

Priced Out of Work, Into School: Minimum Wages and Schooling in Thailand's Dual Labor Market*

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Abstract

This paper examines how a minimum wage increase affects youth schooling in a developing economy where many workers hold uncovered jobs that the minimum wage does not reach. I extend standard single labor market models of the schooling decision to a dual labor market. In the model, a higher minimum wage increases the wage youth could earn by leaving school but can also make those jobs harder to find. It can also raise the value of a high school diploma if covered job losses fall on workers without one. Using Thailand's 300 Baht minimum wage policy and variation in mandated increases across provinces, I find that youth had a harder time finding covered jobs. Therefore, enrollment among youth aged 15–17 increased by about 1.4 percentage points more in a province whose minimum wage increased by 10 percent more than another's, and the share working without attending school decreased by about 1.3 percentage points more. The gain was concentrated among youth from less-educated households. Household consumption expenditure showed no detectable response, pointing to labor market changes rather than household resources as the source of the schooling response.

Keywords: Minimum wage; Educational attainment; Dual labor market; Developing country

JEL Codes: I26, J24, J38, O17.

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1 Introduction

A large body of work examines how the minimum wage affects employment and wages (Dube et al., 2010; Manning, 2021; Lemos, 2009; Işık et al., 2020; Samutpradit, 2024). In developing countries, increase in minimum wages have often been associated with reductions in poverty and income inequality (Alaniz et al., 2011; Del Carpio et al., 2019; Sotomayor, 2021; Engbom and Moser, 2022), although these gains are often conditional on enforcement and can fade as disemployment accumulates (Gindling and Terrell, 2010). Beyond these labor market and distributional effects, minimum wage policy also shapes the educational decisions of youth (Neumark and Wascher, 1995b, 2008) since it changes both the earnings they give up by staying in school and the value of completing it (Smith, 2021). Schooling and work compete for a young person's finite time, so any change in the wages available to youth alters the trade-off between staying in school and entering employment. A minimum wage shifts this trade-off in two ways. It raises the opportunity cost of schooling by increasing the earnings a student forgoes, and it changes the relative return to education by altering the wage gap between more- and less educated workers. The schooling decision outlasts the moment it is made. It shapes an individual's later earnings and vulnerability in the labor market (Becker, 1964; Mincer, 1974), and in the aggregate it becomes the human capital that the economy draws on as youth enter the workforce (Schultz, 1961; Lucas, 1988).

This study investigates how an increase in the minimum wage affects the educational choices of youth in an economy with a dual labor market, where a large share of workers have uncovered employment that the minimum wage does not reach. I extend the human-capital investment framework of Charles et al. (2018) and Smith (2021) to a dual labor market. Youth choose between schooling and work, and those who work sort between covered and uncovered employment. A higher minimum wage moves this decision through two opposing forces. It increases the opportunity cost of schooling by increasing the wages youth can earn now, which draws them out of school. It can also raise the return to a diploma, which draws them back in. The dual structure changes how the minimum wage affects that return. In a single labor market, as in Smith (2021), every job is covered by the minimum wage. A dropout who loses a covered job has no other sector to move into. Graduates earn above the minimum and are largely unaffected. The return to a diploma therefore moves only through the wage gains and job losses of dropouts.

In a dual labor market, an uncovered sector offers a second type of employment. As a higher minimum wage raises the cost of workers paid at the minimum, covered employers may shift their hiring toward more-educated workers. Dropouts who lose covered jobs can then fall back on lower-paying uncovered work, while graduates can move from uncovered work into the covered jobs that open up. The minimum wage therefore changes the prospects of graduates as well as dropouts. These changes can widen the return to a diploma, so a minimum wage increase can reduce dropping out even when it increases the earnings youth give up by staying in school.

To identify this effect, I use Thailand's 300 Baht minimum wage policy, announced during the 2011 general election campaign. The increase was phased in over 2012 and 2013, and I study its cumulative effect from the pre-policy minimum wage to 300 Baht. The policy offers unusually clean variation for two reasons. First, the increase was set through a political process rather than in response to local economic conditions. A party that campaigned for a uniform 300 Baht daily minimum wage won the election and raised the minimum wage to that level in every province, regardless of local growth. This makes the shock plausibly exogenous to provincial labor markets ([Samutpradit, 2024](#)). Second, the increase was large and varied across provinces. The minimum wage rose by between 35.7 and 88.7 percent from before the policy to the end of the second phase. I exploit this cross-provincial variation in treatment intensity, and its large magnitude makes any schooling response easier to detect than a smaller change would allow. Thailand also has a dual labor market. Among adults aged 25 to 40 years, whose employment outcomes youth can look to as a guide to their own future employment, 48 percent were in uncovered employment before the reform, against 39 percent in covered employment ([Table B.1](#)). Recent work on this same policy documents substantial labor-market restructuring, with low-skilled workers moving out of covered employment as the minimum wage increased ([Samutpradit, 2024](#)). In the framework, youth form expectations about the return to a diploma from the labor market outcomes of adults, so a shift of this kind can change their schooling decisions. Whether that restructuring in turn changed the schooling decisions of the youth watching it is the question that this paper takes up.

I estimate the policy's effect using a provincial panel constructed from Thailand's Labour Force Survey, a repeated cross-sectional household survey collected monthly by the National Statistical Office. The survey records individual characteristics, employment status, industry, wages, and school enrollment. My main sample is youth aged 15 to 17, who have completed

compulsory schooling but remain eligible for free upper-secondary education. I also examine adults aged 25 to 40 to measure how the policy changed the return to a diploma. Adults in this age range have finished schooling and hold settled labor market positions. The panel spans 2007 to 2016, covering roughly five years before and after the reform. The empirical design is a continuous difference-in-differences that compares provinces facing larger and smaller minimum wage increases. Provinces with lower pre-policy minimum wages were less developed and received larger increases, so the comparison conditions on baseline province characteristics.

The estimates show that the minimum wage increased youth enrollment, and the gain came almost entirely from one channel. Youth who would otherwise have left school for work stayed enrolled instead. Enrollment among youth aged 15 to 17 rose by 1.4 percentage points more in provinces where the minimum wage increased by an additional 10 percent. Decomposing that enrollment by school-work status shows where the movement came from. The share of youth who were employed and not enrolled fell by 1.3 percentage points more in those provinces. The gain was concentrated among youth from low socioeconomic status households, whose household head lacks a high school degree. It was also larger for male youth than for female youth, though this gender difference is not statistically significant. Two forces held these youth in school, one from the jobs open to them now and one from the jobs they could expect as adults. On the cost side, covered employment among youth fell sharply, so the covered work a young dropout might have left school for became harder to obtain. This lowered the opportunity cost of remaining enrolled. On the return side, I extend the restructuring documented by [Samutpradit \(2024\)](#) by education. The policy reallocated covered employment among adults away from those without a diploma and toward those who held one, which widened the covered employment premium.

The enrollment gain raises a question the labor market alone cannot answer. A minimum wage can also change household income, and a looser household budget could also have contributed to the enrollment gain. I use household expenditure from the Household Socio-Economic Survey (SES) to look for the trace an income effect would have left. Neither consumption expenditure nor food expenditure shows a detectable response. School fees do not respond either, so the additional enrollment does not appear to have cost households anything. The expenditure data therefore give no indication that the household budget contributed, and a household need not have more to spend in order to keep a child in school.

The existing literature is drawn predominantly from developed countries. It finds no consensus but leans toward null or negative effects on schooling, with impacts that differ between demographic subgroups.¹ Much of this work interprets the response through the opportunity cost of schooling. Negative effects are attributed to the wage effect. A higher minimum wage raises the earnings a student forgoes by staying in school and draws students out to work (Card, 1992; Chaplin et al., 2003; Pacheco and Cruickshank, 2007; Rice, 2010). Positive effects are attributed to the disemployment effect. If the higher minimum wage destroys enough of the low-skilled jobs a dropout would have taken, expected earnings from leaving school fall. The opportunity cost of enrollment then falls as well, and students stay in school (Neumark and Wascher, 2008, 207–208; Smith, 2021). The prevalence of null and negative estimates suggests that for youth the wage gain has usually been at least as large as the job loss. These estimates come from single labor markets, where the minimum wage covers nearly every job a dropout could take.

The evidence from developing countries is thinner and also mixed, but in these countries much of the work lies outside the reach of the minimum wage, so the schooling response need not follow the pattern of developed countries. In Indonesia, minimum wage increases lower senior secondary enrollment on average, yet the effect turns positive among low-income households (Pritadrajati, 2021). She considers two explanations for this positive response. The higher minimum wage may relax a tighter budget constraint for these households, an income effect, or it may reduce the chance of finding low-skilled work and with it the opportunity cost of school, a disemployment effect. Her estimates favor the second. Türkiye points more clearly to the disemployment channel. Extending the full adult minimum wage to 15-year-olds lowered the employment of 15-year-old boys by a fifth to a quarter, and school enrollment rose over time to absorb those who lost work (Dayioglu et al., 2022). The reform raised only the wages of 15-year-olds, so it could not work through the income of other household members. Among these studies, only the Brazilian one measures the return to schooling, and there the minimum wage moves it through wages. A higher minimum wage in absolute terms compresses the skill premium and reduces further education

¹Empirical findings vary considerably between studies and countries. In the United States, studies report positive effects (Smith, 2021; Mattila, 1978), negative effects (Lee, 2020; Neumark and Wascher, 1995b,a,c, 2003; Neumark and Nizalova, 2007; Turner and Demiralp, 2001; Chaplin et al., 2003), and mixed or null effects (Ehrenberg and Marcus, 1982; Card, 1992; Warren and Hamrock, 2010). Canadian studies find negative effects (Alessandrini and Milla, 2024; Landon, 1997) and a null effect (Campolieti et al., 2005). Research from New Zealand reports mixed or null effects (Pacheco and Cruickshank, 2007), while UK evidence shows negative effects (Rice, 2010).

among workers, while a higher minimum wage relative to local average wages raises the premium for higher education ([Marioni, 2024](#)).

The Indonesian and Turkish studies either leave the informal sector unobserved or treat it only as a channel for the lighthouse effect, through which the minimum wage raises informal pay ([Pritadrajati, 2021](#); [Dayioglu et al., 2022](#)). Neither treats it as the work that youth or adults fall back on when they lose or cannot find covered jobs. This paper treats the uncovered sector, which in Thailand overlaps heavily with informal work, as that outside option. With it, a minimum wage moves workers between covered and uncovered work rather than only into or out of employment. What a youth gives up by staying in school then depends on the chance of a covered job and on the uncovered work they would otherwise do, and the value of a diploma depends on how the minimum wage shifts covered jobs between adults with and without one.

This paper makes two contributions. First, it places the schooling decision in a dual labor market, where a diploma increases the chance of holding a covered job rather than the wage along a single ladder. That structure changes how a minimum wage moves the return to schooling. In a single labor market, the return moves only through the wage gains and job losses of workers without a diploma. In a dual market, much of the return lies in the gap in covered employment between workers with and without a diploma, and the minimum wage can widen that gap when its job losses fall on workers without one. The direction of the schooling response therefore depends in part on which education group bears the disemployment. This extends two literatures. Work on how labor market conditions shape schooling in developing economies studies shocks to local labor demand that move the cost of schooling alone ([Kruger, 2007](#); [Atkin, 2016](#); [Shah and Steinberg, 2017](#)), with [Gormly and Swinnerton \(2004\)](#) the exception in relating higher adult returns to schooling to children's enrollment. The developing-country literature on minimum wages and schooling is thin. No study in it examines both sides of the schooling decision. The youth labor market sets the cost of schooling, and the adult labor market shapes the return youth expect from a diploma. [Dayioglu et al. \(2022\)](#) trace the youth employment response and [Marioni \(2024\)](#) the adult skill premium, while [Pritadrajati \(2021\)](#) estimates the enrollment response without measuring either.

Second, it separates the labor market channel from the income channel. The income effect is itself ambiguous in sign, since a minimum wage may increase household income through wages

or reduce it through disemployment (Chaplin et al., 2003; Pacheco and Cruickshank, 2007). To my knowledge, no study has tested the two channels against each other directly. Dayioglu et al. (2022) rule the income channel out by design, since an age-specific subminimum wage leaves the earnings of other household members untouched. Pritadrajati (2021) finds enrollment gains among low-income households but cannot say whether they run through a loosened budget or through disemployment. Both channels are live in the Thai case, since the policy affected wages and employment status for adults as well as youth. Household expenditure data from the SES allow me to test the budget channel directly, and the absence of any response leaves the labor market forces to account for the enrollment gain.

The remainder of the paper is organized as follows. Section 2 describes Thailand’s minimum wage policy and its education system, and section 3 describes the data. Section 4 sets out the empirical strategy, a continuous difference-in-differences design that exploits variation in treatment intensity across provinces. Section 5 presents the dual labor market model of the schooling decision and tests its prediction for adults, showing that covered employment shifted away from adults without a diploma and toward adults with one. Section 6 reports the effect on youth enrollment and school–work status, the response of youth employment, how the effect varies across subgroups, and evidence from household expenditure on the household budget channel, followed by robustness checks. Section 7 concludes.

2 Background

2.1 *The minimum wage and the 300 Baht policy*

Before 2012, Thailand employed a decentralized minimum wage system where provinces established wage levels through a two-tiered tripartite governance structure.² According to the Labour Protection Act B.E. 2541 section 87, the minimum wage determinations were based on “cost of living index, inflation rate, standard of living, cost of production, prices of goods and services, capabilities of business, labour productivity, gross domestic product, and social

²Provincial committees deliberate on wage adjustments within their local contexts through tripartite consultation involving government, employer, and employee representatives, subsequently submitting recommendations to the central committee. The Minister of Labor renders the final decision based on input from the National Wages Council, provincial committees, and the Subcommittee on Technical Affairs and Review (Del Carpio and Pabon, 2014).

and economic conditions,” with no legally fixed frequency for minimum wage adjustments (Department of Labour Protection and Welfare, 1998). However, the system faced representational challenges characteristic of developing countries, including inadequate worker representation during minimum wage negotiations, particularly in provinces with large uncovered employment, and the systematic prioritization of political economy considerations over local labor market conditions (Del Carpio and Pabon, 2014; Del Carpio et al., 2019).³ By 2012Q1, this decentralized approach had generated 32 distinct minimum wage levels ranging from 159 to 221 Baht per day across 76 provinces.⁴

Thailand’s 300 Baht per day minimum wage policy represented an unprecedented use of minimum wage policy as an electoral campaign strategy. In July 2011, the new prime minister was elected who campaigned for raising the minimum wage to 300 Baht per day, making this policy a top electoral priority.⁵ Following the November 2011 Cabinet resolution approving the wage increase, the Permanent Secretary for Labor announced a phased implementation strategy comprising two phases. The first phase in April 2012 raised the minimum wage in every province, by about 40 percent on average. In seven provinces that already had relatively high minimum wages, this brought the minimum wage to the 300 Baht target.⁶ The second phase in January 2013 raised the minimum wage in the remaining provinces to 300 Baht, unifying it nationwide. The Cabinet resolution mandated maintaining the 300 Baht rate nationwide through 2015, after which minimum wage setting authority reverted to the provincial level (Del Carpio and Pabon, 2014). The unified rate nonetheless remained in effect through 2016. Overall, the policy increased the minimum wage by about 71 percent on average across provinces.

Thailand has long struggled to enforce the minimum wage. Evidence from the pre-period shows that non-compliance was significant. Del Carpio and Pabon (2014) found that 25 percent

³Another important consideration in Thailand is the existence of a dual economy. The minimum wage law excludes government or state-owned enterprise employees, domestic workers, agricultural workers, and unpaid family members from coverage and is not covered by the Labour Protection Act. Moreover, Thailand’s dual labor market structure poses additional implementation challenges, with 62.6 percent of the working-age population participating in the uncovered employment in 2011, where minimum wage enforcement is typically weaker (National Statistical Office of Thailand, 2016).

⁴Bueng Kan, the 77th province, was administratively separated from Nong Khai Province on March 23, 2011. I merged it back into Nong Khai Province to maintain consistent provincial boundaries throughout the sample period.

⁵Under 2013 enforcement guidance, daily-wage workers were entitled to the full daily minimum even on shifts shorter than eight hours (Samutpradit, 2024).

⁶These seven provinces were Bangkok, Samut Prakan, Nonthaburi, Pathum Thani, Nakhon Pathom, Samut Sakhon, and Phuket.

of manufacturing workers earned below the minimum wage in 2009. Even so, the minimum wage policy still managed to raise average wages despite these gaps (Del Carpio and Pabon, 2014; Lathapipat and Poggi, 2016). Drawing on LFS data from 2009 to 2015, Samutpradit (2024) estimates that compliance among covered employers was roughly 80 percent, with non-complying employers paying about 23 percent below the minimum wage. Despite this incomplete compliance, the same study finds that the 300 Baht policy produced substantial wage gains at the lower end of the covered employment, while wages in the uncovered employment rose modestly through lighthouse effects among low-educated workers with observable wages.

