly relate SEZs to compulsory acquisition. Instead, these states both have, as described in the footnotes, an established practice of compulsory acquisition for industrial development, and by committing to SEZs as a means of achieving said goal, they opened the door to expropriating land for SEZs. The State Government of Maharashtra, via the associated

**Table A.4:** *Descriptive statistics municipalities sample*

| | N | Mean | Median | SD |
|---|---|---|---|---|
| **Population Census (2001)** | | | | |
| Population | 40039 | 3389.1 | 1167 | 69896.6 |
| Population density | 39133 | 77.9 | 3.21 | 1501.6 |
| Agricultural employment | 40045 | 443.6 | 258 | 613.5 |
| **Economic Census (2005)** | | | | |
| Manufacturing employment | 38775 | 103.2 | 6 | 2252.8 |
| Services employment | 38775 | 266.0 | 34 | 6114.4 |
| **Reserve Bank of India (2005)** | | | | |
| At least one bank | 43673 | 0.062 | 0 | 0.24 |
| Number of banks | 43673 | 0.20 | 0 | 7.85 |
| **Agricultural Census (2000)** | | | | |
| Land concentration (Theil) | 35487 | 0.46 | 0.45 | 0.17 |
| **OpenStreetMap (2023)** | | | | |
| Distance to nearest airport (km) | 43673 | 47.8 | 43.3 | 28.3 |
| Distance to nearest port (km) | 43673 | 206.1 | 139.7 | 177.4 |
| Distance to nearest city (>500K, km) | 43673 | 63.5 | 50.6 | 42.0 |
| Distance to nearest power plant (km) | 43673 | 27.7 | 21.2 | 24.1 |
| Distance to nearest railway (km) | 43673 | 11.6 | 8.85 | 9.91 |
| Distance to nearest highway (km) | 43673 | 3.30 | 2.46 | 3.01 |

The unit of observation is a municipality, excluding those that host multiple SEZs.

Maharashtra Industrial Development Corporation, promised SEZ developers an "expeditious" land acquisition.[41] Meanwhile, the West Bengal State Government is permitted to expropriate land for companies if "such work is likely to prove useful to the public" per the West Bengal Land Acquisition Manual (1991).

**Table A.5:** *State legislation permits compulsory acquisition for SEZs*

| State | Name | Date | Clause |
|---|---|---|---|
| Maharashtra | Resolution No.SEZ 2001 | 12/10/2001 | (12) "The State Government will take appropriate steps to declare SEZs as industrial townships (...)"[42] |
| West Bengal | West Bengal Special Economic Zone Act | 03/03/2003 | 28 (1) "The Governor (...) may by notification, declare such Special Economic Zone to be an Industrial Township in accordance with the provisions of the West Bengal Municipal Act, 1993, and upon such declaration, the provisions of the West Bengal Municipal Act, 1993 shall apply (...)"[43] |
| Madhya Pradesh | The Indore Special Economic Zone (Special Provisions) Act | 28/03/2003 | 7 (1) "The State Government may transfer land owned, acquired or controlled by the State Government to the Developer on such terms and conditions as the State Government may prescribe." |
| Gujarat | Gujarat Special Economic Zone Act | 30/03/2004 | 6 (2) (d) "(...) the Authority [SEZ Development Authority] shall have the following powers (...): to acquire land in the Zone, by consent agreement or through proceedings under the Land Acquisition Act, 1894, for the purpose of this ACT." |
| Chandigarh | SEZ Policy of Chandigarh Administration | 14/03/2005 | IV (9) "Chandigarh administration would carry out the acquisition process for land required for the SEZ and would transfer such land to the concerned department/developer of the SEZ." |
| Tamil Nadu | Tamil Nadu Special Economic Zone Act | 15/10/2005 | 27 (1) "The Government may, on request from a Developer for acquisition of land for establishment of a Special Economic Zone, acquire the required land under the Tamil Nadu Acquisition of Land for Industrial Purposes Act, 1997 and transfer the land to the Developer (...)." |
| Haryana | Haryana Special Economic Zone Act | 17/01/2006 | III. 7 (1) "The Government may transfer land owned, acquired or controlled by it to the Developer as per provisions of the Land Acquisition Act, 1894 (1 of 1894), and the rules made thereunder and as per State Government policy." |

[41] See https://mihansez.org/Pages/details/maharashtra-sez-act-policy.

[42] This affirms that SEZs fall under the Maharashtra Industrial Development Act (1961), where clause 32 (1) reads: "If at any time in the opinion of the State Government, any land is required for the purpose of development by the corporation [the Maharashtra Industrial Development Corporation], or for any other purpose in furtherance of the objects of this act, the State Government may acquire such land (...)."

[43] Again, this means that the State Government can use compulsory acquisition for these projects. Moreover, Clause 77 (1) of the West Bengal Municipal Act (1993) states: "When any land (...) is required for any public purpose under this Act, the State Government may, at the request of the Board of Councillors, proceed to acquire it under the Land Acquisition Act, 1894."

