Extended Compulsory Schooling and the Timing of Adolescent Motherhood: Evidence from the Philippines

Erika Salvador and Alex Coiov

Instructor: Caroline Theoharides

Fall 2025

Contents

List of Figures

List of Tables

1 Introduction

Adolescent pregnancy remains a significant challenge in many developing countries, with long-term consequences for women's health and later economic outcomes. The Philippines stands out in this regard as it records some of the highest teenage fertility rates in Asia and globally. Until recently, the country also maintained a basic education system that provided fewer than twelve years of pre-university schooling, which policymakers and educators viewed as insufficient preparation relative to international peers. Taken together, high adolescent fertility and a short basic education cycle suggest scope for extended compulsory schooling to delay early childbearing by keeping adolescents enrolled in school through ages of elevated fertility risk.

Motivated by concerns about academic preparedness relative to international norms, the Philippines enacted the Enhanced Basic Education Act in 2013, which introduced a K–12 system by adding a year of kindergarten and two years of senior high school (SHS) to the previous eleven-year cycle. The SHS reform extended compulsory education through Grade 12 and rolled out gradually across provinces beginning in School Year 2016–17. This staggered implementation generated plausibly exogenous variation, as adjacent birth cohorts faced sharply different exposure to SHS and provinces varied in the intensity of SHS availability. These features allow us to examine whether extended compulsory schooling delays entry into motherhood and reduces early childbearing in a quasi-experimental setting.

This paper analyzes how extended compulsory schooling affects adolescent fertility and the channels through which these effects arise. We examine whether exposure to senior high school delays first births and lowers the probability that women give birth by ages 18 and 20. We then assess whether these effects operate through continued school enrollment during late adolescence, i.e., the "incarceration" effect, changes in sexual behavior, or increased contraceptive use. Evidence on these mechanisms matters for policy. If additional schooling alone reduces adolescent fertility, education reforms may complement, and in some settings partially substitute for, reproductive health interventions.

We combine administrative records from the Department of Education (DepEd) on SHS availability with nationally representative data from the Philippine Demographic and Health

1

Surveys (DHS, 2003–2022). We construct a province-cohort dataset linking variation in SHS exposure to measures of overall fertility, fertility timing, sexual behavior, and contraceptive use. Our empirical strategy exploits two dimensions of plausibly exogenous variation. First, we use cohort-level differences: only women born in 2000 or later were required to complete Grades 11 and 12. Second, provinces differed in the intensity of SHS rollout, measured by the number of schools offering Grades 11–12 per 1,000 Grade 10 students in the pre-reform period. Combining these two dimensions, we estimate DiD models that compare adjacent cohorts within provinces while also exploiting cross-province variation in SHS intensity.

The evidence indicates that SHS exposure delays entry into motherhood and reduces adolescent childbearing. A one-unit increase in SHS capacity—one additional senior high school per 1,000 Grade 10 students—raises age at first birth by approximately 0.14 years (roughly 1.7 months) and lowers the probability of a woman giving birth by ages 18 and 20 by 0.6-0.7 percentage points. These shifts occur right during late adolescence, when fertility risk rises sharply. A range of robustness checks confirms that the results remain stable across alternative trend controls, narrower cohort windows, different measures of SHS exposure, hazard-based specifications for first birth, exclusions of provinces with atypical demographics or extreme SHS capacity, leave-one-province-out exercises, and placebo tests on pre-reform cohorts.

An examination of mechanisms indicates that continued school enrollment is the primary channel through which SHS affects fertility outcomes. Exposure to SHS increases completed years of schooling but produces no meaningful changes in sexual behavior or contraceptive use. This pattern implies that extended compulsory schooling reduces exposure to early childbearing risk by keeping young women enrolled during late adolescence, consistent with the incarceration mechanism emphasized in the literature (Becker, 1960; Breierova and Duflo, 2004; McCrary and Royer, 2011).

The remainder of the paper proceeds as follows. Section 2 describes the historical and institutional context of the K–12 reform and adolescent fertility in the Philippines. Section 3 describes the data and the construction of the province–cohort dataset. Section 4 outlines the empirical strategy and discusses identification assumptions and robustness checks. Section 5 presents the main results on fertility timing and underlying mechanisms. Section 6 concludes

and discusses policy implications and directions for future research.

2 Background

2.1 The Philippine Education System

Before taking its current form, the Philippine education system was shaped by two successive colonial schooling regimes. The first was the Spanish education system, under which formal schooling was largely delivered through religious institutions and access was restricted primarily to boys from elite or urban households. The second was the American education system, which introduced a mass public school model, adopted English as the language of instruction, and established a standardized structure of six years of elementary education followed by four years of secondary education (Romero, 2020). Of these two, the Philippine education system largely retained the American model. Subsequent reforms focused primarily on adjustments to curriculum content—often to increase Philippine-centered material—while the overall structure of the system, in terms of years of schooling, persisted for several decades after independence.

This long-standing ten-year cycle made the Philippines an international outlier. Until the 2010s, the Philippines was one of only three countries worldwide, and the only one in Asia, that had not adopted a twelve-year pre-university education system (Ponce de Leon, 2023).[1] Filipino students typically completed high school at approximately age 16. At this age, students entered tertiary education or the labor market with fewer years of formal instruction than their counterparts in most other countries, which may have limited academic preparation for college-level coursework and reduced the comparability of credentials in domestic and international labor markets. International surveys also showed that the Philippines lagged behind other middle-income countries in secondary completion and learning outcomes, and national assessments in the 2000s documented declines in student performance and persistent

---

[1]Throughout the paper, we use the term "twelve-year pre-university education system" to denote the international norm of basic education prior to tertiary entry. In some countries, including the Philippines following the K–12 reform, this corresponds to thirteen years of compulsory schooling when a mandatory kindergarten or preparatory year is included. For consistency, we follow the common convention in the comparative education literature and refer to these systems as twelve-year cycles.

disparities across regions (World Bank Group, 2016). These concerns provided the policy rationale for extending the length of compulsory schooling.

2.2   The Enhanced Basic Education Act of 2013

The Enhanced Basic Education Act of 2013 (Republic of the Philippines, 2013) extended the basic education cycle from eleven to thirteen years through the addition of Grades 11 and 12. The reform aimed to strengthen preparation for tertiary education and employment and to align the country's education structure with international norms. Under Senior High School (SHS), students select from four tracks corresponding to different postsecondary pathways: Academic,[2] Technical–Vocational–Livelihood,[3] Sports,[4] and Arts and Design.[5] All SHS tracks follow a common national curriculum framework. Under this framework, each track includes standardized core subjects alongside track-specific coursework. Furthermore, instructional time is comparable across tracks. As a result, differences across tracks primarily reflect variation in curricular content rather than differences in time spent in school.

The scale of the reform placed immediate pressure on the supply of senior high school capacity during the transition and required adjustments to curricula, teaching staff, and school facilities. For this reason, implementation did not occur immediately following the law's passage in 2013 but unfolded over several years.[6] The SHS curriculum reached its final form in the following year. It was only after several years of expansion in physical infrastructure and the teacher workforce that the first SHS cohort entered Grade 11 in School Year (SY) 2016–17 and completed Grade 12 in 2018. This cohort was the first to graduate under the full K–12 system.

---

[2]The Academic track comprises strands such as Science, Technology, Engineering, and Mathematics (STEM); Humanities and Social Sciences (HUMSS); Accountancy, Business, and Management (ABM); and a General Academic strand intended for students who remain undecided about their postsecondary plans.

[3]The Technical–Vocational–Livelihood track includes strands offering industry-certified skills in areas such as bookkeeping, automotive servicing, and hospitality services.

[4]The Sports track is designed for students pursuing training related to athletic performance, coaching, and sports management.

[5]The Arts and Design track focuses on creative and design-oriented fields, including visual arts, media, and related creative industries.

[6]The kindergarten component of the reform preceded the expansion of upper-secondary schooling. Universal kindergarten was introduced in School Year (SY) 2011–12, prior to the passage of the Enhanced Basic Education Act, and constituted the "K" in K–12. Consequently, cohorts affected by the later introduction of Grades 11 and 12 did not face concurrent changes at the kindergarten margin.

On the demand side, the Department of Education (DepEd) introduced a nationwide SHS voucher program that subsidized enrollment in private senior high schools (Ronda, 2016). The program aimed to relieve pressure on public schools during the transition and to sustain participation as compulsory schooling expanded. This demand response occurred in a policy environment in which the direct cost of basic education had already been substantially reduced.[7] The 1987 Constitution mandated free and compulsory elementary education (Republic of the Philippines, 1987), and the Free Public Secondary Education Act of 1988 eliminated tuition fees in public high schools (Republic of the Philippines, 1988), which raised lower-secondary enrollment during the 1990s and 2000s. The K–12 law subsequently redefined basic education to include both junior and senior high school, and administrative practice now treats the full thirteen-year cycle as compulsory. In addition, the Universal Access to Quality Tertiary Education Act of 2017 eliminated tuition in state universities and colleges (Republic of the Philippines, 2017). With schooling already widely accessible, the K–12 reform shifted the typical age of school completion upward.

2.3 Compulsory Schooling Laws and Women's Health

Substantial literature uses compulsory schooling laws (CSLs) as sources of exogenous variation in educational attainment. Much of this evidence comes from countries that extended compulsory education far enough in the past that the post-reform system became the institutional baseline. These policies increased the minimum years of compulsory education through statutory change. In most cases, governments paired the legal mandates with complementary investments in school capacity, such as school construction and teacher recruitment, which enabled implementation at scale. Researchers exploit the resulting discontinuities across birth cohorts and, in some settings, regional variation in rollout to identify causal effects. Seminal and subsequent studies document impacts on fertility, health, labor-market participation, and intergenerational outcomes (Breierova and Duflo, 2004).

One of the most consistently documented effects of extended compulsory schooling laws

---

[7]We describe these earlier policies because they inform our empirical strategy. Basic education had long been tuition-free at the primary and lower-secondary levels, so large enrollment shifts driven by fees are unlikely. This reduces concerns that selection, attrition, or intrahousehold reallocation confound our estimates.

is a delay in early marriage and a reduction in early fertility. In Turkey, a nationwide reform in 1997 extended compulsory schooling from five to eight years. Exploiting cohort-based exposure to this reform, Kırdar et al. (2018) show that the likelihood of marriage by age 16 declined by 44 percent and the probability of childbearing by age 17 declined by 36 percent among affected women. Meanwhile, in Thailand, compulsory schooling was extended to nine years in 2003, increasing mandatory attendance through lower secondary school. Using a regression discontinuity design around birth cohort eligibility, Chaijaroen and Panda (2023) find a 4–5 percentage-point reduction in teenage births, with larger effects among Muslim girls, who faced higher baseline fertility risk.

Closely related is literature examining how extended CSLs affects health behaviors and reproductive decision-making. Evidence from middle-income settings suggests that extended schooling can influence reproductive health investments, even when fertility levels themselves do not decline. In Turkey, the same 1997 reform increased the use of modern contraceptives, lengthened birth intervals, and reduced unwanted pregnancies (Güneş, 2015). Meanwhile, a nationwide education reform in Taiwan in 1968 extended compulsory schooling from six to nine years. Exploiting this expansion, Chou et al. (2010) show that additional schooling increased prenatal care utilization and the likelihood of hospital deliveries. A similar extension occurred in Mexico, where compulsory schooling was likewise increased from six to nine years in the early 1990s. Using this policy-induced increase in schooling, Andalón et al. (2014) find that women's knowledge of contraception and the likelihood of contraceptive use at sexual debut increased.

In contrast, evidence from high-income countries does not point to effects of comparable magnitude or direction. In the United Kingdom, compulsory schooling was extended through a series of school-leaving-age reforms, most notably raising the minimum leaving age from 14 to 15 in 1947 and from 15 to 16 in 1972. Exploiting cohort-based exposure to these reforms, Clark and Royer (2013) document limited long-run effects on adult health outcomes. In the United States, compulsory schooling laws were expanded gradually across states during the early twentieth century, typically increasing minimum schooling requirements by one year. Studies exploiting this variation similarly find small or null effects on health and fertility outcomes. Impacts were concentrated primarily in labor-market returns rather than

non-market behaviors (Angrist and Krueger, 1991; Oreopoulos, 2006). These muted responses likely reflect higher baseline educational attainment and the weaker binding of additional schooling at critical life stages.

