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 policies. I study the effect of this practice on industrial composition and local employment, exploiting an unexpected reform that prohibited compulsory acquisition for Indian Special Economic Zones (SEZs). I pin down the effect on entry by constructing a novel dataset on the universe of SEZ proposals before and after the reform. First, I find that the share of manufacturing SEZ proposals, and the share of developed manufacturing SEZs, decreases by almost 50 percent. This effect is most pronounced for (a) more land-intensive industries and (b) areas with higher land fragmentation. Second, I study how restricting compulsory acquisition affects employment in and around SEZs, and find that manufacturing SEZs after the reform are associated with significantly higher local employment than SEZs before the reform. Despite the reduction in entry, post-reform manufacturing SEZs contributed four times more to aggregate employment than their pre-reform counterparts.

JEL classification: L16, O14, Q15

Keywords: Land Reform, Industrial Policy, Industry Mix, India.

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I. INTRODUCTION

In most developing countries, land acquisition is one of to largest obstacles in the transition from agriculture to manufacturing (Deininger, 2003). 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 fragmented 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). There is however no systematic evaluation of how compulsory acquisition impacts the formation and scale of industrial development.

There are two main empirical challenges to answering this question. First, estimating the effect of land reform is often plagued with endogeneity concerns: land reforms are generally anticipated, allowing industrialists to potentially adapt their investment decisions and timing. Second, there is a lack of suitable data, as both compulsory acquisition and (intentions around) entry are generally unobserved. In this paper, I leverage the institutional context of Special Economic Zones (SEZs) in India to provide novel evidence on how compulsory acquisition impacts industrial composition and local employment. 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, instated 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 committed to expropriating land for SEZ developers (Levien, 2012). Then, in 2007, a large protest against a SEZ in West Bengal was violently shut down by the state police, resulting in at least fourteen casualties (Patra, 2019). In response to this tragedy, the Central Government prohibited compulsory acquisition for SEZs and argued for better compensation to landowners (SEZ Board of Approval, 2007). This increased the costs of land acquisition, and especially in those states with a compulsory

[1] According to Land Matrix (2023), a global initiative aimed at creating a cohesive dataset about large-scale land acquisitions, land deals to facilitate non-agricultural activity more than quintupled in the last twenty years, amounting to 552,566.99 square kilometers, which is almost equivalent to France's total land area. More than 25 percent of these deals involved a governmental body, either as an actor in the negotiation process or as a direct investor.

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

acquisition mandate for SEZ developers.

To capture whether the reform changed *participation* in industrial policy, I have constructed a unique dataset of the universe of official SEZ proposals between 2006-2022. I have scraped the Minutes from 112 SEZ Board of Approval meetings, and used text analysis to compile a dataset of 1,435 proposals, with information on the developer, its proposed location, proposed size and planned sector of operation. I supplement this with publicly available data on SEZ notification and operation such that I can track SEZ development over time, and SHRUG village-level data on economic development and sectoral employment (Asher et al., 2021).

To guide the empirical analysis, I rely on a simple conceptual framework that analyses the problem of the SEZ developer in light of existing theories on firm entry and behavior. Here, land acquisition costs act as an entry barrier for potential SEZ developers, and this barrier is especially high for the more land-intensive manufacturing sectors (Batista e Silva et al., 2014). This increase in entry costs might induce two types of reallocation. First, one would expect a reallocation from relatively more land-intensive sectors to sectors that are relatively less land-intensive. Second, there might be reallocation within sectors: following e.g. Hopenhayn (1992), the increase in entry barriers also implies that only developers with a higher productivity will be able to profitably enter. Thus, the reform will affect SEZ-led industrial development as especially *manufacturing* developers are less inclined to set up an SEZ, but the developers that do enter should be more profitable than their older counterparts.

I first study whether the reform indeed affected participation in industrial policy in terms of SEZ size and sectoral choice. Methodologically, I employ a difference-in-differences design, comparing the reform's effects on SEZs across Indian states with and without compulsory acquisition mandates. As states might adopt compulsory acquisition policy especially because they are otherwise unattractive for SEZs, I control for both aggregate fixed effects and local trends to limit endogeneity concerns. Moreover, as I predict a differential response of manufacturing compared to services, the effect of the reform is properly identified insofar as treated states are not also subject to concurrent manufacturing shocks. I find that the share of manufacturing SEZ proposals in states that committed to compulsory acquisition is around seventeen percentage points lower after the reform. With a baseline manufacturing share of 35.7 percent, this implies a reduction in intentions to enter of almost 50 percent. This effect persists across all stages of SEZ development: the share of operational manufacturing SEZs, conditional on entry, is 24.5 percentage points lower. In terms of size, I find that proposed SEZs are slightly smaller but those SEZs that eventually become operational are significantly larger. Finally, I investigate whether it is indeed increased land transaction costs that can plausibly explain the observed change in SEZ sectoral composition. I document that the effect is driven by (a) more land-intensive industries and (b) areas with higher

land fragmentation. My results thus show that an increase in land acquisition costs reduces the share of manufacturing in both proposed and ultimately developed SEZs.

I then analyse whether the compulsory acquisition reform impacted employment in and surrounding SEZs. I use two rounds of the Economic Census (2005-2013), which is a full count of nonagricultural employment, to construct a sample of all villages within 50 kilometers of one of the 139 SEZs that became operational between 2005 and 2013. As SEZs might affect local labor markets beyond their specific location, the natural control group of nearby villages is potentially contaminated. I employ a spatial difference-in-differences strategy, where I effectively study employment changes in directly treated, potentially indirectly treated and unaffected villages (Gallé et al., 2024). I then compare these local labor market effects for SEZs before and after the reform. I find a negative but insignificant effect on local employment for the full sample. The theory would suggest this effect might be stronger for manufacturing, and indeed I find that manufacturing SEZs proposed after the reform are associated with significant employment increases in their municipality compared to their older counterparts. Specifically, I find that nonagricultural employment in SEZ-hosting municipalities increases by 177 percent from a baseline average of 4,749 people. I however find no significant effect on the immediate surrounding villages, suggesting that new developers mainly generate higher employment *within* the SEZ. This result cannot be explained by a reallocation from more land intensive to more labor intensive industries; instead, it seems to be driven by selection within industries. 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 employment. Moreover, a quick back-of-the-envelope calculation suggests that despite the drop in entry, the contribution of post-reform manufacturing SEZs to aggregate employment is four times higher than that of their pre-reform counterparts.

My paper contributes to the literature in three distinct ways. First, it is the first to empirically analyze the impact of compulsory acquisition on the effects of industrial policy. The existing literature mainly consists of careful case studies, highlighting the negative welfare effects of those who are expropriated, as in e.g. Cernea and Mathur (2007); Gironde and Senties Portilla (2016), or theoretical models describing under which conditions compulsory acquisition could foster economic development (Miceli and Sirmans, 2007; Ghatak and Mookherjee, 2014). The latter can serve as a rationale of the former: compulsory acquisition could be an effective policy tool if the ensuing economic development more than offsets the welfare losses of the landlosers. My paper is the first to empirically study how compulsory acquisition practices actually relate to development, as a first step to evaluate this trade-off. The closest related paper to my study, in its focus on land market interventions and economic development, Blakeslee et al. (2021), found that

a land-rezoning program in Karnataka increased firm entry and employment. Instead, I find that the practice of compulsory acquisition might foster private investment in large-scale industries, but might not be as successful in generating local employment.

Second, as compulsory acquisition is used to overcome barriers to land acquisition, my paper contributes to the literature on land market frictions. There is extensive evidence that these have a negative effect on agricultural productivity and output (e.g. Adamopoulos and Restuccia (2014); Britos et al. (2022); Foster and Rosenzweig (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), who finds that manufacturing firms experience lower productivity and output when land fragmentation is higher, but no effect on services firms. He argues that this is because the land intensity of services is on average five times lower than that of manufacturing. My main contribution is to show how land market frictions also shape selection, further contextualizing the findings that already-established firms perform worse in ill-functioning land markets.

Third, it speaks to the economic impact of SEZs. These are one of the most popular industrial policies in the (developing) world, but as of yet little is known about the exact ingredients that make this policy a success (Juhász et al., 2024).[3] Since all other incentives provided by the Indian SEZ Act remained unchanged with the land reform, my results can also be interpreted as the impact of land provision on the participation in and the employment effects of this industrial policy. This understanding is especially relevant for the Indian SEZ experience as the literature finds mixed evidence on economic activity (Hyun and Ravi, 2018; Görg and Mulyukova, 2022; Gallé et al., 2024), and no effect on development outcomes such as education and infrastructure (Aggarwal, 2007; Alkon, 2018).

The next section discusses the institutional environment for SEZs, their tenuous relationship with land and the protest and subsequent policy changes. 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 provide a conclusion and suggestions for future research.

II. Institutional framework

I. The Indian SEZ Act

Starting with industrial estates and townships from the late forties to establishing the first ever Export Processing Zone (EPZ) in 1965, the Indian Central Government was in some sense ahead

[3] In 2022, the 7,000 SEZs across the globe employed more than 100 million people (Trade and Development, 2022).

of the curve when it came to implementing place-based policies (Levien, 2012). The objective for these EPZs was to manufacture commodities for export to obtain foreign exchange, in exchange for tax breaks and smoother trade procedures. In 2000, inspired by the success of Chinese SEZs in the Guangdong province, the EPZs were converted to SEZs, which could process imports duty-free and did not need any license to import (Hyun and Ravi, 2018). Several states, including "economically backward" states, introduced specific SEZ legislation. Nevertheless, it was not until after the ratification of the Special Economic Zone Act on 23 June 2005 that the popularity and prevalence of SEZs fully materialized (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.[4,5] 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.[6] Notably, the Indian Act differs from most other SEZ policies by allowing both public and private developers to set up a SEZ (Central Government of India, 2005).[7] 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, 2005).[8] The BoA

[4] It is important to note that imports from, or exports to, the Domestic Tariff Area (DTA), or the rest of India excluding other Special Economic Zones, fall under the same regulations. However, SEZs are not allowed to sell goods imported from the DTA *as is*; a developer can thus not propose an SEZ and have it function as nothing more but a warehouse for within-India sales (Central Government of India, 2005).

[5] The exemption from the Minimum Alternate Tax, at 18.5% of book profits, was abolished in 2011.

[6] The available evidence suggests that despite this incentive, FDI in these SEZs has been less than expected. One official overseeing SEZs in nine states stated that most SEZs were financed by domestic loans or internal funds (Levien, 2012).

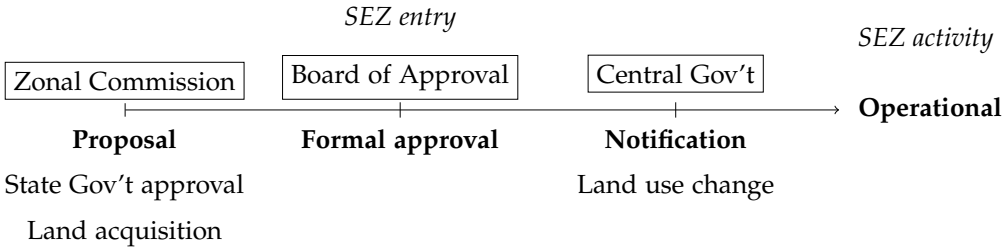
[7] Another notable difference is the minimal size requirement, which varies across industries and is lower than in for example China (Hyun and Ravi, 2018).

[8] The BoA always consists of four high-level officers from the Ministry of Commerce and Industry, the Ministry of

meets multiple times a year to judge whether proposals are of sufficient quality.[9] 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, 2005).[10] 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 SEZ moves to the notification stage, which involves the Central Government changing the land use designation on the SEZ plot to industrial land. The Government does so if it believes that the SEZ will bring about economic development and will be for the greater good (Central Government of India, 2005). 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. The schematic development of a SEZ is given in Figure 1.

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



Within three years after the 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.[11] In total, as of 29 October 2022, the Board of Approval has considered 1,435 proposals, of which more than 700

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, 2005). 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.

[9] 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.

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

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

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. Establishing rightful ownership is further complicated 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.

It is this state of affairs that led the Central and State Governments to worry about private involvement – or rather, the lack thereof – in Special Economic Zones (Levien, 2012). As land is a state subject, the 2005 SEZ Act is silent on the issue of land acquisition for SEZs (Public Accounts Committee, 2018). However, the States could and did 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.[12] 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 – or, as explained before, those in possession of the land to be expropriated – 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).[13] 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, in their role as buyer of land, thereby shield the industrialist from the potential for holdout and the need for individual negotiations

[12] 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 furthermore facilitated the acquisition of private land in 14 villages (Kapoor and Upadhyay).

[13] 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).

(Chakravorty, 2013).

Actual data on the use and intensity of compulsory acquisition is not public, but one indication on states' propensity to engage in land expropriation is reflected in 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. After the 2005 SEZ Act, the West Bengal Industrial Development Corporation and the Salim Group, a private firm, proposed to set up a chemical SEZ in Nandigram, close to Haldia port.[14] This proposal was accompanied by a notification of land acquisition for 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 decided to intervene 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).

Protests against large-scale land acquisition are not uncommon, but this violent repression was exceptional.[15] Consequently, the SEZ at Nandigram was cancelled; the West Bengal Industrial Development Corporation announced it would move the SEZ to Nayachar, an empty strip of land also close to Haldia. Moreover, the Central Government instated an Empowered Group of Ministers (EGoM) to revisit the SEZ policy (Singala et al., 2011). 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 facilitate land acquisition for industrial development in private SEZs (Public Accounts Committee, 2018). It furthermore promised to revise the rules on land acquisition and resettlement and rehabilitation more formally by passing new acts. A first step was the National Policy on Rehabilitation and

[14] Haldia is one of India's major ports, increasingly taking over traffic from Kolkata as Haldia is more easily accessible for ships.

[15] Another notable protest in 2007 was against the Tata Nano car factory in Singur, also in West Bengal (Pal et al., 2022).