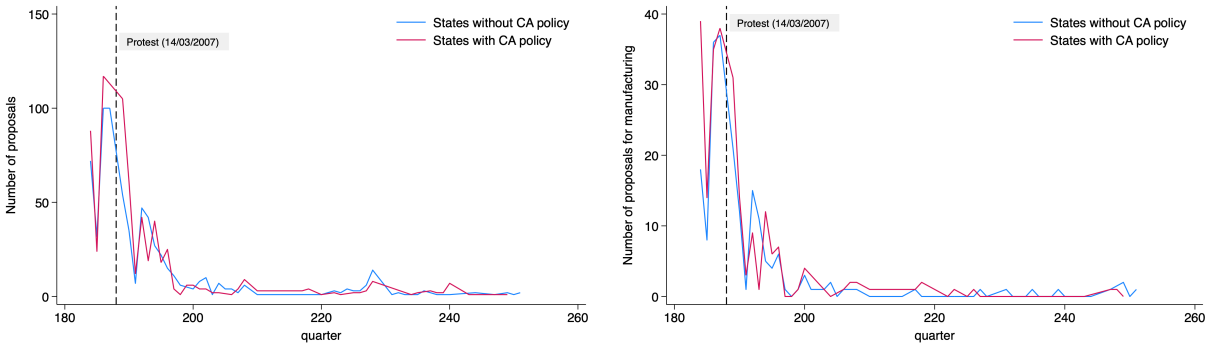
While this introduces variation in the state-level impact of the reform by acting as a proxy for compulsory acquisition, the decision of these states to facilitate investment in SEZs to this extent while others did not might raise concerns for identification. For Union Territories like Delhi this is assuaged as Union Territories do not have a State Government, but fall directly under the Central Government. However, for the other states, a concern remains that the determinants of adopting compulsory acquisition policy for SEZs are correlated with economic characteristics or other factors that influence SEZ development. To understand whether the decision to facilitate compulsory acquisition is driven by state-level characteristics, I regress a dummy that equals one if the state has declared its intention to do so on a variety of initial conditions and employment trends. The results are displayed in Table A.6, where the first three columns display the results for estimating a Linear Probability Model, and the final two columns show the result of a logit regression. The independent variables include most of the controls I have used before, either in levels or in trends, aggregated at the state level. The main takeaway is that trends in employment or population do not significantly influence the decision to engage in compulsory acquisition, but that states with more access to finance (as measured by the number of banks), a larger labor force and specifically manufacturing employment in the period before the Indian SEZ Act are more likely to introduce compulsory acquisition for SEZs, while the size of agricultural and services employment seems to negatively affect said probability. This highlights first of all that it is not necessarily the states that are economically backward that introduced this legislation, mitigating the concern that the compulsory acquisition policies disproportionally drew in investment to otherwise unattractive regions. The effect of the reform does thus not come from SEZ developers switching to strictly better areas in all other aspects besides land acquisition costs. However, the fact that a higher manufacturing employment or number of banks increases the probability of a state adopting compulsory acquisition policy implies that these places are even more suitable for manufacturing SEZs (especially as compared to services), such that the impact of the reform might be overstated – the states that are treated more intensely by the reform are also those that have favorable conditions for industrial development. This threat does not necessarily manifest in the data, as per Table 2, I do not find a significant difference in the share of manufacturing proposals between states with and without compulsory acquisition; still, it remains important to control for these conditions. This is why I include economic region fixed effects, which subsume state fixed effects and would thus capture the inclinations in the below Table, but also allow local economic variables to flexibly capture any preferences of SEZ developers.

**Table A.6:** *States' propensity to introduce compulsory acquisition policy*

| | LPM | | | Logit | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| State size | -0.0105 | | -0.0131 | -0.538 | |
| | (0.0188) | | (0.0338) | (1.999) | |
| Number of banks | 0.235*** | | 0.364*** | 11.60* | |
| (2005) | (0.0761) | | (0.106) | (6.599) | |
| Land | -1.345 | | -1.335 | -2.232 | |
| concentration | (1.121) | | (1.710) | (17.72) | |
| Labor force | 1.272* | | 0.923 | 46.99* | |
| (2001) | (0.661) | | (0.933) | (24.73) | |
| Agricultural | -0.278** | | -0.277* | -8.038* | |
| emp. (2001) | (0.109) | | (0.136) | (4.120) | |
| Population | -0.856 | | -0.739 | -35.62 | |
| (2001) | (0.726) | | (0.797) | (22.42) | |
| Manufacturing | 0.0316 | | 0.0678 | 9.386* | |
| emp. (2005) | (0.0898) | | (0.735) | (5.682) | |
| Services emp. | -0.203 | | 0.0162 | -16.47** | |
| (2005) | (0.341) | | (1.382) | (7.182) | |
| Pop. growth | | -1.048 | 0.484 | | -8.547 |
| (91-01) | | (1.243) | (1.135) | | (7.786) |
| Labor force | | 0.476 | 0.482 | | 3.421 |
| growth (91-01) | | (1.107) | (1.969) | | (6.452) |
| Agricultural | | 0.0316 | 0.366 | | -0.0717 |
| emp. growth (91-01) | | (0.429) | (0.610) | | (2.674) |
| Manufacturing | | 0.00891 | 0.371 | | 0.325 |
| emp. growth (98-05) | | (0.191) | (0.500) | | (1.502) |
| Services emp. | | -0.184 | -0.894 | | -2.068 |
| growth (98-05) | | (0.376) | (0.956) | | (3.447) |
| Observations | 29 | 33 | 29 | 29 | 33 |
| R-squared | 0.419 | 0.0464 | 0.590 | | |
| Log pseudolikelihood | | | | -5.031 | -16.09 |

The dependent variable is a dummy that equals one if a state adopted compulsory acquisition policy and zero otherwise. All independent variables are in logs and computed as the state-level average. See Table 4 for details on these variables. Robust standard errors are in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

## B. ADDITIONAL RESULTS




**(a)** *All proposals* **(b)** *Manufacturing proposals*

**Figure A.5:** *Total number of proposals by state compulsory acquisition policy*

**Table A.7:** *The number of manufacturing proposals decreases after the reform*

| | Number of proposals | | Number of manuf. proposals | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| After protest | -6.272** | | -2.139*** | |
| | (2.573) | | (0.693) | |
| State CA | 3.896 | | 2.533** | |
| | (3.423) | | (1.146) | |
| After protest | -3.679 | -3.271 | -1.995* | -1.965 |
| × State CA | (3.307) | (3.224) | (1.153) | (1.174) |
| P-value | 0.791 | 0.322 | 0.0290 | 0.109 |
| Controls | Yes | No | Yes | No |
| State and Year FE | No | Yes | No | Yes |
| Observations | 290 | 293 | 290 | 293 |
| R-squared | 0.247 | 0.432 | 0.248 | 0.365 |