Unsurprisingly, cross-country evidence suggest that the demographic consequences of CSL expansions depend critically on the socioeconomic context in which reforms are implemented. More consistent positive responses are observed in middle-income settings where fertility risks remain high. This finding is especially relevant for the Philippines, a lower–middle-income country in which overall fertility has declined but early childbearing remains prevalent and adolescent birth rates are high relative to regional peers. Consistent with this context, the Philippines ranks among the highest in East and Southeast Asia in adolescent fertility, with elevated birth rates among girls aged 15–19 (Salama, 2025). Adolescent births frequently occur outside formal marriage, and the average age at first marriage lies well above the ages at which many adolescent births take place. In this context, compulsory schooling reforms such as the K–12 expansion offer a natural setting for examining the effects of extended education on fertility timing.

At the same time, an important finding from research on education interventions more broadly is that reductions in early fertility do not always require declines in sexual activity. In several contexts, schooling interventions reduce early childbearing even when sexual initiation and partnership behavior remain largely unchanged (Baird et al., 2010; Duflo et al., 2015). This evidence points to school attachment as a primary margin through which compulsory schooling delays fertility. When individuals remain enrolled, time in school constrains mobility and limits exposure to sustained partnerships, a mechanism often described as the "time-in-school" or "incarceration" effect. Studies also document changes in reproductive knowledge and contraceptive use after schooling expansions when sexual and reproductive health content forms part of the new curriculum. Given that the Philippines is mechanically increasing time in school through the K–12 reform while also facing high teenage pregnancy rates, it is crucial to examine how schooling impacts fertility timing in this context.

2.4 Research Gap

Evidence on the Philippine K–12 reform remains limited due to its recent implementation and the small number of cohorts that have fully completed Senior High School. Two gaps characterize the existing literature. First, available assessments emphasize short-run outcomes and rely on descriptive indicators and qualitative accounts, which suggest uneven realization of the reform's labor-market objectives (Espinosa and Marasigan, 2025). Second, the small empirical literature to date concentrates almost exclusively on employment-related outcomes (Orbeta et al., 2018; Orbeta Jr and Potestad, 2025). While these dimensions are clearly important, relatively little is known about the reform's demographic effects. Although cross-country evidence indicates that compulsory schooling can delay fertility under certain conditions, it remains unclear whether similar effects emerged in the Philippine context, where adolescent childbearing remains prevalent and often occurs outside formal marriage. Even in the short run, an assessment of whether compulsory Senior High School altered fertility timing among exposed cohorts provides insight into the channels through which extended schooling operates and the margins along which longer-run effects may arise.

Therefore, this paper examines these questions using cohort-based exposure to the K–12 reform and cross-province variation in Senior High School availability. The analysis focuses on fertility timing outcomes and related mechanisms in order to characterize the short-run demographic responses to an extension of compulsory secondary schooling in the Philippines.

3 Data

We combine administrative information on the nationwide rollout of SHS with harmonized microdata from multiple waves of the Philippine Demographic and Health Survey (DHS). The resulting province–cohort dataset links variation in exposure to the reform with measures of fertility behavior, contraceptive use, sexual activity, and women's early-adult well-being. All geographic units are standardized to 2010 provincial boundaries.[8]

---

[8]As a special case, Metro Manila (National Capital Region) does not correspond to a single province but to a metropolitan region comprising 17 cities and one municipality, jointly coordinated by the Metropolitan Manila Development Authority. For analytical consistency, we follow its official geographic subdivision into four districts—Manila (District I), the eastern corridor (District II), the northern corridor (District III),

3.1 Senior High School Expansion Data

The Department of Education provided the official registry of authorized SHS providers for the preparatory and initial implementation years of the K–12 reform. The registry lists each school together with its location and track offerings. We harmonize these files by standardizing geographic identifiers and resolving inconsistencies in school names. Missing information in the source data is retained, and we impose no additional deletions beyond verifying that each school can be assigned to a valid 2010 province. We then aggregate all SHS providers to the province level.

3.2 Philippine Demographic and Health Surveys (DHS)

We draw on the 2003, 2008, 2013, 2017, and 2022 rounds of the DHS, which survey women aged 15 to 49 and provide detailed information on fertility histories, reproductive health, sexual activity, cohabitation and marriage, HIV testing, and indicators of economic and social empowerment. Province identifiers are harmonized to a consistent coding scheme that matches the geographic structure of the SHS exposure data.

Primary Outcomes. Our primary fertility outcomes capture the timing of childbearing, the margin most plausibly altered by additional years of schooling. We use two sets of measures. First, we measure fertility timing through age at first birth, which offers a continuous metric of shifts in the transition to motherhood. Then, we construct early fertility indicators that take the value of one if a woman gave birth before age 18 or before age 20. These thresholds correspond to periods of elevated adolescent fertility risk in the Philippines and align with the ages at which the SHS reform most directly constrained students' time and mobility. These indicators therefore isolate the window in which SHS attendance overlaps with the onset of fertility exposure.

We do not use completed fertility—typically proxied by children ever born in demographic applications—as an outcome. The first SHS-exposed cohorts graduated in 2018 and remain in their teens or early-to-mid twenties during our study window. Children ever born therefore suffers from severe right-censoring and fails to approximate completed reproductive behavior,

---

and the southern corridor (District IV)—and treat these districts as the NCR-equivalent "provinces" in our dataset.

which limits its value as an outcome in this context.

Mechanism Outcomes. The DHS provides information on educational attainment, sexual behavior, and contraceptive use, which we use to examine potential channels through which extended compulsory schooling affected fertility timing. We first study education, the most direct margin targeted by the K–12 reform, by examining completed years of schooling. This outcome verifies that variation in SHS exposure translated into extended enrollment among eligible cohorts and establishes education as a prerequisite mechanism for subsequent fertility responses. We also examine sexual behavior to assess whether fertility timing responded through changes in sexual activity. To do so, we construct measures that capture both the timing of sexual initiation and subsequent sexual exposure. These include age at first sex and indicators for recent sexual activity over multiple recall windows, as well as measures of partnership intensity.[9] These outcomes allow us to distinguish between changes in the onset of sexual activity and changes in behavior among those who have initiated sex. Finally, we study contraceptive behavior to assess whether pregnancy risk declined through greater use of effective contraception. We construct indicators for any contraceptive use and for modern method use. Modern methods include pills, injectables, IUDs, condoms, and sterilization. We assess whether the fertility effects operate through increased contraceptive protection or instead reflect delayed exposure to childbearing risk driven by continued school enrollment.

4   Empirical Strategy

The K–12 reform introduced a sharp increase in compulsory schooling for cohorts who were the first to be required to complete Grades 11 and 12 beginning in School Year 2016–17. At the same time, the intensity of SHS varied substantially across provinces, which generated plausibly exogenous spatial differences in access to the new upper-secondary track. Our empirical strategy leverages this two-dimensional variation by estimating difference-in-

---

[9]We do not examine marriage as a primary mechanism. In the Philippines, the average age at first marriage lies well beyond the ages relevant for adolescent and early-adult fertility (Philippine Statistics Authority, 2022), and a substantial share of early births occur outside formal marriage. The K–12 reform therefore affected fertility timing at ages when marriage decisions played a limited role in the transition to first birth.

differences (DiD) models that compare adjacent birth cohorts within provinces before and after the reform while exploiting cross-province differences in SHS expansion.

4.1 Baseline Specification

We estimate

$$Y_{ipc} = \beta\left(\text{Treated}_c \times \text{SHS}_p\right) + \alpha_p + \gamma_c + \delta_{t(i)} + \mathbf{X}'_{ipc}\theta + \varepsilon_{ipc}, \tag{1}$$

where Y_{ipc} is an early-fertility outcome for woman i in province p and cohort c. For the mechanism analysis, we re-estimate Equation (1) using alternative outcomes that capture educational attainment, sexual behavior, and contraceptive use. Province fixed effects α_p absorb stable differences across provinces. Cohort fixed effects γ_c capture nationwide changes common to all women born in the same year. Survey-year fixed effects $\delta_{t(i)}$ absorb shocks that vary across DHS rounds. Across specifications, the vector $\mathbf{X}_{ipc}$ expands from basic socioeconomic controls—urban residence and household wealth quintile dummies—to a full specification that also includes indicators for religion and ethnicity. Age at interview is not included because it is mechanically determined by birth cohort and survey year and is therefore collinear with the fixed effects. Standard errors are clustered at the province level.

4.2 Treatment Construction

Identification arises from two dimensions. The first is variation across birth cohorts because only women born in 2000 or later were subject to compulsory SHS. The second is variation across provinces in the intensity of SHS expansion prior to implementation.

Cohort-level Variation. Grade 11 was introduced in School Year 2016–17 and became a compulsory part of basic education from that year onward. Under the official age structure of the basic education cycle, students are expected to begin Grade 11 at age sixteen, which implies that the cohort born in 2000 was the first to be subject to the extended secondary cycle. Earlier cohorts could not have been exposed to SHS because the new grade levels did not yet exist when they reached upper-secondary age. We therefore set $\text{Treated}_c = 1$ for women born in 2000 or later. This assignment aligns each birth cohort with the year in which

11

it would have reached the age for Grade 11 under the reform and is consistent with both the DepEd official age schedule and observed schooling patterns.[10]

Province-level Variation. We exploit cross-province differences in pre-reform SHS capacity. We treat SHS availability as fixed at baseline because the rollout was highly front-loaded and nearly all planned SHS schools were in place before the first Grade 11 cohort entered. During the preparatory period from 2014 to 2016, DepEd approved SHS providers, assigned program tracks, and certified facilities, while provinces expanded or adjusted existing secondary schools to accommodate the new grade levels. Establishing SHS capacity required lead time: local governments mobilized financial and staffing resources, and DepEd finalized the curriculum and infrastructure requirements for implementation.

Given this sequencing, SHS_p is defined as a pre-determined, province-level measure of capacity: the number of schools offering Grades 11–12 in School Year 2016–17 per 1,000 Grade 10 students in School Year 2015–16, the last cohort to follow the ten-year basic education cycle. All cohorts take the same SHS_p value for their province of residence at interview.[11]

Therefore, the interaction of cohort eligibility and provincial SHS capacity generates quasi-experimental variation in exposure. The coefficient β identifies how early fertility outcomes change for eligible cohorts in provinces with more intensive SHS expansion, relative to older cohorts in the same province and relative to the corresponding changes in less expanded provinces.

---

[10]The Republic of the Philippines (2013) and its Implementing Rules, together with official DepEd K to 12 materials, specify that the typical entrant age for SHS is sixteen. The Philippine Qualifications Framework classifies SHS completion at Level 2, which implies an expected completion age of seventeen to eighteen (Philippine Qualifications Framework, 2017). These justify defining the cohort born in 2000 as the first exposed cohort. While administrative sources do not report mean SHS entry ages, our concerns about age–grade heterogeneity are limited. Secondary-school attendance among twelve- to eighteen-year-olds is consistently high (Philippine Statistics Authority, 2019), and there is no evidence of systematic delays at the key transition ages of fifteen to seventeen.

[11]We do not consider inter-province migration after upper-secondary age to be a material threat to the assignment of exposure. Using DHS duration-in-residence data, we approximate age at last move and classify women as having migrated after age sixteen when reported years in the current residence imply relocation at or after that age. Only five percent of post-2000 cohorts appear to have moved after age sixteen, compared with 37 percent among older cohorts who have had more time to relocate and were never subject to SHS. Because treated cohorts rarely migrate after the exposure-relevant age, province of residence is a credible proxy for province of schooling. See Appendix 8.1 for details.

4.3 Identifying Assumption

Identification relies on a parallel-trends assumption. In the absence of the K–12 reform, fertility outcomes for younger and older cohorts would have evolved similarly across provinces with different levels of SHS capacity. Provinces that expanded SHS more intensively must not have been on distinct cohort-specific fertility trajectories prior to implementation. Under this assumption, differences in early-fertility outcomes between high- and low-capacity provinces for pre-2000 cohorts provide a valid counterfactual for how these differences would have evolved for the first exposed cohorts.