Following the large increase in minimum wage, economic conditions remained stable. Headline inflation averaged 2.18 percent in 2013 and turned negative in 2015, at -0.9 percent, largely because of falling global oil prices. Core inflation, which excludes energy and raw food prices, remained positive at 1.1 percent in 2015. Labor market conditions were similarly benign, as tight labor demand helped prevent widespread layoffs in 2013, and despite occasional headwinds from drought, global economic shifts, and raw material price changes, the unemployment rate remained below 1 percent through 2015 (BOK, 2013, 2014, 2015).

2.2 Educational enrollment in Thailand

Thailand's formal education system is structured into three levels: six years of primary school (grades 1 through 6, ages 6 to 11), three years of lower secondary school (grades 7 through 9, ages 12 to 14), and three years of upper secondary school (grades 10 through 12, ages 15 to 17). Prior to 2009, the Thai government guaranteed every person the right to at least 12 years of free state-provided education, spanning primary through upper secondary school (grades 1 through 12) (Kingdom of Thailand, 1997). Since 2009, the 15-Year Free Education Program has extended the free provision to 15-years by adding three years of pre-primary education, so that free schooling now runs from pre-primary through upper secondary (Office of the Basic Education Commission et al., 2017). Compulsory attendance is set separately and is shorter. It covers nine years, ending at age 16 or upon completion of grade 9, whichever comes first (Kingdom of Thailand, 1999). Upper secondary schooling is therefore free but not mandatory. In addition, Section 44 of the Labour Protection Act B.E. 2541 (1998) prohibits employers from hiring any child under 15 years of age (Ministry of Labour, 1998). Youth who have completed grade 9 and reached age 15 are thus free

both to leave school and to work, so those aged 15 to 17 choose between continuing to upper secondary school and entering work.

Under the 15-Year Free Education Program, public schools receive per-student subsidies. The largest component covers operating costs related to teaching and learning, including school books, learning materials, and student uniforms, with smaller allocations for curriculum quality and supplementary support targeted at poor students and small schools. Public schools cannot charge tuition directly. However, they may collect other fees with school-board approval (Gauthier and Punyasavatsut, 2019).

3 Data

The analysis draws on the Labour Force Survey (LFS), administered by Thailand’s National Statistical Office monthly and published quarterly, spanning 2007 to 2016. The LFS provides information on educational participation, labor market status, occupational categories, industrial affiliations, and household and individual characteristics. It records school enrollment and employment for the same individuals, which allows me to decompose youth status into mutually exclusive school–work categories. I construct a provincial panel dataset from this repeated cross-sectional survey covering all 76 provinces in Thailand. The analytical sample focuses on youth aged 15–17. Provincial minimum wage data are obtained from Thailand’s Ministry of Labor and merged with the LFS at the provincial-quarter level.

3.1 *School enrollment rate*

This study examines student enrollment rates as the primary outcome variable of interest. For the main analysis, youth participation in education means currently attending formal education institutions.⁷

Figure 1 illustrates enrollment rates by age from 2007 to 2016, excluding April, when Thai schools are on summer break. Enrollment declines monotonically with age. Fifteen-year-olds consistently have the highest enrollment rates, followed by 16-year-olds, with 17-year-olds the

⁷The analysis focuses exclusively on regular schooling, excluding non-formal education and cases with unspecified educational status. Non-formal education encompasses short-term vocational training, Pondok Islamic studies, non-degree courses, and educational programs not aligned with standard academic levels.

lowest. Before 2011, enrollment was relatively flat for all ages. From 2012 onward, it trends upward for all ages, most noticeably among 16- and 17-year-olds. Among 17-year-olds, the enrollment rate rises from about 73 percent in 2007 to 78 percent by 2016.

To see in more detail where changes in enrollment come from, I follow [Ehrenberg and Marcus \(1982\)](#) and [Neumark and Wascher \(1995b\)](#) and classify youth school–work status into four mutually exclusive categories: enrolled and not employed, enrolled and employed, not enrolled and employed, and not enrolled and not employed. Employment status encompasses both covered and uncovered employment across all industries.⁸ This decomposition allows me to track how youth reallocate across school–work categories in response to the minimum wage policy.

[Table 1](#) presents descriptive statistics for youth aged 15 to 17. School enrollment rose modestly from 81 percent in the pre-policy period to 83 percent in the post-policy period. Across the four categories, the share of youth enrolled and not employed rose by 4 percentage points, while the shares enrolled and employed and not enrolled and employed each fell by about 2 percentage points. The share not enrolled and not employed changed little.

3.2 *Youth employment status*

Although school enrollment is the main outcome, employment status serves as a complementary outcome that shows how youth divide their time between school and work. Employment and schooling are not mutually exclusive, since youth can attend school and work at the same time. The share of youth aged 15 to 17 who were employed fell from 17 percent in the pre-policy period to 12 percent in the post-policy period ([Table 1](#)). This decline appeared in both covered and uncovered employment. Among working youth with observed wages, mean daily wages rose from 166 Baht to 252 Baht. This 52 percent increase is smaller than the average minimum wage increase of about 71 percent. These raw changes also reflect general wage growth and shifts in which youth remain employed, so they describe the period rather than the effect of the policy. Wages are observed

⁸Workers are uncovered if the statutory minimum wage does not reach them, comprising unpaid family workers, workers in agriculture or domestic service, the self-employed, and employers. Government and state enterprise employees are also outside the minimum wage but are excluded from the analysis rather than classified as uncovered, since public employment differs from the low-barrier work that constitutes the uncovered sector in the model. All other employed workers are covered. Individuals who did not work and had no job to return to are classified as unemployed if they looked for work in the 30 days before the survey or were available for work immediately. Those who were not available for work during the seven days before the survey, including seasonal workers waiting for the next season, are classified as out of the labor force.

mainly for youth in covered employment.⁹

3.3 *Minimum wage*

Provincial minimum wage data are obtained from official Minimum Wage Decrees issued by the Ministry of Labor of Thailand, which provide the implementation dates and wage rates established for each province. Each LFS respondent is assigned the minimum wage in effect in their province in the quarter in which they were surveyed.

I measure treatment intensity as the change in the log provincial minimum wage between 2012Q1 and 2013Q1, which spans both phases of the increase. [Figure 2a](#) shows provincial minimum wages over the sample period. They were stable and dispersed before the policy, converged to 300 Baht between April 2012 and January 2013, and remained at 300 Baht through 2016. [Figure 2b](#) shows the distribution of treatment intensity across the 76 provinces, which ranges from 0.31 to 0.63, or increases of 36 to 89 percent. The distribution is skewed to the left. Most provinces cluster around a log change of 0.6, since most had relatively low pre-policy minimum wages and therefore faced larger increases. The low tail consists of the seven provinces with the highest pre-policy minimum wages, including Bangkok, which left them with the smallest increases ([section 2](#)).

3.4 *Other variables*

[Table 1](#) also reports the individual and household characteristics. Individual demographic characteristics remained stable across periods. Males consistently represented approximately 51 percent of the sample in both periods, and the mean age of the sample remained unchanged at 16 years.

Household head characteristics show several demographic shifts. The share of youth residing in municipalities (urban area) increased from 28 percent in the pre-period to 37 percent in the post-period, reflecting Thailand's ongoing urbanization over this period. The proportion of male household heads decreased from 68 to 63 percent. The share of married household heads declined modestly from 78 to 76 percent. The mean age of the household heads increased from

⁹Daily wage calculations follow [Samutpradit \(2024\)](#). Among youth aged 15–17 with observed daily wages, 20 percent work in uncovered employment.

approximately 50 to 51 years, consistent with Thailand’s demographic transition to an aging population (World Bank, 2021). The share of household adults aged 25 and older holding a high school degree increased from 17 to 22 percent, reflecting broader improvements in educational attainment over the period.

Table 2 presents baseline characteristics at the provincial level measured in 2007 across all 76 provinces. The average province had an agricultural employment share of approximately 47 percent, a high-education industry share of 6 percent, and an unemployment rate below 1 percent.¹⁰ The baseline enrollment rate for youth aged 15–17 was 79.6 percent in 2007, slightly below the pre-period average in Table 1, because enrollment edged up slightly between 2007 and 2012Q1.

4 Empirical Strategy

I employ a continuous difference-in-differences design and its event study version to identify the causal effects of Thailand’s 300 Baht minimum wage policy. The design compares provinces that faced larger and smaller minimum wage increases, using the treatment intensity defined in subsection 3.3. All regressions are weighted using the survey sampling weights provided in the LFS, so that the estimates are nationally representative.

The baseline continuous difference-in-differences specification is:

$$Y_{ihpt} = \alpha + \beta \Delta \log(MW_p) \times Post_t + \gamma X_{ihpt} + \delta(Z_{p,2007} \times Post_t) + \theta_p + \theta_t + \epsilon_{ihpt} \quad (1)$$

where Y_{ihpt} represents the educational enrollment status or one of the four mutually exclusive categories of school-work status for the individual i in the household h residing in the province p at time t . The treatment intensity is the change in the log minimum wage in province p between 2012Q1, before the policy, and 2013Q1, after both phases were implemented, $\Delta \log(MW_p) \equiv \log(MW_{p,2013Q1}) - \log(MW_{p,2012Q1})$. It captures the full increase in each province. The indicator $Post_t$ equals one from 2012Q2 onward, when the minimum wage increase began, and zero otherwise.

¹⁰High-education industries consist of utilities, finance and business services, and education and health sectors, where more than 50 percent of workers hold a post-secondary education credential.

The vector X_{ihpt} encompasses two sets of covariates: (i) individual demographic characteristics, including indicators of age, birth cohort, and gender; and (ii) household characteristics, including the share of household members aged 25 and older with at least a high school education, an indicator for municipality (urban) residence, and the household head's age, gender, and marital status. Cohort fixed effects account for heterogeneous policy exposure across birth cohorts. Specifically, older cohorts completed their secondary school years before the policy change, middle cohorts experienced partial exposure, and younger cohorts were fully exposed to the higher minimum wage throughout their school years.

The vector $Z_{p,2007} \times Post_t$ captures baseline provincial controls, measured in 2007 and interacted with the post indicator. These controls address a systematic feature of the policy. Under a uniform national minimum wage, provinces starting from a low base faced larger mandated increases. These low-wage provinces tend to be the less developed ones, so the size of the increase serves as an inverse proxy for a province's level of development. Development can shape enrollment in either direction. Wealthier provinces can more readily bear the costs of schooling and tend to offer stronger returns to education, but they also offer youth more covered jobs and a stronger labor market, which raise the opportunity cost of schooling. [Figure H.1](#) plots provincial enrollment among youth aged 15 to 17 over the pre-policy period, from 2007 to 2011, against the minimum wage change. Provinces with larger minimum wage increases had higher enrollment before the policy, consistent with the opportunity cost channel. Interacting these characteristics with the post indicator lets provinces with different starting conditions change by different amounts after the policy, so that those differences are not attributed to the minimum wage.

Four baseline controls are included. The 2007 enrollment rate for youth aged 15 to 17 directly addresses the enrollment differences shown above. The share of agricultural employment among adults aged 25 and older is a proxy for provincial economic development, as less developed provinces tend to have larger agricultural sectors and lower initial minimum wages. It also serves as an inverse proxy for the prevalence of covered employment, the segment in which the minimum wage binds, because agricultural jobs are largely uncovered. The share of adults aged 25 and older employed in high-education industries captures the local return to schooling embodied in the job structure, which may shape enrollment independently of the minimum wage. The unemployment

rate captures baseline labor market conditions and the financial cushion of the province's residents, since remaining unemployed rather than taking any available work requires savings or family support to fall back on while searching.

To address potential confounding, the model includes province fixed effects, θ_p , which absorb time-invariant provincial characteristics that may affect both youth enrollment and minimum wage levels. Period fixed effects, θ_t , absorb shocks common to all provinces in a given period.¹¹ Standard errors are clustered by province, the level at which treatment intensity varies, which also accounts for correlation in outcomes within provinces over time. All regressions are weighted using individual survey sampling weights.

Following [Callaway et al. \(2024\)](#), I interpret β as the two-way fixed effects estimand for a continuous treatment, which is a weighted average of responses across treatment intensities. Since there is no untreated comparison group, β does not recover the effect of the policy relative to no policy. It instead compares provinces that faced larger and smaller minimum wage increases. Under standard parallel trends, this comparison combines the causal response to a larger increase with a selection term that reflects differences across provinces in how they would have responded to the same increase. Therefore, interpreting β as a causal response requires strong parallel trends, which states that provinces receiving the same increase would respond to it in the same way. This is unlikely to hold without conditioning. The provinces that received the largest increases were largely agricultural and low-wage, and they would plausibly respond to a given increase differently from the developed provinces that received the smallest ones. I assume instead that strong parallel trends holds conditional on the 2007 baseline province characteristics, so that only provinces with similar characteristics are required to respond similarly. Under this assumption, β is the effect on the enrollment rate of a larger increase in the log minimum wage, relative to provinces that faced smaller increases. As the dose is measured in logs, a province whose minimum wage rose by 1 percent more than another's experienced an enrollment gain larger by approximately $\beta/100$.

To examine dynamic treatment effects and validate the parallel trend assumption, I

¹¹Periods are calendar years, except that 2011Q1–2012Q1 forms one pre-policy period and 2012Q2–2012Q4 forms one period covering the first phase of the increase.

subsequently estimate an event study specification:

$$Y_{ihpt} = \beta_0 + \sum_{t=2007, t \neq t_0}^{2016} \beta_t \Delta \log(MW_p) \times \mathbf{I}(Year = t) + \gamma X_{ihpt} + \delta (Z_{p,2007} \times \tau_t) + \theta_p + \theta_t + \epsilon_{ihpt} \quad (2)$$

where t_0 denotes the omitted reference period, corresponding to 2011Q1 to 2012Q1, θ_p and θ_t are province and year fixed effects, X_{ihpt} collects individual and household covariates, and $Z_{p,2007} \times \tau_t$ denotes the 2007 baseline controls (agricultural share, high-education industry share, unemployment rate, and baseline enrollment rate) each interacted with period indicators, τ_t . The sample spans 2007 to 2016. Observations are annual except for t_0 and t_1 , where t_1 covers 2012Q2 to 2012Q4 to isolate the quarters immediately following implementation. The remaining periods are annual through 2016.

5 Theoretical Framework: Educational Choice in a Dual Labor Market

I set a simple framework of the schooling decision, building on the human capital investment models of [Charles et al. \(2018\)](#) and [Smith \(2021\)](#) to a labor market with covered and uncovered employment. The minimum wage, ω , is the only exogenous change in the framework. It affects the schooling decision through the earnings a youth gives up by staying in school and the earnings a youth expects as an adult, both of which depend on how the labor market responds to the minimum wage. These responses can push the decision in opposite directions, so the framework does not sign the effect of a minimum wage increase on dropping out. Instead, it shows which responses lower the dropout rate and which raise it. [Appendix A](#) presents the full model and derivations.

Youth differ in academic ability θ_i , which is distributed smoothly over $[0, 1]$.¹² Each youth decides whether to complete high school or to leave school and enter the labor market. Completing high school yields a benefit $B(\theta_i, \omega)$, the difference in expected lifetime earnings between graduates

¹²I refer to θ_i as ability throughout, but it need not be ability alone. It stands for whatever makes schooling easier or more worthwhile for a given youth and can also capture unobserved attributes of the youth's household, such as household finances, access to credit, or how much the household relies on the youth's labor at home or on the farm. A youth with higher θ_i faces a lower cost of staying enrolled, whether that cost takes the form of academic difficulty or the direct expense of schooling. Only the ordering matters for the framework, so the schooling decision remains a threshold in θ_i .

and dropouts.¹³ It also has a cost $C(\theta_i, \omega)$, which consists of the earnings a youth gives up by staying in school and an idiosyncratic cost of schooling that decreases with ability.¹⁴ The two curves cross once at the threshold ability $\bar{\theta}$ where $B(\bar{\theta}, \omega) = C(\bar{\theta}, \omega)$, since the benefit rises and the cost falls with ability.¹⁵ Youth with $\theta_i < \bar{\theta}$ drop out, and those with $\theta_i \geq \bar{\theta}$ complete high school (Figure 3).

A minimum wage increase shifts both the benefit curve and the cost curve, and what matters for the dropout rate is how the benefit curve shifts relative to the cost curve at the threshold. The dropout rate falls if the net benefit of completing school rises at the threshold, that is, if $\partial B / \partial \omega > \partial C / \partial \omega$.¹⁶ The dropout rate rises if the net benefit falls.

The minimum wage moves each curve through two effects. The wage effect raises the pay of workers who hold covered jobs, and the disemployment effect reduces the chance of holding one. On the cost curve, the wage effect raises the earnings a youth gives up by staying in school and shifts the curve up, while the disemployment effect lowers those earnings and shifts the curve down. The benefit curve depends on how the two effects differ between graduates and dropouts, since the benefit is the difference in their expected earnings. The benefit curve shifts up when the two effects together favor graduates and down when they favor dropouts. The two effects can also point in opposite directions. For example, the disemployment effect may fall more heavily on dropouts while dropouts gain more from the wage effect, and the direction then depends on which is larger.