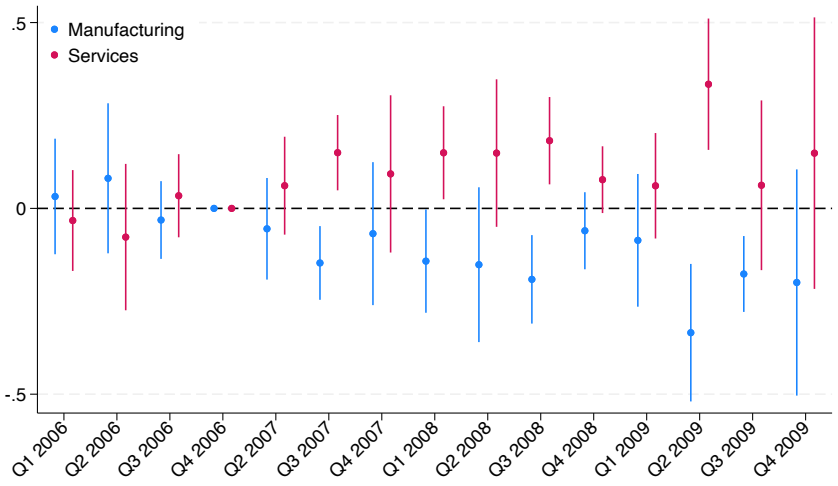
I run a variation of Equation 2, where the dependent variable is the total number of (manufacturing) proposals per quarter in a state. For columns 1 and 3, *P-value* is that of the F-test that the sum of the coefficients on *State CA* and the interaction variable equal 0; for columns 2 and 4 it is the p-value associated with the coefficient on the interaction variable. The controls include log state size, state-level land concentration, the log state population, labor force and agricultural employment in 2001; log nonagricultural employment and log number of banks in 2005. Standard errors, clustered at the state level, are in parentheses.

$^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

**Table A.8:** *Probability of moving to the next development stage is unaffected*

| | Formal Approval | | Notification | | Operational | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | All | Man. | All | Man. | All | Man. |
| After protest | 0.0514 | 0.166 | 0.0696 | 0.104 | -0.00363 | 0.00307 |
| × State CA | (0.0870) | (0.163) | (0.0854) | (0.143) | (0.0487) | (0.0948) |
| Location trends | Yes | Yes | Yes | Yes | Yes | Yes |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Meeting FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 950 | 260 | 950 | 260 | 950 | 260 |
| R-squared | 0.333 | 0.473 | 0.278 | 0.422 | 0.250 | 0.409 |

Each observation is a SEZ-meeting-subdistrict combination, excluding all Utilities and resubmitted proposals. These results come from estimating Equation 2 where the dependent variable is an indicator that equals one if the proposed SEZ is formally approved (columns 1 & 2), notified (columns 3 & 4) or operational (columns 5 & 6) respectively. Every second column restricts the sample to only manufacturing SEZs. See the notes under Table 4 for details on the included location trends. Standard errors, clustered at the state level, are in parentheses.
$^{*}p < 0.1$,$^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.




*Note:* I plot, for both manufacturing and services, the coefficient per quarter relative to the baseline meeting just before the protest.

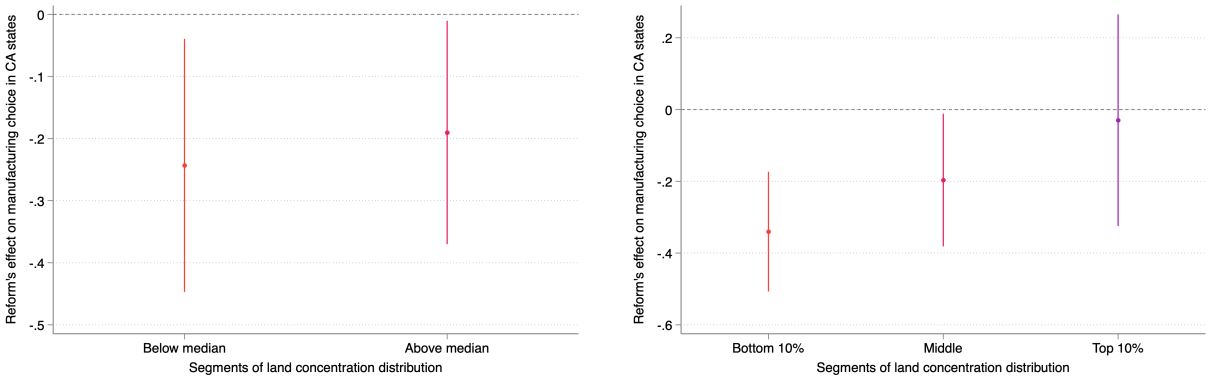
**Figure A.6:** *No general trends in sectoral composition of SEZs*

**Table A.9:** *SEZs after the reform proposed in areas with higher land concentration*

| | All industries | | | Excluding Utilities | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | All | Approval | Not. | All | Approval | Not. |
| After protest | 0.0783* | 0.121** | 0.122** | 0.0822* | 0.122** | 0.125** |
| × State CA | (0.0401) | (0.0466) | (0.0433) | (0.0404) | (0.0458) | (0.0427) |
| Location trends | Yes | Yes | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Meeting FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1233 | 614 | 395 | 1211 | 606 | 391 |
| R-squared | 0.579 | 0.656 | 0.686 | 0.582 | 0.655 | 0.684 |

I estimate Equation 2 with land concentration in the proposed SEZ location as the dependent variable. The first set of results consider all proposals; the second set excludes proposals for Utilities SEZs. Each first column contains all proposals, while the second and third column restrict the sample to formally approved and notified SEZs respectively. For reference, the mean (standard deviation) of the Theil index equals 0.581 (0.279). The included location trends are as in Table 4 excepting the Theil index. Standard errors, clustered at the state level, are in parentheses.
$^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