4.4 Event-Study Specification

To examine dynamic cohort patterns and assess the validity of the identifying assumption, we estimate an event-study specification that interacts SHS capacity with indicators for a woman's relative cohort. Let k index the distance between cohort c and the first treated cohort (born in 2000). Cohorts are grouped into bins $k \in \{\leq -5, -4, -3, -2, -1, 0, 1, 2, 3, 4, \geq 5\}$, with the earliest bin ($k \leq -5$) omitted. We estimate

$$Y_{ipc} = \sum_k \beta_k \, \mathbf{1}\{\text{rel\_cohort} = k\} \times \text{SHS}_p \; + \; \alpha_p + \gamma_c + \delta_{t(i)} + \varepsilon_{ipc}, \qquad (2)$$

where Y_{ipc} is a fertility-timing outcome for woman i in province p and cohort c; α_p, γ_c, and $\delta_{t(i)}$ denote province, cohort, and survey-year fixed effects, respectively.

Because SHS capacity is fixed across cohorts, identification comes from comparing how cohort differences vary by SHS intensity. Under parallel trends, the coefficients for the pre-reform bins ($k = -4$ to -1) should be close to zero. The post-reform coefficients then trace how fertility timing shifts for the first exposed cohorts in provinces with higher SHS capacity.

4.5 Robustness Checks

We assess the sensitivity of the empirical design to several potential threats to identification. The first set of concerns involves differential demographic trends across provinces that may

coincide with SHS rollout. If provinces with higher SHS capacity were already on distinct fertility trajectories, the interaction of SHS intensity and cohort eligibility could capture pre-existing movements rather than reform-induced changes. To address this, we estimate specifications that absorb richer forms of spatial and temporal variation, including province-specific cohort trends, region-by-cohort fixed effects, and province-by-survey-year fixed effects. We also augment the models with province-level DHS aggregates—such as completed fertility among women ages 40–49 and the prevalence of early childbearing—to flexibly account for shifts in local fertility composition that evolve independently of the reform. Appendix 8.2 provides details on the construction of these aggregates.

A second set of checks evaluates whether the findings are sensitive to the choice of comparison cohorts, alternative definitions of SHS exposure, or the way fertility-timing outcomes are constructed. We first restrict the sample to narrow windows around the eligibility cutoff, limiting comparisons to adjacent cohorts that share similar demographic environments and reducing the influence of long-run secular drift. We then replace the continuous exposure measure with binary indicators for high-exposure provinces, defined at the median, 75th percentile, and 90th percentile of the SHS distribution. Finally, to address concerns that age-threshold outcomes may mechanically reflect cohort differences in censoring or timing, we estimate a discrete-time hazard model for first birth, which examines annual fertility risk rather than cumulative fertility indicators defined at fixed ages. Appendix 8.3 describes the construction of the hazard model.

We next assess the robustness of the estimates to influential provinces and to outlying exposure values. We trim extreme observations in SHS capacity and re-estimate the models after excluding the National Capital Region (NCR) and the Bangsamoro Autonomous Region in Muslim Mindanao (BARMM, formerly ARMM), two regions whose demographic and administrative environments differ markedly from the rest of the country.[12] Finally, as a stronger diagnostic for influential observations, we compute leave-one-province-out (LOPO)

---

[12]NCR is almost fully urban and exhibits some of the lowest fertility levels in the Philippines. Schooling attainment is higher, labor markets draw migrants from across the archipelago, and public resources are more concentrated than elsewhere (Demographic and Health Surveys, 2023). BARMM, in contrast, has the youngest age structure in the country and persistently higher fertility (Demographic and Health Surveys, 2023). Education delivery also flows through devolved and conflict-affected administrative channels. These factors create distributional extremes that may shape province–cohort patterns in ways unrelated to SHS rollout.

estimates.

Finally, we assess whether the empirical strategy generates spurious province–cohort patterns in periods unrelated to SHS implementation. We implement placebo exercises that assign pseudo-eligibility thresholds to earlier cohorts that were never exposed to SHS. The absence of systematic placebo effects indicates that the main results are not driven by underlying demographic movements or mechanical cohort–province interactions.

5 Results

The empirical results present a coherent narrative: SHS in the Philippines delayed women's entry into motherhood by keeping them enrolled in school during ages of heightened fertility risk. The evidence unfolds in three steps. First, we establish that fertility timing evolved similarly across provinces prior to the reform and that divergence emerges only for cohorts eligible for SHS. Second, we show that higher SHS exposure leads to later first births and a reduction in early childbearing. Third, we examine potential mechanisms and document that these fertility responses operate primarily through extended schooling rather than through changes in sexual activity or contraceptive behavior. We conclude by demonstrating that the results are robust to a wide range of alternative specifications, comparison groups, and identification checks.

5.1 Event-Study Estimates

We begin by examining dynamic cohort patterns using the event-study specification described in Section 4.4. Figure 1 plots the estimated interaction between SHS capacity and relative cohort indicators for three fertility-timing outcomes: age at first birth, having had a birth by age 18, and having had a birth by age 20. The corresponding coefficients are reported in Table 1.

Across outcomes, the pre-reform coefficients remain small and statistically indistinguishable from zero. Provinces that later experienced more intensive SHS expansion show no differential fertility-timing trends for cohorts born prior to 2000. Formal tests fail to reject the joint null of no pre-trends for age at first birth and for having had a birth by age 18. We therefore find

evidence consistent with parallel trends in these outcomes prior to the reform. Although the pre-trend test for birth by age 20 is marginally rejected, the estimated coefficients remain centered near zero and show no coherent pattern across cohorts. We interpret this behavior as reflecting slow-moving demographic change rather than anticipatory responses to the reform, a concern we examine directly in the robustness analysis.

In contrast, a clear pattern emerges with the first SHS-eligible cohorts. For age at first birth, the post-reform coefficients rise, which indicates later entry into motherhood in provinces with greater SHS capacity. For early fertility outcomes, the post-reform coefficients fall, which reflects lower rates of adolescent childbearing. These changes occur exactly at the cohort threshold defined by reform eligibility and do not appear for earlier cohorts. This timing supports an interpretation in which SHS exposure drives the divergence, rather than coincident cohort-specific shocks.

5.2 Main Effects on Fertility Timing

Table 2 reports the difference-in-differences estimates of SHS exposure on fertility timing outcomes. Across specifications, greater SHS capacity is associated with later transitions into motherhood and lower rates of early childbearing among eligible cohorts. In the preferred specification with the full set of individual controls, a one-unit increase in SHS capacity—corresponding to one additional senior high school per 1,000 Grade 10 students—raises age at first birth by approximately 0.14 years. This effect is statistically significant at the 5 percent level and remains stable across alternative control sets.

Interpreted in the context of the Philippine schooling calendar, the magnitude of this effect is meaningful. The K–12 reform extended compulsory education by two academic years, keeping students enrolled during ages when first births rise sharply in the Philippines (Philippine Statistics Authority, 2020). A 0.14-year increase in age at first birth corresponds to an average delay of about 1.7 months. In practical terms, this delay is equivalent to a substantial portion of a school term in the public secondary system, which typically spans roughly eight to nine months of instruction per academic year.[13] Although modest at the

---

[13]Under Republic Act No. 7797 (Republic of the Philippines, 1994), the Philippine public school system operates on a maximum of 229 instructional days per academic year, which are distributed across several

individual level, this shift reflects a postponement of motherhood across entire cohorts exposed to greater SHS capacity. Because fertility risk increases steeply during late adolescence, even short delays during this window can generate sizable reductions in early childbearing at the population level.

Consistent with this shift in timing, SHS also exposure reduces the incidence of adolescent fertility. Higher SHS capacity lowers the probability of having had a birth by age 18 by about 0.6 percentage points and by age 20 by roughly 0.7 percentage points. These effects are statistically significant at the 5 percent level. Given baseline adolescent fertility rates in the Philippines, particularly in provinces where early childbearing remains common, these reductions represent consequential changes in early-life outcomes.

The timing of the effects aligns closely with the structure of the reform. The reductions in early fertility occur precisely during the ages when cohorts are required to remain enrolled under compulsory senior high school. This pattern is difficult to reconcile with explanations based solely on long-run changes in preferences or norms. Instead, it points to the role of continued attachment to school during late adolescence. Remaining enrolled through an additional segment of the academic calendar exposes students to different daily routines and institutional constraints than similarly aged peers who exit school earlier. Even when social environments remain similar, these institutional differences shape the timing of key life-course transitions, including entry into motherhood.

The evidence indicates that the SHS reform altered the timing of women's entry into motherhood rather than merely redistributing births across nearby ages. Provinces that expanded senior high school more intensively experienced later first births and fewer adolescent births among eligible cohorts. These findings constitute the core empirical result of the paper and motivate the examination of mechanisms in the next subsection.

---

calendar months due to weekends, holidays, and scheduled breaks. While implementation may vary across private schools and across years, the DepEd calendar governs the majority of senior high school enrollment nationwide.

5.3 Possible Mechanisms

Through which channels did expanded compulsory schooling alter fertility timing? To address this question, we examine a set of intermediate outcomes that capture educational attainment, sexual behavior, and contraceptive use. These margins matter for policy because interventions that reduce adolescent fertility may operate through very different mechanisms. A decline in early childbearing could arise because extended schooling delays or reduces sexual activity, or because pregnancy risk falls even when sexual activity occurs through improved knowledge or contraceptive protection. The analysis evaluates which of these margins shifted in response to SHS exposure.

Time-in-School Channel and the "Incarceration" Effect). Educational attainment constitutes the most direct channel through which schooling reforms may affect fertility timing. A large literature shows that additional schooling delays early-life transitions by simply increasing time spent in school and raising the opportunity cost of early fertility (Becker, 1960; Breierova and Duflo, 2004; McCrary and Royer, 2011). In many settings, these delays arise even in the absence of large changes in reproductive preferences or behavior. Table 3 shows that higher SHS capacity led to a clear increase in completed years of schooling among eligible cohorts. A one-unit increase in SHS capacity raised schooling by roughly 0.14 years. This result confirms that variation in SHS exposure translated into extended enrollment at the upper-secondary margin. Extended schooling therefore provides a necessary precondition for the fertility effects documented in Section 5.2.

Sexual Behavior (Proximate Fertility Channel). Sexual behavior represents a proximate determinant of conception risk and provides a natural intermediate margin through which schooling may affect fertility timing. Prior work emphasizes both the timing of sexual initiation and subsequent sexual activity as links between education and early fertility outcomes (Duflo et al., 2015). Two related dimensions are examined. First, sexual initiation captures whether an individual has ever had sexual intercourse. Second, sexual exposure intensity captures subsequent sexual behavior, measured using indicators for sexual activity in the past four weeks and past three months.[14] Tables 4 and 5 provide little evidence of

---

[14]We focus on these measures because longer recall windows, such as six months or one year, exhibit limited variation in the data and therefore provide less informative margins for analysis.

meaningful responses along either dimension. SHS exposure does not significantly affect the probability of ever having had sex, nor does it materially alter recent sexual activity or partnership intensity among women who report sexual experience. Sexual behavior therefore responds little to the reform.

This absence of changes in sexual behavior is informative. In a predominantly co-educational system with compulsory enrollment, the K–12 reform would not be expected to reduce exposure to potential sexual partners and could plausibly increase age-appropriate social interaction. Prolonged time spent in school can preserve contact with same-age peers during adolescence. In this context, compulsory senior high school could reasonably be expected to increase opportunities for sexual relationship formation. Therefore, the null effects on sexual behavior point to continued school enrollment as the primary mechanism underlying the fertility effects, rather than changes in sexual activity or channels that operate through it. Continued enrollment constrains time use, mobility, and opportunities for sustained partnerships that raise the likelihood of pregnancy. Early fertility can thus decline even when sexual behavior itself remains largely unchanged. To further assess this interpretation, the analysis next examines contraceptive use to evaluate whether pregnancy risk declined through changes in fertility prevention.

Contraceptive Use (Risk-Mitigation and Information Channel). The earlier results show that sexual behavior responds little to SHS exposure. A decline in early fertility could nonetheless arise through reductions in pregnancy risk among women already engaging in sexual activity. Contraceptive behavior therefore provides a distinct risk-mitigation channel through which fertility may decline even when sexual activity persists. Existing evidence links schooling and information to increased contraceptive knowledge and greater use of modern methods in settings with formal sexuality education curricula (Ashraf et al., 2014; Duflo et al., 2021).