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).[16]

As mentioned before, state governments had a larger set of strategies at their disposal to facilitate land for private SEZs, although these are 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, and one commended by the then-minister of Commerce, is for SEZ developers to draw upon land banks. These land banks manage and distribute land plots for industrial development; however, these are 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

This section first describes how land fragmentation and other frictions affect land acquisition and thereby entry costs. Then, I will elaborate on the effect of the compulsory acquisition reform on land acquisition costs, and finally the implications for both SEZ entry and their effects on local employment.

Land assembly models such as Miceli (2011) show how land fragmentation 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).[17] Besides this risk, land fragmentation generally implies larger bargaining costs, as there

[16] The aforementioned process eventually culminated in two new bills, introduced in Lok Sabha (Lower House) on 6 December 2007. The Resettlement and Rehabilitation Bill was a formalization of the existing National Policy, while the Land Acquisition (Amendment) Bill most notably redefines "public purpose" beyond strategic or military provisions and infrastructure investments. Specifically, the provision of land for any other project under the umbrella of public purchases is limited to thirty percent of the total area of land necessary, and conditional on the other seventy percent having been legally acquired by the developer (Ministry of Rural Development, 2007). The Land Acquisition (Amendment) Bill was passed in Lok Sabha on 25 February 2009, but both bills lapsed with the dissolution of the parliament on 1 June 2009. It was not until 2011 when both bills were introduced in the combined Land Acquisition, Resettlement and Rehabilition Bill, and finally passed in 2013.

[17] Empirical studies of speculative hold-out, such as 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 how the

are many sellers to negotiate with. In the face of land market frictions, the land acquisition costs 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 enter for SEZ developers, I follow Sood (2022), who in her study of land expansion by manufacturing firms, modelled the land acquisition cost of project i in state s as[18]:

$$LAC_{is} = \xi_s + m_i^{1+\gamma_s}, \tag{1}$$

where m_i is the total cost, or price times acreage, of the land. Then, ξ_s captures the fixed costs associated with land acquisition such as collecting land records and designating a suitable location. Finally, $\gamma_s \geq 0$ governs the convexity of the cost function, capturing the effect of land fragmentation on the risk of holdout and increased bargaining costs.

Consider first the land acquisition costs in states with compulsory acquisition policies before the reform. First, Sood (2022) found that the fixed costs of land acquisition are significantly lower for public firms compared to private firms, such that $\xi_s^{CA} < \xi_s$, and that γ_s was higher in states with more land fragmentation. Further, since compulsory acquisition by definition precludes any bargaining and therefore the holdout problem, we can assume that for any state the cost function is less convex for compulsory than for private acquisition, or $\gamma_s^{CA} < \gamma_s$. 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.[19,20] We would not just expect SEZs in compulsory acquisition states to be larger on average; in terms of sectoral composition, we would expect more large-scale industries in these states. Consider a simple Cobb Douglas production function over land N_{it}, labor L_{it} and capital $K_{it}$with constant returns to scale:

$$Y_i = K_i^{\alpha_1} L_i^{\alpha_2} N_i^{\alpha_3}, \quad \alpha_1 + \alpha_2 + \alpha_3 = 1,$$

government and the court determine market value; as mentioned before, the compensation for compulsory acquisition is based on the circle rate, while the court uses the land rate based on local land transactions in the previous five years. Kitchens (2014) investigated the prevalence of the speculative hold out problem under eminent domain in Tennessee (US) in the 1930s, finding that those that held out and went to court obtained on average about five percent higher compensations.

[18] Strictly speaking, her definition of the cost of land expansion also contains a land friction-induced wedge between the total price of the land and the land cost. However, this wedge parameter and γ_s cannot be estimated jointly; to facilitate interpretation, I assumed any non-price land frictions wedge is collapsed into the convexity parameter.

[19] 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). This is also reflected in the Indian SEZ experience: of those SEZs in states without compulsory acquisition, a substantial share is located in states that are more developed and otherwise industrially progressive (Tewari, 2020). This is expanded upon in the following section.

[20] 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 2013.

from which it can be seen immediately that the larger α_3, or the more land intensive an industry is, the more compulsory acquisition will facilitate entry into that industry.

After the 2007 reform, state-led compulsory acquisition was prohibited, increasing the bargaining costs and the risk of holdout. In the context of this framework, this implies an increase in γ_s for those states engaging in compulsory acquisition. Thus, land acquisition costs increased across the board, but especially for projects requiring larger plots. 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 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 firms are relatively more constrained by land than services firms, the increase in entry costs after the reform will be relatively higher for manufacturing firms, such that there will be relatively fewer manufacturing SEZ proposals. More generally, there will be a reallocation from more land-intensive to less land-intensive industries.

Besides realloction *across* different sectors, the increase in entry costs can also affect selection *within* industries. If private developers are heterogeneous in ability, for example in their ability of bringing about agglomeration economies, studying changes in SEZ entry decisions is not sufficient to understand the impact of land acquisition costs on industrialisation. Stationary models of entry, exit and firm dynamics such as Hopenhayn (1992) and later Melitz (2003) offer insight on firm, or here SEZ, entry when productivity is heterogeneous and uncertain. In these models, it can be shown that the marginal entrant is the one for which the present discounted profits, as a function of their productivity, equals the entry costs. This means that an increase in entry costs acts as a higher barrier to entry, protecting incumbents and increasing selection (Hopenhayn, 1992). 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. As I do not have information on the quality of the developers or their proposals beyond what is already described, I will proxy for this by studying employment around the SEZ. While a higher productivity of an SEZ developer might not directly translate into higher employment in the SEZ itself, it can generate higher demand for local services or local inputs.

In short, I formulate the following hypotheses. First, the share of manufacturing proposals decreases after the reform relative to services proposals in those states with compulsory acquisition policies, reflecting how the entry costs have increased more for manufacturing industries. Second,

I predict that this reduction, especially right after the protest, persists in the following development stages. Finally, to assess how the shock impacted the effectiveness of the industrial policy, I predict that SEZs after the reform generate more local employment.

IV. Data and descriptives

I. Proposal data

The main dataset contains the universe of official Indian SEZ proposals. My principal data source are the BoA Meeting Minutes, which are scanned documents publicly available from SEZ India (2022b). From there, I scraped the minutes for the 112 BoA meetings between 17 March 2006 and 29 October 2022. After collecting the meeting minutes, I 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. The number of proposals exceeds the number of SEZs as some proposals were initially deferred and later resubmitted. If an SEZ proposal did not provide a disaggregated enough location, I used 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 II.i of the Appendix.

It is important to note that while I do know exactly when each 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.

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

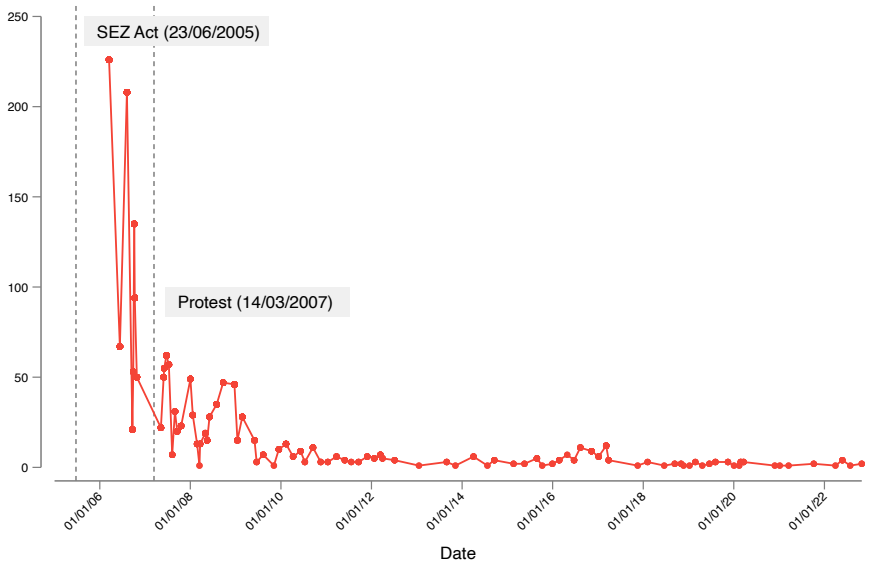


Figure 2: *Number of SEZ proposals per meeting*

village or town.[21] Using their rich open source data, I can then add economic and socio-economic variables 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 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 control for local land fragmentation, 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.[22] A subdistrict is the third-level administrative boundary in India; India consists of more than 6,000 subdistricts. Table 1 summarizes all variables in this dataset at the proposal-meeting level. The average SEZ is 308.7 hectares, or approximately 1.8 by 1.8 kilometers, and 12 percent of SEZs

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

[22] 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. More specifically, consider two plots of land; one shared equally between two landowners and the second divided equally over a thousand landowners. These plots are associated with the same Gini coefficient, but a different Theil index, thereby allowing me to capture land fragmentation as well as inequality. Results based on the Gini coefficient are quantitatively similar and available upon request. Section II.ii in the Appendix provides more information on this measure, including Figure 16 that shows the country-wide variation of this index.

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, and concerned with power generation or oil and gas.

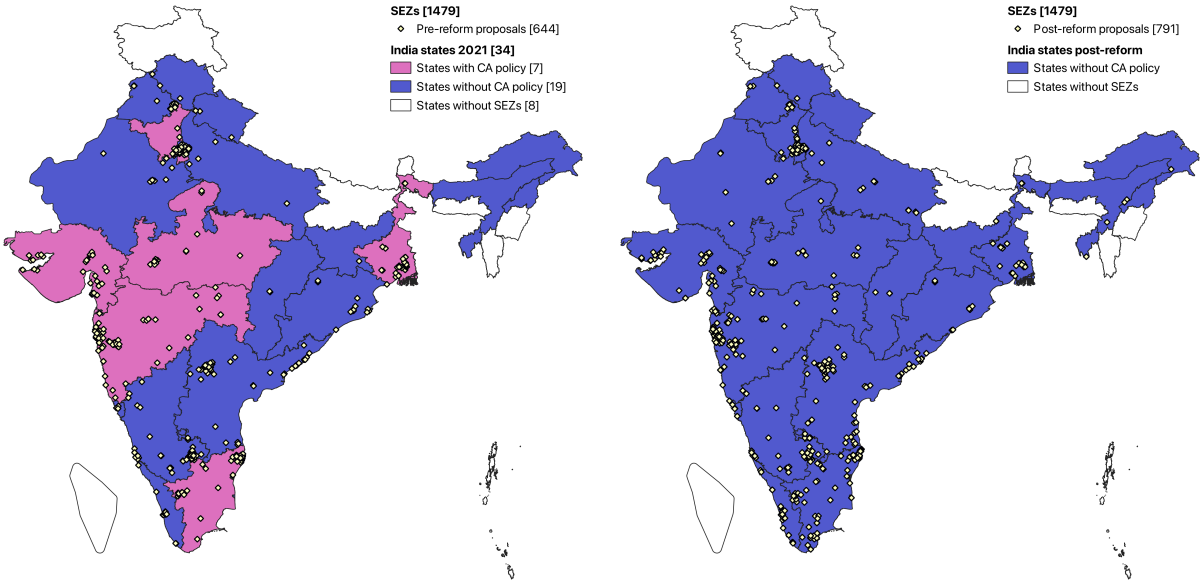
Table 1: *Characteristics of SEZ proposals*

	N	Mean	Median	SD
SEZ Characteristics				
SEZ size (ha.)	1441	308.7	37.5	1226.5
Public developer	1445	0.12	0	0.32
Manufacturing SEZ (share)	1444	0.30	0	0.46
Services SEZ (share)	1444	0.68	1	0.47
Utilities SEZ (share)	1444	0.021	0	0.14
Location Characteristics				
Population (2001)	1446	1442781.9	430856	2603509.9
Labor 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. SEZ characteristics are obtained from the proposal dataset; 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; manufacturing and services 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.

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 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."[23] Figure 3 shows the spatial distribution of SEZ proposals across states by their commitment to eminent domain, before and after the reform.[24] 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).



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

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

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

[23] Table 13 in the Appendix shows all relevant clauses that inform the treatment assignment.

[24] 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.