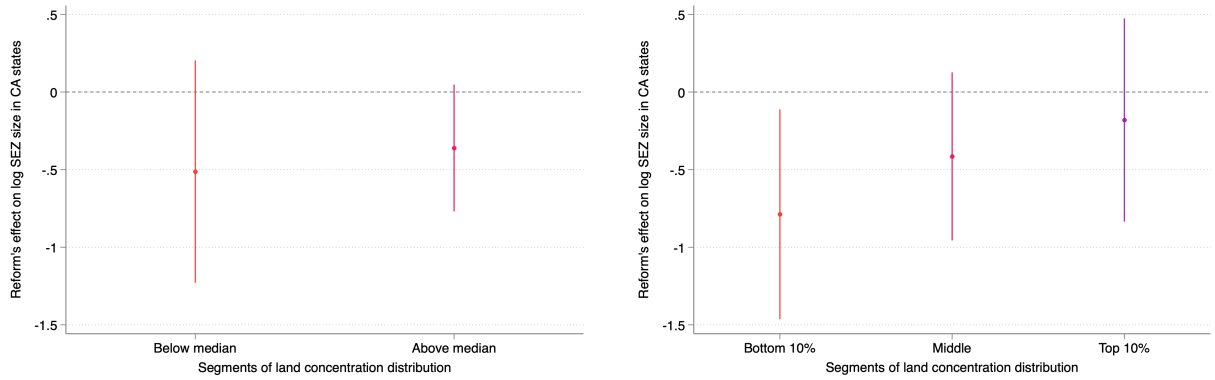
## C. ADDITIONAL ROBUSTNESS CHECKS




**(a)** *Comparing below and above median* **(b)** *Comparing left- and right tail of the distribution*

*Note:* I estimate a variation of Equation 3 with a manufacturing dummy as the dependent variable; instead of the Theil index being a continuous variable I use (a) an indicator variable that equals one if the local land concentration is above the median and zero otherwise, or, for (b), a categorical variable indicating whether an area is in the lower 10th, last 10th or the middle 8 deciles. The coefficients refer to the total effect of the reform.

**Figure A.7:** *Decrease in the manufacturing share most pronounced in areas with low land concentration*




**(a)** *Comparing below and above median* **(b)** *Comparing left- and right tail of the distribution*

*Note:* I estimate a variation of Equation 3 with log SEZ size as the dependent variable; instead of the Theil index being a continuous variable I use (a) an indicator variable that equals one if the local land concentration is above the median and zero otherwise, or, for (b), a categorical variable indicating whether an area is in the lower 10th, last 10th or the middle 8 deciles. The coefficients refer to the total effect of the reform.

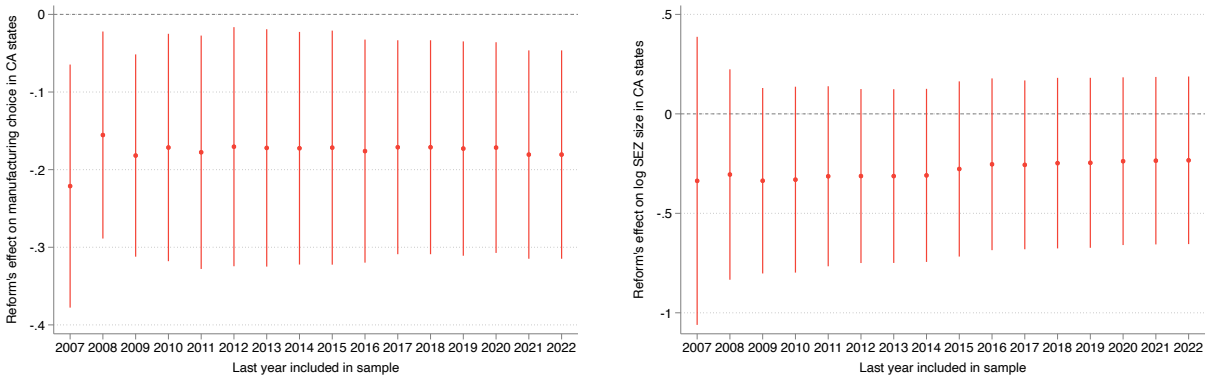
**Figure A.8:** *Decrease in log SEZ size most pronounced in areas with low land concentration*

**Table A.10:** *Post-reform, large-scale industries are less likely*

| | Industry rank | | | Median industry size | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| After protest | -2.050** | -1.605** | -1.676** | -78.78** | -68.01** | -73.42** |
| × State CA | (0.817) | (0.617) | (0.601) | (34.98) | (31.51) | (30.91) |
| Location controls | No | Yes | No | No | Yes | No |
| Location trends | No | No | Yes | No | No | Yes |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Meeting FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1228 | 1228 | 1228 | 1228 | 1228 | 1228 |
| R-squared | 0.301 | 0.413 | 0.410 | 0.216 | 0.284 | 0.281 |

I rerun Equation 2. In the first three columns, the dependent variable is a categorical variable indicating the industry rank of the proposed SEZ, ordered from low to high based on the median SEZ size before the reform as in Table A.2. A higher rank number means a larger-scale industry. In the last three columns, the dependent variable is the median SEZ at the industry level before the reform. See the notes under Table 4 for details on the included location trends. Standard errors, clustered at the state level, are in parentheses.