This channel is especially relevant in the Philippine context. Under the Responsible Parenthood and Reproductive Health Act of 2012 (Republic of the Philippines, 2012), secondary schools are mandated to provide age-appropriate and medically accurate instruction on sexuality, relationships, and reproductive health. These provisions are operationalized through Department of Education Order No. 31 (Department of Education, 2018), which sets

out policy guidelines for the implementation of comprehensive sexuality education (CSE) in schools. The presence of a formal and nationally standardized sexuality education curriculum raises the possibility that extended schooling could reduce early fertility through increased contraceptive knowledge or uptake, even in the absence of changes in sexual behavior.

Table 6 reports estimates for any contraceptive use and for modern method use. Across specifications, the coefficients remain small and statistically insignificant. These results indicate that the decline in early fertility did not operate through increased adoption of contraceptive technologies. Information delivered through schooling alone does not appear sufficient to change contraceptive use at these ages. Changes in contraceptive adoption therefore do not appear to explain the observed reduction in early fertility.

The mechanism evidence points to continued school enrollment as the central channel through which the reform affected fertility timing. SHS exposure increased educational attainment while leaving sexual behavior and contraceptive use largely unchanged. The absence of responses along these proximate margins indicates that the decline in early fertility did not operate through changes in sexual activity or contraceptive adoption. Instead, extended time in school reduced exposure to childbearing risk during late adolescence. This interpretation aligns with the timing patterns documented in Section 5.2 and supports a view in which compulsory schooling delays fertility through institutional attachment rather than behavioral change.

5.4   Robustness of the Main Estimates

We evaluate the sensitivity of the main findings to a set of robustness checks that follow the logic of the empirical strategy in Section 4.5. The exercises assess whether the estimated fertility-timing effects persist under alternative trend controls, comparison cohort windows, exposure definitions, and sample exclusions. Across all families of robustness checks, the results remain stable in sign and broadly similar in magnitude.

Alternative Trend-Absorbing Specifications. We begin by considering whether the estimates are sensitive to alternative ways of absorbing long-run demographic change. Table 7 augments the baseline specification with richer trend controls and higher-dimensional fixed effects. These specifications allow for more flexible province-level fertility dynamics that

could otherwise coincide with SHS expansion. The resulting estimates remain close to the baseline results in Table 2. While precision declines in some cases, particularly for births by age 20, the direction and relative magnitude of the effects persist.

Alternative Comparison Cohort Windows. We next assess whether the results depend on the set of cohorts used for identification. Table 8 restricts the sample to cohorts born closer to the reform cutoff and limits comparisons to cohorts with more similar underlying conditions. The estimates remain consistent with the baseline, which indicates that the main findings are not driven by comparisons across distant birth cohorts or by long-run cohort drift.

Alternative SHS Exposure Definitions. Having established robustness to alternative trends and comparison windows, we then turn to the measurement of SHS exposure itself. v replaces the baseline continuous exposure measure with alternative definitions, including indicator-based measures at different points of the exposure distribution. The estimated effects remain qualitatively unchanged. Thus, the results do not hinge on a particular functional form or scaling of SHS capacity.

Alternative Timing Framework: Discrete-Time Hazard of First Birth. To further probe whether the findings reflect delayed timing rather than mechanical shifts across age thresholds, we next consider an alternative fertility-timing framework. Figure 2 and Table 14 report estimates from a discrete-time hazard model for first birth, described in Appendix 8.3. This approach models annual transitions into motherhood and is less sensitive to the choice of age cutoffs. The hazard estimates show lower first-birth risks at younger ages among more exposed cohorts, with effects attenuating at older ages. This pattern aligns with the main results and supports an interpretation of delayed entry into motherhood.

Exclusion of NCR and BARMM. We then examine whether the estimates are driven by provinces with atypical demographic or administrative environments Table 10 excluding NCR and BARMM leaves the qualitative conclusions unchanged. Estimates remain stable in sign and statistical significance for the main outcomes. For age at first birth, the coefficient is smaller when NCR is excluded than when BARMM is excluded, which is consistent with highly urban provinces contributing more to identifying variation in fertility timing. For births by age 18, excluding BARMM yields slightly smaller estimates, while

21

excluding NCR leaves magnitudes similar. For births by age 20, estimates remain negative across all exclusion combinations and are less precisely estimated, as in the baseline. Overall, the results do not hinge on either NCR or BARMM.

Leave-One-Province-Out Exclusions. Relatedly, we assess sensitivity to influential observations through leave-one-province-out exclusions. Table 11 together with the full set of estimates reported in Table 15, shows that sequentially omitting each province produces coefficients that remain tightly clustered around the baseline estimate. No single province appears pivotal for the results.

Falsification Tests. Finally, we conduct placebo exercises as an additional diagnostic. Table 12 assigns pseudo-reform cutoffs to cohorts born prior to SHS eligibility. The resulting estimates are small and statistically insignificant. Furthermore, there is no systematic pattern across outcomes. These placebo results provide complementary support for the identifying assumption by showing that the empirical framework does not generate spurious treatment effects in periods unrelated to the reform, consistent with the logic of placebo tests commonly used to assess parallel trends in difference-in-differences designs (e.g., Duflo et al. (2015)).

The robustness exercises support the credibility of the main findings in Section 5.2. Across alternative trend controls, cohort windows, exposure definitions, sample exclusions, and an age-specific hazard framework, the estimated effects remain stable in sign and similar in magnitude. The absence of effects in placebo exercises further reinforces the identifying assumptions underlying the design. The consistency of the evidence indicates that the estimated fertility-timing effects reflect the true response to SHS expansion rather than consequences of specification choice, sample composition, or spurious pre-existing trends.

6 Conclusion

This paper studies how extended compulsory schooling affects the timing of adolescent motherhood, using the Philippines' K–12 reform as a natural experiment. The analysis combines sharp cohort eligibility with substantial cross-province differences in SHS capacity. This design permits a comparison of adjacent birth cohorts within provinces while exploiting plausibly exogenous variation in exposure to upper-secondary schooling. The empirical

strategy is supported by event-study evidence, placebo tests, and a wide set of robustness exercises.

The results show that greater exposure to SHS delays entry into motherhood and reduces early childbearing among eligible cohorts. Provinces with more intensive SHS expansion experience later first births and lower probabilities of giving birth by ages 18 and 20. These effects appear at the reform cutoff and are absent for earlier cohorts. The timing of the response aligns closely with the structure of the reform and supports the identifying assumption that fertility trends would have evolved similarly across provinces in the absence of SHS.

The magnitude of the estimated effects is modest at the individual level but meaningful in aggregate. A one-unit increase in SHS capacity raises age at first birth by about 0.14 years, which corresponds to roughly one to two months. This shift occurs during late adolescence, when fertility risk rises sharply. Even short delays during this window can produce siggnificant reductions in adolescent fertility at the population level. Estimates from a discrete-time hazard model reinforce this interpretation by showing lower first-birth risks at younger ages in more exposed provinces. The hazard profile indicates a delay in fertility rather than a redistribution across age thresholds.

The mechanism evidence sharpens the interpretation of these estimates. SHS exposure increases completed years of schooling among eligible cohorts, which confirms that variation in SHS capacity translated into prolonged enrollment. At the same time, the analysis finds little evidence of changes in sexual initiation, frequency of sexual activity, or contraceptive use. These null results are informative. They rule out channels that operate primarily through changes in sexual behavior or fertility control. Instead, the evidence seems to point to continued attachment to school as the central mechanism. Extended compulsory schooling appears to delay fertility by keeping young women attached to school during late adolescence, i.e., the incarceration effect, which reduces exposure to childbearing risk even when proximate behaviors remain unchanged.

Several considerations qualify the scope of these findings. First, the analysis focuses on fertility timing rather than completed fertility. The first SHS-exposed cohorts remain young in the available data, which prevents an assessment of lifetime fertility responses. Second, the Philippine context matters for external validity. The Philippines is a late adopter of a

twelve-year basic education cycle. Many countries introduced upper-secondary schooling decades earlier, often under different demographic conditions. The estimated effects therefore reflect responses to an abrupt extension of compulsory schooling in a system that previously ended earlier than international norms. In contexts where a twelve-year cycle has long been established, fertility behavior may already reflect prolonged enrollment, which limits the scope for comparable timing responses. The results are thus most informative for settings that face recent or ongoing expansions of compulsory secondary schooling. Third, although the robustness exercises address a wide range of identification concerns, the analysis cannot fully account for heterogeneity in school quality across provinces. The SHS exposure measure captures the presence of schools rather than the quality of instruction or resources that students experience. Data limitations prevent a distinction between public and private SHS providers or a direct measure of variation in teacher quality and facilities. If provinces that expanded SHS more intensively also differed in these dimensions, the estimated effects may partly reflect differences in the nature of schooling rather than exposure alone.

Nonetheless, these findings have implications for policy. In many low- and middle-income countries, education reforms and reproductive-health policies are often treated as separate domains. The evidence here shows that access to compulsory secondary education can delay early fertility even in the absence of changes in sexual behavior or contraceptive use. Of course, we are not suggesting that compulsory schooling should extend beyond the existing twelve-year cycle. Instead, the results show the importance of ensuring access to, and completion of, the full basic education cycle that is already in place. Policies that keep girls enrolled through late adolescence can complement reproductive-health interventions by reducing exposure to early childbearing risk through institutional channels. At the same time, the modest magnitudes caution against viewing schooling reforms as a substitute for direct investments in reproductive health services.

The results also highlight several directions for future research. As noted earlier, one important limitation is that exposure to Senior High School may mask substantial heterogeneity in school quality. If data permit, future work could incorporate measures such as school-level infrastructure indicators from the Department of Education or National Achievement Test (NAT) scores, a standardized assessment administered by DepEd, to assess

whether fertility responses vary with the quality of SHS provision. A second, related avenue concerns heterogeneity within SHS itself. The Philippine SHS system comprises distinct strands—including academic, technical-vocational, sports, and arts tracks—that differ in specialized curriculum content, peer composition, and connections to labor markets. These differences may condition how extended schooling influences fertility timing. For instance, academic tracks may reinforce attachment to further education, whereas technical-vocational tracks may facilitate earlier labor-market entry and alter opportunity costs in different ways. Analyzing fertility responses by SHS strand would help distinguish whether the observed effects reflect time spent in school per se or the specific institutional environments students encounter within SHS. While the results suggest that the primary channel operates through an incarceration or time-in-school effect—namely, that continued enrollment mechanically reduces exposure to pregnancy risk—these additional dimensions of heterogeneity remain important avenues for future research.

More broadly, future work can also examine whether delayed fertility among SHS-exposed cohorts translates into changes in completed fertility, marriage timing, or early-adult labor-market outcomes. Additional research can also study heterogeneity by socioeconomic status or local labor-market conditions to identify where the demographic returns to extended schooling are the largest. These extensions would deepen understanding of how compulsory schooling reforms shape life-course transitions and clarify the conditions under which education policies yield broader demographic benefit.

