

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

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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. Industrial zones and special economic zones are among the most widely used instruments for promoting export-oriented growth in developing countries, with over 5,000 zones operating worldwide across contexts ranging from East and Southeast Asia to Sub-Saharan Africa (United Nations Conference on Trade and Development, 2019). A major concern in these settings is that the benefits of industrialization may accrue primarily to workers with sufficient skills to access formal employment, bypassing less-educated households. Whether zones can instead generate broad-based gains that reach disadvantaged families, and whether such gains translate into human capital investments that narrow intergenerational inequality, remains an open question.

The expected effects are theoretically ambiguous. Zones may increase school enrollment through several channels: rising household income can relax budget constraints on educational investment (Basu & Van, 1998; Edmonds, 2005); skilled jobs may raise the perceived returns to education (F. Lu et al., 2023); and associated infrastructure investment may improve access to schools. If, instead, zones primarily expand employment opportunities accessible to teenagers or shift domestic responsibilities onto children as parents enter the expanding local economy, the opportunity cost of schooling rises, pulling children out of school (Atkin, 2016). The implications for intergenerational inequality are

similarly ambiguous. Children from less-educated households, who face the tightest budget constraints, may exhibit the largest enrollment responses to rising income (Edmonds & Pavcnik, 2005). These households are, however, less likely to access formal zone employment, which may limit the magnitude of such gains. Their children are also more likely to be at the margin between school and work, making them more susceptible to the opportunity cost channel (Atkin, 2016).

I study this question in Vietnam, a lower-middle-income country that shares key characteristics with many developing economies pursuing export-oriented industrialization, including substantial intergenerational educational inequality, child labor concentrated in informal household activities, and sharp enrollment declines at the upper-secondary level. Over the past two decades, Vietnam's industrial zone program expanded rapidly, from fewer than 20 zones in the mid-1990s to more than 300 by 2020, with establishment staggered across districts over time. I exploit this variation by combining nationally representative household surveys (VHLSS, 2002–2020) with administrative records on zone establishment from the Ministry of Planning and Investment. The household surveys, which span much of the expansion period, link parental education with child outcomes including school enrollment, child labor, household income by source, and education expenditure. These data allow me to examine both the effects of zone establishment on intergenerational educational inequality and the mechanisms underlying them.

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, 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 results are robust to alternative comparison groups, province-level clustering, sample restrictions to long-term residents, and potential violations of the parallel trends assumption (Rambachan & Roth, 2023).

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 relative to a baseline of 53%. Effects for children of more-educated parents are not statistically distinguishable from zero. The narrowing of the enrollment gap is driven by gains among disadvantaged children rather than a ceiling on enrollment among advantaged children. 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 the primary channel. Child

labor declines alongside the increase in enrollment, ruling out the opportunity cost channel, which would predict the opposite pattern. Less-educated households experience income gains concentrated in informal non-agricultural activities, consistent with spillovers to the local economy rather than direct employment in zones, and allocate part of this additional income toward children's education. Distance to schools also decreases following zone establishment, but these infrastructure improvements are similar across districts with high and low baseline skill intensity, while enrollment effects are larger in high-skill areas. This pattern suggests that supply-side expansion alone cannot account for the concentration of effects among children of less-educated parents, and that demand-side forces, particularly income gains, are the dominant mechanism.

This paper contributes to two strands of the literature. First, it adds to research on the determinants of intergenerational educational inequality. A large literature documents the persistence of educational advantage across generations (Black & Devereux, 2011), and prior work identifies policies that can weaken this transmission, including school construction (Akresh et al., 2023), cash transfers (Barham et al., 2024; Parker & Vogl, 2023; Schultz, 2004), and early childhood investments (Heckman et al., 2010). I show that place-based industrial policy can serve a similar function: industrial zones increase enrollment and upper-secondary completion among children of less-educated parents, narrowing the educational gap across generations. The mechanism operates not through direct policy transfers to households, as in conditional cash transfer programs, but through income gains generated by local economic spillovers from zone activity. This finding identifies a previously undocumented pathway through which industrialization can reduce intergenerational educational inequality.

Second, the paper contributes to the literature on the socioeconomic effects of place-based policies. A rich body of work examines the effects of economic zones on firm outcomes, employment, and local labor markets (Busso et al., 2013; Y. Lu et al., 2019; Tafese et al., 2025; Wang, 2013), and more recently on broader welfare measures including house-

hold wealth, female labor supply, intra-household resource allocation, and human capital (Abagna et al., 2025; Gallé et al., 2024; F. Lu et al., 2023; Zhao & Qu, 2024). This literature has focused on average effects or heterogeneity by zone type, but not on whether zones affect children differentially by family background. Despite long-standing concerns about the distributional consequences of export-oriented industrialization, particularly in settings where informality is prevalent (Goldberg & Pavcnik, 2007), the findings of this paper suggest otherwise. Less-educated households benefit from informal non-agricultural income generated by zone activity (Pham, 2026), and these gains translate into lower child labor and greater educational investment, indicating that income effects dominate opportunity cost effects in this setting.