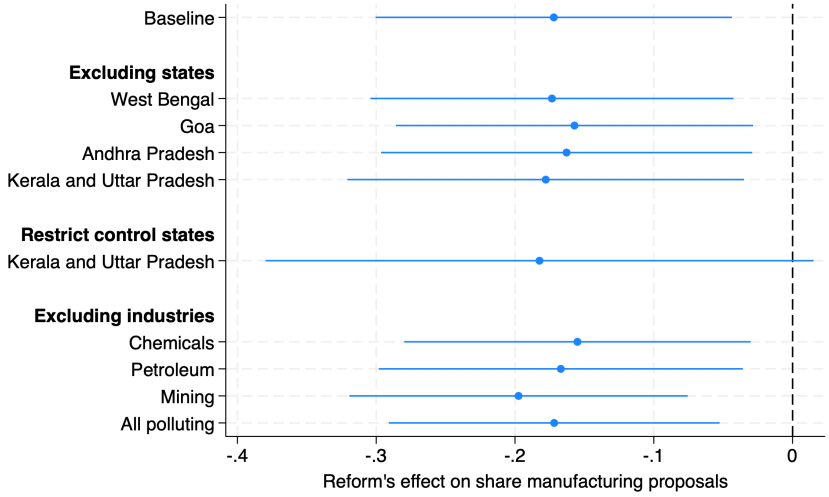
$^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.




**(a)** *Effect on manufacturing share* **(b)** *Effect on log SEZ size*

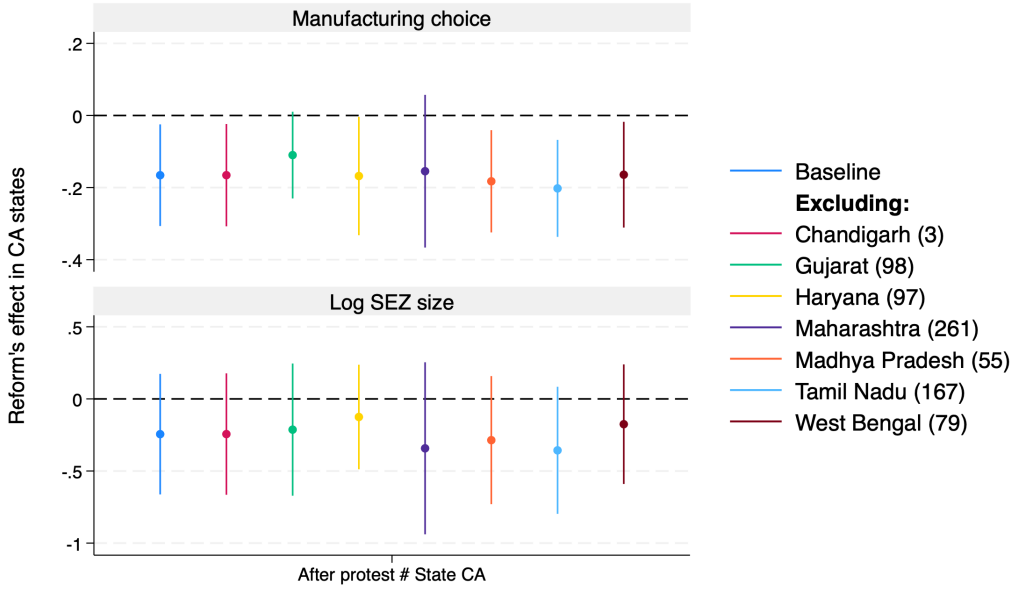
*Note:* I estimate Equation 2, excluding later years from the sample: the coefficient labelled 2007 includes only proposals in meetings in the years 2006 and 2007; each following coefficient extends the sample by one additional year.

**Figure A.9:** *Reform's effect materializes in the first year(s) post-reform*



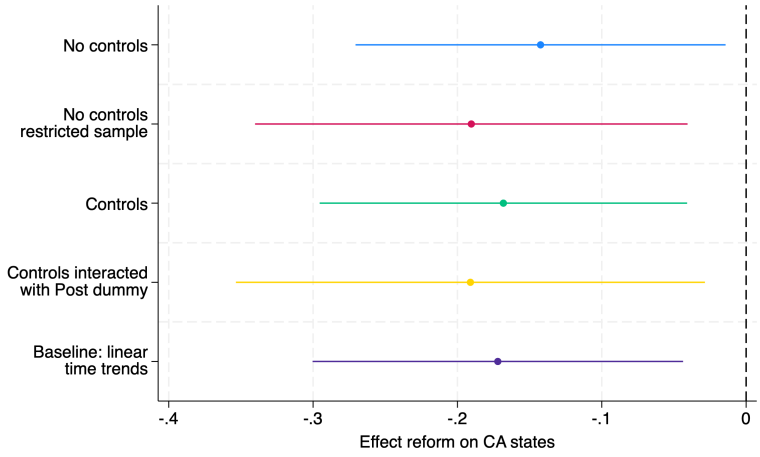

*Note:* This graph studies the robustness of the coefficient in column 2 of Table 4 to different samples. First, I exclude West Bengal, Goa and Andhra Pradesh, respectively. Kerala and Uttar Pradesh, are first excluded from the analysis and then used as the control group. Finally, I exclude polluting industries chemicals, petroleum and mining.

**Figure A.10:** *Robustness checks: sample restrictions*




*Note:* This graph studies the robustness of the coefficient in column 2 of Table 4 to removing one of the treated states at a time from the sample.

**Figure A.11:** *Robustness to removing one treated state at the time*




*Note:* This graph studies the robustness of the coefficient in column 2 of Table 4 to different sets of controls. The first two coefficients are estimated without controls: the first is based on the full sample; the second on the restricted sample used in Table 4 for which all controls are available. The third estimation includes all controls, and the fourth coefficient includes the controls interacted with a *After protest* dummy.

