Land Acquisition and Sectoral Composition: Evidence from India*

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Abstract

In many emerging economies, compulsory land acquisition by the government is an integral part of industrial policy. This use of eminent domain, especially in fragmented land markets, ensures private investors are not burdened by potentially prohibitively high negotiation costs in the land acquisition process. While this tool might stimulate industrialisation and bring about allocative efficiency, it comes at a cost: those whose land is expropriated suffer long-term welfare losses. To investigate this trade-off, this paper studies the impact of an increase in land acquisition costs on industrial development using an unexpected reform that placed restrictions on compulsory land acquisition for Special Economic Zones (SEZs) in India. I combine this policy shock with ex-ante variation across Indian states in compulsory acquisition policy for SEZs in a difference-in-differences design. I find that this increase in land acquisition costs reduces the average size of SEZs because its industrial composition changes, with the share of manufacturing decreasing by almost 40 percent. Using novel data on SEZ proposals, I show that this effect is mostly driven by lower intentions to entry, with no significant differences in actual SEZ operations. Moreover, I show using a spatial difference-in-differences design that manufacturing SEZs under the new policy contribute more to local employment than their older counterparts. I reconcile these findings through the Hopenhayn (1992) model, which shows that a higher entry cost reduces entry but induces selection.

JEL classification: L16, O14, Q15

Keywords: Land Reform, Industry Mix, Structural Change, India.

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I. INTRODUCTION

Since ancient Egypt, compulsory land acquisition – also known as eminent domain or land expropriation – has been used to stimulate economic activity (Roudart and Mazoyer, 2016). Authorities used eminent domain to aggregate farmland into larger plots, thereby increasing *agricultural* productivity, as in 16th century Britain during the enclosure movement. This practice continues to exist in present-day transition economies, where land provision is increasingly adopted as an integral part of *industrial* policy (Lindsay et al., 2017). In the last two decades, there has been a dramatic increase in the total size as well as the share of land deals for non-agricultural activity compared to those for agriculture (Land Matrix, 2023).[1] In China, five percent of all arable land was acquired by the government to be converted to non-agriculture between 1998 and 2004 (Kahn, 2006).

When it comes to large-scale productive investments, there can be an economic justification for eminent domain, namely to shield private investors from prohibitively high negotiation costs (Sarkar, 2007). As land assembly generally requires a large amount of contiguous land, the investor needs to reach an agreement with copious landowners (Miceli, 2011). This process can be especially costly in developing countries, which are characterized by fragmented land ownership and incomplete property rights (Deininger, 2003). This may dissuade private investors, prevent land from being put to more productive use, and thereby detract from allocative efficiency (Menezes and Pitchford, 2004; Miceli and Sirmans, 2007). On the other hand, increased discretion in compulsory acquisition needs not improve allocative efficiency, and might even bring about increased inefficiency (López and Clark, 2013). Moreover, international organizations and researchers have long warned that an appropriate regulatory framework to protect those who can be dispossessed and to arrange suitable compensation for those who are is of the essence (Keith et al., 2009; Choudhury and Narayana, 2020). This necessity is emphasized by research documenting the long-term negative welfare consequences of forced displacement (Cernea and Mathur, 2007; Gironde and Senties Portilla, 2016). What is not yet investigated is how and whether compulsory acquisition actually engenders industrialisation and development.

This paper provides novel evidence of the impact of land acquisition costs on industrial development in the context of an industrial policy. I exploit an unexpected reform that placed restrictions on compulsory land acquisition for Special Economic Zones (SEZs) in India. This context is especially suitable to investigate the relationship between compulsory acquisition and

[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 464,265.74 square kilometres, which is almost equivalent to Spain's total land area.

allocative efficiency. Earlier research found that India's economy is characterized by substantial resource misallocation (Hsieh and Klenow, 2009; Duranton et al., 2015; Bau and Matray, 2023). Misallocation of *land* has mainly been studied in the agricultural sector, but India's highly fragmented land market, lack of secure property rights and overlapping – and sometimes conflicting – land laws impact all sectors. Importantly, Duranton et al. (2016) found that land is the main source of misallocation in Indian manufacturing. A reform that restricts compulsory acquisition should therefore be expected to influence industrial composition, with manufacturing suffering most.

I aim to identify this effect in the context of Indian SEZs. Introduced with the explicit goal of fostering industrial growth, India's 2005 SEZ Act provided a multitude of financial and regulatory benefits to firms located in a SEZ (Central Government of India, 2005). The Indian SEZ experience is notably different from SEZ policies in other countries. First, it explicitly allowed also private developers to create a SEZ. Moreover, SEZs in India are abundant: within three years after the law was introduced, more than 500 SEZs in manufacturing and services were approved. Finally, to ensure the success of this large-scale policy in an ill-functioning land market, seven out of 39 State Governments committed to expropriating land for both public and private developers when the act was introduced (Levien, 2012). Thus, the State Governments shielded SEZ developers, and any firms locating in the SEZ, from the normally high negotiation costs involved in private land acquisition. These features make the Indian SEZ experience an ideal case study, as I can directly relate *government* land acquisition to *private* economic activity.

I exploit an unexpected reform in 2007 that increases land acquisition costs for SEZ developers. In that year, a large protest against a SEZ in West Bengal was violently shut down by the state police, with fourteen farmers being killed and more than a hundred farmers reported as missing. In response, the Central Government announced that from then on, *forced* land acquisition was prohibited in all states, and that landlosers must be compensated properly in terms of rehabilitation and resettlement (SEZ Board of Approval, 2007). Importantly, SEZs that were already approved were exempt from this policy; only new developers were exposed to this dramatic increase in land acquisition costs. Comparing the SEZ size distribution before and after the protest shows a clear shift, with newer SEZs being smaller on average. I furthermore show that the effect of this shock is concentrated in those states that mandated compulsory acquisition for SEZ developers. I then exploit this regional heterogeneity in the ex-ante propensity for expropriation as a measure of exposure to the reform in a difference-in-differences framework.

Existing research on the effects of SEZs uses publicly available data on notified – meaning that the Central Government changed the land use of the plot to industrial – or operational SEZs. This is not informative on the effect of a compulsory acquisition reform, as the plot on which the SEZ is located is generally acquired years in advance. To overcome this problem, I have created a

unique dataset of the universe of official SEZ *proposals* between 2006-2022. The proposal, which is submitted to the SEZ Board of Approval, is the first step in the official SEZ development process. I have scraped the Minutes from all SEZ Board of Approval meetings, and used text analysis to compile a dataset of 1,439 proposals for 1,119 distinct SEZs, with information on the developer, its proposed location, proposed size and planned sector of operation. I supplement this with the aforementioned publicly available data on SEZ notification and operation such that I can track SEZ development over time. Finally, I merge this novel village-level dataset with the SHRUG dataset, using their rich open source data to track economic development and sectoral composition at the village-level (Asher et al., 2021).

My dataset is unique in that it allows me to analyze proposed and actual SEZ operations, and thereby SEZ industrial composition, separately. This has an additional advantage in that I can link it closely to existing theories on firm entry and behaviour. As land is arguably a fixed production factor, the reform restricting compulsory acquisition corresponds to an increase in fixed costs for new entrants. Moreover, this increase is relatively stronger for sectoral specializations that require more land: on average, manufacturing requires around seven times more land than services.[2] A standard Hopenhayn (1992) model would then predict that this reform reduced larger-scale SEZs *intentions to entry* more than smaller-scale SEZs, due to the relative difference in entry barriers. However, the increased entry barrier also has a selection effect, suggesting that new entrants are on average of higher productivity or quality. I disentangle the effect on entry and eventual SEZ outcomes to understand the total effect on industrialisation and development.

The first analysis investigates whether the reform affected the size of SEZs and their sectoral composition. Using the difference-in-differences design outlined before, I show that there is no significant difference in the size of SEZs after including controls, but that the proposed sectoral composition of SEZs changes dramatically. Specifically, the share of manufacturing in SEZ proposals in states that committed to compulsory acquisition is around fourteen percentage points lower, and that of services is thirteen percentage points higher. With a baseline manufacturing share of 35.7 percent, this implies a reduction in intentions to enter of almost 40 percent. This reduction in intentions to enter for manufacturing translates into a lower manufacturing share across all stages of SEZ development: the share of operational manufacturing SEZs is nine percentage points lower, albeit insignificant. My results thus show that a restriction of compulsory acquisition reduces entry into industrial sectors for Special Economic Zones, in line with the prediction of Hopenhayn (1992).

[2] Batista e Silva et al. (2014) showed that services need on average 6.7 times less land than industry to produce one monetary unit of gross value added in the Netherlands and Spain. This is not exclusive to developed countries; as mentioned below, multiple papers have found that especially Indian manufacturing is constrained by land.

