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Trade Liberalization and Human Capital Investment: Evidence from Vietnam

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Abstract

Exploiting a quasi-natural experiment–the Vietnam-U.S. Bilateral Trade Agreement (BTA)–I investigate the impact of trade liberalization on children's human capital investment in Vietnam. Using regional variation in export tariff uncertainty due to the BTA, I find that children in provinces more exposed to tariff reductions were more likely to engage in work rather than attend school. The effects were more pronounced among boys, older children, rural children, and those with less-educated parents. These negative effects are driven by the rise in child labor, with suggestive evidence of higher local wages in more-exposed provinces. The findings indicate that trade liberalization has increased the opportunity cost of education. These results remain robust across various alternative estimations.

Keywords: Trade liberalization; Human capital; Vietnam.
JEL Classification: F14, F16, J24, O12

1 Introduction

Globalization over recent decades has significantly shaped the economic and social development of most countries, particularly developing nations (Goldberg & Pavcnik 2007). Export-led growth driven by trade liberalization and labor-intensive industrialization has been a common strategy among these countries. Prior studies have primarily examined the impacts of tariff reductions on poverty, returns to skills, and income inequality (Pavcnik 2017).

This paper analyzes the effects of trade liberalization on human capital investment in Vietnam during 2002–2022, exploiting the expansion of exports following the Vietnam–U.S. Bilateral Trade Agreement (BTA) as a quasi-natural experiment. I examine whether greater exposure to trade liberalization raises the likelihood that children aged 10 to 17 drop out of school and enter the labor force, and

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study its impact on school attendance, labor force participation, and the intensity of child labor (working hours per day and days per month).

To measure exposure to trade liberalization, I exploit industry-specific tariff reduction data from the BTA (McCaig 2011a). Following Topalova (2010); McCaig (2011b); McCaig & Pavcnik (2018); Zhao et al. (2021), I construct a provincial measure as the employment-weighted average difference between Column 2 and Most-Favored Nation (MFN) tariffs applied to Vietnam's export industries. Similar measures based on China's Permanent Normal Trade Relations (PNTR) status reduced future tariff uncertainty and influenced human capital decisions (Erten & Leight 2021; Zhao et al. 2021).

I then employ a difference-in-differences (DiD) strategy that exploits variation before and after the BTA across provinces with differing exposure, using 10 waves of the Vietnam Household Living Standards Survey (VHLSS) from 2002–2022. Province and year fixed effects absorb time-invariant heterogeneity, and I control for initial provincial socio-economic characteristics interacted with year dummies to address differential trends. A falsification test using pre-2002 rural data assesses the parallel-trends assumption.

I find that a 10% increase in trade liberalization reduces school attendance by 6.11%, consistent in direction and magnitude with prior studies in Mexico (Atkin 2016) and China (Zhao et al. 2021). The mechanism is an increase in child labor: more children drop out of school to work full-time, and labor intensity rises in working hours and days. Trade liberalization thus creates a substitution effect by raising the opportunity cost of schooling as demand for cheap, low-skilled labor expands.

Heterogeneity analysis indicates that boys, older children (14–17), rural children, and those from households with less-educated heads are more strongly affected. Robustness checks–altering the trade-liberalization measure, restricting the sample age range, excluding merged provinces, and a falsification test–support the baseline results. When the exposure measure includes non-tradable sectors, the effects are even larger, suggesting general-equilibrium spillovers into services.

This paper builds on McCaig et al. (2022), who study the BTA and children's human capital over a shorter horizon, and extends their work in three ways. First, the window through 2022 estimates the effect over a longer and more recent horizon than prior work. Second, beyond the *incidence* of child labor I measure its *intensity*–working hours and days–showing that exposure raises not only whether children work but how much. Third, I examine the local wage-premium and provincial income channels directly; the income channel shows no offsetting effect. Where my estimates differ in magnitude, the gap is consistent with my use of tradable-only employment weights and a longer, more recent panel.

The remainder of the paper is organized as follows. Section 2 reviews the BTA and the conceptual framework. Section 3 describes the data and the trade-shock measure. Section 4 presents the empirical strategy and results. Section 5 concludes.

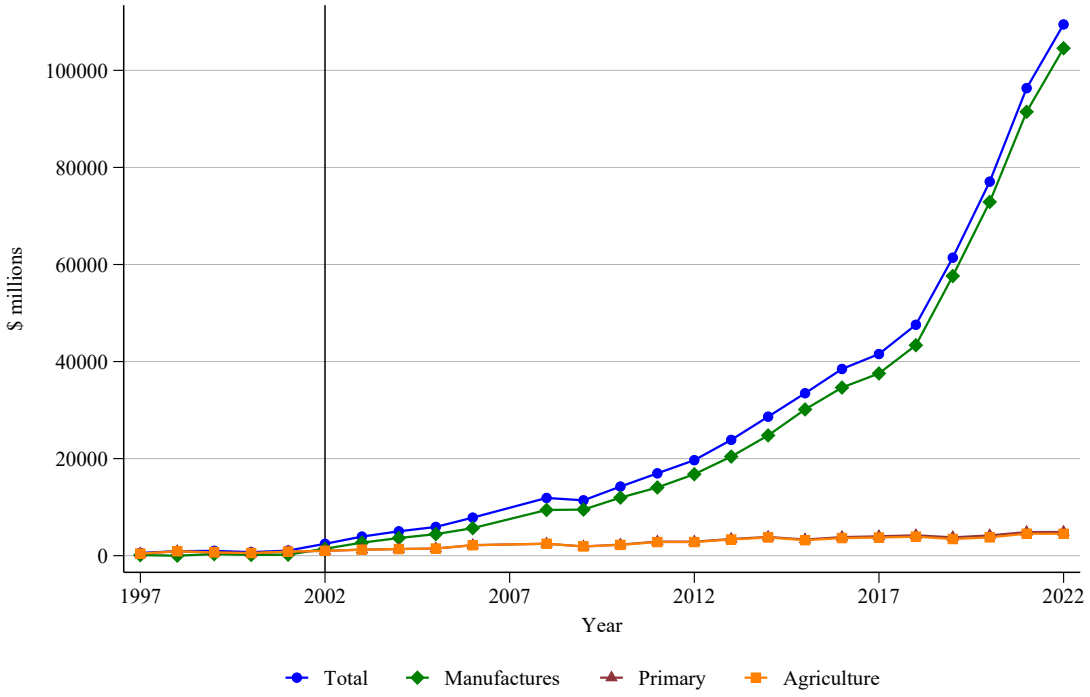
2 Background and Conceptual Framework

2.1 The Vietnam-U.S. Bilateral Trade Agreement

Vietnam's economic and political reforms began in 1986 with the "Doi moi" policy, transitioning from a centrally planned economy toward a socialist-oriented market economy (Riedel & Comer 1997). The globalization wave and surge in international trade in the early 1990s accelerated this transformation, driving a series of domestic changes in subsequent years. Notably, legal reforms such as the Private Enterprise Law and the Enterprise Law recognized private and foreign investments as integral components of the economy within the legal framework (Phan & Coxhead 2013). Vietnam gradually integrated into the global economy and international trade markets, starting with the removal of rice import and export quotas in 1989 (Minot & Goletti 1998). In 1995, Vietnam became a member of the Association of Southeast Asian Nations (ASEAN) and signed the ASEAN Free Trade Agreement (AFTA). However, the era of significant trade liberalization truly began in the early 2000s with the Vietnam–U.S. Bilateral Trade Agreement.

Under the BTA, the United States granted Vietnam Most Favored Nation (MFN) status, providing access to the world's largest consumer market with minimal trade discrimination. Fukase (2013) highlights that although the U.S. lifted its trade embargo on Vietnam in 1994, many Vietnamese goods exported to the U.S. before the signing of the BTA in 2002 were subject to prohibitively high tariffs designed to deter imports. Consequently, prior to the BTA, Vietnam primarily exported agricultural and aquatic products such as shrimp and coffee, as well as raw materials like crude oil, since these items faced near-zero tariff rates. Figure 3 shows that tariff reductions were concentrated in manufacturing sectors (ISIC 15–36), with the largest reductions in industries such as tobacco (ISIC 16), textiles (ISIC 17), and apparel (ISIC 18); in contrast, tariff reductions for mining (ISIC 10–14) and agriculture (ISIC 1, 2, 5) were significantly smaller. Fukase (2013) further documents that Vietnam's exports to the U.S. concentrated in labor-intensive sectors such as apparel, textiles, leather goods, furniture, and components. Figure 1 confirms an increasing trend in the total value of exports from Vietnam to the U.S. across various categories, including industrial goods, essential goods, and agricultural products. Industrial, processed, and manufactured goods dominated Vietnam's exports to the U.S. after 2002, while exports of essential goods and agricultural products increased only slightly, remaining almost negligible over the years.