To determine how the benefit curve moves, I assume that the minimum wage raises pay in covered jobs for graduates and dropouts alike but leaves pay in uncovered jobs unchanged. The new minimum wage of 300 Baht was high enough to make this plausible, since nearly half of

¹³Formally, $B(\theta_i, \omega) = EV(1, \theta_i, \omega) - EV(0, \theta_i, \omega)$, where $EV(s, \theta_i, \omega) = \pi_V(s, \theta_i, \omega) PV^V(s, \theta_i, \omega) + \pi_U(s, \theta_i, \omega) PV^U(s, \theta_i, \omega)$ is the expected present value of lifetime earnings for an adult with schooling $s \in \{0, 1\}$. Here π_V and π_U are the probabilities of covered and uncovered employment, PV^V and PV^U are the present values of earnings in each, and non-employment earns zero.

¹⁴The cost of schooling is $C(\theta_i, \omega) = Y_0(\theta_i, \omega) + \kappa - \rho\theta_i$, with $\kappa, \rho > 0$. Forgone earnings average over the jobs a youth could find by leaving school at $t = 0$, $Y_0(\theta_i, \omega) = \pi_{V,0}(\theta_i, \omega) Y_0^V(\theta_i, \omega) + \pi_{U,0}(\theta_i, \omega) Y_0^U(\theta_i, \omega)$. The youth probabilities $\pi_{V,0}$ and $\pi_{U,0}$ differ from the adult probabilities $\pi_V(s)$ and $\pi_U(s)$, since a youth who leaves school now faces a different labor market from the one an adult faces later.

¹⁵Rising benefits and falling costs ensure that the curves cross at most once. I also assume that $B(0, \omega) < C(0, \omega)$ and $B(1, \omega) > C(1, \omega)$, so that the lowest-ability youth drop out and the highest-ability youth complete high school. Since both curves are continuous in ability, they then cross within the range of abilities, $[0, 1]$.

¹⁶Both derivatives are evaluated at $\bar{\theta}$. Differentiating $B(\bar{\theta}, \omega) = C(\bar{\theta}, \omega)$ gives $d\bar{\theta}/d\omega = -(\partial B/\partial \omega - \partial C/\partial \omega) / (\partial B/\partial \theta - \partial C/\partial \theta)$. The denominator is positive because $B - C$ increases in θ , so $d\bar{\theta}/d\omega < 0$ if and only if $\partial B/\partial \omega > \partial C/\partial \omega$.

graduates in covered jobs earned less than that before the policy.¹⁷ The wage effect then does not clearly favor either group, so the benefit curve moves mainly through the disemployment effect. Since covered jobs pay more than uncovered jobs, what a worker gains from the policy depends on whether they hold one. The benefit of a diploma therefore rises if covered jobs shift toward graduates and away from dropouts. Before the policy, covered employers could pay a dropout less than a graduate for a low-wage job. The new minimum wage requires paying both at least 300 Baht, which removes much of this difference. With dropouts no longer cheaper for these jobs, employers have less reason to choose them. Employers cannot observe productivity directly, and a diploma signals it. As a result, covered employers may shift their hiring toward graduates. In a dual labor market, graduates working in uncovered jobs can fill these openings.

A youth cannot observe the earnings they will receive as an adult. Following [Charles et al. \(2018\)](#), I assume that youth form these expectations from the current labor market outcomes of adults with the same schooling in their province. The effect of the minimum wage on adults with and without a diploma is then its effect on the benefit of completing school. Adult outcomes can therefore show whether covered jobs actually shifted toward graduates. [Table 3](#) reports this effect for adults aged 25 to 40.¹⁸ In provinces that faced larger increases, employment fell among adults without a diploma, while employment among adults with a diploma did not change significantly. The breakdown by employment type shows that covered employment shifted from adults without a diploma to adults with one, who also moved out of uncovered work.¹⁹ This is the shift that raises the benefit of a diploma. It is consistent with [Samutpradit \(2024\)](#), who documents that the same policy moved low-skilled workers out of covered employment, and it adds the education dimension.

The minimum wage moves the cost curve only through forgone earnings, since the idiosyncratic cost of schooling does not depend on it. A higher minimum wage raises the pay of youth who hold covered jobs, which raises the cost of staying in school and shifts the cost curve

¹⁷Between 2007 and 2012Q1, 47 percent of graduates and 82 percent of dropouts aged 25 to 40 in covered jobs earned less than 300 Baht a day. Among workers in covered jobs, a larger share of dropouts was therefore directly affected by the new minimum. However, graduates are more likely than dropouts to hold covered jobs at all. Taking both into account, similar shares of graduates and dropouts were directly affected by the policy.

¹⁸[Appendix B](#) describes the specification and discusses the results in detail.

¹⁹Over the sample period, 33.3 percent of adults without a diploma held covered jobs, 53.3 percent worked in uncovered jobs, and 13.4 percent were not employed. Among adults with a diploma, 53.7 percent held covered jobs, 34.8 percent worked in uncovered jobs, and 11.5 percent were not employed.

up. It can also reduce the chance that a youth finds a covered job, since youth have the least work experience and employers may replace them with older workers when the minimum wage rises. Forgone earnings therefore fall, and the cost curve shifts down. Uncovered work can only partly make up for the lost covered job, since it pays less and does not gain from the minimum wage. The direction of the cost curve depends on which of these two effects is larger.

With covered jobs shifting toward graduates, the benefit curve shifted up. The effect of the minimum wage on dropping out therefore depends on how the cost curve moved. The dropout rate falls if the cost curve shifts down (Figure 4a) or shifts up by less than the benefit curve. It rises only if the cost curve shifts up by more than the benefit curve (Figure 4b). Result section shows whether the dropout rate fell and examines the disemployment effect on the cost curve.

6 Results

This section reports the effects of the minimum wage on youth. Subsection 6.1 estimates the effect on enrollment and school–work status, and subsection 6.2 shows that the estimate is robust to how treatment intensity is weighted and to contemporaneous policies and shocks. Subsection 6.3 examines youth employment, which determines the cost of schooling in the model, and subsection 6.4 studies how the response varies across subgroups. Subsection 6.5 examines the household budget as an alternative channel.

6.1 *Effects on enrollment rate*

Table 4 reports estimates of β from the continuous difference-in-differences specification in Equation 1. Each estimate measures how an outcome responds to a larger minimum wage increase relative to provinces that faced smaller increases. Column (1) reports the effect on overall enrollment, and columns (2) through (5) decompose the youth status into four mutually exclusive categories. The corresponding event study estimates from Equation 2 are shown in Figure 5.

Column (1) of Table 4 shows that enrollment increased more in provinces that faced larger minimum wage increases. The pooled coefficient is 0.146, significant at the 10 percent level, against a baseline enrollment rate of 81.8 percent. A province whose minimum wage increased by 10 percent more than another’s experienced an enrollment gain larger by approximately 1.4

percentage points. The pre-period estimates in [Figure 5a](#) are close to zero except for a spike in 2009, consistent with the parallel trends assumption. After the policy implementation, the yearly estimates rise and remain positive through 2016. Their confidence intervals are wide, so no single year is statistically significant on its own, but together they yield the significant pooled estimate.

Columns (2) through (5) decompose the enrollment response into the four mutually exclusive school-work categories, showing how youth status shifted. The only statistically significant estimate appears in column (4). The share of youth who are employed and not enrolled fell more in provinces that faced larger increases, with a coefficient of -0.141 against a baseline share of 11.5 percent. A province whose minimum wage rose by 10 percent more than another's experienced a decline in the share of youth working without attending school that was larger by about 1.3 percentage points. The corresponding event study in [Figure 5d](#) shows pre-period estimates that are flat and centered on zero, consistent with parallel trends. After the policy, the series declines and settles below zero.

The remaining three categories do not show statistically significant estimates, but their point estimates and event study patterns help trace where the change in column (4) went. The point estimate for the enrolled and employed category in column (3) is positive (0.098). Its event study in [Figure 5c](#) shows a flat pre-period followed by a gradual increase after 2011 that becomes significant at the 10 percent level by 2016, mirroring the decline in the not enrolled and employed group in column (4). Although imprecisely estimated, the two patterns together suggest a change in how youth combine work and school. In provinces that faced larger minimum wage increases, the share of youth working without attending school fell further, while the share working while attending school rose. Columns (2) and (5) change little. Their estimates are small and insignificant, and their event studies show no clear movement after the policy. This is broadly consistent with the model's prediction that the policy mainly affects youth whose ability lies close to the cutoff $\bar{\theta}$.

6.2 *Robustness*

This subsection examines two threats to the baseline estimates. The first is that the baseline coefficient summarizes the response in a single slope, which can be misleading if the response differs across doses. Three checks address this by allowing a nonlinear response, comparing dose quartiles, and trimming the dose distribution. Across these checks, the enrollment estimates

remain positive and are mostly larger than the baseline estimate, so the baseline serves as a conservative headline estimate. The second threat is that other events drove the results. The minimum wage increase coincided with two government programs and a major flood, and each of these affected provinces unevenly. Controlling for exposure to these shocks leaves the estimates unchanged.

6.2.1 Sensitivity to the dose distribution

Under strong parallel trends, the baseline coefficient is a weighted average of the causal responses at each dose, with weights determined by OLS rather than chosen by the researcher (Callaway et al., 2024). These weights are largest near the mean dose and depend on the dose distribution. They matter only if the response differs across doses. Three checks assess how sensitive the baseline estimate is to this weighting. The first adds a squared dose term so that the response can differ across doses. With the dose centered at its survey-weighted mean, the coefficient on the linear term gives the average causal response. This average weights each dose by the share of youth facing it. The second replaces the continuous dose with quartile bins and compares each bin with the lowest quartile. This shows how the effect varies across dose groups without relying on the weights of the continuous specification. The third keeps the continuous specification but trims each end of the dose distribution separately.

Callaway et al. (2024) propose flexible specifications such as polynomials. With only 76 provinces, the first check uses a quadratic in the dose. The quadratic captures heterogeneity by allowing the causal response to the minimum wage to differ across doses. Centering the dose at its survey-weighted mean makes the coefficient on the linear term equal to ACR^o , which averages the marginal response at each dose using weights equal to how common that dose is among youth. By contrast, the baseline estimate is a single slope. It is a weighted average of the causal responses across doses with OLS weights that are largest near the mean of the dose distribution.²⁰ Table 5

²⁰Following Callaway et al. (2024), both the baseline coefficient (β^{twe}) and ACR^o are weighted averages of these causal responses across doses. They differ in how their weights are calculated. Under strong parallel trends, the baseline coefficient can be written as $\beta^{twe} = \int_{d_L}^{d_U} w_1^{acr}(l) ACR(l) dl$, $w_1^{acr}(l) = \frac{(E[D|D \geq l] - E[D])P(D \geq l)}{\text{Var}(D)}$, where $ACR(l) = \partial ATE(l)/\partial l$ is the causal response at dose l , and d_L and d_U are the lowest and highest doses. The weight $w_1^{acr}(l)$ depends on the dose distribution. From this formula, the weight is largest at the mean dose and falls to zero at the lowest and highest doses. It is also positive at doses where no province is observed, such as the empty interval in Figure 2b. In contrast, $ACR^o = E[ACR(D) | D > 0]$ averages the causal responses using the distribution of the dose among youth. Its weight at dose l is the share of youth facing that dose, which reflects only how common the dose is

reports an ACR^o of 0.251 for enrollment. This is larger than the baseline estimate of 0.146, although the difference is not statistically significant. Two different weightings can give different averages only when the causal response is heterogeneous across doses. The positive and significant squared term shows that it is. The response grows as the dose increases, so the dose-response curve is convex.

The second check groups provinces into quartiles of the dose and compares each quartile with the bottom quartile in a single regression (Table 6). This specification allows the response to be heterogeneous across doses without imposing a functional form on it. It also does not rely on the OLS weights of the continuous specification. Under strong parallel trends, lower-dose groups are valid counterfactuals for higher-dose groups. Each coefficient then identifies the difference in causal effects between the two groups even without an untreated group (Callaway et al., 2024). The coefficients increase with the dose, from 0.017 in the second quartile to 0.031 in the third and 0.042 in the top quartile. Expressed in per log point of the dose, they rise from 0.135 to 0.191 and 0.219. These are average slopes measured from the bottom quartile, so their increase means the response grows with the dose. The binned estimates therefore also point to heterogeneity across doses, consistent with the convex dose-response curve from the quadratic specification. The baseline estimate of 0.146 falls within the range of these slopes, near its lower end.

The third check trims the dose distribution. Provinces at its extremes can shape the baseline estimate since the OLS weights depend on the dose distribution. The seven most developed provinces are a clear case. Their high pre-policy minimum wages left them the smallest increase to 300 Baht, and they sit apart from the other provinces at the low end of the distribution (Figure 2b). This check re-estimates the baseline specification first without these seven provinces and then without the top quartile of the dose. Excluding the seven least-treated provinces raises the enrollment estimate to 0.296 (Table G.1). This is larger than the baseline of 0.146, but the difference is not statistically significant. The trimmed sample also retains much less dose variation. The standard deviation of the dose falls from 0.082 to 0.050 and the standard error rises from 0.081 to 0.129, so this is the least precise of the estimates. Excluding the most-treated provinces in the top quartile gives 0.197, which is also above the baseline. Neither trim reverses the sign or removes

in the population. Doses faced by many youth receive more weight, and doses where no province is observed receive none.

the significance of the enrollment effect.

Across the three exercises, the enrollment effect ranges from 0.135 to 0.296 per log point. The baseline estimate of 0.146 lies at the lower end of this range. Both the quadratic and the binned estimates suggest that the response grows with the dose. This pattern is intuitive, since a larger minimum wage increase would have a disproportionately larger disemployment effect. Most of the alternative estimates are larger than the baseline, which indicates that the OLS weighting does not overstate the response. The baseline therefore serves as a conservative headline estimate. The composition of the effect is also stable. The coefficient on the share of youth who are not enrolled and employed ranges from -0.180 to -0.141 and is significant in every specification. The remaining categories stay imprecisely estimated.

6.2.2 *Contemporaneous policies and shocks*

The 300 Baht policy did not arrive alone. The Pheu Thai party, which won the July 2011 election, formed a government in August 2011 and implemented two other programs on an overlapping timetable. A major flood also struck in the months between the election and the first minimum wage increase. Each of these events affected provinces unevenly and could plausibly influence youth schooling. If exposure to any of them correlates with treatment intensity, the baseline estimates would attribute their effects to the minimum wage.

The Rice Pledging Scheme is the most direct concern. The scheme launched in October 2011, six months before the first wage increase. It purchased paddy at a guaranteed 15,000 Baht per ton with no limit on quantity, a price roughly 46 percent above the market rate ([USDA Foreign Agricultural Service, 2017](#)). Provinces were exposed in proportion to how much rice they grew. Rice-growing provinces are less developed, so they began further below the 300 Baht minimum wage and faced larger increases. A transfer to farming households could raise or lower enrollment, and either would be confounded with the wage effect. The loan deferment policy raises a parallel concern. Households with loans below 500,000 Baht could suspend payments for three years, and registration opened in May 2012, one month after the first wage step. Relaxing a binding credit constraint could alter both labor supply and schooling decisions in indebted provinces ([Samutpradit, 2024](#)). Finally, the 2011 floods inundated 26 provinces from late July to mid-December 2011. They damaged schools directly, with the education sector absorbing USD

485 million in damage and loss, and reduced expenditure and wage income in flooded households (Poaponsakorn et al., 2014). Either channel could change enrollment in flooded provinces for reasons unrelated to the minimum wage.

Each shock is controlled for by interacting a measure of provincial exposure with the post indicator in Equation 1. Rice exposure is average paddy production from 2008 to 2010, valued at the guaranteed price and divided by gross provincial product averaged over the same years. Averaging over three years keeps the measure from reflecting the weather of any single harvest. Deferment exposure is the log of average debt per household in 2011. Flood exposure is the share of a province's sub-districts inundated for at least fourteen days.²¹

Table 7 reports the results. Columns (2) through (4) add each control one at a time, and column (5) adds all three. Controlling for these shocks leaves the estimates essentially unchanged. The enrollment effect ranges from 0.135 to 0.148 across specifications, against a baseline of 0.146. The decline in the share of youth who are not enrolled and employed ranges from -0.148 to -0.127 and is significant in every column. The corresponding event studies in Figure G.1 also remain close to the baseline.

The flood control merits a separate remark. Six of the seven most developed provinces were flooded. Those seven also occupy the low end of the dose distribution, so flood exposure and treatment intensity are negatively correlated. The control therefore competes with the treatment for the same variation and could plausibly have absorbed the estimated effect. The estimates are unchanged, which suggests the result is not driven by the floods.

6.3 Youth labor market outcomes

The enrollment results show that the gain came mainly from youth who would otherwise have worked without attending school. This raises a natural question about what happened to the jobs these youth would have taken. This subsection examines how the minimum wage changed youth employment across covered employment, uncovered employment, and non-employment. In the model, these outcomes shape the cost of schooling, since what a youth gives up by staying in school depends on the jobs open to them.