7 Tables and Figures

Event-Study: Fertility Timing Outcomes
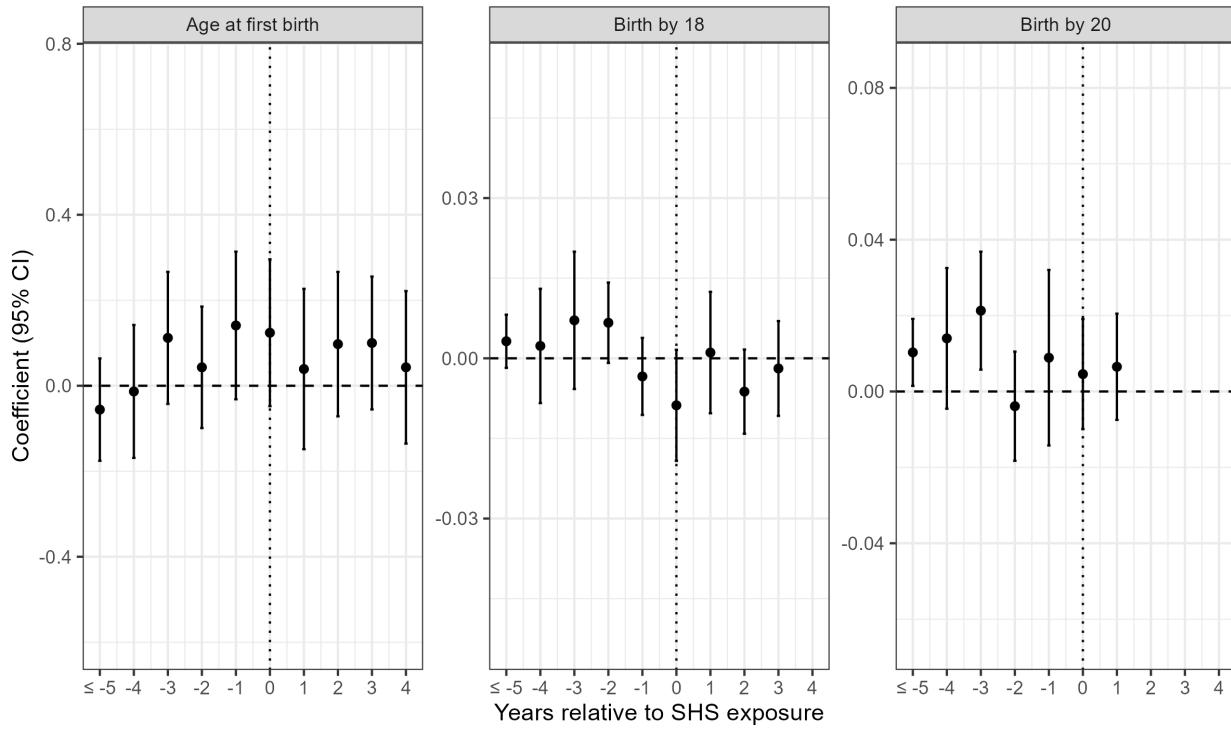
Figure 1: Event-Study Estimates of SHS Exposure on Fertility Timing

Notes: Each panel reports event-study estimates of the effect of SHS exposure on fertility-timing outcomes. Event time is measured in years relative to the first treated cohort (2000), with cohorts born five or more years prior serving as the omitted category. All regressions include province, birth-cohort, and survey-year fixed effects and the full set of individual controls (urban residence, household wealth-quintile, and indicators for religion and ethnicity) and are weighted using DHS sampling weights. Vertical lines mark the first exposed cohort; dashed lines denote zero effects. Confidence intervals are 95%. The exact event-time coefficients corresponding to this figure are reported in Table 1

Table 1: Event-Study Estimates of SHS Exposure on Fertility Timing Outcomes

	Age at first birth	Had a birth by age 18	Had a birth by age 20
Model:	(1)	(2)	(3)
Event time (cohort bins)			
<= -5	-0.0558	0.00319	0.0103**
	(0.0611)	(0.00254)	(0.00450)
-4	-0.0133	0.00232	0.0140
	(0.0793)	(0.00546)	(0.00946)
-3	0.112	0.00711	0.0213***
	(0.0788)	(0.00656)	(0.00793)
-2	0.0431	0.00666*	-0.00388
	(0.0725)	(0.00384)	(0.00734)
-1	0.141	-0.00339	0.00890
	(0.0881)	(0.00368)	(0.0118)
0 (first treated)	0.124	-0.00880	0.00458
	(0.0876)	(0.00530)	(0.00740)
1	0.0391	0.00108	0.00651
	(0.0957)	(0.00580)	(0.00714)
2	0.0974	-0.00624	0
	(0.0862)	(0.00402)	(.)
3	0.100	-0.00190	
	(0.0793)	(0.00453)	
4	0.0431	0	
	(0.0910)	(.)	
>= 5	0		
	(.)		
Fit statistics			
Pre-trend p-value	0.298	0.239	0.015
Pre-treatment observations	58,315	78,716	73,695
Post-treatment observations	909	5,221	2,939
Total observations	59,217	83,928	76,626

Notes: Standard errors in parentheses, clustered at the province level. All regressions include province, birth-cohort, and survey-year fixed effects and are weighted using DHS sampling weights. Birth-by-age outcomes are coded among women old enough to have reached the respective age threshold; Age at first birth is defined for parous women. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Table 2: Difference-in-Differences Estimates of SHS Exposure on Fertility Timing Outcomes

Model:	Age at first birth			Had a birth by age 18			Had a birth by age 20		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables									
Treated$_c$ × SHS$_p$	0.170***	0.135***	0.137***	-0.007***	-0.006**	-0.006**	-0.009*	-0.007	-0.007
	(0.040)	(0.034)	(0.035)	(0.002)	(0.002)	(0.003)	(0.005)	(0.004)	(0.004)
Controls									
Urban residence	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Wealth index	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Religion	No	No	Yes	No	No	Yes	No	No	Yes
Ethnicity	No	No	Yes	No	No	Yes	No	No	Yes
Fit statistics									
Observations	59,224	59,224	59,217	83,937	83,937	83,928	76,634	76,634	76,626
R-squared	0.129	0.166	0.166	0.020	0.051	0.051	0.030	0.083	0.083

Notes: Standard errors in parentheses, clustered at the province level. All regressions include province, birth-cohort, and survey-year fixed effects and are weighted using DHS sampling weights. Birth-by-age outcomes are coded among women old enough to have reached the respective age threshold; Age at first birth is defined for parous women. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Table 3: Reduced-Form Estimates of Educational Attainment

Model:	Years of education (single years)		
	(1)	(2)	(3)
Variables			
Treated$_c$ × SHS$_p$	0.155***	0.140***	0.141***
	(0.045)	(0.042)	(0.042)
Controls			
Urban residence	No	Yes	Yes
Wealth index	No	Yes	Yes
Religion	No	No	Yes
Ethnicity	No	No	Yes
Fit statistics			
Observations	96,277	96,277	96,268
R-squared	0.118	0.292	0.294

Notes: Standard errors in parentheses, clustered at the province level. All regressions include province, birth-cohort, and survey-year fixed effects. Years of education are measured in completed single years. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Table 4: Initiation to Sexual Activity

Model:	Ever had sex		
	(1)	(2)	(3)
Variables			
Treated$_c$ × SHS$_p$	-0.003	-0.003	-0.003
	(0.003)	(0.003)	(0.003)
Controls			
Urban residence	No	Yes	Yes
Wealth index	No	Yes	Yes
Religion	No	No	Yes
Ethnicity	No	No	Yes
Fit statistics			
Number of observations	96,273	96,273	96,264
R-squared	0.374	0.388	0.389
	Age at first sex		
Model:	(1)	(2)	(3)
Variables			
Treated$_c$ × SHS$_p$	0.061**	0.035	0.035
	(0.029)	(0.027)	(0.028)
Controls			
Urban residence	No	Yes	Yes
Wealth index	No	Yes	Yes
Religion	No	No	Yes
Ethnicity	No	No	Yes
Fit statistics			
Number of observations	60,839	60,839	60,834
R-squared	0.108	0.145	0.145

Notes: Standard errors in parentheses, clustered at the province level. All regressions include province, birth-cohort, and survey-year fixed effects, are weighted using DHS sampling weights, and use the same set of individual controls as in the main specification (urban residence, household wealth index, religion, and ethnicity), added sequentially across columns. The dependent variable in Panel A is an indicator for ever having had sex; Panel B reports age at first sexual intercourse (in completed years). *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Frequency and Intensity of Sexual Activity

Model:	Had sex in last 4 weeks		
	(1)	(2)	(3)
Variables			
$\text{Treated}_c \times \text{SHS}_p$	0.005	0.006	0.006
	(0.008)	(0.008)	(0.008)
Controls			
Urban residence	No	Yes	Yes
Wealth index	No	Yes	Yes
Religion	No	No	Yes
Ethnicity	No	No	Yes
Fit statistics			
Number of observations	60,833	60,833	60,828
R-squared	0.022	0.027	0.027

Model:	Had sex in last 3 months		
	(1)	(2)	(3)
Variables			
$\text{Treated}_c \times \text{SHS}_p$	0.008	0.009	0.009
	(0.007)	(0.007)	(0.007)
Controls			
Urban residence	No	Yes	Yes
Wealth index	No	Yes	Yes
Religion	No	No	Yes
Ethnicity	No	No	Yes
Fit statistics			
Number of observations	60,833	60,833	60,828
R-squared	0.014	0.021	0.021

Model:	No. of partners in last year		
	(1)	(2)	(3)
Variables			
$\text{Treated}_c \times \text{SHS}_p$	-0.002	-0.002	-0.002
	(0.008)	(0.008)	(0.008)
Controls			
Urban residence	No	Yes	Yes
Wealth index	No	Yes	Yes
Religion	No	No	Yes
Ethnicity	No	No	Yes
Fit statistics			
Number of observations	36,081	36,081	36,081
R-squared	0.642	0.642	0.643

Notes: Standard errors in parentheses, clustered at the province level. All regressions include province, birth-cohort, and survey-year fixed effects and are weighted using DHS sampling weights. Individual controls (urban residence, household wealth index, religion, and ethnicity) are added sequentially across columns. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Modern and Traditional Contraceptive Use

	Modern or traditional family planning		
Model:	(1)	(2)	(3)
Variables			
$\text{Treated}_c \times \text{SHS}_p$	0.011	0.012	0.012
	(0.009)	(0.009)	(0.009)
Controls			
Urban residence	No	Yes	Yes
Wealth index	No	Yes	Yes
Religion	No	No	Yes
Ethnicity	No	No	Yes
Fit statistics			
Number of observations	60,839	60,839	60,834
R-squared	0.029	0.032	0.035
	Modern family planning		
Model:	(1)	(2)	(3)
Variables			
$\text{Treated}_c \times \text{SHS}_p$	0.006	0.007	0.008
	(0.006)	(0.006)	(0.006)
Controls			
Urban residence	No	Yes	Yes
Wealth index	No	Yes	Yes
Religion	No	No	Yes
Ethnicity	No	No	Yes
Fit statistics			
Number of observations	60,839	60,839	60,834
R-squared	0.030	0.032	0.035

Notes: Standard errors in parentheses, clustered at the province level. All regressions include province, birth-cohort, and survey-year fixed effects and are weighted using DHS sampling weights. Individual controls (urban residence, household wealth index, religion, and ethnicity) are added sequentially across columns. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Robustness to Alternative Trend-Absorbing Specifications

Model:	Age at first birth				
	$(1)^{\dagger}$	(2)	(3)	(4)	(5)
Variables					
$\text{Treated}_c \times \text{SHS}_p$	0.137***	0.040	0.040	0.107***	0.115***
	(0.035)	(0.029)	(0.039)	(0.033)	(0.033)
Trend-absorbing and additional controls					
Province-Specific Cohort Trend	No	Yes	No	No	No
Region $\times$ cohort FE	No	No	Yes	No	No
Province $\times$ survey-year FE	No	No	No	Yes	No
Province-level fertility controls (DHS)	No	No	No	No	Yes
Fit statistics					
Observations	59,217	59,217	59,203	59,217	59,217
R-squared	0.166	0.169	0.184	0.173	0.170
Model:	Had a birth by age 18				
	$(6)^{\dagger}$	(7)	(8)	(9)	(10)
Variables					
$\text{Treated}_c \times \text{SHS}_p$	-0.006**	-0.003	-0.005**	-0.004	-0.003
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Trend-absorbing and additional controls					
Province-specific cohort trend	No	Yes	No	No	No
Region $\times$ cohort FE	No	No	Yes	No	No
Province $\times$ survey-year FE	No	No	No	Yes	No
Province-level fertility controls (DHS)	No	No	No	No	Yes
Fit statistics					
Observations	83,928	83,928	83,928	83,928	83,928
R-squared	0.051	0.053	0.066	0.057	0.056
Model:	Had a birth by age 20				
	$(11)^{\dagger}$	(12)	(13)	(14)	(15)
Variables					
$\text{Treated}_c \times \text{SHS}_p$	-0.007	-0.004	-0.003	-0.004	-0.004
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)
Trend-absorbing and additional controls					
Province-specific cohort trend	No	Yes	No	No	No
Region $\times$ cohort FE	No	No	Yes	No	No
Province $\times$ survey-year FE	No	No	No	Yes	No
Province-level fertility controls (DHS)	No	No	No	No	Yes
Fit statistics					
Observations	76,626	76,626	76,626	76,626	76,626
R-squared	0.083	0.084	0.097	0.088	0.085

Notes: Standard errors in parentheses, clustered at the province level. A dagger (†) marks the preferred baseline specification, corresponding to columns (3), (6), and (9) of Table 2. All regressions include province, birth-cohort, and survey-year fixed effects, are weighted using DHS sampling weights, and use a common set of individual controls (urban residence, household wealth-quintile, religion, and ethnicity). Province-level fertility controls constructed are from DHS microdata (Appendix 8.2). Birth-by-age outcomes are coded among women old enough to have reached the respective age threshold; age at first birth is defined for parous women. *, **, *** denote significance at the 10%, 5%, and 1% levels.