Figure 1: Vietnam's exports to the United States from 1997 to 2022 (in $ million)



A noteworthy feature of the BTA is that it was implemented by immediately switching Vietnam's export tariffs from Column 2 to MFN tariffs. According to McCaig (2011b), the BTA's tariff structure operated as an "all-or-nothing" package, providing a sharp source of cross-industry variation. I therefore exploit this variation in tariff reductions as a trade shock to examine the impact of trade liberalization on human capital investment in Vietnam following the implementation of the BTA, similar to previous studies (McCaig 2011b; McCaig & Pavcnik 2018; Fukase 2013; McCaig & Pavcnik 2015; Ngoc et al. 2024).

2.2 Conceptual Framework

The theoretical literature offers two complementary approaches linking trade liberalization to human capital investment. The first treats education as a consumption good (Haley 1973): households trade off human capital investment against other consumables, with the household's utility function reflecting this trade-off. If trade liberalization raises household income–through expanded business and employment opportunities, especially in export-oriented industrial sectors–households would consume more of the good and invest more in their children's education (Findlay & Kierzkowski 1983; Goldin & Katz 1997). Findlay & Kierzkowski (1983) formalize this idea in a Heckscher-Ohlin model with two production factors and two goods, in which workers choose between remaining unskilled and investing in human capital, and the wages of skilled and unskilled workers and the indirect costs of human capital investment are endogenous. However, when schooling is not viewed as a consumable good due to the time costs of attending school (Lazear 1977), trade liberalization may instead reduce investment

via an income effect: Edmonds et al. (2009); Edmonds (2010) show that Indian parents had less incentive to invest in their children's education when exposed to relative poverty from the removal of protectionist tariffs.

The second approach treats education as an investment yielding long-term returns (Becker 1962, 1964). The schooling decision then rests on a cost-benefit comparison between short-term costs (e.g., tuition, transportation, and the opportunity cost of foregone labor income) and long-term benefits (better job prospects and higher wages). Trade liberalization could affect two distinct groups: those with low human capital, for whom rising opportunity costs of schooling pull them into the labor market, and those with high human capital, for whom new high-skill opportunities may also raise labor-market participation. The two groups can therefore respond very differently to the same trade shock.

In Vietnam during 2002–2022, trade liberalization mainly affected labor-intensive, low-skill industries. This paper focuses on children aged 10–17, who typically have low human capital and have not yet completed high school, so the second channel–rising opportunity costs of schooling–is the most relevant. The empirical literature provides supporting evidence: Atkin (2016) finds Mexican youths dropped out of school in response to export-oriented labor demand in labor-intensive industries; Lin & Long (2020) documents that trade liberalization reduced years of schooling among children in China; and Zhao et al. (2021) report that the U.S.–China PNTR shock increased child labor in China. Pavcnik (2017) stresses that such effects should be studied within the specific context of each country and policy, motivating direct evidence on Vietnam through the BTA.

To empirically distinguish between the two theories in this context, I examine three mechanisms: (i) child labor incidence and intensity, (ii) income effects through provincial revenues and education expenditures, and (iii) wage premia in local labor markets. Specifically, if an income effect dominates, more-exposed provinces should display higher local revenues and education spending, encouraging children to stay in school. Conversely, if rising opportunity costs dominate, child labor and local wages should rise with exposure, pulling children out of school and into the labor market. Examining all three channels jointly allows the data to speak to which theoretical mechanism is empirically operative in Vietnam during this period.

3 Data and Methodology

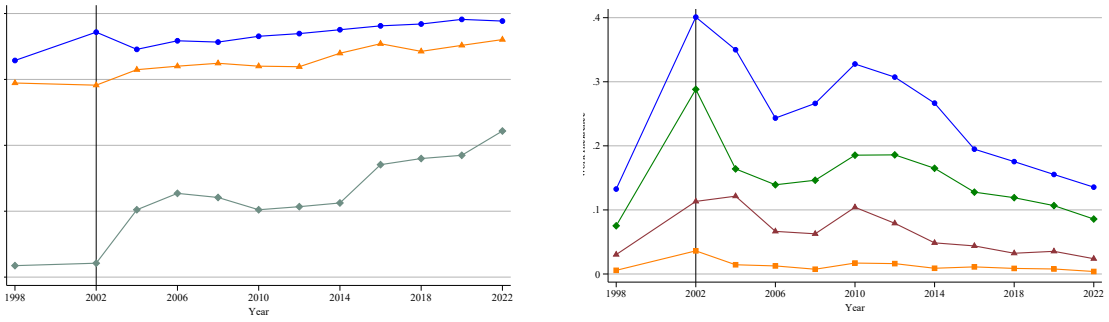
3.1 Data

The primary analysis uses the Vietnam Household Living Standards Survey (VHLSS), tariff data from the BTA, and the 1999 Vietnam Population and Housing Census (VPHC).

3.1.1 Vietnam Household Living Standards Survey (VHLSS)

The VHLSS is a provincially representative household survey conducted by the General Statistics Office (GSO) of Vietnam with technical support from the World Bank. Initiated in 1993, the survey has been conducted biennially since 2002 and provides extensive information on demographics, education, and employment. I use 10 rounds spanning 2002–2022, focusing on children aged 10–17 surveyed in each round. The survey collects employment information only for household members aged 10 and above. According to the 2019 Labor Code, children are allowed to join the labor market at the age of 15, but many children under 10 are still involved in informal household activities, such as agricultural and non-agricultural businesses (Figure 2). Each round surveys approximately 45,000 households, with over 9,000 of them providing data on both income and expenditure; the 2002 survey covered more than 75,000 households, of which over 30,000 reported both income and expenditure data. Surveys conducted before 2010 can be linked to form panel datasets (e.g., 2002–2004–2006 or 2004–2006–2008), but starting in 2010 the GSO adopted a new sampling frame based on the 2009 Population and Housing Census, ruling out panel construction with the 2010 round and subsequent waves.

Figure 2: School Attendance and Child Labor Incidence from 1998 to 2022



a. School Attendance b. Child Labor Incidence

To assess the impact of trade liberalization on human capital investment in Vietnamese children between 2002 and 2022–20 years after the BTA–the main outcome derived from VHLSS is *school attendance*, equal to 1 if the child is currently enrolled in school or attended school in the past 12 months and 0 otherwise. Because VHLSS data are collected quarterly, some children may report not attending school during the summer break; however, the data include reasons for non-enrollment, enabling the construction of a 12-month attendance variable. This approach enhances the reliability of the estimates, as dropout decisions are unlikely to occur abruptly at the time of the survey.

A second key variable is *child labor*, defined as whether a child is engaged in work. According to the International Labour Organization (ILO, 2017), child labor excludes light work permitted for children above the legal minimum working age and includes activities harmful to a child's physical and mental health. Because VHLSS does not consistently capture children's involvement in household chores, I focus on market-based child labor: a child is coded as working if they had a job or participated in household agricultural or non-agricultural businesses

in the past 12 months. Following Edmonds et al. (2010), I consider two scenarios: (i) Work–children who attend school but also work coded as 1, and 0 otherwise; and (ii) Work Only–children who do not attend school but work coded as 1, with those attending school exclusively coded as 0. To capture the intensity of child labor, I additionally use daily working hours and monthly working days, which allows me to test whether trade liberalization affects not only the incidence of child labor but also its intensity.