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

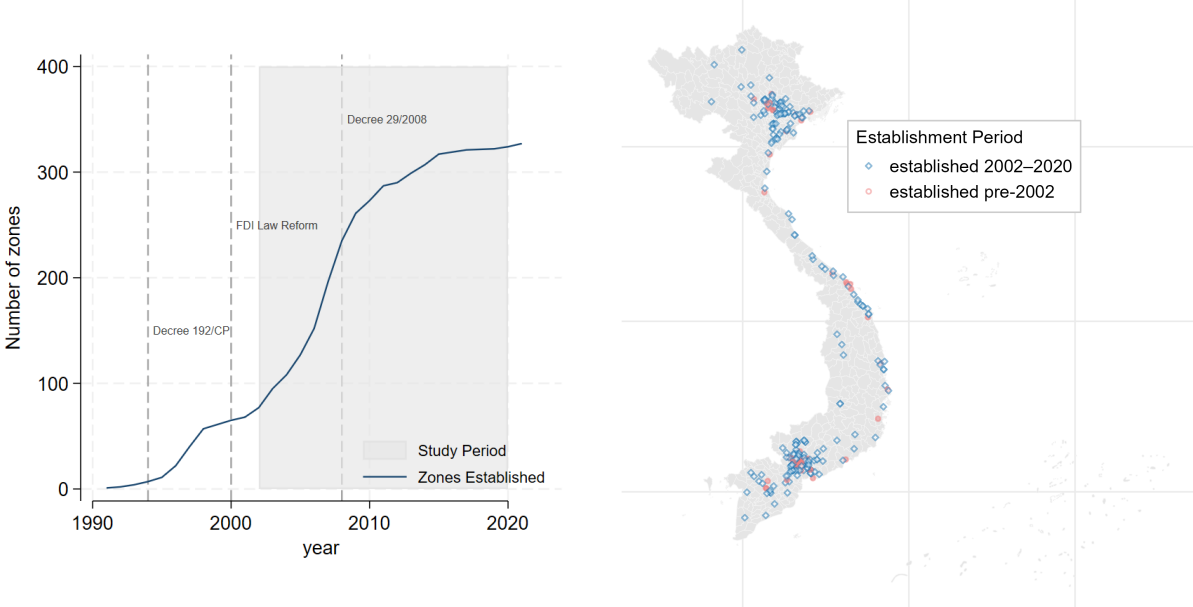
2.1 Industrial Zones in Vietnam

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.

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.

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 regu-

latory environment suggests that zone establishment may affect local educational access through more schools constructed.

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, footwear, 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.

Zones generate employment across a range of manufacturing activities. Data from the 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, including 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 such as 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 school diploma are largely confined to entry-level positions with lower wages and less job security.

Alongside skill requirements, labor regulations restrict the employment of minors in industrial zones. Vietnam's Labor Code sets the minimum legal working age at 15, with substantial restrictions on workers aged 15–17: they may not exceed 8 hours per day or 40 hours per week, and are prohibited from hazardous activities prevalent in zone manufacturing, including carrying heavy loads, handling chemicals, and operating machinery.¹ Foreign-invested enterprises, which account for a large share of zone employment, face additional enforcement pressure through international supply-chain auditing and compliance frameworks such as Better Work Vietnam, an ILO-IFC partnership, which monitors labor standards including child labor provisions and publicly discloses compliance information (Better Work Vietnam, 2015; Hollweg, 2019).

¹These provisions are codified in the Labor Code (No. 45/2019/QH14, Articles 143–147), which updated and consolidated earlier regulations from the 2012 Labor Code. The list of prohibited occupations for workers under 18 is specified by the Ministry of Labor, Invalids, and Social Affairs.

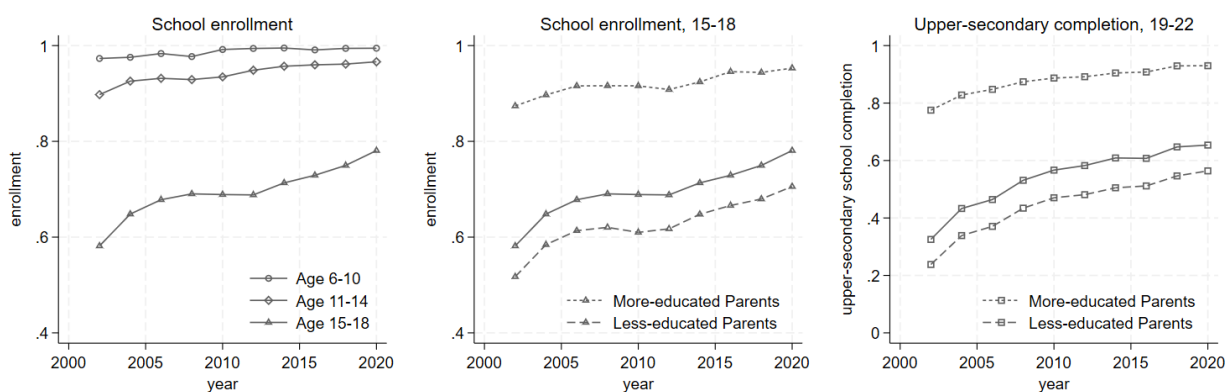
2.2 Education System and Education Trends

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.

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 intergener-

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.

ational 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

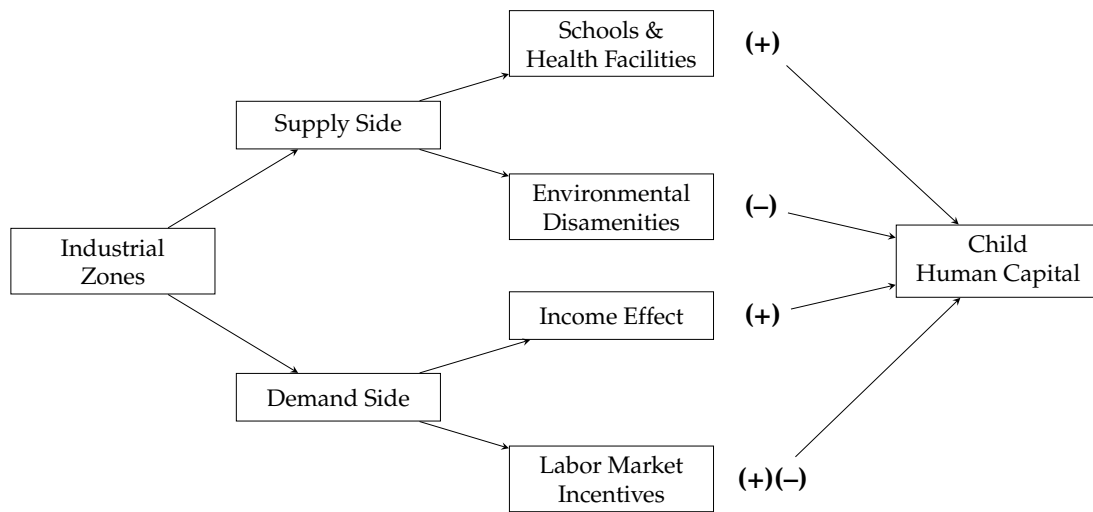
Industrial zones may affect children’s education through both supply-side and demand-side channels, as summarized in Figure 3. In what follows, I discuss the expected effects, their heterogeneity by parental education, and which channels are empirically distinguishable with the available data.

