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Working Paper 35446
http://www.nber.org/papers/w35446

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2026

We thank Aisha Alsayegh, Jared Fung, Tam Le, Bingbin Li, Duong Luu, Roldan Enamorado and Nguyen Hai Thanh for excellent research assistance and Mr. Nguyen Viet Phong at the General Statistics Office Vietnam and Kyle Butts, Edmund Malesky, David McKenzie, and Jeff Zabel for helpful discussions. McCaig and McMillan thank the Private Enterprise in Low-Income Countries program for financial support. We also thank Economic Research Institute for ASEAN and East Asia and the World Bank Datax for financial support. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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The Uneven Impact of Industrial Zones on Formal Job Creation in Vietnam
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NBER Working Paper No. 35446
July 2026
JEL No. L52, O25, O53, R11

ABSTRACT

Industrial zones are a widely used industrial policy tool, yet their effects vary enormously. Matching the timing and location of zone establishment to firm-level data in Vietnam, we estimate that a new zone raises formal employment by 2,000 workers within seven years. This average masks extreme skewness: a handful of zones generate the gains while the rest produce almost none. Zones near major trade hubs account for most of the effect, driven by export-oriented foreign manufacturers. Accounting for spillovers, domestic firms face agglomeration shadows nearby but gain farther out, while foreign manufacturers operate as enclaves, generating few local linkages.

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1 Introduction

Industrial zones are one of the most widely used instruments of industrial policy in developing countries today. Policymakers often view them as a way to accelerate industrialization by attracting manufacturing investment, promoting exports, and creating formal-sector employment. Rather than upgrading infrastructure and administrative capacity across an entire country at once, industrial zones allow governments to concentrate these investments in specific locations where industrial activity can be more easily supported (Farole and Akinci, 2011; Newman and Page, 2017; Fernandes and Reed, 2026). Their popularity reflects, in part, the experience of countries such as Vietnam and China, where rapid export-oriented industrialization since the 1990s coincided with the expansion of industrial zones and large increases in manufacturing employment (Wang, 2013; Lu *et al.*, 2019; Tafese *et al.*, 2025).

Vietnam is often presented as a successful example of zone-based industrialization. Yet, as we will show, the majority of established industrial zones failed to attract a significant number of firms and workers. Between 1991 and the early 2020s, the country established 449 industrial zones across nearly all its provinces (Figure 1). During this period, manufacturing exports and foreign direct investment expanded rapidly, and Vietnam became deeply integrated into global manufacturing supply chains (UNCTAD, 2019; Asian Development Bank, 2022). Industrial zones were a central component of this strategy, providing serviced land, infrastructure, and administrative support intended to attract investment and promote industrial development. Important questions remain about their economic impacts. What are the employment effects of industrial zone establishment? Through what channels do these effects arise? And what spillovers do zones generate in surrounding areas? This paper addresses each of these questions in turn.

We combine ward-level industrial zone establishment data with firm-level data in a difference-in-differences design that simultaneously accounts for staggered timing, non-random placement, and spatial spillovers.[1] The firm-level data are from the annual Vietnam Enterprise Surveys (VES) which cover the universe of registered formal firms between 2000 and

[1] An urban ward or rural commune represent the third subnational administrative unit in Vietnam during our period. For simplicity, we use the term ward to represent these administrative units.

2019 and have been used extensively to study Vietnam's structural transformation (McCaig and Pavcnik, 2013; McCaig *et al.*, 2025). Our identification strategy compares wards that establish industrial zones to nearby wards within the same district that do not, using recent advances in event-study estimation under staggered treatment timing and heterogeneous effects (Sun and Abraham, 2021; Wooldridge, 2021; Borusyak *et al.*, 2024). Our treated wards are those that established industrial zones between 2005 and 2012, ensuring enough pre- and post-treatment periods. Because industrial zone placement is not random, we additionally allow for cohort-specific linear pre-trends and show that event study estimates prior to treatment are precisely estimated around 0. Finally, since the siting of industrial zones may affect formal employment in neighboring locations, spillovers threaten the no-interference condition underlying our difference-in-differences estimator (Butts, 2023). Thus, we estimate spillovers directly by controlling flexibly for time-varying manufacturing activity in surrounding wards across distance rings. Our estimates are highly robust to alternative control groups and different pre-trend specifications, such as the approach of Rambachan and Roth (2023) and synthetic-difference-in-differences (Arkhangelsky *et al.*, 2021).

Seven years after establishment, industrial zones increase formal-sector employment in the host ward by approximately 2,000 workers and increase the number of firms by about 10. These gains in employment are concentrated in foreign manufacturing firms located inside the zones, whereas the increase in the number of firms comes largely from domestic firms that generate little employment. However, this average masks enormous variation. When we exclude the small number of industrial zones that host the most workers, the estimated employment effect falls rapidly and becomes close to zero. The top 10 percent of treated wards, ranked by employment inside zones, account for about 69 percent of the total estimated employment gains, and the top 20 percent for about 90 percent. Bondonio and Greenbaum (2007) make a closely related point in the context of US enterprise zones, arguing that mean treatment effects can mask cross-zone variation that determines whether a place-based policy is succeeding on its own terms.

The heterogeneity in outcomes is strongly related to geography. Industrial zones in proximity to Hanoi, Hai Phong, or Ho Chi Minh City, Vietnam's economic and trade hubs, drive almost all of the average employment effect. Those farther from these hubs generate close to

no additional employment on average. This pattern reflects the geography of export-oriented manufacturing in Vietnam. The industrial zones that generated the most employment primarily attract foreign manufacturers integrated into global value chains. These firms export approximately 80 percent of their output (employment-weighted) and import roughly two-thirds of their intermediate inputs (McCaig *et al.*, 2025), so access to major trade and logistics hubs appears to be a critical determinant of success.

Location is necessary but not sufficient: even among zones close to Hanoi, Hai Phong, and Ho Chi Minh City, many generate few jobs. A natural explanation for this within-location variation is the zone developer. We first test whether developer nationality matters, comparing zones developed by foreign or joint-venture developers with those developed by domestic firms among zones close to major trade hubs. We find no statistically significant difference in employment between the two; indeed, the most successful zones in our sample are at least as likely to be domestically developed as foreign. Developer ownership is thus a poor guide to which individual zones succeed.

We argue instead that the operative margin is developer capacity—financial stability, prior experience in industrial-zone development, and the ability to clear land and relocate residents—which is distinct from ownership or nationality and not well captured by it. Because direct measures of capacity are not consistently available across our zones, we develop this argument through case studies of two zone pairs and three individual zones, selected to hold location or developer nationality nearly constant. The cases reveal that zone outcomes turn on these capacity dimensions rather than on nationality. They also document a recurring institutional response: when initial developers fail, Vietnamese authorities have in multiple instances reassigned stalled zones to developers with established industrial-zone experience, with measurable activation following. These findings are consistent with Sheldon and Kwon (2023), who emphasize the role of developer capability in shaping zone outcomes.

The spillover results reveal an important asymmetry between foreign and domestic firms. Employment in foreign manufacturing firms shows little relationship with nearby manufacturing activity, consistent with the enclave nature of many export-oriented firms in Vietnam's industrial zones: they import a large share of their intermediate inputs and export most of their output, limiting local production linkages (McCaig *et al.*, 2025; Vu-Thanh and Do-

Thien, 2020). Domestic firms, by contrast, are sensitive to nearby manufacturing density. At close distances, domestic firms experience what economic geography models describe as an agglomeration shadow, likely reflecting competition for land, labor, and local customers. At moderate distances, however, domestic firms benefit from positive spillovers associated with broader manufacturing activity (Fujita *et al.*, 1999; Hornbeck *et al.*, 2024).

Our paper contributes to a growing literature on special economic zones and place-based industrial policy in developing countries. Evidence from China, India, and Vietnam documented positive average effects on employment, investment, and productivity (Wang (2013); Chaurey (2017); Hyun and Ravi (2018); Lu *et al.* (2019); Tafese *et al.* (2025)). However, evidence from Indonesia suggests no effects on firm entry or output (Rothenberg *et al.*, 2025). Beyond average effects, a few papers study heterogeneity across zones. Chen *et al.* (2019) show that zones within 500 kilometers of the three major seaports in China significantly affect firm productivity, while those farther away do not. In contrast, Lu *et al.* (2019) find no evidence of differential impact of zones across Chinese counties with poor versus good infrastructure. In India, Görg and Mulyukova (2024) find that productivity increases for firms in privately developed zones.[2] This literature suggests zone performance varies substantially but mostly leaves open what drives that variation and whether most zones succeed or fail.

We contribute to this literature in three ways. First, we document enormous variation in employment generation across industrial zones: most generate few jobs, while a minority become major manufacturing hubs, with job creation concentrated in foreign-owned manufacturing firms. We then systematically analyze how industrial zone performance varies with location and developer ownership, and combine the regression evidence with case studies of industrial zones that succeeded and failed in similar locations. We date treatment to official establishment, which includes the assignment of a developer to the zone. Thus, unlike Tafese *et al.* (2025), our sample retains zones that were established but never built out and captures the land-clearance and construction phase where failures tend to concentrate.

Second, we provide the most geographically detailed analysis of Vietnamese industrial zones to date. Because zones are small relative to districts and provinces, coarser data aver-

[2]Other papers explore heterogeneity across workers and industries: Hyun and Ravi (2018); Gallé *et al.* (2024); Brussevich (2024); Tafese *et al.* (2025).

age their effects together with surrounding untreated areas, attenuating estimated impacts and obscuring the heterogeneity we study. Ward-level data instead match the spatial scale at which trade costs, market access, and agglomeration operate. They also sharpen identification. Within a district, some wards contain an industrial zone and others do not, so ward-level data supply untreated control wards in the same district as treated ones. Our ward-level regressions include district-by-year fixed effects, comparing treated and control wards in the same district and year. This absorbs not only fixed district characteristics but any district-level shock, policy change, or trend that varies over time, isolating the zone's association with the remaining difference. Ward-level data thus offer a within-district comparison that district- or province-level data cannot, where the unit of observation and the potential confounder both sit at the district level. To support this and future work, we also develop and publicly release a new dataset that harmonizes ward, district, and province boundaries from 1999 to 2019, providing a consistent geographic framework for linking Vietnamese censuses, firm data, and other micro data.

Third, we improve existing spillover measurement by using time-varying manufacturing employment density in twenty concentric 10 km distance bands around each ward, rather than the binary or short-radius indicators used in most prior work (Chaurey, 2017; Lu *et al.*, 2019; Gallé *et al.*, 2024). This flexible approach allows us to identify both agglomeration shadows at close distances and positive spillovers at moderate distances, and to distinguish the spillover dynamics of foreign export-oriented firms from those of domestic firms operating in local markets.

More broadly, our findings suggest that Vietnam's industrial zone program is best understood not as a uniform policy success but instead is consistent with a decentralized process of experimentation. Provincial governments competed aggressively to attract investment, developers differed widely in capability, and industrial zones were established under highly varied local conditions (Malesky, 2004, 2007, 2008; Malesky and London, 2014; Vu-Thanh and Do-Thien, 2020). Some industrial zones became major manufacturing hubs integrated into global production networks, while many others remained small or underutilized. The Vietnamese experience therefore highlights both the potential and the limits of zone-based industrial policy. The industrial zones that generated the most employment required a spe-

cific combination of favorable geography, capable developers, and access to export-oriented foreign investment. But the broader lesson may be less about replicating any single configuration and more about understanding industrial policy as a process of experimentation and learning rather than the implementation of a fixed blueprint (Hausmann and Rodrik, 2003; Rodrik, 2004).

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting of Vietnam's industrial zone program. Section 3 presents the data. Section 4 outlines the empirical strategy and presents the main results, including heterogeneity by location and developer, case studies, and spatial spillovers. Section 5 concludes.

2 Institutional Setting

Vietnam's industrial zone program represents one of the most extensive zone-based development strategies in Southeast Asia (Vu-Thanh, 2015; Asian Development Bank, 2022). Between 1991 and 2023, the number of industrial zones grew from one to nearly 450, spreading from initial clusters around Hanoi and Ho Chi Minh City to locations across the country. Understanding this program requires attention to three features that are central to our analysis: the regulatory framework governing zone establishment, the decentralized governance structure that generated substantial variation in zone implementation, and the resulting heterogeneity in zone performance.

2.1 Industrial zone definition and regulatory framework

Vietnamese law distinguishes industrial zones from export processing zones (EPZs). EPZs are defined as zones specializing in manufacturing export goods and operate as non-tariff areas physically separated from domestic markets (Decree 36/CP, 1997; Decree 29/2008/ND-CP). Industrial zones, by contrast, permit firms to serve both domestic and international markets. This flexibility made industrial zones the preferred vehicle for attracting foreign investment, as firms could access Vietnam's growing domestic consumer base while simultaneously participating in export-oriented global value chains (Vu-Thanh, 2015; Asian Development Bank, 2022). Although EPZs initially received considerable policy attention, mirroring

successful East Asian models, their restrictive export-only framework limited domestic linkages. Today, industrial zones constitute the dominant form of zone-based industrialization in Vietnam (Vu-Thanh and Do-Thien, 2020; Phương, 2025). We use "zones" and "industrial zones" interchangeably hereafter.

The establishment of an industrial zone follows a two-step approval process. First, a proposed zone must be included in the national master list approved by the Prime Minister. Inclusion requires demonstrating favorable geographic location, adequate land, capacity to attract investors, sufficient labor supply, and the ability to develop supporting infrastructure including housing and services for workers. The master list, led by the Ministry of Planning and Investment in coordination with provincial governments, specifies proposed zones, their provinces, and the time periods during which they are prioritized (Decision 1107 in 2006; Decree 29/ND-CP in 2008).

Second, for zones on the master list, developers submit bids specifying their strategy for attracting investment and providing essential infrastructure—electricity, water, transportation, and waste treatment (Decree 36/CP, 1997, Article 4). Based on our interviews, it is not uncommon for only a single developer to submit a bid.[3] If the proposal is approved, an establishment decree is issued—by the Prime Minister for the period 1997–2008, and by the relevant provincial government from 2008 onward (Decree 29/2008/ND-CP, Article 10). Establishment decrees specify the developer, the exact location down to the ward level, total land area, initial investment capital, and an operational duration of 50 years, extendable to a maximum of 70. During this period, developers pay land rent to the government and lease serviced land to firms operating within the zone.

The establishment decree is not the final step in industrial zone development. Rather, it occurs after inclusion in the national master list and initial planning, but before detailed implementation, land clearance, infrastructure construction, and tenant attraction. We define treatment based on the year the establishment decree was issued.

[3]In February 2023 we met with representatives from the central government and two provincial governments in charge of overseeing industrial zones. We also met with a representative of a prominent domestic industrial zone developer.

2.2 Decentralization and provincial governance

A defining feature of Vietnam's approach to industrial zone development is the substantial autonomy retained by provincial authorities. While the central government sets overarching policy through the Ministry of Planning and Investment, provincial governments exercise considerable control over zone location, infrastructure provision, regulatory implementation, and promotion to investors (Malesky (2007); Decree 29/2008/ND-CP, Article 35).

This decentralization enabled what Malesky (2004, 2007, 2008) and Malesky and London (2014) describe as a process of policy experimentation, in which provincial authorities engaged in "fence-breaking" (phá bỏ rào)—local innovations and deliberate deviations from central regulations to attract investment. Provinces such as Song Be (later divided into Binh Duong and Binh Phuoc), Dong Nai, Ba Ria-Vung Tau, and subsequently Vinh Phuc and Long An were at the forefront of these initiatives. Many provisions of later national reforms, including elements of Public Administration Reform, the 2003 Land Law, and the 2000 Enterprise Law, originated in these provincial experiments (Malesky, 2007). This experimental approach, characteristic of both Vietnamese and Chinese reform trajectories, allowed successful local innovations to be scaled nationally while unsuccessful ones remained confined to the local level.

The flip side of decentralization has been a "race to the bottom" in which provinces established an excessive number of zones even when local conditions were insufficient to support them (Vu-Thanh and Do-Thien, 2020; Tang, 2023). The result has been inefficient land use and stalled development. The Dong Nam A Industrial Zone in Long An province illustrates this pattern: established in 2007, it underwent three changes in developers and, as of 2020, had only been partially developed despite Long An seeking central government approval for additional projects (Huy, 2020).

Although better-governed provinces systematically attract more investment and subsequent firm expansion (Malesky, 2007; VCCI-USAID, 2024), we show that within the same province, zone performance varies significantly.

2.3 Expansion and geographic spread

Vietnam's earliest industrial estates predate the modern program. Viet Tri (1962) was developed with Soviet and Chinese assistance, while Bien Hoa (1963) was established under the Republic of Vietnam government. Both were nationalized in 1975 but were not designed for the global production networks Vietnam would later join (Vu-Thanh and Do-Thien, 2020). The modern era of zone-based development began in 1991 with Tan Thuan EPZ in Ho Chi Minh City.

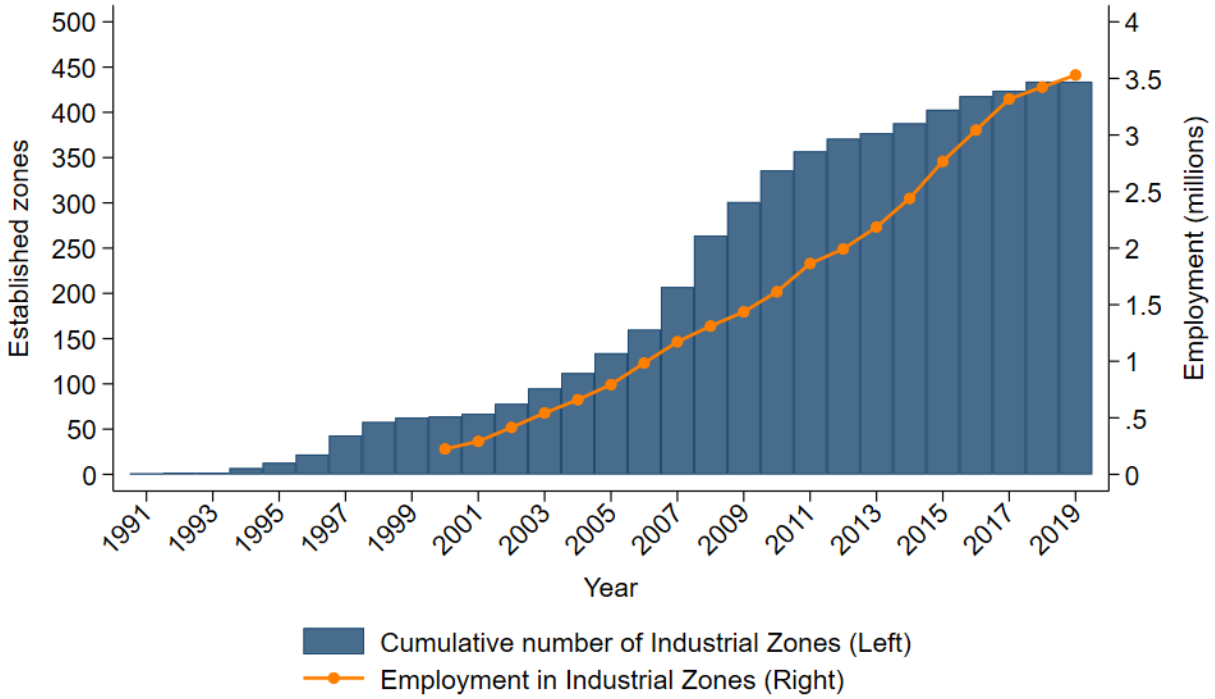
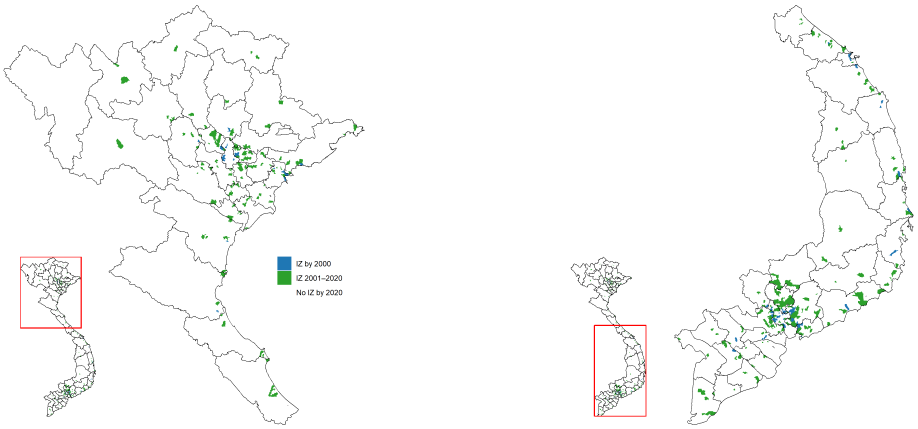


Figure 1: Evolution of Industrial Zones in Vietnam

Notes: The figure shows total employment in firms operating inside industrial zones per year (right axis) as calculated by the authors using data from the Vietnam Enterprise Surveys and the cumulative number of industrial zones established as of each year (left axis).