Several limitations of VHLSS data are worth noting. First, VHLSS lacks information on migration, including child migration. Because VHLSS collects data based on household registration, individuals who have migrated to another locality for work or study may still be recorded at their household's registered location. This mismatch could in principle bias estimations of trade liberalization impacts: a child impacted by trade liberalization in Province B may migrate to Province A for work, leading to overestimation of the effect in Province B and underestimation in Province A. Following McCaig et al. (2022), I assume minimal inter-provincial migration due to Vietnam's household registration system, treating each province as a separate labor market. Official data support this assumption, with inter-provincial migration rates of 2.5%, 2.9%, and 4.3% in 1989, 1999, and 2009, respectively (Kim Anh et al. 2012). Moreover, because the VHLSS is collected at the household's registered place of residence, children who have moved are still recorded in their province of registration, so each child is assigned the trade exposure of the labor market to which they remain administratively tied. As a back-of-the-envelope bound, migration could fully account for the baseline only if differential out-migration of school-leavers from high-exposure provinces reached about 5% of their school-age population–above the all-age inter-provincial migration rate (2.5–4.3%), and teenagers migrate well below that rate (McCaig & Pavcnik 2018). Second, since pre-BTA data are limited, a standard parallel-trends test is infeasible. Although Vietnam conducted Vietnam Living Standards Surveys (VLSS) in 1992–1993 and 1997–1998, these datasets lack provincial representativeness and include relatively few households (about 6,000), and the VLSS omitted households from certain provinces (including Lai Chau and Bac Kan) and urban areas in over half of the provinces. As a result, VLSS data cannot be used for a standard parallel-trend test in this paper. Instead, I control for pre-BTA socio-economic characteristics across provinces and use the 1998 rural VLSS sample to conduct a falsification test.

3.1.2 Tariff Data

U.S. tariff data on imports from Vietnam come from McCaig (2011a) and include Column 2 (pre-BTA) and MFN (post-BTA) tariffs weighted by industry import shares in 2001. Industries are classified under ISIC Rev.3 at the 3-digit level, covering agriculture (ISIC 1, 2, 5), mining (ISIC 10–14), manufacturing (ISIC 15–37), and selected services (ISIC 40, 74, 92, 93). Table 8 reports tariff changes by sector.

3.1.3 Vietnam Population and Housing Census 1999

I use a 3% IPUMS sample from the 1999 VPHC to compute industry-level employment for each province at the ISIC Rev.3 3-digit level. Following McCaig (2011b), these employment shares weight industry-level tariff cuts to construct provincial trade-liberalization exposure. Table 1 reports descriptive statistics.

Table 1: Summary Statistics

Variable	N	Mean	SD	Min	Max
Panel A: Tariff Gap by Province					
Tradable industries only	60	0.102	0.037	0.075	0.325
All industries	60	0.080	0.014	0.062	0.148
Panel B: Provincial Initial Conditions					
Rural	60	0.782	0.151	0.190	0.958
Minority	60	0.204	0.285	0.000	0.962
Manufacturing	60	0.142	0.086	0.009	0.391
Panel C: Individual's outcomes					
School attendance	86,750	0.869	0.337	0	1
Work	86,750	0.178	0.382	0	1
Work only	86,750	0.100	0.300	0	1
Working hours (per day)	69,333	1.130	2.470	0	16
Working days (per month)	69,333	3.770	8.130	0	30
Panel D: Individual and household characteristics					
Age	86,750	13.535	2.279	0	17
Sex (Male = 1)	86,750	0.524	0.499	0	1
Ethnic (Kinh = 1)	86,750	0.820	0.384	0	1
Household size	86,750	4.944	1.528	2	18
Urban	86,750	0.268	0.443	0	1
Head's sex (Male = 1)	86,750	0.832	0.374	0	1
Head's age	86,750	44.698	9.336	15	98
Head's education					
No education	86,750	0.176	0.381	0	1
Primary	86,750	0.293	0.455	0	1
Secondary	86,750	0.318	0.466	0	1
High school	86,750	0.156	0.363	0	1
University and above	86,750	0.056	0.231	0	1

Notes: This table presents summary statistics for the 2002–2022 VHLSS sample. Questions regarding the number of working hours and days were not asked in 2022; therefore the working hours and working days variables are based on 69,333 observations from 2002–2020.

3.2 Measuring the Trade Shocks

I construct a provincial index of exposure to BTA tariff reductions for 60 provinces, weighted by the pre-BTA employment structure in tradable sectors (ISIC Rev.3 2-digit codes 1–36, comprising agriculture, forestry, fisheries, mining, and manufacturing), following Edmonds et al. (2010) for India and Asghar & McCaig (2024) for Vietnam. Each province is treated as a distinct labor market under the assumption of negligible inter-provincial mobility. The provincial tariff cut is:

$$\text{Tariff Cuts}_p = \sum_{j=1}^{N} \left(\frac{L_{jp}}{\sum_{j=1}^{N} L_{jp}} \times \text{Tariff Cut}_j \right) \tag{1}$$

where j represents the ISIC Rev.3 3-digit industry, p denotes the province. L_{jp} is the number of workers in industry j in province p, $\sum_{j=1}^{N} L_{jp}$ is the total workforce in province p, and Tariff Cut$_j$ is the tariff reduction for industry j, calculated prior to the BTA using the following:

$$\text{Tariff Cut}_j = \text{Column 2 Tariff}_j - \text{MFN Tariff}_j \tag{2}$$

where Column 2 tariffs are pre-BTA and MFN tariffs are post-BTA; Tariff Cut$_j$ is always positive. Figure 5 maps provincial exposure. Following Kovak (2013), employment weights are computed using only tradable sectors to capture general-equilibrium spillovers into non-tradables.

Panel A of Table 1 reports descriptive statistics for both measures. The primary analysis uses Tariff Cut 1 (tradable-only weights); Tariff Cut 2 (all sectors) is reserved for robustness. The two measures are highly correlated ($\rho = 0.885$; Figure 4), with average reductions of 0.102 and 0.080. Ho Chi Minh City has the highest exposure and Ca Mau the lowest. Both measures are expressed as shares (proportions between 0 and 1): the average Tariff Cut 1 of 0.102 corresponds to a 10.2-percentage-point reduction in the average tariff faced by a province's workers. Accordingly, each coefficient on Tariff Cut reported below measures the effect of moving the index across its full 0-to-1 range, so that a 1-percentage-point (0.01) increase in exposure changes the outcome by 0.01 times the coefficient, and a 10-percentage-point increase–roughly the average province's exposure–by 0.10 times the coefficient.

4 Empirical Analysis

4.1 Baseline Specification

I estimate the impact of the BTA on human capital investment using a difference-in-differences (DiD) approach. The 2002 survey collects 12-month retrospective information; although this window can reach into the BTA's December 2001 entry into force, it is the earliest available wave and serves as the pre-BTA baseline, with children in 2002 as the control group. The 1998 pre-period reported in Section 4.4 confirms that this timing does not drive the result. The baseline model is:

$$Y_{ipt} = \beta_1 \times \text{Post}_t \times \text{Tariff Cut}_p + \theta X_{it} + \mu_p + \phi_t + \epsilon_{ipt} \quad (3)$$

where Y_{ipt} is the outcome for individual i in province p at time t; Post_t equals 1 for survey years from 2004 onward; X_{it} is a vector of individual and household controls (age and age squared, gender, household head's age, age squared, gender, and education, urban dummy, Kinh ethnicity, household size, and year-of-birth dummies); μ_p and ϕ_t are province and year fixed effects; and ϵ_{ipt} is the error term clustered at the province level.

Pre-BTA data limitations preclude a direct test of parallel trends. To mitigate the concern that pre-existing socio-economic differences (e.g., coastal vs. mountainous provinces) may bias the BTA effect, I add initial provincial characteristics measured pre-BTA from the 2002 VHLSS–shares of manufacturing employment, ethnic minorities, and rural population–following McCaig et al. (2022). Because Tariff Cut is a continuous, province-level dose, the interaction coefficient should be read as an average partial effect across the exposure distribution rather than a single treatment effect, an interpretation that is reasonable here given the relatively homogeneous, labor-intensive nature of the BTA shock (Callaway et al. 2024):

$$Y_{ipt} = \beta_1 \times \text{Post}_t \times \text{Tariff Cut}_p + \theta X_{it} + \tau_t W_p + \mu_p + \phi_t + \epsilon_{ipt} \quad (4)$$

where W_p is a vector of pre-BTA provincial characteristics.

Table 2 reports the baseline results, with school attendance as the outcome. Column (1) includes only province and year fixed effects; Column (2) adds individual and household controls; Column (3) further includes pre-BTA provincial characteristics. The coefficient on $\text{Post}_t \times \text{Tariff Cut}_p$ is negative and statistically significant in all specifications and grows in absolute magnitude when controls are added. This pattern indicates that observed and initial provincial characteristics are correlated with both school attendance and trade exposure, and omitting them biases the estimated effect toward zero. Provinces with greater exposure to trade liberalization therefore experience a larger reduction in school attendance among children once these correlations are accounted for.