Table 8 reports estimates of β for youth employment status from Equation 1, and Figure 6

²¹Appendix F describes the construction of each measure and its sources.

shows the corresponding event studies from [Equation 2](#). Youth are classified into three mutually exclusive categories by employment status alone, so these categories differ from the school–work categories in [subsection 6.1](#). A youth may work and attend school at the same time, and the not employed category includes youth both in and out of school. Column (1) is covered employment, column (2) is uncovered employment, and column (3) is not employed. At baseline, 4.8 percent of youth work in covered employment and 10.0 percent in uncovered employment.

The clearest result appears in covered employment. The pooled estimate in column (1) implies that a 10 percent larger minimum wage increase is associated with a decline in youth covered employment about 1.2 percentage points larger, a quarter of the baseline rate of 4.8 percent. The event study in [Figure 6a](#) shows how this effect evolved. The pre-period is roughly flat. The 2007 and 2008 estimates lie slightly above zero but are not significant, and there is no differential trend in the years just before the policy. After the policy, the yearly estimates are consistently negative, with those for 2013 and 2015 individually significant and the others negative but imprecise.

The point estimate for uncovered employment in column (2) is positive (0.085). Its event study in [Figure 6b](#) is flat from 2011 through 2014 and drifts upward from 2015, though imprecisely. The estimate for non-employment in column (3) is smaller (0.043), and its event study in [Figure 6c](#) fluctuates around zero. Taken at face value, the point estimates suggest that most of the decline in covered employment is offset by uncovered work. However, neither estimate is significant.

These results show that the minimum wage made covered jobs harder for youth to find. A youth who left school became less likely to find a covered job, so this disemployment effect shifted the cost curve down. Youth who kept covered jobs earned more, which pushes the cost curve the other way. However, their raises fell short of the policy. The average daily wage of working youth stayed below 300 Baht after the policy and rose by less than the minimum wage did ([Table 1](#)). Work therefore paid less than the new minimum wage promised for a youth considering leaving school. Since the fall in covered jobs was large while the raises were limited, the downward push on the cost curve likely outweighed the upward one. Together with the adult evidence in [section 5](#), which shows that a diploma became more valuable, the results point to two forces that pulled youth toward school.

6.4 *Heterogeneous effects*

I examine whether the enrollment response differs across youth subgroups, chosen for reasons the schooling decision might vary. I first split by gender, where several margins could produce different responses. Returns to schooling differ between male and female students (Deolalikar, 1993; Dougherty, 2005), girls can face different physical barriers to attendance (Burde and Linden, 2013; Muralidharan and Prakash, 2017), and boys' schooling is especially sensitive to the low-skill labor market (Black et al., 2005a; Cascio and Narayan, 2022). Among adults aged 25 to 40 years before the policy, women are less likely than men to hold covered or uncovered employment and are more likely to be out of the labor force. However, the covered employment payoff to completing high school is more than twice as large for women as for men.²² Therefore, young women would find jobs harder to get, but a diploma would pay them more. Young men would find paid work more readily available, which would raise the cost of staying in school.

Next, I split by whether the household head holds a high school degree or higher. The head is likely to have more say in the schooling decision than other household members and is typically the main earner, so the head's own attainment plausibly shapes both the resources available for schooling and the household's sense of what a diploma is worth in the labor market. The head's education also serves as a proxy for parental education. Intergenerational mobility is measured as the persistence of schooling from parents to children (Hertz et al., 2007; Narayan et al., 2018), so parental attainment is the relevant background characteristic, and the head is the closest measure of it available in this dataset.²³ The evidence on whether parental education increases children's schooling is mixed, some studies finding little effect (Behrman and Rosenzweig, 2002; Black et al., 2005b) and others a positive effect (Suhonen and Karhunen, 2019; Huang et al., 2025). Conditional on individual and household characteristics, youth whose household head holds a high school degree are 15.6 percentage points more likely to be enrolled. This advantage comes almost entirely from a higher share of youth who attend school without working.²⁴ Table 9 reports the results for both splits.

²²Appendix C reports these patterns in Thailand's labor market before the policy.

²³The LFS records each member's relationship to the household head but does not link youth to their own parents, so parental education cannot be constructed directly. Among youth aged 15 to 17, 67 percent are the head's own child and a further 23 percent are the head's grandchild, with the remainder other relatives of the head. Where the head is a grandparent, their attainment reflects a cohort schooled decades earlier under lower average attainment.

²⁴Appendix D reports these estimates.

Columns (1) and (2) of [Table 9](#) split the sample by the household head's education. Youth whose head holds a high school degree enroll at 93.7 percent, close to universal attendance, against 79.0 percent for youth whose head does not. Among youth whose head lacks a high school degree, a 10 percent larger minimum wage increase is associated with an enrollment gain about 1.8 percentage points larger. For youth whose head holds a degree, the estimate is close to zero and not statistically significant. The difference between the two groups is significant at the 10 percent level ($p = 0.075$), so the schooling response is concentrated among youth from less educated households. This pattern is consistent with [Smith \(2021\)](#), who argues that low socioeconomic status teens face a flatter cost curve. With less academic support at home, higher ability may do less to make schooling easier, so the cost of staying in school falls less steeply as ability rises. The same shift in the two curves then moves more youth across the threshold.

Columns (3) and (4) split the sample by gender. Female youth enroll at 86.5 percent, compared with 77.3 percent for male youth. Among male youth, a 10 percent larger minimum wage increase is associated with an enrollment gain about 1.9 percentage points larger. The corresponding gain for female youth is about 0.9 percentage points and is not statistically significant. The difference between the two is not statistically significant ($p = 0.366$), so the larger effect for male youth is suggestive rather than conclusive. The gender differences in the labor market described above suggest why the response may be larger for male youth. A diploma raises covered employment less for men than for women ([Table C.1](#)), so for male youth higher ability may do less to raise the payoff to a diploma, and the benefit curve rises less steeply. Paid work is also more readily available to men, so higher ability may do more to raise what male youth could earn by leaving school, and the cost of staying in school falls less steeply as ability rises. Their net benefit of completing school would then rise more slowly with ability than for female youth, so a given shift in the two curves would bring more male youth into school.

6.5 *Alternative channel: The household budget*

An alternative reading of the enrollment increase runs through the household budget. A minimum wage could affect household resources, which play a role in education decisions in their own right ([Becker and Tomes, 1976, 1979](#)). Such shocks have been shown to move children from work to school in a range of settings, including pension eligibility in South Africa ([Edmonds, 2006](#)) and

cash transfers in Mexico and Malawi (Schultz, 2004; Baird et al., 2011). Edmonds and Pavcnik (2005) study a setting closer to a minimum wage, where a single shock moves both household resources and what a child can earn from working. In Vietnam, rising rice prices raised both, yet child labor fell, so the income channel dominated. Pritadrajati (2021) finds that the Indonesian minimum wage increases raised enrollment among low-income households, but does not identify whether the effect ran through the income channel or the disemployment channel. Whether the household budget contributed in Thailand is therefore an open question. The enrollment response alone cannot answer it, since a minimum wage changes household resources and the availability of youth work at the same time. This subsection takes the question to the Thai case, using household expenditure to look for the trace an income effect would have left.

An income effect that raises enrollment works by loosening the budget, so it would appear as higher household consumption. I use SES data to test whether household expenditure responded to the minimum wage increase, following the specification in Equation 1. The sample retains one observation per household with at least one child aged 15 to 17. I control for dwelling type and construction material rather than a contemporaneous income measure, since the policy may itself shift household income while these housing characteristics move slowly. All expenditure is deflated to constant 2007 Baht using the regional consumer price index.

Four outcomes are examined. Consumption expenditure is the income test, and I report it both in total and net of all education spending. The net measure excludes school fees, textbooks and school equipment, special lessons, and other education expenses, so a child who remains enrolled does not raise it mechanically. Food expenditure is a component of consumption that responds closely to household resources. Households report it in a separate module with a weekly recall period, which the survey converts to a monthly average, so it does not share the measurement error of the consumption block. School fees are the fourth outcome. The policy does not change what schools charge, so school fees respond to the minimum wage only through enrollment and measure what the additional enrollment cost households. The three consumption outcomes enter in logs, and school fees enter as an inverse hyperbolic sine, since 41.7 percent of households report no school fees.

In constant 2007 Baht, total consumption expenditure averages 16,462 Baht per month, and 15,872 Baht net of all education spending. Education therefore accounts for 590 Baht, about 3.6

percent of household consumption. Food expenditure averages 6,470 Baht, roughly 40 percent of the total. School fees average 359 Baht per month, about 2.2 percent of household consumption. The rest of education spending covers special lessons, textbooks, school equipment, and other education expenses. As described in [section 2](#), public schools cannot charge tuition under the 15-Year Free Education Program but may collect other fees. Schooling is therefore not entirely free, but it is inexpensive relative to what a household spends on everything else. [Appendix E](#) reports summary statistics for the household sample.

[Table 10](#) reports the results. None of the four estimates is statistically significant. The three consumption measures test the income channel, and none shows a detectable response, so there is no evidence that the enrollment gain was financed from additional household resources. Their point estimates are mildly negative. If anything, this points away from a loosened budget and could reflect the loss of covered work among less educated adults.

School fees also show no response, so the additional enrollment did not raise what households paid. This fits the low cost of schooling, since a household does not need to spend more to keep a child enrolled. The enrollment gain therefore rests on labor market forces rather than on household budgets. Youth stayed in school because the minimum wage changed the jobs open to them and the value of a diploma, not because their households had more to spend.

7 Conclusion

This paper examines how Thailand's 300 Baht minimum wage policy affected the schooling of youth aged 15 to 17 in a dual labor market. Enrollment increased more in provinces that faced larger minimum wage increases. A province whose minimum wage rose by 10 percent more than another's saw an enrollment gain larger by about 1.4 percentage points, and a decline in the share of youth working without attending school larger by about 1.3 percentage points. Every province received an increase, so these are relative effects. They compare provinces that faced larger and smaller increases and do not measure the total effect of the policy on national enrollment. The gain was larger among youth whose household head lacks a high school degree than among youth whose head holds one, and this difference is statistically significant. In the model, a flatter cost curve in less educated households would produce this pattern, since the same shift in the two

curves then moves more youth into school. The gain was also larger for male youth than for female youth, although the difference between them is not statistically significant.

The model explains this response through the benefit and cost curves of completing school, and the policy moved each curve through a different population. Among youth, covered employment fell. A youth who left school became less likely to find a covered job, which shifted the cost curve down. The minimum wage also raised the pay of youth who kept covered jobs, which pushed the cost curve up. Among adults aged 25 to 40, covered employment shifted away from workers without a diploma and toward workers with one. Finishing school thus came to matter more for holding a covered job, which shifted the benefit curve up. Whichever way the cost curve moved on net, the enrollment gain implies that the benefit curve shifted up relative to the cost curve.

The household budget does not explain the gain. Neither consumption nor food expenditure responds, leaving no sign that household budgets loosened. Households also paid no more in school fees for the additional enrollment. The enrollment gain therefore rests on the labor market.

These findings carry implications for policy in economies with large informal sectors. A minimum wage increase need not draw youth out of school, and it may raise enrollment by changing the jobs available to them and the value of a diploma. However, part of the gain reflects job losses among youth and less educated adults. These costs are worth weighing when such policies are designed. The response also depended on schooling being inexpensive, which suggests that keeping upper secondary education affordable allows youth to stay in school when the labor market changes. Whether the additional enrollment translates into completed schooling and better adult outcomes remains an important question for future work.

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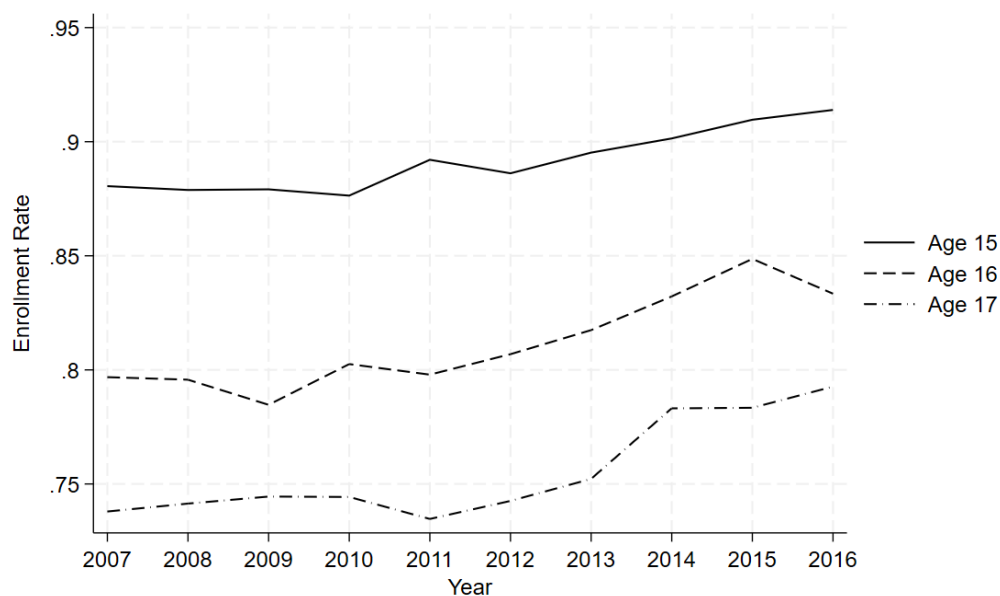
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Figures and Tables

Figure 1: School Enrollment by Age



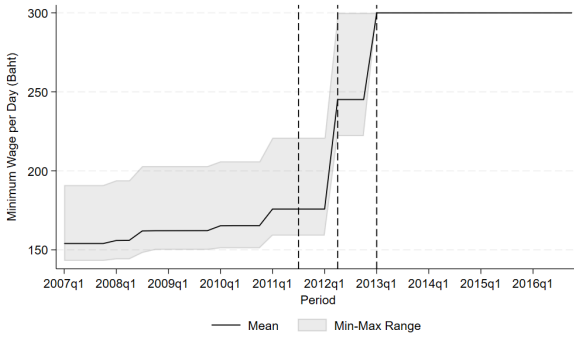
Notes: This figure shows raw school enrollment rates among youth aged 15, 16, and 17, plotted separately by single year of age over the study period (2007–2016). April observations are excluded because schools are on vacation that month. Enrollment is restricted to those attending regular educational institutions. The 300 Baht minimum wage policy was implemented in two phases, in April 2012 and January 2013. Source: LFS.

Table 1: Descriptive Statistics for Youth Aged 15–17

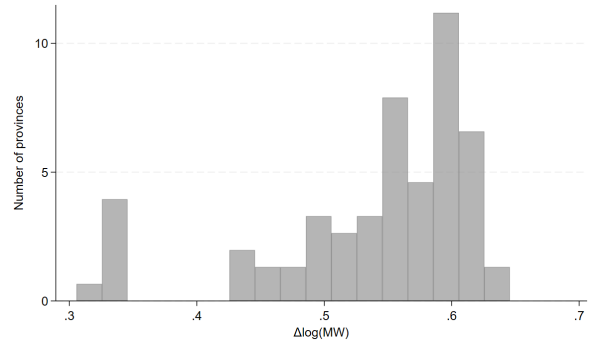
	Pre-period		Post-period	
	Mean	SD	Mean	SD
<i>Individual characteristics</i>				
Male	0.51	0.50	0.51	0.50
Age	16.00	0.82	16.03	0.82
<i>Household characteristics</i>				
Household head male	0.68	0.47	0.63	0.48
Household head's age	49.88	13.23	51.32	13.44
Household head married	0.78	0.42	0.76	0.43
Household head completed HS degree	0.17	0.38	0.21	0.41
Share of household adults (25+) with HS degree	0.17	0.32	0.22	0.35
Municipality (urban)	0.28	0.45	0.37	0.48
<i>Employment & wages</i>				
Working	0.17	0.37	0.12	0.33
Covered	0.05	0.22	0.04	0.20
Uncovered	0.11	0.32	0.08	0.27
Daily wage (Baht)	166.02	52.49	251.49	68.95
<i>Outcomes</i>				
Enrolled in school	0.81	0.40	0.83	0.37
<i>Outcome decomposition</i>				
Enrolled, not employed	0.77	0.42	0.81	0.39
Enrolled, employed	0.04	0.20	0.02	0.15
Not enrolled, employed	0.13	0.33	0.10	0.30
Not enrolled, not employed	0.07	0.25	0.07	0.25
Observations	185,688		159,210	

Notes: This table reports means and standard deviations for youth aged 15–17 in the pre-policy period (2007Q1–2012Q1) and the post-policy period (2012Q2–2016Q4). Working is the sum of covered and uncovered employment. Daily wage is reported in Baht and is conditional on having a recorded wage. All statistics are weighted using survey sampling weights and exclude April observations. Source: LFS.