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 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 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 11 in Section II 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 second set of hypotheses concerns a change in the local labor market impact of SEZs after the reform. 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. Because the last available wave of the EC is in 2013, I have to restrict my SEZ sample to the SEZs that became operational beforehand. 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.[25] 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. After removing the 19 SEZs that became operational before the introduction of 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 confirmed 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. There are 70 municipalities that host multiple SEZs;

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

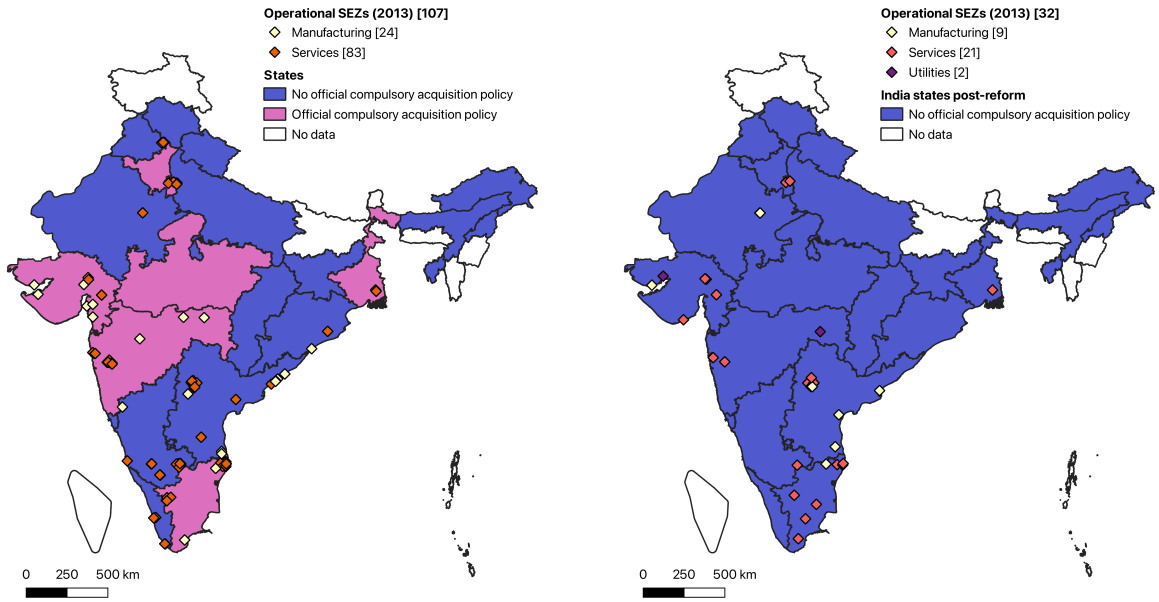
Table 2: *Characteristics of pre-reform proposals by state's compulsory acquisition policy*

	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						
Log population (2001)	13.066	(1.354)	13.522	(1.250)	0.456	(0.466)
Log labor force (2001)	12.083	(1.305)	12.527	(1.199)	0.443	(0.476)
Log agricultural employment (2001)	9.982	(0.949)	10.314	(0.763)	0.332	(0.358)
Log manufacturing employment (2005)	9.652	(1.853)	10.084	(1.684)	0.432	(0.617)
Log services employment (2005)	10.663	(1.712)	11.056	(1.620)	0.393	(0.526)
Log distance to airport (km)	2.897	(0.926)	3.052	(0.826)	0.155	(0.174)
Log distance to port (km)	5.240	(1.139)	4.475	(1.364)	-0.765*	(0.414)
Log distance to power plant (km)	2.458	(0.793)	2.618	(0.804)	0.160	(0.136)
Log distance to city (>500K, km)	3.219	(0.959)	3.114	(0.909)	-0.106	(0.151)
Log distance to highway (km)	0.791	(0.578)	0.668	(0.488)	-0.123	(0.126)
Log 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)
Log 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 Primary Census Abstract; manufacturing and services employment from the 2005 Economic Census, 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.

these are mainly large metropolitan areas, such as the urban center of Bangalore.[26] 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),

[26] Note that these municipalities are both special in terms of their economic and actual size; the Bangalore metropolis occupies an area of 741 kilometers.



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

Figure 4: *Operational SEZs up to 2013*

the Population Census Abstract (2001) and the land fragmentation 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 to virtually all amenities listed, and are characterised by higher nonagricultural employment and access to banks.

V. IMPACT ON SEZ DEVELOPMENT

I. Empirical strategy

I estimate variants of the following equation to identify the impact of the compulsory acquisition reform on SEZ entry decisions regarding sectoral choice or size:

$$Y_{it} = \beta Post_{it} \cdot CA_{s(i)} + \alpha_{r(i)} + \alpha_t + \epsilon_{it}, \tag{2}$$

where Y_{it} refers to the characteristic of a proposal for SEZ i discussed at a meeting at date t. To understand how the reform affected the passthrough of proposals, I repeat this exercise with the probability of an SEZ achieving formal approval, notification or operation as an outcome variable. $Post_{it}$ is a dummy designating whether the proposal whether the proposal i was discussed before

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 density	1.610	1.579	1.409	1.338	1.284	1.285	1.212	1.197	1.213	1.220	1.172	0.325
	(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 employment	5.661	5.343	5.290	5.394	5.477	5.538	5.482	5.421	5.435	5.360	5.397	0.123
(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 employment	4.934	4.481	4.206	4.117	4.039	4.077	3.973	3.931	3.923	3.832	3.794	0.858***
	(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 (Theil)	0.498	0.490	0.486	0.485	0.469	0.456	0.461	0.469	0.451	0.442	0.438	0.042*
	(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 airport (km)	3.082	3.165	3.276	3.393	3.527	3.622	3.698	3.803	3.894	3.967	4.027	-0.540***
	(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 port (km)	4.546	4.653	4.683	4.718	4.761	4.858	4.952	4.969	4.984	4.959	5.026	-0.312*
	(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 city (>500K,	3.595	3.553	3.607	3.724	3.828	3.872	3.923	3.983	4.083	4.143	4.187	-0.278**
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 highway (km)	0.852	1.012	1.126	1.163	1.190	1.206	1.232	1.268	1.274	1.277	1.339	-0.353***
	(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 railway (km)	1.598	1.627	1.817	1.913	1.978	2.057	2.157	2.248	2.350	2.370	2.465	-0.459***
	(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 power plant (km)	2.362	2.596	2.718	2.810	2.869	2.932	3.049	3.129	3.202	3.267	3.288	-0.570***
	(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.

or after the protest and $CA_{s(i)}$ equals 1 if the proposed SEZ is located in a state s which ex ante committed to providing land to SEZ developers. I also include meeting fixed effects α_t and economic region fixed effects $\alpha_{r(i)}$.[27] India has 78 economic regions, which partition states into 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; results with state fixed effects are quantitatively similar and are available on request. Finally, ϵ_{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 β: 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; Section IV in the Appendix contains robustness checks using a logit model instead.

Identification concerns 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 concern, as with shift-share designs in general, is that ϵ_{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 *sectoral composition* of SEZs proposed to locate in that state around the time of 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. Table 14 in the Appendix shows how the propensity of a state to introduce compulsory acquisition policy

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

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.

To account for these 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. More broadly, states 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. 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 fragmentation, 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 nonrandom. This leads to the main estimating equation in this paper:

$$Y_{it} = \beta Post_{it} \cdot CA_{s(i)} + \sum_{n \in N} t \cdot X_{sd(i),2005} + \alpha_{r(i)} + \alpha_t + \epsilon_{it}, \tag{3}$$

where $\sum_{n \in N} t \cdot X_{sd(i),2005}$ includes a linear trend at the subdistrict level across meeting dates for all controls described above.[28] I use this to verify the parallel trends assumption for entry into manufacturing and services before the reform. In Figure 5 I plot, for both manufacturing and services, the coefficient per quarter relative to the baseline meeting just before the protest. This regression includes the aforementioned controls and region fixed effects, and the standard errors are clustered at the state level. The mass of proposals before the protest makes it very difficult to properly test for pretrends; estimating this at the quarter level is a balance between sufficient time variation and mitigating bias from the large changes in number of proposals per meeting. The figure shows that the pretrends are reasonably similar for manufacturing and services SEZs. Moreover, while the coefficients generally have the expected sign, they jump around. Since I cannot control for year fixed effects in this specification, this might again point to time-varying heterogeneity. In the Appendix, I will show the results are robust to different windows around the

[28] 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; these results are available on request. I also test how robust the results are to other strategies of including control variables, more on that below.

timing of the protest.

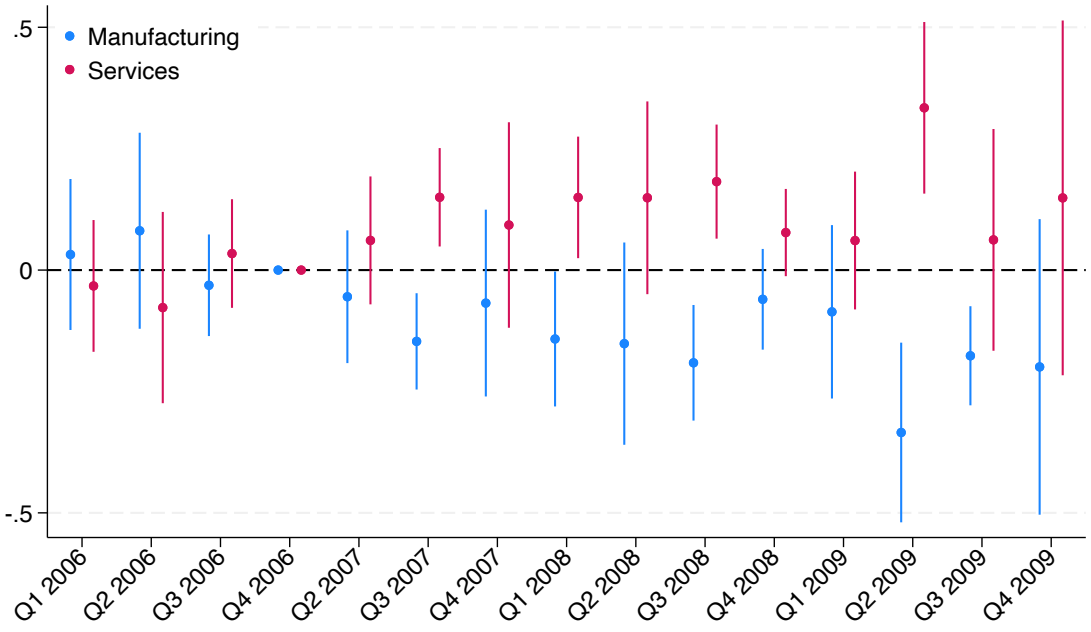


Figure 5: *Proposed sectoral composition SEZs over time*

There are several other potential threats to identification, which I now discuss in turn. For example, the reform could have instigated a dramatic change in the Board of Approval's strategy, such that the observed effect is coming from the BoA treating proposals differently based on whether the state used eminent domain for SEZs. The fact that the Board is appointed by the Central Government and that the members are there *ex officio* means there is no change in board composition that could induce a different strategy. 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 can start favoring certain states after the reform. Alternatively, the effect I find might not be driven by the reform, but instead an increase in uncertainty for specific industries, as the protesters also lamented the expected pollution from the Nandigram SEZ (Levien, 2012); I verify whether the effect is driven by polluting SEZs in the robustness checks. I also directly estimate the impact of the reform on the probability of any proposal reaching the next stage, and find no significant change for both the total sample of proposals and manufacturing proposals only.

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. In defense of the first point I draw on Levien (2012), who did extensive work on understanding why SEZs and land have such a tenuous relationship. His interviews with officials at the Indian Chamber of Commerce (ASSOCHAM), industry consultants, high-level state bureaucrats, and industrial development corporation officials in Gujarat and West Bengal documented that these governments rely on land provision to attract large investments to their state. The second concern is more difficult to assuage – it should however be noted that this kind of misclassification would bias the estimate towards zero, so that the coefficient should be seen as a lower bound on the actual effect of the reform. In a robustness check, I verify this directly by comparing the effect of the reform on the states with compulsory acquisition policy to two states that explicitly prohibited land provision for private SEZs: Uttar Pradesh and Kerala.[29]

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 for new SEZs. As mentioned before, comparing the left-hand and right-hand figure in Figure 3 provide suggestive evidence that location choice of SEZ developers seems relatively similar before and after the reform. To test more directly whether SEZs after the reform are proposed in different places to avoid competing with existing SEZs, I employ two different strategies. First, I directly control for the number of (manufacturing) SEZs that are notified at a state-year level in the robustness section; I would expect this to capture SEZ competition at the state level as notification means that developers are allowed to start construction on the zone. Second, I check whether the estimated effect is significantly different in locations close to the port, which are both especially desirable for manufacturing and especially limited in terms of land availability. This allows me to test whether compulsory acquisition states simply exhausted the available land close to the port, and that this disincentivizes manufacturing developers from entry in treated states.

[29] 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; instead, they will have to acquire the land on their own. Kerala confirmed their stance against land provision for private SEZ developers in their 2008 SEZ policy.

II. Results

II.i. Main results

In this section, I provide evidence that the reform affected entry of SEZ developers as well as characteristics of their proposals. First, Table 4 displays the results of estimating Equation 2 and 3 with the dependent variable indicating whether the proposed SEZ is in manufacturing. Because only two percent of all proposals are for Utilities sectors, I exclude all these proposals from my sample, such that services sectors are the reference category.[30] 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.[31] 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.

This raises the question of how this affects the sectoral composition of proposals at later development stages. Critical here is to isolate any impacts of the reform on entry decisions from a potential change in Board of Approval strategy; Table 15 in Section IV in the Appendix shows that the probability of a proposal moving to the next stage of SEZ development is not significantly affected by the shock, both for proposals in general and manufacturing specifically. 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.[32] First, columns 3 and 4 display the result of estimating Equation 2 and 3 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 suggest that this shift in sectoral composition persists until the operations stage.

If the treatment captures a change in land acquisition costs, and specifically a change in the convexity of the cost function, one would expect SEZs to become smaller on average. Estimating

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

[31] Figure 18 in Section IV 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.

[32] The rationale for this is that resubmitting a pre-reform proposal after the reform is arguably also treated, since the developer could decide to not resubmit the proposal after the circumstances changed so drastically. Including all proposals does reduce the effect size slightly but otherwise does not change the results; these results are available upon request.

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 Primary Census Abstract, and data on log nonagricultural employment, also at the subdistrict level, from the 2005 Economic Census. 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. Standard errors, clustered at the state level, are in parentheses.

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

Equation 3 with the log proposed SEZ size as the outcome variable, Table 5 shows how size changed after the reform in compulsory acquisition states compared to other SEZ-hosting states, In column 1 and 2, I consider the full sample of proposals: column 1 shows that proposed SEZs on average are about 17.1% smaller, for manufacturing SEZs this is more than doubled at 56.5%. SEZs proposed after the reform that are formally approved are also smaller, at around 32.4%, while the size of formally approved manufacturing proposals tends to be slightly larger. The third group of estimates follows from the proposals for SEZs that have been notified, where, again, those SEZs proposed after the protest seem slightly smaller. Column 6 provides qualitative evidence that notified manufacturing SEZs proposed after the reform tend to be larger than their older counterparts, albeit not significantly so. This direction is confirmed in column 7, which shows that operational SEZs proposed after the reform are on average 70.4% larger; I do not have sufficient degrees of freedom to estimate the change in size for operational manufacturing zones proposed after the reform. Thus, while there is a tendency for all, and manufacturing, SEZ proposals to shrink after the reform, those that become operational tend to be larger.