To probe the exogeneity of exposure more directly, Appendix Table 10 compares pre-BTA (2002) characteristics across the top and bottom exposure quartiles. The more-exposed provinces differ in initial structure–they are more urban, more ethnically Kinh, and more manufacturing-intensive–which is precisely why the specification controls for these characteristics and their interactions with year. Reassuringly, pre-BTA *school attendance* itself is statistically indistinguishable across the two groups (0.812 versus 0.813, $p = 0.96$), so the post-BTA divergence in schooling does not simply extend a pre-existing gap. Appendix Figure 7 shows the same with the 1999 Census: attendance-by-age profiles for the top and bottom exposure quartiles nearly coincide over ages 10–17, so the two groups shared a common pre-BTA trajectory. The estimate is also not driven by a single high-exposure outlier: excluding Ho Chi Minh City–the most-exposed province–leaves the coefficient at -0.638 (s.e. 0.357). Because identification necessarily comes from the more-exposed provinces, where the BTA shock was concentrated,

the coefficient is best read as the average response over that range rather than a uniform effect across the entire exposure distribution.

Table 2: Baseline Results

	(1) School Attendance	(2) School Attendance	(3) School Attendance
Post x Tariff Gap	-0.254^{c}	-0.317^{c}	-0.531^{b}
	(0.09)	(0.08)	(0.21)
Age		0.117^{c}	0.117^{c}
		(0.01)	(0.01)
Age squared		-0.005^{c}	-0.005^{c}
		(0.00)	(0.00)
Sex (Male = 1)		-0.007^{a}	-0.007^{a}
		(0.00)	(0.00)
Ethnic (Kinh = 1)		0.048^{c}	0.048^{c}
		(0.01)	(0.01)
Household size		-0.013^{c}	-0.013^{c}
		(0.00)	(0.00)
Urban		0.023^{c}	0.024^{c}
		(0.00)	(0.00)
Head's age		-0.002^{b}	-0.002^{b}
		(0.00)	(0.00)
Head's age squared		0.000^{c}	0.000^{c}
		(0.00)	(0.00)
Head's sex (Male = 1)		0.011^{c}	0.012^{c}
		(0.00)	(0.00)
Head's education (Base = no education)			
Primary		0.096^{c}	0.096^{c}
		(0.01)	(0.01)
Secondary		0.155^{c}	0.155^{c}
		(0.01)	(0.01)
High school		0.194^{c}	0.193^{c}
		(0.01)	(0.01)
University and above		0.191^{c}	0.191^{c}
		(0.01)	(0.01)
Constant	0.888^{c}	0.174^{c}	0.210^{c}
	(0.01)	(0.06)	(0.07)
Province FE, Year FE	Yes	Yes	Yes
Individual & Household Controls	No	Yes	Yes
Provincial Initial Conditions	No	No	Yes
Observations	86,750	86,750	86,750
Adjusted R^2	0.157	0.204	0.205

Notes: This table presents the baseline results of the impact of trade liberalization on children's human capital. Column (2) adds individual and household controls, including age, age squared, sex, ethnic majority (Kinh) dummy, household size, urban dummy, head's age, head's age squared, head's sex and head's education. Column (3) adds time-specific provincial initial conditions in 2002, including proportion of rural population, proportion of minority population and proportion of employment in manufacturing. Standard errors are clustered at province level. [a]p<0.1; [b]p<0.05; [c]p<0.01.

The control variables behave as in the prior literature: school attendance is nonlinear in child age, higher for girls than boys (consistent with boys leaving school more readily for low-skilled work), increasing in the household head's education, lower in larger households, and higher in urban areas.

Quantitatively, the Column (2) coefficient of -0.317 implies that a 1 percentage-point increase in tariff reduction lowers school attendance by 0.317 percentage points; the Column (3) coefficient of -0.531 is consistent in sign and magnitude with Edmonds et al. (2010) for India, where coefficients ranged from -0.360 to -0.620. Compared with the average school attendance rate of 0.869, a province with average exposure ($\approx 10\%$, i.e., 0.10) sees a 5.31 percentage-point reduction (0.531×0.10), equivalent to about 6.11% ($5.31 \div 86.9$).

4.2 Alternative Mechanism

4.2.1 Child Labor

The decline in school attendance in more-exposed provinces likely reflects rising demand for low-skilled labor in manufacturing and export sectors, raising the opportunity cost of schooling and pulling children into work as low-skilled substitutes for adult labor (Basu & Van 1998). I examine this mechanism using Equation (4) with child-labor outcomes.

Table 3 reports results for four outcomes: (1) a Work indicator; (2) a Work-Only indicator (works and does not attend school); (3) average daily working hours over the past 12 months; and (4) average monthly working days, both set to 0 for non-working children. All coefficients are positive: more-exposed provinces have significantly higher child labor incidence (significant at the 10% level for (1)–(2) and at the 1% level for (3)–(4)). Control variables behave as in the baseline: child labor rises with age, is higher for boys, and is lower with more-educated household heads.

In Column (1), the coefficient of 0.421 implies that a 1 percentage-point increase in exposure raises the child-labor rate by 0.421 percentage points; compared with the average child-labor rate of 0.178, a province with average exposure ($\approx 10\%$, i.e., 0.10) experiences a 4.21 percentage-point increase in child labor for children aged 10–17 (0.421×0.10), equivalent to about 23.65% ($4.21 \div 17.8$). For labor intensity, the Column (3) coefficient of 3.531 means that moving the exposure index across its full 0-to-1 range raises average daily working hours by 3.531; equivalently, a province with average exposure ($\approx 10\%$, i.e., 0.10) sees children aged 10–17 work about 0.35 more hours per day (3.531×0.10) over the past 12 months than those in an unexposed province. Table 6 reports how these child-labor responses vary across subgroups by gender, age, urban/rural residence, and household-head education.

These findings contrast with Edmonds & Pavcnik (2005) and Kis-Katos & Sparrow (2011), who found that trade liberalization *reduced* child labor by raising household income; both studies focused on 1990s agricultural integration. In contrast, the BTA shock concentrates on labor-intensive manufacturing (textiles, garments, footwear), creating low-skill jobs that reduce the returns to higher ed-

ucation. This is consistent with McCaig (2011b), who finds short- and medium-term declines in school attendance and wage gains for primary-educated workers, and with Fukase (2013), who documents a narrowing skill wage gap. The next subsections examine income and wage-premium channels.

Table 3: Mechanism: Child Labor

	(1) Work	(2) Work Only	(3) Working Hours	(4) Working Days
Post x Tariff Gap	0.421^a	0.326^a	3.531^c	11.371^c
	(0.21)	(0.18)	(1.12)	(4.00)
Age	-0.052^c	-0.114^c	-0.690^c	-1.640^c
	(0.01)	(0.01)	(0.06)	(0.22)
Age squared	0.003^c	0.005^c	0.031^c	0.075^c
	(0.00)	(0.00)	(0.00)	(0.01)
Sex (Male = 1)	0.011^b	0.008^b	0.057^a	0.038
	(0.00)	(0.00)	(0.03)	(0.09)
Ethnic (Kinh = 1)	-0.109^c	-0.051^c	-0.601^c	-1.623^c
	(0.01)	(0.01)	(0.06)	(0.21)
Household size	0.011^c	0.011^c	0.074^c	0.224^c
	(0.00)	(0.00)	(0.01)	(0.03)
Urban	-0.062^c	-0.028^c	-0.338^c	-1.184^c
	(0.01)	(0.00)	(0.04)	(0.13)
Head's age	0.001	0.002^b	0.004	0.021
	(0.00)	(0.00)	(0.01)	(0.02)
Head's age squared	-0.000^a	-0.000^c	-0.000^a	-0.000^b
	(0.00)	(0.00)	(0.00)	(0.00)
Head's sex (Male = 1)	0.006	-0.003	0.018	0.123
	(0.00)	(0.00)	(0.03)	(0.10)
Head's education (Base = no education)				
Primary	-0.074^c	-0.072^c	-0.547^c	-1.678^c
	(0.01)	(0.00)	(0.04)	(0.13)
Secondary	-0.128^c	-0.121^c	-0.939^c	-2.839^c
	(0.01)	(0.00)	(0.04)	(0.14)
High school	-0.179^c	-0.152^c	-1.304^c	-4.116^c
	(0.01)	(0.01)	(0.05)	(0.16)
University and above	-0.168^c	-0.136^c	-1.315^c	-4.118^c
	(0.01)	(0.01)	(0.06)	(0.20)
Constant	0.547^c	0.780^c	5.485^c	14.780^c
	(0.09)	(0.06)	(0.50)	(1.86)
Observations	86,750	86,750	69,333	69,333
Adjusted R^2	0.236	0.198	0.224	0.214

Note: This table exploits the child labor mechanism of the effects. All specifications control for individual and household characteristics, provincial initial conditions, province fixed effects, year fixed effects, and birth cohort dummies. Standard errors are clustered at province level. [a]p<0.1; [b]p<0.05; [c]p<0.01.