As Hopenhayn (1992) also states that increased entry costs induce selection, I study whether the eminent domain reform also impacted local labour markets. To do so, I use two rounds of the Economic Census (2005-2013), which contains firm-level employment data for both formal and informal enterprises. As the last wave of the Economic Census was in 2013, I restrict my sample to the 139 SEZs that became operational before this year. Reproducing the difference-in-differences design would lead to biased estimates, as SEZ entry would alter local labour market conditions also in nearby villages. Instead, I follow a recent paper by Gallé et al. (2023) and employ a spatial difference-in-differences strategy: I create bins of five kilometres around each SEZ, and compare the villages in which an SEZ became operational between 2005 and 2013 to those nearby villages that do not have an SEZ. This specification, which includes directly treated, presumably indirectly treated and villages that were not affected, allows for unbiased estimation of the direct treatment effect (Butts, 2023). I then extend their analysis by studying this difference between SEZs that were first proposed before the reform versus those that were proposed afterwards. I find no effect for the full sample, but employment in the municipalities within ten kilometres from a manufacturing SEZ increased significantly more for those that were proposed afterwards. However, the spillovers to municipalities further away are not significantly different. This provides suggestive evidence that SEZs proposed after the reform are on average more productive, which is consistent with the hypothesis that the eminent domain restrictions increased entry costs and thereby induced selection.

My paper contributes to the literature in three distinct ways. First, it is the first to analyze the impact of compulsory acquisition on the effects of industrial policy. I complement existing research on compulsory land acquisition, which mainly involves careful documentation of the negative welfare effects of those who are expropriated, as in e.g. Cernea and Mathur (2007); Gironde and Senties Portilla (2016), the regulatory process governments engaged in compulsory acquisition should adhere to (Keith et al., 2009; Lindsay et al., 2017), or explore in which settings compulsory acquisition, if compensation is fair, could foster economic development in general (Miceli and Sirmans, 2007; Sarkar, 2007; Ghatak and Mookherjee, 2014). The closest related paper to my study, Blakeslee et al. (2021), found that a land-rezoning program in Karnataka increased firm entry and employment. 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. In contrast, my paper can inform on the degree to which *access* to land is the main constraint private firms face.

The second contribution is to enrich existing evidence on the impact of land barriers on firm performance by analysing how they affect firm entry. There is extensive evidence that land market frictions, such as fragmented ownership and weak property rights, have a negative effect on

agricultural productivity and output (e.g. Adamopoulos and Restuccia (2014); Britos et al. (2022); Foster and Rosenzweig (2022)), manufacturing output and employment (Duranton et al., 2016; Pal et al., 2022; Sood, 2022), but not on services output and employment (Mehta, 2022). A virtue of my study is that I not only investigate its effect on actual sectoral composition, as in Mehta (2022), but that my unique dataset allows me to understand how *entry* into manufacturing and services is affected differently by land market frictions. This complementary finding adds a new dimension in which manufacturing and services behave differently in frictional land markets.

Third, my paper contributes to the literature on the effects of SEZs in India, providing a novel explanation to appraise their relatively mediocre success. SEZs are one of the most popular development strategies; in 2006, there were 3,500 SEZs across 130 countries (Frick et al., 2019). They are also economically important: estimates suggest that SEZs were responsible for more than 20 percent of all exports and employed more than 60 million people in the mid-2000s. In the case of India however, there is mixed evidence on economic activity (Hyun and Ravi, 2018; Görg and Mulyukova, 2022; Gallé et al., 2023), and no effect on development outcomes such as education and infrastructure (Aggarwal, 2007; Alkon, 2018). Those papers that found negative effects on economic developments have rationalized this through the strict regulatory environment and the potential for corruption by local politicians (e.g. Levien (2012)). This paper proposes a complementary explanation, showing that the intensity of compulsory acquisition, and thereby the opportunity of private firms to avoid the land acquisition process, influences the sectoral composition and the performance of these SEZs.

Finally, its discussion of sectoral composition naturally relates to a set of papers describing India's growth puzzle and India's atypical structural transformation, characterised by services-led growth with a still-growing manufacturing sector.[3] Competing explanations for this phenomenon include costly skill accumulation (Chari et al., 2016), informality of the workforce (Djidonou and Foster-McGregor, 2022), or a combination of productivity shocks and income effects (Fan et al., 2023). This paper asserts that land market imperfections, due to their differing impact on manufacturing and services, might contribute to differences in factor reallocation from agriculture to these respective sectors and thereby influence structural transformation.

The next section discusses the related literature in more detail and highlights the contributions of this paper. In Section III, the institutional environment for SEZs, their tenuous relationship with land and the protest and subsequent policy changes will be elaborated upon. Section IV discusses the conceptual framework, Section V the data, Section VI the empirical methodology and Section VII contains the results. Finally, section VII will provide a conclusion and suggestions for future

[3] As noted in Fan et al. (2023), this pattern of structural transformation is not unique to India, as the combination of growth in services with limited industrialization is observed in other developing countries.

research.

II. Literature Review

As outlined above, this paper contributes to multiple strands of literature, which will be elaborated on below.

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, as the investments following land expropriation improved access to nearby villages, but 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).

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.[4] In contrast, my paper can inform on

[4] Strict land zoning regulation is an especially important barrier in developed countries: Herkenhoff et al. (2018)

the degree to which access to land is the main constraint private firms face.

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. The bulk of evidence on this relationship 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 imperfections hinders farms from achieving their optimal scale (Britos et al., 2022). Furthermore, the inefficiently small plot sizes generate underutilization of labour 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 labour 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 acquire additional land slowly over time rather than buying a sufficiently large plot before starting production. These manufacturing firms are 22 percent smaller than their counterparts in regions with more concentrated land ownership; this lack of expansion 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.

The only other study – that I am aware of – 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, 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 not only investigate the impact of the policy environment on firm-level outcomes, but

show how state-level land use regulations have depressed macroeconomic activity in the US.

that I can analyse entry directly. Understanding how land policy affects selection is of the utmost importance to contextualize the findings that already-established firms perform worse.

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. (2019)). 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 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. (2023) 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 considering land as an input in the production function, and attempting to catalogue the opportunity costs of these SEZs.

IV. India's atypical structural transformation

Finally, since my paper discusses reallocation within non-manufacturing in India, it naturally relates to a set of papers exploring India's atypical structural transformation. In general, structural transformation relates to factor reallocation across the three broad sectors: agriculture, manufacturing and services (McMillan and Rodrik, 2011; Herrendorf et al., 2014). This phenomenon differs significantly from India's factor reallocation in the last decades, which is characterised by

services-led growth with a still-growing manufacturing sector.[5] Chari et al. (2016) argued that this is due to costly skill accumulation rather than limited labour mobility. Relatedly, Djidonou and Foster-McGregor (2022) contended that the relative underperformance of the manufacturing sector in India is driven by informality, as labour reallocates from agriculture to the informal sector. According to Dehejia and Panagariya (2014), the growth puzzle can partly be explained by intersectoral linkages: they showed that the large growth in services in India is partly generated by the growth in manufacturing, which created a stronger internal market for services. Finally, Fan et al. (2023) analysed the growth of services in India, stating that the development process led to a productivity increase in services, and the growth was reinforced by increased demand due to income effects. My paper is the first to assert that compulsory acquisition, due to their differing impact on manufacturing and services, might contribute to differences in factor reallocation from agriculture to these respective sectors and thereby influence structural transformation.

III. 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 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 Export-Import policy was established as a precursor to the SEZ Act (Hyun and Ravi, 2018). The pre-existing EPZs were converted to SEZs, could process imports duty-free and did not need any license to import. At the same time, the focus shifted from exports to general processing, as evidenced by SEZ developments inland instead of close to the port. Several states, including "economically backward" states, introduced specific SEZ legislation. Nevertheless, it was not until after the ratification of the 2005 Special Economic Zone Act that the popularity and prevalence of SEZs fully materialized (Tewari, 2020). Established with the goal of increasing employment and thereby economic growth, 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).[6] Firms locating inside an SEZ can profit from duty-free imports, single window

[5] As noted in Fan et al. (2023), this pattern of structural transformation is not unique to India, as the combination of growth in services with limited industrialization is observed in other developing countries.

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

clearance and 15-year income tax benefits.[7] 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.

The development of an SEZ proceeds through three stages. First, the developer submits a proposal to the State Government of the proposed location, which is upon approval forwarded to the SEZ Board of Approval (BoA). The Board of Approval is appointed by the Central Government, and always consists of four high-level officers from the Ministry of Commerce and Industry, the Ministry of Home Affairs and the Ministry of Finance; the Director General of Foreign Trade; at most ten officers from relevant ministries including the Ministry of Law and Justice and the Ministry of Science and Technology; and a Professor in the Indian Institute of Management or the Indian Institute of Foreign Trade (Central Government of India, 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. Importantly, members are determined *ex officio*, meaning that they are nominated for the BoA by virtue of the office they hold. This also implies that one's term as BoA member comes to an end when they no longer hold said position. Moreover, all decisions on proposals are to be made with consensus.

The BoA meets, at a regularity they set themselves, multiple times a year to judge whether proposals are of sufficient quality. These proposals are, based on the subsample of proposals for which I have the actual submission date, more or less discussed in the order in which they were submitted. 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).[8] 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 to be discussed again at a later time.

