Place-based Industrial Policies and Intergenerational Educational Inequality: Evidence from Vietnam

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Abstract

Intergenerational educational inequality remains substantial in many countries. This paper studies whether place-based industrialization can reduce the intergenerational transmission of educational disadvantage. Using Vietnam's expansion of industrial zones and household survey data, I implement a staggered difference-in-differences design comparing individuals differentially exposed to zone openings. Industrial zones increase school enrollment among 15–18-year-olds, with effects concentrated among children whose parents did not complete upper-secondary school, narrowing enrollment gaps by parental education. Mechanism evidence points to household income as the primary channel: less-educated households gain income from informal non-agricultural activities—local demand spillovers rather than direct zone employment.

Keywords: place-based policy, education inequality, Vietnam

JEL Codes: I25, O14, J62

1 INTRODUCTION

Human capital accumulation is central to economic development, and its intergenerational transmission shapes inequality across generations. Children of more-educated parents tend to achieve higher educational attainment themselves, perpetuating disparities in earnings and social mobility (Becker, 1994; Black & Devereux, 2011). Understanding what factors can disrupt this cycle, enabling children from disadvantaged backgrounds to close the gap with their more advantaged peers, remains an important question for both researchers and policymakers.

In this paper, I examine whether a prominent form of place-based economic development policy—industrial zones—reduces educational inequality across generations. Vietnam presents a useful setting to study this question. Despite relatively low income levels, the country has achieved notable educational outcomes, with enrollment rates and test scores exceeding those of countries at similar development levels (Dang & Glewwe, 2018; Dang et al., 2023). Over the past two decades, the gap in school enrollment between children of less-educated and more-educated parents has narrowed: among 15–18 year-olds, this gap falls from 36 percentage points in 2002 to 25 percentage points by 2020. During this same period, Vietnam pursues export-oriented industrialization, with the number of industrial zones expanding from fewer than 20 in the mid-1990s to over 300 by 2020. Whether these two trends are connected—specifically, whether the expansion of industrial zones causally contributes to reducing intergenerational educational inequality—is the question this paper addresses.

The relationship between industrial zones, child human capital, and intergenerational inequality is theoretically ambiguous. On the demand side, zones may increase school enrollment through several channels. Rising household income could relax budget constraints, enabling families to cover both the direct costs of schooling (tuition, supplies) and the indirect costs (forgone child labor) (Basu & Van, 1998; Edmonds, 2005). Zones may also raise the perceived returns to education if they create skilled jobs that reward schooling (Jensen, 2010; F. Lu et al., 2023). On the supply side, zone development is often linked to social infrastructure investment, including schools, potentially improving access to education. However, countervailing forces may operate. If zones primarily expand low-skill employment opportunities accessible to teenagers, the opportunity cost of schooling rises, potentially pulling children out of school and into work (Atkin, 2016). Environmental disamenities associated with industrial activity could also harm child health and learning (Currie et al., 2009).

These mechanisms may operate differently by parental education, with direct implications for intergenerational inequality. If formal zone employment drives income gains, children of

more-educated parents may benefit more, as these households are better positioned to access skilled jobs within zones, potentially widening the educational gap. Alternatively, if less-educated households also experience income gains and face binding budget constraints, their children may show larger enrollment responses, thereby narrowing the gap. Whether industrial zones affect school enrollment, and whether they reduce or exacerbate intergenerational educational inequality, are ultimately empirical questions.

To investigate, I combine data from the Vietnam Household Living Standards Survey (VHLSS) spanning 2002–2020 with administrative records on industrial zone establishment from the Ministry of Planning and Investment. The VHLSS provides detailed information on school enrollment, child labor, household income by source, and education expenditure, along with parental education and household demographics. I observe the location and establishment year of over 400 industrial zones, allowing me to construct district-level treatment indicators based on proximity to zones.

One empirical challenge is that zone establishment is not random. Districts receiving zones earlier differ systematically from those receiving zones later: they have lower ethnic minority shares, higher shares of children with educated parents, higher baseline enrollment, and lower rates of child labor. Simple comparisons between treated and untreated, or between early-treated and late-treated districts would therefore conflate zone effects with pre-existing characteristics. Thus, I implement the heterogeneity-robust estimator of de Chaisemartin and d'Haultfoeuille (2024), which addresses biases in conventional two-way fixed effects estimators when treatment effects vary across cohorts and over time. Because the data are repeated cross-sections rather than a panel of individuals, identification comes from comparing the evolution of enrollment across successive cohorts of 15–18 year-olds in districts exposed to zones against those in districts not yet exposed or never exposed. The identifying assumption is that, absent zone establishment, enrollment trends would have evolved similarly across these districts, which is supported by pre-treatment trends showing no systematic differences prior to zone arrival. To determine the appropriate spatial definition of treatment, I estimate effects across distance bins from zone boundaries for each treatment cohort. Consistent with previous literature on place-based policies (e.g., Abagna et al., 2025; Gallé et al., 2024), effects attenuate with distance from zone centers. This evidence motivates the 15 kilometer threshold used in the main analysis—a conservative choice that likely attenuates estimates toward zero.

The analysis yields two main findings. First, industrial zones increase school enrollment among children aged 15–18 by approximately 6 percentage points, with parallel declines in child labor. Second, effects are concentrated among children whose parents did not complete upper-secondary school: for this group, enrollment increases by 7.3 percentage points rela-

tive to a baseline of 53%. Effects for children of more-educated parents are not statistically distinguishable from zero. A similar pattern emerges for upper-secondary completion among 19–22 year-olds, suggesting that enrollment gains translate into educational attainment. Together, these findings indicate that industrial zones contribute to narrowing intergenerational educational inequality. By period 6 after zone establishment, the differential effect represents approximately 30% of the baseline enrollment gap of 36 percentage points between children of more-educated and less-educated parents.

The mechanism evidence points to household income as an important channel. Less-educated households experience income gains concentrated in informal non-agricultural activities, suggesting they benefit from spillovers to the local economy rather than direct employment in zones. Part of this additional income is allocated toward children's education. Child labor declines alongside enrollment gains, indicating that income effects dominate any opportunity cost effects from expanded employment opportunities. Supply-side improvements also contribute: distance to schools decreases following zone establishment.

This paper contributes to research on intergenerational educational inequality by linking place-based industrial policy to gaps in human capital investment across family backgrounds. A large literature documents the persistence of educational advantage across generations (Black & Devereux, 2011), and prior work identifies factors that can weaken this transmission, including for example, school construction (Akresh et al., 2023), cash transfers (Barham et al., 2024; Parker & Vogl, 2023; Schultz, 2004), and early childhood skill investments (Heckman et al., 2010). Separately, a growing body of evidence examines how place-based policies affect firm outcomes, local labor markets and, more recently, human capital (Abagna et al., 2025; Busso et al., 2013; Gallé et al., 2024; F. Lu et al., 2023; Y. Lu et al., 2019; Tafese et al., 2025; Wang, 2013). This literature has focused on average effects or heterogeneity by zone type, but not on whether industrial zones affect children differentially based on parental education. I provide evidence that they do: enrollment and upper-secondary school completion gains are concentrated among children of less-educated parents, implying that industrial zones can narrow intergenerational educational inequality rather than reinforcing existing advantages.

The findings also speak to debates over the distributional consequences of export-oriented industrialization. A central concern is that gains accrue disproportionately to workers able to access formal employment, potentially bypassing less-educated households (Goldberg & Pavcnik, 2007). Consistent with evidence that such households benefit from local spillovers rather than direct zone employment (Pham, 2026), I find their income gains are concentrated in informal activities. That these contemporaneous gains translate into higher enrollment and lower child labor—rather than children being pulled into work—suggests income effects

3

dominate opportunity cost effects (Atkin, 2016; Basu & Van, 1998), extending the welfare analysis of industrial zones to include human capital accumulation among disadvantaged families.

The remainder of the paper proceeds as follows. Section 2 describes the background on industrial zones and education in Vietnam. Section 3 outlines potential mechanisms through which zones may affect human capital outcomes and intergenerational inequality. Section 4 describes the data and construction of key variables. Section 5 presents the empirical strategy. Section 6 reports the main results and robustness checks. Section 7 examines mechanisms, and Section 8 concludes.

2 INSTITUTIONAL BACKGROUND AND EDUCATIONAL TRENDS
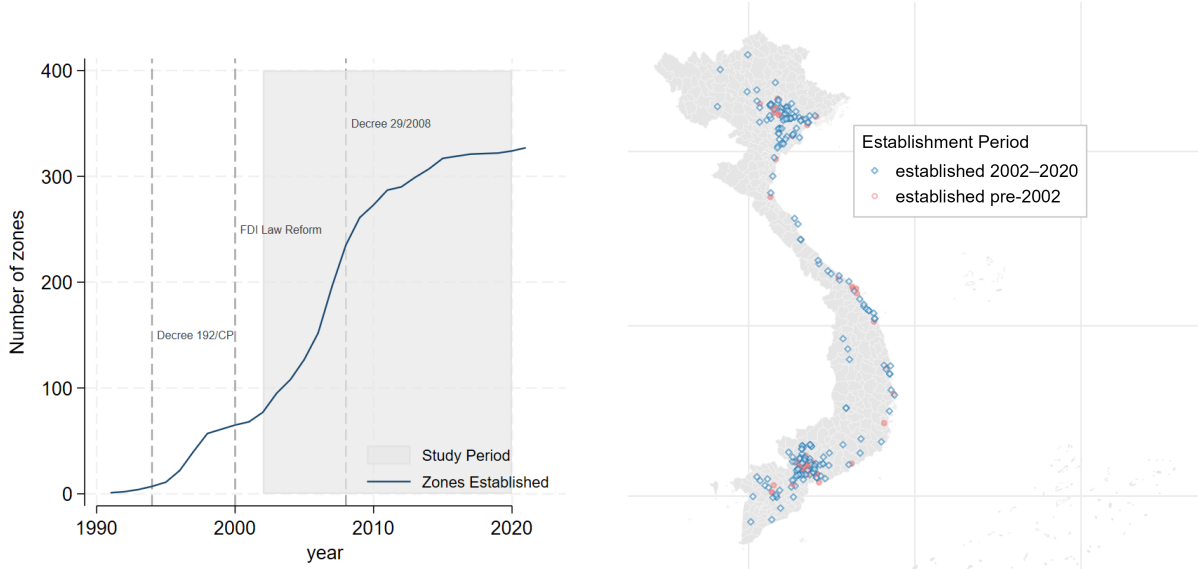
2.1 Industrial Zones in Vietnam

In Vietnam, industrial zones are designated areas intended to concentrate industrial production and manufacturing. Their core objectives are to attract foreign direct investment, promote export-oriented growth, and create employment. Zones are equipped with dedicated infrastructure and offer investment incentives to both domestic and international firms.