Table 8: Robustness to Alternative Comparison Cohort Windows

Model:	Age at first birth			Had a birth by age 18			Had a birth by age 20		
	(1)†	(2)	(3)	(4)†	(5)	(6)	(7)†	(8)	(9)
Variables									
Treated$_c$ × SHS$_p$	0.137***	0.032	0.040	-0.006**	-0.007	-0.008**	-0.007	0.002	-0.009*
	(0.035)	(0.042)	(0.033)	(0.003)	(0.004)	(0.003)	(0.004)	(0.009)	(0.005)
Cohort-window restrictions									
Full estimation sample	Yes	No	No	Yes	No	No	Yes	No	No
Cohorts 1998–2002 (±2)	No	Yes	No	No	Yes	No	No	Yes	No
Cohorts 1997–2003 (±3)	No	No	Yes	No	No	Yes	No	No	Yes
Fit statistics									
Observations	59,217	1,550	2,311	83,928	6,419	9,061	76,626	4,538	6,110
R-squared	0.166	0.347	0.334	0.051	0.057	0.066	0.083	0.102	0.109

Notes: Standard errors in parentheses, clustered at the province level. Daggers (†) mark preferred baseline specifications using the full estimation sample, corresponding to the main estimates in Table 2. All regressions include province, birth-cohort, and survey-year fixed effects and the full set of individual controls (urban residence, household wealth-quintile, and indicators for religion and ethnicity) and are weighted using DHS sampling weights. Birth-by-age outcomes are coded among women old enough to have reached the respective age threshold; age at first birth is defined for parous women. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Robustness to Alternative SHS Exposure Definitions

Model:	Age at first birth			
	$(1)^{\dagger}$	(2)	(3)	(4)
Variables				
$\text{Treated}_c \times \text{SHS}_p$ (continuous)	0.137***	–	–	–
	(0.035)			
$\text{Treated}_c \times \text{HighSHS}_p$ (binary)	–	0.586***	0.481***	0.430*
		(0.159)	(0.154)	(0.220)
Exposure definition				
High-exposure: $\geq$ median	No	Yes	No	No
High-exposure: $\geq$ 75th percentile	No	No	Yes	No
High-exposure: $\geq$ 90th percentile	No	No	No	Yes
Fit statistics				
Observations	59,217	59,217	59,217	59,217
R-squared	0.166	0.166	0.166	0.166

Model:	Had a birth by age 18			
	$(5)^{\dagger}$	(6)	(7)	(8)
Variables				
$\text{Treated}_c \times \text{SHS}_p$ (continuous)	-0.006**	–	–	–
	(0.003)			
$\text{Treated}_c \times \text{HighSHS}_p$ (binary)	–	-0.034***	-0.030**	-0.010
		(0.012)	(0.013)	(0.013)
Exposure definition				
High-exposure: $\geq$ median	No	Yes	No	No
High-exposure: $\geq$ 75th percentile	No	No	Yes	No
High-exposure: $\geq$ 90th percentile	No	No	No	Yes
Fit statistics				
Observations	83,928	83,928	83,928	83,928
R-squared	0.051	0.051	0.051	0.051

Model:	Had a birth by age 20			
	$(9)^{\dagger}$	(10)	(11)	(12)
Variables				
$\text{Treated}_c \times \text{SHS}_p$ (continuous)	-0.007	–	–	–
	(0.004)			
$\text{Treated}_c \times \text{HighSHS}_p$ (binary)	–	-0.041**	-0.030	0.031*
		(0.020)	(0.019)	(0.016)
Exposure definition				
High-exposure: $\geq$ median	No	Yes	No	No
High-exposure: $\geq$ 75th percentile	No	No	Yes	No
High-exposure: $\geq$ 90th percentile	No	No	No	Yes
Fit statistics				
Observations	76,626	76,626	76,626	76,626
R-squared	0.083	0.083	0.083	0.083

Notes: Standard errors in parentheses, clustered at the province level. A dagger (†) marks the preferred baseline specification, corresponding to columns (3), (6), and (9) of Table 2. All regressions include province, birth-cohort, and survey-year fixed effects, are weighted using DHS sampling weights, and use a common set of individual controls (urban residence, household wealth-quintile, religion, and ethnicity). Birth-by-age outcomes are coded among women old enough to have reached the respective age threshold; age at first birth is defined for parous women. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.
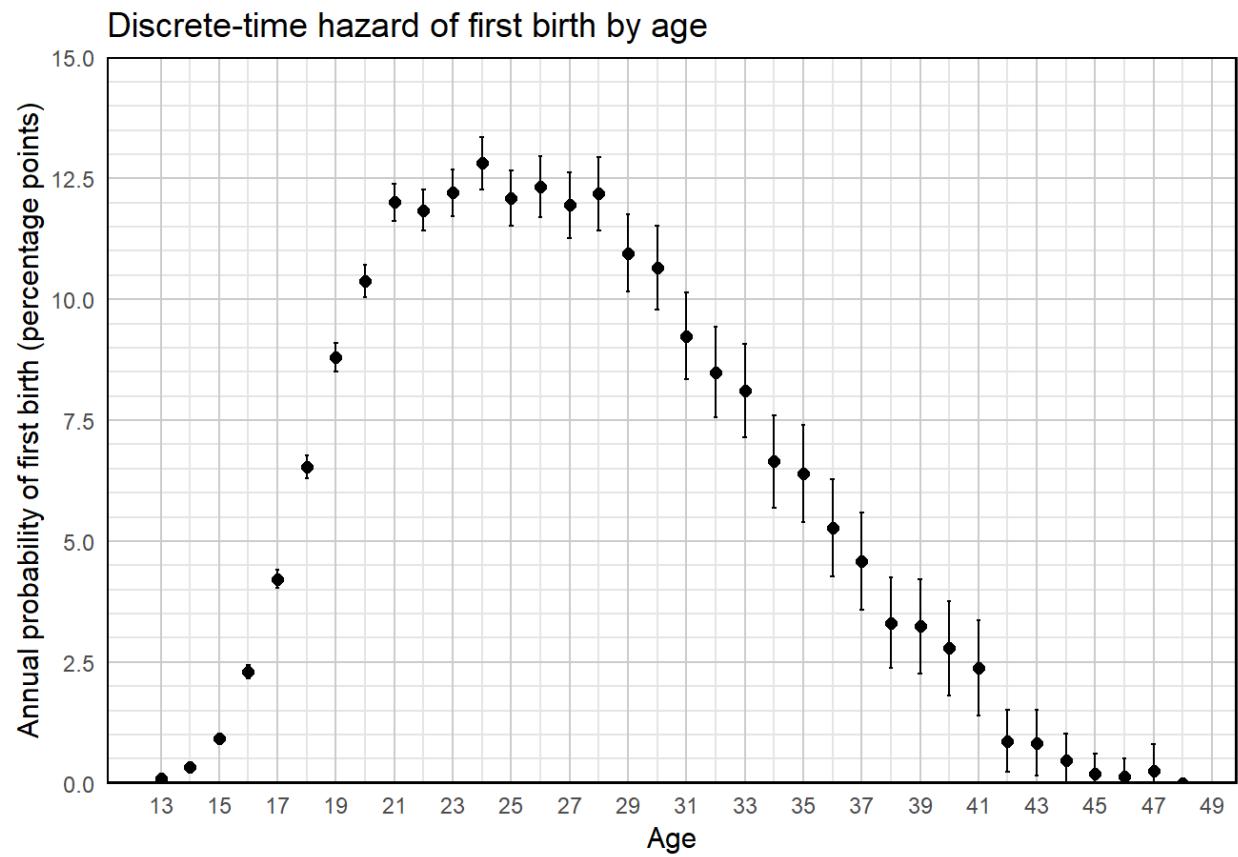
Figure 2: Discrete-Time Hazard of First Birth by Age

Notes: This figure plots the unconditional annual probability of first birth by age. Points show estimates and bars show 95% confidence intervals. Appendix 8.3 details the construction of the hazard model. The same appendix also reports the full set of age-specific hazard estimates underlying the figure, specifically, Table 14.

Table 10: Robustness to Excluding NCR and BARMM

Model:	Age at first birth			
	$(1)^{\dagger}$	(2)	(3)	(4)
Variables				
$\text{Treated}_c \times \text{SHS}_p$	0.137***	0.101***	0.134***	0.096**
	(0.035)	(0.035)	(0.037)	(0.038)
Excluded regions				
Exclude NCR	No	Yes	No	Yes
Exclude BARMM	No	No	Yes	Yes
Fit statistics				
Number of observations	59,217	54,221	56,069	51,073
R-squared	0.166	0.166	0.166	0.166

Model:	Had a birth by age 18			
	$(5)^{\dagger}$	(6)	(7)	(8)
Variables				
$\text{Treated}_c \times \text{SHS}_p$	-0.006**	-0.007***	-0.005*	-0.006**
	(0.003)	(0.003)	(0.003)	(0.003)
Excluded regions				
Exclude NCR	No	Yes	No	Yes
Exclude BARMM	No	No	Yes	Yes
Fit statistics				
Number of observations	83,928	75,621	79,340	71,033
R-squared	0.051	0.051	0.051	0.051

Model:	Had a birth by age 20			
	$(9)^{\dagger}$	(10)	(11)	(12)
Variables				
$\text{Treated}_c \times \text{SHS}_p$	-0.007	-0.005	-0.006	-0.004
	(0.004)	(0.004)	(0.005)	(0.004)
Excluded regions				
Exclude NCR	No	Yes	No	Yes
Exclude BARMM	No	No	Yes	Yes
Fit statistics				
Number of observations	76,626	69,028	72,521	64,923
R-squared	0.083	0.081	0.083	0.081

Notes: Standard errors in parentheses, clustered at the province level. A dagger (†) marks the preferred baseline specification, corresponding to columns (3), (6), and (9) of Table 2. All regressions include province, birth-cohort, and survey-year fixed effects, are weighted using DHS sampling weights, and use a common set of individual controls (urban residence, household wealth-quintile, religion, and ethnicity). *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 11: Robustness to Leave-One-Province-Out (LOPO) Exclusions

	Age at first birth	Had a birth by 18	Had a birth by 20
Variables			
$\text{Treated}_c \times \text{SHS}_p{}^{\dagger}$	0.137***	-0.006**	-0.007
	(0.035)	(0.003)	(0.004)
LOPO Distribution			
Mean coefficient	0.137	-0.006	-0.007
Std. dev.	0.004	0.000	0.001
LOPO Extremes			
Minimum	0.112	-0.008	-0.009
	(Capital District)	(Eastern Manila District)	(Eastern Manila District)
Maximum	0.149	-0.005	-0.004
	(Surigao del Sur)	(Capital District)	(Capital District)

Notes: Entries summarize leave-one-province-out (LOPO) estimates for the coefficient for $\text{Treated}_c \times \text{SHS}_p$. The baseline rows report the full-sample difference-in-differences estimate and its standard error, as in columns (3), (6), and (9) of Table 2. Minimum and maximum rows identify the smallest and largest LOPO coefficients and the province whose exclusion generates each value. All regressions include province, birth-cohort, and survey-year fixed effects, are weighted using DHS sampling weights, and use a common set of individual controls (urban residence, household wealth-quintile, religion, and ethnicity). *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively. The full set of leave-one-province-out estimates is reported in Table 15.