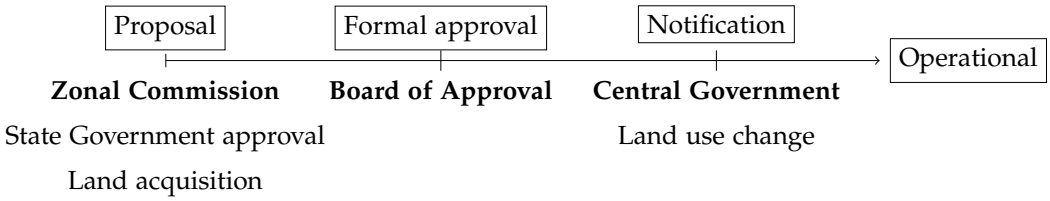
After the formal approval is received, the SEZ moves to the notification stage, which involves the Central Government changing the land use designation on the SEZ plot to industrial land. The Government does so if it believes that the SEZ will bring about economic development and will be for the greater good (Central Government of India, 2005). After notification, the first unit can be constructed and eventually start operating, which is the final phase of SEZ development. The developer then sets up their own SEZ Board of Approval, which then decides which firms to allow into the SEZ. The schematic development of a SEZ is given in Figure 1 below:

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

[7] The latter is only for units operating before 30 June 2020.

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

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



then operational SEZs equalled 87.45 billion dollars, which was a growth of 31 percent compared to the previous year and was almost 20 percent of India's total imports.[9] In total, as of 29 October 2022, the Board of Approval has considered 1,459 proposals, of which more than 700 were approved.

II. Land in SEZs

In India, acquiring land is a time-consuming and costly process. First, there is a lack of accurate and up-to-date land records, and thereby insecure property rights (Prabhakar et al., 2020). Moreover, land ownership in India is extremely fragmented, with an average parcel size one hundred times smaller than in the U.S. – 2.9 acres versus 234 acres (Sood, 2022). Finally, land conflicts constitute 25 percent of cases at the Supreme Court, with land disputes in general threatening investments worth 200 billion dollars (Wahi, 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. Anecdotal evidence suggests this fear was not unfounded, as many private developers stated that state investment in land was a prerequisite for many private developers (Levien, 2012). In anticipation of this, State Governments started to expropriate land for private developers, often before their proposal was to be discussed in the Board of Approval meetings. One tool that was used for this purpose the colonial Land Acquisition Act (LAA, 1894), which allowed the government to forcibly acquire land for "public purposes" (Singh, 2020). In 1984, this law was amended to allow the state to acquire land also on behalf of private investors. While the SEZ Act does not explicitly mention the LAA as a tool to facilitate private SEZs, both anecdotal evidence and state-specific SEZ policies and acts suggest that this strategy, and other land acquisition strategies such as through land banks, were used in abundance. Nine states have published either a state-specific SEZ policy or state-specific rules before 2007; the other states adhered to the national SEZ Act. Of these nine, seven states –

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

Chandigarh (2005), Gujarat (2004), Haryana (2006), Madhya Pradesh (2003), Maharashtra (2001), Tamil Nadu (2005) and West Bengal (2003) – have declared that the government would provide the necessary land; the first four states explicitly name the Land Acquistion Act as the appropriate method to do so. Upon completing the compulsory land acquisition, the State Governments would transfer this land at a lower cost to developers (Levien, 2012). Thus, the State Governments shielded private developers, and any firms locating in the SEZ, from the normally high transaction costs.

III. Protests and land acquisition reforms

To understand the effect of land transaction 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.[10] 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). Moreover, there were extensive reports of acts of sexual violence by the police officers.

This protest, and its violent ending, not just changed the trajectory for Nandigram but also for the rest of India. First, 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. Second, the West Bengal State Government would eventually lose the next election due to their involvement in repressing the protest (Patra, 2019). Finally, and most relevant for this paper, the Central Government revisited the SEZ policy. After three months, during which the BoA meetings were suspended, the Central Government announced that effective immediately, *forced* land acquisition was prohibited and landlosers must be compensated properly (SEZ Board of Approval, 2007). 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 Resettlement in October 2007, which advocates for land-for-land compensation, and preference to the landlosers for employment.[11] It wasn't until 2011 when both bills were

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

[11] The process eventually culminated in two new bills, introduced in Lok Sabha (Lower House) on 6 December 2007.

introduced in the combined Land Acquisition, Resettlement and Rehabilition Bill in 2011. 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).

IV. CONCEPTUAL FRAMEWORK

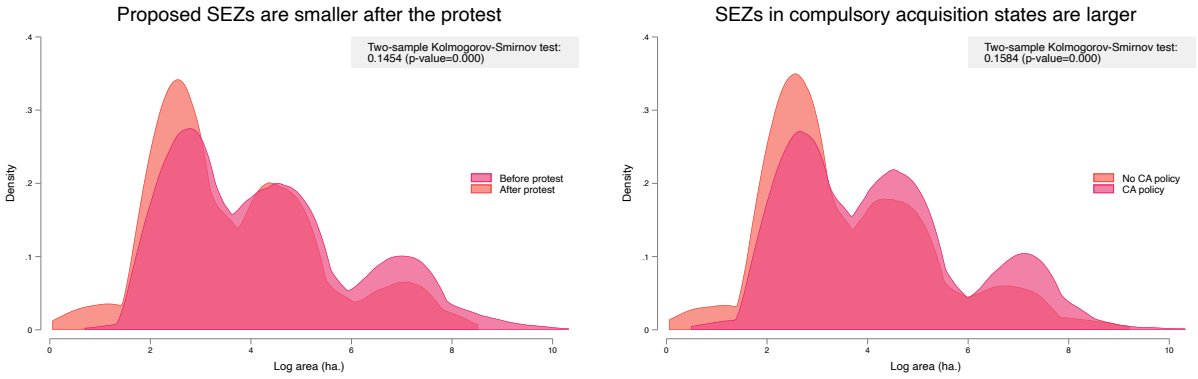
To understand the impact of a restriction of eminent domain on industrialisation of SEZs, we need a framework that connects (compulsory) land acquisition, private investment and local labour markets. A natural candidate are land assembly models such as Miceli and Sirmans (2007), where a private investor needs to acquire a large plot of land, and has to negotiate with a large number of landowners in this process. These models conclude that government intervention in land markets can prevent the holdout problem (Miceli, 2011). The holdout problem arises when a owner of a valuable resource decides to "hold out" on selling, despite the existence of a positive surplus between buyer and seller (Menezes and Pitchford, 2004). This prevents the resource from being put to more productive use, thereby detracting from allocative efficiency. Potential sources of this inefficiency are local monopoly power on the side of the landowners or an increase in negotiation costs. Even without efficiency considerations, the holdout problem occurs when the buying process proceeds sequentially, meaning that those landowners that are approached later have higher bargaining power than those approached initially. A speculative seller will then "hold out" in anticipation of a higher price (Sarkar, 2007). This implies private investors are potentially dissuaded from investing in the first place if the process is too costly, providing an economic justification for compulsory acquisition for both public and private projects Miceli and Sirmans (2007).[12]

The reform that placed restrictions on compulsory land acquisition corresponds to an increase in rehabilitation and resettlement and thereby negotiation costs. Moreover, this increase in land transaction costs was relatively larger for those developers in states that engaged in land

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.

[12] Empirical studies of speculative hold-out suggest that landowners that went to court during the land assembly process did obtain larger compensations (Pal et al., 2022). 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.

expropriation for SEZs. Even if the government would still undertake the negotiations with the numerous landowners, the higher costs of resettlement and rehabilitation would be borne by the developers.[13] Besides an incentive for investors to reduce the land size of their project, these models would predict that this reform mainly acts as an entry barrier and reduces the number of SEZs, especially by private investors. Moreover, this decrease would be strongest for (1) investments that require especially large plots of land and (2) locations where the cost increase is largest. Figure 2 provides suggestive evidence that substantiates this interpretation. The left-hand figure shows the size distribution of SEZs before and after the protest, where it is clear that SEZs discussed after the protest are smaller than before. Moreover, the right-hand figure shows that the distribution of SEZ size in states that committed to compulsory acquisition lies to the right of the same distribution for states that did not adopt a compulsory acquisition policy.



Author's calculations based on SEZ proposal data.

Figure 2: *Density plots of SEZ size*

Connecting this hypothesis to industrialisation is straightforward, as industrial sectors are generally more land-intensive than services. As mentioned before, services need on average 6.7 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). Table 1 shows that in my sample of SEZ proposals before the reform, the manufacturing SEZs are on average 19 times larger than services SEZs. Besides a difference in land intensity, there is limited albeit convincing evidence that manufacturing firms are especially impacted by land market frictions. Sood (2022) analysed how manufacturing firms resort to a land-biting strategy, in which firms first acquire a small plot of land and slowly expand their plot over the years. Furthermore, Duranton et al. (2016) estimated that the biggest source of misallocation for manufacturing firms was land, while the biggest source for services firms was labour. If manufacturing firms are relatively more constrained by

[13] This is for example explicitly mentioned in the SEZ policy of Uttar Pradesh (2008).

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.

Table 1: *Land area statistics by sector*

	N	Mean	Median	SD
Manufacturing	233	1206.3	346	2799.5
Services	409	64.1	20.2	153.4
Utilities	13	419.8	180	476.6
Total	655	477.5	52.8	1759.3

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

One limitation of these models is that the potentially foregone investment is always efficiency-enhancing; López and Clark (2013) showed that this increased discretion might induce opportunistic policymakers to engage in non-efficiency-enhancing eminent domain, which is suboptimal compared to voluntary exchange, and brings about rent-seeking by developers. If private developers are heterogeneous in quality or productivity, studying changes in entry decisions is not sufficient to understand the impact of eminent domain on industrialisation. A natural way to extend these models is to allow for heterogeneous productivity as in the stationary model of entry, exit and firm dynamics in Hopenhayn (1992) and later Melitz (2003). In this model, firms are faced with uncertainty about their productivity. Upon paying an entry cost, firms learn their productivity and decide whether to enter the market. 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).