The left panel of Figure 1 shows the cumulative number of zones over time. Fewer than 20 zones existed nationwide by the mid-1990s. Growth accelerated in the early 2000s following reforms to the Foreign Direct Investment Law, with the total rising from around 70 to over 200 within a few years. By 2020, over 400 zones had been planned, of which approximately 300 were operational. The right panel illustrates the spatial expansion of zones into more disadvantaged regions over the study period 2002–2020.

The country's legal framework has increasingly linked industrial zone development to surrounding social infrastructure over the study period. Early regulations such as Decree 36/CP (1997) stipulate that zone proposals address infrastructure both inside and outside zone boundaries, including worker housing, schools, and medical facilities, though these provisions function primarily as planning considerations rather than binding mandates. Decree 29/2008/ND-CP more explicitly assigns provincial authorities responsibility for organizing the construction of social infrastructure outside zone boundaries, specifying roads, utilities, job-training establishments, worker housing, medical facilities, schools, and other public works to meet the needs of zone development (Article 35.9). This regulatory environment suggests that zone establishment may affect local educational access through more schools constructed.

Zones generate employment across a range of manufacturing activities. Data from the

Figure 1: Temporal and Spatial Evolution of Industrial Zones in Vietnam

Notes: The left panel shows the cumulative number of industrial zones in Vietnam by year. The right panel shows the spatial distribution of industrial zones over time. Early zones (red circles) concentrated around major economic centers, including Hanoi, Ho Chi Minh City, Can Tho, and Da Nang. Zones established since 2004 (blue diamonds) are more geographically dispersed, extending into the northwest, central coast, Central Highlands, and Mekong River Delta, reflecting a policy shift toward balanced regional development. Source: Zone data are from the Ministry of Planning and Investment.

2016 Vietnam Enterprise Survey suggests that labor-intensive industries, including garments, footwear, and food processing, account for the largest share of zone employment (roughly 45%) and rely heavily on production workers performing assembly and processing tasks. Higher-skill segments such as electronics, machinery, and transport equipment manufacturing make up a smaller but growing share of zone employment (Table 1). Compared to firms outside industrial zones, zone firms are significantly more concentrated in these manufacturing sectors.

Educational requirements vary across occupations in the labor market. A survey of detailed skills conducted by the World Bank finds that approximately one-third of manufacturing occupation categories require less than secondary education—positions involving simple machine operation (such as sewing machine operators), elementary assembly tasks, or freight handling (Granata et al., 2023). However, 43% require at least upper secondary education but not a university degree. These are more technical positions, including technicians, assemblers, and machine operators, where employers value quality control skills and trainability. The remaining positions—supervisory, professional, and engineering roles—require post-secondary credentials. This skill gradient means that upper secondary completion opens access to a substantially broader range of zone jobs, while those without an upper-secondary

Table 1: Employment Characteristics, Outside and Inside Zones

	Outside Zones	Inside Zones	Difference (1) − (2)	
	Mean (1)	Mean (2)	Coef. (3)	p-value (4)
Employment Distribution by Industry				
Agriculture	0.029	0.006	-0.023	0.000
Mining and quarrying	0.011	0.004	-0.007	0.000
Manufacturing				
Textiles, footware, wood and furniture	0.201	0.357	0.156	0.000
Food and beverage processing	0.033	0.082	0.049	0.000
Chemicals, rubber and plastics	0.024	0.071	0.047	0.000
Electronics and electrical equipment	0.026	0.166	0.140	0.000
Metals and fabricated metal products	0.025	0.044	0.019	0.001
Transport equipment	0.010	0.042	0.032	0.002
Others	0.048	0.059	0.011	0.173
Public utilities	0.022	0.007	-0.015	0.000
Construction	0.172	0.027	-0.145	0.000
Sales, trade, hotels, restaurants	0.192	0.052	-0.140	0.000
Transports, storage, communication	0.076	0.032	-0.045	0.000
Finance, insurance, professional, business services	0.072	0.044	-0.028	0.000
Community, social, government services	0.060	0.008	-0.052	0.000

Notes: Columns (1) and (2) report mean values of the variables listed in the left-hand column for firms located outside and inside zones, respectively. Column (3) reports the coefficient on the inside-zone indicator from regressions of each left-hand-side variable on an inside-zone dummy, controlling for district fixed effects; standard errors are clustered at the district level. Column (4) reports the corresponding p-value for the null hypothesis that the coefficient in column (3) equals zero. Source: Calculations using the Vietnam Enterprise Survey 2016.

school diploma are largely confined to entry-level positions with lower wages and less job security.

2.2 Education System and Education Trends

Vietnam's education system comprises five years of primary school (grades 1–5), four years of lower secondary (grades 6–9), and three years of upper secondary (grades 10–12). Compulsory education covers grades 1–9, typically completed by age 15 (World Bank, 2018). While primary and lower secondary enrollment expanded rapidly following government universalization efforts, upper secondary enrollment has lagged, particularly among less-educated

households. This makes the transition at age 15 a critical margin: it marks the end of compulsory schooling, the point at which school-work tradeoffs intensify, and the stage at which household resource constraints bind most tightly.

Figure 2 illustrates these patterns. The left panel shows that enrollment is near-universal for children aged 6–10 (approximately 97% throughout the period) and high for those aged 11–14 (rising from 90% to 96%). In contrast, enrollment among 15–18 year-olds starts substantially lower (58% in 2002) and, despite rising to 78% by 2020, remains well below younger age groups. The middle panel reveals that this lower enrollment rate is driven almost entirely by children of less-educated parents—those who did not complete upper-secondary schooling. In 2002, enrollment among 15–18 year-olds with less-educated parents was at just 51%, compared to 87% for those with more-educated parents, a gap of 36 percentage points. By 2020, this gap had narrowed to 25 percentage points (70% versus 95%), representing an 11-percentage-point reduction. The right panel shows a similar pattern for educational attainment: upper secondary completion among 19–22 year-olds with less-educated parents rises from 24% in 2002 to 56% by 2020, narrowing the gap with children of more-educated parents from 54 to 37 percentage points.

Figure 2: Education Trends in Vietnam

Notes: This figure presents three panels. The left panel shows school enrollment rates by age group. The middle panel displays enrollment rates for children aged 15–18 by parental education. The right panel shows upper secondary completion rates among individuals aged 19–22 by parental education. Source: VHLSS 2002–2020.

These patterns motivate two related questions. First, did industrial zone expansion contribute to the observed gains in school enrollment? Second, did zones disproportionately benefit children from less-educated households, thereby narrowing intergenerational educational inequality? The next section outlines potential mechanisms through which industrial zones may affect human capital accumulation and how such effects might differ by parental education.
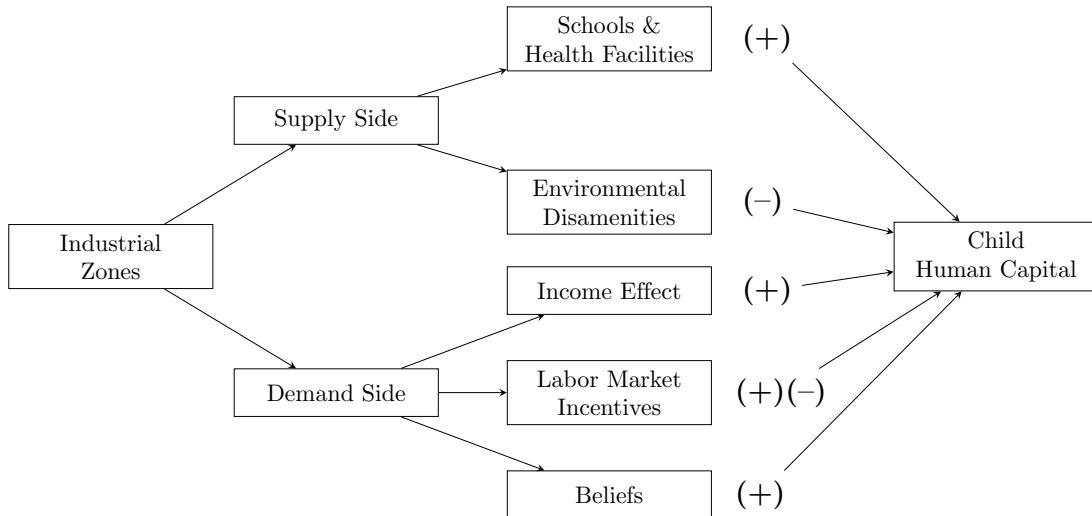
3   CONCEPTUAL FRAMEWORK

Industrial zones may affect education through both supply-side and demand-side channels, as summarized in Figure 3. In what follows, I discuss the expected effects and their heterogeneity by parental education, drawing on previous literature.

3.1   Supply-Side Channels

Local service provision and infrastructure.   Industrial zones can improve access to education and healthcare by catalyzing local infrastructure and service provision. In many settings, zones are explicitly designed to concentrate public services and infrastructure (e.g., transport links, utilities, permitting offices) to reduce firms' costs, and these investments can also increase access to schools and health facilities for nearby households (United Nations Conference on Trade and Development, 2019). Yet, improvements are not automatic: many zones rely heavily on fiscal incentives, so increases in local tax revenue may be muted in the short run, and the extent of public investment may depend on zone governance and financing arrangements. To the extent that zones expand the supply or quality of local education and health infrastructure, these changes are expected to benefit children broadly, regardless of parental background.