The program expanded rapidly during the 2000s, propelled by the U.S.–Vietnam Bilateral Trade Agreement in 2001, Vietnam's accession to the World Trade Organization in 2007, intensifying competition among provinces for investment, and national policies promoting industrialization. As shown in Figure 1, the number of zones rose from 11 in 1995 to 65 in 2000, 135 in 2005, 337 in 2010, and 418 in 2016. The pace of new establishment slowed after 2011. Despite the rapid growth, only about 60 percent of zones with available status

information were operational as of 2016 (authors' calculations).[4]



(a) North (b) South

Figure 2: The geographic distribution of industrial zones

Figure 2 illustrates that zones are heavily concentrated around the major economic hubs of Hanoi and Ho Chi Minh City. In 2000, most zones were clustered around Hanoi and extending toward the northern coast, around Ho Chi Minh City, and in several coastal areas of the central and south-central regions. Zones established between 2000 and 2020 largely expanded along these same corridors, with many located in close geographic proximity to one another, forming dense clusters. This geographic concentration is directly relevant to our empirical analysis: it creates both the trade hub-proximity variation that drives our heterogeneity results and the spatial density that underlies our spillover analysis.

2.4 What zones offer firms and who locate there

Industrial zones offer several advantages to firms, particularly foreign manufacturers. First, zones provide ready-made industrial infrastructure including factory buildings, reliable electricity, water supply, waste treatment facilities, and telecommunications, services that may be unreliable or unavailable in surrounding areas (Decree 29/2008/ND-CP, article 35). Second, zone management boards serve as one-stop administrative centers for business registration, licensing, and regulatory compliance, reducing bureaucratic complexity and time costs for investors (Vu-Thanh and Do-Thien (2020); Decree 29/2008/ND-CP, Article 37). Third, zones

[4]Of the 418 industrial zones established by 2016, operational status information is available for only 388. Among these, 230 zones are operational, or about 59 percent.

typically offer fiscal incentives such as tax exemptions and subsidies for infrastructure investment (Decree 29/2008/ND-CP, Article 16; Decree 124/2008/ND-CP, Article 16). Fourth, the concentration of industrial activity facilitates agglomeration benefits including supplier networks, labor pooling, and knowledge spillovers. Malesky (2007) finds that the presence of existing foreign investors is one of the strongest predictors of new investment, suggesting these agglomeration economies are substantial.

These features make zones especially attractive to export-oriented foreign manufacturers, who have come to dominate zone employment. As illustrated in Figure 3, employment within zones grew from negligible levels in 2000 to over 2.5 million by 2017, with the majority in foreign manufacturing firms.

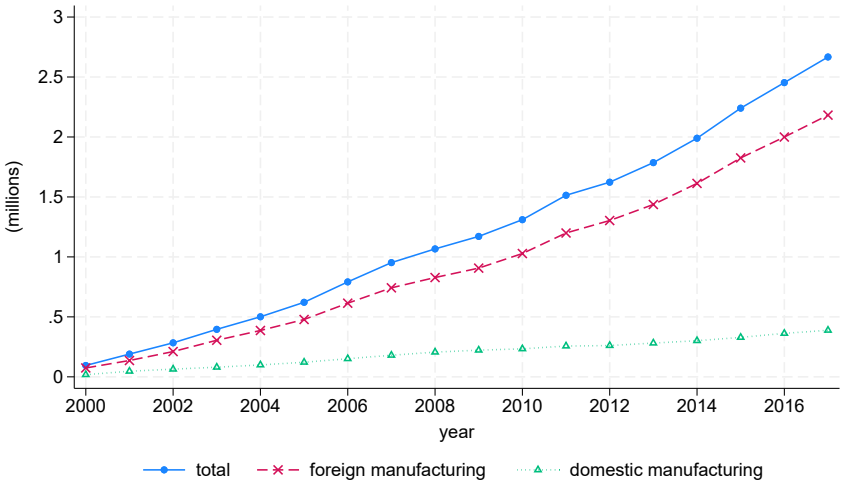


Figure 3: Manufacturing employment inside zones

Notes: The figures show total employment in foreign-invested manufacturing firms and private domestic firms located inside industrial zones. Foreign-invested includes 100% wholly foreign owned as well as joint ventures with Vietnamese firms. Domestic private includes 100% private ownership as well as joint private-state firms where the state's ownership is less than 50%. Whether a firm is located inside an industrial zone or not is based on the firm's address.

Foreign manufacturers are overwhelmingly export-oriented, exporting approximately 80 percent of their output (employment-weighted) and importing roughly two-thirds of their intermediate inputs (McCaig *et al.*, 2025). This high export concentration reflects Vietnam's integration into global value chains, where the country serves primarily as an assembly and processing location within broader regional production networks. However, this pattern also raises concerns about limited domestic linkages and "enclave" production, wherein foreign

manufacturing firms import most inputs and export finished goods with minimal integration into the domestic economy (Vu-Thanh and Do-Thien, 2020). Throughout the 1990s and into the twenty-first century, policy frameworks favored foreign private investors over domestic private firms, reinforcing this export-oriented pattern (Vu-Thanh and Do-Thien, 2020).

The dominance of export-oriented foreign manufacturing firms in zones has a direct implication for our analysis: it means that access to major trade hubs, which include logistics, customs, airports, and seaports, should be a critical determinant of zone success, a hypothesis we test in Section 4.4.

3 Data

We draw on three primary data sources: a comprehensive dataset of industrial zones that we construct, including their precise locations and establishment dates; annual firm-level data from the Vietnam Enterprise Surveys; and a self-constructed dataset of consistently defined administrative boundaries. This section describes each in turn and presents summary statistics comparing treated and control wards.

3.1 Zone data

An important contribution of our research is the creation of a comprehensive dataset that includes the exact location and establishment date of each industrial zone.

In most cases, the administrative lists from the Ministry of Planning and Investment contain information on the name of the zone, the name of the developer or local management board, the year of establishment, and the district. We then add to the dataset information on the ward, or multiple wards, in which the zone is located by referring to establishment decrees, the 2007 Establishment Census, the address reported by firms in the Vietnam enterprise surveys, and online searching. A very important component of this verification process involved identifying firms that reported being in an industrial zone according to their address and comparing this to the address of the ward(s) listed for this particular zone in the official decree. There are many instances in which the two sources differed in their identification of the zone by ward(s). In these cases, we did extensive online searching referring

to provincial investment board websites, compared zone maps to administrative boundary maps where possible, and used the available address and zone location information available in the enterprise data (see section 3.2 for further information on the firm data).

This process left us with a comprehensive list of 449 industrial zones established as of 2023. We have the year of establishment for all but two industrial zones and the ward location for all of them.

3.2 Firm data

We use firm-level data spanning 2000 to 2019 from the annual enterprise surveys conducted by the General Statistics Office (GSO) of Vietnam. The surveys cover all businesses registered as an enterprise according to the Enterprise Law. In Vietnam, all state, collective, and foreign businesses must register as an enterprise. For private domestic businesses, however, registration as an enterprise is legally required only if the business has more than 10 workers or operates in more than one location. Nonetheless, many smaller private businesses choose to register even when not legally required. We define formal employment as employees of registered enterprises.

Although the surveys cover all enterprises, not all receive the same questionnaire. From 2000 through 2002, all enterprises received a detailed questionnaire. Starting in 2003, as the number of small private enterprises grew rapidly, the GSO introduced a shorter questionnaire for the set of small private enterprises that were to be "listed" only. All state, foreign, collective, and large private enterprises continued to receive the detailed questionnaire. Among small private enterprises, a fraction was randomly selected for the detailed questionnaire while the rest were only listed. The questionnaire for listed small private enterprises consistently collected information on location (including both the administrative geographic unit and address), industry, and employment. Hence, we have this information and ownership for the complete set of enterprises for all years. Despite the limited outcomes available in some years for small private enterprises, a strength of our data is that it covers all registered enterprises regardless of size.

We use the firm data to construct measures of ward-level employment on an annual basis. We focus on employment in domestic private and foreign firms. Our key variable for

defining employment is end-of-year employment. The questionnaires also consistently ask about start-of-year employment. We take advantage of this to create an estimate of end-of-year employment within wards in 1999 by approximating it with start-of-year employment in 2000. For other years, the correlation between end-of-year employment in year t and start-of-year employment in year $t+1$ ranges between 0.954 (between 2000 and 2001) and 0.996 (between 2006 and 2007). Hence, start-of-year employment in year $t+1$ appears to be an accurate estimate of end-of-year employment in year t.

We define a foreign firm as either 100 percent wholly foreign-owned or as a joint venture between foreign and domestic firms regardless of the domestic firm's ownership share. In 2017, 95 percent of manufacturing employment in foreign firms was in 100 percent wholly foreign-owned firms (McCaig *et al.*, 2025). We define a private firm as a domestic-owned firm with less than 50 percent state ownership; the vast majority of these have 0 percent state ownership.

We additionally make use of each firm's reported address to identify whether it is located within an industrial zone. Specifically, we search for terms related to an industrial zone, such as *khu công nghiệp* and various permutations including abbreviations. Since not all firms reported their address in every year and not all firms in an industrial zone include a permutation of *khu công nghiệp* in their address—some list only a lot number within the zone, for example—we extensively checked the firm-level data for inconsistencies in the generated indicator for being in an industrial zone. We also cross-checked this with a direct question on whether the firm is located inside a zone, which is available in a few of the later survey years. These cross-checks indicate the industrial-zone indicator is measured accurately.

3.3 Consistent administrative boundaries

Our analysis is conducted at the third subnational administrative unit, urban wards and rural communes, which we simply refer to as wards. Administrative boundaries in Vietnam changed frequently during our study period as communes, wards, districts, and provinces were split, merged, or reclassified. These changes complicate longitudinal analysis because observations from different years may refer to geographic units with different spatial extents.

To address this issue, we construct a dataset of consistently defined administrative units

that harmonizes all communes and wards to a common geographic framework. The harmonization is based on official administrative boundary changes and ensures that each observation can be mapped to a stable geographic unit over time. The resulting dataset allows researchers to combine population censuses, enterprise surveys, household surveys, establishment censuses, and other administrative records collected in different years without introducing artificial changes caused by boundary redefinitions.

Our dataset substantially improves on existing consistently defined administrative boundaries datasets in three ways. First, it is at a much more detailed administrative unit, wards and communes, as compared to provinces, such as that available via IPUMS International. Second, it includes a broader set of geographic codes used across various micro data sources. Third, it contains year-by-year changes which researchers could use to construct their own consistently defined administrative boundaries within the 1999 to 2019 period.

Because this dataset may be useful for other researchers working with Vietnamese microdata, we describe its construction in detail in Appendix section A.1 and make the resulting crosswalks and geographic files available for replication and future research.

3.4 Summary statistics

In our main analysis in Section 4, we focus on wards that established their first industrial zone in 2005 through 2012. Our primary control group is other wards that have not established an industrial zone as of 2023, but identification comes from within-district comparisons via the inclusion of district-year fixed effects. Table 1 displays mean employment and number of firms in the formal sector in 2000 for our treatment and control groups. We expand our set of initial conditions by including information from the 1999 population census. In the final column, we report the p-value from a test of statistical significance where the dependent variable is the outcome shown in the respective row, the key explanatory variable is an indicator variable for being a treated ward, and we include district fixed effects as controls. The strongest differences between treated and control wards are that within districts they have a greater share of ethnic Kinh, have a greater population, and are closer to one of Hanoi, Ho Chi Minh City, or Hai Phong. Throughout the paper, we measure access to Vietnam's major trade and logistics gateways using the minimum distance to Hanoi, Hai Phong, or Ho

Chi Minh City. These locations anchor the country's principal export-manufacturing corridors and account for a large share of merchandise trade through a combination of seaports, airports, customs facilities, and associated logistics networks.

Table 1: Initial characteristics of wards in 2000, by treatment status

	Treated (mean)	Control (mean)	p-value
Employment			
Total Employment	151.69	112.86	0.85
Foreign	48.26	18.13	0.47
Domestic	103.43	94.72	0.87
Manufacturing	110.84	58.12	0.79
Business services	1.66	3.60	0.60
Number of Firms			
Total number of firms	3.79	3.44	0.54
Foreign	0.14	0.10	0.51
Domestic	3.65	3.34	0.57
Manufacturing	1.34	0.75	0.03**
Business services	0.17	0.27	0.34
Initial conditions (1999)			
Share of workers in informal firms	0.87	0.89	0.06*
Share of workers in foreign firms	0.01	0.00	0.50
Share of workers in manufacturing firms	0.09	0.06	0.14
Share of urban population	0.11	0.15	0.38
Share of Kinh population	0.93	0.77	0.00***
Log of population	9.05	8.72	0.00***
Minimum distance to Hanoi / HCMC / Hai Phong (km)	128.37	163.15	0.00***
Number of wards	182	9,121	9,303

Notes: The table shows mean outcomes for two sets of wards, those that established their first industrial zone in 2005 through 2012, the treated wards, and those that had not established an industrial zone as of 2023, the control wards. In each row we report the mean outcome for treated and control wards. The employment, based on end of year employment, and number of firm outcomes are based on the 2000 Vietnam Enterprise Survey. The initial conditions (1999) outcomes are based on the 1999 population censuses, dated 1 April 1999, and include workers in all sectors, not just those covered by the enterprise survey. Each p-value is from a difference in means regression where the key explanatory variable is an indicator for treatment status, and district fixed effects are included. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Before introducing our empirical framework in the next section, we descriptively show the variation in zone performance in two ways. Figure 4 shows the variation over time in ward-level employment among the wards that belong to the cohort that established their first zone in 2007. A striking feature emerges: employment increases rapidly in a small number of wards, much more slowly on average, and there are a few wards in which employment barely increases in the long run. See Figure A1 for similar patterns for each treatment cohort. That is, there is large variation across wards in the increase in employment after establishing their

first industrial zone.

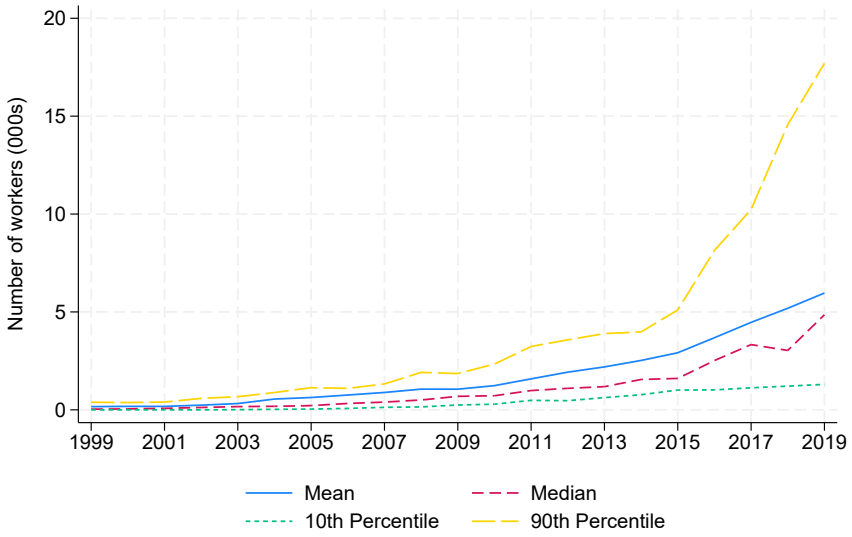


Figure 4: Variation in employment over time for wards within the 2007 treatment cohort

Note: The sample is wards that established their first industrial zone in 2007. When the zone spans more than one ward, we sum employment over all wards that share the same zone and divide by the number of wards. When calculating the means and percentiles, we weight the observations by number of wards that share the zone.

Figure 5 shows this variation in a different way. For each treated ward across all treatment cohorts, we plot the change in employment between years 7 and -1 relative to treatment on the vertical axis and employment in relative year -1 on the horizontal axis. We mark wards separately based on whether they are within approximately 115 km of Hanoi, Hai Phong, or Ho Chi Minh City (based on the median minimum distance across all wards). The scatterplot reveals several notable features. First, there is a handful of wards for which employment increased considerably after zone establishment from a base of very little employment prior to establishment—the points in the upper left. Second, some wards already had relatively large employment before establishing their first zone—the points to the right. Third, most wards see a relatively modest increase in employment following establishment; the median increase was about 900 workers, with a quarter of wards seeing an increase of less than 280. Fourth, even within wards close to Hanoi, Hai Phong, or Ho Chi Minh City, there is considerable variation, underscoring that proximity to trade hubs is an important but not sufficient condition for zone success.

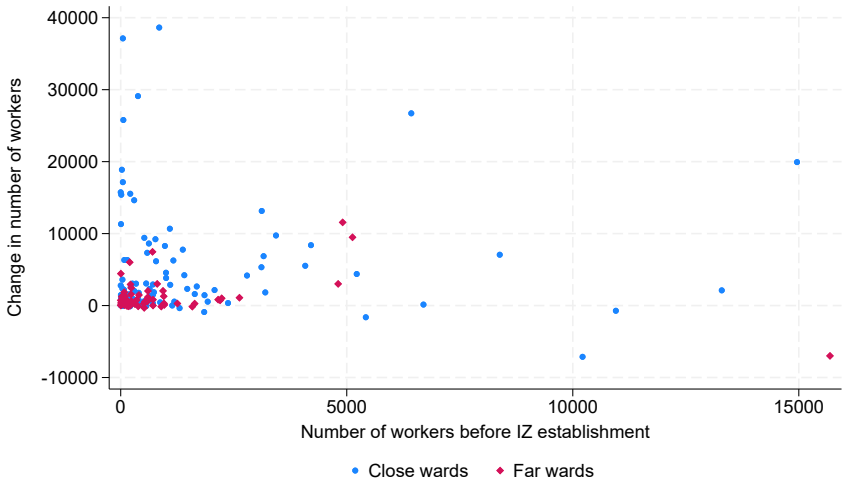


Figure 5: Change in employment after establishment relative to initial employment by ward distance to major trade hubs

Note: The sample is all wards that established their first industrial zone in 2005 through 2012. When the zone spans more than one ward, we sum employment over all wards that share the same zone and divide by the number of wards. A ward is defined as close if it is less than 115 km from one of Hanoi, Hai Phong, or Ho Chi Minh City and as far otherwise. On the horizontal axis we plot ward employment in the year prior to treatment. On the vertical axis we plot the change in ward employment between years 7 and -1 relative to treatment.