In conclusion, the reform that placed restrictions on compulsory land acquisition corresponds to an increase in entry costs, especially in those nine states that committed to compulsory acquisition for SEZs. Importantly, I posit that this increase is higher for manufacturing than for services SEZs. At the same time, the higher entry barrier implies that the marginal entrant requires a higher discounted profit; the average new entrant should therefore be of higher productivity than before. 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: over time, the new manufacturing SEZ entrants will be of higher productivity, reflected in the quality of their proposals. Finally, for a more formal analysis of quality, I predict that SEZs after the reform generate more local employment.

V. Data and Descriptives

This section provides an overview of the data that is used in analysing the effect of the eminent domain reform on SEZ characteristics, entry and local development.

I. SEZ data

The main dataset is a unique dataset on 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 1,459 proposals for 1,119 distinct 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. For some SEZs, the proposal did not provide a disaggregated enough location; in those cases, I used data from Land Matrix (2023), OpenStreetMap or newspaper articles to georeference the SEZs at their exact location. I complement this data with similarly collected data from the corresponding meeting agendas of the BoA, which provide, for a subset of firms around the shock, information on initial proposal dates, state government approval and land possession (SEZ India, 2022a). I also collected data on whether the SEZ developer is a public or private entity and the exact location at the village level.[14] I also add the notification date for all SEZs as published by the BoA. Finally, for those SEZs that were notified in 2014, the Ministry of Commerce and Industry published information on the share of vacant land in each zone (SEZ India, 2014).

It is important to note that while I do know exactly when which proposal was discussed, I have almost no information on when the proposal was submitted. This matters because while the SEZ Act was ratified on 23 June 2005, but the first BoA meeting did not take place on 17 March 2006. I thus have a nine-month period in which proposals could be submitted but were not discussed, which generated an unusually large number of proposals to be discussed in the first few meetings. This bunching is illustrated in Figure 3, which shows a substantial decline in the

[14] 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).

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 discuss how this affects my empirical strategy in the next section.

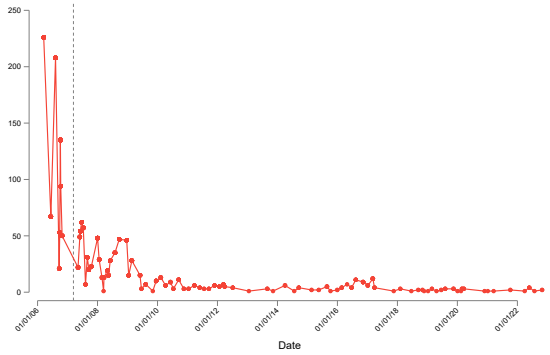


Figure 3: *Number of SEZ proposals per meeting*

I then spatially join the georeferenced SEZ data with the Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG) (Asher et al., 2021). The unit of aggregation in the SHRUG is a shrid; this is a location-based identifier that contains at least one village or town. A certain level of aggregation is unavoidable for panel data, as Indian villages are often subjected to changing boundaries. SHRUG provides consistent administrative boundaries for over 500,000 villages and 8,000 towns between 1991-2021. 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. Using their rich open source data, I can then add economic and socio-economic variables at the shrid-level. I use one round of the *Primary Census Abstract* (PCA, 2001) which contains information on municipality population, the labour force and agricultural employment. I also use the *Economic Census* (EC, 1998-2013), which is a complete count of all economic units (both agricultural and non-agricultural) in the country except for crop production, plantation and production solely for direct consumption and takes place once every seven to eight years. The EC contains information for each village-industry combination on hired and non-hired employment, the number of active firms, the share of firms using power and the share of public firms. To proxy for credit frictions, I compute the number of bank branches at the municipality level using Garg and Gupta (2020). Finally, I add night lights data (2000-2020) at the shrid level from Li et al. (2021).

Another important variable is a measure of land fragmentation, as that should relate directly to land acquisition costs. To that end, I scraped the *Agricultural Census* (AC, 2000), which provides me with the exact plot size distribution, crop types and irrigation in India's subdistricts. A

subdistrict is the third administrative boundary level in India; India consists of more than 6,000 subdistricts. This dataset is publicly available, but one needs to download the data separately for each subdistrict-year combination.[15] I then 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 use the Gini coefficient and the percentile ratio of the top and bottom ten percent. Table 2 summarizes all variables in this dataset at the proposal-meeting level.

Finally, I obtain information on state propensity to engage in eminent domain by parsing through all state-specific SEZ acts, rules and policies, as there is – to my knowledge – 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."[16] Figure 4 shows the variation across states in their commitment to eminent domain for SEZs, overlayed with the proposal data.

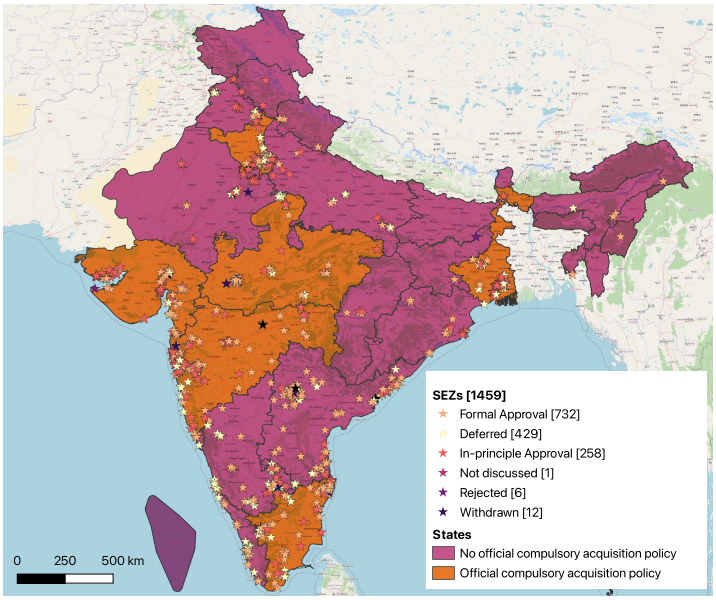


Figure 4: *SEZ proposals and states' propensity to expropriation for SEZs*

One concern is that SEZs, and especially their industrial composition, are different depending on whether the state they proposed to locate in engages in eminent domain for SEZs. Table 3

[15] I obtained these records from https://agcensus.dacnet.nic.in.

[16] The Haryana SEZ Act (2005) was retrieved from https://www.indiacode.nic.in.

Table 2: *Characteristics of SEZ proposals and their locations.*

	N	Mean	Median	SD
SEZ Characteristics				
Log size (ha.)	1462	3.96	3.69	1.75
Public	1466	0.11	0	0.32
Manufacturing	1465	0.31	0	0.46
Services	1465	0.67	1	0.47
Utilities	1465	0.021	0	0.14
Location Characteristics				
Log agricultural employment (2001)	1452	10.2	10.4	1.01
Log population (2001)	1452	13.1	12.9	1.29
Log size of subdistrict (ha.)	1452	10.4	10.8	1.64
Log manufacturing employment (2005)	1436	9.62	9.40	1.63
Log services employment (2005)	1436	10.5	10.2	1.57
Log labour force (2001)	1452	12.1	12.0	1.22
Log number of banks (2005)	1433	2.70	2.64	1.59
Log distance to airport (km)	1467	3.03	3.01	0.87
Log distance to port (km)	1467	4.74	5.37	1.37
Log distance to power plant (km)	1467	2.50	2.60	0.87
Log distance to city (>500K, km)	1467	3.24	3.07	0.99
Log distance to highway (km)	1467	0.74	0.65	0.54
Log distance to railway (km)	1467	1.58	1.61	0.88
At least one bank (2005)	516	1	1	0
Log number of banks (2005)	1433	2.70	2.64	1.59
Land concentration (Gini)	1224	0.51	0.51	0.091

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 Gini 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.

shows how SEZs discussed before the protest differ between states with and states without a compulsory acquisition policy. This confirms first of all what we saw in Figure 2: SEZs in treated states are on average 68 percent larger than their counterparts in untreated states. Reassuringly, all other SEZ characteristics in the first panel show no significant differences between treated and control states; the exception here is that states that engaged in eminent domain see significantly more proposals for oil and gas and power generation SEZs. Since these types of SEZs are a minority, with only 30 proposals in the whole sample, I am not too worried about this being a threat to identification. 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, distance to a power plant and presence of banks. This is not surprising: Figure 8 showed, there is a mass of SEZs in states that are more inland and engage in compulsory acquisition, explaining why these are further away from the port. 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.

II. Municipality data

The second set of hypotheses concerns an increase in quality or productivity of new SEZ entrants. I investigate whether these SEZs engendered different changes in local labour market conditions using two rounds of the Economic Census (2005-2013), which as mentioned before contains the universe of firms in the non-agricultural sector. Because the last 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.[17] 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 5.