Figure 2: Minimum Wage in Thailand



(a) Minimum Wage (2007 - 2016)



(b) Minimum Wage Changes by Province

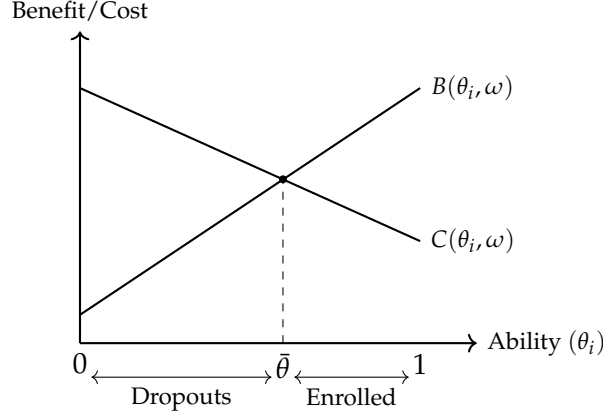
Notes: This figure (a) displays the mean, minimum, and maximum minimum wage across all 76 provinces by quarter throughout the study period (2007–2016). The three vertical dashed lines mark, from left to right, the announcement of the 300 Baht minimum wage policy (the July 2011 election), the first-phase implementation (April 2012), and the second-phase implementation (January 2013). Figure (b) shows the log difference in provincial minimum wages between 2012Q1 and 2013Q1, capturing the overall change in the minimum wage policy implementation phases. Source: Ministry of Labor, Thailand.

Table 2: Province Baseline Characteristics (2007)

	Mean	SD
Agriculture employment share	0.466	0.186
High-education industry share	0.060	0.016
Unemployment rate	0.008	0.004
Enrollment rate of age 15-17	0.796	0.065
Provinces	76	

Notes: This table reports baseline characteristics measured in 2007 for the 76 provinces. Each province's characteristics are computed using survey sampling weights, and the mean and standard deviation are taken across provinces. Adult variables are defined over individuals aged 25 and older. The agriculture employment share is the share of employed adults working in agriculture. The high-education industry share is the share of employed adults working in utilities, finance and business services, or education and health. The unemployment rate is the share of adults in the labor force who are unemployed. The enrollment rate is the share of youth aged 15–17 enrolled in school. Source: LFS.

Figure 3: The Schooling Decision in a Dual Labor Market



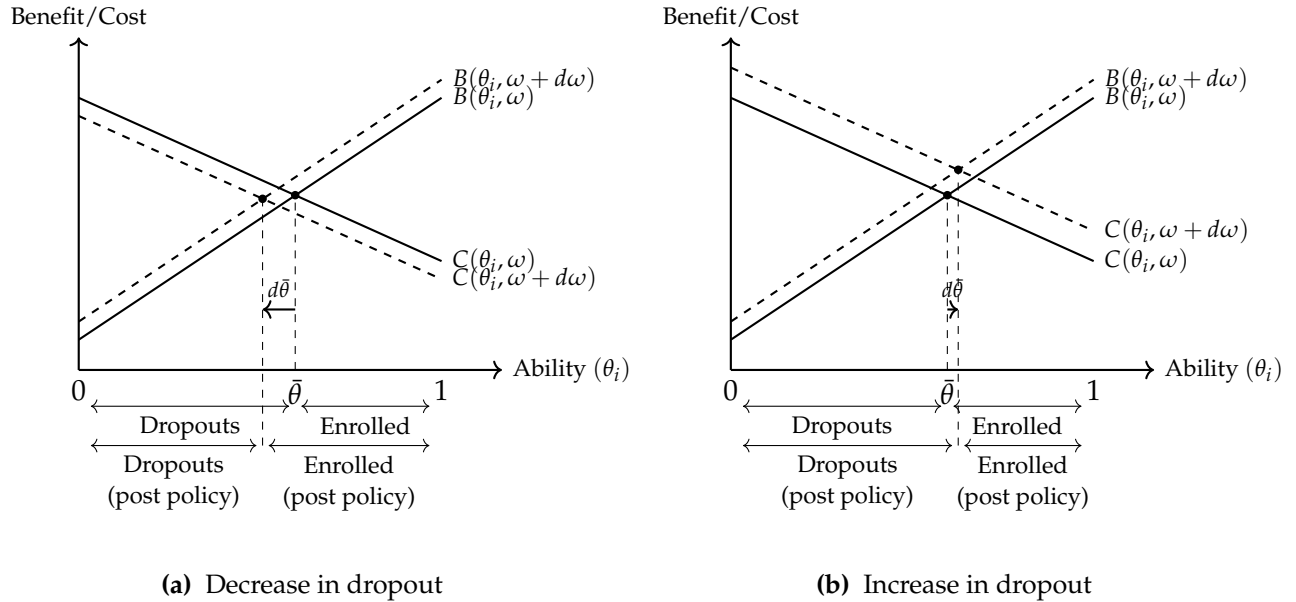
Note: The benefit curve represents the difference in expected present value of lifetime earnings between high school graduates, $EV(1, \theta_i, \omega)$, and dropouts, $EV(0, \theta_i, \omega)$. The cost curve captures both the opportunity cost, $Y_0^{opp}(\theta_i, \omega)$, of foregone earnings and the psychic cost, $\kappa - \rho\theta_i$, of remaining in school.

Table 3: Effects of the Minimum Wage on Employment by Education Level (Adults 25–40)

	Employed (1)	Covered (2)	Uncovered (3)	Not employed (4)
$\Delta \log(MW_p) \times \text{Post}$	-0.074* (0.044)	-0.193*** (0.068)	0.119 (0.083)	0.074* (0.044)
$\Delta \log(MW_p) \times \text{Post} \times \text{HS degree and above}$	0.030 (0.024)	0.336*** (0.059)	-0.307*** (0.062)	-0.030 (0.024)
Total effect (HS degree and above)	-0.045 (0.045)	0.143* (0.074)	-0.188** (0.084)	0.045 (0.045)
Mean dep. var. (Below HS)	0.866	0.333	0.533	0.134
Mean dep. var. (HS degree and above)	0.885	0.537	0.348	0.115
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,855,660	1,855,660	1,855,660	1,855,660
R-squared	0.012	0.162	0.147	0.012

Notes: This table reports the effect of the minimum wage on the labor market status of adults aged 25 to 40, estimated from the specification in Equation 11, which allows the effect to differ by educational attainment. Column (1) reports the effect on employment, defined as working in either covered or uncovered employment. Columns (2) through (4) decompose labor market status into three mutually exclusive and exhaustive categories, namely covered employment, uncovered employment, and not employed, which includes both the unemployed and those out of the labor force. Columns (2) and (3) sum to column (1). The total effect row gives the combined effect for adults with a high school degree or above. Controls include age, age squared, a marital status indicator, an indicator for municipality (urban) residence, and the baseline (2007) agricultural employment share, high-education industry share, and unemployment rate, each interacted with the post indicator. All specifications include province and year fixed effects and are weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

Figure 4: Effect of a Minimum Wage Increase on the Schooling Cutoff



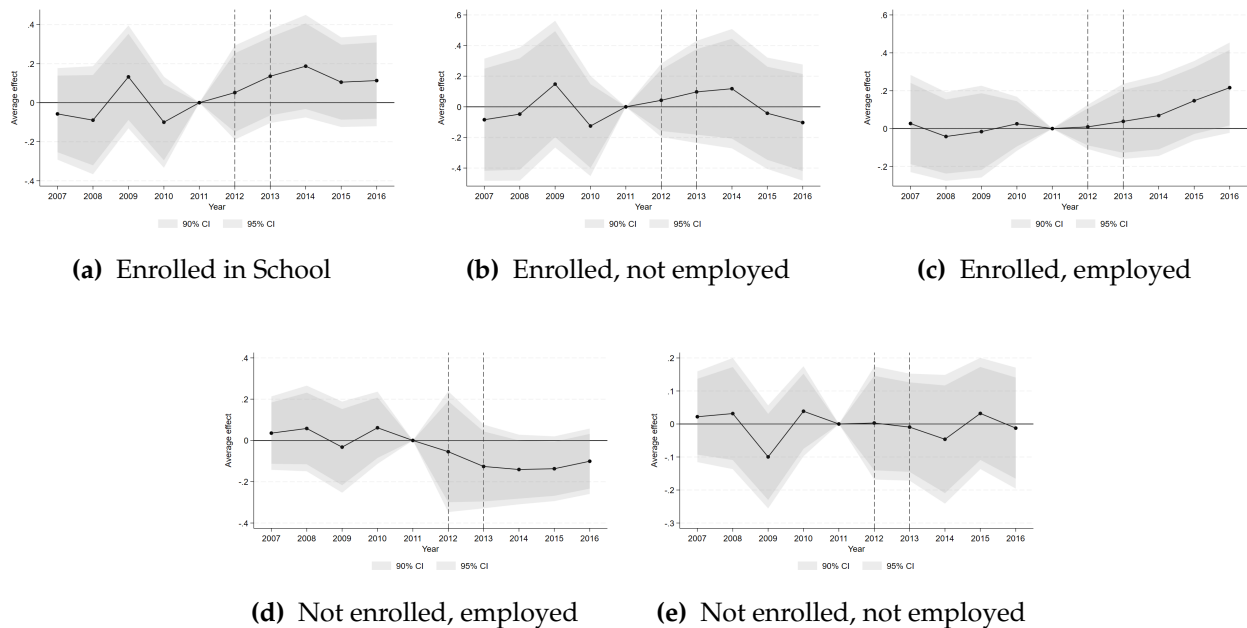
Note: In both panels the benefit curve shifts upward because graduates gain more from the minimum wage increase than dropouts, reflecting their greater access to covered employment. The panels differ in the direction of the cost curve shift. In Panel (a) the disemployment effect dominates, lowering the opportunity cost of schooling and shifting the cutoff $\bar{\theta}$ leftward, so fewer youth drop out. In Panel (b) the wage effect dominates, raising the opportunity cost of schooling and shifting the cutoff rightward, so more youth drop out.

Table 4: Minimum Wage Effects on School Enrollment Among Youth Aged 15–17

	Enrolled school	Enrolled, not employed	Enrolled, employed	Not enrolled, employed	Not enrolled, not employed
	(1)	(2)	(3)	(4)	(5)
$\Delta \log(MW_p) \times \text{Post}$	0.146* (0.081)	0.048 (0.108)	0.098 (0.071)	-0.141** (0.054)	-0.005 (0.056)
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.818	0.785	0.033	0.115	0.067
Observations	344,898	344,898	344,898	344,898	344,898
R-squared	0.087	0.097	0.037	0.081	0.019

Notes: This table reports the effect of the 300 Baht minimum wage policy on youth school enrollment and school–work status, following the specification in Equation 1. The sample consists of youth aged 15–17. The treatment variable is the change in the log provincial minimum wage from 2012Q1 to 2013Q1 interacted with a post-policy indicator. Column (1) reports the effect on overall school enrollment. Columns (2) through (5) decompose youth status into four mutually exclusive categories, namely enrolled and not employed, enrolled and employed, not enrolled and employed, and not enrolled and not employed. All specifications include province and year fixed effects. Individual controls comprise indicators for age, birth cohort, and gender. Household controls comprise the household head’s age, gender, and marital status, the share of household members aged 25 and older with at least a high school education, and an indicator for municipality (urban) residence. All specifications also include the 2007 baseline province characteristics interacted with the post indicator, and are weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

Figure 5: Event Study Estimates of the Enrollment Rate and school-Work Decomposition



Notes: Event study estimates of the effect of the 300 Baht minimum wage policy on youth school enrollment and school-work status among those aged 15–17, following the specification in [Equation 2](#). Panel (a) reports the effect on overall school enrollment. Panels (b) through (e) decompose youth status into four mutually exclusive categories: enrolled and not employed, enrolled and employed, not enrolled and employed, and not enrolled and not employed. Shaded regions denote 90 and 95 percent confidence intervals computed from standard errors clustered at the province level. The vertical dashed lines mark the first (2012) and second (2013) minimum wage increases. Source: LFS.

Table 5: Average Causal Response: Enrollment and School–Work Status

	Enrolled school (1)	Enrolled, not employed (2)	Enrolled, employed (3)	Not enrolled, employed (4)	Not enrolled, not employed (5)
$\Delta \log(MW_p) \times \text{Post}$	0.251** (0.095)	0.189 (0.139)	0.062 (0.102)	-0.166** (0.072)	-0.085 (0.065)
$(\Delta \log(MW_p))^2 \times \text{Post}$	0.877*** (0.300)	1.180* (0.689)	-0.303 (0.587)	-0.204 (0.280)	-0.672*** (0.223)
TWFE coefficient	0.146* (0.081)	0.048 (0.108)	0.098 (0.071)	-0.141** (0.054)	-0.005 (0.056)
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.818	0.785	0.033	0.115	0.067
Observations	344,898	344,898	344,898	344,898	344,898
R-squared	0.087	0.097	0.037	0.081	0.019

Notes: Each column reports a quadratic specification in the dose $\Delta \log(MW_p)$, centered at its survey-weighted mean. The coefficient on the linear term is the average causal response, ACR^0 , which weights each dose by the share of youth facing it (Callaway et al., 2024). The row labeled TWFE coefficient reports the linear specification from Table 4 for comparison. Column (1) is overall enrollment, and columns (2)–(5) are the four mutually exclusive school–work categories. All specifications include province and year fixed effects, individual and household controls, and 2007 baseline province characteristics interacted with the post indicator, and are weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

Table 6: Enrollment by Quartile of Treatment Intensity

	Enrolled in school
Q2 $\times$ Post	0.017* (0.009)
Q3 $\times$ Post	0.031** (0.013)
Q4 $\times$ Post	0.042*** (0.012)
Per log point, Q2	0.135
Per log point, Q3	0.191
Per log point, Q4	0.219
Province FE	Yes
Year FE	Yes
Controls	Yes
Mean of dep. var.	0.818
Observations	344,898

Notes: Provinces are grouped into quartiles of treatment intensity $\Delta \log(MW_p)$, and each quartile is compared with the bottom quartile in a single regression following Equation 1. Quartiles are constructed across the 76 provinces rather than across observations. No province was untreated, so each coefficient measures the difference in the average treatment effect between that quartile and the bottom quartile. “Per log point” divides each coefficient by the difference in mean dose between that quartile and the bottom quartile, placing the estimates on the same scale as the continuous estimate of 0.146 in Table 4. The specification includes province and year fixed effects, individual and household controls, and 2007 baseline province characteristics interacted with the post indicator, and is weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

Table 7: Robustness to Contemporaneous Economic Shocks

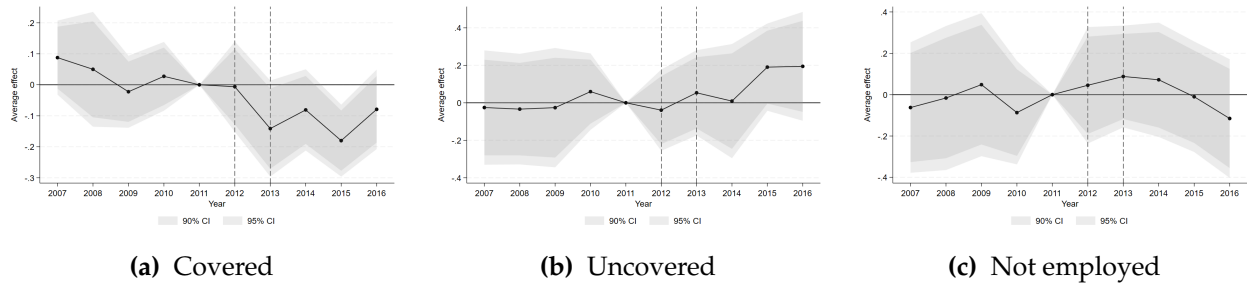
	(1) Baseline	(2) + Rice	(3) + Deferment	(4) + Floods	(5) + All	Mean of dep. var.
Enrolled in school	0.146* (0.081)	0.148* (0.083)	0.135* (0.078)	0.148* (0.080)	0.144* (0.083)	0.818
<i>Decomposition: School–work status</i>						
Enrolled, not employed	0.048 (0.108)	0.040 (0.110)	0.067 (0.105)	0.053 (0.106)	0.085 (0.108)	0.785
Enrolled, employed	0.098 (0.071)	0.109 (0.070)	0.068 (0.063)	0.095 (0.071)	0.059 (0.067)	0.033
Not enrolled, employed	-0.141** (0.054)	-0.148*** (0.054)	-0.127** (0.051)	-0.141** (0.054)	-0.134*** (0.050)	0.115
Not enrolled, not employed	-0.005 (0.056)	-0.000 (0.059)	-0.009 (0.053)	-0.007 (0.056)	-0.010 (0.059)	0.067
Rice pledging exposure	Yes				Yes	
Loan deferment exposure					Yes	
Flood exposure					Yes	
Observations	344,898	344,898	344,898	344,898	344,898	

Notes: Each cell reports the coefficient on $\Delta \log(MW_p) \times Post_t$ from a separate regression following Equation 1 for youth aged 15–17, with rows for overall enrollment and the four mutually exclusive school–work categories. Column (1) reproduces the baseline specification. Columns (2) through (4) each add one exposure measure interacted with the post indicator, for the Rice Pledging Scheme, the loan deferment policy, and the 2011 floods, and column (5) adds all three. All three exposures are measured before the first minimum wage increase in April 2012, using 2008–2010 paddy output, 2011 household debt, and the 2011 floods, so none can respond to the minimum wage change (Appendix F). All specifications include province and year fixed effects, individual and household controls, and 2007 baseline province characteristics interacted with the post indicator, and are weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS, SES, Office of Agricultural Economics, and Poaponsakorn et al. (2014).