**Figure A.12:** *Robustness to inclusion of controls*

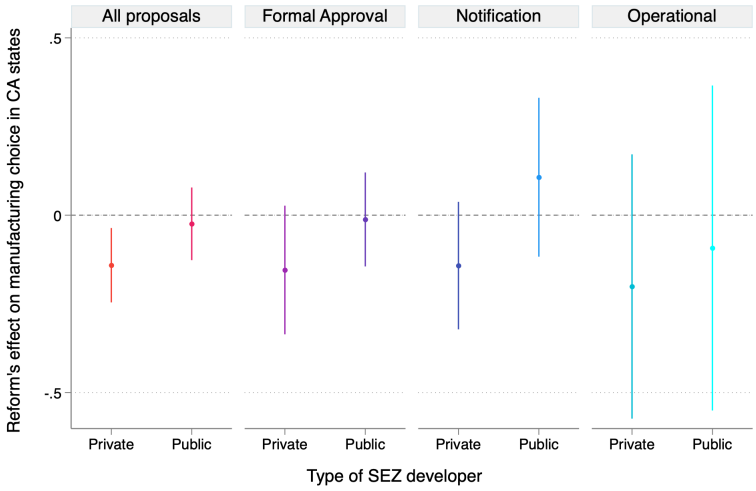
**Table A.11:** *Placebo tests pre-reform yield no significant effect on sectoral composition*

| | Placebo pre-protest | | Exclude meetings | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | 09/08/2006 | 25/09/2006 | Meeting 1 | Meeting 1 & 2 |
| After placebo | -0.0119 | -0.0240 | | |
| × State CA | (0.0918) | (0.136) | | |
| After protest | | | -0.131** | -0.0794 |
| × State CA | | | (0.0603) | (0.0548) |
| Location trends | Yes | Yes | Yes | Yes |
| Region FE | Yes | Yes | Yes | Yes |
| Meeting FE | Yes | Yes | Yes | Yes |
| Observations | 533 | 533 | 1078 | 1031 |
| R-squared | 0.436 | 0.436 | 0.345 | 0.350 |

This analysis replicates column 2 in Table 4. The first half of the table executes placebo tests with the sample restricted to proposals discussed before the protest. The first column assigns all proposals discussed after 09/08/2006 as treated, which is about half the pre-protest *sample*. The second column assigns all proposals proposed after 25/09/2006 as treated, corresponding to half of the pre-protest *meetings* being treated. The last two columns exclude proposals from either the first meeting or both the first and second meeting. Note that the number of proposals in the sample is lower because of availability of controls. See the notes under Table 4 for details on included location trends. Standard errors, clustered at the state level, are in parentheses.
$^{*}p < 0.1$,$^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

## D. Additional analysis on local labor markets

As mentioned before, the estimated spillovers in Equation 4 reflect the average spillovers on control units in each respective distance bin. If spillovers are additive, or otherwise affect local labor markets differentially depending on the number of nearby SEZs, the interpretation of these coefficients is potentially misleading (Butts, 2023). There are two specific reasons why that might be the case for Indian SEZs. First, around 10% (100%) of my sample is exposed to more than one SEZ within 20 (50) kilometers and is thus effectively treated by multiple SEZs. Second, 7 (39) percent of my sample observes multiple SEZs *in the same distance bin* within 20 (50) kilometers. Together, this means that the average spillover on a control unit in a certain distance bin potentially also contains the spillover effect of another SEZ within range, or even another SEZ in the same distance bin. To disentangle these spillover effects, I follow Butts (2023) and regress employment outcomes in a municipality on the full count of SEZs in each distance bin within 50 kilometers.




*Note:*

These coefficients are based on a triple difference-in-differences estimation where the third interaction variable is a dummy that equals one if the SEZ developer is a public organization or joint venture. For the last three models, the sample excludes resubmitted proposals and only those that made it to this development stage.

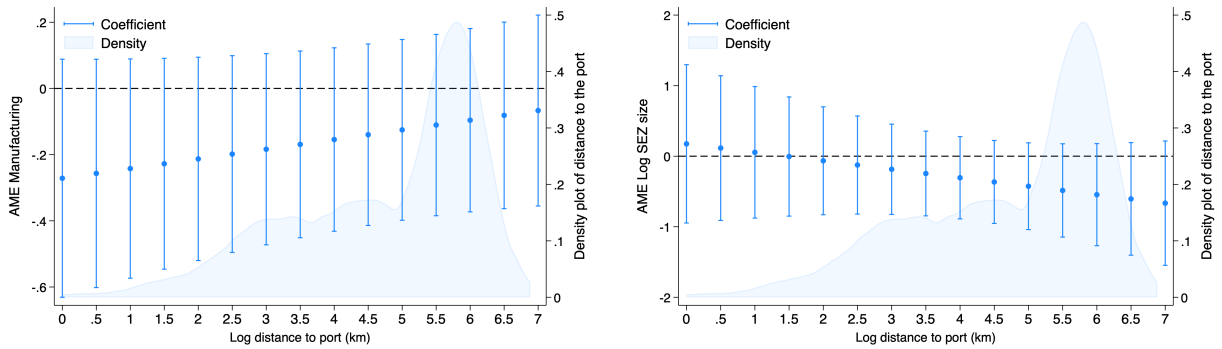
**Figure A.13:** *Effect is substantially (albeit insignificantly) larger for private SEZs than for public SEZs*

**Table A.12:** *Relative decrease of manufacturing share not driven by SEZ competition*

| | (1) All | (2) Formal Approval | (3) Notification | (4) Operational |
|---|---|---|---|---|
| After protest | -0.183*** | -0.255** | -0.215** | -0.403*** |
| × State CA | (0.0608) | (0.0912) | (0.0915) | (0.0897) |
| Location trends | Yes | Yes | Yes | Yes |
| Notified manuf. SEZs | Yes | Yes | Yes | Yes |
| Region FE | Yes | Yes | Yes | Yes |
| Meeting FE | Yes | Yes | Yes | Yes |
| Observations | 1210 | 600 | 384 | 183 |
| R-squared | 0.354 | 0.411 | 0.488 | 0.660 |

This Table replicates column 2 in Table 4, but also controls for the number of state-level notified manufacturing SEZs per year. Controlling for the total number of state-level notified SEZs per year yields similar results. See the notes under Table 4 for details on the sample specification and the included location trends. Standard errors, clustered at the state level, are in parentheses.