To identify the local labour 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. (2023); Görg and Mulyukova (2022), as there is no data on exact SEZ boundaries. I assume all SEZs are circular around the precisely georeferenced location. Then, using the exact size of the SEZ, I computed the radius and drew a buffer around the central point to approximate the size of the SEZ. After I confirmed

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

Table 3: *Characteristics of pre-reform proposals by state's compulsory acquisition policy*

	No CA policy		CA policy		Difference	
Log size (ha.)	3.998	(1.684)	4.519	(1.846)	0.521**	(0.199)
Public	0.110	(0.314)	0.144	(0.351)	0.033	(0.037)
Manufacturing	0.330	(0.471)	0.376	(0.485)	0.046	(0.070)
Services	0.667	(0.472)	0.589	(0.493)	-0.078	(0.072)
Utilities	0.003	(0.057)	0.034	(0.183)	0.031**	(0.013)
Log population (2001)	13.096	(1.383)	13.225	(1.163)	0.129	(0.112)
Log labour force (2001)	12.127	(1.336)	12.258	(1.116)	0.131	(0.093)
Log agricultural employment (2001)	10.034	(1.098)	10.253	(0.982)	0.219	(0.137)
Log manufacturing employment (2005)	9.589	(1.835)	9.771	(1.402)	0.182	(0.140)
Log services employment (2005)	10.626	(1.747)	10.605	(1.366)	-0.021	(0.147)
Log distance to airport (km)	2.900	(0.927)	3.051	(0.826)	0.151	(0.081)
Log distance to port (km)	5.233	(1.146)	4.470	(1.364)	-0.763***	(0.096)
Log distance to power plant (km)	2.463	(0.796)	2.620	(0.801)	0.157**	(0.059)
Log distance to city (>500K, km)	3.228	(0.963)	3.118	(0.906)	-0.110	(0.076)
Log distance to highway (km)	0.796	(0.579)	0.677	(0.491)	-0.120	(0.068)
Log distance to railway (km)	1.508	(0.785)	1.560	(0.884)	0.052	(0.035)
At least one bank (2005)	0.328	(0.470)	0.394	(0.489)	0.066*	(0.032)
Log number of banks (2005)	2.245	(1.463)	3.159	(1.502)	0.914***	(0.115)
Land concentration (Gini)	0.524	(0.087)	0.499	(0.090)	-0.025	(0.015)
Observations	308		348		656	

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. 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 Gini coefficient 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.

that the villages covered by the buffer were indeed SEZ-hosting municipalities, I drew 10 distance bins of five kilometres each around each SEZ and restricted the municipality sample to those that were within these bins. The final sample contains 60,137 distinct municipalities, which I classify into these distance bins based on their nearest SEZ. Table 4 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 kilometres from an SEZ, and the directly treated villages. The balancing table clearly shows

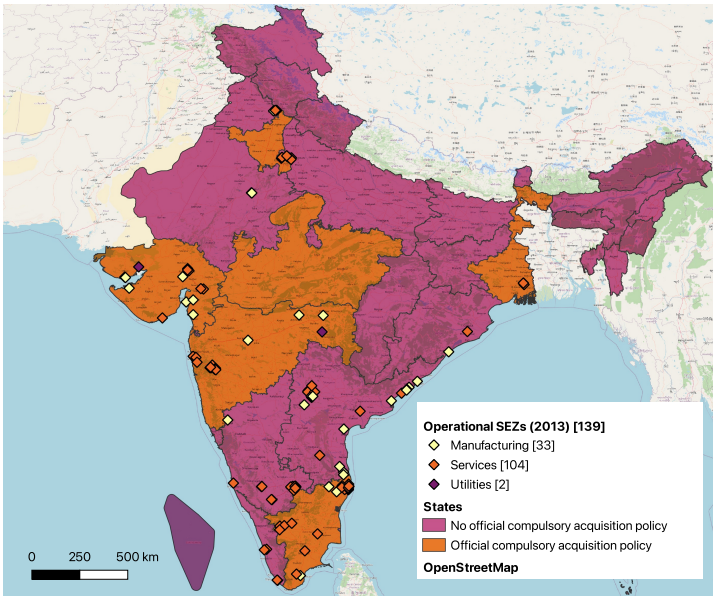


Figure 5: *Operational SEZs in 2013*

that SEZ-hosting villages are closer to virtually all amenities listed, and are characterised by higher formal employment and population. To ensure that the results will not be driven by nonparallel trends, I follow Gallé et al. (2023) and control for time trends in these variables.

VI. Empirical methodology

I. *SEZ analysis*

To analyse the impact of the eminent domain reform of SEZ entry, I use a difference-in-differences estimation strategy. Specifically, I compare the change in proposal characteristics in states with compulsory acquisition policy after the reform to the states in which such policy was not adopted. This results in the following regression equation:

$$Y_{i(rst)} = \beta Post_t \cdot LA_s + \mathbb{X}_{rs}\gamma + \alpha_s + \alpha_y + \epsilon_{i(rst)}, \tag{1}$$

where $Y_{i(rst)}$ refers to any outcome, sector choice or area, of a proposal for SEZ i in region r in state s discussed at a meeting at time t. $Post_t$ is a dummy designating whether the proposal i was discussed before or after the protest and LA_s equals 1 if the state s ex ante committed to providing land to SEZ developers. I furthermore control for variables that influence the location choice of SEZ developers in $\mathbb{X}_{rs}$, including subdistrict-level population and labour force in 2001, manufacturing and services employment in 2005, distances to places of interest and the Gini coefficient that measures land concentration. I add year fixed effects α_y and state or subdistrict fixed effects α_s or

Table 4: *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 distance to	3.075	3.130	3.242	3.355	3.468	3.540	3.625	3.727	3.812	3.873	3.937	0.465***
airport (km)	(0.826)	(0.792)	(0.737)	(0.700)	(0.633)	(0.592)	(0.534)	(0.500)	(0.496)	(0.528)	(0.504)	(0.093)
Log distance to	4.308	4.652	4.784	4.823	4.871	4.973	5.064	5.098	5.099	5.090	5.133	0.665***
port (km)	(1.350)	(1.206)	(1.142)	(1.084)	(1.052)	(1.001)	(0.946)	(0.913)	(0.902)	(0.898)	(0.887)	(0.163)
Log distance to	0.812	0.998	1.119	1.162	1.185	1.196	1.201	1.215	1.214	1.269	1.329	0.384***
highway (km)	(0.533)	(0.596)	(0.623)	(0.636)	(0.640)	(0.647)	(0.633)	(0.638)	(0.656)	(0.684)	(0.689)	(0.056)
Log distance to	3.529	3.478	3.525	3.636	3.729	3.764	3.828	3.907	4.001	4.062	4.113	0.235***
city (>500K, km)	(0.907)	(0.816)	(0.786)	(0.741)	(0.699)	(0.647)	(0.613)	(0.558)	(0.520)	(0.495)	(0.493)	(0.085)
Log distance to	1.655	1.681	1.820	1.920	1.983	2.077	2.178	2.257	2.308	2.337	2.392	0.422***
railway (km)	(0.803)	(0.759)	(0.742)	(0.762)	(0.812)	(0.842)	(0.842)	(0.856)	(0.881)	(0.896)	(0.913)	(0.105)
Log distance to	2.490	2.651	2.749	2.822	2.868	2.922	3.027	3.101	3.143	3.180	3.205	0.431***
power plant (km)	(0.967)	(0.794)	(0.755)	(0.750)	(0.745)	(0.733)	(0.703)	(0.690)	(0.696)	(0.713)	(0.693)	(0.119)
Log formal	4.552	3.564	3.106	2.966	2.873	2.763	2.633	2.685	2.632	2.576	2.568	-1.789***
employment	(3.008)	(2.318)	(1.988)	(1.893)	(1.870)	(1.850)	(1.727)	(1.788)	(1.729)	(1.711)	(1.726)	(0.306)
Log municipality	8.128	7.412	7.132	7.074	7.020	6.961	6.867	6.821	6.842	6.787	6.777	-1.166***
population	(1.950)	(1.423)	(1.214)	(1.161)	(1.197)	(1.266)	(1.262)	(1.293)	(1.256)	(1.258)	(1.274)	(0.206)
Observations	430	3,122	5,986	8,026	10,226	12,294	14,306	15,526	16,552	17,312	16,478	12,724

This table reports the mean and standard deviation of several location characteristics for municipalities at different distance bins to their nearest SEZ. Column (12) shows the difference-in-means between villages 20-25 kilometres 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 is retrieved from the 2001 Primary Census Abstract and formal employment is taken from the 2005 Economic Census. Standard errors are clustered at the respective SEZ level.

α_r respectively. As mentioned before, I only observe proposals at the meeting level; to account for the fact that the number of proposals discussed per meeting varies dramatically over time, I cluster the standard errors at the meeting level. Ideally, the standard errors should be clustered at the treatment level, which would be states in my setting, but there are fewer than thirty states that ever see an SEZ so this strategy is untenable. Instead, I can cluster on the subdistrict-level or on the SEZ level. All results are robust to these clustering strategies.

In what follows, I will parametrically test the null-hypothesis that $\beta = 0$ for three outcome variables: log SEZ size, the probability of a proposal being for a manufacturing SEZ and the respective probability for services. For the latter two outcome variables, I will use a linear probability model. If $\beta > 0$, this implies that after the protest, the outcome variable is relatively larger in states that employed compulsory acquisition policies for SEZs.