3.1 Supply-Side Channels

Local service provision and infrastructure. Industrial zones can improve access to education by catalyzing local infrastructure investment. As discussed in Section 2, Vietnam’s regulatory framework increasingly links zone development to surrounding social infrastructure, including schools, and zones are often accompanied by public investment in transport and utilities that can also benefit nearby households (United Nations Conference on Trade and Development, 2019). To the extent that zones expand the supply

of local education infrastructure, these improvements are expected to benefit children broadly, regardless of parental background. Differential effects concentrated among one parental education group would therefore point toward demand-side rather than supply-side mechanisms. The household survey reports distance to the nearest school, providing a direct measure of whether school access improves following zone establishment.

Figure 3: Mechanisms Linking Industrial Zones to Child Human Capital



Environmental disamenities. Zones may also generate negative externalities. Industrial activity can increase air and water pollution, worsening child health and reducing learning through increased school absences and impaired cognitive performance (Currie et al., 2009; Greenstone & Hanna, 2014). These effects could partially offset gains from improved infrastructure or higher income, and may disproportionately affect disadvantaged households that have fewer avoidance options or are more likely to reside near industrial sites. However, outcomes most sensitive to this channel, such as test scores or detailed health measures, are not available in the survey data. Thus, the effects of zones on educational quality remain an open question that I return to in the conclusion.

3.2 Demand-Side Channels

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

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 account for most of the decline in child labor among households that moved out of poverty during the 1990s.

If income effects dominate, children from less-educated households are expected to benefit more, as these households face tighter budget constraints and are closer to the subsistence margin (Edmonds & Pavcnik, 2005). The nature of the income gains matters for interpretation. In Vietnam's industrial zones, less-educated households are unlikely to access formal zone employment directly, given the skill requirements documented in Section 2. If their income gains instead come from informal non-agricultural activities, specifically local demand spillovers generated by zones, this would indicate that industrialization reaches disadvantaged families through indirect channels rather than direct employment. The data records household income by source and education expenditure, allowing direct examination of whether income rises and which sector drives the gains.

Labor market incentives. Zones may also affect human capital investment by changing the value of working relative to continued schooling. These effects depend on the skill composition of labor demand and can operate through both actual and perceived returns to education.

If zones primarily expand less-skilled employment opportunities, the opportunity cost of schooling rises, potentially increasing dropout. Atkin (2016) documents this mechanism in Mexico, where expansions in export manufacturing increased school dropout, driven by 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. A related force operates within the household: if zones draw parents into the expanding local economy, children may substitute into household tasks (e.g., farming, tending the family business) that parents previously performed, further increasing the demands on children's time. Under this channel, children of less-educated parents are most vulnerable to dropout, as 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). Zones would then widen rather than narrow intergenerational educational inequality, with child labor increasing alongside enrollment declines.

Conversely, if zones generate sizable wage premiums for educated workers, households may increase schooling in anticipation of higher returns. F. Lu et al. (2023) provides evidence consistent with this channel: technology-oriented zones in China that demand skilled labor increase upper-secondary enrollment. Zones may also shift perceived returns by making formal-sector wage differentials visible, even holding actual returns fixed (Jensen, 2010; Nguyen, 2008). Less-educated households may update beliefs more strongly if they have less prior exposure to formal-sector opportunities, but more-educated households may be better positioned to act on new information due to greater resources and familiarity with the schooling system (Attanasio & Kaufmann, 2014). The implied heterogeneity by parental education is therefore ambiguous.

3.3 Summary of Testable Predictions

Given Vietnam's zone composition, where labor-intensive manufacturing in garments, footwear, and food processing accounts for the largest share of employment (Table 1),

both income and labor market channels are relevant. These channels yield distinguishable empirical predictions. If the income channel dominates, enrollment rises, child labor falls, household income increases, and education expenditure grows, with effects concentrated among children of less-educated parents whose households likely face the tightest budget constraints. If the opportunity cost channel dominates, child labor increases and enrollment falls, particularly among children of less-educated parents near the school-work margin. Supply-side improvements are testable through changes in school proximity: if they account for the results, effects should be broadly similar across parental education groups. The returns-to-education channel is partially testable by examining whether enrollment effects vary with the baseline skill intensity of the local labor market. However, as the household survey does not collect data on perceived returns, this evidence is suggestive. The environmental channel cannot be tested with the available data. Section 7 examines these predictions systematically, focusing on the channels where the data are most informative.

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.² 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 and subsequent waves cover over 45,000 households across more than 3,000 communes. The survey uses a three-stage stratified cluster design. Strata are defined by province and urban/rural status. Communes are selected as primary sampling units with probability proportionate to size, followed by the selection of enumeration areas within communes, and then households within enumeration areas. The survey defines household members as individuals who have eaten and lived with the household for at least six months and share a collective fund. Notably, pupils and students studying in other localities but still financially supported by the family are regarded as household members of their origin household, ensuring that young adults attending school elsewhere are captured in the data.³

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 iden-

²<https://data.humdata.org/dataset/cod-ab-vnm>

³For details, see [Vietnam Household Living Standards Survey: Operational Handbook](#). 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).