Figure 3: Mechanisms Linking Industrial Zones to Child Human Capital

Environmental disamenities.   At the same time, zones may generate negative externalities that directly reduce human capital formation. Industrial activity can increase air and water pollution, which can worsen child health and reduce learning by increasing school absences and impairing cognitive performance (Currie et al., 2009; Greenstone & Hanna, 2014).

These health-related channels work in the opposite direction of infrastructure improvements and can result in ambiguous impacts even if schooling and health access expands.

3.2   Demand-Side Channels

On the demand side, household decisions about children's time allocation and investments respond to changes in income and in the labor market incentives. These forces can generate opposing predictions.

Income effects.   If zones increase adult earnings, rising household income can relax budget constraints and enable greater investment in children's education. Basu and Van (1998) formalize this intuition with the "luxury axiom": households send children to work only when income from non-child sources is sufficiently low, implying that child labor falls and schooling rises as families move away from subsistence. Consistent with this mechanism, Edmonds (2005) shows in Vietnam that improvements in household expenditure can account for most of the decline in child labor among households that moved out of poverty during the 1990s. More broadly, Edmonds and Pavcnik (2005) emphasize that poverty is a fundamental driver of child labor worldwide. Under this mechanism, industrial zones could increase schooling and reduce children's work as household income rises.

Labor-market incentives: wage premiums versus job opportunities.   Zones may also affect human capital investment by changing the perceived and actual returns to education relative to the value of working immediately. Two opposing channels are plausible, depending on the skill composition of labor demand.

First, if zones generate sizable wage premiums for educated workers and expand skilled job opportunities, households may increase schooling in anticipation of higher returns. F. Lu et al. (2023) provides evidence consistent with this channel in China: technology-oriented zones that demand skilled labor increase upper-secondary school enrollment, and the mechanism is linked to wage premiums and education-intensive job opportunities.

Second, if zones primarily expand less-skilled employment opportunities, including jobs that are accessible to teenagers or marginal students, the opportunity cost of schooling rises, potentially increasing dropout and youth employment. Atkin (2016) documents this mechanism in Mexico, where local expansions in export manufacturing increased school dropout with economically meaningful magnitudes, and attributes the pattern to less-skilled jobs raising the value of leaving school at the margin. In the Chinese context, F. Lu et al. (2023) similarly finds that export-led zones discourage enrollment, consistent with job opportunities for less-educated workers increasing the relative attractiveness of work over continued schooling. Under this mechanism, zones may decrease schooling and increase

youth work, especially for children near the dropout margin.

Beliefs about returns to schooling. Even holding measured returns fixed, zones may affect schooling by shifting beliefs about the payoff to education. Information frictions can lead households and students to underestimate returns and providing credible information can increase perceived returns and schooling investments. For example, Jensen (2010) shows that students in the Dominican Republic substantially underestimate returns to secondary schooling and update beliefs when provided with correct information. Complementary evidence highlights that parents' subjective expectations also matter for education decisions: Nguyen (2008) elicits parents' perceived returns in Madagascar and studies how information and role models shift beliefs and schooling behavior, while Attanasio and Kaufmann (2014) uses data with both youths' and mothers' expectations in Mexico to link subjective returns to schooling choices. Industrial zones may therefore operate as a local "information shock" by making formal-sector wage differentials visible and salient, especially where households have limited prior exposure to such jobs.

Taken together, the net demand-side effect of zones on education is ambiguous. It depends on the magnitude of income gains, whether zones increase skill premiums or mainly expand low-skill job opportunities, and how strongly households update beliefs about the returns to schooling.

3.3 Heterogeneity by Parental Education

Each of the mechanisms discussed above implies different patterns of heterogeneity by parental education.

Income channel. If income effects dominate, children from less-educated households may benefit more. These households tend to face tighter budget constraints and may be closer to the subsistence margin emphasized by Basu and Van (1998). As a result, the marginal impact of additional income on schooling and child labor can be larger among low-education households, consistent with the idea that poverty reductions drive declines in child labor (Edmonds, 2005; Edmonds & Pavcnik, 2005).

Wage-premium and belief channels. If zones increase wage premiums for educated workers (and/or make such premiums more salient), two countervailing forces suggest ambiguous heterogeneity. On one hand, less-educated households may update beliefs more because they have less prior exposure to formal-sector returns. Given that information frictions are common, providing information or salient role models can change perceived returns and schooling investments (Jensen, 2010; Nguyen, 2008). On the other hand, more-educated households may be better positioned to act on new opportunities due to greater resources,

10

knowledge of the schooling system, and networks that facilitate accessing higher-quality schools or skilled jobs. As a result, their children may respond more on the investment margin (Attanasio & Kaufmann, 2014). Thus, even if learning about returns is stronger for low-education households, realized schooling responses could be larger for high-education households if constraints bind.

Job-opportunity (opportunity cost) channel. If zones primarily expand low-skill job opportunities, children from less-educated households may be more vulnerable to dropout or reduced schooling intensity because they are more likely to be at the margin between school and work and more sensitive to short-run earnings opportunities (Atkin, 2016; Edmonds & Pavcnik, 2005). In this case, zones could widen intergenerational gaps in educational attainment.

Environmental disamenities. Finally, if industrial activity worsens local environmental quality, the human capital costs may disproportionately affect disadvantaged households if they have fewer avoidance options or are more likely to live near industrial sites. Pollution has been linked to higher absences and lower test performance, providing a pathway through which zones could harm learning even absent changes in schooling access (Currie et al., 2009; Ebenstein et al., 2016).

Given these competing forces, whether industrial zones affect schooling outcomes, and whether they narrow or widen educational gaps across generations is ultimately an empirical question. The next sections describe the dataset and empirical strategy used to estimate the impact of industrial zones, as well as potential threats to identification.

4   DATA AND CONSTRUCTION OF KEY VARIABLES

4.1   Data Sources

I combine two primary data sources: a comprehensive database of industrial zones and nationally representative household surveys.

Industrial zones. The Ministry of Planning and Investment maintains a database of all industrial zones in Vietnam, recording each zone's name, administrative location (ward, district, province), establishment date, operational status as of June 2023, and performance indicators including the number of domestic and foreign projects. I manually geo-reference each zone using Google Maps, as the original database lacks geographic coordinates.

The empirical strategy exploits variation at the district level, which is Vietnam's second-level administrative division below provinces. I obtain district boundary shapefiles from

the Humanitarian Data Exchange, reflecting 2019–2020 administrative divisions.[1] Because districts were split and merged over the study period, I harmonize boundaries by aggregating newly created subdivisions to their original parent districts, yielding a consistent panel of approximately 650 districts compared to over 700 in the unadjusted boundaries. I then spatially join georeferenced zones to these harmonized districts.

Household surveys. Individual- and household-level data come from the repeated cross-sectional Vietnam Household Living Standards Survey (VHLSS), conducted biennially by the General Statistics Office from 2002 to 2020. The 2002 wave covers approximately 30,000 households; subsequent waves cover over 45,000 households across more than 3,000 communes. The survey uses a stratified multi-stage cluster design, with communes sampled based on urban/rural status and geographic region, followed by random selection of households within communes. The survey covers all de jure (usual resident) household members. Importantly, individuals are counted as household members even if they have lived in the household for less than six months, and students living away from home during the school year are included as members of their origin household, provided it remains their usual residence. This definition ensures that young adults attending school elsewhere are captured in the data.[2]

The sample is representative at national, regional, urban/rural, and provincial levels, but not at the district level. The main analysis uses district-level variation to capture the localized effects of zone expansion. I also conduct robustness checks using province-level estimation where representativeness is assured. All analyses apply sampling weights.

I merge household survey data with the industrial zone database using district identifiers. Although the VHLSS does not report exact household locations, district codes allow me to determine exposure to industrial zones based on the harmonized boundaries described above.

4.2 Construction of Variables.

School enrollment. The VHLSS Education Module records whether individuals are currently enrolled, on summer break, or attended school during the past 12 months. I code enrollment as one if any condition is met, and zero otherwise.

Labor participation. For individuals aged 10 and above, the Employment Module records participation in income-generating activities over the past 12 months, including household farm work, non-farm business, and wage employment. I code labor participation as one

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[1]https://data.humdata.org/dataset/cod-ab-vnm

[2]Tabulations of the data indicate that approximately 3% of all individuals, and 7% of those aged 10–22, were temporarily absent but remained registered household members (residing outside for more than six months in the past 12 months).

if the individual engaged in any activity, and zero otherwise. Because this information is unavailable for younger children, the main analysis focuses on those aged 10–18.

Parental education. A central goal of this paper is to examine whether industrial zones reduce the educational gap across generations. I construct a measure of parental education for each child using the household roster. For children of the household head (92.4% of observations), I assign parental education as the highest level attained by the head or spouse. For grandchildren of the head (7.6% of observations), I assign parental education as the maximum education level among all household members classified as children of the head, which captures the most-educated adult in the caregiving generation. Within this group, 28% reside in households with only one adult child present, allowing for clean identification of biological parents' education. The remaining grandchildren live in multi-generational households with multiple adult children, where linking to specific biological parents is not possible. Overall, approximately 95% of observations have clearly identified parental education.

Mechanisms. To explore potential mechanisms, I draw on additional survey modules.
Household income and expenditure. I construct household labor and business income by aggregating earnings across members. For wage workers, the survey reports compensation directly, including base wages, bonuses, and allowances. For the self-employed, I compute profits from the Business Module as revenues minus costs. In addition, a stratified sub-sample of approximately 9,000 households per wave reports detailed expenditure data, including education expenditure (tuition, textbooks, supplies, tutoring) and health expenditure (inpatient and outpatient). All monetary values are deflated to 2010 Vietnamese Dong using the national CPI.
Access to schools. The VHLSS Commune Module, covering approximately 2,200 communes, reports the distance to the nearest primary, lower secondary, and upper secondary school attended by children in the commune. I use these measures to examine whether industrial zone establishment is associated with improved school access.

4.3 Summary Statistics

Table 2 presents characteristics of children aged 10–18 by zone establishment timing, using data from the 2002 household survey. Columns (1)–(3) compare never-treated districts with those receiving zones during the study period. Never-treated districts have higher ethnic minority shares (36% versus 14%), lower shares of children with educated parents (14% versus 17%), lower baseline enrollment (74% versus 76%), and higher labor participation (38% versus 27%).