4 Empirical Implementation

4.1 Estimation Strategy

We employ the following event study framework to estimate dynamic treatment effects:

$$Y_{it} = \alpha_i + \gamma_{dt} + \lambda_c t + \theta X_i \phi_t + \underbrace{\sum_{c} \sum_{\substack{l=-k \\ l\neq -1 \\ l\neq -5}}^{L} \mu_{l,c} D_{it}^{l} D_{i}^{c}}_{\text{direct effect}} + \underbrace{\sum_{r=1}^{20} \sum_{\substack{b=\text{med}, \\ \text{high}}} \beta_{br} I_{brit}}_{\text{spillovers from nearby wards}} + \underbrace{\sum_{r=1}^{20} \sum_{b=\text{med,high}} \psi_{br} Z_{brit}}_{\text{spillovers from nearby zones}} + \varepsilon_{it}. \quad (1)$$

The framework is broadly designed to estimate both the direct effect of industrial zone establishment on local employment as well as spillovers (the last two terms on the right hand side), which are of interest themselves and additionally help to address concerns that

direct treatment effects might be confounded by other nearby zones or economic activity more broadly.

Y_{it} is the outcome of interest, such as employment or the number of firms, for ward i in year t, α_i is a ward fixed effect, γ_{dt} is a district-year fixed effect, $\lambda_c t$ is a linear cohort trend, and $X_i \phi_t$ is a vector of ward initial conditions interacted with year fixed effects. Ward initial conditions are taken from the 1999 census. They include employment variables—specifically, the shares of workers in manufacturing, foreign-owned enterprises, and informal businesses—as well as the log population, the share of ethnic Kinh Vietnamese, and an indicator for whether the ward is urban. The first summation term is a series of interactions between an indicator for the treatment cohort and the relative year to treatment. D_i^c is an indicator variable that takes the value 1 if ward i belongs to treatment cohort c and 0 otherwise. D_{it}^l is an indicator variable that takes the value 1 if ward i in year t is l years relative to treatment and 0 otherwise. The coefficients of interest are $\mu_{l,c}$, which measure the dynamic effects of treatment in relative period l for wards in cohort c. We omit the indicator for 5 years prior to treatment. This is to avoid the multicollinearity problem from having a complete set of relative year indicators (other than the base -1) and a linear trend (Borusyak *et al.*, 2024). We'll return to discussing the additional summation terms shortly.

In this framework, we're following the approaches outlined by Sun and Abraham (2021) and Wooldridge (2021) where in the first stage we estimate separate relative year effects for each cohort. In the second stage, we aggregate over the cohorts within each relative year effect to estimate a weighted average relative year effect, μ_l, where the weights are the number of wards in the treatment cohort.

Our identification strategy is to compare changes in outcomes for treated wards to other untreated wards within the same district, hence the inclusion of district-year fixed effects. This ensures the comparisons are being conducted within relatively small geographic areas and helps to mitigate concerns about unobserved characteristics, such as access to infrastructure, differences in local governance, etc., contaminating our estimates of the treatment effects. Our main sample of treated wards is those that established an industrial zone in 2005 through 2012, which ensures a sufficient number of both pre- and post-establishment periods for all treatment cohorts. We drop from the sample wards that established an industrial zone

prior to 2005 or after 2012, up to and including 2023. Hence, we are making comparisons between treated and never treated wards. In total, we have 182 treated wards across 133 districts and 9,121 control wards across 607 districts. In reporting results, we focus on the estimates for relative years -4 through 7, which can be estimated for all of our treatment cohorts. This ensures that any changes between relative years are not due to changes in the composition of which cohorts are included in the weighted average effects.

Returning now to the last two summation terms in equation (1), we additionally control for potential spillovers from nearby wards and from nearby industrial zones (Butts, 2023). Specifically, we create 10 km rings around the centroid of each ward, up to 200 km away. For each of the 20 rings around each ward, we compute two measures. First, excluding the focal ward, we sum manufacturing employment in the previous year over all wards with their centroid within the ring and divide this by the total area of wards within the ring to arrive at a measure of manufacturing workers per square km in the previous year. Second, we sum manufacturing employment in firms located in industrial zones in the previous year over all wards with their centroid within the ring and divide this by total area of wards within the ring to arrive at a measure of zone-based manufacturing worker per square km in the previous year.

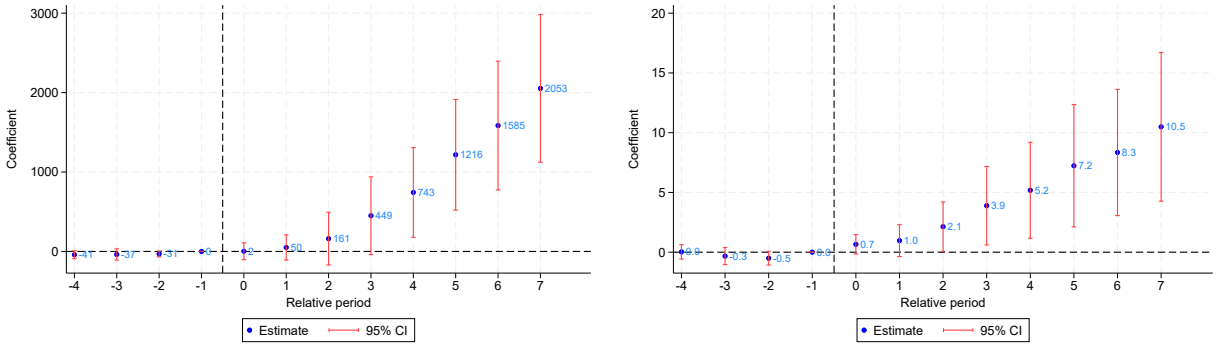
Following Kline and Moretti (2014), we take these continuous measures and construct indicators for medium and high values of manufacturing employment density overall and within zones. Specifically, over all years we calculate the 65th and 85th percentiles for manufacturing employment density and separately for the density of manufacturing employment in zones.[5] I_{brit} is an indicator for ward i in year t being exposed to $b \in \{medium, high\}$ manufacturing employment density in ring r and similarly for Z_{brit} in terms of manufacturing employment density in zones. These measures of spillovers are quite flexible and, relative to existing literature on zones, take into consideration that there could be multiple zones within a ring and the extent to which zones within the ring have generated employment. The spillovers are meant to capture possible interactions between wards within a district. They allow, for example, for the possibility that manufacturing employment in ward i could lead

[5]These correspond to 10.8 and 37.5 manufacturing workers per square km for overall manufacturing employment density and 1.1 and 8.6 manufacturing workers in zones per square km.

to an increase in manufacturing employment in ward $j \neq i$ through agglomeration effects or to a decrease through agglomeration shadows (Fujita *et al.*, 1999; Hornbeck *et al.*, 2024).

4.2 Baseline results

In this subsection, we report the results from estimating equation (1) using treatment cohorts 2005 through 2012 with never treated wards as the control wards. The main results we present are two-stage estimates where we report weighted averages of the first-stage results, where the weights are the cohort sizes, following Wooldridge (2021). Figure 6 shows the results for total employment and number of firms, including domestic private firms and foreign-owned firms. The results show no differential increase in employment or the number of firms prior to the establishment of a ward's first industrial zone. That is, conditional on the control variables included, our treatment and control wards were on similar trends prior to zone establishment. A few years after the establishment, both employment and the number of firms begin to increase and continue to increase throughout our period of analysis. By 7 years after establishment, total employment and number of firms have increased by 2,053 workers and 10.5 firms.



(a) Employment (b) Number of firms

Figure 6: Total employment and number of firms

Note: Each point reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated following equation (1). The dependent variable in panel (a) is total employment in private firms, i.e., private domestic and foreign-owned, within the ward. The dependent variable in panel (b) is the total number of private firms within the ward. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

The employment gains we document are consistent with aggregate structural change

during this period as workers moved out of agriculture into manufacturing and services, and out of informal firms to formal firms. (McCaig and Pavcnik, 2013, 2015; Asghar and McCaig, 2024). Asghar and McCaig (2024) use individual panel data from the 2002 through 2018 Vietnam Household Living Standards Surveys to study individuals moving into formal manufacturing firms in the industries most affected by the reductions in U.S. tariffs on imports from Vietnam due to the 2001 U.S.-Vietnam Bilateral Trade Agreement. They show that workers entered formal manufacturing directly from school, agriculture, and the informal sector. Additional analysis using the 2009 population census suggests that among individuals who live in wards that established an industrial zone by 2007 and that report working for a foreign manufacturing firm, 56% report moving across provincial boundaries within the past 5 years.[6] Together, this evidence suggests that industrial zones draw workers from multiple margins of adjustment, including sectoral reallocation from agriculture and informal employment as well as migration across provinces.[7]

Robustness of main results: In our main results, we omit the indicator for 5 years prior to treatment. This is to avoid the multicollinearity problem from having a complete set of relative year indicators (other than the base -1) and a linear trend (Borusyak *et al.*, 2024). To support the choice of defining the pre-existing trend by the period -5 to -1, in Section A.3 we plot the first-stage results for each treatment cohort, 2005 through 2012, from estimating equation (1) when the linear trends are excluded and we include a full set of pre-treatment indicators, other than the base -1. The cohort-by-cohort results show that when we do not include cohort linear trends, there is suggestive evidence of pre-existing trends for both employment (Figure A2) and the number of firms (Figure A3) within many cohorts. Where these pre-existing trends are potentially evident, they typically begin around 5 years prior to the establishment of the industrial zone. These pre-existing trends could be possible anticipation effects, as planning is typically underway by this time. Alternatively, they could

[6]Note that the location information in population census is based on where the individual lives, which is not necessarily the same location in which they work if they commute across administrative boundaries.

[7]Note that most migrants are moving from more rural, less developed provinces to more urban, developed provinces, suggesting they are likely leaving agriculture or the informal sector. Additionally, McCaig *et al.* (2022) report that many young migrants, 15 to 17 years old, work in formal manufacturing suggesting school to migrant worker transitions.

be due to improving economic conditions in the location ahead of industrial establishment. Note, however, that given our focus on zone establishment as our treatment timing, these pre-existing trends will not be due to activity in the zone itself as this predates land clearance, infrastructure development, and attracting tenants. To be cautious and to help reduce the concern that industrial zones are established in wards that are growing faster, which could lead us to overestimate the effect of industrial zone establishment, we introduce the cohort linear trends such that our estimates are based on deviations from the differences in trends between treatment and control wards. If these pre-existing trends are due to anticipation effects, then our estimate will be an underestimate as we're estimating deviations from the pre-existing trends.

To establish the reliability of our main findings, we conduct several robustness checks. First, we report results defining the pre-existing trend over the period -4 to -1 (Section A.4), applying the relative-magnitudes approach of Rambachan and Roth (2023) (Section A.5), and using the synthetic difference-in-differences estimator of Arkhangelsky *et al.* (2021) (Section A.6). All three yield estimates close to our main results in both pattern and magnitude. Second, Appendix Section A.8 presents regression results using an alternative set of control wards. We omit from the sample never treated wards that are in districts in which no ward is ever treated. That is, we only use control wards in the districts in which there is at least one treated ward. The findings remain consistent with our main results. Third, our results are robust to clustering standard errors at higher administrative levels, specifically at the level of districts and provinces (Appendix Sections A.9 and A.10).

Results by subgroup: In Table 2, we report the weighted average effects 7 years after establishment by ownership, by sector, and by location. These results highlight that the overall employment results shown in Figure 6 are largely accounted for by foreign firms, in manufacturing, and located inside the industrial zone.[8] For the number of firms, private domestic firms, manufacturing firms, and firms located inside the industrial zone account for most of the overall effects. In Section A.7, we show the year-by-year results for each of

[8]We do not provide regression-based estimates for employment and number of firms inside zones since these are necessarily 0 in our treatment wards. Hence, we infer activity inside zones from the difference between total and outside of zones.

these outcomes.

Table 2: Employment and number of firms by ownership, sector, and location 7 years after establishment

	Employment	Number of firms
All	2053.27***	10.49***
	(474.64)	(3.17)
	By ownership	
Foreign	1947.39***	2.72***
	(416.04)	(0.45)
Domestic	105.88	7.77**
	(195.70)	(3.10)
	By sector	
Manufacturing	1977.28***	5.28***
	(464.10)	(1.21)
Business services	15.66	0.98**
	(9.70)	(0.50)
	By location	
Outside zone	-184.19	4.78
	(189.55)	(2.93)

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each coefficient and standard error, in parentheses, reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated following equation (1). The dependent variable varies by column and row as indicated by the column and row headings. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

4.3 Heterogeneity in employment generation

The previous results focus on average effects on local employment due to industrial zone establishment. However, Figures 4 and 5 report substantial variation in local employment levels following initial industrial zone establishment. We begin exploring this heterogeneity by successively estimating equation (1) with different samples. Specifically, we sequentially remove from the sample treated wards with the highest levels of employment within the zones six years after establishment.[9] We report the results in Figure 7. In panel A, we first report results for the full sample, which corresponds to the year 7 employment estimate in Figure

[9]We rank the employment inside zones six years after establishment, rather than seven years, because firm-address information is only available through 2018 and thus is not available in year 7 (2019) for the 2012 cohort.

6. We then remove the top 5, 10, and 15 wards in terms of industrial zone employment from the set of treatment wards. As we do so, the estimate drops considerably, from 2,053 for the full sample to less than half of that, 903, after the top 15 have been removed.

In panel B of Figure 7 we remove treatment wards based on deciles. Once we have removed the top two deciles of treatment wards the point estimate has fallen to only 250 and is no longer statistically different from 0 at typical significance levels. We interpret these results cautiously as we're defining our sample based on an outcome of the industrial zones, the number of workers in firms in industrial zones. Nonetheless, these results demonstrate that the majority of wards that establish their first industrial zone experience negligible employment gains by 7 years after initial establishment. The average gains are driven by a minority of wards in which employment inside the industrial zone increases appreciably. Indeed, the top 10 percent of treated wards, ranked by employment inside zones, account for about 69 percent of the total estimated employment gains, and the top 20 percent for about 90 percent.

In the subsequent subsections we explore possible contributors to this pattern: proximity to major trade hubs, the ownership of the zone developer, and case studies to highlight other contributing factors. We take each in turn, location in subsection 4.4, developer ownership in subsection 4.5, and case studies in subsection 4.6.

4.4 Heterogeneity based on location

As reported in Table 2, the majority of employment gains come from foreign manufacturers inside zones. McCaig *et al.* (2025) document that foreign manufacturers export about 80% of their output (employment-weighted) and import about two-thirds of their inputs. For these firms, port access is not a marginal consideration but a precondition. Consistent with this, the most important provinces for merchandise trade by the end of our period are Bac Ninh, Ho Chi Minh City, Hai Phong, Binh Duong, Dong Nai, and Hanoi: in 2017 the customs departments in these six provinces accounted for 81% of total merchandise trade cleared by provincial customs departments.[10] Bac Ninh, Hai Phong, and Hanoi are in the northern corridor, while Binh Duong and Dong Nai are immediately adjacent to Ho Chi Minh City.

[10] Authors' calculations based on data reported in General Department of Viet Nam Customs (2017).

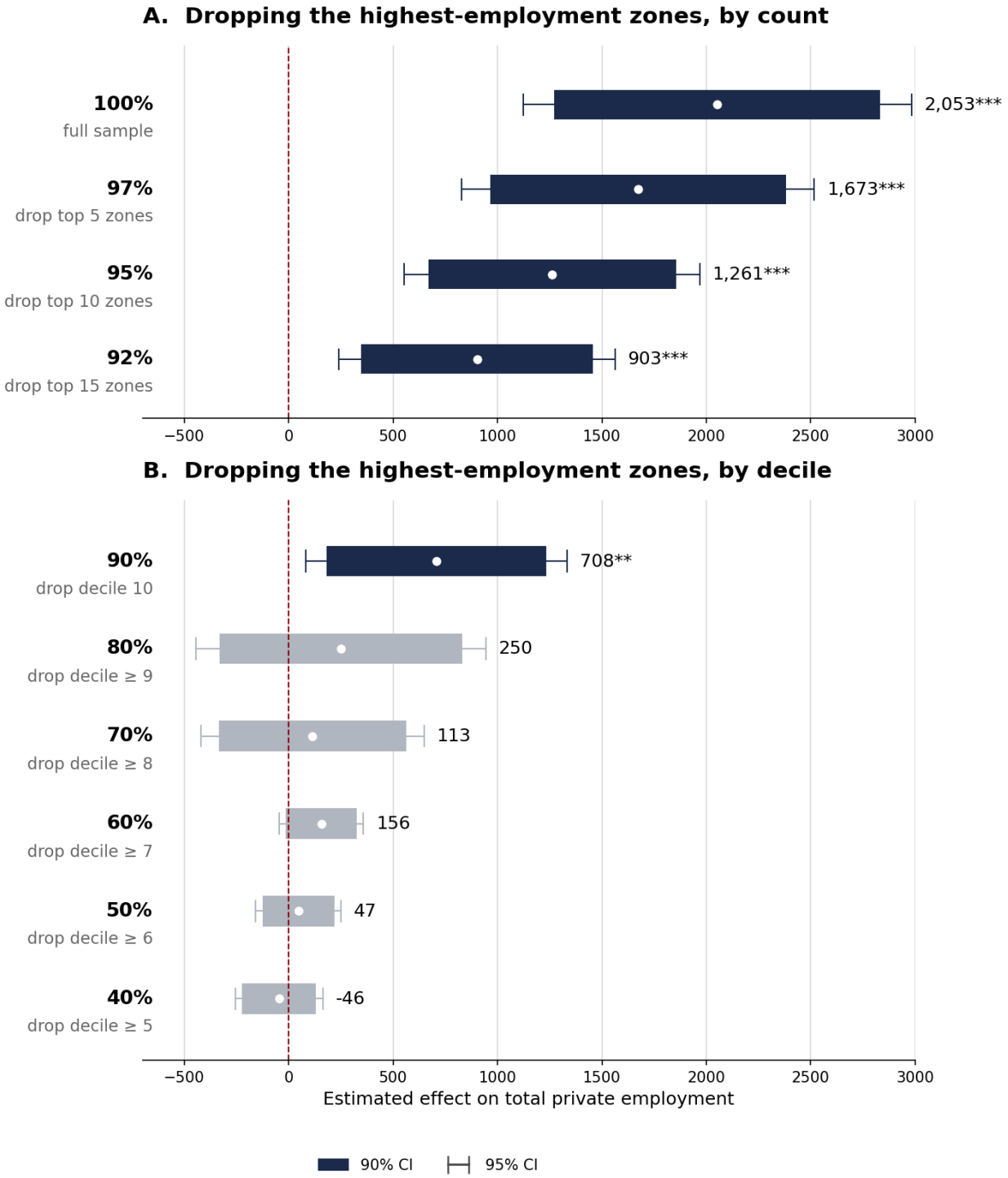
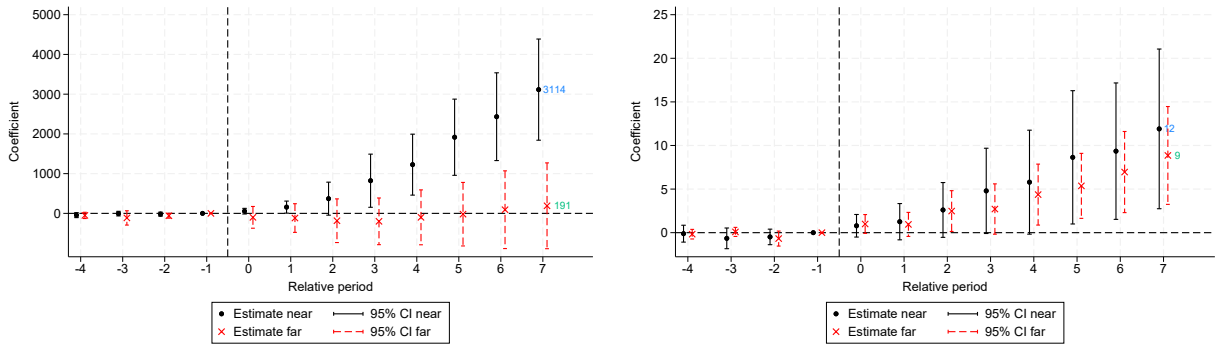


Figure 7: Most zones show no employment effect seven years out

Note: Each row reports the estimated effect on total private employment in the host ward seven years after zone establishment, from a separate estimation following equation (1). The top row ("full sample") is the baseline ATE from Figure 6; the remaining rows are estimated on subsamples that successively drop the treated wards with the highest employment inside zones six years after establishment, as reported in Table A2. Panel A drops the top N treated wards by count (top 5, 10, 15). Panel B drops treated wards at or above the indicated decile of the distribution of employment inside zones six years after establishment. The percentage at the left of each row is the share of the original treated sample retained. We rank on employment inside zones six years after establishment, rather than seven years, because firm-address information is only available through 2018. Thick bars show 90 percent confidence intervals; whiskers show 95 percent confidence intervals. Estimates are based on cohorts that established their first zone between 2005 and 2012. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Trade hub proximity. To explore the role of location, we calculate the minimum distance between each ward, including treated and control wards, and Hanoi, Hai Phong, and Ho Chi Minh City.[11] We then split the sample at the median minimum distance across all wards, a cutoff of roughly 115 km. This places 71% of treated wards in the near group.[12] We estimate equation (1) separately for wards nearer than the median distance and for wards farther than the median distance.