tifiers. 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 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 as revenues minus costs, using details from the Agricultural Production and Non-Agricultural Business Module. 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 (1)	Districts with Zones since 2002 (2)	Difference (2) – (1) (3)	Districts with Zones 2002–2004 (4)	Districts with Zones 2005–2008 (5)	Districts with Zones 2009–2020 (6)	Difference (5) – (4) (7)	Difference (6) – (4) (8)
<i>Panel A: Demographics</i>								
Age	13.936 [0.527]	13.970 [0.426]	-0.034 (0.050)	13.989 [0.420]	13.937 [0.425]	14.027 [0.440]	-0.052 (0.070)	0.038 (0.086)
Male	0.517 [0.103]	0.520 [0.086]	-0.003 (0.010)	0.513 [0.095]	0.529 [0.084]	0.511 [0.074]	0.016 (0.015)	-0.002 (0.016)
Ethnic minority	0.363 [0.407]	0.139 [0.232]	0.224 (0.037)	0.069 [0.175]	0.146 [0.232]	0.243 [0.280]	0.077 (0.032)	0.174 (0.045)
Educated parents	0.138 [0.147]	0.174 [0.149]	-0.036 (0.016)	0.216 [0.135]	0.140 [0.138]	0.192 [0.181]	-0.077 (0.021)	-0.024 (0.033)
Urban	0.126 [0.182]	0.137 [0.207]	-0.011 (0.020)	0.151 [0.248]	0.122 [0.174]	0.153 [0.216]	-0.030 (0.034)	0.002 (0.044)
Long-term registration	0.994 [0.014]	0.994 [0.014]	0.001 (0.001)	0.993 [0.013]	0.995 [0.014]	0.992 [0.016]	0.002 (0.002)	-0.001 (0.003)
<i>Panel B: School and Labor</i>								
School enrollment, 10–18	0.736 [0.168]	0.756 [0.121]	-0.020 (0.017)	0.802 [0.090]	0.730 [0.125]	0.748 [0.138]	-0.071 (0.018)	-0.053 (0.023)
Educated parents, 15–18	0.828 [0.287]	0.838 [0.247]	-0.010 (0.040)	0.886 [0.194]	0.804 [0.256]	0.842 [0.295]	-0.082 (0.044)	-0.044 (0.064)
Less-educated parents, 15–18	0.513 [0.229]	0.507 [0.199]	0.006 (0.025)	0.550 [0.191]	0.476 [0.193]	0.513 [0.218]	-0.074 (0.033)	-0.037 (0.042)
Labor participation	0.376 [0.223]	0.271 [0.152]	0.105 (0.021)	0.249 [0.154]	0.292 [0.149]	0.253 [0.150]	0.043 (0.024)	0.004 (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%. Panel B disaggregates enrollment among 15–18-year-olds by parental education, showing that enrollment is substantially lower for children of less-educated parents across all district types.

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 (Section 6.2, 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.⁴

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:

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

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 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

⁴The zone database indicates only one termination as of June 2023, due to infrastructure delays.

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 (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

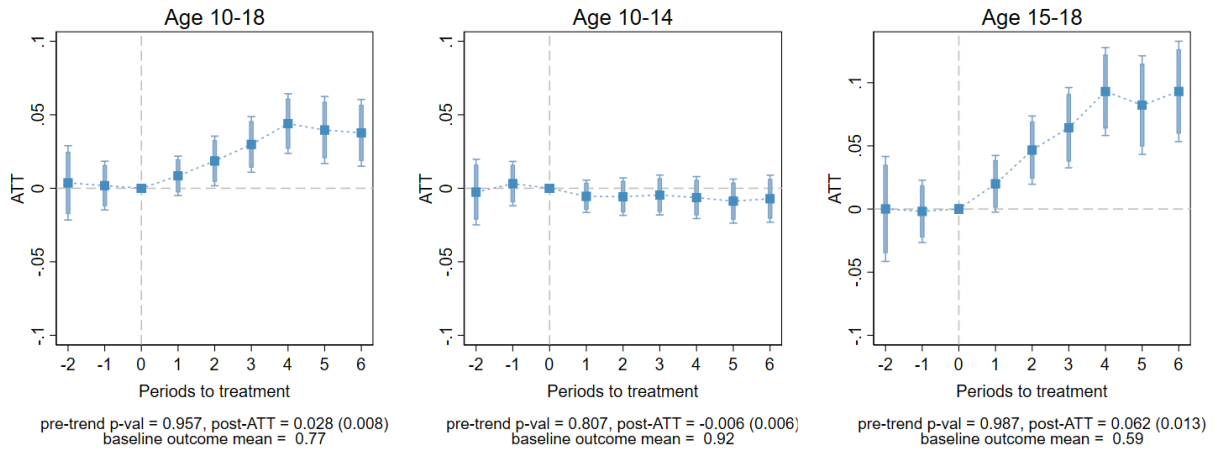
Figure 4 presents the dynamic effects of industrial zone establishment on school enrollment. The left panel shows results for children aged 10–18, while the middle and right panels disaggregate by age group. Across all panels, pre-treatment coefficients are jointly statistically insignificant, supporting 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.

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 markedly in the post-treatment period. 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 by period 4. In contrast, children of more-educated parents experience smaller and less consistent effects that do not persist over time.⁵ By period 6, the difference between the two groups

⁵Robustness 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

Figure 4: Industrial Zones and School Enrollment



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.

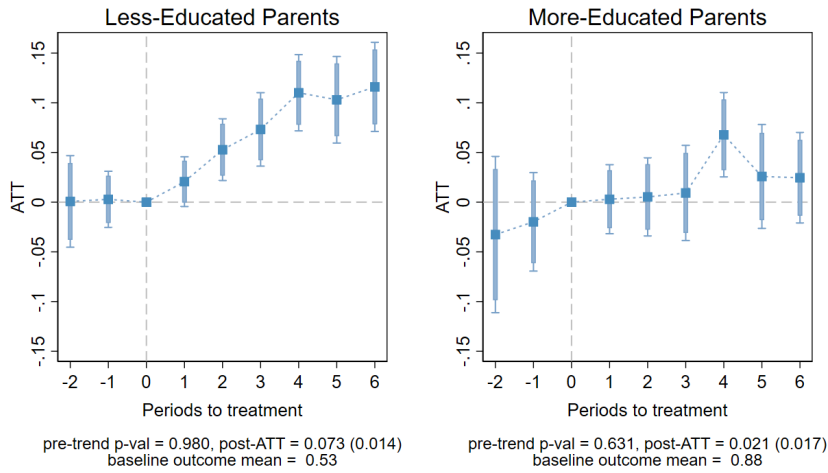
reaches 11 percentage points ($p = 0.038$, stratified clustered bootstrap at the district level). Relative to the baseline enrollment gap of 36 percentage points in 2002 (Figure 2), this differential effect represents approximately 30% of the gap, suggesting that industrial zones contributed meaningfully to narrowing intergenerational educational inequality in treated areas. Notably, this differential is not driven by a ceiling in enrollment among the more-educated group: baseline enrollment for 15–18-year-olds with more-educated parents in treated districts is approximately 84% (Table 2, Panel B), leaving a feasible margin of 16 percentage points, which is sufficient to accommodate an effect comparable to that observed for children of less-educated parents. The muted response for the more-educated group therefore reflects a genuinely small treatment effect rather than a binding constraint on enrollment.