4.2.2 Income Channel

If human capital investment is treated as a normal good, BTA-induced income gains should raise investment in education through expanded job opportunities for parents. To test whether this income channel is empirically operative, I follow Leight & Pan (2024); Lu et al. (2023) and regress provincial budget revenue, total expenditure, and education expenditure on the BTA tariff-cut measure using Equation (4). Using provincial budgets instead of household income avoids well-known endogeneity problems: because the VHLSS is collected quarterly, household income and spending can be heavily influenced by seasonal factors such as holiday spending and rural agricultural cycles. Provincial-budget aggregates smooth out this noise, and they should track household income changes if the income channel is operative, since personal income tax and household consumption taxes both feed into provincial revenue. Increases in provincial revenue would then translate into higher local government spending on education through budget allocations.

I use data on provincial budgets and expenditure structures during 2000–2020, provided by the GSO. Table 4 reports the regression results from Equation (4): Column (1) uses log total provincial budget revenue, Column (2) uses log total provincial budget expenditure, and Column (3) uses log provincial budget expenditure on education. All specifications include province and year fixed effects and control for initial socio-economic factors of provinces calculated from the 1999 VPHC.

Table 4: Mechanism: Income Effect

	(1) ln(Total Fiscal Revenue)	(2) ln(Total Fiscal Expenditure)	(3) ln(Educational Expenditure)
Post × Tariff Cut	5.918	7.407	8.085
	(5.82)	(6.62)	(6.61)
Constant	6.791^{c}	6.227^{a}	8.677^{c}
	(1.74)	(3.30)	(2.08)
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Province initial conditions	Yes	Yes	Yes
Observations	1,310	1,310	1,310
Adjusted R^2	0.936	0.851	0.924

Notes: This table exploits the income effect channel mechanism. Standard errors are clustered at province level. [a]p<0.1; [b]p<0.05; [c]p<0.01.

The Post × Tariff Cut coefficient is positive across all three outcomes but never statistically significant, indicating that BTA-induced income gains do not translate into systematically higher provincial revenues or education spending. The income channel therefore does not appear to drive the BTA's negative effect on schooling, consistent with Adukia et al. (2020); Lu et al. (2023), who similarly find limited income effects of trade liberalization on human capital. A plausible explanation is that more-exposed provinces–hosting industrial parks, export-

processing zones, and labor-intensive manufacturing plants–tend to have lower average per-capita income to begin with, so any modest income gains in these provinces may be small relative to the rising opportunity cost of schooling, leaving the net effect on human capital negative.

4.2.3 Wage Premium

An alternative channel is a wage premium: firms in provinces more exposed to trade liberalization, facing rising production and export opportunities, may bid up wages to attract labor, raising the short-run opportunity cost of schooling for children. This surge in local wages can also explain the rise in school dropouts and child labor participation if the opportunity cost of attending school increases sharply in the short term.

Following Lu et al. (2023), I use the GSO Enterprise Census (2000–2014). The dependent variable is the log of average employee compensation per worker, and the model mirrors Equation (4) with the key coefficient on Post $\times$ Tariff Cut. In place of the individual- and household-level controls, I include firm-level controls–log employment, log capital, and log productivity (TFP, following Wooldridge (2009)), export and import dummies, and ownership type–with industry and year fixed effects; Column (3) additionally includes firm fixed effects.

Table 5 presents the results, with standard errors clustered at the province level throughout. All three columns are estimated on the same sample (the most demanding Column 3 specification), so that the coefficients differ only because of the specification. In Columns (1) and (2), firms in more-exposed provinces pay significantly higher wages: the coefficient on Post $\times$ Tariff Cut is 1.802 and 1.931 (both significant at the 1% level) and is stable when firm-level controls are added. Because Tariff Cut is a 0–1 share, the Column (2) estimate implies that a 10-percentage-point increase in provincial exposure is associated with about 0.19 log points–roughly 21%–higher wages (1.931×0.10). This cross-sectional premium is, however, not robust to firm fixed effects: in Column (3) the coefficient falls to 0.380 and is statistically insignificant. The premium therefore reflects differences *across* firms and provinces–more-exposed provinces host higher-paying firms–rather than within-firm wage growth, so we read the wage evidence as suggestive rather than conclusive. A back-of-the-envelope check reinforces this caution: the Column (2) coefficient implies a top-minus-bottom exposure-quartile wage gap of about 0.27 log points, whereas the raw difference-in-differences in average enterprise wages across these quartiles is only about 0.05, confirming that the coefficient largely captures permanent province wage differences rather than BTA-induced wage growth.

Table 5: Mechanism: Wage Premium

	(1) ln(Wage)	(2) ln(Wage)	(3) ln(Wage)
Post × Tariff Cut	1.802^{***}	1.931^{***}	0.380
	(0.089)	(0.089)	(0.517)
ln(Employment)		-0.008	-0.086^{***}
		(0.009)	(0.005)
ln(Capital)		0.053^{***}	0.061^{***}
		(0.003)	(0.007)
ln(Productivity)		0.092^{***}	0.091^{***}
		(0.012)	(0.010)
Export		-0.027^{*}	0.069^{***}
		(0.015)	(0.017)
Import		0.107^{***}	0.058^{**}
		(0.016)	(0.028)
Constant	2.916^{***}	2.343^{***}	2.467^{***}
	(0.026)	(0.091)	(0.076)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm & ownership controls	No	Yes	Yes
Firm FE	No	No	Yes
Observations	1,840,776	1,840,776	1,840,776
Adjusted R^2	0.441	0.507	0.588

Notes: The dependent variable is the log of average employee compensation per worker (employee income / average employment) from the Vietnam Enterprise Census, 2000–2014. All three columns are estimated on the same sample (the most demanding Column 3 specification). Column (1) includes year and industry (2-digit ISIC) fixed effects; Column (2) adds firm-level controls (log employment, log capital, log productivity, export and import dummies, and ownership type); Column (3) adds firm fixed effects. Standard errors are clustered at the province level throughout and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

In provinces with greater exposure, the presence of higher-paying firms raises the local opportunity cost of schooling, creating incentives for children to leave school for the labor market; this cross-sectional pattern is consistent with Fukase (2013), who finds that the BTA narrowed the high-skill/low-skill wage gap in Vietnam. Because the premium does not survive firm fixed effects, however, we treat this channel as suggestive. Taken together, the mechanism tests point to an opportunity-cost channel: BTA exposure raised local demand for low-skilled labor–visible in higher child-labor incidence and intensity and in higher local wages across firms–while the income channel shows no offsetting rise in provincial revenue or education spending, leaving the net effect on schooling negative.

4.3 Heterogeneity Analysis

Table 6 reports the heterogeneous impacts of trade liberalization across four dimensions: (i) gender, (ii) age, (iii) urban versus rural residence, and (iv) the

household head's education level. All specifications include province and year fixed effects, individual- and household-level controls, and initial provincial socioeconomic conditions.

Panels A and B show the effects on male and female children, respectively. Both groups in more-exposed provinces are less likely to attend school and more likely to participate in the labor market. However, the effect on all human-capital-investment outcomes, child labor rates, and labor intensity is stronger and statistically significant for males. This indicates that boys are more vulnerable to the increased demand for low-skilled labor created by the BTA. The result does not necessarily imply that boys bear a heavier overall labor burden than girls: because the labor outcomes in this paper consider only market-based employment, and because VHLSS does not consistently collect information on domestic work such as housework, it is possible that girls leave school to take on full-time household responsibilities, perhaps replacing their mothers if trade liberalization creates more (potentially high-skilled) employment opportunities for women.

Panels C and D analyze heterogeneity by age group. Trade liberalization significantly reduces school attendance and increases labor-market participation for older children (aged 14–17), who are more susceptible to labor-demand shocks and therefore more likely to drop out of school. In contrast, the effects on younger children's school attendance (aged 10–13) are smaller in magnitude and statistically insignificant, consistent with the fact that younger children face fewer immediate labor-market opportunities and stronger compulsory-schooling expectations.