Columns (4)–(8) compare districts by treatment timing within the study period. Districts

receiving zones later differ from those receiving zones earlier along similar dimensions. Ethnic minority shares are 7%, 15%, and 24% across the three cohorts respectively. The share of children with educated parents is 8 percentage points lower in the second cohort than in the first. School enrollment is 5–7 percentage points lower in later cohorts, and labor participation is 4 percentage points higher.

Table 2: Characteristics of Sample Children in 2002

	Never-treated Districts	Districts with Zones since 2002	Difference (2) − (1)	Districts with Zones 2002–2004	Districts with Zones 2005–2008	Districts with Zones 2009–2020	Difference (5) − (4)	Difference (6) − (4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Demographics								
Age	13.936	13.970	-0.034	13.989	13.937	14.027	-0.052	0.038
	[0.527]	[0.426]	(0.050)	[0.420]	[0.425]	[0.440]	(0.070)	(0.086)
Male	0.517	0.520	-0.003	0.513	0.529	0.511	0.016	-0.002
	[0.103]	[0.086]	(0.010)	[0.095]	[0.084]	[0.074]	(0.015)	(0.016)
Ethnic minority	0.363	0.139	0.224	0.069	0.146	0.243	0.077	0.174
	[0.407]	[0.232]	(0.037)	[0.175]	[0.232]	[0.280]	(0.032)	(0.045)
Educated parents	0.138	0.174	-0.036	0.216	0.140	0.192	-0.077	-0.024
	[0.147]	[0.149]	(0.016)	[0.135]	[0.138]	[0.181]	(0.021)	(0.033)
Urban	0.126	0.137	-0.011	0.151	0.122	0.153	-0.030	0.002
	[0.182]	[0.207]	(0.020)	[0.248]	[0.174]	[0.216]	(0.034)	(0.044)
Long-term registration	0.994	0.994	0.001	0.993	0.995	0.992	0.002	-0.001
	[0.014]	[0.014]	(0.001)	[0.013]	[0.014]	[0.016]	(0.002)	(0.003)
Panel B: School and Labor								
School enrollment	0.736	0.756	-0.020	0.802	0.730	0.748	-0.071	-0.053
	[0.168]	[0.121]	(0.017)	[0.090]	[0.125]	[0.138]	(0.018)	(0.023)
Labor participation	0.376	0.271	0.105	0.249	0.292	0.253	0.043	0.004
	[0.223]	[0.152]	(0.021)	[0.154]	[0.149]	[0.150]	(0.024)	(0.027)

Notes: This table summarizes 2002 characteristics of children aged 10–18 across district types. Columns (1)–(3) compare never-treated versus ever-treated districts: Column (1) includes districts that have not received nor in proximity to any zone during the study period; Column (2) includes those within 15 km of a zone established in 2002 or later; Column (3) reports mean differences. Columns (4)–(7) compare treatment timing within the study period: Column (4) includes districts within 15 km of a zone established 2002–2004; Column (5) includes those within 15 km of a zone established 2005–2008; Column (6) includes those within 15 km of a zone established 2009–2020. Columns (7)–(8) report mean differences. Standard deviations in brackets; robust standard errors in parentheses. Sampling weights applied throughout. Data on long-term registration is from VHLSS 2004. Source: Data from VHLSS 2002–2004.

Despite these differences, districts in the study sample share a common feature: substantial rates of child labor. Across all treatment cohorts, at least one in four children aged 10–18 were engaged in economic activities at baseline, and enrollment rates remained below 80%.

14

5 EMPIRICAL STRATEGY

Given the systematic differences documented in Table 2—both between treated and never-treated districts, and across treatment cohorts—simple comparisons would conflate zone effects with pre-existing characteristics. I address this challenge by leveraging the staggered introduction of zones across districts in a difference-in-differences design.

Treatment definition. For spatial variation, I classify a district as treated if any portion of its boundary lies within 15 kilometers of an industrial zone's centroid. This threshold is motivated by two considerations. First, existing evidence suggests that economic spillovers from place-based policies concentrate within 10–15 km (e.g., Abagna et al., 2025; Gallé et al., 2024). Second, my analysis of treatment effects across distance bins confirms this pattern: effects on school enrollment are statistically significant within 10 km but attenuate sharply beyond this range (Appendix Figure A2). To the extent that effects concentrate within 10 km while the treatment definition includes districts up to the boundary, the estimates represent a conservative estimation of the effects.

For temporal variation, treatment timing is defined by zone establishment. This happens when non-infrastructure projects receive official approval, which often initiates concrete development activity and triggers anticipatory responses from investors, workers, and local governments. Since administrative delays between approval and operation are common, the establishment date better captures when local economic dynamics begin responding. The sample excludes zones that failed to become operational by the study period's end.[3]

Appendix Table A1 documents zone establishment patterns. Activity was concentrated early: 12.9% during 2002–2008, but only 3.2% and 1.4% during 2009–2012 and 2014–2020, respectively. Importantly, approximately 35% of districts never received a zone or were in proximity to any zones throughout the study period, providing a substantial comparison group.

Event-study specification. Based on the treatment definition, I estimate an event-study specification as follows:

$$y_{idt} = \gamma_d + \gamma_t + \sum_k \beta_k \cdot \mathbb{I}(t = \text{Establishment}_d + k) + \varepsilon_{idt} \tag{1}$$

where y_{idt} is the outcome of individual i in district d at time t. District fixed effects γ_d absorb district-specific time-invariant characteristics, while year fixed effects γ_t control for nationwide year-specific shocks. The coefficients β_k capture the dynamic treatment effects

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[3]The zone database indicates only one termination as of June 2023, due to infrastructure delays.

15

k periods relative to zone establishment. The omitted period is $k = 0$, normalizing effects relative to the year of treatment. Standard errors are clustered at the district level to account for spatial and temporal correlation.

Identifying assumptions. Causal interpretation of β_k requires two key assumptions. First, the parallel assumption holds that absent zone establishment, school enrollment of children would have evolved similarly on average across treated and never-treated districts. Second, the no-anticipation assumption requires that future treatment timing does not affect current outcomes in untreated periods.

Although the parallel trends cannot be directly verified, the event-study specification allows me to partially test its plausibility by examining trend differences before zone arrival. In particular, joint insignificance of the pre-treatment coefficients ($\beta_k, k < 0$) would suggest no systematic pre-existing differences in trends, and thus the parallel trends may plausibly continue to hold after the treatment absent of it. This test also rules out anticipatory behavioral changes.

Estimation approach. Recent DiD literature demonstrates that conventional two-way fixed effects estimation can yield biased estimates when treatment effects vary across cohorts or time, since early-treated units may serve as implicit controls and some comparisons receive negative weights (e.g., Borusyak et al., 2024; de Chaisemartin & d'Haultfoeuille, 2020; Goodman-Bacon, 2021). This concern is particularly important here because: (i) zones established in different periods tend to serve distinct economic functions (United Nations Industrial Development Organization, 2019; World Bank Group, 2019), and (ii) early- and late-adopting districts differ significantly in baseline characteristics (Table 2). I therefore implement the heterogeneity-robust estimator of de Chaisemartin and d'Haultfoeuille (2024), which addresses biases arising from heterogeneous treatment effects across cohorts and over time. In the main specification, I restrict comparisons to never-treated units as controls, preventing contamination from already-treated units serving as comparisons. Results are robust to including not-yet-treated units as controls. Placebo estimates for pre-treatment periods assess the parallel trends and no-anticipation assumptions.

Because this estimation approach does not accommodate triple-difference specifications, to examine heterogeneity by parental education, I estimate separate models for children whose parents completed upper-secondary school or higher education versus those who did not. I then use a stratified clustered bootstrap (at the district level, stratified by treatment timing) to test whether the difference in coefficients is statistically significant. This approach preserves the distribution of treatment timing across replications.

6 EMPIRICAL RESULTS AND ROBUSTNESS CHECKS

6.1 The Impacts of Zones on School Enrollment and Degree Attainment

Figure 4 presents the dynamic effects of industrial zone establishment on school enrollment. The left panel shows results for children aged 10–18, while the middle and right panels disaggregate by age group. Across all panels, pre-treatment coefficients are jointly statistically insignificant, supporting the parallel trends and no anticipation assumptions. Following zone establishment, school enrollment increases gradually for the full sample, reaching approximately 4 percentage points by period 4 and remaining stable thereafter. This pattern consistent with a permanent level shift in enrollment rather than a transitory response. The effect, however, is entirely driven by children aged 15–18, for whom enrollment increases by 6.2 percentage points on average in the post-treatment period, representing 10% increase relative to the baseline mean. For younger children (ages 10–14), estimated effects are close to zero with tight confidence intervals.

Figure 4: Industrial Zones and School Enrollment

pre-trend p-val = 0.957, post-ATT = 0.028 (0.008), baseline outcome mean = 0.77

pre-trend p-val = 0.807, post-ATT = -0.006 (0.006), baseline outcome mean = 0.92

pre-trend p-val = 0.987, post-ATT = 0.062 (0.013), baseline outcome mean = 0.59
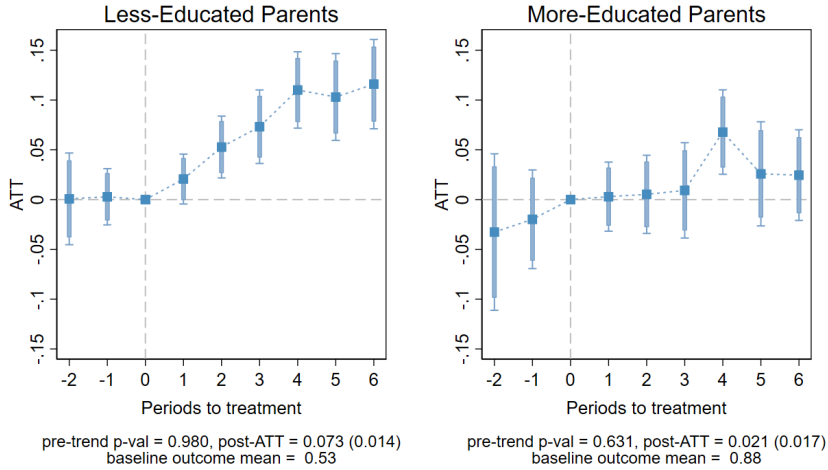
Notes: This figure shows the effects of industrial zone exposure on school enrollment of children across age groups, using data from VHLSS 2002–2020. The outcome is whether a child has attended school during the past 12 months before the interview. Square markers indicate the point estimates of the coefficients. Darker vertical lines with caps show 95% confidence intervals, and lighter bars represent 90% confidence intervals. *pre-trend* p-val is the p-value from the joint test that pre-treatment effects are zero. *post-ATT* represents the average treatment effect on the treated across post-treatment periods, with standard errors clustered at the district level in parentheses. Estimates are derived using the method proposed by de Chaisemartin and d'Haultfoeuille (2024). Sampling weights are applied throughout.