Table 5: *Manufacturing SEZs that develop further tend to be larger*

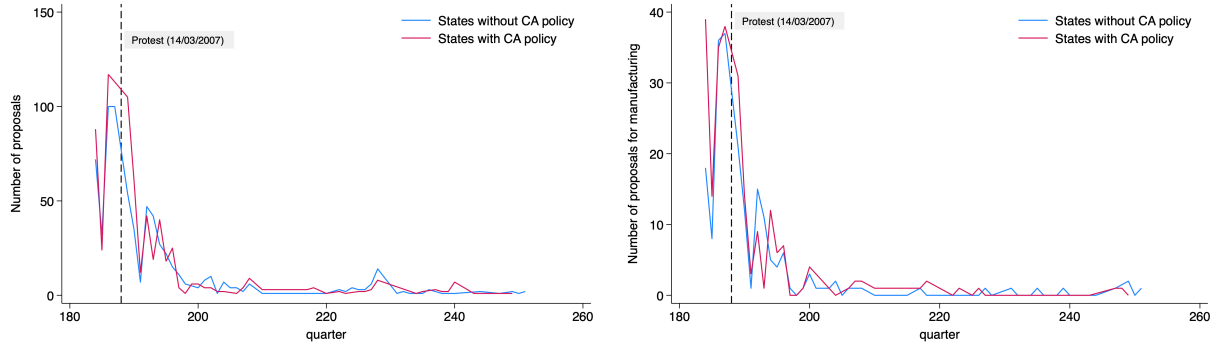
	All proposals		Formal approval		Notification		Operational
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Man.	All	Man.	All	Man.	All
After protest	-0.171	-0.565*	-0.324	0.0929	-0.297	0.0762	0.704*
× State CA	(0.216)	(0.272)	(0.197)	(0.675)	(0.300)	(0.702)	(0.340)
Location trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Meeting FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1217	328	608	96	390	55	184
R-squared	0.488	0.447	0.546	0.735	0.605	0.821	0.780

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 included location trends. Standard errors, clustered at the state level, are in parentheses.
$^{*}p < 0.1$,** $p < 0.05$,*** $p < 0.01$.

Absolute change in sectoral composition 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. To do so, I analyse how the number of proposals in every state in each quarter evolves differently for treated and control states after the reform, extending Equation 2 with the same controls, this time at the state level, 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 first three columns of Table 6 show the results of this estimation for the total number of proposals. The first column, including state-level controls but no fixed effects shows clearly that there is indeed a significant decrease in the number of proposals after the reform. Moreover, this decrease is similar for states with and without compulsory acquisition, suggesting that this mechanical effect is indeed similar for treated and untreated states.[33] The preferred specification in column 3 implies that on average, the number of proposals in treated states decreases by around 3, although this is not significantly different from zero. The second set

[33] More formally, the p-value on the F-test that the sum of the coefficient on CA and the interaction variable equals zero is 0.6449.

of columns repeats the analysis for the number of manufacturing proposals. Column 4 highlights that the number of proposals decreases after the reform, and especially for treated states: the p-value on the F-test comparing the total effect on states with and without compulsory acquisition is 0.0021. This is confirmed in the last column with state and year fixed effects: on average, the number of manufacturing proposals is almost 2 lower in compulsory acquisition states after the reform – this is borderline significant at the 10.9% level. In conclusion, compulsory acquisition states see both relatively and actually fewer manufacturing SEZs.



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

Figure 6: *Total number of proposals by state compulsory acquisition policy*

II.ii. Mechanism

The above results are consistent with the reform leading to a relative increase in land acquisition costs in states that engage in compulsory acquisition; in this subsection, I will present more direct evidence for this channel. First, I analyse whether investment in SEZs indeed decreased relatively more in areas with higher land fragmentation. To that end, I extend Equation 3 by interacting the treatment variable with the local land concentration in the subdistrict of the proposed SEZ as the outcome variable, resulting in:

$$\begin{aligned} Y_{it} =& \beta_1 Post_{it} \cdot CA_{s(i)} \cdot T_{sd(i)} + \beta_2 Post_{it} \cdot T_{sd(i)} + \beta_3 Post_{it} \cdot CA_{s(i)} + \beta_5 CA_{s(i)} \cdot T_{sd(i)} \\ &+ \beta_6 T_{sd(i)} + \sum_{n \in N} t \cdot X_{sd(i),2005} + \alpha_{s(i)} + \alpha_t + \epsilon_{it}, \end{aligned} \tag{4}$$

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. Figure 7 plots the marginal effects of this regression for states with compulsory acquisition policy, comparing the impact of the reform on (a) the probability of a proposal being

Table 6: *The number of manufacturing proposals decreases after the reform*

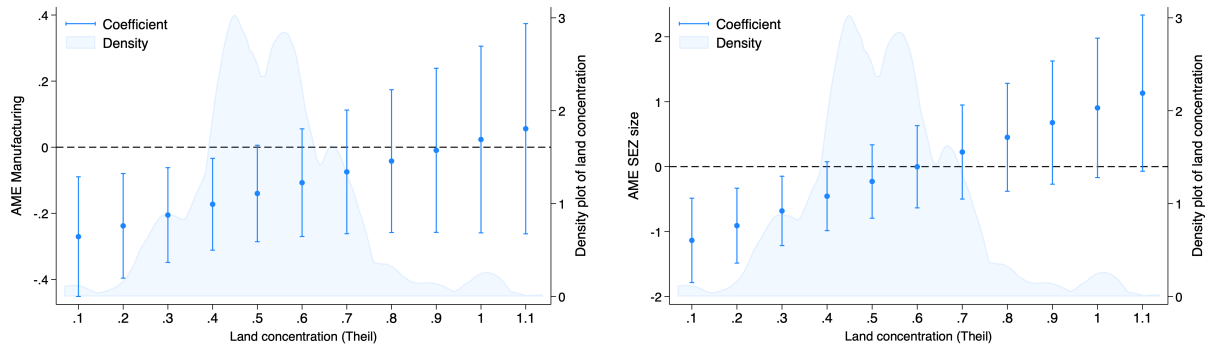
	Total proposals			Total manufacturing proposals		
	(1)	(2)	(3)	(4)	(5)	(6)
After protest	-6.694**			-2.226***		
	(2.974)			(0.753)		
State CA	5.894	5.197		3.557**	3.349**	
	(4.766)	(4.554)		(1.414)	(1.378)	
After protest	-5.480	-4.816	-3.271	-2.817*	-2.648*	-1.965
× State CA	(4.547)	(4.188)	(3.224)	(1.458)	(1.398)	(1.174)
Controls	Yes	Yes	No	Yes	Yes	No
Year FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Observations	240	240	293	240	240	293
R-squared	0.271	0.402	0.432	0.281	0.355	0.365

The dependent variable is the number of proposals in each quarter in each state, either for all sectors or only manufacturing. The regression includes either state-level fixed effects or state-level controls. The controls include log state size, state-level land concentration, the log state population, labor force and agricultural employment in 2001, log nonagricultural employment in 2005 and log number of banks in 2005. Standard errors, clustered at the state level, are in parentheses.
$^{*}p < 0.1$,** $p < 0.05$,*** $p < 0.01$.

in manufacturing or (b) the proposed SEZ size across the distribution of local land concentration, depicted by the light blue area. Two striking patterns emerge. First, the effect of the reform in compulsory acquisition states is largest in areas with lower land concentration, and is significantly different from zero for areas with a Theil index below 0.5. To put this into perspective: the average (median) land concentration is 0.59 (0.53). In Section IV in the Appendix I verify that this result is not driven by the assumption of this mediating effect being linear (Table 16) and directly reflected in SEZ developers' preferences, as they are significantly more likely to propose an SEZ in an area with higher land concentration (Table 17).

The second insight is that the insignificant coefficient on log SEZ size obscures substantial heterogeneity across areas with different land concentration. Specifically, the log average SEZ size is significantly smaller in subdistricts with a Theil index below 0.4, and almost significantly higher in areas with relatively high land concentration. This is consistent with the previous results, as manufacturing SEZs tend to be larger; 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. 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 can be interpreted as SEZ developers trying to mitigate the bargaining costs now that compulsory acquisition can no longer be used.



(a) *Effect on Manufacturing* **(b)** *Effect on log proposed size*

Figure 7: *Average marginal effects on SEZ proposal characteristics by local Theil index*

A second supporting fact derives from industrial variation within the broad sectors of manufacturing and 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; this can be seen more clearly in Table 10 in Section II of the Appendix. Thus, if the reform represents an increase in land acquisition costs, this would most affect the more land-intensive industries beyond sectoral designation. I proxy land intensity for each industry with its pre-reform median SEZ size as displayed in Table 10. In Table 7, I estimate Equation 3 with industry size rank as the outcome. This is a categorical variable which ranks all industries on median plot size before the reform from small to large. Thus, a positive coefficient on the treatment variable suggests that SEZs in larger-scale industries are more likely to be proposed after the reform. In column (1), only region and date fixed effects are included; the coefficient indicates that on average, the proposed SEZ industries after the reform are significantly smaller in terms of rank, with on average a drop in rank of around two. Adding location controls as in column (2) or location trends, in column (3), does not affect the results dramatically. I verify in Section IV of the Appendix (Table 19) that this result is robust to a nonlinear estimation. The last three columns instead take the pre-reform industry median SEZ size as a dependent variable. Again, I find that SEZs after the reform are more likely to be in smaller-scale industries than before the reform. Thus, the shift in sectoral composition is not just between the broad sectors manufacturing

and services, but also within these sectors there is a reallocation from relatively large-scale to relatively smaller-scale industries.

Table 7: *Post-reform, large-scale industries are less likely*

	Industry rank			Median industry size		
	(1)	(2)	(3)	(4)	(5)	(6)
After protest	-2.050**	-1.640**	-1.708**	-78.78**	-68.58**	-73.15**
× State CA	(0.817)	(0.621)	(0.596)	(34.98)	(31.98)	(31.17)
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	0.285	0.282

In the first three columns, the dependent variable is a categorical variable indicating the proposed industry, ordered from low to high based on the median SEZ size before the reform. 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.

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

II.iii. Robustness

My results show that the compulsory acquisition reform reduced the share of manufacturing SEZs in states that engaged in compulsory acquisition. I further provide evidence that this can be explained by increased land acquisition costs, as the effects were strongest for more land-intensive industries and areas with higher land fragmentation. A primary concern is that this effect is driven by certain states or industries, rather than a consistent pattern across states with and without compulsory acquisition policy. In Figure 8, I rerun the baseline equation and display the coefficient on the treatment variable with several such sample restrictions and show they do not yield significantly different effect sizes. For convenience, it includes the baseline coefficient on the treatment interaction from column 1 in Table 4. The first set of robustness checks excludes specific states from the sample. First, I exclude states that faced especially many protests, to ensure that it is not increased stigma on SEZs that drags the coefficient of interest down. The first coefficient shows the result when excluding all SEZs proposed in West Bengal; besides being one of the states engaging in compulsory acquisition and sees large protests against this practice, it also

has the highest land fragmentation in India (Sarkar, 2007).[34] I also exclude Goa, where protests against land grabs pushed the Goa State Government to eventually abolish all SEZ projects in 2009.[35] To see whether the effect is driven by economic prowess in non-compulsory acquisition states, I exclude Andhra Pradesh as one of the most popular states for SEZ developers.[36] Finally, I check whether this coefficient simply reflects the difference between states that actively engage in compulsory acquisition and those that prohibit it. Specifically, I first exclude Kerala and Uttar Pradesh, which explicitly prohibited compulsory acquisition for private SEZs in 2008 and 2007 respectively. Then, I rerun the analysis excluding all *other* control states and compare the share of manufacturing proposals after the reform in treated states and Kerala and Uttar Pradesh. I obtain a similar coefficient to the baseline – albeit less significant as the sample size is almost halved. Figure 19 in Section IV of the Appendix further shows that this main result is not driven by any specific compulsory acquisition state either. In conclusion, the estimates are robust to excluding these states from the sample: the large negative effect cannot be explained by states that were forced to take a step back, a large substitution from compulsory acquisition states to other economically attractive states, or by states where compulsory acquisition was prohibited just before and after the nation-wide reform.

As mentioned before, a second concern is that my effect size does not capture the effect of the protest and subsequent reform on land acquisition costs, but simply signifies a pivot away from more polluting SEZs, as the environmental impact of an SEZ is often, including in the case of Nandigram, a motivating concern for protesters (Land Matrix, 2023). The third set of robustness checks in Figure 8 shows how the coefficient changes upon excluding certain industries – see also Figure 15 in the Appendix for information on how prominent these industries are. Whether I exclude proposals for chemical, petroleum or mining SEZs, or even all these polluting industries from the sample, I still obtain a significant negative coefficient.

Third, the estimated negative effect of the reform on the entry of manufacturing SEZs might also point to the number of potential SEZs being exhausted already. More specifically: if before the reform, compulsory acquisition states have successfully incentivized manufacturing SEZs to be

[34] Additionally, the ruling party in the State Government, lost the 2011 election to a party that was vehemently against land grabs for industrialisation (Patra, 2019). This election ended a 34 year reign of the Left Front; Trinamool Congress became the new ruling party platforming on "Maa, Maati, Manush" or "Mother, Land, People". Indeed, after this party came into office, only 2 SEZ proposals were submitted in West Bengal.

[35] Starting in 2007, citizens felt that its nine SEZ projects were a land grab and not appropriate investments for the Goan government, especially since Goa is one of the smallest Indian states. After continued protests, the Goan State Government ultimately retracted the SEZ policy in 2009 and all SEZ projects were denotified.

[36] In my sample, Andhra Pradesh and Telangana, which split from Andhra Pradesh in 2014, received proposals for 172 distinct SEZs.