Table 8: Effects of the Minimum Wage on Youth Employment Status (Ages 15–17)

	Covered (1)	Uncovered (2)	Not employed (3)
$\Delta \log(MW_p) \times Post$	-0.129*** (0.044)	0.085 (0.083)	0.043 (0.082)
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean of dep. var.	0.048	0.100	0.852
Observations	344,898	344,898	344,898
R-squared	0.046	0.073	0.092

Notes: This table reports the effect of the 300 Baht minimum wage policy on the employment status of youth aged 15–17, following the specification in Equation 1. Column (1) reports the effect on covered employment, column (2) on uncovered employment, and column (3) on non-employment. The three categories are mutually exclusive and exhaustive. The specification includes province and year fixed effects, individual and household controls, and 2007 baseline province characteristics interacted with the post indicator, and is weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

Figure 6: Event Study Estimates of the Minimum Wage Effect on Youth Employment Status

Notes: Event study estimates of the effect of the 300 Baht minimum wage policy on youth employment status among those aged 15–17, following the specification in Equation 2. The three panels report mutually exclusive and exhaustive categories: (a) covered sector employment, (b) uncovered sector employment, and (c) non-employment. Shaded regions denote 90 and 95 percent confidence intervals computed from standard errors clustered at the province level. The vertical dashed lines mark the first (2012) and second (2013) minimum wage increases. Source: LFS.

Table 9: Heterogeneous Effects of the Minimum Wage on School Enrollment

	Household head's educ.		Gender	
	Without HS (1)	With HS (2)	Male (3)	Female (4)
$\Delta \log(MW_p) \times \text{Post}$	0.189* (0.101)	-0.048 (0.079)	0.200* (0.112)	0.090 (0.088)
<i>p</i> -value, difference across groups	0.075		0.366	
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean of dep. var.	0.790	0.937	0.773	0.865
Observations	269,340	75,558	173,032	171,866
R-squared	0.075	0.048	0.093	0.059

Notes: Each column reports the effect of the 300 Baht minimum wage policy on school enrollment among youth aged 15–17, estimated separately by subgroup following Equation 1. The dependent variable is an indicator for school enrollment. Columns (1) and (2) split the sample by whether the household head has completed at least a high school education (HS). Columns (3) and (4) split the sample by gender. The *p*-value tests whether the coefficients differ across the two groups, using a specification that allows all coefficients, fixed effects, and baseline province controls to differ by subgroup, which is equivalent to estimating the two columns separately. All specifications include province and year fixed effects, individual and household controls, and 2007 baseline province characteristics interacted with the post indicator, and are weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

Table 10: Effects of the Minimum Wage on Monthly Household Expenditure

	Consumption exp.			
	Total (1)	Net of education (2)	Food (3)	School fee (4)
$\Delta \log(MW_p) \times \text{Post}$	-0.302 (0.224)	-0.308 (0.228)	-0.175 (0.214)	-0.581 (1.557)
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean of dep. var. (2007 Baht)	16,462	15,872	6,472	359
Observations	52,449	52,449	52,441	52,449
R-squared	0.457	0.446	0.361	0.266

Notes: This table reports the effect of the minimum wage on monthly household expenditure, estimated from the specification in Equation 1. The sample is one observation per household with at least one child aged 15 to 17. All expenditure is measured in constant 2007 Baht, deflated by the regional consumer price index (2007 = 100). Column (1) is total consumption expenditure, column (2) subtracts all education spending from column (1), column (3) is food expenditure, and column (4) is school fees. The dependent variable in columns (1)–(3) is the natural log of expenditure. Column (4) uses the inverse hyperbolic sine instead, since 41.7 percent of households report no school fees. Eight households report zero food expenditure and are dropped from column (3). Controls comprise the household head's age, gender, and marital status, an indicator for municipality (urban) residence, the share of household members aged 25 and older with at least a high school education, the number of children in each of three age bands (6–11, 12–14, 15–17), dwelling type, and construction material. All specifications include province and year fixed effects and the 2007 baseline province agricultural employment share, high-education industry share, unemployment rate, and youth enrollment rate, each interacted with the post indicator. Regressions are weighted by household sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: SES.

Appendices

A The Dual Labor Market Model

This appendix derives the results summarized in [section 5](#). Youth differ in ability $\theta_i \in [0, 1]$ and face a minimum wage ω . Each youth chooses whether to complete high school ($s = 1$) or to leave school at $t = 0$ and enter the labor market ($s = 0$). An adult with schooling s holds a covered job with probability $\pi_V(s, \theta_i, \omega)$, holds an uncovered job with probability $\pi_U(s, \theta_i, \omega)$, and is otherwise not employed, which earns zero. Expected lifetime earnings are

$$EV(s, \theta_i, \omega) = \pi_V(s, \theta_i, \omega) PV^V(s, \theta_i, \omega) + \pi_U(s, \theta_i, \omega) PV^U(s, \theta_i, \omega), \quad (1)$$

where $PV^X(s, \theta_i, \omega) = \sum_{t=1}^T \beta^t Y_t^X(s, \theta_i, \omega)$ is the present value of earnings in job type $X \in \{V, U\}$, Y_t^X is earnings in period t , T is the last working period, and $\beta \in (0, 1)$ is the discount factor. The benefit of completing high school is $B(\theta_i, \omega) = EV(1, \theta_i, \omega) - EV(0, \theta_i, \omega)$. A youth who leaves school at $t = 0$ finds a covered job with probability $\pi_{V,0}(\theta_i, \omega)$ and an uncovered job with probability $\pi_{U,0}(\theta_i, \omega)$, so the cost of completing high school is

$$C(\theta_i, \omega) = \pi_{V,0}(\theta_i, \omega) Y_0^V(\theta_i, \omega) + \pi_{U,0}(\theta_i, \omega) Y_0^U(\theta_i, \omega) + \kappa - \rho\theta_i, \quad (2)$$

where the first two terms are the earnings a youth forgoes by staying in school, with Y_0^V and Y_0^U the period-zero earnings in each job type, and $\kappa - \rho\theta_i$, with $\kappa, \rho > 0$, is the idiosyncratic cost of schooling. As in the main text, covered jobs pay more than uncovered jobs, so $PV^V > PV^U$ and $Y_0^V > Y_0^U$. Below, I write $\partial/\partial\theta$ for the derivative with respect to ability and suppress the arguments (θ_i, ω) where they are clear from context.

The benefit rises and the cost falls with ability, so $B - C$ increases in θ_i and the two curves cross at most once. Since $B(0, \omega) < C(0, \omega)$ and $B(1, \omega) > C(1, \omega)$, and both curves are continuous in ability, they cross exactly once, at the threshold $\bar{\theta}$ where

$$B(\bar{\theta}, \omega) = C(\bar{\theta}, \omega). \quad (3)$$

Youth below the threshold drop out, and youth at or above it complete high school, so the dropout rate is the share of youth with ability below $\bar{\theta}$. Totally differentiating [Equation 3](#) with respect to ω and rearranging gives

$$\frac{d\bar{\theta}}{d\omega} = - \frac{\frac{\partial B}{\partial \omega} - \frac{\partial C}{\partial \omega}}{\frac{\partial B}{\partial \theta} - \frac{\partial C}{\partial \theta}}, \quad (4)$$

with all terms evaluated at $\bar{\theta}$. The denominator is positive because $B - C$ increases in ability. The threshold therefore falls, and the dropout rate with it, if and only if $\frac{\partial B}{\partial \omega} > \frac{\partial C}{\partial \omega}$. For a given shift in the curves, the threshold moves more where $B - C$ rises slowly with ability, and the dropout rate changes more where many youth have ability close to the threshold.

Differentiating [Equation 1](#) separates the response of expected earnings into a disemployment effect, which changes the chance of holding each type of job, and a wage effect, which changes what those jobs pay,

$$\frac{\partial EV(s)}{\partial \omega} = \underbrace{\frac{\partial \pi_V(s)}{\partial \omega} PV^V(s) + \frac{\partial \pi_U(s)}{\partial \omega} PV^U(s)}_{\text{disemployment effect}} + \underbrace{\pi_V(s) \frac{\partial PV^V(s)}{\partial \omega} + \pi_U(s) \frac{\partial PV^U(s)}{\partial \omega}}_{\text{wage effect}}. \quad (5)$$

The cost has the same structure with the youth probabilities and period-zero earnings, since the idiosyncratic cost does not depend on ω . Taking the difference between graduates and dropouts, the response of the benefit has four parts,

$$\begin{aligned} \frac{\partial B}{\partial \omega} = & \underbrace{\frac{\partial \pi_V(1)}{\partial \omega} PV^V(1) + \frac{\partial \pi_U(1)}{\partial \omega} PV^U(1)}_{\text{disemployment effect, graduates}} - \underbrace{\left(\frac{\partial \pi_V(0)}{\partial \omega} PV^V(0) + \frac{\partial \pi_U(0)}{\partial \omega} PV^U(0) \right)}_{\text{disemployment effect, dropouts}} \\ & + \underbrace{\pi_V(1) \frac{\partial PV^V(1)}{\partial \omega} - \pi_V(0) \frac{\partial PV^V(0)}{\partial \omega}}_{\text{covered wage effect}} + \underbrace{\pi_U(1) \frac{\partial PV^U(1)}{\partial \omega} - \pi_U(0) \frac{\partial PV^U(0)}{\partial \omega}}_{\text{uncovered wage effect}}. \end{aligned} \quad (6)$$

The benefit curve shifts up if these four parts together favor graduates.

The main text assumes that the minimum wage raises pay in covered jobs for graduates and dropouts alike but leaves pay in uncovered jobs unchanged, so the uncovered wage effect is zero. The benefit curve then shifts up if the other four effects favor graduates. The covered wage effect does not favor dropouts when

$$\pi_V(1) \frac{\partial PV^V(1)}{\partial \omega} \geq \pi_V(0) \frac{\partial PV^V(0)}{\partial \omega}. \quad (7)$$

This can hold even if dropouts in covered jobs receive larger raises than graduates in covered jobs, provided that graduates are more likely to hold covered jobs. The disemployment effect favors graduates when covered jobs shift from dropouts toward graduates. Suppose dropouts lose covered jobs and their overall employment does not rise, so that $\frac{\partial \pi_V(0)}{\partial \omega} + \frac{\partial \pi_U(0)}{\partial \omega} \leq 0$. Since earnings are non-negative and covered jobs pay more, the expected earnings of dropouts then fall,

$$\frac{\partial \pi_V(0)}{\partial \omega} PV^V(0) + \frac{\partial \pi_U(0)}{\partial \omega} PV^U(0) \leq \frac{\partial \pi_V(0)}{\partial \omega} (PV^V(0) - PV^U(0)) < 0.$$

Suppose also that graduates gain covered jobs and that any loss of uncovered work satisfies

$$-\frac{\partial \pi_U(1)}{\partial \omega} PV^U(1) \leq \frac{\partial \pi_V(1)}{\partial \omega} PV^V(1). \quad (8)$$

The expected earnings of graduates then do not fall. This condition holds whenever graduates lose no more uncovered jobs than they gain covered ones. It can also hold when they lose more uncovered jobs, as long as covered jobs pay enough more to make up the difference. Under these conditions, $\frac{\partial B}{\partial \omega} > 0$ and the benefit curve shifts up.

The benefit curve can still shift up if uncovered pay also rises. If the minimum wage raises pay in uncovered jobs by the same amount for graduates and dropouts, the uncovered wage effect becomes

$$-(\pi_U(0) - \pi_U(1)) \frac{\partial PV^U}{\partial \omega}. \quad (9)$$

This term favors dropouts if they are more likely to hold uncovered jobs. It would favor graduates only if each graduate's raise in uncovered jobs exceeded each dropout's raise by at least the factor $\pi_U(0)/\pi_U(1)$. In either case, the benefit curve still shifts up as long as the uncovered wage effect is smaller than the gain to graduates from the other four effects. Since the minimum wage reaches uncovered jobs only through spillovers, the uncovered wage effect is likely small.

Under the main text's assumption, the response of the cost is

$$\frac{\partial C}{\partial \omega} = \underbrace{\frac{\partial \pi_{V,0}}{\partial \omega} Y_0^V + \frac{\partial \pi_{U,0}}{\partial \omega} Y_0^U}_{\text{disemployment effect}} + \underbrace{\pi_{V,0} \frac{\partial Y_0^V}{\partial \omega}}_{\text{wage effect}}. \quad (10)$$

The wage effect is positive, since youth who hold covered jobs earn more. If youth lose covered jobs and their overall employment does not rise, the same argument as for dropouts shows that the disemployment effect is negative. Unlike the benefit curve, the cost curve involves only youth who leave school, so the two effects are not compared across groups. The cost curve shifts down if the disemployment effect is larger and up if the wage effect is larger. If uncovered pay also rises, the cost gains a positive term, $\pi_{U,0} \frac{\partial Y_0^U}{\partial \omega}$, which raises the cost of staying in school.

When the benefit curve shifts up, the dropout rate falls if the cost curve shifts down or shifts up by less than the benefit curve, and it rises only if the cost curve shifts up by more. The model does not determine which case holds, so the direction of the dropout response is an empirical question.

B Adult Labor Market Sample (Ages 25-40)

The adult sample characterizes the labor market that youth anticipate. I restricted the analysis to individuals 25 to 40 years old for several reasons. Younger adults under 25 may still be pursuing higher education, and Thai men who serve in the military typically complete their service by their mid-twenties, having served between six months and two years depending on their education level and volunteer status. Restricting to age 25 and above also ensures the adult sample does not overlap with the youth aged 15 to 17 whose enrollment response is the focus of the paper, so the adult labor market outcomes reflect the conditions youth anticipate rather than their own contemporaneous treatment. This age range therefore captures workers who are expected to be active labor market participants and whose outcomes plausibly represent the labor market that youth expect to enter.

Table B.1 summarizes the adult sample. Adults aged 25 to 40 are highly attached to the labor market, with 87 percent working and 13 percent not employed in both the pre- and post-policy periods. Although the share working stays fixed, its split between the two sectors shifts. The covered employment share rises from 39 percent before the policy to 45 percent afterward, while the uncovered employment share falls from 48 to 43 percent. The sample also becomes more educated and more urban. The share holding at least a high school degree rises from 37 to 48 percent, and the municipality (urban) share rises from 34 to 44 percent. The increase in attainment reflects cohort turnover rather than schooling completed after age 25. The average daily wage rises from 290 to 418 Baht, an increase of about 44 percent that reflects both the policy and broader wage growth.

To let the response to the minimum wage differ by education, I estimate

$$Y_{ipt} = \alpha + \beta_1 (\Delta \log MW_p \times Post_t) + \beta_2 (\Delta \log MW_p \times Post_t \times HS_i) + \beta_3 HS_i + \beta_4 (Post_t \times HS_i) + \gamma X_{ipt} + \delta (Z_{p,2007} \times Post_t) + \theta_p + \theta_t + \epsilon_{ipt} \quad (11)$$

where Y_{ipt} is an indicator for a labor market outcome of individual i in province p at time t . The outcomes are employment and three mutually exclusive labor market states, namely covered employment, uncovered employment, and not employed. Covered and uncovered employment sum to employment. The indicator HS_i equals one for individuals holding at least a high school degree. The coefficient β_1 gives the response for adults without a high school degree, and β_2 gives the difference for adults with one. Their sum $\beta_1 + \beta_2$ is reported as the total effect and gives the response for adults with a degree. The term β_4 allows the two education groups to follow different post-policy trends, so that β_2 captures only the part of the divergence that scales with treatment intensity. It also absorbs the rise in attainment across periods, to the extent that it is common across provinces. Treatment intensity and the post indicator are as defined in section 4. The vector X_{ipt} comprises age, age squared, marital status, and a municipality (urban) indicator. The vector $Z_{p,2007}$ comprises the 2007 agricultural employment share, high-education industry share, and unemployment rate. All regressions are weighted by survey sampling weights, and standard errors are clustered at the province level.

Table 3 shows that the minimum wage shifted covered employment away from adults without a diploma and toward adults with one. At baseline, adults without a diploma are concentrated in uncovered employment (53.3 percent), while adults with a diploma are concentrated in covered employment (53.7 percent). Among adults without a diploma, a 10 percent larger minimum wage increase is associated with a decline in employment about 0.7 percentage points larger. Covered employment falls by about 1.8 percentage points more. Uncovered employment does not rise reliably in response, so part of the loss of covered work appears as exit from employment.