Panels E and F examine urban and rural households separately. Rural areas experience stronger adverse effects from trade liberalization, particularly in child labor incidence and intensity. While urban areas show no statistically significant change, rural areas display a clear decrease in school attendance and an increase in labor-market participation. This pattern suggests that trade liberalization placed greater pressure on rural children, increasing the demand for unskilled labor in these areas, and highlights the need for targeted support policies–particularly in rural regions–to mitigate the negative impacts on education.

Finally, Panels G and H examine heterogeneity by the household head's education level. The estimated effects are significantly larger for children in households where the head has lower education levels. Assuming household income is positively correlated with the head's education, this implies that the negative impact of the BTA shock on schooling disproportionately affects lower-income households, suggesting an intergenerational persistence of low human capital accumulation that may perpetuate disadvantage across generations. These findings argue for government policies aimed at encouraging school attendance among children, particularly those from disadvantaged households.

4.4 Robustness Check

I conduct four robustness checks (Table 7): (i) an alternative trade-liberalization measure; (ii) changing the sample age range; (iii) a coverage-validity (placebo) test; and (iv) excluding merged provinces.

Table 6: Heterogeneity Analysis

	(1) School Attendance	(2) Work	(3) Work Only	(4) Working Hours	(5) Working Days
Panel A: Male					
Post x Tariff Gap	-0.571^{b}	0.524^{b}	0.363^{a}	3.730^{c}	12.521^{c}
	(0.24)	(0.22)	(0.20)	(1.21)	(4.39)
Observations	44,810	44,810	44,810	35,759	35,759
Adjusted R^2	0.203	0.235	0.200	0.224	0.210
Panel B: Female					
Post x Tariff Gap	-0.490^{b}	0.310	0.295	3.463^{b}	10.424^{b}
	(0.23)	(0.25)	(0.19)	(1.44)	(4.66)
Observations	41,940	41,940	41,940	33,574	33,574
Adjusted R^2	0.218	0.243	0.206	0.232	0.224
Panel C: 10-13 years old					
Post x Tariff Gap	-0.191	0.200	0.039	1.465	5.405
	(0.15)	(0.20)	(0.08)	(0.88)	(3.53)
Observations	42,772	42,772	42,772	34,018	34,018
Adjusted R^2	0.069	0.139	0.052	0.118	0.121
Panel D: 14-17 years old					
Post x Tariff Gap	-0.855^{c}	0.641^{b}	0.596^{a}	5.563^{c}	16.975^{b}
	(0.29)	(0.31)	(0.30)	(1.96)	(6.58)
Observations	43,978	43,978	43,978	35,315	35,315
Adjusted R^2	0.192	0.218	0.182	0.184	0.178
Panel E: Urban					
Post x Tariff Gap	-0.160	0.088	-0.017	1.241	3.672
	(0.21)	(0.19)	(0.16)	(1.39)	(4.69)
Observations	23,290	23,290	23,290	17,795	17,795
Adjusted R^2	0.155	0.164	0.139	0.156	0.142
Panel F: Rural					
Post x Tariff Gap	-0.632^{b}	0.545	0.539^{b}	4.373^{b}	14.361^{b}
	(0.28)	(0.37)	(0.24)	(1.96)	(6.73)
Observations	63,460	63,460	63,460	51,538	51,538
Adjusted R^2	0.217	0.241	0.211	0.235	0.222
Panel G: Low educated head					
Post x Tariff Gap	-0.716^{c}	0.589^{b}	0.481^{b}	4.601^{c}	14.146^{c}
	(0.24)	(0.25)	(0.19)	(1.42)	(5.00)
Observations	68,309	68,309	68,309	56,489	56,489
Adjusted R^2	0.209	0.237	0.206	0.229	0.217
Panel H: High educated head					
Post x Tariff Gap	-0.173	0.095	0.048	0.630	4.499
	(0.13)	(0.15)	(0.12)	(0.79)	(2.94)
Observations	18,441	18,441	18,441	12,844	12,844
Adjusted R^2	0.052	0.126	0.051	0.103	0.105

Note: This table presents the heterogeneous impact of trade liberalization on children's human capital across different characteristics. All specifications control for individual and household characteristics, provincial initial conditions, province fixed effects, year fixed effects, and birth cohort dummies. Standard errors are clustered at province level. [a]p<0.1; [b]p<0.05; [c]p<0.01.

Table 7: Robustness Check

	(1) School Attendance	(2) Work	(3) Work Only	(4) Working Hours	(5) Working Days
Panel A: Alternative Trade Measures					
Post x Tariff Gap	-0.946^{a}	0.191	0.455	2.654	8.458
	(0.52)	(0.63)	(0.43)	(3.84)	(12.29)
Observations	86750	86750	86750	69333	69333
Adjusted R^2	0.205	0.236	0.198	0.224	0.214
Panel B: Alternative Age Group: 18-22					
Post x Tariff Gap	-0.334	0.210	0.261	2.702	2.885
	(0.40)	(0.44)	(0.41)	(3.27)	(12.82)
Observations	44313	44313	44313	36155	36155
Adjusted R^2	0.226	0.212	0.219	0.166	0.168
Panel C: Falsification Test					
Post x Tariff Gap	-0.308	0.436	-0.188	-4.084	-7.662
	(0.40)	(0.54)	(0.34)	(3.25)	(7.65)
Observations	26200	26200	26200	26200	26200
Adjusted R^2	0.249	0.246	0.239	0.261	0.273
Panel D: Drop Merged Provinces					
Post x Tariff Gap	-0.524^{b}	0.435^{b}	0.332^{a}	3.582^{c}	11.175^{c}
	(0.21)	(0.22)	(0.18)	(1.14)	(3.99)
Observations	80,244	80,244	80,244	64,314	64,314
Adjusted R^2	0.205	0.235	0.196	0.222	0.213

Note: This table presents the heterogeneous impact of trade liberalization on child labor incidence across different characteristics. All specifications control for individual and household characteristics, provincial initial conditions, province fixed effects, year fixed effects, and birth cohort dummies. Standard errors are clustered at province level. [a]p<0.1; [b]p<0.05; [c]p<0.01.

Alternative trade measure. As discussed above, following Kovak (2013), I use only tradable-sector employment weights when computing labor weights by province. An alternative measure based on all sectors–used by Topalova (2010); McCaig (2011b); Fukase (2013)–implicitly assumes that trade expansion does not affect non-traded-goods prices. Kovak (2013) argues this is a strong assumption: empirical evidence from Brazil shows it inflates the estimated impact of trade on wages. Excluding non-traded sectors also avoids the sensitivity of the trade-liberalization measure to the size of service-sector employment, which is often large. Panel A reports the regression results using this alternative measure: the school-attendance effect rises sharply from 0.531 in the baseline to 0.946. This larger magnitude likely reflects bias when the alternative measure overweights provinces with large non-traded sectors that nonetheless are classified as highly trade-liberalized. The remaining labor-related coefficients are unchanged in direction but lose statistical significance, which is the expected result if children primarily participate in tradable sectors such as labor-intensive manufacturing, processing, and textiles.

Age range. The 10–17 sample is chosen because VHLSS collects employment data only from age 10, and these ages span the key decision points about continuing secondary or higher education. Previous studies likewise estimate the im-

pact of trade liberalization on human capital through these decision points (Atkin 2016; Lin & Long 2020). For older ages, the impact may no longer be significant because young adults who have already chosen higher education are less likely to be diverted into low-skill labor markets by trade liberalization. To verify that the chosen age range is appropriate, Panel B re-estimates the model on the 18–22 college-age group. All coefficients are statistically insignificant, confirming that the BTA shock primarily affects pre-college schooling decisions, consistent with the conceptual framework.

Merged provinces. A potential concern is that the re-merging of provinces that were administratively split from another province during the sample period could distort the exposure measure, because the trade-liberalization measure for the new province may be over- or under-stated relative to the original. Moreover, post-separation socio-economic factors are likely to differ from those of the parent province. To address this, I exclude observations from Dien Bien, Dak Nong, and Hau Giang (separated from Lai Chau, Dak Lak, and Can Tho, respectively). Panel D shows that the direction of the effect is unchanged across all dependent variables; the magnitude changes only marginally, with the school-attendance coefficient at -0.524 versus -0.531 in the baseline.