(a) Employment (b) Number of firms

Figure 8: Employment and number of firms by location

Note: Each point reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated following equation (1). The sample is split into wards that are less than the median distance to either Hanoi, Hai Phong, or Ho Chi Minh City and those that are greater than the median distance. The dependent variable in panel (a) is total employment in private firms, i.e., private domestic and foreign-owned, within the ward. The dependent variable in panel (b) is the total number of private firms within the ward. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

We report the results for total employment and total number of firms in Figure 8. The split is decisive: employment seven years after establishment is about 3,000 workers higher in wards within 115 km of Hanoi, Hai Phong, or Ho Chi Minh City, whereas among treated wards farther away the average employment effect is close to zero. The effects on the number of firms, by contrast, are similar across the two groups: after seven years there are approximately 12 and 9 more firms in the near and far treated wards, respectively. This apparent puzzle, similar firm gains but very different employment gains, is resolved in Table 3, which reports the year-7 estimates for employment and number of firms disaggregated by ownership, sector, and inside versus outside the zone, by ward location. In the far wards, the

[11] Bac Ninh is adjacent to Hanoi, while Binh Duong and Dong Nai are adjacent to Ho Chi Minh City.

[12] The developer analysis below is conducted at the zone level; the 121 near-hub zones used there correspond to these near treated wards, the counts differing because some zones span more than one ward.

increase in the number of firms is entirely accounted for by domestic firms, which are primarily in wholesale and retail trade (Table A1) and generate little employment. In the near wards, the firm increase includes foreign firms (4.1), and it is these foreign firms that account for almost all of the employment increase (2,834), which is concentrated in manufacturing and in firms located within the zone.

Table 3: Employment and number of firms by ownership, sector, and location 7 years after establishment by distance to major trade hubs

	Employment		Number of firms	
	Close	Far	Close	Far
All	3114.49***	190.94	11.90**	8.86***
	(649.368)	(550.838)	(4.672)	(2.863)
	By ownership			
Foreign	2833.83***	379.89***	4.09***	0.45
	(615.283)	(144.805)	(0.652)	(0.323)
Domestic	280.66**	-188.94	7.81*	8.40***
	(128.516)	(516.973)	(4.548)	(2.810)
	By sector			
Manufacturing	3033.37***	76.44	6.96***	1.82***
	(630.001)	(550.869)	(1.786)	(0.638)
Business services	28.96*	12.97**	1.32*	1.18**
	(16.750)	(5.457)	(0.781)	(0.539)
	By location			
Outside zone	-142.28	-191.71	4.32	6.85**
	(246.371)	(289.086)	(4.260)	(2.698)

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each coefficient and standard error, in parentheses, reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated following equation (1). The dependent variable varies by column and row as indicated by the column and row headings. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

4.5 Heterogeneity based on ownership of zone developer

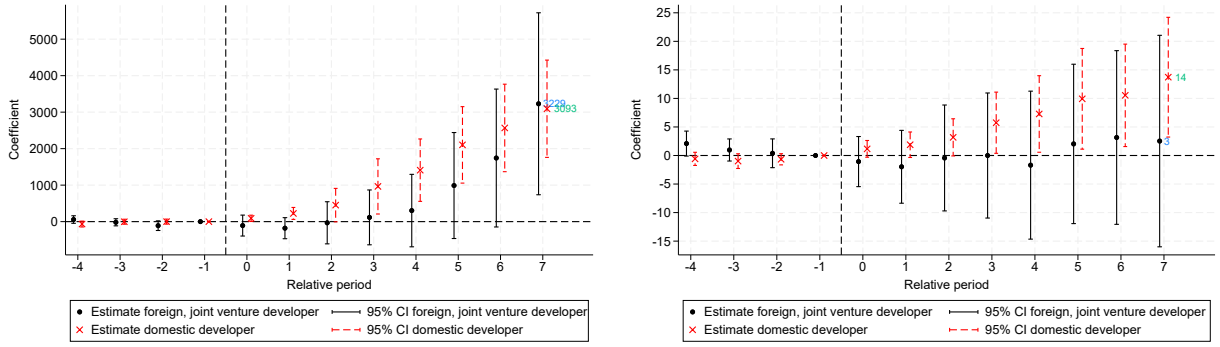
The location heterogeneity documented in Section 4.4 explains a substantial share of the variation in zone performance, but Figure 5 makes clear that even among wards close to Hanoi, Hai Phong, or Ho Chi Minh City, a meaningful fraction of zones generates few jobs after establishment. A natural margin of heterogeneity to check is the ownership of the zone developer. Foreign and joint-venture developers typically bring larger capital pools and might have stronger networks for attracting foreign manufacturing firms, which we previously documented were the source of most of the employment gains.

To test for the difference between foreign and domestic developers, Figure 9 repeats our regression analysis on the subsample of wards within median distance of major trade hubs, but separately includes treatment indicators in equation (1) for domestic and foreign-joint venture developers. In panel (a) the employment effects are generally similar in magnitude, and we consistently fail to reject the null hypothesis that the estimates are equal. In panel (b) there is suggestive evidence that domestic developers are associated with a greater increase in the number of firms, but again we consistently fail to reject the null hypothesis that the estimates are equal.

4.6 Developer capacity and zone implementation: Case studies

Section 4.5 demonstrates that the variation in employment outcomes in wards near major hubs is not systematically related to whether the zone developer is a domestic or foreign-joint venture company. We argue that much of this within-location variation reflects *developer capacity*: the developer's financial stability, its prior experience in industrial-zone development, and its ability to clear land and relocate residents. A central theme of the cases below is that this capacity is distinct from developer ownership or nationality and is not well captured by it. Capable domestic developers have built some of the most successful zones in our sample, while foreign developers without industrial-zone experience have failed.

Because direct measures of capacity—financial reserves, prior project experience, sectoral expertise—are not consistently available across our zones, we develop the argument through detailed case evidence on seven zones, comprising two paired comparisons and three



(a) Employment (b) Number of firms

Figure 9: Employment and number of firms by domestic versus foreign or joint venture developer for wards near major trade hubs

Note: Each point reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated following equation (1). The sample includes wards that are less than the median distance to either Hanoi, Hai Phong, or Ho Chi Minh City and separately includes treatment indicators for domestic and foreign-joint venture developers. The dependent variable in panel (a) is total employment in private firms, i.e., private domestic and foreign-owned, within the ward and the dependent variable in panel (b) is the total number of private firms, including both private domestic and foreign-owned, within the ward. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

individual cases. The first four form two paired comparisons that hold location or developer nationality nearly constant, isolating capacity from these confounds. The remaining three show that even well-resourced or foreign developers fail when they lack relevant experience or confront binding external constraints. Additionally, they document a recurring institutional response: when initial developers fail, Vietnamese authorities have in multiple instances reassigned stalled zones to developers with established industrial-zone experience, with measurable activation following.

Yen Phong I and Yen Phong II. Yen Phong I was established in 2006 and Yen Phong II in 2008. Both are in Yen Phong district, Bac Ninh province, about 35 km from Hanoi. Yen Phong I gained more than 40,000 workers within six years of establishment, anchored by Samsung's manufacturing complex; Yen Phong II saw essentially no employment gain. The contrast is striking because both initial developers were domestic Vietnamese firms. Yen Phong I was developed by Viglacera, a state-owned building materials and construction enterprise. Yen Phong II was developed by Song Da Construction Joint Stock Company (SICO), the construction arm of the Song Da hydropower company, with no prior experience

developing industrial zones. SICO went bankrupt and the project was terminated in 2017. Yen Phong II was subsequently split into three areas (A, B, and C) and reassigned to Western Pacific Infrastructure, VSIP (a joint venture between Vietnam's Becamex IDC and a Singaporean consortium led by Sembcorp), and Viglacera (the developer of Yen Phong I), respectively, all of which are established industrial-zone developers. Within a pair of zones held essentially constant on location and on developer nationality, the failure is associated with developer financial collapse and sectoral inexperience rather than ownership type.

Yen Binh and Nam Pho Yen. Yen Binh was established in 2012 and Nam Pho Yen in 2007. Both are in Pho Yen district, Thai Nguyen province, which borders Hanoi. Yen Binh gained nearly 15,000 workers within seven years of establishment, while Nam Pho Yen attracted only about 200. By 2022, fifteen years after establishment, the developers of Nam Pho Yen had cleared and compensated only about 50 percent of planned industrial land and had completed only parts of the zone's infrastructure. Nam Pho Yen had two developers, neither with a core background in industrial-zone development: Xuan Kien Vinaxuki Motor Joint Stock Corporation, a Vietnamese motor vehicle manufacturer that accumulated heavy debts and auctioned its assets in 2020, and Le Trach, a real estate developer originally from Taiwan operating in Vietnam through a joint venture. Le Trach completed less than half of the required infrastructure even fifteen years after establishment. Notably, the presence of a foreign-affiliated developer did not avert failure here.

What the paired cases show. Read together, the two pairs point to two dimensions of developer capacity: financial stability (SICO's bankruptcy, Vinaxuki's debts) and prior industrial-zone development experience (Viglacera's construction background versus SICO's hydropower and Le Trach's real estate backgrounds). They also point away from a nationality-based explanation. The first pair contrasts two domestic developers with sharply different outcomes, and the second includes a Taiwanese joint venture among the failing developers. Within locations held nearly fixed, it is capacity, not ownership, that separates the zones that succeeded from those that stalled.

Ownership does not track which zones succeed. This pattern is not confined to the paired cases; it is visible in the raw distribution of zone employment. Among the 121 near-hub zones in our sample, the 101 developed by domestic firms span employment levels from 0 to 41,644 workers (median 678), while the 20 developed by foreign or joint-venture firms range from 0 to 19,481 workers (median 312). Nine of the ten highest-employment zones in this subsample have domestic developers, including Viglacera (Yen Phong I, 41,644 workers), Nam Tan Uyen Corporation, Sonadezi Long Binh, and Saigon VRG. Within both ownership categories the dispersion in outcomes is wide, the bulk of zones generate little employment, and the most successful zones are at least as likely to be domestically developed as foreign, consistent with the results in Section 4.5. Developer ownership is thus a poor guide to which individual zones succeed—consistent with the paired cases above and with the three further cases below, in which the operative distinction is repeatedly capacity and experience rather than nationality.

The three remaining cases reinforce this conclusion from another angle. The first two show that even capable, experienced developers can fail when external constraints bind; the third shows that foreign ownership does not substitute for industrial-zone expertise. All three also illustrate the institutional response noted above, in which stalled zones are reassigned to experienced developers and subsequently activate.

Becamex-Binh Phuoc. Becamex IDC, the same state-owned domestic developer that successfully developed the My Phuoc and Thoi Hoa zones in Binh Duong province, was charged with developing Becamex-Binh Phuoc in Binh Phuoc province, which received its investment certificate in December 2008. Seven years after establishment, the zone featured no workers. The difficulties appear to lie in land clearance and compensation rather than developer capacity per se. As recently as 2018, a decade after establishment, the project still faced unresolved land-clearance issues, in part because the People's Committee of Chon Thanh town had not approved the necessary land support policy.[13] The case illustrates that zone success can be derailed by political and administrative factors largely outside the developer's control, even when the developer has demonstrated capacity elsewhere.

[13] https://becamexbinhphuoc.com.vn/en-US/news-events/2023-3/create-favorable-conditions-for-secondary-investors-in-industrial-zones-and-clusters-by-tran-canh.

Cam Dien-Luong Dien. The project was approved by the Prime Minister in 2008 with Phuc Hung Ltd. as the initial investor. After limited construction in 2009 and 2010, work was halted by household protests over compensation. In 2015 it was transferred to VSIP Hai Duong, a subsidiary of the VSIP joint venture, an established industrial-zone developer. Yet protests continued into 2016: of approximately 1,300 households affected, 70 had not received compensation and refused to relocate.[14] VSIP ultimately scaled down the industrial zone's footprint from its original 205.28 hectares to 149 hectares.[15] The case demonstrates that land clearance and household displacement can stall zone development even when the developer is well-resourced and experienced and has a strong track record elsewhere.

Viet Hoa-Kenmark. Viet Hoa-Kenmark Industrial Park in Hai Duong province received its investment certificate in 2006, with Kenmark Investment and Development Company, a Taiwanese investment company, as developer. The project was Kenmark's first industrial-zone development in Vietnam. The original investment plan called for $500 million in capital and modern industrial-park infrastructure on a 46-hectare site. After roughly three years of partial construction, Kenmark abruptly withdrew from Vietnam in 2010, leaving the zone unfinished and $67.6 million in debt to three Vietnamese banks. The zone sat abandoned for nearly a decade. The Bank for Investment and Development of Vietnam (BIDV), the largest creditor, organized an auction process beginning in late 2017, and An Phat Holdings, a Vietnamese industrial-materials group that subsequently expanded into industrial-park development, acquired it in 2018. The zone was officially renamed An Phat Complex by Hai Duong Province in March 2019, and subsequently activated as an operational industrial park.[16] The case illustrates that foreign ownership did not avert failure when the developer lacked industrial-zone expertise, and that reassignment of a stalled zone to a domestic developer with operational capacity produced activation of the previously abandoned project.

Synthesis. Taken together, the cases and the raw distribution of zone employment point to three conclusions about zone implementation.

[14] See https://vir.com.vn/hai-duong-to-hand-over-land-to-vsip-despite-protests-from-residents-41109.html.

[15] https://vir.com.vn/hai-duong-to-hand-over-land-to-vsip-despite-protests-from-residents-41109.html, and https://www.vsip.com.vn/Business/BranchDetail?branchID=9

[16] See https://vir.com.vn/iz-purchase-brings-an-phat-holdings-closer-to-1-billion-revenue-goal-63047.html.

First, developer capacity—financial stability and prior sectoral experience in industrial-zone development—matters substantially for whether a zone in a favorable location ultimately succeeds. This capacity does not align with developer ownership or nationality: capable domestic developers like Viglacera have generated highly successful zones, while foreign developers without industrial-zone expertise like Kenmark have failed. Neither the cases nor the raw distribution of zone employment supports the view that foreign or joint-venture ownership is itself a marker of success.

Second, factors largely outside the developer's control, particularly land clearance and compensation disputes, can stall even high-capacity developers. In our cases, the more common failure mode in favorable locations is not a zone left permanently idle but one that never finishes construction or whose original developer fails to execute.

Third, in multiple instances stalled zones have been reassigned to developers with established industrial-zone experience, and the reassigned zones have subsequently activated: Yen Phong II reassigned to three specialist developers, Cam Dien-Luong Dien to VSIP, and Viet Hoa-Kenmark to An Phat.

The pattern suggests that the policy lever for improving zone outcomes lies in two places: in the selection of developers with the financial and sectoral capacity to execute, and in the institutional capacity to redirect zones from incapable to capable developers when initial assignments fail.

4.7 Spillovers

Our analysis has thus far focused on the direct effects of industrial zone establishment on employment and firm creation within treated wards. However, as Figure 2 illustrates, industrial zones and manufacturing activity more broadly are geographically clustered. This raises a natural question: does the density of manufacturing activity in surrounding areas affect employment outcomes within a given ward? And does it matter whether that nearby manufacturing is located inside industrial zones or not?

To address these questions, we exploit the spatial structure of our data. As described in Section 4.1, for every ward in our sample—whether or not it has an industrial zone—we compute the density of manufacturing employment, excluding the focal ward, in each of 20

concentric 10 km rings extending up to 200 km from the ward's centroid. We construct parallel measures for manufacturing employment specifically within industrial zones. Following Kline and Moretti (2014), we categorize each ring-year observation as having low, medium, or high manufacturing density and estimate the effects of medium and high density (relative to low) at each distance.

This approach is considerably more flexible than the methods employed in previous studies, which typically use a simple indicator for having a zone in an adjacent administrative unit or within some fixed radius (Lu *et al.*, 2019; Chaurey, 2017; Gallé *et al.*, 2024). Our specification allows spillover effects to vary continuously with distance, to depend on the intensity of nearby economic activity rather than just its presence, and to evolve over time as zones become occupied and new zones are established. Because the spillover variables in our main specification control for nearby manufacturing activity, they also serve an important econometric purpose: they help ensure that the estimated direct effects of zone establishment are not confounded by the influence of economic activity in neighboring wards.

To estimate the spillover effects, we re-estimate equation (1) replacing district-year fixed effects with year fixed effects. We do this because district-year fixed effects would absorb much of the spatial variation we aim to measure. In this section, we do not focus on the effects of own treatment, for which we prefer the inclusion of district-year fixed effects. We only report estimates of the spillovers in this section.

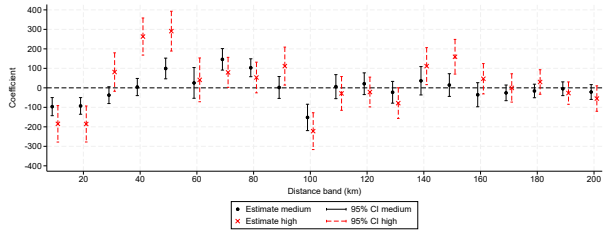
The overall pattern: agglomeration shadows and positive spillovers Figure 10 presents the spillover estimates for employment and the number of firms. Consider first the effects of overall manufacturing density on employment (Figure 10a). At close distances (0–20 km), the relationship is negative: wards surrounded by high-density manufacturing activity have approximately 100–200 fewer workers than otherwise similar wards with low-density surroundings. This pattern is consistent with what the economic geography literature terms "agglomeration shadows" (Fujita *et al.*, 1999; Hornbeck *et al.*, 2024). When there is substantial manufacturing activity nearby, competition for workers, land, and local customers tends to suppress employment in the focal ward. Workers may commute to the neighboring area where wages are slightly higher; firms may face elevated land costs due to nearby

demand; and producers oriented toward local markets face more intense product market competition.