For adults with a diploma, covered employment moves in the opposite direction. Their overall employment changes little, but its composition shifts toward covered work. A 10 percent larger minimum wage increase is associated with covered employment about 1.4 percentage points higher and uncovered employment about 1.8 percentage points lower. The interaction term is large and significant for both covered and uncovered employment, enough to reverse the sign of the covered employment response between the two groups. A diploma increasingly determines whether an adult holds a covered job.

Table B.1: Descriptive Statistics for Adults Aged 25–40

	Pre-period (2007Q1 - 2012Q1)		Post-period (2012Q2 - 2016Q4)	
	Mean	SD	Mean	SD
<i>Individual characteristics</i>				
Age	32.61	4.65	32.67	4.68
Male	0.49	0.50	0.50	0.50
Whether married	0.71	0.45	0.67	0.47
HS degree and above (1)	0.37	0.48	0.48	0.50
Municipality (urban)	0.34	0.47	0.44	0.50
<i>Employment status</i>				
Working	0.87	0.33	0.87	0.33
Covered	0.39	0.49	0.45	0.50
Uncovered	0.48	0.50	0.43	0.49
Daily wage (Baht)	290.42	202.83	418.29	230.87
Observations	1,009,751		845,909	

Notes: This table reports means and standard deviations for adults aged 25–40 in the pre-policy period (2007Q1–2012Q1) and the post-policy period (2012Q2–2016Q4). The three employment categories, covered employment, uncovered employment, and not employed, are mutually exclusive and exhaustive. Working is the sum of covered and uncovered employment, and not employed includes both the unemployed and those out of the labor force. HS degree and above indicates completion of at least a high school education. Daily wage is reported in Baht and is conditional on having a recorded wage. All statistics are weighted by survey sampling weights. Source: LFS.

C Education Decision by Gender

Thailand shows distinct enrollment trends by gender. [Figure C.1](#) plots raw enrollment rates for youth aged 15, 16, and 17 over the sample period, separately for males and females. Enrollment declines monotonically with age in each gender. Both panels show a broadly rising trend and these age patterns are similar between genders. The level difference, with girls more likely to enroll, points to the labor market as a natural place to look for an explanation.

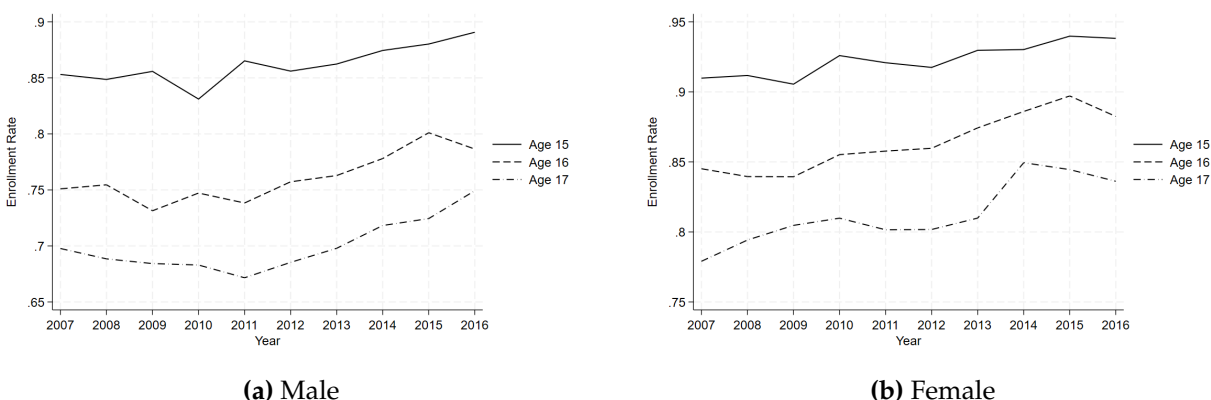
[Table C.1](#) reports the probability of each labor market state by education and gender among adults aged 25 to 40 in the pre-policy period. I focus on this age range because it plausibly captures the labor market that youth expect to enter, and so the returns they weigh when deciding whether to stay in school. The descriptive picture is one of widespread uncovered employment. Nearly half of adults work in uncovered employment and about 40 percent in covered employment, while about 13 percent are not employed.

Two patterns stand out. First, among adults without a high school degree, women are less attached to paid work than men. They are about 10 percentage points less likely to hold covered employment and about 5 percentage points less likely to hold uncovered employment, and about 15 percentage points more likely to be not employed. Second, completing high school carries a substantial payoff that reshapes the kind of work adults do, and this payoff is much greater for women. For men, a high school degree and above increases the probability of covered employment by 7.3 percentage points, drawing workers out of uncovered employment and not in the labor force. For women, the same degree is associated with about an 18 percentage point higher probability of covered employment, more than double that of men, along with a 13 percentage point lower probability of uncovered employment and a 5 percentage point lower probability of not being employed. Among adults with a degree, the gender gap in covered employment essentially disappears. The gain in access to covered employment from finishing high school is therefore considerably greater for women than for men.

[Table C.2](#) decomposes the school-work status of youth aged 15 to 17 into four mutually exclusive categories, estimated separately by gender using the specification in [Equation 1](#). The response is concentrated in a single category and a single group. Among male youth, a 10 percent larger minimum wage increase is associated with a decline in the share not enrolled and employed about 2.4 percentage points larger. The corresponding estimate for female youth is small and insignificant. None of the remaining categories is significant for either gender.

[Table C.3](#) shows which employment states account for this pattern. Among male youth, a 10 percent larger minimum wage increase is associated with a decline in covered employment about 1.8 percentage points larger. The corresponding estimate for female youth is smaller and insignificant. The uncovered and non-employment estimates are not significant for either gender, but their point estimates differ. For male youth, the decline in covered employment is offset mainly by non-employment, which includes youth in school. For female youth, the offset appears mainly in uncovered work.

Figure C.1: School Enrollment by Gender



Notes: This figure shows enrollment rates by age group and gender for those attending regular schooling institutions, excluding April due to school vacation. The minimum wage policy happened in 2012 April and 2013 January. Source: Labor Force Survey.

Table C.1: Employment Probability by Education and Gender (Adults 25–40, Pre-Period)

	Covered (1)	Uncovered (2)	Not employed (3)
Female	-0.098*** (0.005)	-0.053*** (0.006)	0.151*** (0.005)
HS degree and above	0.073*** (0.009)	-0.066*** (0.007)	-0.007 (0.005)
HS degree and above × female	0.110*** (0.007)	-0.063*** (0.008)	-0.047*** (0.005)
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean of dep. var.	0.394	0.481	0.126
Observations	1,009,751	1,009,751	1,009,751
R-squared	0.161	0.147	0.053

Notes: This table reports pre-period (2007Q1–2012Q1) associations between labor market status and both gender and education for adults aged 25 to 40. The estimates are conditional associations rather than policy effects, since the sample predates the reform. The dependent variable in each column is an indicator for one of three mutually exclusive labor market states, namely covered employment, uncovered employment, and not employed, which includes both the unemployed and those out of the labor force. The specification includes indicators for being female and for holding at least a high school degree, together with their interaction. All specifications include province and year fixed effects and controls for age, age squared, marital status, and municipality (urban) residence. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

Table C.2: Minimum Wage Effects on School–Work Status by Gender

	Male				Female			
	Enrolled, not employed (1)	Enrolled, employed (2)	Not enrolled, employed (3)	Not enrolled, not employed (4)	Enrolled, not employed (5)	Enrolled, employed (6)	Not enrolled, employed (7)	Not enrolled, not employed (8)
$\Delta \log(MW_p) \times \text{Post}$	0.091 (0.133)	0.109 (0.078)	-0.257*** (0.077)	0.058 (0.085)	0.003 (0.111)	0.086 (0.068)	-0.024 (0.058)	-0.065 (0.056)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.735	0.038	0.161	0.067	0.837	0.027	0.068	0.068
Observations	173,032	173,032	173,032	173,032	171,866	171,866	171,866	171,866
R-squared	0.106	0.041	0.084	0.022	0.064	0.033	0.042	0.022

Notes: This table reports the effect of the minimum wage on the school–work status of youth aged 15–17, estimated separately by gender following the specification in Equation 1. Columns (1)–(4) report estimates for male youth and columns (5)–(8) for female youth, each covering the four mutually exclusive school–work categories. All specifications include province and year fixed effects, individual and household controls, and 2007 baseline province characteristics interacted with the post indicator, and are weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

Table C.3: Effects of the Minimum Wage on Youth Employment Status by Gender

	Male			Female		
	Covered (1)	Uncovered (2)	Not employed (3)	Covered (4)	Uncovered (5)	Not employed (6)
$\Delta \log(MW_p) \times \text{Post}$	-0.189*** (0.069)	0.040 (0.098)	0.149 (0.110)	-0.067 (0.040)	0.129 (0.080)	-0.062 (0.076)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.065	0.133	0.802	0.030	0.065	0.905
Observations	173,032	173,032	173,032	171,866	171,866	171,866
R-squared	0.047	0.081	0.099	0.035	0.044	0.050

Notes: This table reports the effect of the 300 Baht minimum wage policy on the employment status of youth aged 15–17, estimated separately by gender following the specification in Equation 1. Columns (1)–(3) report estimates for male youth and columns (4)–(6) for female youth, on covered employment, uncovered employment, and non-employment, respectively. Within each gender, the three categories are mutually exclusive and exhaustive. All specifications include province and year fixed effects, individual and household controls, and 2007 baseline province characteristics interacted with the post indicator, and are weighted by survey sampling weights. Gender is omitted from the individual controls, since the sample is split by gender. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

D Education Decision by Household Head Education

[Table D.1](#) documents how the household head's education relates to youth schooling in the pre-policy period (2007–2012Q1). The estimates describe baseline associations rather than policy effects, and motivate the division of the heterogeneity analysis by the head's education.

The association is strong. Youth whose household head holds a high school degree are 15.6 percentage points more likely to be enrolled, against a sample mean of 80.6 percent. The decomposition shows that this gap is more than accounted for by the pure-student category, where the association is 16.8 percentage points. The largest offset comes from the share not enrolled and employed, which is 10.8 percentage points lower, followed by a 4.8 point reduction in the share neither enrolled nor employed. Youth with an educated head are also slightly less likely to combine school with work. The head's education is therefore a strong predictor of where a youth sits on the school-work margin, which is what makes it a natural dimension along which to expect the minimum wage response to vary.

[Table D.2](#) reports estimates for each school-work category, estimated separately by the head's education. The two groups start from very different positions. Among youth whose head holds a degree, 91.5 percent are enrolled and not employed and 3.6 percent are not enrolled and employed. Among youth whose head does not, these shares are 75.5 and 13.3 percent. The response is concentrated in the not enrolled and employed category and in the group more often in that state. Among youth whose head lacks a degree, a 10 percent larger minimum wage increase is associated with a decline in the share not enrolled and employed about 1.6 percentage points larger. None of the remaining categories is significant for either group. However, For youth whose head lacks a degree, the point estimates in the two enrolled categories are positive and sum to the enrollment gain reported in [subsection 6.4](#).

[Table D.3](#) shows which employment states account for this pattern. Among youth whose head lacks a high school degree, a 10 percent larger minimum wage increase is associated with a decline in covered employment about 1.4 percentage points larger. The corresponding estimate for youth whose head holds a degree is essentially zero. Neither the uncovered nor the non-employment estimate is significant for either group. Therefore, the decline in covered employment is concentrated among youth whose head lacks a high school degree, the same group in which the enrollment gain appears.

Table D.1: Association Between Household Head Education and Youth Enrollment (Pre-Period)

	Enrolled school (1)	Enrolled, not employed (2)	Enrolled, employed (3)	Not enrolled, employed (4)	Not enrolled, not employed (5)
HH head with HS degree	0.156*** (0.006)	0.168*** (0.006)	-0.012*** (0.002)	-0.108*** (0.005)	-0.048*** (0.004)
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.806	0.765	0.040	0.127	0.067
Observations	185,688	185,688	185,688	185,688	185,688
R-squared	0.080	0.091	0.046	0.075	0.016

Notes: This table reports the pre-policy (2007Q1–2012Q1) association between the household head’s education and the school–work status of youth aged 15–17. The estimates are conditional associations rather than policy effects, since the sample predates the reform. The explanatory variable of interest is an indicator for whether the household head has completed at least a high school education. Column (1) reports the association with overall enrollment, and columns (2)–(5) with the four mutually exclusive school–work categories. All specifications include province and year fixed effects, indicators for age, birth cohort, and gender, the household head’s age, gender, and marital status, and an indicator for municipality (urban) residence. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

Table D.2: Minimum Wage Effects on School–Work Status by Household Head’s Education

	Head without HS				Head with HS			
	Enrolled, not employed (1)	Enrolled, employed (2)	Not enrolled, employed (3)	Not enrolled, not employed (4)	Enrolled, not employed (5)	Enrolled, employed (6)	Not enrolled, employed (7)	Not enrolled, not employed (8)
$\Delta \log(MW_p) \times \text{Post}$	0.088 (0.133)	0.101 (0.078)	-0.168** (0.065)	-0.021 (0.070)	-0.103 (0.083)	0.055 (0.046)	0.006 (0.048)	0.042 (0.049)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.755	0.035	0.133	0.077	0.915	0.022	0.036	0.026
Observations	269,340	269,340	269,340	269,340	75,558	75,558	75,558	75,558
R-squared	0.084	0.037	0.077	0.015	0.061	0.040	0.041	0.013

Notes: This table reports the effect of the minimum wage on the school–work status of youth aged 15–17, estimated separately by the household head’s education following the specification in [Equation 1](#). Columns (1)–(4) report estimates for youth whose household head has not completed high school and columns (5)–(8) for youth whose head has completed at least a high school education, each covering the four mutually exclusive school–work categories. All specifications include province and year fixed effects, individual and household controls, and 2007 baseline province characteristics interacted with the post indicator, and are weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

Table D.3: Effects of the Minimum Wage on Youth Employment Status by Household Head's Education

	Head without HS			Head with HS		
	Covered (1)	Uncovered (2)	Not employed (3)	Covered (4)	Uncovered (5)	Not employed (6)
$\Delta \log(MW_p) \times \text{Post}$	-0.143*** (0.052)	0.076 (0.097)	0.067 (0.098)	0.001 (0.039)	0.060 (0.046)	-0.061 (0.060)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.055	0.113	0.832	0.018	0.041	0.941
Observations	269,340	269,340	269,340	75,558	75,558	75,558
R-squared	0.047	0.071	0.087	0.033	0.048	0.058

Notes: This table reports the effect of the 300 Baht minimum wage policy on the employment status of youth aged 15–17, estimated separately by the household head's education following the specification in Equation 1. Columns (1)–(3) report estimates for youth whose household head has not completed high school and columns (4)–(6) for youth whose head has completed at least a high school education, on covered employment, uncovered employment, and non-employment, respectively. Within each group, the three categories are mutually exclusive and exhaustive. All specifications include province and year fixed effects, individual and household controls, and 2007 baseline province characteristics interacted with the post indicator, and are weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

E Household Expenditure Sample

The expenditure analysis in [subsection 6.5](#) is based on the Socio-Economic Survey (SES), which records household consumption and education spending. The sample retains one observation per household with at least one child aged 15 to 17, matching the age range of the enrollment analysis. [Table E.1](#) reports weighted means and standard deviations separately for the pre-policy and post-policy periods.

The household composition is stable across the two periods. Heads are male in roughly two-thirds of households and married in about three-quarters in both, and mean head age rises only from 50.1 to 51.5 years. The number of children aged 15 to 17 is fixed at 1.06 by construction, since the sample requires at least one, and the younger age bands thin slightly as the household size falls from 4.31 to 4.18 members. That decline is consistent with Thailand's decreasing fertility over the period.

Two characteristics shift more noticeably. The share of household adults who have a high school degree increases from 0.17 to 0.22, reflecting cohort turnover among adults aged 25 and older rather than the schooling completed during the period. The urban share increases from 0.30 to 0.36 and housing characteristics move along with it, with cement or brick construction rising from 0.45 to 0.52 while wood dwellings fall from 0.25 to 0.18. Dwelling type is otherwise stable, with detached houses accounting for roughly 85 percent of the sample throughout.

The outcome variables rise across the two periods, as nominal expenditure would over a decade. The total consumption expenditure increases from 16,372 to 21,220 Baht per month. Since a child who remains enrolled mechanically raises education spending, I also report consumption net of all education expenses, which increases from 15,737 to 20,534. Food expenditure is a component of consumption that closely moves with household resources. The survey collects it in its own module with a weekly recall period that it converts to a monthly average, so it does not share the measurement error of the consumption block. It rises from 6,431 to 8,373. All three increase by roughly 30 percent. School fees rise from 363 to 446 Baht per month and special lesson spending from 89 to 94. The distributions of the two education measures are heavily skewed, since a minority of households spending on private education account for much of the total.