One might worry that school enrollment does not guarantee educational attainment.

period 4 for more-educated parents reflects noise in early cohorts rather than a true treatment effect.

Figure 5: Industrial Zones and School Enrollment 15–18

Heterogeneity by Parental Education



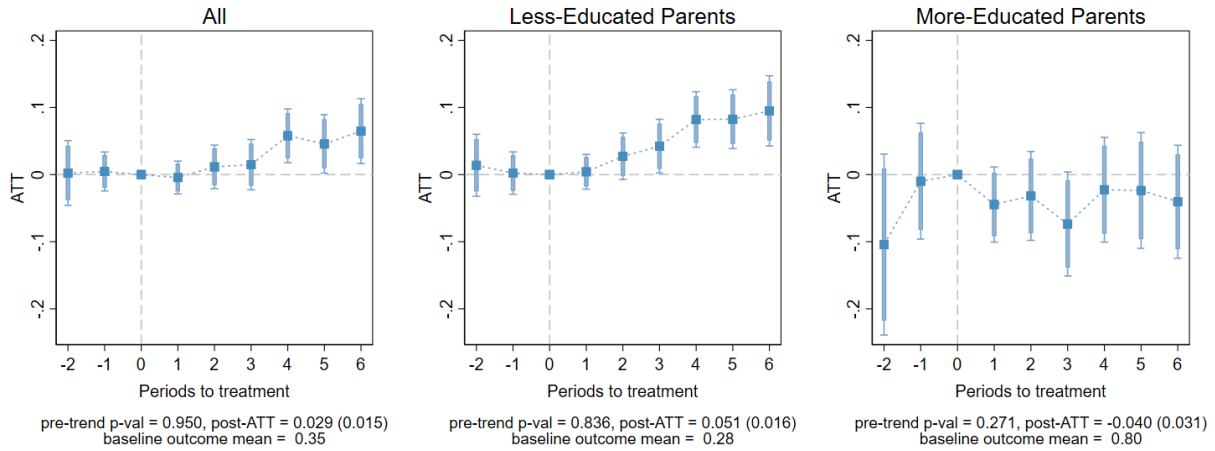
Notes: This figure shows the effects of industrial zone exposure on school enrollment of children by parental education, using data from VHLSS 2002–2020. The outcome is whether a child has attended school during the past 12 months before the interview. Square markers indicate the point estimates of the 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.

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



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 demographic shifts induced by the zones, I estimate the baseline specification without controlling for individual characteristics. Although such controls could improve precision, they risk serving as “bad controls” if they are themselves influenced by the treatment. As shown in Column (4), including controls for age, gender, and ethnic minority does not meaningfully change the estimated treatment effects, suggesting that compositional shifts along these dimensions do not confound the results.

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 (0.013)	0.059 (0.012)	0.062 (0.015)	0.055 (0.012)	0.062 (0.013)	0.057 (0.013)	0.064 (0.013)	0.054 (0.019)	0.052 (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 (0.014)	0.069 (0.014)	0.073 (0.016)	0.064 (0.014)	0.074 (0.014)	0.065 (0.015)	0.067 (0.014)	0.072 (0.023)	0.053 (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 (0.016)	0.048 (0.016)	0.051 (0.014)	0.046 (0.016)	0.049 (0.016)	0.051 (0.016)	0.040 (0.013)	0.046 (0.022)	0.009 (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.

Migration. Migration could affect the interpretation of the results in two ways. First, if zone establishment changes the composition of local households, for instance, by attracting less-educated families seeking economic opportunities, then comparisons across treated and untreated districts could conflate behavioral responses with demographic sorting. Second, migration could itself constitute a channel through which zones affect educational outcomes: if less-educated families relocate to zone areas and their children subsequently benefit from improved local conditions, the enrollment effects would reflect geographic reallocation of families rather than local spillovers reaching incumbent households. Distinguishing between these interpretations matters for understanding how place-based policies generate their effects. The same set of evidence speaks to both concerns.

I begin by testing whether zone establishment induces migration. Under Vietnam's household registration system (*hộ khẩu thường trú*), permanent registration in a commune provides priority access to local public services, while migrants typically hold only temporary registration (Demombynes & Vu, 2016).⁶ Appendix Table A3 presents the results. The overall share of children without household registration in the same commune or ward does not change following zone establishment (Column 1). Disaggregating by parental education reveals a small but statistically significant increase in non-registered children among more-educated families (Column 2), consistent with educated households being more geographically mobile and better positioned to access formal employment in zones. Importantly, however, there is no corresponding increase among less-educated families (Column 3), the group whose children drive the main enrollment results. Because household registration status changes with any move across commune boundaries, including short-distance moves within a district, this measure is sensitive even to local residential mobility. The absence of any response among less-educated families therefore rules out migration as a driver of the main enrollment results. This pattern

⁶In the surveys, individuals are asked about their place of household registration: whether it is in the same commune or ward, elsewhere in the province, in another province, or if they have never registered.

is consistent with the nature of the analytic sample: districts treated after 2002 are mostly secondary locations with low baseline non-registration rates (1.2% in 2004, rising modestly to 2.3% by 2020), unlike the major urban centers treated before the study period where migration pressures are substantially higher.

I next test whether the overall composition of households changes in treated districts. Despite the small inflow of more-educated families, Column (4) of Appendix Table A3 shows no significant change in the share of children with more-educated parents following zone establishment. The magnitude of the migration response in Column (2) is too small in absolute terms to meaningfully alter the local distribution of parental education.