The improvement in school enrollment among 15–18 documented above is concentrated among children whose parents did not complete upper-secondary school. Figure 5 presents results from estimating equation (1) separately by parental education: the left panel for

children without an upper-secondary school-educated parent, and the right panel for children with at least one parent holding an upper-secondary school diploma. Both groups exhibit parallel pre-trends, with joint tests failing to reject the null of zero pre-treatment effects.

The effects differ significantly in the post-treatment period by parental education. For children of less-educated parents, school enrollment increases by 7.3 percentage points on average, with effects growing over time and reaching approximately 10–12 percentage points since period 4. In contrast, children of more-educated parents experience smaller and less consistent effects that do not persist over time.[4] By period 6, the difference between the two groups reaches 11 percentage points (p = 0.038, stratified clustered bootstrap at the district level). Relative to the baseline enrollment gap of 36 percentage points in 2002 (Figure 2), the differential effect represents approximately 30% of the gap, suggesting that industrial zones contributed meaningfully to narrowing intergenerational educational inequality in treated areas.

Figure 5: Industrial Zones and School Enrollment 15–18

Heterogeneity by Parental Education

pre-trend p-val = 0.980, post-ATT = 0.073 (0.014)
baseline outcome mean = 0.53

pre-trend p-val = 0.631, post-ATT = 0.021 (0.017)
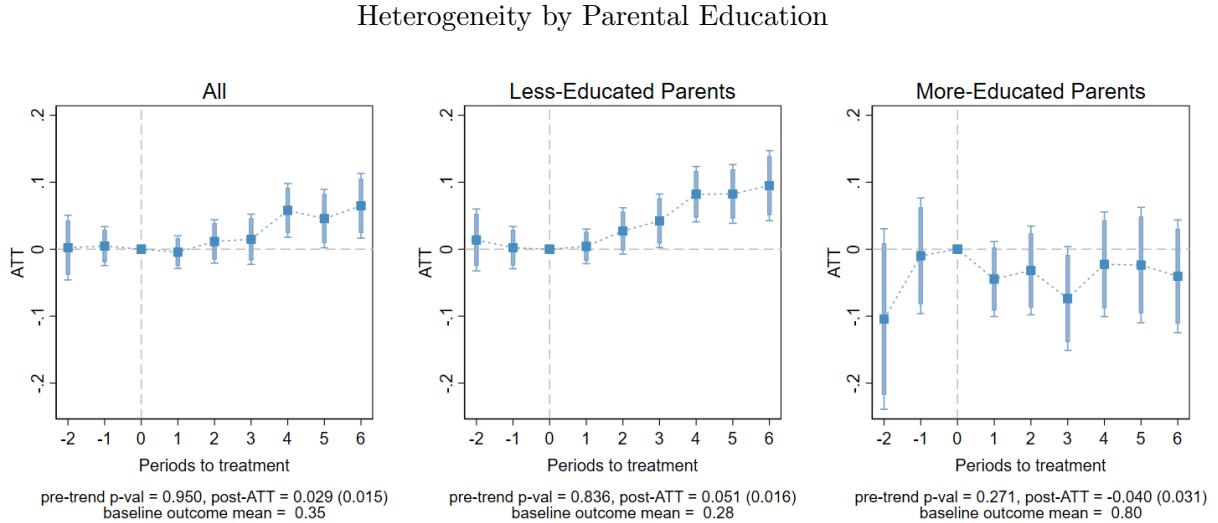baseline outcome mean = 0.88

Notes: This figure shows the effects of industrial zone exposure on school enrollment of children by parental education, using data from VHLSS 2002–2020. The outcome is whether a child has attended school during the past 12 months before the interview. Square markers indicate the point estimates of the coefficients. Darker vertical lines with caps show 95% confidence intervals, and lighter bars represent 90% confidence intervals. *pre-trend* p-val is the p-value from the joint test that pre-treatment effects are zero. *post-ATT* represents the average treatment effect on the treated across post-treatment periods, with standard errors clustered at the district level in parentheses. Estimates are derived using the method proposed by de Chaisemartin and d'Haultfoeuille (2024). Sampling weights are applied throughout.

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[4]Robustness checks reveal that the period 4 estimate for children of more-educated parents is sensitive to the exclusion of early survey years (2004–2008), while estimates for less-educated parents remain stable regardless of which years are excluded (Appendix Table A2). This suggests the temporary elevation in period 4 for more-educated parents reflects noise in early cohorts rather than a true treatment effect.

One might worry that school enrollment does not guarantee educational attainment. Children may enroll but subsequently drop out, or enrollment could partly reflect grade repetition rather than progression—both common in developing country contexts (Glewwe & Muralidharan, 2016). This distinction matters for labor market access: as discussed in Section 2, 43% of manufacturing occupation categories require at least upper secondary completion, especially for technical positions such as technicians, assemblers, and machine operators (Granata et al., 2023). To shed light on school attainment, I estimate effects on upper secondary completion among individuals aged 19–22—those old enough to have finished upper-secondary school. The results in Figure 6 suggest that industrial zones improve not only enrollment but also degree attainment. Upper secondary completion among 19–22 year-olds with less-educated parents increases by approximately 10 percentage points ($p < 0.01$) in period 6, a 35% increase relative to the baseline mean of 28%. Effects for children of more-educated parents are much less precisely estimated. The difference between groups is statistically significant at the 1% level ($p = 0.004$, stratified clustered bootstrap).

Figure 6: Industrial Zones and Upper-Secondary Completion

Heterogeneity by Parental Education

pre-trend p-val = 0.950, post-ATT = 0.029 (0.015)
baseline outcome mean = 0.35

pre-trend p-val = 0.836, post-ATT = 0.051 (0.016)
baseline outcome mean = 0.28

pre-trend p-val = 0.271, post-ATT = -0.040 (0.031)
baseline outcome mean = 0.80

Notes: This figure shows the effects of industrial zone exposure on upper-secondary school completion of individuals 19–22 by parental education, using data from VHLSS 2002–2020. The outcome is whether an individual has obtained an upper-secondary school degree or higher. Square markers indicate the point estimates of the coefficients. Darker vertical lines with caps show 95% confidence intervals, and lighter bars represent 90% confidence intervals. *pre-trend* p-val is the p-value from the joint test that pre-treatment effects are zero. *post-ATT* represents the average treatment effect on the treated across post-treatment periods, with standard errors clustered at the district level in parentheses. Estimates are derived using the method proposed by de Chaisemartin and d'Haultfoeuille (2024). Sampling weights are applied throughout.

Together, the results indicate that industrial zones increase school enrollment among

children aged 15–18, with effects concentrated among those whose parents did not complete upper-secondary school. These enrollment gains are accompanied by higher rates of upper secondary completion, suggesting lasting effects on educational attainment. The findings are consistent with industrial zones contributing to a narrowing of the educational gap across generations. Before examining the mechanisms underlying these patterns, I assess the robustness of the main findings to alternative specifications and sample restrictions.

6.2 Robustness Checks

I conduct several robustness checks along the following dimensions: alternative comparison groups, different levels of clustering, sample restrictions, and alternative estimators. Table 3 presents the results, focusing on school enrollment among children aged 15–18 and upper-secondary school completion among individuals aged 19–22.

Alternative Comparison Group. The baseline specification uses only never-treated districts as controls, minimizing potential bias from compositional changes when not-yet-treated units eventually become treated (Baker et al., 2025). However, never-treated districts may systematically differ from treated districts in unobserved ways. Column (2) extends the comparison group to include not-yet-treated districts, results remain quantitatively similar across outcomes.

Inference. In the baseline analysis, I cluster standard errors at the district level—the level of treatment. However, zone planning and coordination occur at the province level, which might introduce spatial correlation in both treatment assignment and outcomes across districts within the same province. As a robustness check, Column (3) clusters standard errors at the province level (63 clusters) to account for this potential correlation. Conclusions remain unchanged: all the effects are significant at the 1% level.

Compositional change. To capture the total impact of industrial zone exposure, including any migration induced by the zones, I estimate the baseline specification without controlling for any demographic variables. Although including these variables could improve precision, they risk serving as "bad controls" if they are themselves influenced by the treatment. In particular, if industrial zones attract migrants and thereby alter local demographics in ways related to the outcomes of interest, controlling for these variables could bias the estimated effects. As shown in Column (4), including controls for individual characteristics such as age, gender, and ethnic minority does not meaningfully change the estimated treatment effects. Across panels, the coefficients remain closely aligned with the baseline estimates, reinforcing the robustness of the findings.