Table E.1: Summary Statistics for the Household Expenditure Sample

	Pre-period		Post-period	
	Mean	SD	Mean	SD
<i>Household characteristics</i>				
Household head male	0.69	0.46	0.65	0.48
Household head's age	50.14	13.09	51.48	13.38
Household head married	0.77	0.42	0.76	0.43
Household adult share with HS degree	0.17	0.32	0.22	0.35
Municipality (urban)	0.30	0.46	0.36	0.48
Household size	4.31	1.54	4.18	1.50
No. of children aged 6-11	0.34	0.58	0.28	0.53
No. of children aged 12-14	0.24	0.46	0.18	0.41
No. of children aged 15-17	1.06	0.26	1.06	0.26
<i>Dwelling type</i>				
Detached house	0.86	0.34	0.84	0.37
Row house	0.08	0.28	0.09	0.29
Town house/twin house	0.04	0.19	0.05	0.22
Flat or apartment	0.01	0.08	0.02	0.13
Room or rooms	0.00	0.06	0.00	0.06
Improvised quarter	0.00	0.06	0.00	0.03
Others	0.00	0.02	0.00	0.01
<i>Construction material</i>				
Cement or brick	0.45	0.50	0.52	0.50
Wood	0.25	0.43	0.18	0.38
Wood and cement or brick	0.29	0.45	0.29	0.45
Local materials	0.01	0.09	0.00	0.07
Re-used materials	0.00	0.03	0.00	0.03
Others	0.00	0.06	0.00	0.04
Outcomes (Baht per month)				
Consumption expenditure	16,372.09	13,864.09	21,219.89	18,585.97
Consumption expenditure excluding education	15,736.59	13,131.21	20,533.59	17,628.59
Food, beverage, and tobacco expenditure	6,431.13	3,556.73	8,372.91	4,868.87
School fees	362.91	1,296.38	446.19	1,678.67
Special lesson expenditure	89.03	463.28	93.80	508.48
Observations	28,094		24,355	

Notes: This table reports means and standard deviations for the estimation sample used in the household expenditure analysis. The pre-period is 2007–2011 and the post-period is 2012–2016. The sample is one observation per household with at least one child aged 15 to 17. Consumption expenditure excluding education covers all household consumption other than school fees, textbooks, special lessons, and other education expenses. Food expenditure covers food, beverages, and tobacco, which the survey records in a separate module. All expenditure figures are monthly averages in nominal Baht. All statistics are weighted by household sampling weights. Source: SES.

F Data Source and Construction

F.1 *Minimum wage*

Provincial minimum wage rates come from the official Wage Committee Notifications issued by the Ministry of Labour of Thailand and published on the Ministry's minimum wage page. Each notification lists the daily minimum wage set for every province and the date on which it takes effect, and a consolidated table maintained by the Ministry records all rates and effective dates over the sample period. From these notifications, I build a quarterly series of the daily minimum wage in force in each province, covering every quarter from 2007 through 2016.

Minimum wage data are available on the Ministry of Labour's website at [Ministry of Labour Minimum Wage website](#), accessed [Nov. 6, 2024].

F.2 *Labour Force Survey*

Individual-level data come from the Labour Force Survey (LFS), administered monthly and published quarterly by the National Statistical Office of Thailand, for the years 2007 through 2016. I construct a province-by-quarter panel that covers all 76 provinces. Bueng Kan, administratively separated from Nong Khai in 2011, is merged back into Nong Khai throughout so that provincial boundaries are consistent over the sample period. April observations are excluded because Thai schools are on summer break that month, so enrollment is not representative of the academic year.

The analysis sample is restricted to youth aged 15 to 17, the group at the upper-secondary margin. Government and state enterprise workers are excluded, since public employment differs from low-barrier work that constitutes the uncovered sector in the model. Individuals with unknown educational status are dropped, as are youth living in group quarters such as dormitories, for whom household characteristics are not available. A small number of records with missing or unclassifiable industry codes are also dropped, and daily wages above the 99.5th percentile are trimmed as outliers.

F.3 *Socio-Economic Survey*

The household-level data come from the Socio-Economic Survey (SES), administered by the National Statistical Office of Thailand, and are used for the expenditure analysis. As with the LFS, Bueng Kan is merged back into Nong Khai to keep provincial boundaries consistent, and government and state enterprise workers are excluded. The analysis sample retains one observation per household with at least one child aged 15 to 17, which avoids double counting households with more than one such child. Household education is measured over members aged 25 and above, following the same rule as in the LFS, and the 2007 provincial baseline characteristics are constructed within the SES using the same definitions.

Employment is classified differently from the LFS because the SES records both a principal and a subsidiary industry for each worker. I assign a sector by prioritizing the principal activity, treating a worker as covered if the principal job is in a covered industry, and turning to the subsidiary industry only when the principal is uninformative. Workers not in employment are classified as unemployed if they are still searching for work and as out of the labor force otherwise. The resulting categories match those used in the LFS.

The survey records education spending in two places, once for each household member over the academic year and once for the household over the past twelve months, and the two occasionally disagree. Households reporting individual-level education expenses while the household-level education total is missing are dropped, comprising 0.2 percent of the sample. The

remaining missing values in household education items are treated as zero, which is consistent with those households that report no individual-level expenses either. All expenditure measures are the survey's computed monthly averages. Consumption expenditure is measured net of all education spending, since school fees and materials rise mechanically when a child remains enrolled, and food expenditure is drawn from a separate module with a weekly reference period.

F.4 *Gross Provincial Product*

The gross provincial product comes from the Office of the National Economic and Social Development Council, Office of the Prime Minister, which publishes provincial accounts annually in both current prices and chain volume measures with 2002 as the reference year. These data are available on the Office of the National Economic and Social Development Council's website at [NESDC National Accounts](#), accessed [Oct. 7, 2024].

F.5 *Consumer Price Index*

The consumer price index comes from the Trade Policy and Strategy Office, Ministry of Commerce, which publishes the general consumer price index for Bangkok and its vicinity (Bangkok, Samut Prakan, Nonthaburi, and Pathum Thani) and for the Central, Northern, Northeastern, and Southern regions. I use the annual regional indices with 2007 as the base year (2007 = 100). These data are available on the Trade Policy and Strategy Office's website at [TPSO Consumer Price Index](#), accessed [Oct. 8, 2026].

F.6 *Household Debt*

Provincial household debt comes from the National Statistical Office's series on average household debt by purpose of borrowing, which reports total debt per household and its composition by province for 2004 through 2023 and is derived from the Socio-Economic Survey. My measure is the log of total debt per household in 2011, the last observation before the first minimum wage increase. Bueng Kan was created from Nong Khai in March 2011 and was not surveyed that year. The variable is a per-household average rather than a total, so Nong Khai's figure is used for the combined province. These data are available on the National Statistical Office's website at [NSO Average Household Debt by Province](#), accessed [Aug. 26, 2026].

F.7 *Rice Production*

The Main-crop paddy output by province comes from the Office of Agricultural Economics, published through its Agricultural Information Center, which reports planted area, harvested area, production, and yield per rai for each crop year at 15 percent moisture. My measure is the potential transfer to a province under the Rice Pledging Scheme relative to its economy. The numerator is the average output over 2008 to 2010, in tons, valued at the scheme's guaranteed price of 15,000 Baht per ton for white paddy ([USDA Foreign Agricultural Service, 2017](#)). The denominator is nominal gross provincial product, averaged over the same years. Output is averaged over three years rather than taken from a single year because paddy harvests are weather-sensitive, and averaging both sides keeps a bad harvest from inflating the ratio. Both components predate the scheme, which was announced as an election platform in July 2011. Pre-scheme output is a close predictor of the 2011/12 crop the government purchased. Bueng Kan is folded back into Nong Khai every year so that the boundaries match the LFS. These data are available in the Office of Agricultural Economics' data catalog at [OAE Main-Crop Rice Production dataset](#), accessed [Aug. 26, 2026].

F.8 *Flood Exposure*

Exposure to the 2011 floods is constructed from Table 4 of [Poaponsakorn et al. \(2014\)](#), which reports the number of sub-districts inundated for at least one day and at least fourteen days in each of the 26 affected provinces, alongside the province's total number of sub-districts. My measure is the share inundated for at least fourteen days, the criterion the government used when compensating farmers. Provinces absent from the table are coded as unexposed. Satellite imagery cannot detect flooding beneath dense urban development, so the underlying study treats all households in Bangkok as flooded.

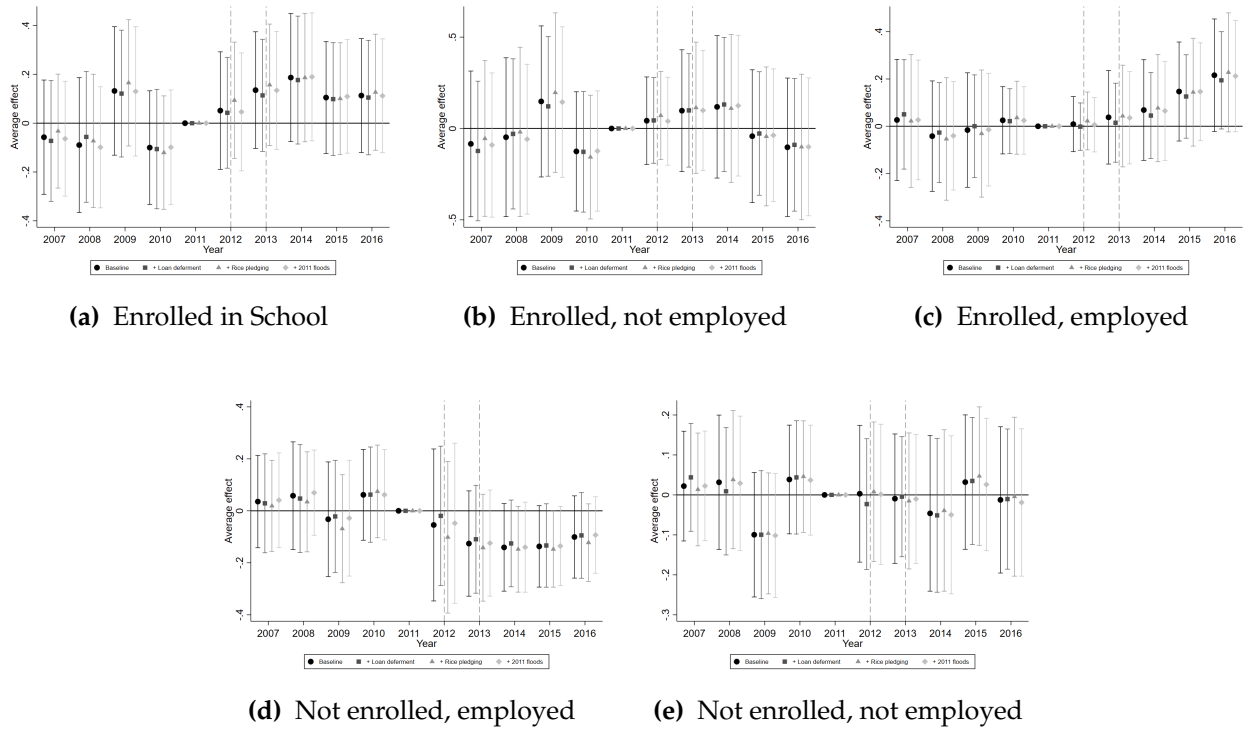
G Additional Robustness Results

Table G.1: Dose Heterogeneity: Estimates on Trimmed Samples

	Enrolled school (1)	Enrolled, not employed (2)	Enrolled, employed (3)	Not enrolled, employed (4)	Not enrolled, not employed (5)
<i>Panel A: Excluding the seven least-treated provinces</i>					
$\Delta \log(MW_p) \times \text{Post}$	0.296** (0.129)	0.231 (0.169)	0.065 (0.130)	-0.180** (0.087)	-0.117 (0.081)
Mean of dep. var.	0.815	0.779	0.036	0.118	0.067
Provinces	69	69	69	69	69
Observations	310,166	310,166	310,166	310,166	310,166
R-squared	0.082	0.092	0.036	0.079	0.018
<i>Panel B: Excluding top quartile of dose</i>					
$\Delta \log(MW_p) \times \text{Post}$	0.197* (0.104)	0.141 (0.108)	0.056 (0.064)	-0.167*** (0.061)	-0.029 (0.071)
Mean of dep. var.	0.808	0.779	0.029	0.122	0.071
Provinces	51	51	51	51	51
Observations	232,968	232,968	232,968	232,968	232,968
R-squared	0.097	0.109	0.040	0.088	0.021

Notes: The table re-estimates the continuous difference-in-differences specification in [Equation 1](#) for youth aged 15–17 on two trimmed samples. Each coefficient is comparable to the full-sample estimate of 0.146 in [Table 4](#). Panel A excludes the seven least-treated provinces, leaving 69. These are the most developed provinces, and their high pre-policy minimum wages left them the smallest gap to 300 Baht. Panel B excludes the most-treated provinces, those in the top quartile of treatment intensity $\Delta \log(MW_p)$, leaving 57. Quartiles are constructed across the 76 provinces rather than across observations. The specification includes province and year fixed effects, individual and household controls, and 2007 baseline province characteristics interacted with the post indicator, and is weighted by survey sampling weights. Standard errors clustered by province are in parentheses. Significance levels are * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Source: LFS.

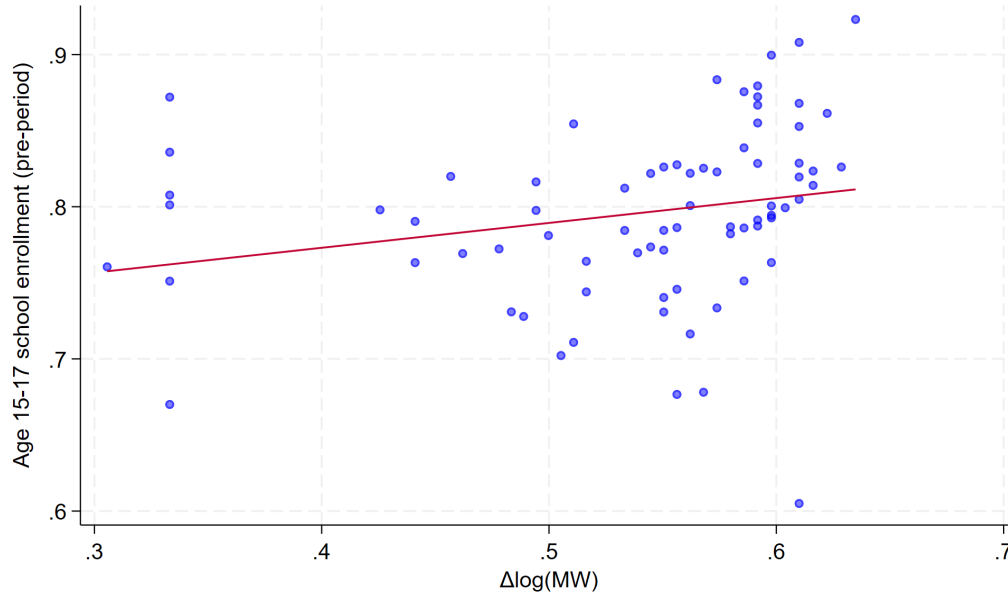
Figure G.1: Event Study Estimates Controlling for Contemporaneous Economic Shocks



Notes: Event study estimates of the effect of the 300 Baht minimum wage policy on youth school enrollment and school–work status among those aged 15–17, following the specification in [Equation 2](#), adding controls for exposure to three contemporaneous policy and environmental shocks. Each shock measure is interacted with the full set of year dummies and added one at a time. Loan deferment exposure is the log of average debt per household in 2011, before registration for the deferment opened in May 2012. Rice pledging exposure is the value of average 2008–2010 main-crop paddy output at the guaranteed price of 15,000 Baht per ton as a share of nominal gross provincial product over the same years. Flood exposure is the share of a province’s sub-districts inundated for at least fourteen days during the 2011 floods, which ran from late July to mid-December 2011. Panel (a) reports overall school enrollment and panels (b) through (e) decompose youth status into four mutually exclusive categories. Capped vertical lines denote 95 percent confidence intervals computed from standard errors clustered at the province level. The vertical dashed lines mark the first (2012) and second (2013) minimum wage increases. Source: LFS, SES, Office of Agricultural Economics, and [Poaponsakorn et al. \(2014\)](#).

H Figures and Tables

Figure H.1: Pre-Policy Youth Enrollment and Minimum Wage Exposure Across Provinces



Notes: This figure plots the pre-policy school enrollment rate of youth aged 15 to 17 against the size of the provincial minimum wage change, $\Delta \log(MW)$, with one observation per province. Enrollment rates are averaged over the pre-policy period (2007 to 2011) within each province using survey weights, and April observations are excluded. The treatment variable $\Delta \log(MW) = \log(MW_{2013Q1}) - \log(MW_{2012Q1})$ measures the log change in the provincial minimum wage induced by the unification toward a common nationwide minimum wage; larger values correspond to provinces that began with lower minimum wages, which tend to be the less developed provinces. The solid line is the ordinary least squares fit across provinces, with a slope of 0.163 (p-value = 0.044). Source: Labor Force Survey.