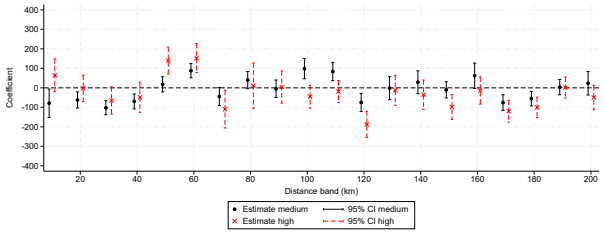
As distance increases, the competitive pressures attenuate while the benefits of being situated within a broader economic region persist. At moderate distances (40–60 km), the spillovers turn positive: focal wards with high manufacturing density 40–60 km away have more employment than focal wards with low density at that distance. At this range, firms are far enough from their neighbors to avoid direct competition for workers and local customers, but close enough to benefit from supply chain linkages, a deeper pool of specialized suppliers, and the thicker labor markets that develop around manufacturing clusters. Beyond 60 km, the estimates become smaller in magnitude and are generally not statistically distinguishable from zero, although negative around 100 km, consistent with the attenuation of both competitive and cooperative linkages over greater distances.

The spillover pattern for the number of firms (Figure 10c) mirrors that for employment. High manufacturing density in the closest rings is associated with fewer firms in the focal ward—about 20 fewer firms for high density in the 10–20 km ring—with positive effects emerging at moderate distances before fading at greater distances.

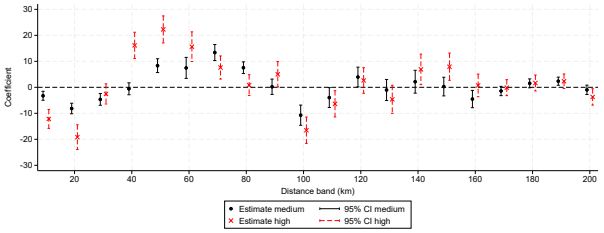
Do industrial zones generate unique spillovers? An important question for policy is whether manufacturing activity inside industrial zones generates spillovers above and beyond those from general manufacturing activity. Figures 10b and 10d address this by reporting the estimated effects of manufacturing employment specifically in nearby zones, controlling for overall manufacturing density. The zone-specific spillover estimates are notably smaller in magnitude and less precisely estimated than those for general manufacturing density. This suggests that zones do not create unique spatial externalities; rather, their effect on surrounding areas operates primarily through their contribution to overall manufacturing density. As a further check, in Appendix Figures A14 and A15 we separately distinguish nearby manufacturing employment located inside industrial zones from nearby manufacturing employment located outside zones. The patterns are broadly similar across the two, reinforcing that the spatial effects are driven primarily by manufacturing concentration itself rather than by the formal zoning status of firms. From a policy perspective, this implies that zones succeed



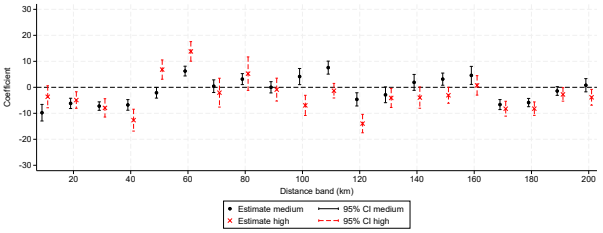
(a) Nearby manufacturing employment and employment



(b) Nearby manufacturing employment in zones and employment



(c) Nearby manufacturing employment and the number of firms



(d) Nearby manufacturing employment in zones and the number of firms

Figure 10: Spillovers on employment and the number of firms from nearby manufacturing employment and nearby manufacturing employment inside zones

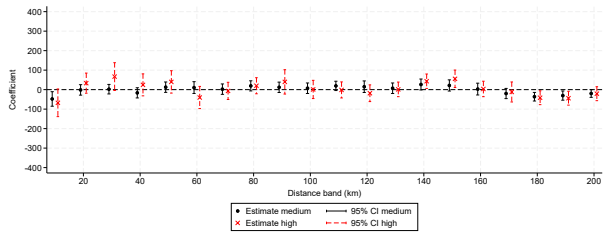
Note: Each point and 95% confidence interval are estimated following equation (1). The estimates reported in panels (a) and (b) are from one regression using total employment as the outcome and the estimates reported in panels (c) and (d) are from one regression using total number of firms as the outcome. In panels (a) and (c) we report the estimates of coefficients on indicators for medium- and high-density manufacturing employment in the respective distance ring while in panels (b) and (d) we report estimates of coefficients on indicators for medium- and high-density manufacturing employment in industrial zones in the respective distance ring. Controls include ward fixed effects, year fixed effects, treatment cohort linear time trends, and ward initial conditions interacted with year fixed effects. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

by concentrating economic activity—providing infrastructure, reducing coordination costs, and attracting firms to a single location—rather than by generating distinctive agglomeration economies that would not arise from equivalent manufacturing activity outside zone boundaries.

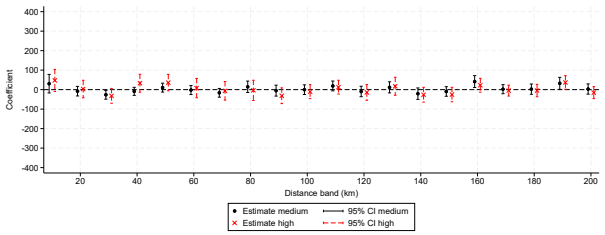
Spillover patterns differ by firm ownership The aggregate spillover patterns mask a striking difference by firm ownership that sheds light on the underlying mechanisms. In Figure 11, we decompose the spillover estimates by whether the affected employment is in foreign or domestic firms.

Foreign firms exhibit essentially no spatial spillovers at any distance (Figures 11a and 11b). The coefficients are small, precisely estimated near zero, and show no discernible pattern across distance rings. This finding is consistent with the nature of foreign manufacturing in Vietnam during this period. As documented in McCaig *et al.* (2025), foreign manufacturers export approximately 80 percent of their output (employment-weighted) and import roughly two-thirds of their intermediate inputs. Thus, they are largely decoupled from local output and input markets, two primary channels through which agglomeration shadows operate. A foreign electronics assembler in Bac Ninh exporting to global markets is neither competing for customers with nor sourcing inputs from a garment factory 10 km away. Although foreign manufacturers are still competing for workers, the evidence suggests this channel does not meaningfully contribute to spillovers for foreign manufacturers, consistent with large internal migration during this period (Huynh, 2025). The density of surrounding manufacturing activity simply does not bear on these firms' operations in a meaningful way.

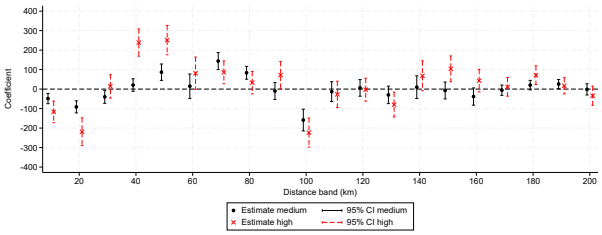
In contrast, domestic firms drive the entirety of the observed spillover patterns (Figures 11c and 11d). Domestic manufacturers, which are more oriented toward local and national markets and rely more heavily on domestic inputs, are directly affected by the competitive environment in their vicinity. When there is dense manufacturing activity nearby, domestic firms feel the squeeze: local wages are bid up, customers have alternative suppliers, and land costs rise. But when a thick manufacturing region exists at moderate distances (40–60 km), domestic firms benefit from access to a broader customer base, a deeper network of specialized



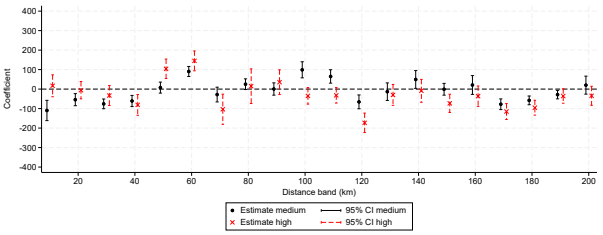
(a) Nearby manufacturing employment and employment in foreign firms



(b) Nearby manufacturing employment in zones and employment in foreign firms



(c) Nearby manufacturing employment and employment in domestic firms



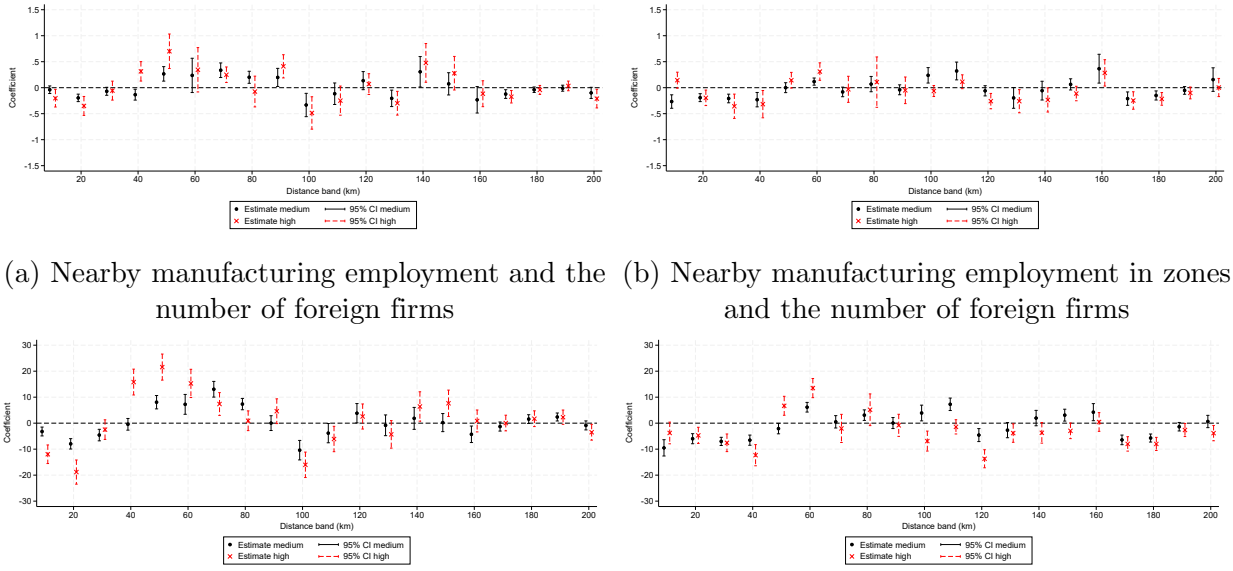
(d) Nearby manufacturing employment in zones and employment in domestic firms

Figure 11: Spillovers on employment in foreign and domestic firms from nearby manufacturing employment and nearby manufacturing employment inside zones

Note: Each point and 95% confidence interval are estimated following equation (1). The estimates reported in panels (a) and (b) are from one regression using employment in foreign firms as the outcome and the estimates reported in panels (c) and (d) are from one regression using employment in domestic firms as the outcome. In panels (a) and (c) we report the estimates of coefficients on indicators for medium- and high-density manufacturing employment in the respective distance ring while in panels (b) and (d) we report estimates of coefficients on indicators for medium- and high-density manufacturing employment in industrial zones in the respective distance ring. Controls include ward fixed effects, year fixed effects, treatment cohort linear time trends, and ward initial conditions interacted with year fixed effects. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

suppliers, and the general economic dynamism of an active regional economy—without the direct head-to-head competition that comes with immediate geographic proximity.

The number of firms shows a parallel pattern (Figure 12). Foreign firm counts are only slightly affected by surrounding manufacturing density, with effects that are an order of magnitude smaller than those for domestic firms. Domestic firm counts, by contrast, respond strongly and in the same pattern: fewer firms near dense manufacturing clusters, more firms at moderate distances.



(a) Nearby manufacturing employment and the number of foreign firms

(b) Nearby manufacturing employment in zones and the number of foreign firms

(c) Nearby manufacturing employment and the number of domestic firms

(d) Nearby manufacturing employment in zones and the number of domestic firms

Figure 12: Spillovers on the number of foreign and domestic firms from nearby manufacturing employment and nearby manufacturing employment inside zones

Note: Each point and 95% confidence interval are estimated following equation (1). The estimates reported in panels (a) and (b) are from one regression using the number of foreign firms as the outcome and the estimates reported in panels (c) and (d) are from one regression using the number of domestic firms as the outcome. In panels (a) and (c) we report the estimates of coefficients on indicators for medium- and high-density manufacturing employment in the respective distance ring while in panels (b) and (d) we report estimates of coefficients on indicators for medium- and high-density manufacturing employment in industrial zones in the respective distance ring. Controls include ward fixed effects, year fixed effects, treatment cohort linear time trends, and ward initial conditions interacted with year fixed effects. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

Implications These spatial spillover results carry several implications. First, the finding of agglomeration shadows at close distances suggests that the spatial placement of industrial zones matters. Concentrating too many zones in close proximity may be counterproductive,

as the resulting competition for workers, land, and (for domestic firms) local customers can reduce employment in each individual location. Second, the absence of zone-specific spillovers beyond general manufacturing density effects suggests that the case for zones rests on their ability to concentrate and coordinate economic activity, not on generating unique externalities. Thus, our evidence indicates that zones act primarily as a coordination device. Third, the sharp contrast between foreign and domestic firms highlights that the spatial dynamics of industrialization depend fundamentally on the nature of the firms involved. In contexts where manufacturing is oriented toward global markets with imported inputs—as with foreign manufacturing firms in Vietnam—conventional predictions from the economic geography literature may have limited applicability as foreign and domestic firms respond differently to various spatial incentives and pressures.

5 Conclusion

Vietnam's industrial zone program is widely cited as a successful example of zone-based industrialization, but the aggregate picture obscures a more complex reality. Using ward-level data on the location and establishment of industrial zones established in Vietnam between 2005 and 2012 and firm-level employment data covering the universe of formal-sector firms between 2000 and 2019, we document four sets of findings that together suggest a more nuanced reading of the Vietnamese experience.

First, the average effect of zone establishment on private formal-sector employment, approximately 2,000 workers and 10.5 firms within seven years, masks substantial heterogeneity in performance. A small share of zones generated tens of thousands of jobs, while the majority generated far fewer. 69 (90) percent of the total estimated employment gains are accounted for by the top 10 (20) percent of treated wards. This distribution is consistent with a decentralized process of provincial experimentation in which many local trials produced modest results and a small number became highly productive. It is also a reminder that summary statistics of place-based policies can be misleading when the underlying distribution of effects is heavily skewed (Bondonio and Greenbaum, 2007).

Second, two dimensions of variation account for part of the gap between successful and

unsuccessful zones. Proximity to Hanoi, Hai Phong, and Ho Chi Minh City is essential because the zones that generated the most employment overwhelmingly attracted foreign manufacturers integrated into global value chains, for whom trade hub access is a precondition rather than a marginal consideration. Among well-located zones, however, we find no statistically significant difference in employment between those developed by foreign or joint-venture developers and those developed by domestic firms; the most successful zones in our sample are at least as likely to be domestically developed as foreign. Developer ownership is thus a poor guide to which individual zones succeed. Our case studies suggest that the operative margin is instead developer capacity—financial stability, sectoral experience in industrial-zone construction, and the ability to clear land and relocate residents—which is distinct from ownership or nationality and not well captured by it. Capable domestic developers like Viglacera have generated highly successful zones, while foreign developers without industrial-zone expertise like Kenmark have failed.

Third, the spatial spillovers generated by industrial zones differ sharply by firm ownership. Foreign manufacturers show essentially no spatial relationship with nearby manufacturing activity, consistent with the enclave character of export-oriented firms that import a large share of inputs and export most of their output. Domestic firms, by contrast, experience agglomeration shadows at close distances and positive spillovers at moderate distances. The zone-specific component of these spillovers, beyond what is explained by overall manufacturing density, is modest. This suggests that industrial zones function primarily as coordination devices that concentrate infrastructure and economic activity in a single location, rather than as generators of distinctive agglomeration externalities, although we do not study possible agglomeration forces within zones.

A fourth pattern emerges across our case studies that complements the regression evidence. When initial developers failed, Vietnamese authorities in multiple instances reassigned stalled zones to developers with established industrial-zone experience, with measurable activation following: Yen Phong II was reassigned to three specialist developers, Cam Dien to VSIP, and Viet Hoa-Kenmark to An Phat. This points to institutional capacity for course-correction as a feature of the Vietnamese system distinct from the initial selection of developers, and one that may matter as much as the initial assignment for whether a zone

in a favorable location ultimately operates.

Three policy implications follow. The first concerns site selection: when the firms expected to anchor a zone are foreign export-oriented manufacturers, location relative to international trade infrastructure is not a soft preference but a binding constraint. The second concerns developer selection and the institutional capacity to redirect zones away from incapable developers: zone outcomes depend heavily on developers' financial capacity, sectoral experience, and ability to navigate land clearance and compensation. The Vietnamese record suggests that the ability to reassign stalled zones to capable developers is itself a meaningful source of variation in eventual outcomes. The third concerns the institutional environment surrounding zones: land tenure, compensation frameworks, and the relationship between developers and local authorities can stall even high-capacity projects, as our case studies of Becamex-Binh Phuoc and Cam Dien-Luong Dien illustrate.

Several limitations bound the interpretation of our results. We identify effects using comparisons within districts, which controls for many sources of unobserved variation but cannot fully rule out selection of zone sites on ward-level trends not absorbed by our cohort-trend specification. Although our robustness checks using Rambachan and Roth (2023) and synthetic difference-in-differences (Arkhangelsky *et al.*, 2021) yield estimates close to our main results, the steepness of the pre-trends in some cohorts warrants caution. The developer-ownership analysis cannot fully separate developer capacity from selection of higher-quality developers into better sites within the near-trade hubs sample. Our case studies are designed to hold location or developer nationality nearly constant but cannot, with two pairs and three individual cases, identify the role of developer capacity independently of idiosyncratic factors such as the location decisions of large anchor tenants. We document that a substantial share of workers in foreign manufacturing firms located in zones in treated wards are inter-provincial migrants, but our firm data do not allow us to trace the welfare implications of zone establishment for local versus migrant workers, nor to assess incidence at the household level.

The Vietnamese experience also has features that limit direct extrapolation to other developing countries. Vietnam's deep integration into East Asian production networks, its WTO accession in 2007 and the U.S.–Vietnam Bilateral Trade Agreement in 2001, and its

proximity to mature manufacturing in southern China shape the demand for zone-based platforms in ways that many other countries cannot replicate. The lesson of the Vietnamese record is therefore less about copying a specific configuration of zones and more about understanding industrial policy as a process of decentralized experimentation and learning rather than the implementation of a fixed blueprint (Hausmann and Rodrik, 2003; Rodrik, 2004). For developing-country policymakers considering zone-based industrialization, the central design question is not which zone characteristics to mandate but how to build the institutional capacity, provincial competition, developer entry, the political willingness to absorb failed zones, and the analytical capacity to learn from them and redirect resources accordingly.

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A Appendix

A.1 Construction of Consistent Commune and Ward Boundaries

Vietnam experienced numerous administrative boundary changes during the period covered by our analysis, 1999 to 2019. New communes and wards were created through subdivision of existing units, some units were merged, others were reclassified as urban wards or townships, and some were reassigned to different districts or even provinces. These changes create challenges for longitudinal analysis because the same administrative code may refer to different geographic areas over time, while newly created units may have no direct counterpart in earlier datasets.

In 1999, Vietnam had 61 provinces, 614 districts, and 10,471 wards and communes. In 2019, there were 63 provinces, 712 districts, and 11,165 wards and communes. Our consistently defined administrative boundaries, based on 1999 boundaries as close as possible, features 61 provinces, 614 districts, and 10,135 wards and communes.