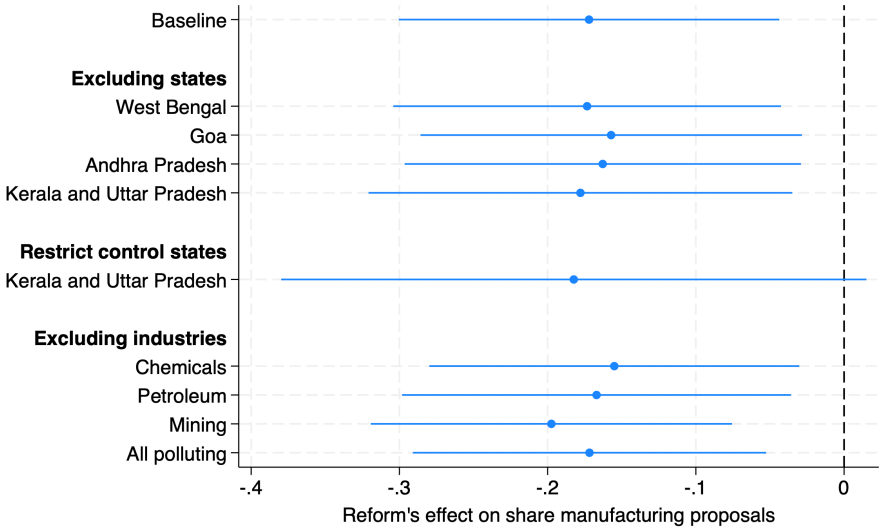


Figure 8: *Robustness checks: sample restrictions*

set up in their state, these states will be less attractive for new manufacturing SEZ developers; be it because of exhaustion of production factors such as land or labor or fiercer competition with other SEZs. To test whether the effect of the land acquisition is driven by SEZ competition, I include the yearly total of notified manufacturing SEZs in a state as a control.[37] These results are presented in Table 8, and show that even conditional on state-level manufacturing SEZ presence, the reform has substantial effects on the industrial composition of proposed and operational SEZs. Figure 21 in Section IV of the Appendix further shows that the results is not driven by a location preference for being close to the port.

Finally, 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 Table 9. In the first column, I classify any proposal discussed after August 9, 2006, or the third meeting out of eight before the protest, as treated; this corresponds to about half of the pre-protest sample. The second column assigns a placebo protest after the fourth meeting on 24 September 2006, or at the halfway point of the pre-protest meetings. In both cases, also without controlling for local characteristics or trends, I obtain a small insignificant effect on the share of manufacturing. Column 3 excludes the 164 proposals from the first meeting, which yields a slightly smaller but significant (at the 5.5% level) coefficient. Finally, column 4 excludes the first and the second meeting or the first 221 proposals, resulting in

[37] Results are robust to including all notified SEZs as a control instead.

Table 8: *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
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

The dependent variable is a dummy that equals one if the proposed SEZ is in *Manufacturing*. See the notes under Table 4 for details on the sample specification and the included location trends; this analysis also controls for the number of state-level notified manufacturing SEZs per year. Standard errors, clustered at the state level, are in parentheses.

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

qualitatively similar results, although the coefficient is smaller and not significant. Moreover, in Figure 20, I show that the estimated effect is robust to excluding later years in the sample: the result is thus not driven by later changes in land or SEZ policy.

VI. Impact on local employment

I. Empirical strategy

In the second analysis, I aim to understand whether the change in SEZ entry decisions after the eminent domain reform also impacted SEZs' effects on local employment. I use two rounds of the Economic Census (2005-2013), which contains the universe of firms, and is therefore especially suited to analyze both formal and informal employment in and around SEZs. As mentioned before, I only consider villages within 50 kilometers of an SEZ that host at most one SEZ; descriptive statistics for the full baseline sample are provided in Section iii of the Appendix. Another important note is that due to data restrictions, I can only consider the 139 SEZs that were operational before 2013. 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). For example, the new firms entering the

Table 9: *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	-0.0244	-0.0250		
× State CA	(0.0959)	(0.139)		
After protest			-0.128*	-0.0807
× State CA			(0.0627)	(0.0560)
Location trends	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Meeting FE	Yes	Yes	Yes	Yes
Observations	543	543	1096	1049
R-squared	0.411	0.411	0.333	0.341

The dependent variable is a dummy that equals one if the SEZ is for *Manufacturing*. 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 study the robustness of the result to excluding either the first meeting (164 proposals) or the first and second meeting (221 proposals). See the notes under Table 4 for details on included location trends. Standard errors, clustered at the state level, are in parentheses.
$^{*}p < 0.1$,** $p < 0.05$,*** $p < 0.01$.

SEZ might contribute to an increase in local demand for services, generating new employment in villages around the SEZ. In this case, β will be biased downward, as the control group of nearby villages is also treated, albeit to a lesser extent. Another possibility, which is more relevant for place-based policies in developed economies, is that SEZs start competing with nearby firms, thereby inducing employment to relocate to these SEZ villages. Now β will be biased upward, as the nearby villages experience a negative local labor market shock upon realisation of the SEZ.

In such a setting, one can recover the total treatment effect using the 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 spilllovers 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.

To estimate the spillover effects, two different strategies can be employed. First, the spatial DiD can be estimated 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. An advantage of this strategy is that it also uncovers the total treatment effect provided that the parallel trends assumption holds. However, these estimates might be misleading if villages are exposed to more than one SEZ within 50 kilometers and spillovers are additive. A potential solution is to count the number of SEZs in each distance bin a village is exposed to, and estimate all these spillover effects separately (Butts, 2023). 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). This means that the first strategy will be most suited at estimating the total treatment effect on SEZ-hosting municipalities, and that the second strategy allows for a deeper understanding of the spillover effects of SEZs.

Estimating the total treatment effect I follow both strategies in order to understand the differential impact of post-reform SEZs on local labor markets. First, I use the aforementioned concentric ring approach to estimate the direct treatment effect. Following Gallé et al. (2024), I assume spillovers do not extend beyond 20-25 kilometers from the nearest SEZ, and use this as the baseline. For the concentric ring approach, I divide the villages in 50 kilometers around each SEZ into five kilometer distance bins, using the baseline of 20-25 kilometers as the reference category. My baseline regression equation is:

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

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 fixed effects (α_d and α_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 with the treatment dummy. Finally, following Butts (2023), I estimate the standard errors ϵ_{mt} following Conley (1991); this means that I allow for a common error term component for villages within 25 kilometers of each other. In the Appendix, 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 5:

$$\begin{aligned} \mathrm{d}\ln Y_{mt} = &\sum_{d=0,d\neq 5}^{10} \beta_d (D_{[d_m=d]} \times Post_{mt} \times Man_{mt}) + \sum_{d=0,d\neq 5}^{10} \delta_d^M (D_{[d_m=d]} \times Man_{mt})) \\ &+ \delta_1^P (Post_{mt} \times Man_{mt}) + \gamma' (\mathbb{X}_m \times Post_{mt}) + Post_{mt} + \alpha_d + \alpha_t + \epsilon_{mt}, \end{aligned} \tag{6}$$

where Man_{mt} is a dummy equal to one if the nearest SEZ to municipality m is in manufacturing. The coefficients of interest, β_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.

Finally, to distinguish between between and within sectoral reallocation, I can include industry (NIC2) level fixed effects in this regression specification. If the result remains similar after controlling for industry-specific time invariant characteristics, the effect cannot be explained by a reallocation to sectors that are less land intensive and thus potentially more labor intensive. Instead, it points to the entry barriers inducing selection as in Hopenhayn (1992).

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 the Results section 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 5 using the Economic Census in 1998 and 2005. This supports the parallel trends assumption if the placebo estimates do not show any significant pattern.

Estimating spillovers effects As mentioned before, the estimated spillovers in Equation 5 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. 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. More specifically, I estimate the below equation using the full municipality sample:

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

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. S_m^{cbd} and S_m^{cad} are indicator variables representing the number of SEZs c in distance bin d surrounding municipality 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, β_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. I rerun the same equation focussing on the spillover effect of manufacturing SEZs:

$$\begin{aligned} \mathrm{d}\ln Y_{mt} = \sum_{d=0}^{10}\sum_{c=1}^{3}\left[\beta^{cbd}M_m^{cbd} + \beta^{cad}M_m^{cad} + \sum_{j=1}^{3}\beta^{cjd}\left(M_m^{cbd}\cdot M_m^{cad}\right)\right] \\ + \gamma'(\mathbb{X}_m \cdot Post_{mt}) + Post_{mt} + \alpha_d + \alpha_t + \epsilon_{mt}, \end{aligned} \tag{8}$$

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 these estimates to create a similar set of coefficients as in the previous analysis. Specifically, I use the coefficients estimated in Equation 7 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 β_d^{1a} and β_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.

II. Results

II.i. Main results

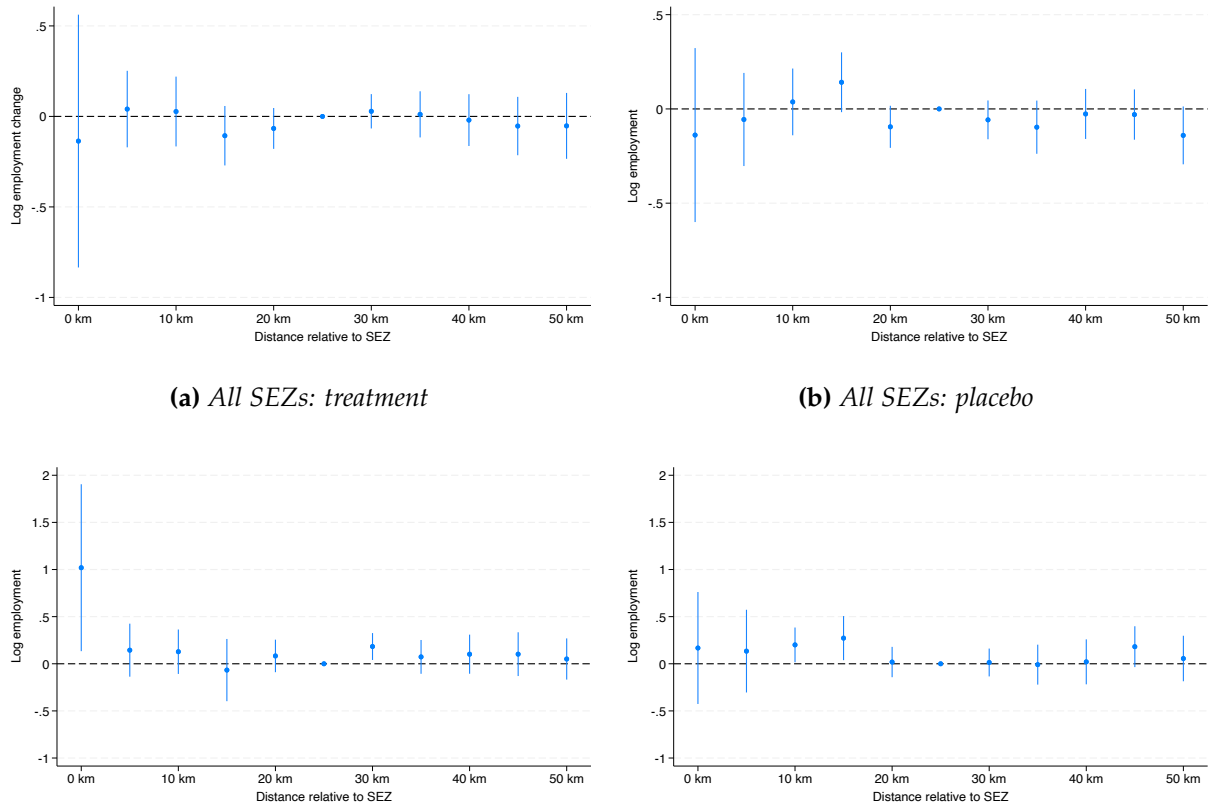
This section describes how the effect of SEZs on local labor market outcomes differs for pre- and post-reform SEZs. As mentioned before, the compulsory acquisition reform – through increasing land acquisition costs – can impact SEZ-led employment growth in two ways. First, it can induce reallocation from more to less land intensive industries: if these industries tend to be more labor-intensive industries, one would expect to see more local employment in SEZs proposed after the reform. Second, it can induce within-industry selection, where the higher land acquisition costs act as an entry barrier, potentially shutting out less productive potential developers. Then, one would also expect an increase in employment for post-reform SEZs. It is worth repeating that Gallé et al. (2024) have shown that SEZs are associated with increased village-level nonagricultural employment up to 10 kilometers from the SEZ; I find a similar positive effect of pre-reform SEZs on local employment. These results simply illustrate whether post-reform SEZs generate more or less employment in surrounding villages than pre-reform SEZs.

Estimating the total treatment effect To understand the effect of an SEZ opening on SEZ-hosting villages, I estimate Equation 5 with log nonagricultural employment as outcome. The results are displayed in Figure 9, 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 13 percentage point lower employment growth between 2005 and 2013 than villages that host a pre-reform SEZ.[38] 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

[38] As $\exp^{-.136} = 0.873$.

9 shows the impact on employment changes for post- compared to pre-reform manufacturing SEZs for villages within 50 kilometers of said SEZ. I find a positive and significant effect for SEZ-hosting villages: on average, employment in villages with a post-reform manufacturing SEZ increases by 177% more than villages with a pre-reform manufacturing SEZ. To contextualize this estimate, note that the average SEZ-hosting municipality has 4,749 nonagricultural employees in 2005. I 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.