Table 12: Robustness to Alternative Pseudo-Reform Cutoffs

	Age at first birth				
Model:	$(1)^\dagger$	(2)	(3)	(4)	(5)
Variables					
$\text{Treated}_c \times \text{SHS}_p$	0.137***	0.000	0.056**	0.067**	0.064**
	(0.035)	(0.000)	(0.023)	(0.027)	(0.026)
Cutoff					
1975	No	Yes	No	No	No
1980	No	No	Yes	No	No
1985	No	No	No	Yes	No
1990	No	No	No	No	Yes
Fit statistics					
Number of observations	59,217	34,432	34,432	34,432	34,432
R-squared	0.166	0.168	0.168	0.168	0.168
	Had a birth by age 18				
Model:	$(6)^\dagger$	(7)	(8)	(9)	(10)
Variables					
$\text{Treated}_c \times \text{SHS}_p$	-0.006**	0.000	-0.002	-0.002	-0.005**
	(0.003)	(0.000)	(0.002)	(0.002)	(0.002)
Cutoff					
1975	No	Yes	No	No	No
1980	No	No	Yes	No	No
1985	No	No	No	Yes	No
1990	No	No	No	No	Yes
Fit statistics					
Number of observations	83,928	46,295	46,295	46,295	46,295
R-squared	0.051	0.052	0.052	0.052	0.052
	Had a birth by age 20				
Model:	$(11)^\dagger$	(12)	(13)	(14)	(15)
Variables					
$\text{Treated}_c \times \text{SHS}_p$	-0.007	0.000	-0.001	-0.002	-0.000
	(0.004)	(0.000)	(0.002)	(0.002)	(0.002)
Cutoff					
1975	No	Yes	No	No	No
1980	No	No	Yes	No	No
1985	No	No	No	Yes	No
1990	No	No	No	No	Yes
Fit statistics					
Number of observations	76,626	44,315	44,315	44,315	44,315
R-squared	0.083	0.083	0.083	0.083	0.083

Notes: Standard errors in parentheses, clustered at the province level. A dagger (†) marks the preferred baseline specification, corresponding to columns (3), (6), and (9) of Table 2. All regressions include province, birth-cohort, and survey-year fixed effects, are weighted using DHS sampling weights, and use a common set of individual controls (urban residence, household wealth-quintile, religion, and ethnicity). *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

8 Appendix

8.1 Migration and Exposure Misclassification

To evaluate whether migration threatens the assignment of SHS exposure, we use the DHS question on the number of years a woman has lived in her current place of residence. We convert this measure into an approximate age at last move by subtracting years-in-residence from the respondent's age:

$$\text{move\_age}_i = \text{age}_i - \text{yearsHere}_i.$$

We define migration after SHS-entry age as:

$$\text{moved\_after16}_i = \mathbb{1}(\text{age}_i > 16),$$

where age 16 corresponds to the official entrant age to Grade 11 under Republic Act 10533. This definition identifies individuals most likely to be misclassified if province of current residence differed from the province where secondary schooling occurred.

Table 13 presents weighted migration rates for pre-reform cohorts (born before 2000) and SHS-exposed cohorts (born in 2000 or later). Two patterns emerge. First, migration after age 16 is extremely rare among SHS-exposed cohorts: only **5.1%** report moving across provinces at or after age 16. Second, the higher incidence among older cohorts (37.4%) reflects cumulative lifetime mobility rather than movement around secondary-school ages.

Because almost all SHS-eligible women remain in the same province through age 16, misclassification of exposure based on province of residence is minimal.

8.2 Construction of Province-Level Fertility Controls

We describe the construction of the province-by-survey-year fertility aggregates and explain their role in the trend-absorbing robustness checks. These aggregates capture demographic movements that may evolve independently of the SHS reform and may bias the DiD estimates if they correlate with variation in SHS rollout. Let p index provinces, t index DHS survey

Table 13: Migration After Age 16 (Approximate), by Birth Cohort

	Stayed before 16	Moved at 16+	Total
Pre-2000 cohorts	62.62%	37.38%	100%
Born 2000+	94.87%	5.13%	100%
All cohorts	66.51%	33.49%	100%

Notes: Table reports weighted percentages using NDHS sampling weights. *Moved at 16+* is an indicator equal to 1 if the respondent's approximate age at last move is at least 16. Treated cohorts are women born in 2000 or later, the first cohorts eligible to enter Grade 11 under the 2016–17 SHS rollout.

years, and i index women in the Household Member Recode (PR). All statistics use sampling weights w_{it}.

Firstly, we calculate the mean number of children ever born among women ages 40–49:

$$\text{pf\_ceb\_40\_49}_{pt} = \frac{\sum_{i \in (p,t)} w_{it} \cdot \text{CEB}_{it} \cdot \mathbf{1}(40 \leq \text{age}_{it} \leq 49)}{\sum_{i \in (p,t)} w_{it} \cdot \mathbf{1}(40 \leq \text{age}_{it} \leq 49)}.$$

This variable measures slow-moving fertility conditions inside each province. These conditions differ across provinces and evolve over time. They may also correlate with the intensity of SHS expansion. Without adjustment, they may load onto the treatment variable and affect the estimates.

Consequently, we calculate early fertility norms, proxied by the share of women ages 18–49 who had a live birth before age 18:

$$\text{pf\_birthby18}_{pt} = \frac{\sum_{i \in (p,t)} w_{it} \cdot \mathbf{1}(\text{ageFirstBirth}_{it} < 18) \cdot \mathbf{1}(18 \leq \text{age}_{it} \leq 49)}{\sum_{i \in (p,t)} w_{it} \cdot \mathbf{1}(18 \leq \text{age}_{it} \leq 49)}.$$

This statistic captures province-level norms and constraints that influence very early fertility. These norms shift over time and show heterogeneity across provinces. If these shifts align with SHS rollout, they may confound the reform's estimated effect.

Analogously, we compute the share of women ages 25–49 who had a live birth before age

25:

$$\text{pf\_birthby25}_{pt} = \frac{\sum_{i \in (p,t)} w_{it} \cdot \mathbf{1}(\text{ageFirstBirth}_{it} < 25) \cdot \mathbf{1}(25 \leq \text{age}_{it} \leq 49)}{\sum_{i \in (p,t)} w_{it} \cdot \mathbf{1}(25 \leq \text{age}_{it} \leq 49)}.$$

This statistic reflects medium-run movements in fertility timing. Provinces follow distinct paths with respect to early-adult fertility. These paths may not arise from the reform, yet may coincide with variation in SHS exposure. This coincidence poses a risk of bias.

These aggregates form a fertility-control vector that absorbs demographic movements at the province-year level. The event-study diagnostics reveal mild deviations from parallel pre-trends among older cohorts. The direction of these deviations matches long-run demographic drift, not anticipation of the reform. By incorporating the province-year aggregates into the robustness specifications, we allow provinces to follow distinct fertility paths across survey waves. This restriction limits the identifying variation to within-province changes across adjacent cohorts after accounting for long-run and medium-run demographic movements. This adjustment strengthens the credibility of the causal interpretation, since it prevents secular fertility transitions from entering the treatment effect.

8.3   Discrete-Time Hazard Model for First Birth

Whereas the primary analysis studies cumulative fertility indicators—such as having had a birth by age 18 or 20, and age at first birth—the hazard framework models the *annual transition into motherhood*, conditioning on remaining childless up to each age. This approach decomposes cumulative timing measures into their underlying age-specific risks and mitigates concerns that threshold-based outcomes mechanically reflect differences in censoring or interview timing across cohorts.

For each woman i, we observe her age at interview a_i and her age at first birth T_i, both measured in completed years. We define the risk window as $[a_{\min}, a_{\max}]$, set to $[12, 49]$ in the baseline specification. We first restrict the sample by excluding women who report a first birth prior to age $a_{\min}$, so that all women are childless at entry into the risk set. Each woman

42

then contributes a sequence of person–year observations for ages

$$a \in \{a_{\min}, a_{\min} + 1, \ldots, \min(a_i, a_{\max})\}.$$

The resulting stacked dataset contains one row for each woman–age pair (i, a), representing a year in which the woman is at risk of experiencing her first birth.

The discrete-time event indicator is defined as

$$Y_{ia} = \begin{cases} 1 & \text{if } T_i = a, \\ 0 & \text{if } T_i > a \text{ or } T_i \text{ is unobserved by age } a. \end{cases}$$

Women who have not had a first birth by the interview date contribute $Y_{ia} = 0$ for all ages in which they remain at risk. To ensure that each woman contributes at most one event, all person–year observations strictly after the first birth are excluded. For example, a woman whose first birth occurs at age 19 contributes zero-valued outcomes for ages 12–18, contributes one event at age 19, and contributes no observations thereafter.

Let h_{ia} denote the discrete-time hazard of first birth at age a:

$$h_{ia} \equiv \Pr(Y_{ia} = 1 \mid Y_{ia'} = 0 \text{ for all } a' < a, \, X_i, \, \text{FE}).$$

We estimate this hazard using a linear probability model with fixed effects:

$$Y_{ia} = \alpha_a + \beta\big(\text{SHS}_p \times \text{Post}_c\big) + X_i'\gamma + \mu_p + \lambda_c + \delta_t + \varepsilon_{ia}, \tag{3}$$

where α_a is a full set of single-year age indicators capturing the baseline hazard; SHS_p is province-level SHS exposure measured as SHS schools per 1,000 Grade 10 students; and Post_c is an indicator for SHS-eligible cohorts (born in 2000 or later). The fixed effects $(\mu_p, \lambda_c, \delta_t)$ correspond to province, birth-cohort, and survey-year fixed effects. The vector X_i includes the same individual controls used in the main timing specifications: urban residence,

wealth-quintile indicators, and indicators for religion and ethnicity. All regressions apply DHS sampling weights, and standard errors are clustered at the province level.

The coefficient β measures how SHS exposure shifts the annual probability of experiencing a first birth at age a, conditional on having remained childless through age $a - 1$. A negative value of β therefore indicates that higher SHS exposure reduces the likelihood of entry into motherhood at a given age among women still at risk.

This hazard framework complements the main difference-in-differences results in three respects. First, whereas cumulative timing indicators aggregate fertility events over an interval, the hazard model isolates the period-specific transition into motherhood and clarifies whether SHS expansion primarily affects the *timing* of early fertility rather than only cumulative incidence by a given age. Second, because the hazard specification conditions on survival to age a, it is less sensitive to mechanical differences in censoring arising from variation in interview timing across cohorts. Third, the inclusion of a fully flexible baseline hazard $\{\alpha_a\}$ allows the fertility–age profile to vary nonparametrically with age, reducing the risk that the estimated treatment effect reflects differences in the shape of age-specific fertility risk rather than treatment-related shifts.

In addition to the regression estimates, we report empirical age-specific hazards in levels. For each age a, we compute

$$\hat{h}(a) = \Pr(Y_{ia} = 1 \mid i \text{ is in the risk set at age } a)$$

as the DHS-weighted mean of the hazard outcome among women who remain childless at age a. These age-specific annual probabilities summarize the observed hazard profile of first birth in the data and are reported alongside the estimated treatment effect in Table 14.

Before presenting the full set of age–specific hazard coefficients, we note although the risk window is defined over ages 12–49, we report empirical age–specific hazards for ages 13–48 only, excluding ages at which the risk set is either degenerate (age 12) or extremely

sparse (ages 48–49). The table below displays the SHS×Post coefficient and the estimated age–specific hazards for ages 13–48.

Table 14: Full Discrete-Time Hazard Model Estimates for Age at First Birth

	Coefficient	
Main regressor		
$\text{Treated}_c \times \text{SHS}_p$	-0.0031***	(0.0009)
Age-specific baseline hazards		
Age 13	0.0010***	(0.0001)
Age 14	0.0033***	(0.0003)
Age 15	0.0091***	(0.0004)
Age 16	0.0230***	(0.0007)
Age 17	0.0422***	(0.0009)
Age 18	0.0653***	(0.0012)
Age 19	0.0880***	(0.0015)
Age 20	0.1038***	(0.0017)
Age 21	0.1201***	(0.0020)
Age 22	0.1184***	(0.0022)
Age 23	0.1220***	(0.0024)
Age 24	0.1281***	(0.0028)
Age 25	0.1210***	(0.0029)
Age 26	0.1233***	(0.0032)
Age 27	0.1194***	(0.0035)
Age 28	0.1218***	(0.0039)
Age 29	0.1095***	(0.0041)
Age 30	0.1066***	(0.0044)
Age 31	0.0924***	(0.0045)
Age 32	0.0849***	(0.0048)
Age 33	0.0811***	(0.0049)
Age 34	0.0665***	(0.0049)
Age 35	0.0639***	(0.0052)
Age 36	0.0527***	(0.0051)
Age 37	0.0459***	(0.0051)
Age 38	0.0331***	(0.0048)
Age 39	0.0324***	(0.0050)
Age 40	0.0278***	(0.0050)
Age 41	0.0238***	(0.0050)
Age 42	0.0087***	(0.0033)

Continued on next page

Table 14 (continued)

	(1) Full controls	
Age 43	0.0083**	(0.0035)
Age 44	0.0046*	(0.0028)
Age 45	0.0019	(0.0021)
Age 46	0.0013	(0.0019)
Age 47	0.0024	(0.0028)
Age 48	0.0000	(0.0000)
Fit statistics		
Observations	649,734	
R^2	0.0981	

Notes: The dependent variable is an indicator for having a first birth at age a (discrete-time hazard). Each woman contributes one person-year observation for every age between 12 and 49 (or her interview age, whichever is smaller) until her first birth occurs; ages after first birth are excluded from the risk set. The model is estimated as a linear probability model with province, birth-cohort, survey-year, and single-year age fixed effects, and is weighted using DHS sampling weights. Controls include urban residence, household wealth-quintile dummies, and indicators for religion and ethnicity. Coefficients are interpreted as percentage-point changes in the annual probability of first birth at age a. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

8.4 Leave-One-Province-Out (LOPO) Estimates

Table 15 reports the complete set of leave-one-province-out (LOPO) estimates, listing the treatment effects obtained after sequentially excluding each province from the estimation sample. These results are provided for completeness; a summary of the LOPO exercise is discussed in Table 11.