Table 3: Robustness Checks

	Baseline	Not-yet treated	Province clusters	Demo-graphic controls	Long-term residents	Baseline Linear Trends	Alternative Estimator	Timing mismatch	Province Analysis
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Outcome is school enrollment, sample of 15–18									
Post-ATT	0.062	0.059	0.062	0.055	0.062	0.057	0.064	0.054	0.052
	(0.013)	(0.012)	(0.015)	(0.012)	(0.013)	(0.013)	(0.013)	(0.019)	(0.038)
p-value pre-trend	0.987	0.943	0.985	0.994	0.986	0.971	.	0.975	0.899
Observations	63407	74843	63407	63407	62784	60820	.	39994	41587
Switcher-Period	1185	1185	1185	1185	1185	1158	.	376	1463
Panel B: Outcome is school enrollment, sample of 15–18, less-educated Parents									
Post-ATT	0.073	0.069	0.073	0.064	0.074	0.065	0.067	0.072	0.053
	(0.014)	(0.014)	(0.016)	(0.014)	(0.014)	(0.015)	(0.014)	(0.023)	(0.041)
p-value pre-trend	0.980	0.998	0.981	0.974	0.942	0.965	.	0.641	0.918
Observations	52068	61546	52068	52068	51566	49770	.	33221	34116
Switcher-Period	1174	1174	1174	1174	1174	1147	.	378	1459
Panel C: Outcome is upper-secondary completion, sample of 19–22, less-educated Parents									
Post-ATT	0.051	0.048	0.051	0.046	0.049	0.051	0.040	0.046	0.009
	(0.016)	(0.016)	(0.014)	(0.016)	(0.016)	(0.016)	(0.013)	(0.022)	(0.015)
p-value pre-trend	0.836	0.916	0.806	0.757	0.776	0.797	.	0.485	0.584
Observations	45213	53089	45213	45213	44281	43235	.	28344	30512
Switcher-Period	1163	1163	1163	1163	1163	1137	.	366	1454

Notes: "Baseline" represents the estimate of equation (1), where the comparison group includes never treated units only, standard errors are clustered at the district level, using estimator by de Chaisemartin et al. (2024). Column (2) also includes not-yet-treated districts as comparison group. Column (3) is the same as the baseline specification but clusters standard errors at the province level. Column (4) is similar to the baseline specification but also controls for demographic characteristics including age, gender, and ethnic minority. Column (5) is similar to the baseline specification but restricts the sample to long-term residents only. Column (6) controls for baseline demographic characteristics (urban and minority population shares) interacted with linear time trends. Column (7) employs a different staggered DiD estimator by Callaway and Sant'Anna (2021). Column (8) addresses concern with timing mismatches between annual treatment data and biennial outcome observations through split-sample estimation strategies. Column (9) presents the province-level analysis where the treatment variable is number of zones established in a province, weighted by the share of population living within 15-km radius of any zone.

To further address migration-induced compositional change, Column (5) restricts the sample to individuals with permanent household registration (*hộ khẩu thường trú*) in their commune of residence. Under Vietnam's household registration system, permanent status provides priority access to local public services, while migrants typically hold only temporary registration (Demombynes & Vu, 2016). Results remain qualitatively similar, likely because districts treated after 2002, which is the primary analytic sample, have low rates of non-registered residents (1.2% in 2004, rising modestly to 2.3% by 2020), unlike early-treated

urban centers before 2002 where non-registration rates are substantially higher.[5]

Baseline trends.   Column (6) controls for baseline urban and minority population shares (in 2002) interacted with linear time trends, allowing for differential trajectories across districts with different initial characteristics. The results remain the same.

In addition, given the temporal coverage of the household surveys and treatment timing, parallel trends can only be assessed using limited number of pre-treatment periods. A failure to reject the null of zero pre-treatment effects does not guarantee that parallel trends hold. To test the robustness of the findings to potential violations, I implement the sensitivity analysis of Rambachan and Roth (2023). The primary concern in this setting is that treated and control districts may follow different secular trends. Under the smoothness restriction which allows any pre-existing linear trend to continue into the post-treatment period, results remain statistically significant (Appendix Figure A1).

Alternative estimator.   Column (7) re-estimates using Callaway and Sant'Anna (2021), which differs from the baseline de Chaisemartin and d'Haultfoeuille (2024) estimator in its parallel trends assumptions. The former imposes unconditional parallel trends and compares treated to never-treated units; the latter requires conditional parallel trends within baseline-treatment cohorts. The results are consistent across both approaches.

Timing mismatch.   Another potential concern with the analysis is that industrial zones are established annually, while VHLSS outcomes are observed biennially. In the baseline analysis, I assign treatment retrospectively: a zone established in 2009 is coded as treated starting in 2010. To assess whether this timing approximation affects the results, Column (8) implements a split-sample strategy following de Chaisemartin and d'Haultfoeuille (2024) and de Chaisemartin et al. (2024) by restricting to observations where treatment status changed in a survey year. The results are similar to the baseline estimates, suggesting that the timing mismatch does not materially bias the findings.

Treatment definition and distance-based analysis.   In the baseline specification, I define treatment as being within a 15 km radius of any zone. This implicitly assumes that districts beyond this range are unaffected by zones and thus serve as valid controls. To examine this assumption, I estimate treatment effects across distance bins from zone centers:

$$y_{idt} = \gamma_d + \gamma_t + \sum_{b=1, b\neq 9}^{B} \delta_b \cdot \text{POST}_{dt} \times \text{Distance}_{d=b} + X_{d0} \cdot t + \varepsilon_{idt} \tag{2}$$

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[5]In the surveys, individuals are asked about their place of household registration: whether it is in the same dwelling within the commune/ward, elsewhere in the province, in another province, or if they have never registered.

where POST_{dt} is a binary indicator equal to one for post-treatment years in district dd d. The term $\text{Distance}_{d=b}$ indicates whether district d falls within distance bin b from the nearest zone. Bins are defined in 5 km increments up to 35 km, with the final two bins covering 35–45 km and 45–60 km to ensure sufficient observations. Bin 9 (45–60 km) is the omitted category. [6] The term $X_{d0} \cdot t$ represents baseline demographic characteristics (e.g., share of ethnic minority and share of urban population) linear time trends.

To address concerns with staggered timing (Borusyak et al., 2024; Goodman-Bacon, 2021), I estimate the specification separately for three subsamples, each comprising never-treated districts and a single treatment cohort (2004, 2006, or 2008). Together, these cohorts account for roughly 70% of all zone establishments. Appendix Figure A2 presents results by cohort. Across panels, effects on school enrollment are statistically significant within 0–10 km but attenuate and become indistinguishable from zero beyond this range. These are consistent with evidence that place-based policy spillovers concentrate within 10–15 km of intervention sites (Abagna et al., 2025; Ehrlich & Seidel, 2018; Gallé et al., 2024; Tafese et al., 2025). This localized pattern supports the Stable Unit Treatment Value Assumption and motivates the 15 km threshold used throughout. Because effects are strongest within 10 km, this threshold likely represents a conservative definition that attenuates estimates toward zero.

Province-level analysis. A potential concern with the baseline district-level analysis is that VHLSS is designed to be representative at the province level, and thus the effects captured might reflect compositional change—shifts in the types of households sampled rather than true behavioral responses. Although controlling for demographics and restricting the analysis to long-term residents do not materially affect the results, I provide additional evidence by estimating at the province level using a continuous treatment measure.

Because zone effects are spatially concentrated, a simple count of zones may not accurately capture exposure. I instead construct a treatment intensity index by interacting the number of zones with the share of the provincial population residing within 15 km of any zone, capturing both the number of zones and population exposure. As all provinces contain at least one zone during the study period, this specification captures intensive margin variation—whether greater exposure intensity leads to larger effects—rather than the extensive margin comparison between exposed and unexposed areas in the baseline analysis.[7] I estimate this specification using the approach proposed by de Chaisemartin and

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[6]Districts are assigned to the bin corresponding to their nearest zone; the establishment year of that zone determines post-treatment timing.

[7]Results are qualitatively similar using only the count of zones, though coefficients are smaller and less precisely estimated.

d'Haultfoeuille (2024) and de Chaisemartin et al. (2024).

For school enrollment among 15–18 year-olds, province-level estimates are positive and consistent in sign with the baseline (Column 9), though less precisely estimated. The effects on upper secondary completion among 19–22 year-olds are attenuated and not statistically significant. This attenuation may reflect the more localized nature of completion effects—completing upper-secondary school requires sustained household resources over several years, whereas enrollment captures a more immediate response—or reduced statistical power given the smaller sample of 19–22 year-olds and fewer province-level clusters.

Permutation-based Placebo test. I conduct a permutation-based placebo test by randomly reassigning each district's treatment timing to another district's outcome data across 1,000 Monte Carlo iterations. Appendix Figure A3 (left panel) shows that the baseline estimate falls well outside the distribution of placebo coefficients, which are centered around zero. Approximately 5% of placebo estimates are significant at the 5% level, close to the rejection rate expected under the null.

Leave-one-out test. Finally, to assess whether results are driven by outlier districts, I sequentially exclude each district and re-estimate the model. Appendix Figure A3 (right panel) shows that coefficients remain stable across iterations, with estimates closely aligning with the baseline.
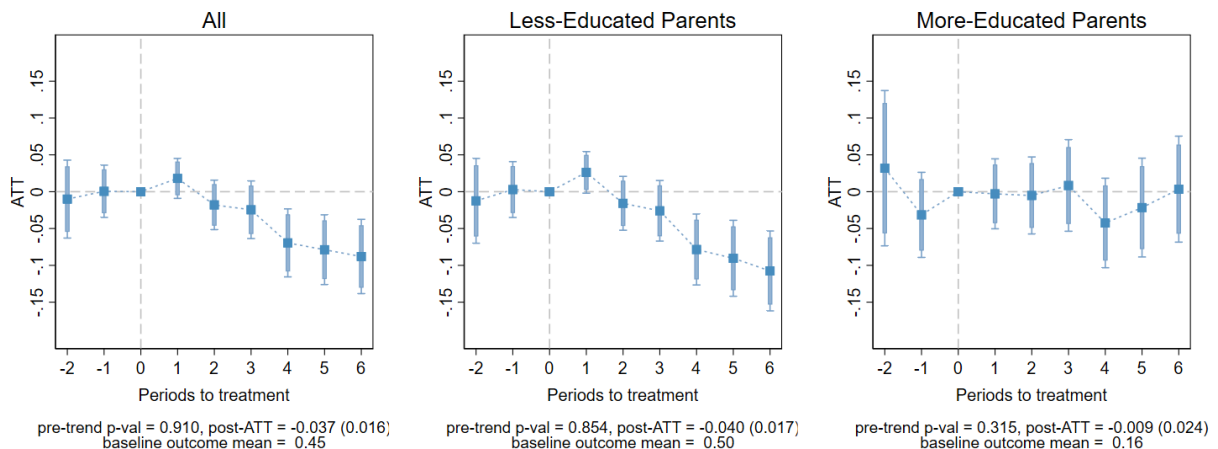
7   POTENTIAL MECHANISMS

The positive effects on school enrollment are concentrated among individuals at the school-work margin (ages 15–18) and from households with likely binding budget constraints (less-educated parents). This pattern suggests that channels increasing enrollment (and eventually attainment) dominate any countervailing forces from opportunity costs or environmental disamenities. In this paper, I do not directly test for disamenities, as outcomes most sensitive to pollution such as academic performance are unavailable, and thus the effects on educational quality remain an open question. In what follows, I provide suggestive evidence on the remaining channels outlined in the conceptual framework.