(a) *All SEZs: treatment*

(b) *All SEZs: placebo*

(c) *Manufacturing SEZs: treatment*

(d) *Manufacturing SEZs: placebo*

Figure 9: *Employment effects on nearby villages from operational SEZs*

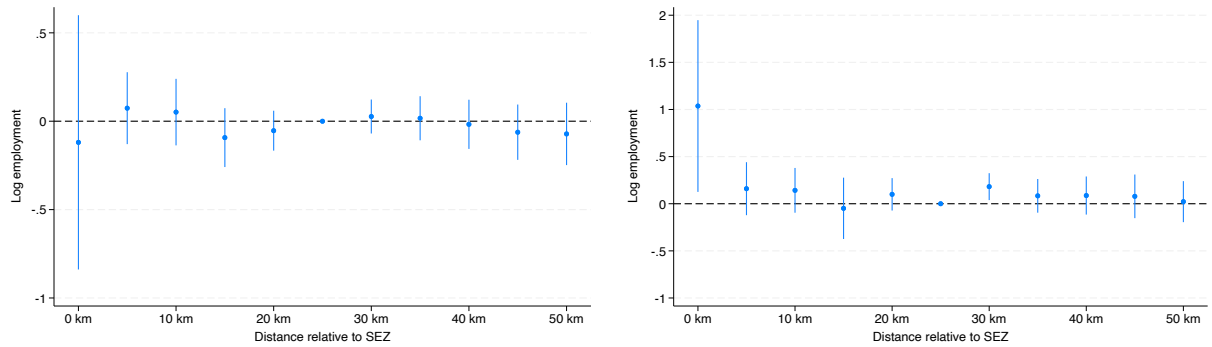
One concern is that this increase in employment reflects relocation rather than creation of new jobs, as is often the case with place-based policies in Western countries (Criscuolo et al., 2022). This is however not reflected in the results I obtain, as municipalities outside the SEZ generally see no significant change in employment. While my analysis is restricted in the sense that I do not check for relocation effects from villages further than 50 kilometers away, it is important to note

that migration costs in India are sizable (Topalova, 2010; Munshi and Rosenzweig, 2016). As Gallé et al. (2024) argue, 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. My results do not reflect large-scale migration across the distance bins, and thus suggest that this is new employment.

Finally, it is interesting to understand how the decrease in entry and increase in local employment for manufacturing SEZs relate to one another. 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 6. 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.[39] This together suggests that compulsory acquisition reduces entry into manufacturing, but is associated with both higher local and higher aggregate industrial employment.

Between versus within sectoral reallocation As mentioned before, the positive and significant effect of post-reform manufacturing SEZs on nonagricultural employment might simply reflect a move away from more land-intensive manufacturing and instead to more labor-intensive manufacturing. To see if that is the case, I rerun the above regressions and include NIC2 industry-level fixed effects. Insofar production factor intensity is industry-specific, a positive effect in this version of the model would imply that the reform gave rise to selection within industries. The results for this exercise, both in the full SEZ sample and for manufacturing SEZs, are given in Figure 10. Interestingly, the results are almost identical to the standard analysis: in general, post-reform SEZ-hosting villages see on average 12% lower employment growth; for manufacturing, employment increases by 181%. This provides suggestive evidence that the reform led to stricter selection within industries, rather than to reallocation to more labor-intensive industries.

[39] 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 $11 \times (\exp^{1.019281} - 1) \times 4,749 = 92,525.8$.



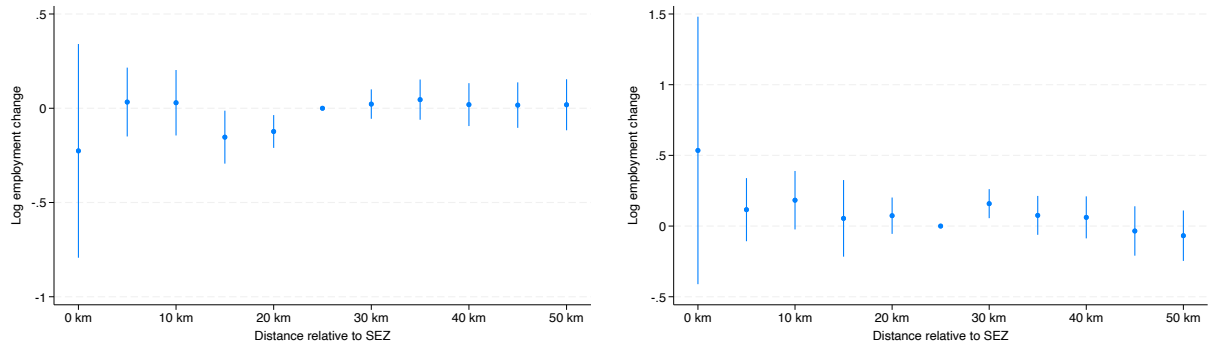
(a) *All sectors* **(b)** *Manufacturing sectors*

Figure 10: *Employment effects on nearby villages are driven by within-industry effects*

Estimating spillover effects To better understand the spillover effects of the development of a nearby SEZ, I no longer condition exposure on the nearest SEZ. Instead, I consider the full sample and thereby the full set of SEZs in the vicinity of each village, estimating a separate effect for each distance-bin and SEZ count combination following Equation 7. Figure 11 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; again, note this strategy yields a coefficient on the SEZ-hosting municipalities that does not correspond to the total treatment effect estimated above. More specifically, we observe a smaller coefficient than in the earlier results, 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 8 are shown, where the coefficient at 10 kilometers tells us by how much post-reform manufacturing SEZs. Here, I find that the direct effect of an SEZ on a 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.

In conclusion, I find no differential effect on local labor markets for post-reform SEZs in the full sample, but I find evidence that manufacturing SEZs that were proposed after the reform are associated with significantly more employment in the villages that host these SEZs and suggestive

evidence that they generate higher spillovers on villages further away. Finally, I showed that this increase in employment is unlikely to be driven by a reallocation from less to more labor intensive industries.



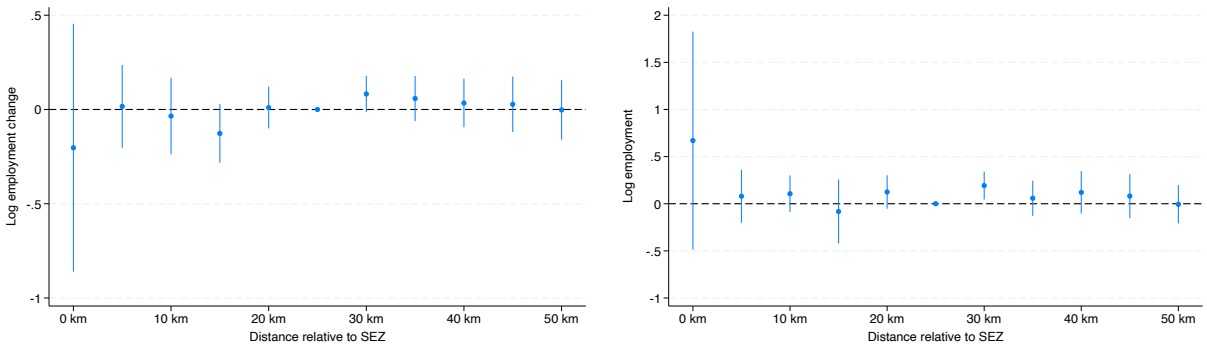
(a) *All sectors* **(b)** *Manufacturing sectors*

Figure 11: *Spillover effects of one nearby post-reform SEZ*

II.ii. Robustness

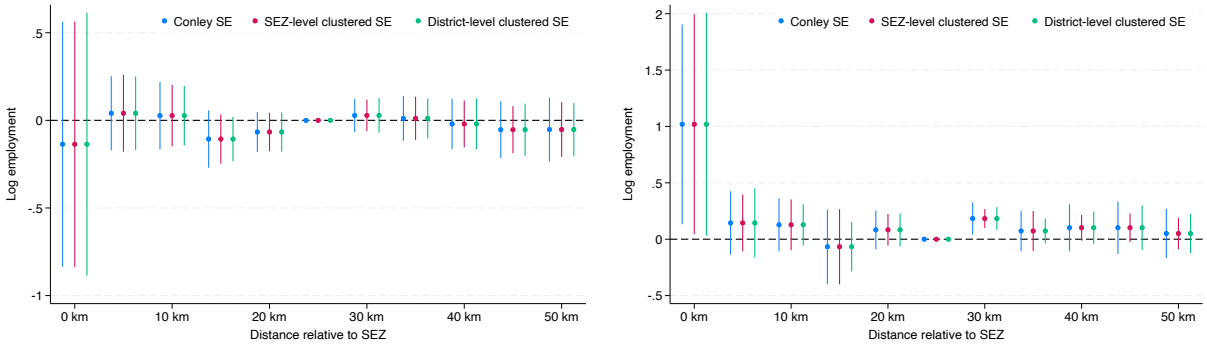
Beyond the placebo tests discussed earlier, this subsection provides two further robustness checks. First, I rerun the specification without any controls; if the results are similar, this supports the claim that locational differences do not add significant bias to the estimates. These results are displayed in Figure 12, and I indeed find that except for the coefficient on SEZ-hosting villages being slightly lower, the estimates are comparable to the baseline results. Based on this, one would conclude that while the controls do alleviate some bias coming from comparing treatment and control units, they mainly serve to improve precision.

Second, I verify how robust the results are to different strategies for estimating standard errors. For example, the baseline specification with spatially clustered standard errors might be wrong if the local labor market impact of a SEZ is contained within district borders. Alternatively, estimated standard errors might have a common component across villages affected by the same SEZ. To validate the empirical exercise, I rerun the estimation using either standard errors clustered at the nearest SEZ level, or at the district level. These results, together with the baseline estimation, are displayed in Figure 13; again I find that the main results are not affected by the estimation strategy for the standard errors.



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

Figure 12: *Employment effects on nearby villages from operational SEZs without controls*



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

Figure 13: *Estimation with different standard errors*

VII. CONCLUSION

Compulsory land acquisition, or eminent domain, has been a long-standing practice for governments across the world and levels of development. In settings where land transaction costs are prohibitively high, compulsory acquisition might be an efficient policy tool to stimulate economic activity. Indeed: in the last few decades, governments in transition economies have increasingly adopted compulsory acquisition to facilitate the implementation of industrial policy. 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. Leveraging an unexpected reform that prohibited compulsory acquisition and novel data on SEZ development, I study how the reform impacted participation in and the employment effects of the SEZ policy. I set up a simple conceptual framework where restricting compulsory

acquisition would correspond to an increase in land acquisition costs and thereby entry costs. One would then expect sectoral reallocation from more land intensive to less land intensive industries. Specifically, given that manufacturing is significantly more land intensive than services, the reform would reduce entry into manufacturing relatively more. At the same time, if manufacturing developers are heterogeneous, this entry barrier might induce positive within-industry selection as in Hopenhayn (1992).

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 is lower by 24.5 percentage points. I provide evidence in favor of the theory that this increased land acquisition costs and thereby the entry barrier, since the effect is strongest for the most land intensive industries and the areas with the highest land fragmentation. 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 SUTVA violations 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 implies a change in employment effects. Specifically, I compare whether nonagricultural employment in villages surrounding post-reform SEZs changed differently than employment in villages around pre-reform SEZs. I find no significant effects for the full sample, but – in accordance with manufacturing being more land intensive – find that post-reform manufacturing SEZs generate 177% more employment in SEZ-hosting villages than their pre-reform counterparts. This result cannot be explained by a reallocation from more land intensive to more labor intensive industries; instead, it seems to be driven by selection within industries. Finally, I show that in the aggregate, the 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. Since SEZs are also a trade policy, it would be interesting to complement these employment results with other measures of developer quality, such as contributions to international trade, capital and agglomeration effects, to better pinpoint what elements determine a successful SEZ. 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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APPENDIX

I. Literature Review

This paper contributes to multiple strands of literature, which will be elaborated on below.

I.i. Compulsory acquisition

Large-scale land acquisition by the government has been a documented practice at least since 3000 BC, with records on the expropriation of villages to create large public estates in the Old Kingdom of Egypt (Roudart and Mazoyer, 2016). For as long, it has been a tenuous and delicate strategy, with dispute on the tradeoff between investments for public purpose and the consequence of dispossession (Keith et al., 2009). This tension in turn has spurred an economic literature that mainly focuses on documenting the consequences of compulsory acquisition for those dispossessed. Often based on detailed case studies, this research highlights the long-term negative welfare effects on citizens whose land is expropriated (Cernea and Mathur, 2007). Moreover, Gironde and Senties Portilla (2016) showed that there are important spillovers to other villagers: while the investments following land expropriation improved access to nearby villages, they also dramatically reduced job security, especially for farmers. Relatedly, Ghatak and Mookherjee (2014) argued that not compensating the farmers that do not own but work on the land that is acquired means that the landowner does not internalize the farmers' losses upon sale of said land. They showed that in fact farmers need to be overcompensated to curb the owner's socially excessive incentive to sell. Understanding how and who should be compensated, especially in contexts with weak property rights and tenure security, remains a complicated but important issue (Lindsay et al., 2017). Especially in inactive land markets, government intervention might be necessary to engender industrialisation (Banerjee et al., 2007). Finally, the theoretical literature that shows the benefits of compulsory acquisition for private investment does assume policymakers are not opportunistic and therefore make the economically sound decisions. López and Clark (2013) showed how opportunistic policymakers could misuse compulsory acquisition, leading at best to non-efficiency enhancing investments and at worst to direct corruption.

Even though improving allocative efficiency is often used as an economic justification for this practice, there is little research on what the actual implications of compulsory acquisition are on industrialisation. The closest paper to this one is Blakeslee et al. (2021), who studied the impact of land-zoning laws on economic activity in the context of the Industrial Areas policy in Karnataka. They argued that their findings –– Industrial Areas stimulate firm entry and employment –– demonstrate the burden of strict land zoning laws that hinder land use conversion, and that

the act of providing industrial land at market rates is an effective enough place-based policy for emerging economies. This important result can however not distinguish between the barriers to land acquisition and the barrier to land use conversion, two barriers that are present in varying degrees across both developed and developing countries.[40] In contrast, my paper can inform on the degree to which access to land is the main constraint private firms face.

I.ii. Impact of land market frictions on economic outcomes

My paper also relates to the literature that describes the impact of land market imperfections, such as land fragmentation or weak property rights, on economic outcomes. As discussed before, these imperfections are used to justify compulsory acquisition, and prohibiting this practice would expose private investors to especially higher bargaining costs. The bulk of evidence on this is for the agricultural sector, showing that land barriers are an important factor in explaining the agricultural productivity gap between developed and developing countries (Adamopoulos and Restuccia, 2014). First, it has been shown that land market imperfections hinder farms from achieving their optimal scale (Britos et al., 2022). Furthermore, the inefficiently small plot sizes generate underutilization of labor or disguised employment: in the case of India, Foster and Rosenzweig (2022) showed that if all farms were at optimal size, output per worker would increase by 68% while reducing the total agricultural labor force by 16%. Finally, Kitamura (2022) showed how land market frictions and credit frictions interact: using a large land redistribution policy in Japan, he found that increased access to land and increases credit access through higher collateral. This incentivizes farmers to invest in technology, thereby increasing agricultural productivity.