To facilitate consistent analysis across multiple data sources and years, we construct a harmonized system of commune- and ward-level administrative units based on a fixed reference geography.

The harmonization combines information from:

1. Official administrative codebooks issued by the General Statistics Office (GSO) that provide names and codes for provinces, districts, and communes/wards
2. Population census files (1999, 2009, and 2019)
3. Vietnam Enterprise Surveys from 2000 through 2019
4. Vietnam Household Living Standards Surveys from 2002 through 2018
5. Official records documenting administrative splits, mergers, and reclassifications.
6. Administrative boundary shapefiles

The harmonization proceeds in four steps:

1. We identify all commune- and ward-level administrative units appearing in the source datasets and establish correspondence across administrative code systems used in different years.

2. We document all administrative changes affecting these units, including splits, mergers, code changes, and transfers across higher-level jurisdictions.

3. We define a fixed reference geography and assign each historical unit to a harmonized identifier. When a unit is subdivided, all successor units are linked back to the original reference unit. When units are merged, all merged communes and wards are linked to the same consistent code.

4. We construct geographic files, centroids, and crosswalks linking harmonized units to the original administrative codes used in each year.

Several validation exercises were performed to ensure consistency:

1. All harmonized units were checked against official administrative records. Where discrepancies existed, such as a geographic code existing in a microdataset but not the official codes, we did extensive searching and mapping to reconcile the discrepancy

2. Geographic boundaries were visually inspected using GIS software to verify that merged and split units corresponded to contiguous geographic areas. Note that there are a small number of districts in Vietnam that feature two non-contiguous components, even among those that do not include islands. We kept that feature in our dataset.

3. Aggregate population totals were compared before and after harmonization to ensure that no population was lost or duplicated during the crosswalk process.

The final dataset includes:

1. Harmonized commune and ward identifiers

2. Crosswalks to original administrative codes

3. Notes of year-to-year changes in administrative boundaries, including decree numbers where available

4. Geographic boundaries

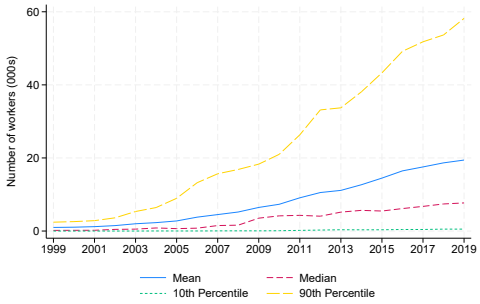
5. Centroids (note that some centroids do not appear within the boundaries of the respective ward/commune or district due to the shape of the administrative unit)

6. Area measures

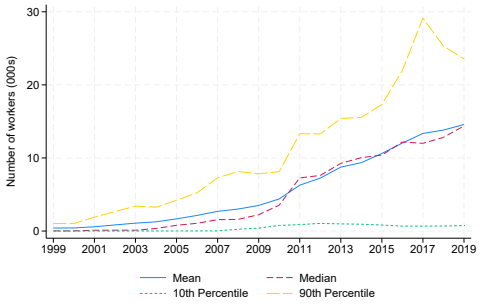
7. Harmonized district and province identifiers

These files provide a consistent geographic framework for combining Vietnamese microdata across multiple years and sources.

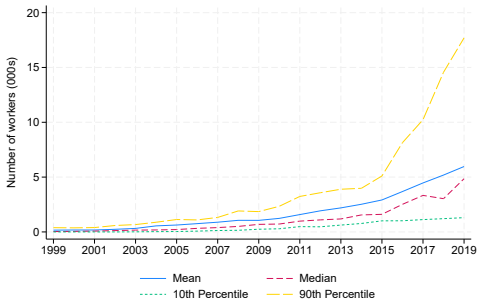
A.2 Additional descriptive figures



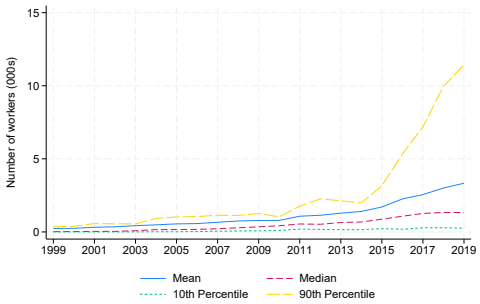
(a) 2005



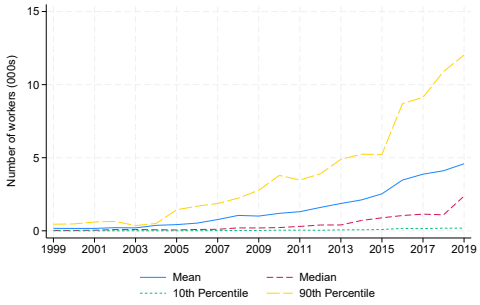
(b) 2006



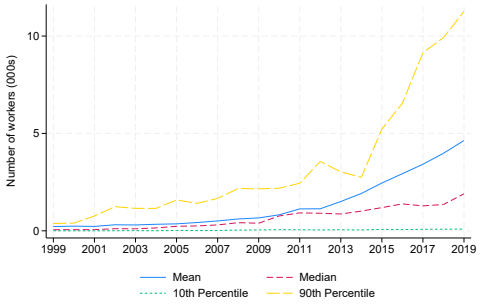
(c) 2007



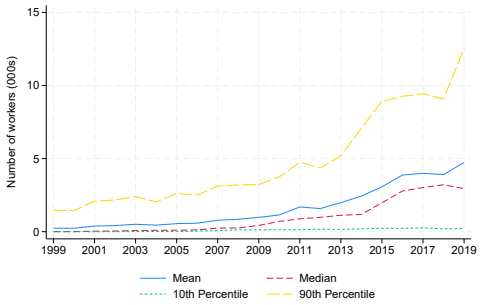
(d) 2008



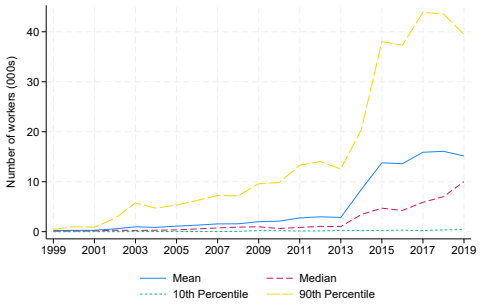
(e) 2009



(f) 2010



(g) 2011



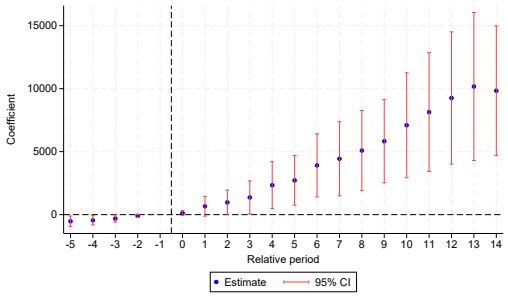
(h) 2012

Figure A1: Variation in employment over time for wards within each treatment cohort

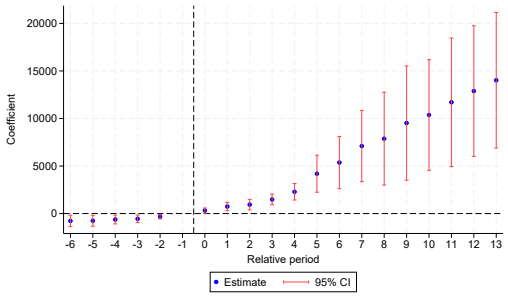
Note: Each panel corresponds to wards that established their first industrial zone in the indicated cohort year (2005 through 2012). When the zone spans more than one ward, we sum employment over all wards that share the same zone and divide by the number of wards. When calculating the means and percentiles, we weight the observations by the number of wards that share the zone.

A.3 Results by cohort without linear trends

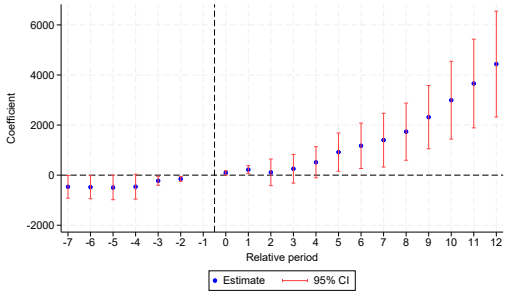
Our main results are based on estimating equation (1), which includes cohort linear trends. We exclude the indicator for five years prior to treatment such that our pre-existing linear trends are defined by the period -5 to -1. In this subsection, we report results based on excluding the linear time trend and including the indicator for 5 years prior to industrial zone establishment. Note that the vertical scales differ across cohorts within the same figure.



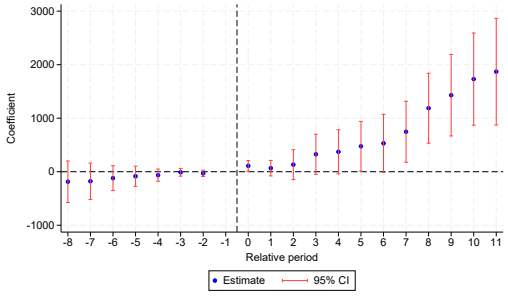
(a) 2005



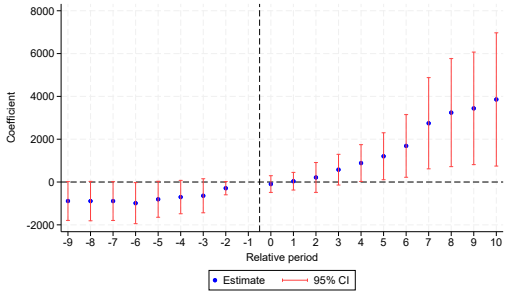
(b) 2006



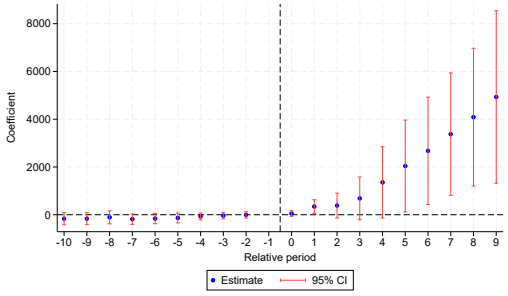
(c) 2007



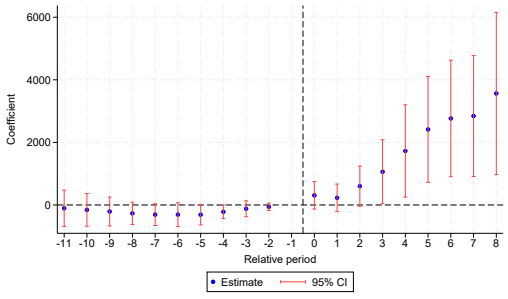
(d) 2008



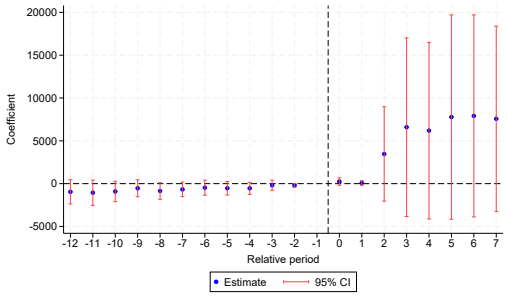
(e) 2009



(f) 2010



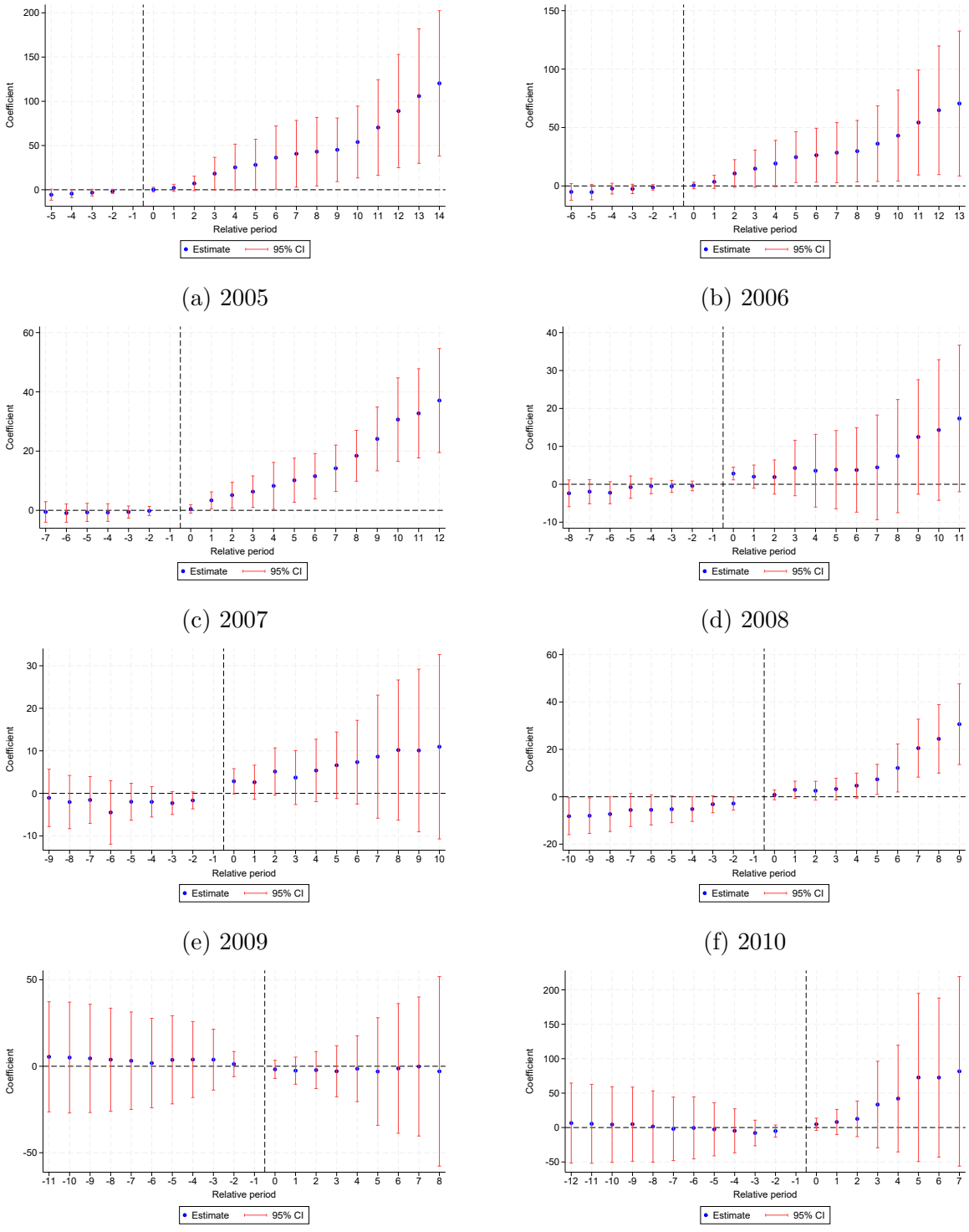
(g) 2011



(h) 2012

Figure A2: Employment by cohorts

Note: Each point reflects a relative year effect for the respective cohort, where they are estimated following equation (1). Each panel (a)–(h) corresponds to a treatment cohort from 2005 through 2012, and the dependent variable is total employment in private firms, i.e., private domestic and foreign-owned, within the ward. Note that the vertical scales differ across cohorts. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.



(a) 2005

(b) 2006

(c) 2007

(d) 2008

(e) 2009

(f) 2010

(g) 2011

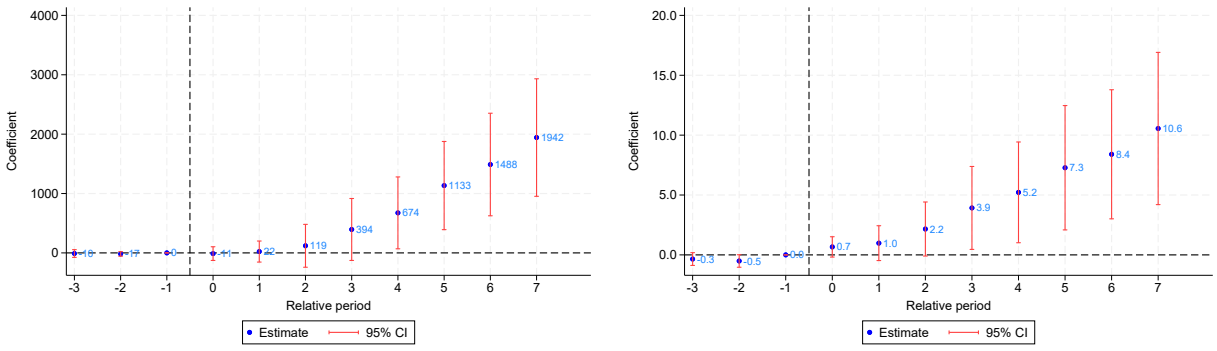
(h) 2012

Figure A3: Number of firms by cohorts

Note: Each point reflects a relative year effect for the respective cohort, where they are estimated following equation (1). Each panel (a)–(h) corresponds to a treatment cohort from 2005 through 2012, and the dependent variable is the total number of private firms within the ward. Note that the vertical scales differ across cohorts. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

A.4 Results based on alternative definitions of the linear trend

In our main results, we exclude the pre-treatment indicator for 5 years prior to treatment. With the inclusion of a cohort linear trend, this implies that the linear trend is defined by the periods -5 to -1. Although the choice of omitting the period -5 is motivated by the results shown in Section A.3, the choice is nonetheless arbitrary to some degree. As such, in this section we estimate equation (1) with the period -4, instead of -5, omitted such that the pre-trend is defined over the period -4 to -1. We report the results for total employment and firms in Figure A4. The results for employment are very similar to our main results reported in Figure 6 as there is again minimal evidence of a pre-existing trend and employment steadily increases after the establishment of the industrial zone, to about 1,942 additional workers in year 7. For the number of firms, we similarly see no evidence of effects prior to the establishment of the industrial zone and the estimate in year 7, 10.6, is very close to our main estimate.