Coverage validity. As mentioned, the limitation of pre-BTA VHLSS data prevents a direct test of the parallel-trends assumption. To address this, I control for time-varying socio-economic factors of each province in the main estimation. In addition, I perform a "quasi-parallel assumption" test, also referred to as a coverage-validity test, by combining rural households from the 1998 VLSS and 2002 VHLSS, since about three-quarters of households in the VHLSS sample are rural. I treat 1998 as a placebo "pre-treatment" year, with 2002 representing the year after a counterfactual liberalization, and re-estimate a similar regression. The test is informative because if the regression coefficient remains statistically significant with the same direction, the results would be driven by underlying development trends rather than the BTA itself. Panel C shows that the estimated impact is statistically insignificant for all outcomes and the direction of the effect changes, supporting the parallel-trends assumption in this context.

Pre-period and anticipation. Because the BTA was signed in 2000 and took effect in December 2001, I check whether anticipation or the 2002 baseline timing drives the result (Table 9). A "donut" dropping the wave(s) around the entry date leaves the effect near -0.53, and using the pre-signing 1998 rural VLSS as the baseline yields -0.999 ($p < 0.01$). The result is thus not an artifact of anticipation or the retrospective window.

5 Conclusion

This paper shows that provinces with greater exposure to the Vietnam–U.S. Bilateral Trade Agreement experienced lower school enrollment among children aged 10–17. Model estimates indicate that a 10% increase in the level of trade liberalization reduces the enrollment rate by 6.11%, a result consistent in both direction and magnitude with findings from other developing countries, such as Mexico (Atkin 2016) and China (Zhao et al. 2021). I show that this negative im-

pact operates through an increase in child labor, including the rate of children working while attending school, the rate of children dropping out to join the labor force full-time, and the intensity of child labor measured in working hours per day and working days per month. Trade liberalization thus generates a substitution effect by raising the opportunity cost of schooling as export-led demand for cheap, low-skilled labor expands.

The estimated effect is robust to using the pre-BTA 1998 rural VLSS as the baseline and to "donut" specifications that drop the survey waves around the BTA's entry, as well as to alternative trade measures, sample restrictions, and a falsification test. Relative to existing work on the BTA, the longer window through 2022 and the intensity margins (working hours and days) show that the schooling response is neither short-lived nor confined to the extensive margin of child labor, contributing evidence on the short- and medium-term effects of trade liberalization on human capital investment.

Heterogeneity analysis shows that male children, older children (aged 14–17), those living in rural areas, and those from households with less-educated heads are more strongly affected than other groups. Robustness checks–altering the trade-liberalization measure, changing the sample age range, excluding merged provinces, and conducting a coverage-validity placebo test–all support the baseline results. Notably, when using a trade-liberalization exposure measure that includes non-tradable sectors, the impact is even larger, providing evidence that the general-equilibrium effect of trade not only reallocates labor within tradable sectors but also creates spillovers into non-tradable sectors such as services.

Overall, the findings raise concerns that, while household welfare may improve through expanded employment opportunities during the process of economic integration, trade liberalization policies and export-led growth may have a negative impact on human capital investment, with potentially undesirable long-term consequences for human capital formation. The differential impacts across provinces–driven by varying levels of exposure to trade liberalization–may also exacerbate human capital disparities, reflected in differences in education and skills across regions with high concentrations of low-skilled, labor-intensive industries. Policymakers should therefore consider the asymmetric impacts of trade liberalization when designing regional export-promotion strategies. One potential remedy is to upgrade technology and product sophistication in manufacturing and export sectors, increasing the value added of exports and the demand for skilled labor. In the long run, such policies could help mitigate the negative consequences of trade liberalization on human capital while supporting Vietnam's transition to a high-income economy.

Conflict of interest statement

The author declare none.

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Appendix: Additional Tables and Figures

Table 8: Tariff Changes across Industries due to the BTA

Industry	Number of industries	Mean pre-BTA Tariff (Column 2)	Mean post-BTA Tariff (MFN)	Mean Tariff Cut	SD
Agriculture, hunting and forestry	3	0.085	0.016	0.069	0.010
Fishing	1	0.013	0.002	0.011	0.002
Mining	9	0.027	0.001	0.026	0.045
Manufacturing	57	0.330	0.034	0.296	0.148
Other	6	0.080	0.002	0.077	0.111

Notes: U.S. tariffs on imports from Vietnam are classified under ISIC Rev.3 and weighted by 2001 industry import shares. Tariff Cut is the difference between the pre-BTA Column 2 tariff and the post-BTA MFN tariff.

Table 9: Robustness Check: Anticipation and Pre-Period

	(1) Drop 2004	(2) Drop 2004 & 2006	(3) Pre-period (1998)
Post × Tariff Cut	-0.539^{b}	-0.534^{b}	-0.999^{c}
	(0.212)	(0.217)	(0.344)
Age	0.124^{c}	0.122^{c}	0.156^{c}
	(0.007)	(0.008)	(0.011)
Age squared	-0.005^{c}	-0.005^{c}	-0.006^{c}
	(0.000)	(0.000)	(0.000)
Sex (Male = 1)	-0.009^{b}	-0.007^{a}	-0.017^{c}
	(0.004)	(0.004)	(0.005)
Ethnic (Kinh = 1)	0.052^{c}	0.053^{c}	0.062^{c}
	(0.009)	(0.010)	(0.012)
Head's age	-0.003^{c}	-0.003^{b}	-0.002^{a}
	(0.001)	(0.001)	(0.001)
Head's age squared	0.000^{c}	0.000^{c}	0.000^{b}
	(0.000)	(0.000)	(0.000)
Head's sex (Male = 1)	0.014^{c}	0.014^{c}	0.019^{c}
	(0.004)	(0.004)	(0.006)
Head's education (Base = no education)			
Primary	0.095^{c}	0.097^{c}	0.079^{c}
	(0.006)	(0.007)	(0.007)
Secondary	0.154^{c}	0.153^{c}	0.136^{c}
	(0.008)	(0.008)	(0.008)
High school	0.192^{c}	0.191^{c}	0.168^{c}
	(0.008)	(0.008)	(0.010)
University and above	0.189^{c}	0.187^{c}	0.177^{c}
	(0.008)	(0.008)	(0.009)
Household size	-0.013^{c}	-0.013^{c}	-0.010^{c}
	(0.001)	(0.001)	(0.001)
Urban	0.024^{c}	0.023^{c}	
	(0.004)	(0.004)	
Constant	0.177^{c}	0.187^{c}	-0.023
	(0.064)	(0.067)	(0.112)
Province, Year & Cohort FE	Yes	Yes	Yes
Prov. initial conditions × year	Yes	Yes	Yes
Observations	80,690	75,075	46,274
Adjusted R^2	0.208	0.212	0.187

Notes: The dependent variable is school attendance. Columns (1)–(2) are "donut" specifications that drop the survey wave(s) closest to the BTA's December 2001 entry into force, comparing the 2002 pre-period with 2006–2022 and 2008–2022, respectively. Column (3) replaces the 2002 baseline with the 1998 rural VLSS—fielded before the agreement was signed—for the rural sample (the urban control is therefore omitted), with provincial initial conditions interacted with the placebo post indicator. All columns include province, year, and birth-cohort fixed effects, individual and household controls, and 2002 provincial initial conditions interacted with year. Standard errors clustered at the province level in parentheses. [a]p<0.1; [b]p<0.05; [c]p<0.01.

Table 10: Pre-BTA (2002) Balance across Top and Bottom Exposure Quartiles

	Low exposure (bottom Q1)	High exposure (top Q4)	Difference (Q4 − Q1)
Panel A: Outcomes (2002)			
School attendance	0.813	0.812	-0.002
Work	0.322	0.198	-0.124^{c}
Work only	0.155	0.146	-0.008
Panel B: Child and household characteristics			
Age	13.447	13.525	0.078
Sex (Male = 1)	0.518	0.520	0.002
Ethnic (Kinh = 1)	0.584	0.961	0.376^{c}
Household size	5.785	5.420	-0.364^{a}
Urban	0.143	0.280	0.137^{b}
Head's sex (Male = 1)	0.876	0.785	-0.091^{c}
Head's age	44.446	46.118	1.671^{c}
Head's edu: Primary	0.224	0.289	0.065^{b}
Head's edu: Secondary	0.292	0.258	-0.034
Head's edu: High school+	0.161	0.159	-0.002
Panel C: Provincial initial conditions (2002)			
Rural share	0.832	0.674	-0.158^{c}
Minority share	0.410	0.034	-0.376^{c}
Manufacturing share	0.064	0.229	0.165^{c}
Tariff Cut	0.081	0.139	0.057^{c}
Observations	6,282	6,807	

Notes: Provinces are sorted into quartiles by their Tariff Cut exposure (15 provinces each); the table compares the bottom (Q1) and top (Q4) quartiles using the pre-BTA 2002 VHLSS wave. Differences are coefficients from a regression of each characteristic on a top-quartile indicator, with standard errors clustered at the province level. [a]p<0.1; [b]p<0.05; [c]p<0.01.