Table 15: Full Leave-One-Province-Out (LOPO) Estimates by Dropped Province

Excluded province	Age at first birth	Had a birth by 18	Had a birth by 20
None (Original)	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
			Continued on next page

Table 15 (continued)

Removed province	Age at first birth	Had a birth by 18	Had a birth by 20
Abra	0.141*** (0.036)	-0.007** (0.003)	-0.007* (0.004)
Agusan del Norte	0.135*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Agusan del Sur	0.135*** (0.036)	-0.007** (0.003)	-0.007 (0.004)
Aklan	0.140*** (0.036)	-0.007** (0.003)	-0.007 (0.004)
Albay	0.132*** (0.035)	-0.006** (0.003)	-0.007* (0.004)
Antique	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Apayao	0.140*** (0.037)	-0.007** (0.003)	-0.007 (0.004)
Aurora	0.137*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Basilan	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Bataan	0.140*** (0.036)	-0.006** (0.002)	-0.007 (0.004)
Batanes	0.137*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Batangas	0.134*** (0.036)	-0.007** (0.003)	-0.007 (0.004)
Benguet	0.140*** (0.036)	-0.007** (0.003)	-0.007 (0.004)
Biliran	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Bohol	0.135*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Bukidnon	0.134*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Bulacan	0.137*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Cagayan	0.138*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Camarines Norte	0.138*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Camarines Sur	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Camiguin	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Capital District	0.112*** (0.028)	-0.005** (0.002)	-0.004 (0.004)
Capiz	0.140*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Catanduanes	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Cavite	0.138*** (0.035)	-0.006** (0.002)	-0.006 (0.004)
Cebu	0.136*** (0.035)	-0.006** (0.003)	-0.006 (0.004)
Compostela Valley	0.136*** (0.035)	-0.006** (0.002)	-0.007 (0.004)
Davao del Norte	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Davao del Sur	0.130*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Davao Oriental	0.138*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Dinagat Islands	0.137*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Eastern Manila District	0.136*** (0.036)	-0.008*** (0.002)	-0.009** (0.004)
Eastern Samar	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Guimaras	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)

Continued on next page

Table 15 (continued)

Removed province	Age at first birth	Had a birth by 18	Had a birth by 20
Ifugao	0.140*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Ilocos Norte	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Ilocos Sur	0.138*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Iloilo	0.137*** (0.036)	-0.006** (0.003)	-0.006 (0.004)
Isabela	0.132*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Kalinga	0.137*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
La Union	0.143*** (0.035)	-0.006** (0.003)	-0.007* (0.004)
Laguna	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Lanao del Norte	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Lanao del Sur	0.134*** (0.036)	-0.006** (0.002)	-0.006 (0.004)
Leyte	0.139*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Maguindanao	0.136*** (0.035)	-0.006** (0.002)	-0.006 (0.004)
Marinduque	0.137*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Masbate	0.138*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Misamis Occidental	0.137*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Misamis Oriental	0.136*** (0.035)	-0.006** (0.002)	-0.007 (0.004)
Mountain Province	0.147*** (0.037)	-0.007** (0.003)	-0.007 (0.005)
Negros Occidental	0.134*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Negros Oriental	0.135*** (0.036)	-0.006** (0.003)	-0.008* (0.004)
North Cotabato	0.126*** (0.035)	-0.006** (0.003)	-0.008* (0.004)
Northern Manila District	0.141*** (0.042)	-0.006** (0.003)	-0.006 (0.005)
Northern Samar	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Nueva Ecija	0.143*** (0.035)	-0.007** (0.003)	-0.007 (0.004)
Nueva Vizcaya	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Occidental Mindoro	0.138*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Oriental Mindoro	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Palawan	0.134*** (0.036)	-0.006** (0.003)	-0.008* (0.004)
Pampanga	0.131*** (0.036)	-0.006** (0.002)	-0.006 (0.004)
Pangasinan	0.138*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Quezon	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Quirino	0.136*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Rizal	0.135*** (0.036)	-0.006** (0.002)	-0.007 (0.004)
Romblon	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Samar	0.137*** (0.036)	-0.006** (0.003)	-0.007 (0.004)

48

Table 15 (continued)

Removed province	Age at first birth	Had a birth by 18	Had a birth by 20
Sarangani	0.135*** (0.037)	-0.006** (0.003)	-0.008* (0.004)
Siquijor	0.138*** (0.036)	-0.006** (0.003)	-0.007 (0.004)
Sorsogon	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
South Cotabato	0.140*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Southern Leyte	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Southern Manila District	0.131*** (0.036)	-0.007** (0.003)	-0.007 (0.005)
Sultan Kudarat	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Sulu	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Surigao del Norte	0.137*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Surigao del Sur	0.149*** (0.035)	-0.007*** (0.003)	-0.007 (0.004)
Tarlac	0.133*** (0.036)	-0.006** (0.002)	-0.007 (0.004)
Tawi-Tawi	0.137*** (0.037)	-0.006** (0.003)	-0.007 (0.005)
Zambales	0.138*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Zamboanga del Norte	0.135*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Zamboanga del Sur	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)
Zamboanga Sibugay	0.136*** (0.035)	-0.006** (0.003)	-0.007 (0.004)

Notes: Each row reports the coefficient on $\text{Treated}_c \times \text{SHS}_p$ from re-estimating the main difference-in-differences specification after excluding the indicated province from the sample. Standard errors (in parentheses) are clustered at the province level. The baseline rows report the full-sample difference-in-differences estimate and its standard error, as in columns (3), (6), and (9) of Table 2. All regressions include province, birth-cohort, and survey-year fixed effects, are weighted using DHS sampling weights, and use a common set of individual controls (urban residence, household wealth-quintile, religion, and ethnicity). *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

References

Andalón, M., Williams, J., and Grossman, M. (2014). Empowering women: The effect of schooling on young women's knowledge and use of contraception. Technical report, National Bureau of Economic Research.

Angrist, J. D. and Krueger, A. B. (1991). Does compulsory school attendance affect schooling and earnings? *The Quarterly Journal of Economics*, 106(4):979–1014.

Ashraf, N., Field, E., and Lee, J. (2014). Household bargaining and excess fertility: an experimental study in Zambia. *American Economic Review*, 104(7):2210–2237.

Baird, S., Chirwa, E., McIntosh, C., and Özler, B. (2010). The short-term impacts of a schooling conditional cash transfer program on the sexual behavior of young women. *Health Economics*, 19(S1):55–68.

Becker, G. S. (1960). An economic analysis of fertility. In *Demographic and economic change in developed countries*, pages 209–240. Columbia University Press.

Breierova, L. and Duflo, E. (2004). The impact of education on fertility and child mortality: Do fathers really matter less than mothers?

Chaijaroen, P. and Panda, P. (2023). Women's education, marriage, and fertility outcomes: Evidence from Thailand's compulsory schooling law. *Economics of Education Review*, 96:102440.

Chou, S.-Y., Liu, J.-T., Grossman, M., and Joyce, T. (2010). Parental education and child health: evidence from a natural experiment in Taiwan. *American Economic Journal: Applied Economics*, 2(1):33–61.

Clark, D. and Royer, H. (2013). The effect of education on adult mortality and health: Evidence from Britain. *American Economic Review*, 103(6):2087–2120.

Demographic and Health Surveys (2023). Philippines demographic and health survey 2022.

Department of Education (2018). DepEd Order No. 031, s. 2018: Policy Guidelines on the Implementation of the Comprehensive Sexuality Education.

Duflo, E., Dupas, P., and Kremer, M. (2015). Education, HIV, and early fertility: Experimental evidence from Kenya. *American Economic Review*, 105(9):2757–2797.

Duflo, E., Dupas, P., and Kremer, M. (2021). The impact of free secondary education: Experimental evidence from Ghana. Technical report, National Bureau of Economic Research.

Espinosa, A. and Marasigan, A. (2025). What's Failing Philippine Education: Learning or Leadership?

Güneş, P. M. (2015). The role of maternal education in child health: Evidence from a compulsory schooling law. *Economics of Education Review*, 47:1–16.

Kırdar, M. G., Dayıoğlu, M., and Koç, İ. (2018). The effects of compulsory-schooling laws on teenage marriage and births in Turkey. *Journal of Human Capital*, 12(4):640–668.

McCrary, J. and Royer, H. (2011). The effect of female education on fertility and infant health: evidence from school entry policies using exact date of birth. *American Economic Review*, 101(1):158–195.

Orbeta, A. C., Lagarto, M. B., Ortiz, M. K. P., Ortiz, D. A. P., and Potestad, M. V. (2018). Senior high school and the labor market: Perspectives of grade 12 students and human resource officers. Technical report, PIDS Discussion Paper Series.

Orbeta Jr, A. C. and Potestad, M. V. (2025). On the Employability of Senior High School Graduates in the Philippines: Evidence from the Labor Force Survey. *Philippine Journal of Development*, 49(1):85–136.

Oreopoulos, P. (2006). Estimating average and local average treatment effects of education when compulsory schooling laws really matter. *American Economic Review*, 96(1):152–175.

Philippine Qualifications Framework (2017). The Philippine Education and Training System. https://pqf.gov.ph/Home/Details/16.

Philippine Statistics Authority (2019). 2019 functional literacy, education, and mass media survey.

Philippine Statistics Authority (2020). Teenage pregnancy in the philippines: Vital statistics report.

Philippine Statistics Authority (2022). Fertility of women and childbearing patterns in the philippines.

Ponce de Leon, I. (2023). A decade of tweaking, rethinking: Looking back at K-12 program. *Philippine Daily Inquirer.*

Republic of the Philippines (1987). 1987 Philippine Constitution: Constitution of the Republic of the Philippines.

Republic of the Philippines (1988). Republic Act No. 6655: An Act Establishing and Providing for a Free Public Secondary Education and for Other Purposes.

Republic of the Philippines (1994). Republic Act No. 7797: An Act to Lengthen the School Calendar from Two Hundred (200) Days to Not More Than Two Hundred Twenty (220) Class Days.

Republic of the Philippines (2012). Republic Act No. 10354: An Act Providing for a National Policy on Responsible Parenthood and Reproductive Health.

Republic of the Philippines (2013). Republic Act No. 10533: An Act Enhancing the Philippine Basic Education System by Strengthening its Curriculum and Increasing the Number of Years for Basic Education, Appropriating Funds Therefore and for Other Purposes.

Republic of the Philippines (2017). Republic Act No. 10931: An Act Promoting Universal Access to Quality Tertiary Education by Providing for Free Tuition and Other School Fees in State Universities and Colleges, Local Universities and Colleges and State-Run Technical Vocational Institutions, Establishing the Tertiary Education Subsidy and Student Loan Program, Strengthening the Unified Student Financial Assistance System for Tertiary Education and Appropriating Funds Therefor.

Romero, N. (2020). Postcolonial philosophy of education in the Philippines. In *Oxford Research Encyclopedia of Education.*

Ronda, R. A. (2016). DepEd OKs 80,000 vouchers for Grade 11 students. *The Philippine Star.*

Salama, O. (2025). The current teenage pregnancy crisis in the philippines.

World Bank Group (2016). *Assessing Basic Education Service Delivery in the Philippines: Public Education Expenditure Tracking and Quantitative Service Delivery Study.* World Bank.