Child labor. If opportunity costs were the dominant channel, we would expect child labor to increase as zones create employment opportunities accessible to young workers. Instead, Figure 7 shows the opposite pattern: following zone establishment, labor participation among children aged 15–18 decreases gradually, reaching roughly 4 percentage points on average in the post-treatment period. As with school enrollment, this effect is concentrated among

children whose parents did not complete upper-secondary school—the group with higher baseline labor participation (50% compared to 16% for children of more-educated parents). The reduction in child labor (4 percentage points) accompanies the increase in enrollment (7 percentage points), which is consistent with children substituting away from work toward schooling. The difference in magnitudes likely reflects that school attendance and work are not mutually exclusive: some children combine both, and zones may enable a shift toward schooling without fully eliminating work. Together, these patterns suggest that positive channels such as higher household income, increased returns to education, or expanded school access dominate any opportunity cost effects.

Figure 7: Industrial Zones and Labor Participation 15–18

pre-trend p-val = 0.910, post-ATT = -0.037 (0.016)
baseline outcome mean = 0.45

pre-trend p-val = 0.854, post-ATT = -0.040 (0.017)
baseline outcome mean = 0.50

pre-trend p-val = 0.315, post-ATT = -0.009 (0.024)
baseline outcome mean = 0.16

Notes: This figure shows the effects of industrial zone exposure on labor participation of children aged 15–18, using data from VHLSS 2002–2020. The left panel presents results for all individuals, the middle panel for children whose parents did not complete upper-secondary school, and the right panel for children with at least one parent holding an upper-secondary diploma. The outcome is whether a child participated in any economic activity (wage work, household farm work, or non-farm business) during the 12 months preceding the interview. Square markers indicate the point estimates of the coefficients. Darker vertical lines with caps show 95% confidence intervals, and lighter bars represent 90% confidence intervals. *pre-trend* p-val is the p-value from the joint test that pre-treatment effects are zero. *post-ATT* represents the average treatment effect on the treated across post-treatment periods, with standard errors clustered at the district level in parentheses. Estimates are derived using the method proposed by de Chaisemartin and d'Haultfoeuille (2024). Sampling weights are applied throughout.

Household income and education expenditure. The decline in labor participation suggests that households can afford to keep children in school rather than relying on their labor income. A central mechanism in the conceptual framework is that industrial zones rise household earnings, relaxing budget constraints for educational investments. To test this channel directly, I examine whether household income and education expenditure increase

following zone establishment.

The results, presented in Table 4, show differential patterns of income gains by parental education. For households with more-educated parents (Panel A), income gains are concentrated in wage earnings from formal non-agricultural employment, with a point estimate of 4.48 million VND (approximately 25% relative to the baseline mean, $p < 0.05$). Pre-trend tests for this group do not reject parallel trends across income sources.

Table 4: Industrial Zones, Income and Education Expenditure

| Sector | Income from Wage and Household Business Profits | | | | Expenditure |
| | Agriculture | Formal Non-Ag | Informal Non-Ag | All Sectors | Education |
	(1)	(2)	(3)	(4)	(5)
Panel A: Sample Includes 15–18 Children of More-Educated Parents					
Post-ATT	-1.589	4.483	-0.176	2.719	0.255
	(1.395)	(1.890)	(1.796)	(2.180)	(0.320)
Mean Outcome	13.72	18.37	15.19	47.28	1.84
p-value pre-trend	0.678	0.398	0.334	0.581	0.167
Observations	6113	6113	6113	6113	1842
Switcher-Period	794	794	794	794	433
Panel B: Sample Includes 15–18 Children of Less-Educated Parents					
Post-ATT	-1.358	1.778	4.430	4.850	0.313
	(0.831)	(0.373)	(0.799)	(1.005)	(0.067)
Mean Outcome	20.18	2.84	11.30	34.32	0.72
p-value pre-trend	0.029	0.001	0.875	0.002	0.744
Observations	27467	27467	27467	27467	10411
Switcher-Period	1171	1171	1171	1171	1047
District FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y

Notes: This table shows the effects of industrial zone exposure on household annual income by sector and education expenditure, separately for children where parents completed upper-secondary school (Panel A) and those where parents did not (Panel B). Income includes labor compensation (wages and benefits) earned by household members and profits from household enterprises. Agricultural income includes agricultural wages and farm profits. Formal non-agricultural income includes wages from formal employment. Informal non-agricultural income includes informal wages and non-farm enterprise profits. Income measures are winsorized at the top and bottom 1% within each sector-year. All outcomes are measured in 2010 million Vietnamese Dong. Estimates are derived using the method proposed by de Chaisemartin and d'Haultfoeuille (2024). Standard errors, clustered at the district level, are shown in parentheses. Mean outcomes are calculated based on pre-treatment periods. Data from VHLSS 2002–2020. Sampling weights are applied throughout.

For households with less-educated parents (Panel B), income from informal household non-agricultural activities increases by 4.43 million VND (40% relative to baseline, $p <$

0.01), with a pre-trend p-value of 0.875 suggesting parallel trends for this outcome. However, other income measures for this group (including agriculture, formal non-agricultural, and total income) show evidence of non-parallel pre-trends (p-values of 0.029, 0.001, and 0.002, respectively), warranting caution in their causal interpretation. The significant effect on informal non-agricultural income is consistent with less-educated parents working in the informal sector that expands alongside zone development, rather than obtaining formal employment within zones directly.

Alongside income gains, households also increase spending on children's education. Among children of less-educated parents, education expenditure rises by 0.31 million VND (43% relative to baseline). This pattern is consistent with households allocating additional resources toward schooling, though the evidence is suggestive rather than definitive of a causal chain from income to spending to enrollment.

Returns to education. An alternative channel may also contribute to the increased school enrollment and decreased child labor among 15–18 year-olds. The conceptual framework suggests that zones could raise (perceived) returns to education by creating jobs that reward schooling. If this channel is operative, effects are expected to larger in areas where zones generate higher-skilled employment.

To explore this possibility, I proxy for local skill intensity using the share of workers with at least an upper-secondary school diploma in 2002, the earliest available survey round, and classify districts above the median as "high-skill." Districts with higher baseline skill shares may attract zones with greater demand for educated workers, making the returns to schooling more salient to local households. The results in Columns (1)–(4) of Table 5 suggest that enrollment effects, particularly children of less-educated parents, are somewhat larger in high skill intensity areas.[8]

However, a concern with this interpretation is that baseline skill intensity may correlate with other district characteristics that independently affect enrollment. For example, if high-skill districts also experience greater school expansion, the differential enrollment effects could reflect supply-side improvements rather than returns to education.

To explore this, I examine whether distance to the nearest primary, lower-secondary, and upper-secondary school decreases following zone establishment, separately by baseline skill intensity. Columns (3)–(6) show that school access improves in both high and low skill intensity areas, suggesting that supply-side expansion contributes to enrollment gains overall. However, the magnitude of infrastructure improvement is similar across both skill-level groups, while enrollment effects are larger in high-skill areas. This pattern suggests that

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[8]The difference is not significant at conventional levels with stratified clustered bootstrapping.

Table 5: Industrial Zones, Skill Intensity, and Distance to School

| | School Enrollment | | School Enrollment | | Distance to Nearest School (km) | | | |
| | 15–18 | | 15–18 Less-Educated Parents | | Lower Secondary School | | Upper Secondary School | |
	Low-Skill (1)	High-Skill (2)	Low-Skill (3)	High-Skill (4)	Low-Skill (5)	High-Skill (6)	Low-Skill (7)	High-Skill (8)
Post-ATT	0.056	0.091	0.060	0.103	-3.108	-3.146	-7.312	-7.406
	(0.017)	(0.020)	(0.018)	(0.025)	(1.024)	(1.070)	(3.001)	(3.190)
Mean Outcome	0.54	0.66	0.50	0.58	2.79	2.40	8.79	11.83
p-value pre-trend	0.481	0.303	0.407	0.226	0.579	0.333	0.910	0.194
Observations	38694	22126	33573	16197	13000	5193	6896	2726
Switcher-Period	620	538	618	529	306	166	238	124

Notes: This table shows the effects of industrial zone exposure on school enrollment of children 15–18 and distance to schools by district's skill intensity level. A district is considered "high-skill" if the share of working individuals with at least an upper-secondary school diploma is higher than the national-median in 2002, and "low-skill" otherwise. Estimates are derived using the method proposed by de Chaisemartin and d'Haultfoeuille (2024). Standard errors, clustered at the district level, are shown in parentheses. Mean outcomes are calculated based on pre-treatment periods. Data from VHLSS 2002–2020. Sampling weights are applied throughout.

supply-side expansion alone cannot explain the differential enrollment response by skill intensity, consistent with the returns to education channel playing an additional role, although I cannot rule out other unobserved differences between high and low skill intensity districts.

I also examine effects on another dimension of human capital: children's health insurance coverage and health expenditure. The results in Appendix Table A3 show that insurance coverage increases by 3.7 percentage points overall ($p < 0.05$), with suggestive evidence of larger effects among children of more-educated parents (5.7 percentage points vs. 2.9). The effects on health expenditure are less precisely estimated and more difficult to interpret, as higher spending could reflect either greater health investment or increased medical needs. The point estimates suggest that less-educated parents increase spending on inpatient and outpatient treatment, while the negative coefficient for more-educated parents could indicate a shift toward preventive care that reduces curative expenses. However, these patterns are speculative given the imprecision of the estimates.