Figure 3: Tariff Cut by Industry

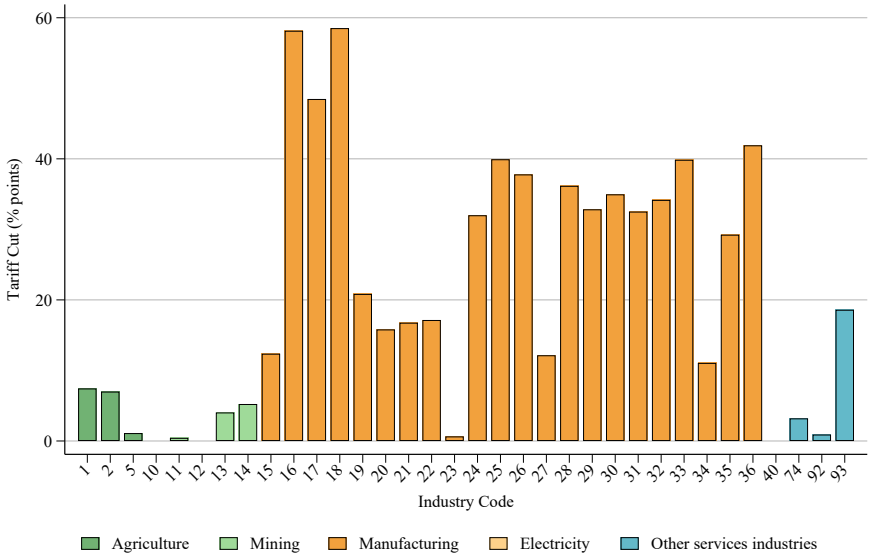


Figure 4: Tariff Gap by Province: Measure 1 and Measure 2

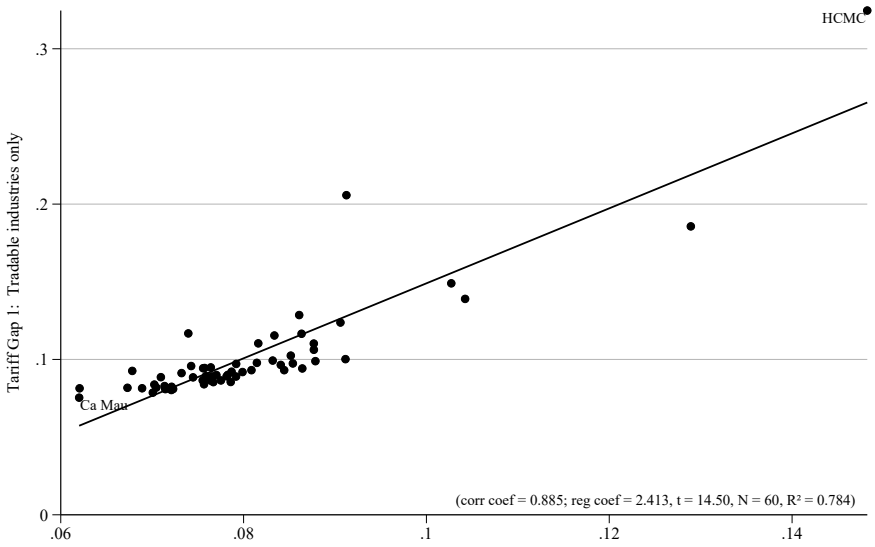


Figure 5: Tariff Gap by Province

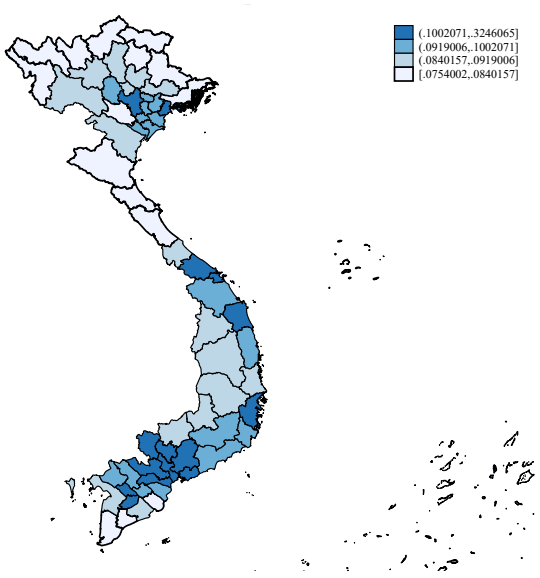


Figure 6: School Attendance and Work Incidence by Province

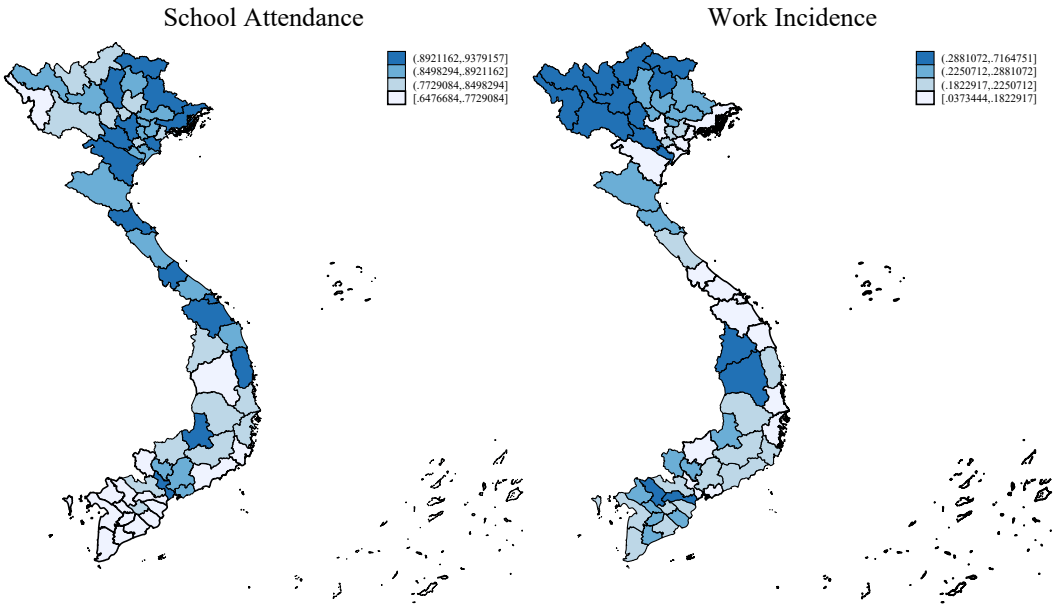
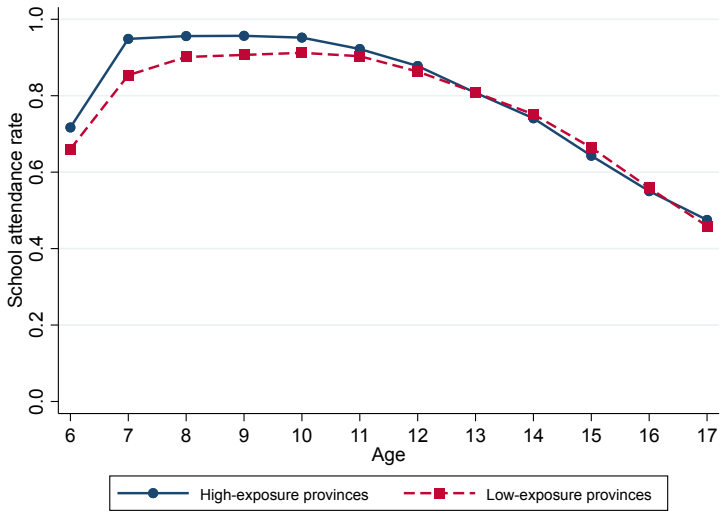


Figure 7: Pre-BTA School Attendance by Age, High- vs. Low-Exposure Provinces (1999 Census)



Notes: School-attendance rates by single year of age from the 1999 Vietnam Population and Housing Census (3% IPUMS sample), for children in the top and bottom quartiles of provincial Tariff Cut exposure. Rates are weighted by census person weights. The near-coincidence of the two profiles over ages 10–17 indicates that high- and low-exposure provinces had similar pre-BTA schooling patterns.