More recently, the effect of land market frictions on the manufacturing sector has been investigated. Duranton et al. (2016) established that in India, land misallocation is the main driver for output misallocation in manufacturing. This has been corroborrated more formally by Sood (2022), who found that manufacturing firms in regions with higher land fragmentation are 22 percent smaller than their counterparts in regions with more concentrated land ownership and expand less over time; the impact of which equates to a reduction in lifetime producer profits of 6.5%. Pal et al. (2022) developed a model that shows that stricter land ceilings, which cap the amount of land a landowner can hold, reduce both capital investment and industrial output through increasing land fragmentation. The only other study that describes the impact of land market frictions on both manufacturing and services is Mehta (2022). In his paper, he investigated whether firms perform worse in states with more land fragmentation, finding that only manufacturing firms have significantly lower output and employment in such states. Furthermore,

[40] Strict land zoning regulation is an especially important barrier in developed countries: Herkenhoff et al. (2018) show how state-level land use regulations have depressed macroeconomic activity in the US.

the effect size is higher for states that also exhibit more land disputes or an ill-functioning land rental market, suggesting that it is differences in land requirements that drive this phenomenon. A virtue of my study is that I can show not just the effect of the reform on the local employment effects of SEZs by sector, but also show that the institutional context determines who develops an SEZ in the first place. Understanding how land policy affects selection is of the utmost importance to contextualize the findings that already-established firms perform worse.

I.iii. Impact of SEZs in developing countries

Third, my paper contributes to the stream of literature on the (socio-)economic impact of place-based policies in general and SEZs in India specifically. Research on spatial policies initially focused on developed countries due to data availability, showing mixed effects (e.g. Greenstone et al. (2010), Brachert et al. (2019) and Criscuolo et al. (2022)). Koster et al. (2019) pointed out the the impact of place-based interventions might well play out differently in developing countries, as these are generally focused on well-performing firms or areas. This reduces the chance that the local benefits such as agglomeration effects are dwarfed by firm and job displacement in surrounding areas. In their paper, they found large increases in firm productivity and local wages in industrial parks in Shenzen, China. Partly because of this success, SEZs have become one of the more prominent development strategies (Frick et al., 2019).

However, the Indian SEZ experience has generally been less impressive: Görg and Mulyukova (2022) showed, based on PROWESS data, how the productivity of firms in close proximity of SEZs is actually negatively affected. He showed that this effect is most pronounced for state-owned SEZs. In a complementary paper, Gallé et al. (2024) found positive employment effects of SEZs, mainly driven by small informal firms. Alkon (2018) investigated the oft-made claim that SEZs not only bring economic but also developmental benefits, such as improvements in human capital or infrastructure, finding no effect. This is complementary to Aggarwal (2007), who finds that SEZs create jobs but have limited impact on human development. Finally, Hyun and Ravi (2018) used night light data to show that SEZs boosted economic activity. They further provided evidence that SEZs draws workers out of informality, such that the formal sector grew in size and productivity. My paper is complementary to this existing literature by highlighting how compulsory acquisition, and its restriction two years into the SEZ policy, also affect the local employment effects of SEZs.

II. Data

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

II.i. 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.[41] 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 14 (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 a 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.

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 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

[41] 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 14: *Building the proposal dataset: MIDC's Akola SEZ*

the tables (around 90 pages) to readable text, and extract the aforementioned characteristics as in Figure 14 (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 15 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 utilities there is no consistent pattern. Regarding land intensity, Table 10 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 11 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.

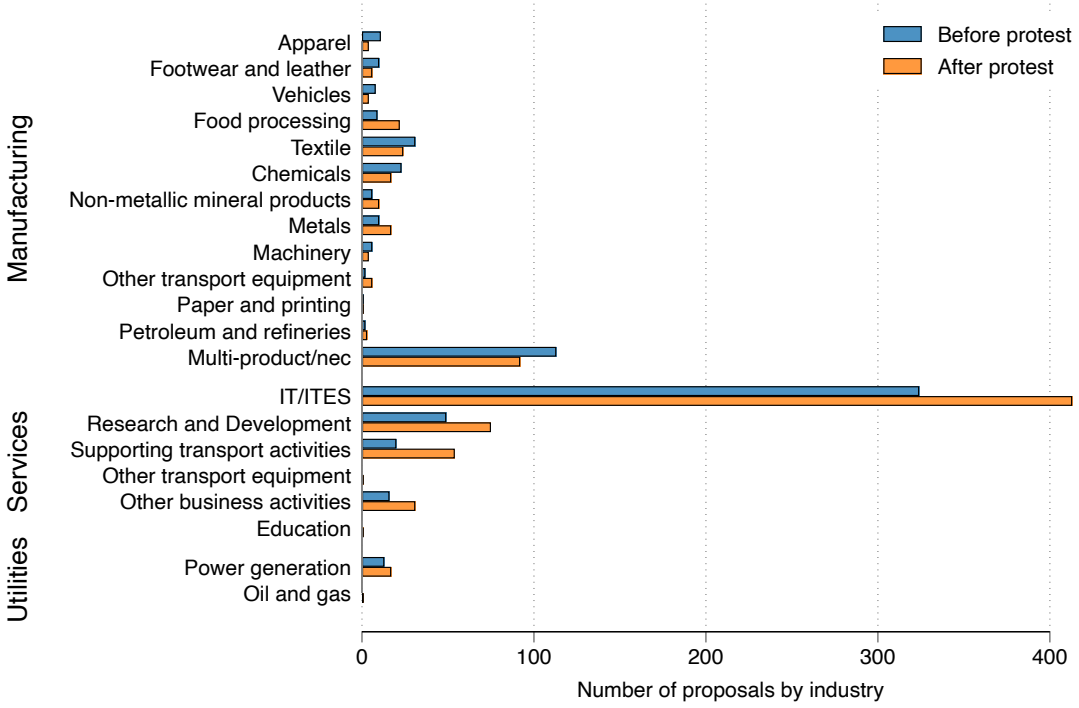


Figure 15: *Proposals by industry, before and after reform*

II.ii. Data on land fragmentation

To obtain a measure of land fragmentation, 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.[42] 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 16 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

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

Table 10: *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 11: *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.

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

as Bihar and Jammu and Kashmir were not sampled in 2000, and some subdistricts could not be matched perfectly.

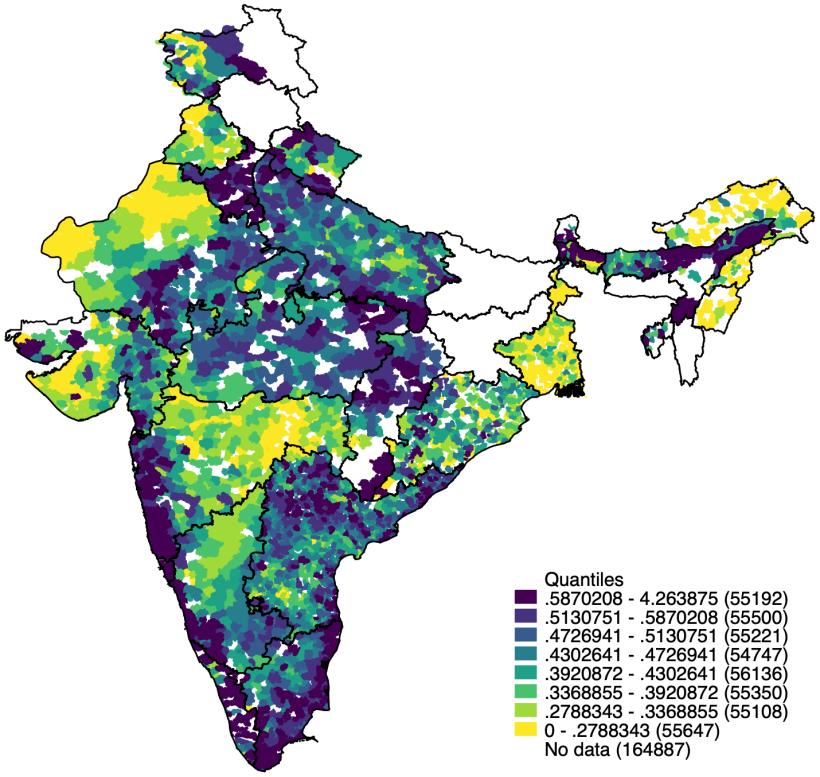


Figure 16: *Theil-T index on land concentration*

II.iii. 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 17 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 12 describes the baseline sample.

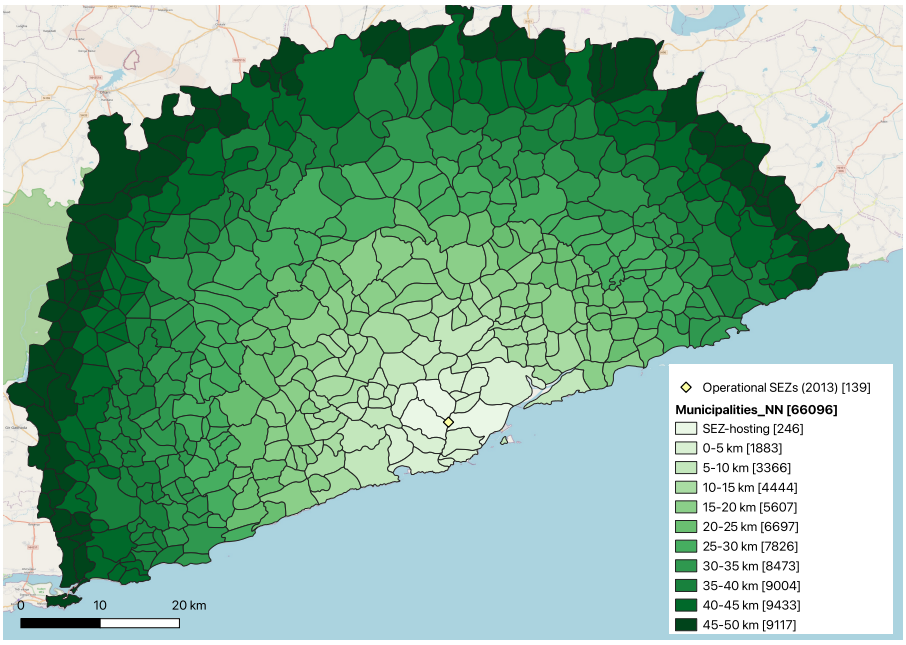


Figure 17: *Villages neighbouring the E. Complex SEZ in Amreli, Gujarat*

Table 12: *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.

III. 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 13 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 Maharashtra Industrial Development Corporation, promised SEZ developers an "expeditious" land acquisition.[43] 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).

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 14, where the first three columns display the results

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

[44] 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 (...)."

[45] 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."

Table 13: *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 (...)"[44]
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 (...)"[45]
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."

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.

IV. Additional results on entry

This section contains supplementary results on entry.

Direct effect reform on moving on through development process Table 15 estimates Equation 3, using a different outcome variable across each two columns. Specifically, it is an indicator that equals one if the proposal is eventually formally approved, notified or operational. This would highlight whether the Board of Approval or the Central Government judge proposals differently after the reform for formal approval or notification respectively. In the first column, I show that there is a positive but insignificant effect on the probability of a SEZ being formally approved in a compulsory acquisition state after the reform. Column (2) repeats this analysis, restricting the sample to manufacturing SEZs, again finding no significant effect. The third and fourth column study whether the probability of becoming notified is affected by the reform; I find no significant

Table 14: *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. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

change there either. Finally, column (5) and (6) show the effect of the reform on the probability of becoming operational. Now, the coefficient is negative for manufacturing proposals, but it is very small and insignificant. Thus, I cannot find any evidence that the reform caused a significant change in the share of approved, notified or operational proposals.

Table 15: *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

The dependent variable is an indicator for the proposed SEZ being formally approved, notified or operational respectively. Each observation is a SEZ-meeting-subdistrict combination, excluding all Utilities and resubmitted proposals. See the notes under Table 4 for details on the included location trends. Standard errors, clustered at the state level, are in parentheses.
$^{*}p < 0.1$,** $p < 0.05$,*** $p < 0.01$.

Mediating effect of land concentration on industrial composition If the reform reflects an increase in land acquisition costs, this would reduce investment most in those subdistricts with relatively high land fragmentation. Table 16 shows that the decrease in the share of manufacturing SEZ proposals is indeed mainly driven by subdistricts with a below-median Theil index. Again, I now use state rather than region fixed effects. Column 1 and 2 show the reform-induced change in the manufacturing SEZ share in areas with a below-median Theil index is around 40 percent larger than the change in areas with a relatively high Theil index. The difference between the below- and above-median Theil index becomes especially pronounced for the share of manufacturing SEZs in further stages of development, with the share of formally approved manufacturing proposals being 45 percentage point lower in areas with more land fragmentation whereas the corresponding change in more concentrated areas is negative but not significantly different from zero. For notified SEZs, the share of manufacturing SEZs is 32.6 percentage points lower in more equal land areas, with again no significant change for subdistricts with stronger land concentration. This suggests

that the reaction to the reform is more pronounced in areas that have historically more land fragmentation. This result is reflected more directly in Table 17, which shows how developers after the reform in states with compulsory acquisition policy are significantly more likely to propose a SEZ in an area with a higher land concentration. The increase in the average Theil index associated with proposals equals 0.0853, which is one-fourth of a standard deviation. Alternatively, this increase would imply a move from the median to the 70th percentile in terms of land concentration. The change is more pronounced for those SEZs that are formally approved or become notified; I do not have sufficient degrees of freedom to estimate the change in the Theil index associated with SEZs that become operational. In conclusion, SEZ developers tend to propose SEZs in locations that are more unequal in terms of land ownership after the reform. This can be interpreted as SEZ developers trying to mitigate the bargaining costs now that states are not allowed anymore to facilitate compulsory acquisition.