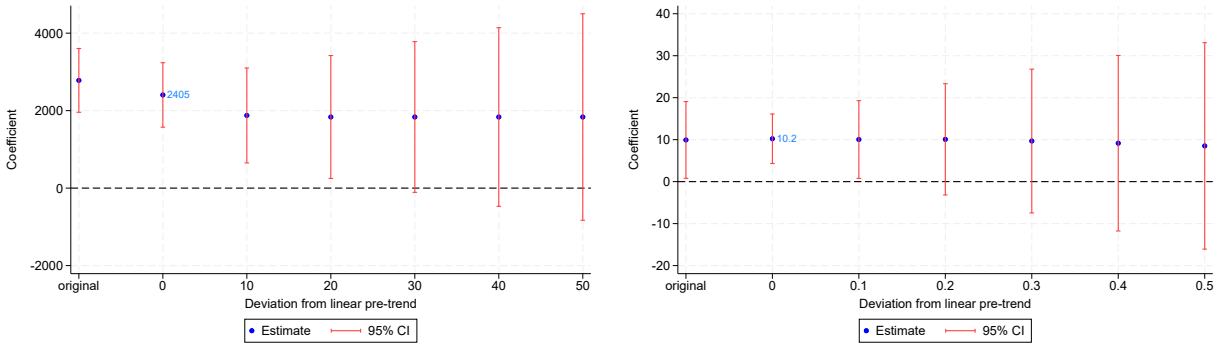
(a) Employment (b) Number of firms

Figure A4: Total employment and number of firms, omitted year -4 to detrend linearly

Note: Each point reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated following equation (1). The dependent variable in panel (a) is total employment in private firms, i.e., private domestic and foreign-owned, within the ward. The dependent variable in panel (b) is the total number private firms within the ward. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

A.5 Robustness to possible violations of pre-existing trends

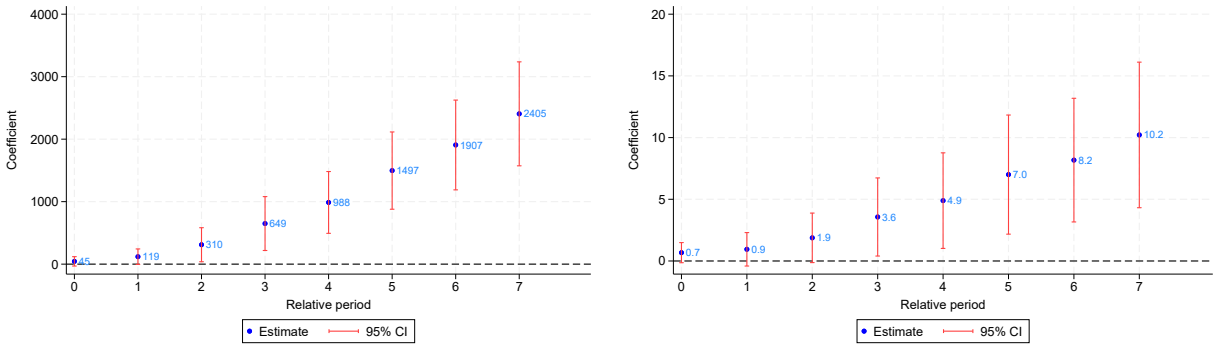
In this section, we report estimates from using the `HonestDiD` package from Rambachan and Roth (2023). We estimate (1) without the cohort linear trends, $\lambda_c t$, and then feed these estimates into the `HonestDiD` package. We report the results for relative year 7 in Figure A5 and relative years from 0 to 7 in Figure A6. For the employment results, the "original" estimate represents the estimates derived from estimating equation (1) without the cohort linear trends and then calculating a weighted average of the estimate of $\mu_{7,c}$ where the weights are the number of wards in each cohort. The "0" estimate represents the relative year 7 estimate adjusted for a linear pre-existing trend as calculated by the approach of Rambachan and Roth (2023). The estimate, 2,405, is slightly higher than the estimate 2,053 we reported in Figure 6. Similarly, the "0" estimates for the number of firms using the approach of Rambachan and Roth (2023) suggests 10.2 more firms in year 7 as compared to 10.5 in Figure 6. Together, the closeness of our main estimates, which define the pre-existing trend between periods -5 and -1, to those that instead adjust for the linear pre-existing trend estimated by the `HonestDiD` package of Rambachan and Roth (2023) indicates that this adjustment does not materially change our conclusions.



(a) Employment (b) Number of firms

Figure A5: Honest DID results for coefficient relative year 7

Note: Each point reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated by the the `HonestDiD` package following estimating equation (1) without cohort linear trends. The dependent variable in panel (a) is total employment in private firms, i.e., private domestic and foreign-owned, within the ward. The dependent variable in panel (b) is the total number private firms within the ward. Controls include ward fixed effects, district-year fixed effects, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.



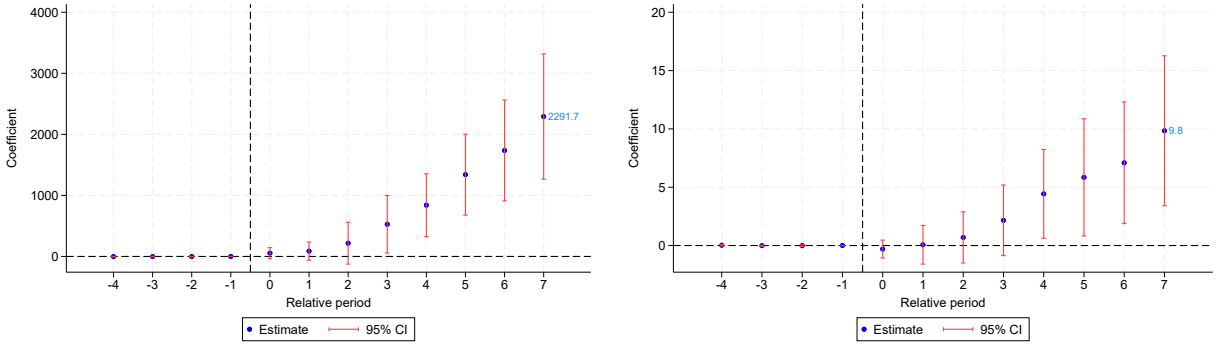
(a) Employment (b) Number of firms

Figure A6: Honest DID results for coefficient relative years 0-7, adjusting for linear pre-existing trend

Note: Each point reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated by the the `HonestDiD` package adjusted for a linear pre-existing trend as calculated by the approach of Rambachan and Roth (2023). The dependent variable in panel (a) is total employment in private firms, i.e., private domestic and foreign-owned, within the ward. The dependent variable in panel (b) is the total number private firms within the ward. Controls include ward fixed effects, district-year fixed effects, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

A.6 Estimates based on synthetic difference-in-differences

In this section, we report results from using the synthetic difference-in-differences estimator (SDID) by Arkhangelsky *et al.* (2021). The method reweights observations to match pre-exposure trends. We present the results in Figure A7. We estimate a simplified version of equation (1) where the district-year fixed effects have been replaced by year fixed effects, the vector of initial conditions interacted with year fixed effects are dropped, as are the spillovers. The SDID estimates, about 2,300 workers and 9.8 firms seven years after establishment, are close to our main estimates of 2,053 workers and 10.5 firms in Figure 6, despite the simpler specification.

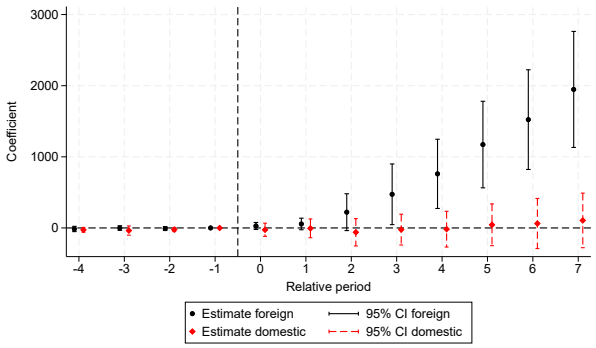


(a) Employment (b) Number of firms

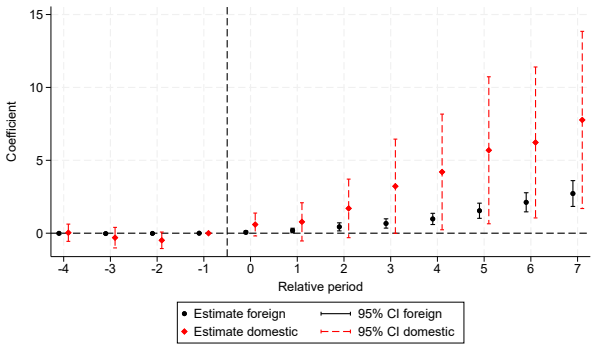
Figure A7: Synthetic difference in difference without controlling for spillovers

A.7 Time profile of results for other outcomes

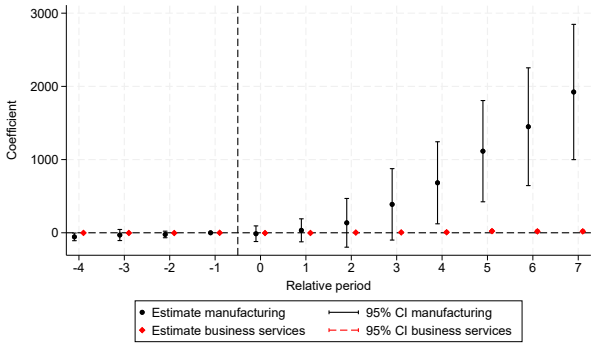
In Table 2 we reported the results for a number of different outcomes 7 years after industrial zone establishment. In this subsection we report the results for each of these outcomes for relative years -4 to 7, as in Figure 6 for total employment and total number of firms. We report the results in Figure A8. The estimates show that the results consistently build over time, as for total employment and total number of firms. For example, employment in foreign firms shows no evidence of a pre-existing trend in panel (a) and then slowly starts to increase around 2 years after the zone establishment. We see a similar pattern for manufacturing employment. Employment outside zones fluctuates around 0 for the whole 7 years.



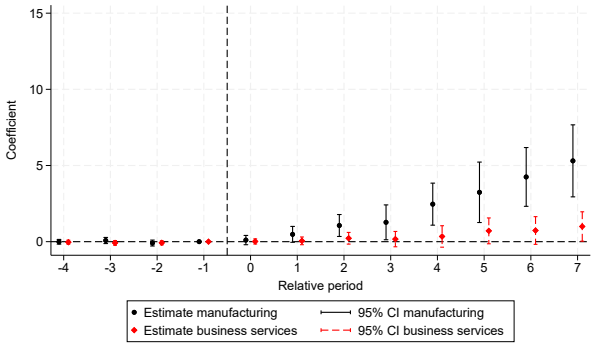
(a) Employment by foreign and domestic



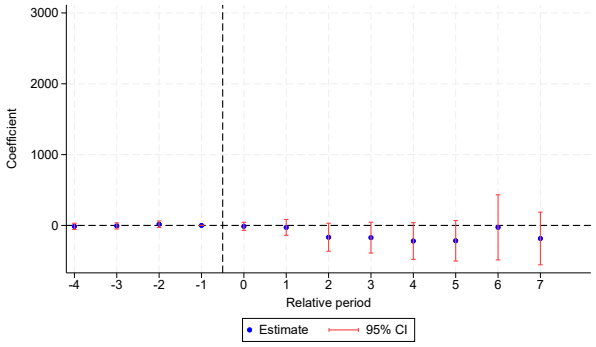
(b) Number of firms by foreign and domestic



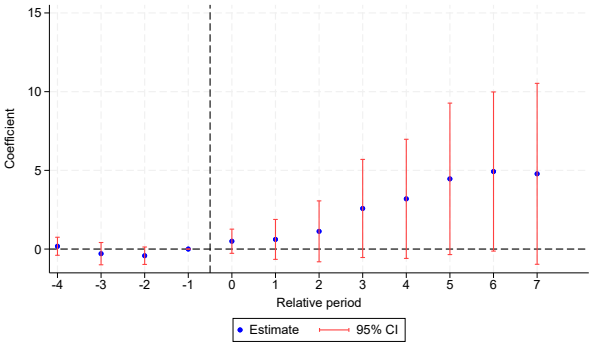
(c) Employment in manufacturing and business services



(d) Number of firms in manufacturing and business services



(e) Employment outside of zones



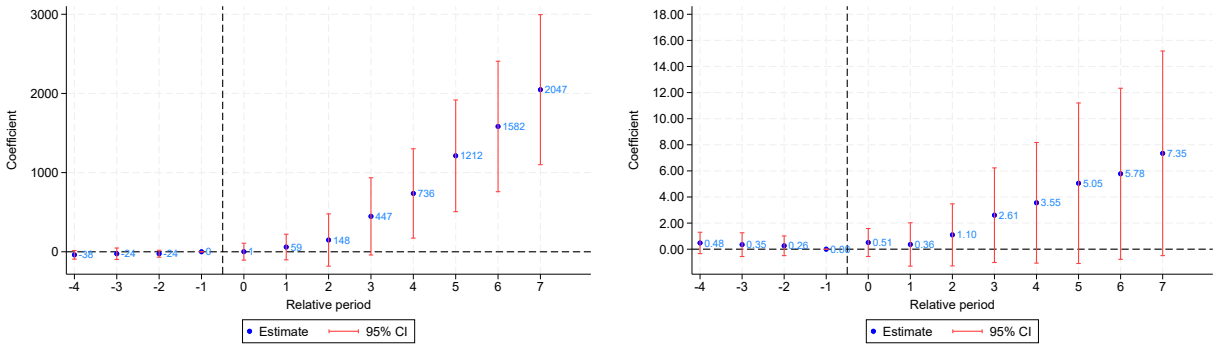
(f) Number of firms outside of zones

Figure A8: Employment and number of firms by ownership, sector, and location within the ward

Note: Each point reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated following equation (1). Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

A.8 Alternative definitions of control wards

In this section we explore the sensitivity of our main results to a change in the set of control wards. In our main results, we use all wards that are untreated as of 2023 as control wards. This includes wards in districts in which no ward is ever treated. Controls wards in never-treated districts could potentially influence our estimates of treatment indirectly by influencing the estimated coefficients on included controls. We examine this possibility in Figure A9, which reports results from estimating equation (1) using as control wards only those in districts in which at least one ward is treated. The results confirm our main results.

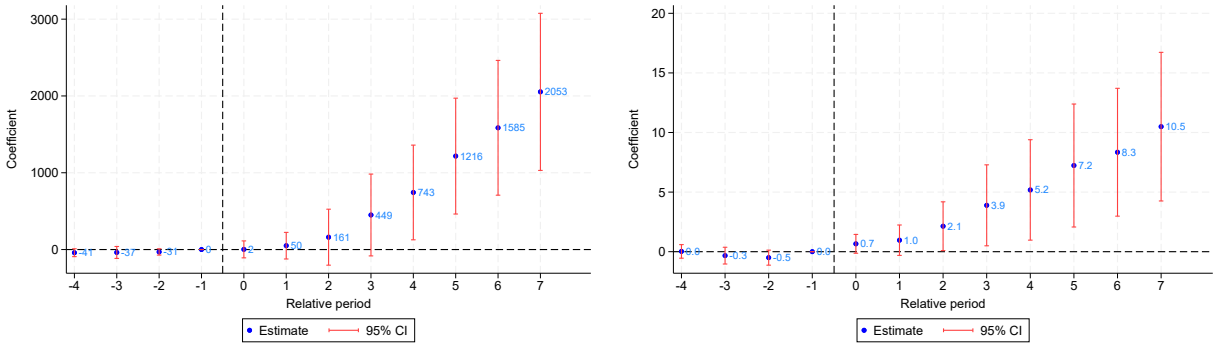


(a) Employment (b) Number of firms

Figure A9: Total employment and number of firms using control wards in districts in which at least one ward is treated

Note: Control group consists of non-treated wards in treated districts. Each point reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated following equation (1). The dependent variable in panel (a) is total employment in private firms, i.e., private domestic and foreign-owned, within the ward. The dependent variable in panel (b) is the total number private firms within the ward. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by ward.
Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

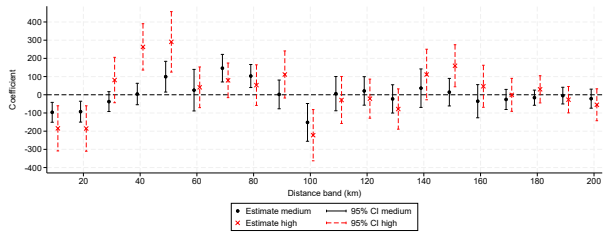
A.9 Standard errors clustered by district



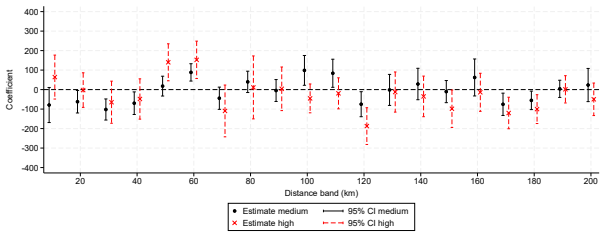
(a) Employment (b) Number of firms

Figure A10: Total employment and number of firms. Standard errors are clustered by district.

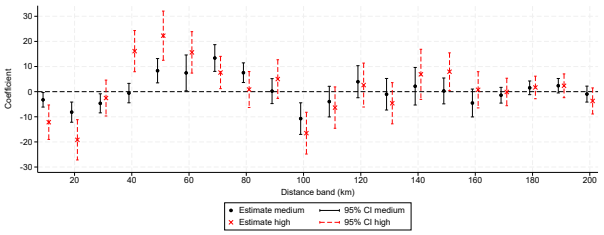
Note: Control group consists of untreated wards as of 2023. Each point reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated following equation (1). The dependent variable in panel (a) is total employment in private firms, i.e., private domestic and foreign-owned, within the ward. The dependent variable in panel (b) is the total number private firms within the ward. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by district. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.



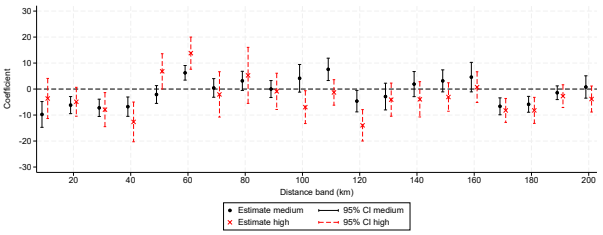
(a) Nearby manufacturing employment and employment



(b) Nearby manufacturing employment in zones and employment



(c) Nearby manufacturing employment and the number of firms

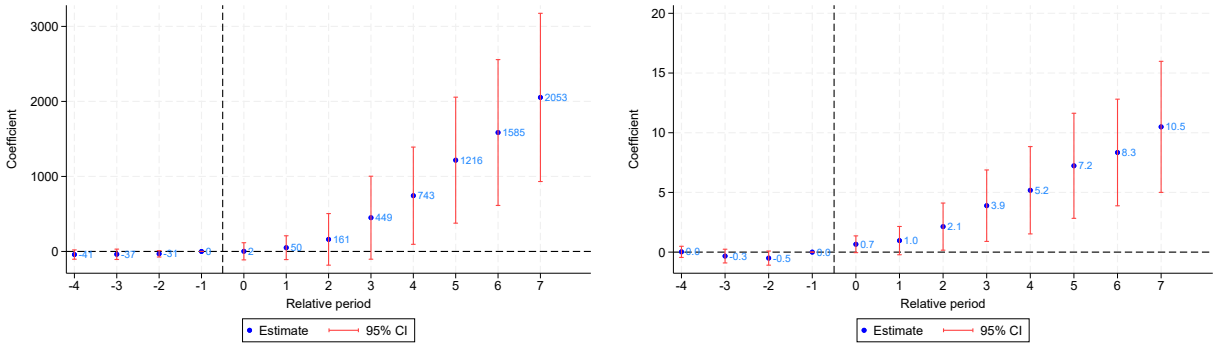


(d) Nearby manufacturing employment in zones and the number of firms

Figure A11: Spillovers on employment and the number of firms from nearby manufacturing employment and nearby manufacturing employment inside zones. Standard errors are clustered by district.

Note: Each point and 95% confidence interval are estimated following equation (1). The estimates reported in panels (a) and (b) are from one regression using total employment as the outcome and the estimates reported in panels (c) and (d) are from one regression using total number of firms as the outcome. In panels (a) and (c) we report the estimates of coefficients on indicators for medium- and high-density manufacturing employment in the respective distance ring while in panels (b) and (d) we report estimates of coefficients on indicators for medium- and high-density manufacturing employment in industrial zones in the respective distance ring. Controls include ward fixed effects, year fixed effects, treatment cohort linear time trends, and ward initial conditions interacted with year fixed effects. Standard errors clustered by district. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

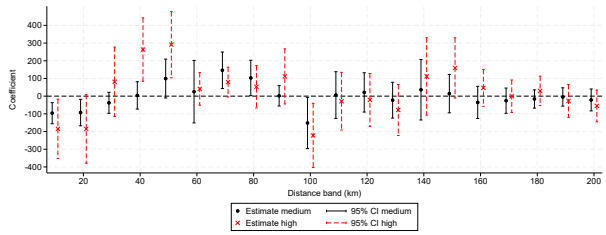
A.10 Standard errors clustered by province



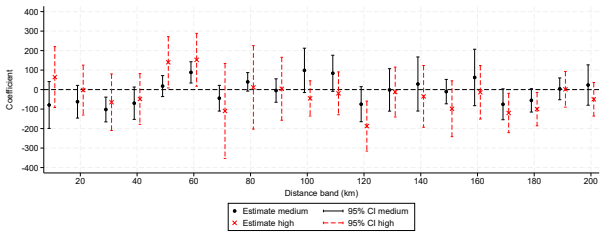
(a) Employment (b) Number of firms

Figure A12: Total employment and number of firms. Standard errors are clustered by province.