$^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.




**(a)** *Effect on manufacturing share* **(b)** *Effect on log SEZ size*

*Note:* I estimate a variation of Equation 3, where I interact the treatment variable with distance from the port for the proposed SEZ locations. The coefficients are estimated marginal effects across log distance to the port in compulsory acquisition states; the lightblue area depicts the density of distance to the port.

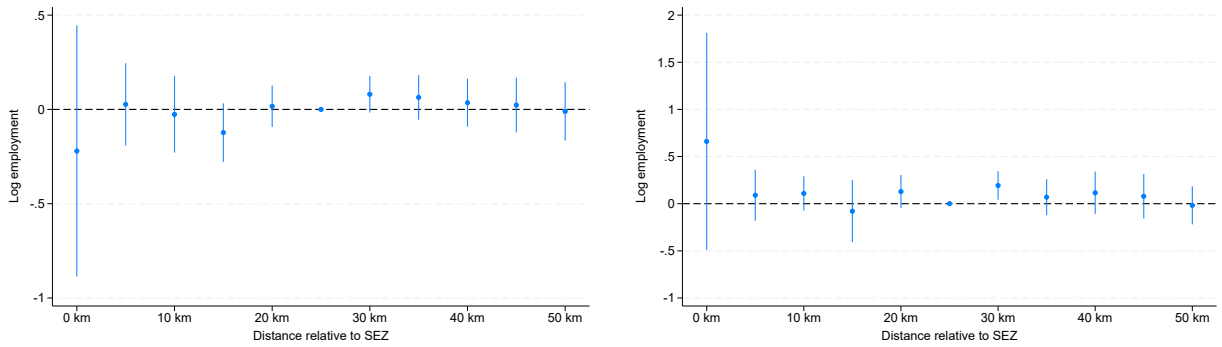
**Figure A.14:** *Average marginal effects with respect to distance to the port*

**Table A.13:** *Odds of a manufacturing SEZ lower in CA states after reform*

| | All proposals | | | Proposals (2006-2009) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| After protest | 0.678 | 0.609** | 0.650* | | | |
| | (0.176) | (0.133) | (0.148) | | | |
| After protest | 0.592 | 0.584 | 0.572 | 0.487** | 0.472** | 0.465** |
| × State CA | (0.213) | (0.193) | (0.196) | (0.170) | (0.145) | (0.144) |
| Location controls | No | Yes | No | No | Yes | No |
| Location trends | No | No | Yes | No | No | Yes |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | Yes | Yes | Yes |
| Observations | 1232 | 1232 | 1232 | 1067 | 1067 | 1067 |
| Log conditional likelihood | -525.4 | -472.8 | -474.2 | -463.2 | -413.0 | -414.0 |

The dependent variable is a dummy that equals one if the SEZ is for *Manufacturing*; the model specification is a conditional logit and the reported estimates are odds ratios. The first three columns consider all proposals but there are insufficient degrees of freedom to include time fixed effects. The last three columns restrict the sample to proposals discussed between 2006 and 2009 and include year fixed effects. Degrees of freedom restrictions aside: each first column replicates column 1 and each third column column 3 in Table 4. See the notes under Table 4 for details on the included location trends. Standard errors, clustered at the state level, are in parentheses.

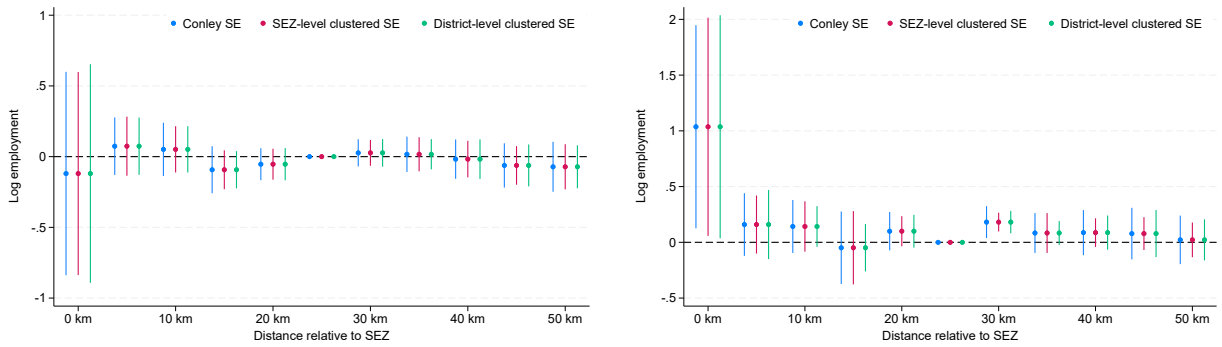
$^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.




**(a)** *Full sample* **(b)** *Manufacturing*

*Note:* These coefficients are based on re-estimating (a) Equation 4 and (b) Equation 5 without any controls.

**Figure A.15:** *Employment effects on nearby villages from operational SEZs without controls*




**(a)** *Full sample* **(b)** *Manufacturing*

*Note:* These coefficients are based on re-estimating (a) Equation 4 and (b) Equation 5 with different estimation strategies for the standard errors. For each distance bin, the first is the baseline estimation, the second clusters standard errors at the nearest SEZ level, and the final set of standard errors are clustered at the district level.