Table 16: *SEZs after the reform proposed in areas with higher land concentration*

	All proposals		Formal Approval		Notification	
	(1)	(2)	(3)	(4)	(5)	(6)
	Below	Above	Below	Above	Below	Above
After protest	-0.246**	-0.178**	-0.450***	-0.0769	-0.326**	-0.0547
× State CA	(0.0890)	(0.0813)	(0.0927)	(0.0692)	(0.121)	(0.0369)
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	592	602	279	321	173	210
R-squared	0.315	0.276	0.400	0.363	0.527	0.420

The dependent variable is a dummy that equals one if the proposed SEZ is in *Manufacturing*, each observation is a SEZ-meeting-subdistrict combination. The sample is split according to the Theil index of the subdistrict in which the SEZ is located: subdistricts with Theil index below the median are classified as *Below* while those with an above-median Theil index are classified as *Above*. For each group of equations, the first two columns contains all proposals, column 3 and 4 restrict the sample to those SEZs that were formally approved and the final two columns consider only notified proposals. See the notes under Table 4 for details on the included location trends, although this analysis does not control for the number of banks and the total labor force. Standard errors, clustered at the state level, are in parentheses.

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

Table 17: *SEZs after the reform proposed in areas with higher land concentration*

	Theil index		
	(1) All	(2) Approval	(3) Not.
After protest	0.0853*	0.123**	0.134**
× State CA	(0.0429)	(0.0475)	(0.0458)
Location trends	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Meeting FE	Yes	Yes	Yes
Observations	1233	614	395
R-squared	0.587	0.662	0.696

The dependent variable is the Theil index on land concentration in the subdistrict where the SEZ is to be located. Each observation is a SEZ-meeting-subdistrict combination. The first column contains all proposals, while the second and third column restrict the sample to those SEZs that were formally approved or notified respectively. See the notes under Table 4 for details on the included location trends. Standard errors, clustered at the state level, are in parentheses.

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

V. Additional robustness checks

In this section, I provide additional robustness checks on the results from Section **??**. First, I show that the reform's effect is robust to alternative strategies to including controls. The results are in Figure 18. The first two coefficients are estimated based on Equation 2 – i.e. regressing the Manufacturing dummy on the treatment interaction, including region and meeting fixed effects but without any controls – using the full sample and the restricted sample where I exclude all observations with missing controls. The third model extends the previous one with controls; whereas the fourth includes all controls interacted with a post reform dummy. Finally, the last coefficient display the reform's effect per the baseline regression as in Table 4. The figure shows that the main effect is consistently negative and significant, and is not affected much by the exact specification of the control variables.

The second check is regarding measurement in the treatment variable, specifically exploring how robust the effect is to the inclusion of each compulsory acquisition state. Panel (a) in Figure 19 shows how the baseline effect of the reform on manufacturing entry changes if one of the treated

states at a time is removed from the sample altogether. 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. The coefficient for the reform's effect on log SEZ size remains negative but insignificant in all leave-one-out specifications.

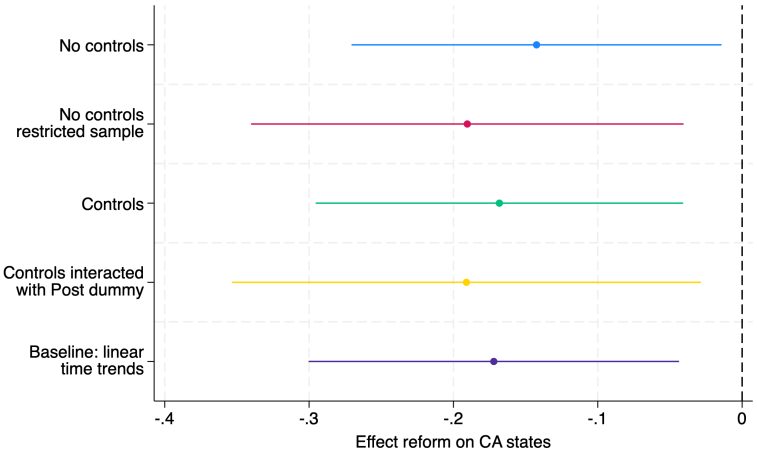
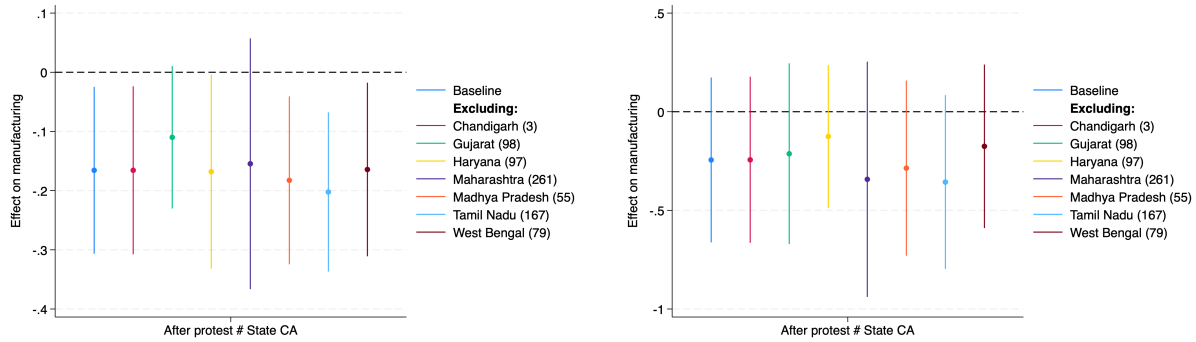


Figure 18: *Robustness to inclusion of controls*

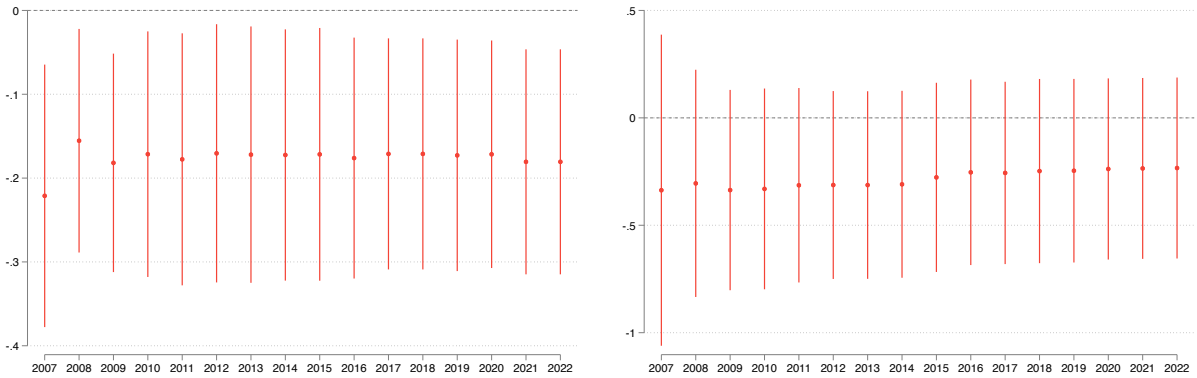


(a) *Effect on manufacturing share* **(b)** *Effect on log SEZ size*

Figure 19: *Robustness to removing one treated state at the time*

A second concern is that the effect is driven by changes in either land or SEZ policy after the reform but before the end of the sample. As mentioned before, the Right to Fair Compensation and Transparency in Land Acquisition Resettlement and Rehabilitation Act, ratified in 2013, increased

the compensation to be paid for compulsory acquisition and aimed to restrict its use for private endeavours. To understand this, I rerun the baseline regression – excluding Utilities proposals as in column 1 of Table 4 – for different samples based on the year of the proposal; the results and the corresponding 95% confidence intervals are displayed in Figure 20. More specifically, 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. The left-hand panel shows that effect on the share of manufacturing proposals is relatively stable across the years, with the most sizeable effect in the year directly following the protest. This coefficient thus remains negative and significant for each sample restriction, suggesting that the effect I find is not driven by later years that saw more significant national changes in policy and environment. I find a similar pattern for log SEZ size as seen in the right panel; the effect remains negative albeit insignificant.

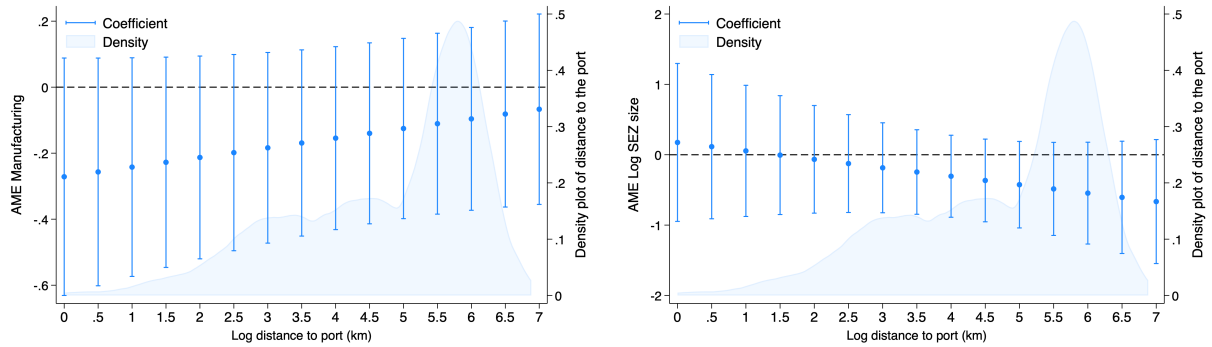


(a) *Effect on manufacturing share* **(b)** *Effect on log SEZ size*

Figure 20: *Excluding later years from the sample*

Third, one might wonder whether the effect of the reform is more so a reflection of scarcity of land in desirable areas. Controlling for location characteristics and showing that the results are robust to including measures of SEZ competition, one remaining concern has to do with the export-oriented nature of SEZs and, especially for manufacturing SEZs, a potential desire to be close to a port. I estimate a variation of Equation 4, where instead of interacting the treatment variable with the local land concentration index, I allow for heterogeneous treatment effects of the reform based on distance from the port. Figure 21 shows the average marginal effects of distance to the port on the probability of a proposal being for a manufacturing SEZ with 95% confidence intervals; I also include a density of distance to the port to facilitate interpretation. The left-hand panel displays a clear pattern: the effect on manufacturing is strongest closest to the port and tends to zero at the other end of the distance distribution. However, this slope is not significantly different from zero, no estimate is significantly different from zero, and the proposed locations are

generally further away from the port, where the coefficient is very similar to the effect size I find in the baseline. Panel (b) shows the results from a similar margin analysis with log SEZ size as the outcome variable. I find that there is a negative relationship between distance to the port and the effect of the reform on log SEZ size. Again, this relationship and any marginal effects are not significantly different from zero.



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

Figure 21: *Average marginal effects with respect to distance to the port*

First, Table 18 replicates the first three columns of Table 4. The first three columns consider all proposals; note that there are insufficient degrees of freedom to include time fixed effects. To take macroeconomic shocks in account, I restrict the sample to proposals discussed between 2006 and 2009, allowing me to include year fixed effects. Note that Figure 20 implies that excluding proposals after 2010 does not significantly affect the estimate of the reform's effect. The first three columns show that the odds of a manufacturing proposal were reduced after the protest, and that this decrease is more marked in compulsory acquisition states. The preferred specification in column (3) shows that after the protest, the odds of a proposal being for a manufacturing SEZ is 35% lower than before; in compulsory acquisition states, this is more than 60 percent lower than before the reform.[46] In the second three columns, I repeat the analysis for this subset of proposals and add year-fixed effects. The preferred specification in column (6) shows that after the reform in compulsory acquisition states, the odds of a proposal being for a manufacturing SEZ were 53% lower.

I adopt a similar approach in replicating the results on industry rank from Table 7. In the first three columns, I show the results for the full sample without time fixed effects; the dependent variable is a categorical variable ordering industries from low to high pre-reform average size. The results show that the odds of a given proposal of being in a larger industry (ceteris paribus

[46] As $0.65 \times 0.572 \approx 0.37$.

Table 18: *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. The reported estimates are odds ratios. See the notes under Table 4 for details on the included location trends. Standard errors, clustered at the state level, are in parentheses.
$^{*}p < 0.1$,** $p < 0.05$,*** $p < 0.01$.

the value of the control variables) are lower after the reform, and especially in those states with compulsory acquisition policy. The preferred specification in column (3) shows that the odds of the highest category are 0.619 times greater than all other lower categories after the reform. In compulsory acquisition states, the odds of a proposal being in a larger-scale industry in terms of rank are 56% smaller after the reform.[47] Finally, the last three columns consider only the first three years and include year fixed effects. A similar pattern emerges, with the odds of a proposal being in a relatively larger industry being almost halved in compulsory acquisition states after the reform.

[47] As $0.712 \times 0.619 \approx 0.44$.

Table 19: *Odds of SEZs in large-scale industries lower in CA states after reform*

	All proposals			Proposals (2006-2009)		
	(1)	(2)	(3)	(4)	(5)	(6)
After protest	0.708**	0.650**	0.712			
	(0.120)	(0.117)	(0.163)			
After protest	0.685	0.628*	0.619*	0.623	0.592	0.590*
× State CA	(0.179)	(0.165)	(0.168)	(0.187)	(0.189)	(0.187)
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	21484	21484	21484	18579	18579	18579
True observations	1242	1242	1242	1075	1075	1075
Log conditional likelihood	-8949.1	-7665.2	-7659.7	-7844.8	-6715.9	-6723.6

The dependent variable is a categorical variable indicating the proposed industry, ordered from low to high based on the average size before the reform. The model specification is a fixed-effects ordered logit, the reported estimates are odds ratios. See the notes under Table 4 for details on the included location trends; note that these specifications, for degrees of freedom consistency, do not include log number of banks in 2005 as a control. Standard errors, clustered at the state level, are in parentheses.

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