Third, Column (5) of Table 3 restricts the sample to individuals with permanent household registration in their commune of residence, directly excluding any recent migrants from the estimation. Results remain qualitatively similar to the baseline across all panels.

Taken together, these results indicate that zone establishment does not induce meaningful migration into treated districts, nor does it alter the composition of local households by parental education. The enrollment effects documented above therefore reflect behavioral responses among incumbent households. This interpretation is consistent with the mechanism evidence presented in Section 7, which points to income gains in the local informal economy as the primary channel.

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 and comparison group. The former requires parallel trends only between treated cohorts and the never-treated group, whereas the latter imposes parallel trends between all groups across every pair of consecutive periods, conditional on baseline treatment status. 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:

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

where POST_{dt} is a binary indicator equal to one for post-treatment years in district 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.⁷ 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

⁷Districts are assigned to the bin corresponding to their nearest zone; the establishment year of that zone determines post-treatment timing.

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.⁸ I estimate this specification using the approach proposed by de Chaisemartin and 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 Panel A 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 Panel B shows that coefficients remain stable across iterations, with estimates closely aligning with the baseline.

⁸Results are qualitatively similar using only the count of zones, though coefficients are smaller and less precisely estimated.

7 POTENTIAL MECHANISMS

The conceptual framework in Section 3 identifies two primary demand-side channels through which industrial zones may affect school enrollment: income effects and labor market incentives. These channels yield opposing predictions for child labor, providing a direct discriminating test. The framework also identifies supply-side improvements and returns to education as potentially contributing factors, with distinguishable implications for heterogeneity across parental education groups. This section examines each prediction in turn.

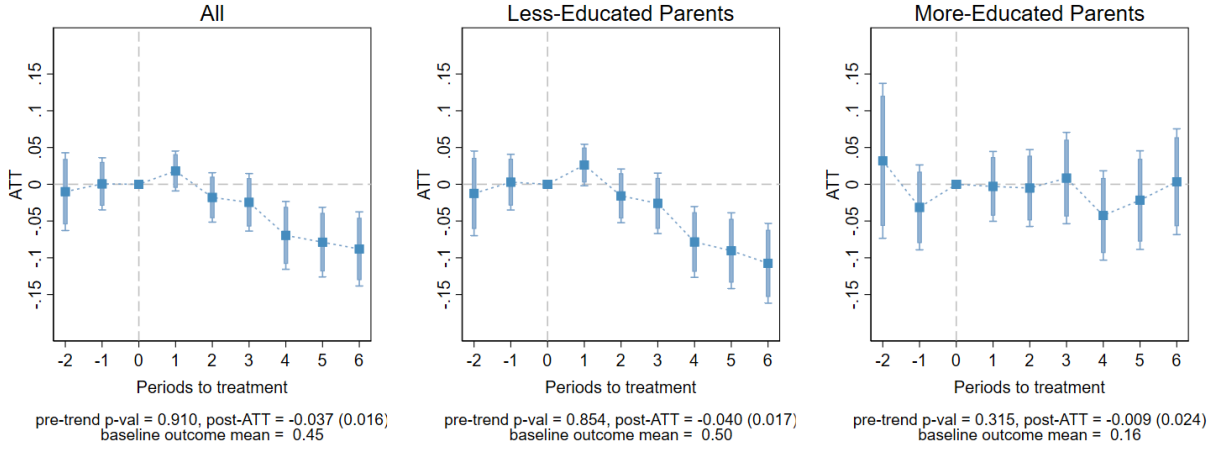
7.1 Income Effects versus Opportunity Costs

The income and opportunity cost channels generate opposing predictions for child labor. If rising household income relaxes budget constraints, children substitute away from work toward schooling, and child labor declines. If, instead, expanded employment opportunities raise the value of children's time, child labor increases and enrollment falls. Figure 7 presents the results.

Following zone establishment, labor participation among children aged 15–18 decreases gradually, reaching approximately 4 percentage points on average in the post-treatment period. 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), consistent with children substituting away from work toward schooling. The difference in magnitudes 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. The decline in child labor rules out the opportunity cost channel as the dominant force and points instead toward income effects.

To examine the income channel directly, I test whether household income and edu-

Figure 7: Industrial Zones and Labor Participation 15–18



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.

cation expenditure increase following zone establishment. Table 4 presents the results, separately 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.

For households with less-educated parents (Panel B), income from informal non-farm activities increases by 4.43 million VND (40% relative to baseline, $p < 0.01$), with a pre-trend p-value of 0.875. This is the income category for which identification is most credible. Other income measures for this group, including agriculture, formal non-agricultural, and total income, show evidence of non-parallel pre-trends, warranting caution in their

causal interpretation. I therefore base the income channel interpretation primarily on informal non-agricultural income, which has clean pre-trends and is consistent with less-educated households benefiting from local demand spillovers generated by zone activity rather than obtaining formal employment within zones directly.

Table 4: Industrial Zones, Income and Education Expenditure

Sector	Income from Wage and Household Business Profits				Expenditure
	Agriculture (1)	Formal Non-Agriculture (2)	Informal Non-Agriculture (3)	All Sectors (4)	Education (5)
Panel A: Sample Includes 15–18 Children of More-Educated Parents					
Post-ATT	-1.589 (1.395)	4.483 (1.890)	-0.176 (1.796)	2.719 (2.180)	0.255 (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 (0.831)	1.778 (0.373)	4.430 (0.799)	4.850 (1.005)	0.313 (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.

Alongside income gains, households increase spending on children’s education. Among children of less-educated parents, education expenditure rises by 0.31 million VND (43%

relative to baseline). The effect for children of more-educated parents is of similar absolute magnitude but smaller relative to a higher baseline, and less precisely estimated. 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 education spending and enrollment.

The differential income patterns across parental education groups are informative about how zones reach different households. More-educated households gain primarily through formal non-agricultural wages, consistent with direct access to skilled employment in or around zones. Less-educated households gain through informal non-agricultural activities, consistent with indirect spillovers to the local economy. That both groups experience income gains, but through different sectors, reinforces the interpretation that zones generate broad-based economic activity that reaches households at different points in the skill distribution through distinct channels.