**Figure A.16:** *Estimation with different standard errors*

More specifically, I estimate the below equation using the full municipality sample:

$$\begin{aligned} \mathrm{d}\ln Y_{mt} = \sum_{d=0}^{10}\sum_{c=1}^{3}\left[\beta^{cbd}S_m^{cbd} + \beta^{cad}S_m^{cad} + \sum_{j=1}^{3}\beta^{cjd}\left(S_m^{cbd}\cdot S_m^{cad}\right)\right] \\ + \gamma'(\mathbb{X}_m \cdot Post_{mt}) + Post_{mt} + \alpha_d + \alpha_t + \epsilon_{mt}, \end{aligned} \tag{6}$$

where $b$ and $a$ denote before and after the protest respectively and $c$ indicates the number of SEZs, where $c = 0$ means no SEZ and $c = 3$ means three or more SEZs.[44] $S_m^{cbd}$ and $S_m^{cad}$ are indicator variables representing the number of SEZs $c$ in distance bin $d$ surrounding municipality

[44] Of all villages, 13% (1.8%) has more than three pre-reform SEZs in a certain distance bin (within 20 kilometers of said village). For post-reform SEZs, this figure is 0.02%.

$m$ proposed before and after the reform. I further include interaction terms to allow spillovers to differ based on different combinations of pre- and post-reform SEZs in the vicinity of each municipality. Then, $\beta_d^{cb}$ can be interpreted as the change in employment for a village in the vicinity of $c$ pre-reform SEZs at distance $d$. Here, the identifying assumption is that villages with the same number of nearby SEZs at the same distance are comparable after controlling for time-varying heterogeneity. It should be noted that this does not allow for the unbiased estimation of the total treatment effect; the estimated spillover effect is an average over treatment and control units with the same SEZ exposure in each distance bin. Not only does the coefficient for SEZ-hosting municipalities not include spillovers on the treated, it will not uncover the direct treatment effect unless the exposure mapping is specified correctly (Butts, 2023). Thus, the following results are aimed at producing unbiased estimates for the spillover effects only.

I rerun the same equation focussing on the spillover effect of manufacturing SEZs:

$$\begin{aligned} \mathrm{d}\ln Y_{mt} = &\sum_{d=0}^{10}\sum_{c=1}^{3}\left[\beta^{cbd}M_m^{cbd} + \beta^{cad}M_m^{cad} + \sum_{j=1}^{3}\beta^{cjd}\left(M_m^{cbd}\cdot M_m^{cad}\right)\right] \\ &+ \gamma'(\mathbb{X}_m \cdot Post_{mt}) + Post_{mt} + \alpha_d + \alpha_t + \epsilon_{mt}, \end{aligned} \tag{7}$$

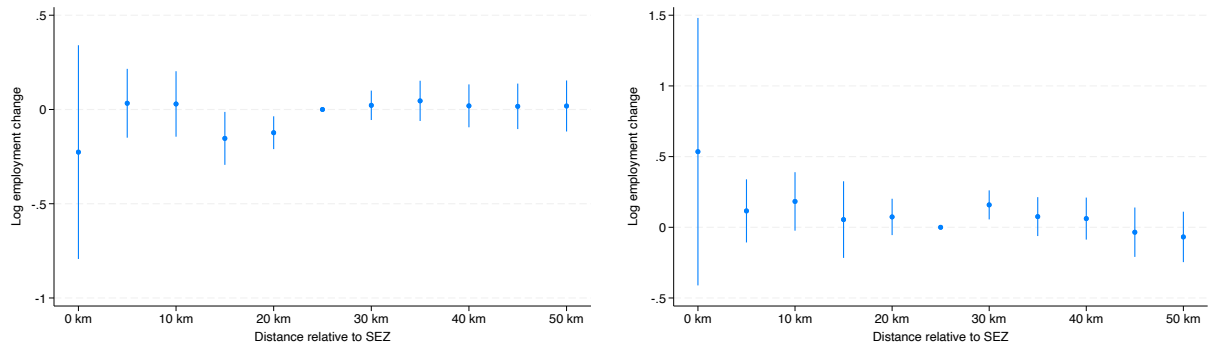
where $M_m^{cbd}$ and $M_m^{cad}$ are the corresponding indicator variables equal to one if at distance $d$ from municipality $m$ there are $c$ pre- or post-reform *manufacturing* SEZs respectively.

To facilitate interpretation, I use the coefficients estimated in Equation 6 to create linear combinations for $d = 0, ..., 10$ and test whether these are different from zero:

$$\tilde{\beta}_d^1 = (\beta_d^{1a} - \beta_5^{1a}) - (\beta_d^{1b} - \beta_5^{1b}).$$

The difference between $\beta_d^{1a}$ and $\beta_5^{1a}$ gives me the average spillover effect of being exposed to one post-reform SEZ at distance $d$ compared to one at 20-25 kilometers away. Subtracting $\beta_d^{1b} - \beta_5^{1b}$ from this tests whether that spillover is significantly different compared to that generated by a pre-reform SEZ at the same distance.

Figure A.17 presents the results with the corresponding 90% confidence intervals from this exercise, where each coefficient can be interpreted as the average spillover effect of being exposed to one post-reform SEZ instead of one pre-reform SEZ in a specific distance bin. Panel (a) shows the results for the full sample of SEZs, where we observe a smaller coefficient for SEZ-hosting municipalities than before, implying that the *spillovers* on treated municipalities positively impacted employment. We see there that the spillover effects up until 20 kilometers from a post-reform SEZ are slightly smaller than in the baseline; this result is even significant and negative for villages between 10 and 20 kilometers away, suggesting that there, pre-reform SEZs outperform their post-reform counterparts. In panel (b), the results from estimating Equation 7 are shown. Here, I




**(a)** *All sectors* **(b)** *Manufacturing sectors*

**Figure A.17:** *Spillover effects of one nearby post-reform SEZ*

find that the direct effect of an SEZ on an SEZ-hosting municipality is positive but insignificant. Again, this suggests that spillovers from other SEZs on treated municipalities are instrumental in explaining the employment growth associated with SEZs. In general, the average spillovers of post-reform SEZs are larger than in the baseline results; however, no estimate except at 25-30 kilometers away is significantly different from zero.