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. However, the experimental assignment of observations to treatment and control are not truly random. Even though pre-reform SEZs in treated and control states are mostly comparable as shown in Table 3, there are a few marked differences, especially in SEZ size and utilities as a broad sector choice. Controlling for these will restore balance in the observables, but there might still be time-varying unobserved heterogeneity. One specific concern is a violation of the parallel trends assumption for entry into manufacturing and services before the reform. In Figure 6 I plot, for both manufacturing and services, the coefficient per quarter relative to the baseline meeting just before the protest. This regression includes controls and state fixed effects, and the standard errors are clustered at the date level. The mass of proposals before the protest makes it very difficult to properly test for trends; 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. The figure also shows that while the coefficients generally have the expected sign, they jump around a lot. Since I cannot control for year fixed effects in this specification, this might again point to time-varying heterogeneity. In the Results section, I will show the results are robust to different windows around the timing of the protest.

Another threat to identification is a dramatic change in the Board of Approval's strategy at the time of the reform, implying 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 suggest a different strategy. Moreover, as the Board contains a variety of members from different political parties – and even some without official

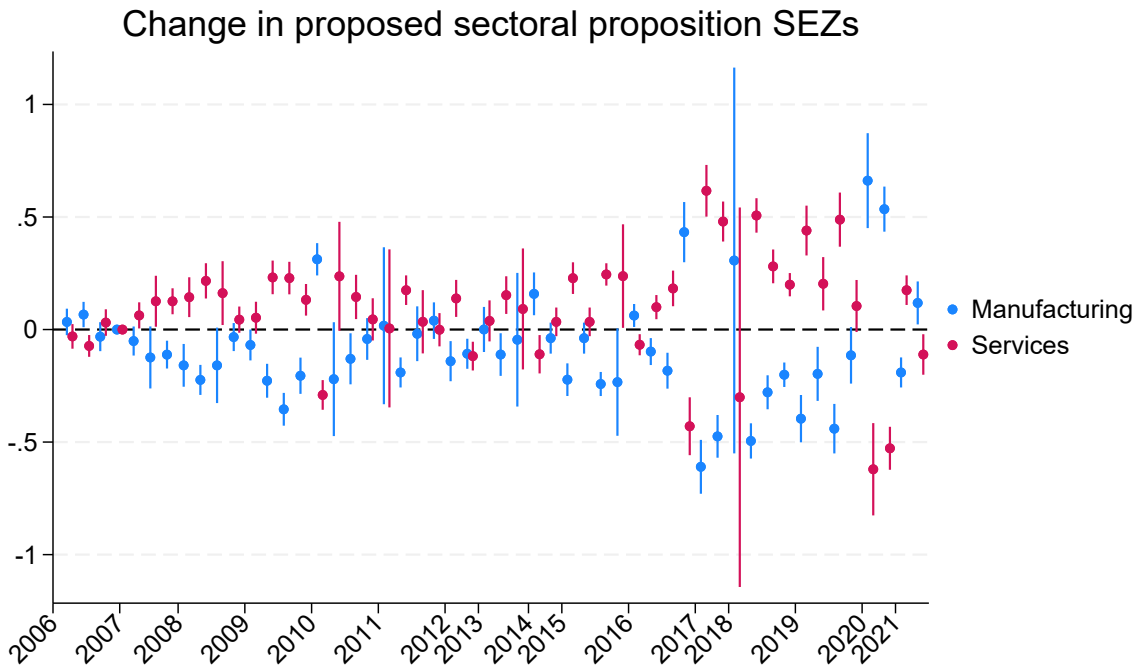


Figure 6: *Proposed sectoral composition SEZs over time*

political affiliation – and all decisions need to be reached with general consensus, it is unlikely that members can start favour certain states after the reform.

Another threat to identification is measurement error in the treatment variable. Because I cannot observe directly how much 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) some states might, despite the policy, only infrequently use compulsory acquisition for SEZs and (2) some states 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 mainly use land provision to attract large investments to their state. This is especially relevant for investors looking to develop an SEZ in West Bengal, which is characterised by the highest degree of land fragmentation in India (Sarkar, 2007). 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.

A final concern is SUTVA violations, which in this context would imply that entry decisions are affected by previous entrants. A decrease in manufacturing entry in treated states could for example because of fierce local competition with other already developed SEZs. There are two elements of my setting that help subside this concern. First, as can been seen from comparig Figure 4 and 5, India (unfortunately) boasts a very low proposal-to-operational conversion; while there might be many proposed or even formally approved SEZs, the corresponding number for actually operational SEZs is significantly lower. Even more recently, on 31 March 2019, there were only 219 operational SEZs. Second, if choosing an SEZ location is related to existing entrants, the relationship seems to be the other way around, with Tewari (2020) documenting the development of large industrial belts that consists of multiple SEZs. In a future version of this paper, I will add distances to other SEZs as a control variable to check for this directly.

I.i. Municipality analysis

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. Unfortunately, the use of this data restricts my analysis to the 139 SEZs that were operational before 2013. To capture the both the direct treatment effect – i.e. the additional employment in the SEZ – and the local spillovers of an SEZ, I follow Galle (2023) and employ a spatial difference-in-differences strategy. This involves creating bins of five kilometres around each SEZ, and comparing the villages in which an SEZ became operational between 2005 and 2013 to those nearby villages that do not have an SEZ. I then extend their analysis by studying this difference between SEZs that were first proposed before the reform versus those that were proposed afterwards. Specifically, my regression equation is:

$$\begin{aligned} Y_{mt} = \sum_{d=0, d\neq 5}^{10} \beta_d (D_{[d_m=d]} \times PostProtest_{mt}) + \gamma' (\mathbb{X}_m \times PostProtest_{mt}) \\ + PostProtest_{mt} + \alpha_r + \alpha_t + \epsilon_{mt}, \end{aligned} \tag{2}$$

where Y_{mt} is any outcome at municipality m at time t. $D_{[d_m=d]}$ indicates whether municipality m is in distance bin d to an operational SEZ in the post-treatment year, where $d_m = 0$ indicates municipalities that host an SEZ, $d_m = 1$ reflects municipalities up to five kilometres from an SEZ, until $d_m = 10$ which contains municipalities 50 kilometres away from an SEZ. This is multiplied with the treatment dummy $PostProtest_{mt}$, which equals one if municipality m is near to an operational SEZ that was proposed after the protest. The model further includes municipality and

year fixed effects, and, to capture any outcome trends that are correlated to baseline characteristics, the controls listed in Table 4 at the municipality level $\mathbb{X}_m$ interacted with the treatment dummy.

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. 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).

To further help mitigate any concerns about violation of the conditional mean independence assumption, I have several strategies planned for the near future. First, I will run a placebo test by estimating Equation 2 using the Economic Census in 1998 and 2005. This supports the parallel trends assumption if the results do not show any significant pattern. Second, to test more formally for different pre-trends between SEZ villages and their control group before and after the reform, I undertake an event study analysis using annual night lights data, which is a good proxy for economic activity (Hyun and Ravi, 2018). Taking the date of notification as the reference point, as from that moment onwards one can start building, this will allow me to track economic activity in treated villages over time.

VII. RESULTS

I. SEZ outcomes

In this section, I provide evidence that the policy shock changed the proposed and sectoral composition of SEZs.

Table 5 shows the results of estimating Equation 1 for the universe of SEZ proposals. The first three columns show the results for this equation with Manufacturing as the outcome variable, where Manufacturing is a dummy that equals one if the SEZ proposal was for a sector within manufacturing. In column (1), we see that the reduction in manufacturing SEZ proposals after the protest is significantly higher in states that explicitly committed to compulsory acquisition for SEZs. Column (2) adds year fixed effects, and in the third column subdistrict fixed effects are used in lieu of state fixed effects and subdistrict-level controls. The preferred baseline specification in the third column shows that the probability of a SEZ proposal being in manufacturing is 13.6 percentage point lower after the reform in those states that use compulsory acquisition for SEZs.

This translates to a decrease of more than one third, as the baseline average is 35 pecent. It should be noted that the number of observations changes somewhat across specification due to missing data points for certain controls such as the Gini coefficient, while in column (3) the number of observations is less than the full sample due to the presence of subdistrict singletons. The results are robust to excluding controls with missings for some observations. The last three columns show that the share of services SEZs (or rather, the probability that an SEZ is in the services sector) increases by 11.4 to 12.7 percentage points depending on the specification, from a baseline average of 62.9 percent.

Table 5: *Change in proposed SEZ composition*

	Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)
PostProtest=1	-0.0638**			0.0577*		
	(0.0297)			(0.0311)		
PostProtest=1	-0.0986**	-0.0984*	-0.139**	0.116**	0.115**	0.129**
× Compulsory acquisition policy=1	(0.0493)	(0.0526)	(0.0612)	(0.0508)	(0.0539)	(0.0539)
Location controls	Yes	Yes	No	Yes	Yes	No
State FE	Yes	Yes	No	Yes	Yes	No
Year FE	No	Yes	Yes	No	Yes	Yes
Subdistrict FE	No	No	Yes	No	No	Yes
Observations	1204	1204	1283	1204	1204	1283
R-squared	0.283	0.292	0.559	0.299	0.306	0.582

The dependent variable is a dummy that equals one if the approved SEZ is for *Manufacturing* or *Services*, respectively. *PostProtest* is a dummy that takes the value 1 if the meeting in which the proposal is discussed happens after 17 March 2007. The controls include measures of subdistrict-level population, labour force and agricultural employment from the 2001 Primary Census Abstract, and data on manufacturing and services 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 regressions control for local land concentration using data from the 2000 Agricultural Census, which provides the plot size distribution across eight size categories in a subdistrict. Standard errors, clustered at the meeting level, are in parentheses.