7.2 Supply-Side Improvements and Returns to Education

The conceptual framework predicts that if supply-side improvements drive the enrollment gains, effects should be broadly similar across parental education groups. The concentration of effects among children of less-educated parents, documented in Figure 5, is therefore itself evidence against a purely supply-side explanation. Columns (1)–(4) of Table 5 provide further evidence: distance to the nearest lower-secondary and upper-secondary school decreases following zone establishment, but these reductions are similar across districts with high and low baseline skill intensity.⁹ Supply-side improvements therefore contribute to enrollment gains but do not vary in a way that can account for the differential enrollment response across districts to be discussed next.

To explore whether returns to education play a complementary role, I examine whether enrollment effects vary with the baseline skill composition of the local labor market.

⁹I proxy for local skill intensity using the share of workers with at least an upper-secondary diploma in 2002 and classify districts above the median as “high-skill.”

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

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

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.

Districts with higher baseline skill shares may attract zones with greater demand for educated workers, making the returns to schooling more visible to local households. Columns (5)–(8) indicate that enrollment effects, particularly for children of less-educated parents, are somewhat larger in high-skill areas, though the difference is not statistically significant at conventional levels.¹⁰ That infrastructure improvements are comparable across skill-intensity groups while enrollment effects are larger in high-skill areas suggests that demand-side forces beyond income, potentially including perceived returns to education, may reinforce the enrollment response. Children of less-educated parents in high-skill districts exhibit the strongest enrollment response, consistent with these households facing both relaxed budget constraints and stronger signals about the value of continued schooling. This evidence is, however, suggestive, as the household survey does not collect data on perceived returns and baseline skill intensity may correlate with other unobserved district characteristics.¹¹

¹⁰Based on stratified clustered bootstrapping at the district level.

¹¹I also examine effects on children’s health insurance coverage and health expenditure (Appendix Table A4). Insurance coverage increases by 3.7 percentage points ($p < 0.05$). Effects on health expenditure are

8 CONCLUSION

This paper examines whether industrial zone expansion increases school enrollment and narrows the educational gap between children of less-educated and more-educated parents. Using a staggered difference-in-differences design with administrative records of industrial zones and nationally representative household surveys from Vietnam spanning 2002–2020, 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, for whom enrollment increases by 7 percentage points relative to a baseline of 53%. Child labor declines in parallel, indicating that children substitute away from work toward schooling rather than being drawn into expanded employment opportunities.

The mechanism evidence points to household income as the primary channel. Less-educated households experience income gains concentrated in informal non-agricultural activities, consistent with spillovers to the local economy rather than direct zone employment, and allocate part of this additional income toward children’s education. Supply-side improvements in school access also contribute, while suggestive evidence indicates that perceived returns to education may play a complementary role in areas with higher baseline skill intensity.

These findings carry several implications. First, they suggest that place-based industrial policies can reduce intergenerational educational inequality, not through direct transfers to households, but through indirect income gains generated by local economic activity. The fact that less-educated households benefit mainly through the informal economy, rather than through formal zone employment, implies that the distributional reach of industrial zones extends beyond the workers they employ directly. Second, the results in-

imprecisely estimated.

dicate that income effects dominate opportunity cost effects in this setting: the expansion of employment opportunities associated with zones does not pull children out of school but instead enables families to invest more in their children's education. This finding is relevant for policy design, as it suggests that the human capital costs of industrialization emphasized in prior work (Atkin, 2016) need not materialize in all settings.

Two caveats are important. First, while I document increases in enrollment and upper-secondary completion, I cannot observe educational quality. If industrial zones generate environmental disamenities that affect child health or cognitive development, the quantity gains documented here could be partially offset. Effects on academic achievement remain an open question for future research. Second, three features of the Vietnamese setting are central to the results: a large informal economy through which spillovers reach less-educated households, binding budget constraints on educational investment at the upper-secondary level, and regulatory linkages between zone development and local infrastructure provision. These features are shared by many developing countries pursuing export-oriented industrialization, including Bangladesh, Cambodia, and Ethiopia, where similar zone programs are expanding rapidly. Whether the specific magnitudes documented here obtain in other contexts is an empirical question, but the underlying mechanism, that informal-sector spillovers from industrial activity can relax household budget constraints on education, is likely not specific to Vietnam.

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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.

Table A2: Robustness Checks: Period 4 School Enrollment Effects

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

	Share of children without long-term registration status in the same commune			Share of children with educated parents
	All (1)	Educated Parents (2)	Less Educated Parents (3)	(4)
Post-ATT	0.003 (0.003)	0.009 (0.004)	0.003 (0.003)	0.014 (0.010)
Mean Outcome	0.01	0.01	0.01	0.17
p-value pre-trend	0.581	0.828	0.636	0.689
Observations	49882	7975	41574	64276
Switcher-Period	837	660	833	1185

Notes: This table shows the effects of industrial zone exposure on proxies for migration, including share of children without long-term registration status by educational level of their parents (Columns 1–3), and share of children with educated parents. The sample includes children 15–18 years old. 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.

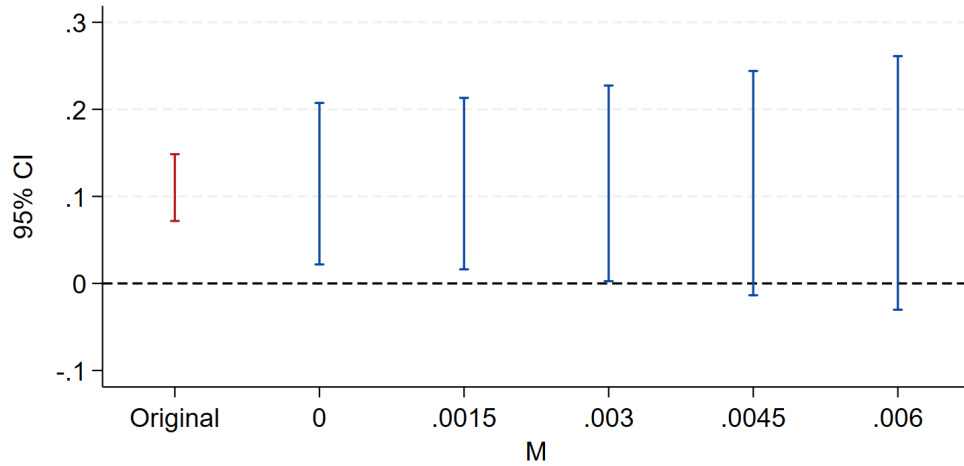
Table A4: Industrial Zones and Health Outcomes

	Insurance Coverage			Health Expenses		
	All (1)	Less-educated Parents (2)	More-educated Parents (3)	All (4)	Less-educated Parents (5)	More-educated Parents (6)
Post-ATT	0.037 (0.018)	0.029 (0.019)	0.057 (0.030)	0.044 (0.048)	0.088 (0.053)	-0.068 (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.