Together, these findings point to household income as the primary channel through which industrial zones increase school enrollment. Child labor declines following zone establishment, indicating that opportunity cost effects do not dominate. Household income rises, particularly through informal non-agricultural activities for less-educated households, suggesting that these families benefit from spillovers to the local economy rather than direct employment in zones. Higher income translates into increased spending on children's educa-

tion. Supply-side improvements also contribute: distance to schools decreases following zone establishment. However, these infrastructure gains are similar across districts with high and low baseline skill intensity, while enrollment effects are larger in high-skill areas. This differential response suggests that perceived returns to education may play an additional role: in districts where zones employ a larger share of skilled workers, households may update their beliefs about the value of schooling. Importantly, this district-level pattern reinforces rather than contradicts the individual-level finding that effects concentrate among children of less-educated parents: Table 5 shows that less-educated households in high-skill districts exhibit the strongest enrollment response, consistent with these households facing both relaxed budget constraints and stronger signals about returns to education.

8 CONCLUSION

This paper examines whether industrial zone expansion in Vietnam contributes to increased school enrollment and reduced intergenerational educational inequality. Using a staggered difference-in-differences design and data from the national household surveys over the last two decades, I find that industrial zone establishment increases school enrollment among children aged 15–18 by approximately 6 percentage points. This effect is concentrated among children whose parents did not complete upper-secondary school—the group with the lowest baseline enrollment and highest rates of child labor. For this group, enrollment increases by 7 percentage points, a substantial effect relative to the baseline gap of 36 percentage points. Child labor declines in parallel, suggesting that children substitute away from work toward schooling.

The evidence is consistent with household income as an important channel. Less-educated households experience income gains concentrated in informal non-agricultural activities, suggesting spillovers to the local economy rather than direct zone employment, and increase spending on children's education. Supply-side improvements also contribute: distance to schools decreases following zone establishment. There is also suggestive evidence that perceived returns to education may play an additional role in areas with higher baseline skill intensity.

These findings have important policy implications for developing countries pursuing export-oriented industrialization. A common concern is that less-educated households cannot benefit from industrial zones because they lack the skills required for formal employment. The results suggest otherwise: zones appear to generate spillovers to the local informal economy that reach less-educated households, who in turn increase spending on children's education. This indirect channel may be important for the distributional impacts of place-based

industrial policies. If zone benefits accrued only through formal employment, less-educated households would be largely excluded, potentially widening rather than narrowing educational inequality.

Several limitations warrant discussion. First, while I document increases in enrollment, I cannot observe educational quality. If industrial zones generate environmental disamenities that affect child health or learning, the quantity gains documented here could be partially offset. This concern is particularly relevant for less-advantaged households, who may have lower capacity to mitigate environmental harms through residential sorting or defensive investments (Dasgupta et al., 2005; Deschenes et al., 2017; Leonova & Rentschler, 2022). Effects on academic achievement remain an open question for future research. Second, the analysis focuses on Vietnam, a country with relatively strong baseline educational infrastructure and enrollment rates. The findings may not generalize to settings with weaker institutions or different labor market structures.

Despite these limitations, this paper provides evidence that industrial zones—a prominent development policy—can contribute to reducing intergenerational educational inequality. Industrialization, through income gains that reach less-educated households via the local economy, may help narrow the gap in educational attainment across generations.

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A APPENDIX TABLES AND FIGURES

A.1 Appendix Tables

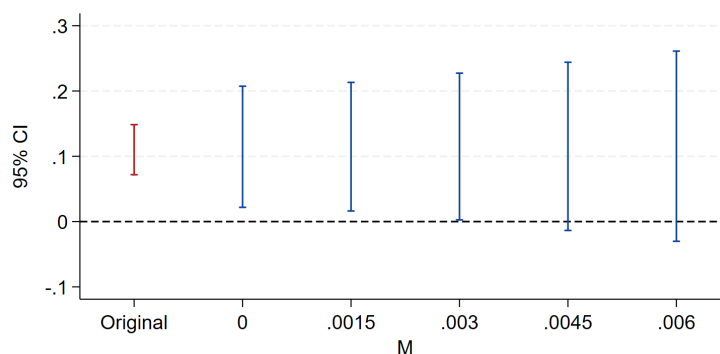Table A1: Establishment of Industrial Zones Across Districts, 2002–2020

	Share of districts hosting zone (1)	Share of districts within 15-km radius of zone (2)
2002-2004	0.038	0.419
2005-2008	0.091	0.157
2009-2012	0.032	0.050
2013-2020	0.014	0.026
Never-treated	0.826	0.348

Notes: This table presents the percentage of districts that either have an industrial zone within their boundaries (Column 1) or are located within a 15-kilometer radius of one (Column 2) during the study period 2002–2020.

A.2 Appendix Figures

Figure A1: Industrial Zones and School Enrollment 15–18, Less-Educated Parents

Under Potential Violations of Parallel Trends Assumption

Notes: This figure shows the robustness of the estimated effects on school enrollment of children aged 15–18 in period 4 to potential violations of the parallel trends assumption, following Rambachan and Roth (2023). The smoothness parameter M bounds the maximum change in the differential trend slope between consecutive periods. The red bar labeled "Original" reports the baseline estimate. Blue bars show 95% confidence intervals under increasingly large values of M. At $M = 0$, any pre-existing linear differential trend is allowed to continue into the post-treatment period. At $M > 0$, the trend is additionally allowed to bend by up to M per period. The effect for children of less-educated parents remains statistically significant at 5% level up to $M \approx 0.003$.

Table A2: Robustness Checks: Period 4 School Enrollment Effects

	More-Educated Parents	Less-Educated Parents
	(1)	(2)
Baseline	0.068	0.110
	(0.022)	(0.020)
2002	0.043	0.101
	(0.026)	(0.022)
2004	0.009	0.119
	(0.028)	(0.020)
2006	0.019	0.105
	(0.027)	(0.021)
2008	0.022	0.111
	(0.026)	(0.022)
2010	0.025	0.103
	(0.026)	(0.022)
2012	0.071	0.096
	(0.023)	(0.022)
2014	0.078	0.103
	(0.022)	(0.022)
2016	0.072	0.108
	(0.022)	(0.020)
2018	0.074	0.110
	(0.022)	(0.020)
2020	0.069	0.111
	(0.022)	(0.020)

Notes: Each row reports the period 4 treatment effect when excluding the indicated survey year from estimation. Column 1 shows effects for children of more-educated parents. Column 2 shows effects for children of less-educated parents. Estimates for less-educated parents remain stable across specifications, while estimates for more-educated parents are sensitive to the exclusion of early survey years (2004–2010). Standard errors clustered at district level are in parentheses.

Table A3: Industrial Zones and Health Outcomes

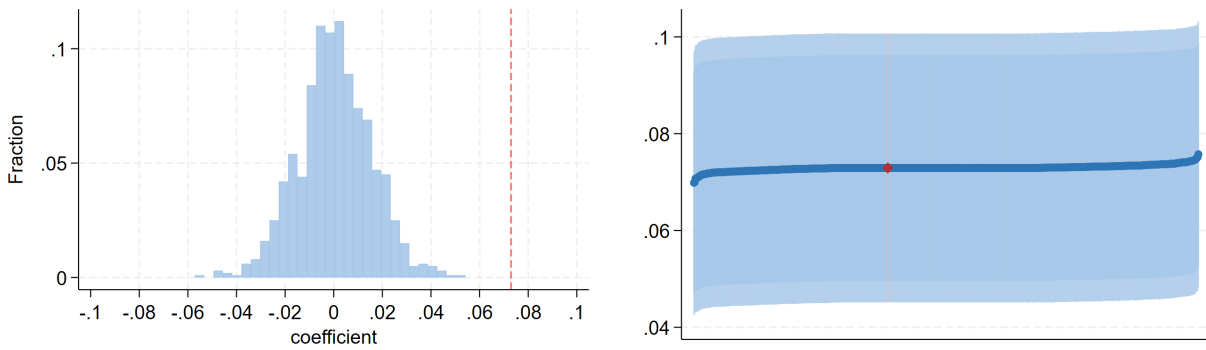|	Insurance Coverage			Health Expenses		
	All	Less-educated Parents	More-educated Parents	All	Less-educated Parents	More-educated Parents
	(1)	(2)	(3)	(4)	(5)	(6)
Post-ATT	0.037	0.029	0.057	0.044	0.088	-0.068
	(0.018)	(0.019)	(0.030)	(0.048)	(0.053)	(0.105)
Mean Outcome	0.54	0.51	0.68	0.10	0.10	0.13
p-value pre-trend	0.098	0.072	0.238	0.419	0.213	0.993
Observations	49882	41574	7975	12632	10411	1842
Switcher-Period	837	833	660	1116	1047	433

Notes: This table shows the effects of industrial zone exposure on insurance (0/1) and health expenses (in million 2010 Vietnamese Dong). Estimates are derived using the method proposed by de Chaisemartin and d'Haultfoeuille (2024). Standard errors, clustered at the district level, are shown in parentheses. Mean outcomes are calculated based on pre-treatment periods. Data from VHLSS 2002–2020. Sampling weights are applied throughout.

Figure A2: Industrial Zones and School Enrollment by Distance Bin

Notes: The outcome is school enrollment of 15–18-year-olds. Thick bars indicate 90% confidence intervals and thin bars with caps indicate 95% confidence intervals. Circle symbols represent estimated coefficients $\hat{\delta}_b$ from equation (2). Sampling weights are applied throughout. Source: Data from VHLSS 2002–2020.

Figure A3: Placebo and Leave-One-Out Tests

Notes: The outcome is school enrollment of 15–18-year-olds whose parents did not complete upper-secondary school education. The left panel shows the distribution of estimated coefficients from 1,000 Monte Carlo simulations in which each district's treatment timing is randomly reassigned to another district's outcome data. The red vertical line indicates the baseline estimate, which falls well outside the distribution of placebo coefficients. The right panel shows estimates from a leave-one-district-out exercise, in which each district is sequentially excluded from the sample. The red diamond indicates the baseline estimate; blue circles show estimates from each iteration. Results are stable across iterations, suggesting findings are not driven by outlier districts. Sampling weights are applied throughout.