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

Moving to the next step in the SEZ development process, Table 6 displays the results of estimating Equation 1 for those SEZs that moved to Formal Approval. Here, the effect of the protest seems to be fully driven by changes in entry in states that mediated land acquisition. The preferred specification for Manufacturing in column (3) indicates that the likelihood of an approved SEZ being in manufacturing decreases by 15.1 percentage points in states that carried out

compulsory acquisition for SEZs. The last three columns show that this is almost fully mirrored by the effect on services entry, with the share of services increasing by 12.4 percentage points in states with higher land acquisition intensity.

Table 6: *Change in approved SEZ composition*

	Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)
PostProtest=1	-0.0104			-0.00199		
	(0.0394)			(0.0394)		
PostProtest=1	-0.171**	-0.157**	-0.151***	0.174**	0.158**	0.124**
× Compulsory acquisition policy=1	(0.0692)	(0.0778)	(0.0555)	(0.0681)	(0.0768)	(0.0518)
Location controls	Yes	Yes	No	Yes	Yes	No
State FE	Yes	Yes	No	Yes	Yes	No
Year FE	No	Yes	Yes	No	Yes	Yes
Subdistrict FE	No	No	Yes	No	No	Yes
Observations	605	604	565	605	604	565
R-squared	0.281	0.290	0.503	0.301	0.310	0.513

Observations are restricted to those that eventually were granted formal approval. The dependent variable is a dummy that equals one if the approved SEZ is for *Manufacturing* or *Services*, respectively. *PostProtest* is a dummy that takes the value 1 if the meeting in which the proposal is discussed happens after 17 March 2007. The controls include measures of subdistrict-level population, labour force and agricultural employment from the 2001 Primary Census Abstract, and data on manufacturing and services 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 regressions control for local land concentration using data from the 2000 Agricultural Census, which provides the plot size distribution across eight size categories in a subdistrict. Standard errors, clustered at the meeting level, are in parentheses.
$^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

Third, Table 7 displays the results of estimating Equation 1 for those SEZs that were eventually notified, meaning that the Central Government also approved of the SEZ and the land use was officially changed to industrial or commercial. Again, column (1) shows that the effect of the protest seems to be fully driven by changes in entry in states that mediated land acquisition: the coefficient there is almost five times larger and significant. The preferred specification for Manufacturing in column (3) indicates that the likelihood of a notified SEZ being in manufacturing decreases by 11.1 percentage point in states that carried out compulsory acquisition for SEZs, although this finding is not significant. The last three columns show that this is almost fully mirrored by the effect on services entry, with the share of services increasing by 8.6 to 11.8 percentage points in states with higher land acquisition intensity.

Table 7: *Change in notified SEZ composition*

	Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)
PostProtest=1	-0.0331			0.0343		
	(0.0428)			(0.0414)		
PostProtest=1	-0.163**	-0.138	-0.111	0.118	0.0931	0.0861
× Compulsory acquisition policy=1	(0.0714)	(0.0831)	(0.0680)	(0.0739)	(0.0866)	(0.0687)
Location controls	Yes	Yes	No	Yes	Yes	No
State FE	Yes	Yes	No	Yes	Yes	No
Year FE	No	Yes	Yes	No	Yes	Yes
Subdistrict FE	No	No	Yes	No	No	Yes
Observations	393	392	349	393	392	349
R-squared	0.299	0.312	0.474	0.315	0.327	0.479

Observations are restricted to those that eventually were notified. The dependent variable is a dummy that equals one if the approved SEZ is for *Manufacturing* or *Services*, respectively. *PostProtest* is a dummy that takes the value 1 if the meeting in which the proposal is discussed happens after 17 March 2007. The controls include measures of subdistrict-level population, labour force and agricultural employment from the 2001 Primary Census Abstract, and data on manufacturing and services 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 regressions control for local land concentration using data from the 2000 Agricultural Census, which provides the plot size distribution across eight size categories in a subdistrict. Standard errors, clustered at the meeting level, are in parentheses.
$^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

The final stage of development is the SEZ becoming operational, meaning that the SEZ contains at least one operating firm. Repeating the analysis above, but now restricting the observations to those SEZs that became operational, Table 8 shows that again, there is a decrease in the share of manufacturing SEZs in states with compulsory acquisition policies after the protest. The coefficient is now slightly less stable in size across all three specifications, especially for those where manufacturing is the dependent variable, but the results are not significant. It should be noted that, even though both pre- and post-reform the share of SEZs becoming operational is similar, the total sample is relatively small compared to the SEZs reaching earlier stages. It might thus be that the analysis is underpowered. On the other hand, it might be that the difference pre- and post-reform is just not that unlikely. In that case, it might be that the relatively higher entry barrier for manufacturing might have translated into fewer attempts at entry, but those who entered after the reform were of higher quality or potential, such that the actual sectoral composition of SEZs is not dramatically different after the reform. This will be explored in the

next subsection.

Table 8: *Change in operational SEZ composition*

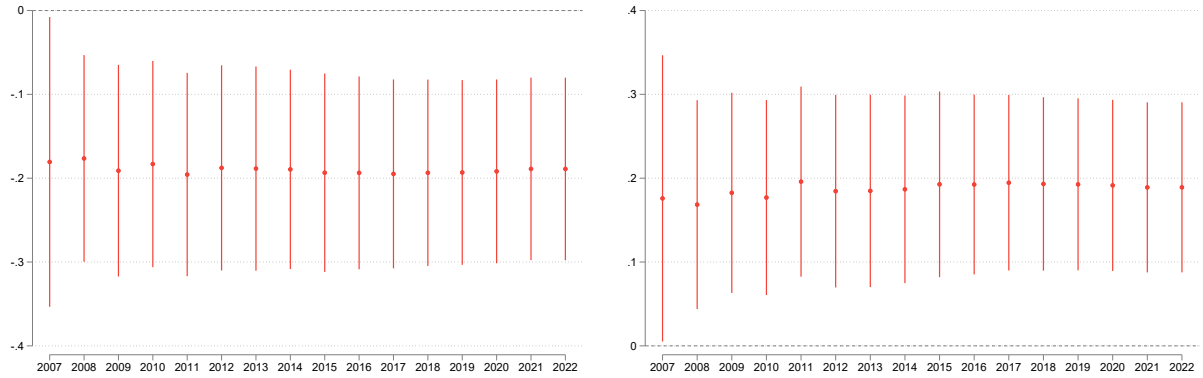
	Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)
PostProtest=1	-0.00485			0.00259		
	(0.0607)			(0.0602)		
PostProtest=1	-0.120	-0.213	-0.0979	0.0790	0.172	0.102
× Compulsory acquisition policy=1	(0.113)	(0.127)	(0.0772)	(0.117)	(0.129)	(0.0805)
Location controls	Yes	Yes	No	Yes	Yes	No
State FE	Yes	Yes	No	Yes	Yes	No
Year FE	No	Yes	Yes	No	Yes	Yes
Subdistrict FE	No	No	Yes	No	No	Yes
Observations	211	209	183	211	209	183
R-squared	0.343	0.375	0.604	0.376	0.416	0.605

Observations are restricted to those that eventually became operational. The dependent variable is a dummy that equals one if the approved SEZ is for *Manufacturing* or *Services*, respectively. *PostProtest* is a dummy that takes the value 1 if the meeting in which the proposal is discussed happens after 17 March 2007. The controls include measures of subdistrict-level population, labour force and agricultural employment from the 2001 Primary Census Abstract, and data on manufacturing and services 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 regressions control for local land concentration using data from the 2000 Agricultural Census, which provides the plot size distribution across eight size categories in a subdistrict. Standard errors, clustered at the meeting level, are in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

Robustness To substantiate the claim that the results are not driven by time-varying heterogeneity, especially event far in the future, I estimate the preferred version of Equation 1 – with subdistrict-fixed effects – across different sample selection. Specifically, I first limit my sample to all proposals before 31 December 2007, then extend the sample to 31 December 2008, until 31 December 2022. The coefficient on the treatment variable, with probability of entry for a manufacturing or services SEZ respectively, are plotted in Figure 7. Both coefficients are robust to the different sample specifications.

II. Local outcomes

This section describes the results from estimating Equation 2 with log employment as outcome. As in Gallé et al. (2023), I compare the villages in which an SEZ became operational between 2005



(a) *Manufacturing SEZs* **(b)** *Services SEZs*

Figure 7: *Effects of reform on entry for different sample selections*

and 2013 to those nearby villages that do not have an SEZ to identify the spatial spillovers of SEZs on local development. I however extend their analysis by computing the difference between operational SEZs that were first proposed before the reform versus those that were proposed afterwards. Figure 8 shows the result of estimating Equation 2 for the full sample; I find a positive coefficient for villages up to 10 kilometres from an SEZ but none of the results are significant.