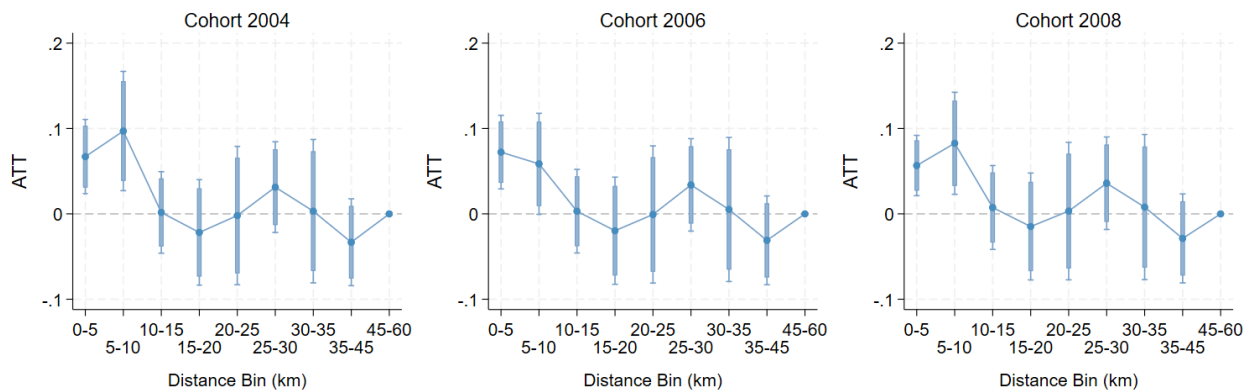
A.2 Appendix Figures

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 $M \approx 0.003$.

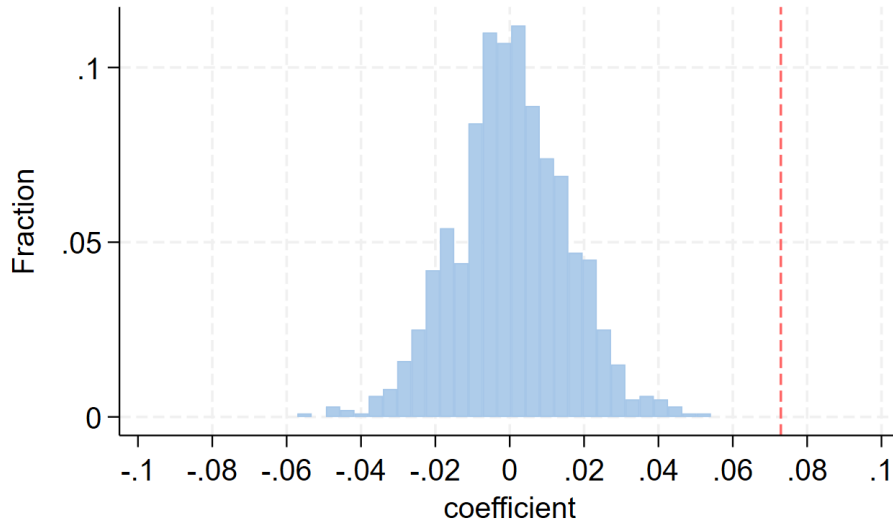
Figure A2: Industrial Zones and School Enrollment by Distance Bin



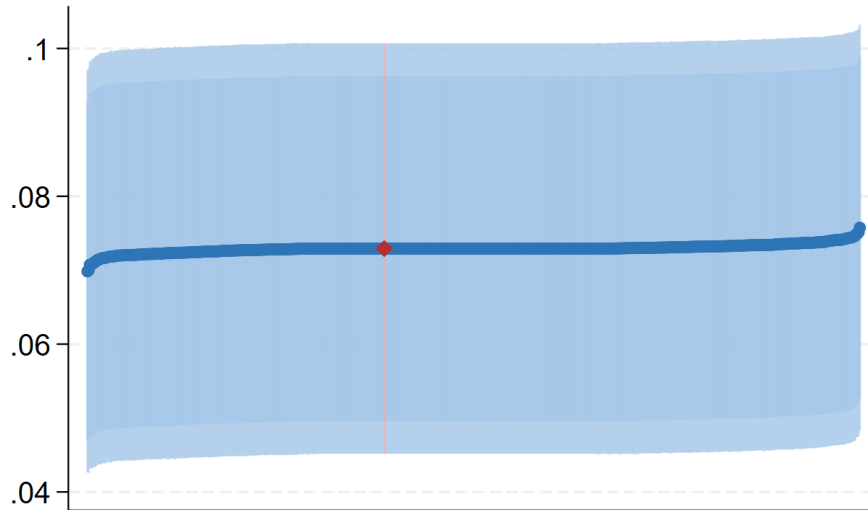
Notes: The outcome is school enrollment of 15–18-year-olds. Thick bars indicate 90% confidence intervals and thin bars with caps indicate 95% confidence intervals. Circle symbols represent estimated coefficients $\hat{\delta}_b$ from equation (2). Sampling weights are applied throughout. Source: Data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

Figure A3: Placebo and Leave-One-Out Tests

Panel A: Monte-Carlo Permutation-based Placebo Test



Panel B: Leave-One-District-Out Test



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.