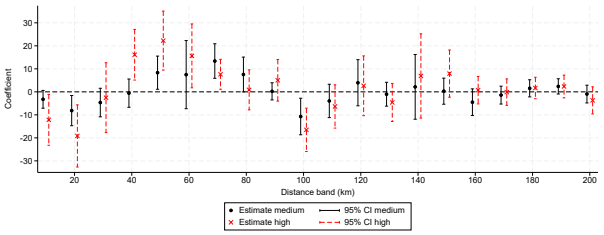
Note: Control group consists of untreated wards as of 2023. Each point reflects a weighted average of relative year effects, where the relative year effects by cohort are estimated following equation (1). The dependent variable in panel (a) is total employment in private firms, i.e., private domestic and foreign-owned, within the ward. The dependent variable in panel (b) is the total number private firms within the ward. Controls include ward fixed effects, district-year fixed effects, treatment cohort linear time trends, ward initial conditions interacted with year fixed effects, and indicators for medium- and high-density manufacturing employment in nearby wards and in nearby industrial zones. Standard errors clustered by province. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.



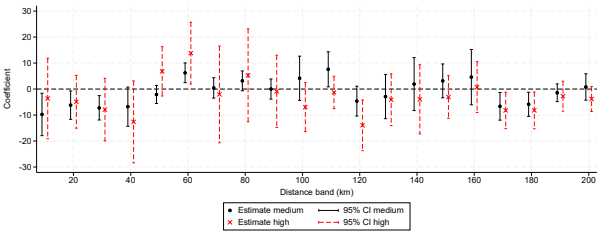
(a) Nearby manufacturing employment and employment



(b) Nearby manufacturing employment in zones and employment



(c) Nearby manufacturing employment and the number of firms



(d) Nearby manufacturing employment in zones and the number of firms

Figure A13: Spillovers on employment and the number of firms from nearby manufacturing employment and nearby manufacturing employment inside zones. Standard errors are clustered by province.

Note: Each point and 95% confidence interval are estimated following equation (1). The estimates reported in panels (a) and (b) are from one regression using total employment as the outcome and the estimates reported in panels (c) and (d) are from one regression using total number of firms as the outcome. In panels (a) and (c) we report the estimates of coefficients on indicators for medium- and high-density manufacturing employment in the respective distance ring while in panels (b) and (d) we report estimates of coefficients on indicators for medium- and high-density manufacturing employment in industrial zones in the respective distance ring. Controls include ward fixed effects, year fixed effects, treatment cohort linear time trends, and ward initial conditions interacted with year fixed effects. Standard errors clustered by province. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

A.11 Additional Regression Tables and Figures

Table A1: Number of firms private domestic firms by industry 7 years after establishment

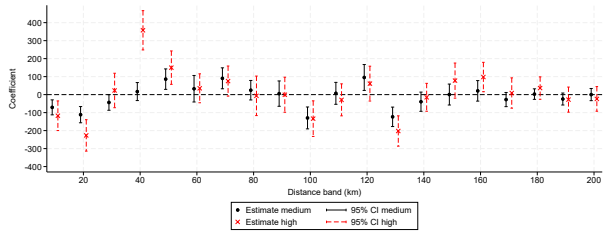
	Number of firms
A: Agriculture, hunting and forestry	-0.01
	(0.17)
B: Fishing	-0.23
	(0.16)
C: Mining and quarrying	-0.03
	(0.12)
D: Manufacturing	1.38
	(1.18)
E: Electricity, gas and water supply	0.18*
	(0.11)
F: Construction	1.21
	(0.76)
G: Wholesale and retail trade; repair of motor vehicles and household goods	3.83*
	(2.23)
H: Hotels and restaurants	0.35
	(0.32)
I: Transport, storage and communications	0.44
	(0.40)
J: Financial Intermediation	0.09
	(0.11)
K: Scientific and industrial activities	-0.00
	(0.02)
L: Real estate, renting and business activities	1.22*
	(0.63)
M: Public administration and defence; compulsory social security	0.00
	(0.00)
N: Education	-0.11
	(0.08)
O: Health and social work	-0.00
	(0.04)
P: Sporting and other recreational activities	-0.03
	(0.05)
Q: Activities of the party, organizations and associations	0.01
	(0.01)
T: Community, social and personal service activities	0.01
	(0.01)
U: Private households with employed persons	0.01
	(0.01)
V: Extra-territorial organizations and bodies	0.01
	(0.01)

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

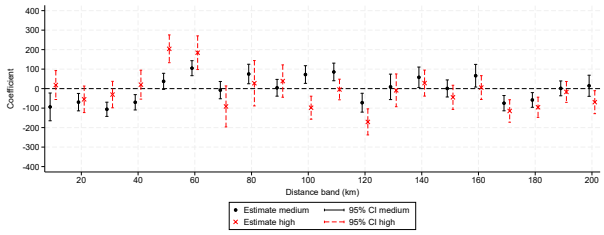
Table A2: Most zones show no employment effect seven years out

	Total private employment
full sample	2053.27***
	(474.64)
drop top 5 zones	1672.73***
	(430.79)
drop top 10 zones	1261.28***
	(360.69)
drop top 15 zones	902.71***
	(337.59)
drop top 20 zones	626.55*
	(322.67)
drop top 25 zones	481.89
	(327.30)
drop decile 10	708.08**
	(319.74)
drop decile >= 9	249.51
	(353.93)
drop decile >= 8	113.30
	(272.42)
drop decile >= 7	155.68
	(103.23)
drop decile >= 6	46.79
	(104.33)
drop decile >= 5	-46.14
	(107.48)

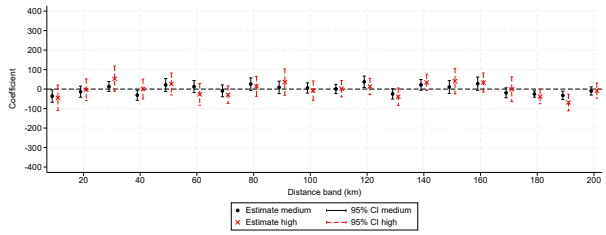
Note: Each coefficient and standard error, in parentheses, reflects a weighted average of relative year effects estimated following equation (1), seven years after establishment. Estimates are based on cohorts that established their first zone between 2005 and 2012. Each row re-estimates the effect on total private employment after excluding the wards with highest employment inside zones 6 years after establishment: "drop top N" excludes the N wards with highest employment inside zones 6 years after establishment; "drop decile" excludes wards with zones at or above the indicated decile of the employment 6 years after establishment. The full-sample estimate is the same as that reported in Table 2, and the specification follows equation (1). Standard errors clustered by ward. We rank the employment inside zones 6 years, instead of 7 years, after establishment because we only have firm addresses until 2018. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.



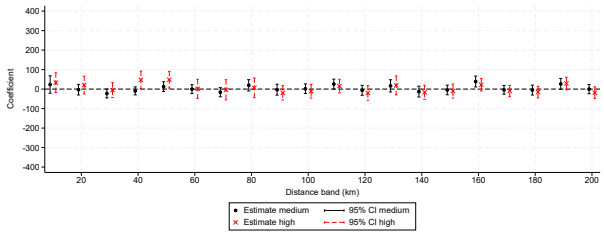
(a) Nearby manufacturing employment **outside zones** and **employment**



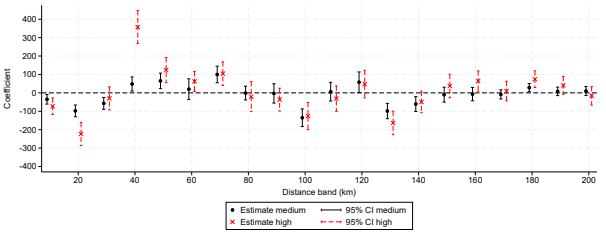
(b) Nearby manufacturing employment **in zones** and **employment**



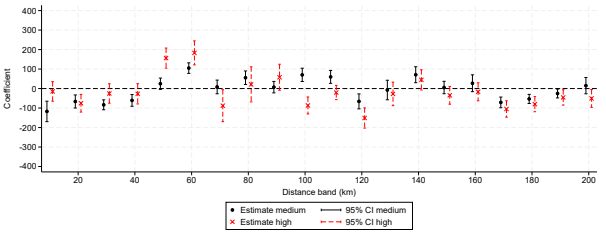
(c) Nearby manufacturing employment **outside zones** and **foreign employment**



(d) Nearby manufacturing employment **in zones** and **foreign employment**



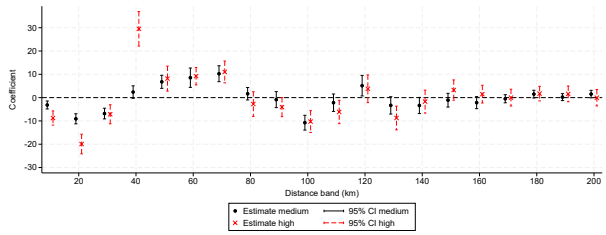
(e) Nearby manufacturing employment **outside zones** and **private domestic employment**



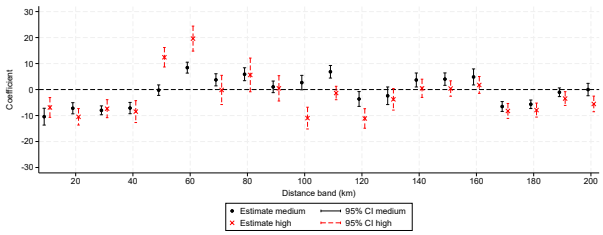
(f) Nearby manufacturing employment **in zones** and **private domestic employment**

Figure A14: Spillovers on **employment** from nearby manufacturing employment **inside zones and outside of zones**. **Regressions account for spillovers from nearby zones and nearby non-zone areas**

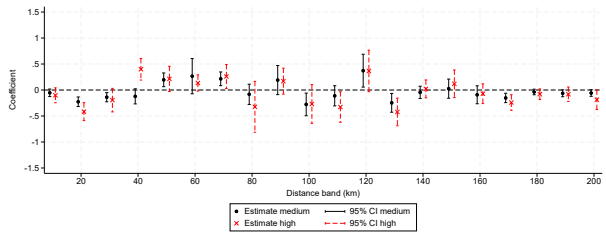
Note: Each point and 95% confidence interval are estimated following equation (1). The estimates reported in panels (a) and (b) are from one regression using the employment of firms as the outcome, the estimates reported in panels (c) and (d) are from one regression using the employment of foreign firms as the outcome, and the estimates reported in panels (e) and (f) are from one regression using the employment of domestic firms as the outcome. In panels (a), (c), and (e), we report the estimates of coefficients on indicators for medium- and high-density manufacturing employment outside industrial zones in the respective distance ring. In panels (b), (d), and (f), we report estimates of coefficients on indicators for medium- and high-density manufacturing employment in industrial zones in the respective distance ring. Controls include ward fixed effects, year fixed effects, treatment cohort linear time trends, and ward initial conditions interacted with year fixed effects. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.



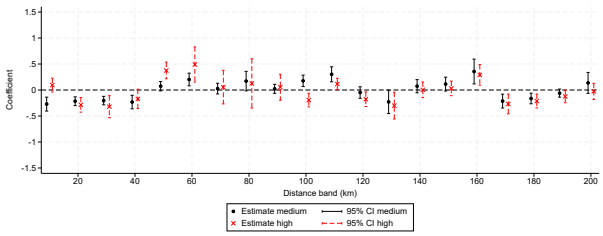
(a) Nearby manufacturing employment **outside zones** and **number of firms**



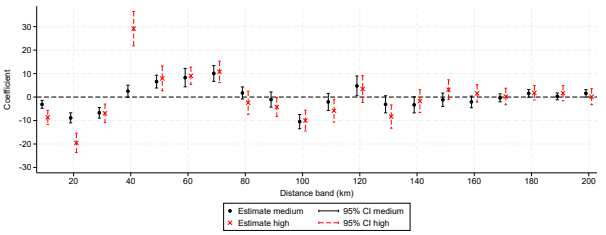
(b) Nearby manufacturing employment **in zones** and **number of firms**



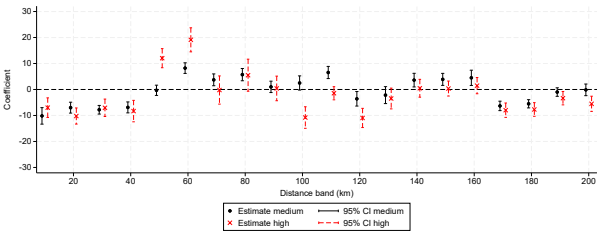
(c) Nearby manufacturing employment **outside zones** and **number of foreign firms**



(d) Nearby manufacturing employment **inside zones** and **number of foreign firms**



(e) Nearby manufacturing employment **outside zones** and **number of private domestic firms**



(f) Nearby manufacturing employment **in zones** and number of **private domestic firms**

Figure A15: Spillovers on **number of firms** from nearby manufacturing employment inside zones and outside of zones. **Regressions account for spillovers from nearby zones and nearby non-zone areas**

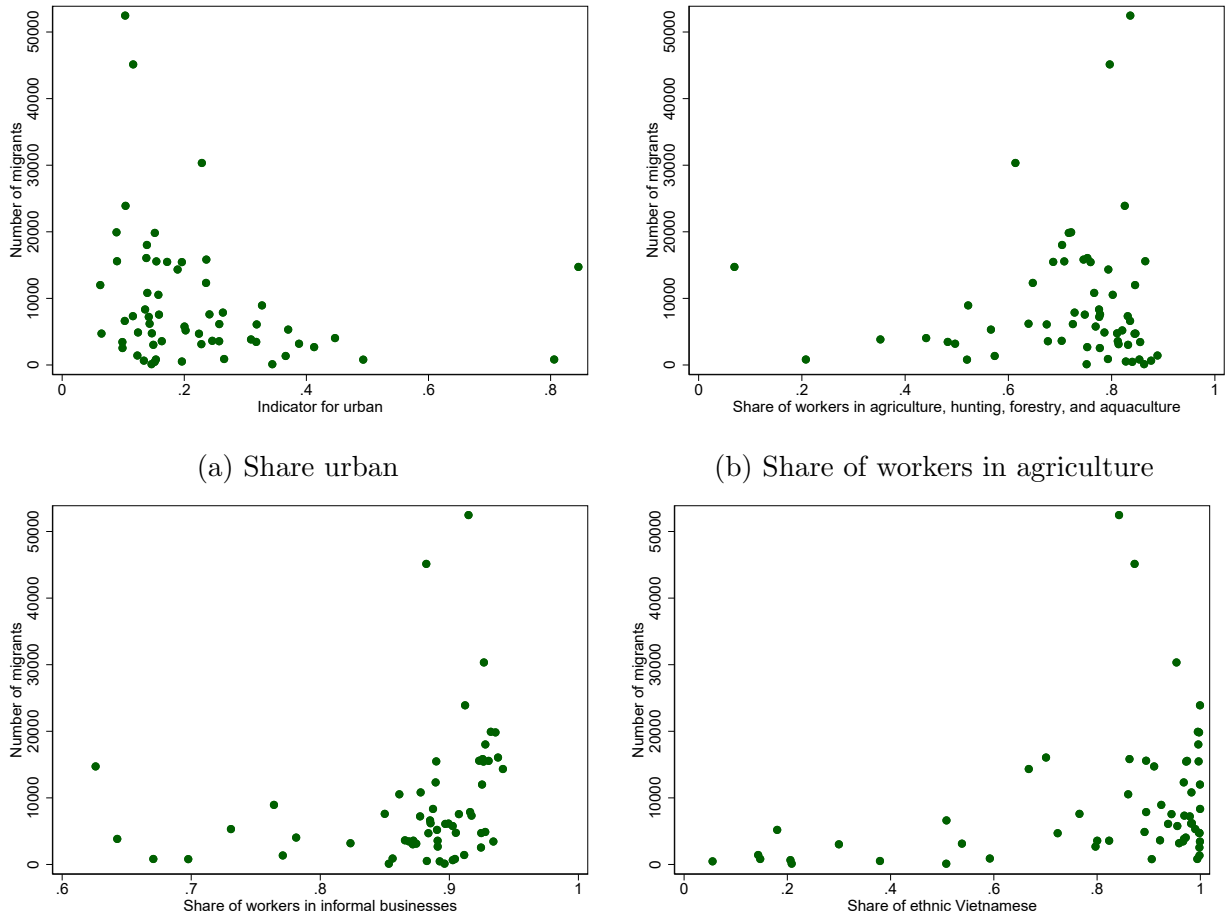
Note: Each point and 95% confidence interval are estimated following equation (1). The estimates reported in panels (a) and (b) are from one regression using the number of firms as the outcome, the estimates reported in panels (c) and (d) are from one regression using the number of foreign firms as the outcome, and the estimates reported in panels (e) and (f) are from one regression using the number of domestic firms as the outcome. In panels (a), (c), and (e), we report the estimates of coefficients on indicators for medium- and high-density manufacturing employment outside industrial zones in the respective distance ring. In panels (b), (d), and (f), we report estimates of coefficients on indicators for medium- and high-density manufacturing employment in industrial zones in the respective distance ring. Controls include ward fixed effects, year fixed effects, treatment cohort linear time trends, and ward initial conditions interacted with year fixed effects. Standard errors clustered by ward. Source: Authors' calculations using the 2000 through 2019 Vietnam Enterprise Surveys and self-collected data on the location and timing of industrial zones.

A.12 Background on migrant workers in foreign manufacturing

We focus on workers in the 2009 population census who report working in a foreign manufacturing firm and having migrated across provinces within the 5 years prior to the census. We create a count of such individuals by origin province and compare the characteristics of origin provinces that send a lot of migrants to origin provinces that send comparatively fewer migrants.

The origin provinces with the highest number of migrant workers in foreign manufacturing are Thanh Hoa (52,469), Nghe An (45,135), An Giang (30,335), and Ha Tinh (23,899), all of which are relatively rural provinces. The provinces with the fewest migrant workers in foreign manufacturing are Kon Tum (113), Lai Chau (124), Cao Bang (469), and Lao Cai (525), all of which have a low share of ethnic Vietnamese.

In Figure A16 we provide scatterplots of the number of migrants from each origin province working in foreign manufacturing in 2009 versus provincial characteristics in 1999. In general, the number of migrants leaving their origin province is higher in less urbanized, more agricultural, more informal, and more ethnic Vietnamese origin provinces. This suggests that as these workers are leaving their origin provinces for jobs in foreign manufacturing firms in other provinces, these workers are most likely leaving agriculture, the informal sector, or transitioning directly from school (McCaig *et al.*, 2022). It is probably less common that they are leaving an existing job in the formal sector as these jobs are relatively rare in the origin provinces that are sending the most migrants.



(a) Share urban

(b) Share of workers in agriculture

(c) Share of workers in informal firms

(d) Share of ethnic Vietnamese

Figure A16: Number of migrants working in foreign manufacturing by source province characteristics

Note: The estimate of the number of migrants working in foreign manufacturing by origin province are based on authors' calculations using the 2009 population census. The provincial characteristics are based on authors' calculations using individuals ages 15 to 64 in the 1999 population census. An informal firm is defined as a firm that is not registered as an enterprise under Vietnam's Enterprise Law and includes household farms.