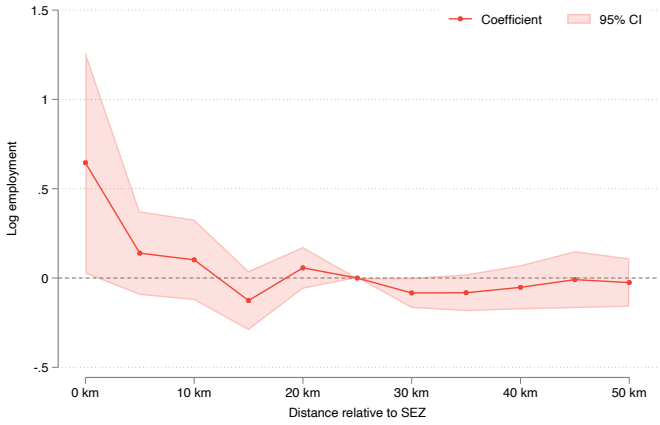
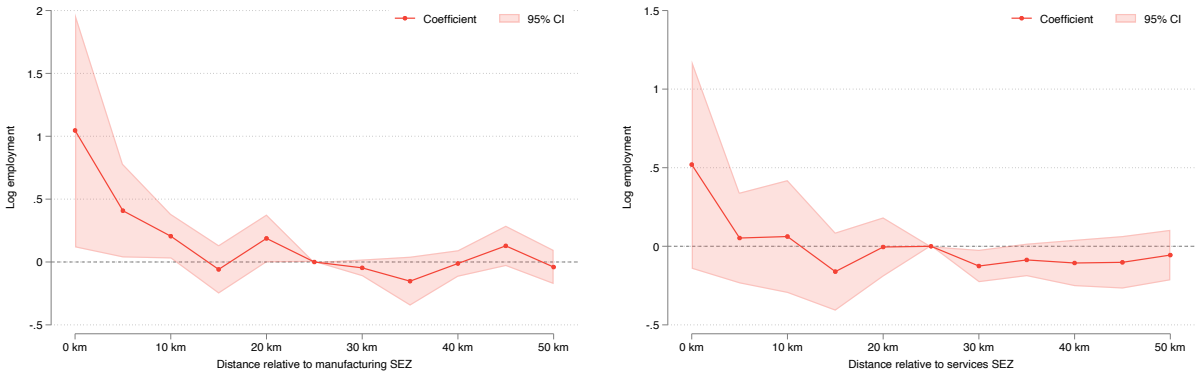


Figure 8: *SEZs proposed after the protest do not generate significant employment effects*

However, given that manufacturing and services were affected differently by the reform, they should be analysed separately. These results are displayed in Figure 9. Operational manufacturing SEZs that were proposed after the protest generated significantly more employment in the SEZ-hosting municipalities and nearby villages up to 10 kilometres away, with no differing effects at distances beyond that. These coefficients are substantial: for SEZ-hosting villages, the coefficient is slightly more than 1, implying an increase in employment by more than 171 percent. One



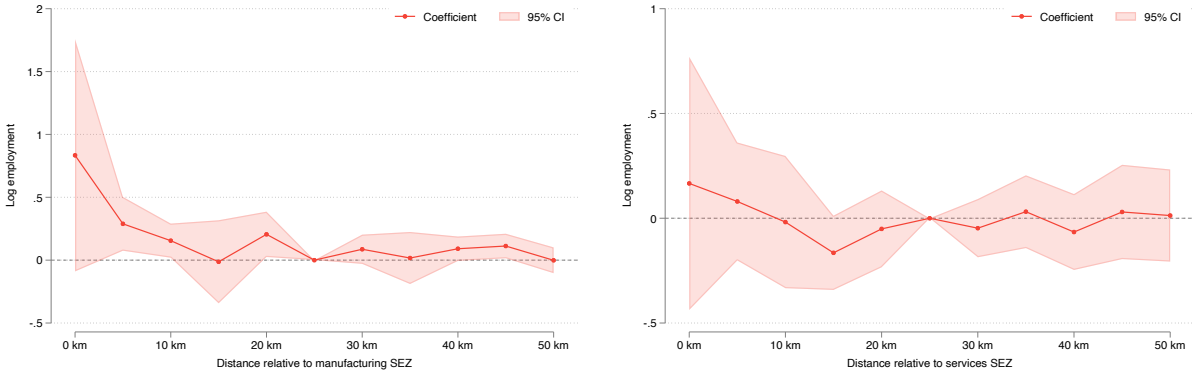
(a) *Manufacturing SEZs* **(b)** *Services SEZs*

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

reason for this might be the fact that some municipalities host multiple SEZs. Specifying spatial spillovers by creating concentric rings should, conditional on parallel trends, lead to an unbiased estimate of the direct treatment effect – misspecifiying the spatial spillovers may bias the estimates of the indirect treatment effectsButts (2023). However, as the sample divides the municipalities based on their nearest SEZ, I cannot capture these additive spillovers on the directly treated. In future work, I will carefully take additive spillovers into account as proposed by Butts (2023) and reestimate Equation 2 with only those villages that host one SEZ. Nevertheless, even if the effect size is quite large, the number of villages with multiple SEZs is not enough to explain this full effect–it is clear manufacturing SEZs proposed after the protest generated more employment than their older counterparts. For services however, there is also a positive coefficient for these nearby villages, albeit insignificant and at half the size of the corresponding estimates for manufacturing SEZs. Thus, this analysis provides tentative evidence that the compulsory acquisition reform might have reduced entry for manufacturing SEZs, but that those who do enter generate more local employment.

Robustness As mentioned before, if the specification without controls provides similar results as the preferred specification, this supports the claim that locational differences do not add significant bias to the estimates. Figures 10 and 11 show the results of this exercise: even though the estimates are slightly less robust, the general pattern is comparable to the main results. Nevertheless, as mentioned before, to fully substantiate the claim that the conditional mean independence assumption is not violated, the other strategies need to be executed as well.

Figure 10: *SEZs proposed after the protest do not generate significant employment effects*



(a) *Manufacturing SEZs* **(b)** *Services SEZs*

Figure 11: *Employment effects on nearby villages from operational SEZs without controls*

VIII. Conclusion

Compulsory land acquisition by the government, also known as expropriation or use of eminent domain, has been a long-standing practice for governments across the world and levels of development. In the last few decades, we have observed an increasing number of transition economies utilizing compulsory acquisition as an integral part of industrial policy. The rationale for this is that the land markets are sufficiently imperfect that prohibitively high land transaction costs dissuade private investment. In that setting, it might be efficient for the government to leverage eminent domain to stimulate economic activity.

This paper is the first to provide quasi-experimental evidence on the impact of compulsory acquisition, or rather, a restriction in compulsory land acquisition, on industrialisation. Based on the fact that manufacturing requires vastly more land than services, I separate the factor reallocation from agriculture to non-agriculture into manufacturing and services to investigate how compulsory acquisition affects structural transformation.

I exploit an unexpected reform in 2007 that placed restrictions on compulsory land acquisition for Special Economic Zones (SEZs) in India. In that year, a large protest against a SEZ in West Bengal was violently shut down by state police, with fourteen farmers being killed and more than a hundred missing. In response, the Central Government announced that from then on, *forced* land acquisition was prohibited, and that landlosers must be compensated properly in terms of rehabilitation and resettlement (SEZ Board of Approval, 2007). Importantly, SEZs that were already approved were exempt from this policy; only new developers were exposed to this dramatic increase in land acquisition costs.

I obtain causal estimates by comparing states that officially committed to compulsory acquisition for SEZs to those that did not have such policies. The idea is that the former State Governments shielded private developers, and any firms locating in the SEZ, from the normally high transaction costs due to India's imperfect land markets. This allows me to directly relate *government* land acquisition to *private* economic activity.

Based on the stationary version of the Hopenhayn (1992) model, I predict that the reform reduced the share of manufacturing proposals in those states that introduced compulsory acquisition policies for SEZs, as the entry barrier increased relatively more for manufacturing these states. However, the increased entry barrier also has a selection effect, suggesting that new entrants are on average of higher productivity or quality. This implies that separating the entry decision and ultimate productivity are important. My hand-collected dataset on SEZ proposals, complemented with information on operation and firm activity, is uniquely suited for this exercise. In the results, I show that the increase in land acquisition costs results in a decrease in the share of proposals for

manufacturing by thirteen percentage points, while the corresponding share for services increases by twelve percentage points. The effect sign and size are consistent across all stages of SEZ development, although the results are not significant for notified and operational SEZs.

Finally, I employ a spatial difference-in-differences strategy to study whether this selection effect materializes in the form of local employment. By comparing villages that host an operational SEZ to nearby villages that do not host an SEZ, I find that in general, there is no significantly different effect on local employment for SEZs proposed before and those proposed after. However, when I split the sample by broad sector denomination, I find a positive significant effect on local employment within 10 kilometres of manufacturing SEZs proposed after the reform, beyond the general employment increase following the opening of an SEZ. The corresponding estimates for services SEZs are not significant and smaller in size, suggesting that the increased land acquisition cost had more bite for the more land-intensive manufacturing sector. In conclusion: restricting eminent domain for SEZs may reduce entry of more land-intensive sectors, but also induces selection and thereby engenders more local employment.

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