The Impact of Adolescent-Friendly Family Planning Services on Teenage Fertility in Peru*

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Abstract

This paper studies the impact of a family planning reform in Peru that removed age-based restrictions and introduced confidential and adolescent-friendly services. Using a difference-in-differences design with administrative birth records and nationally representative survey data, I find that the reform significantly reduced adolescent fertility—by up to 15% overall and 21% among girls living near treated facilities. These effects were concentrated among adolescents from wealthier, urban, and male-headed households, and were driven by increased condom use, better contraceptive knowledge, and a possible transition toward more formal sources of information. The reform also led to declines in early cohabitation and modest improvements in school progression. Although COVID-19 temporarily disrupted these gains, fertility reductions resumed once services reopened, showing how the importance of institutional access and service continuity in shaping adolescent reproductive outcomes.

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1 Introduction

Teenage pregnancy continues to be a persistent policy challenge across many low- and middle-income countries (LMICs), despite its central place in the Sustainable Development Goals (SDGs). While adolescent fertility has declined in parts of the world, it remains high in regions where access to sexual and reproductive health services is limited, stigmatised, or conditional. Early childbearing is associated with well-documented negative consequences on educational attainment, labor market participation, and upward mobility, making it both a public health and development concern (Aizer et al., 2022; Azevedo et al., 2012; Lang and Weinstein, 2015). In Latin America, adolescent fertility remains elevated despite broad improvements in education, health infrastructure, and declining total fertility rates (Azevedo et al., 2012; Sanhueza et al., 2023).

Peru illustrates this divergence particularly clearly. The total fertility rate has fallen from around 6.7 children per woman in the 1970s to just over 2.2 in recent years (World Bank, 2025a). Yet adolescent fertility in Peru remained high, around the mid-50s births per 1,000 girls aged 15–19 in the mid-2010s, and exceeds levels observed in several Latin American comparators and higher-income benchmarks (e.g., 49.1 vs. 23.7 in 2018 for Peru versus Chile) (World Bank, 2025b). This gap is also large relative to the OECD average (8.7 per 1,000) (OECD, 2023). Regional and urban–rural disparities are also marked: nearly 10 percent of Peruvian girls aged 15–19 have begun childbearing, with much higher rates in rural and Amazonian regions such as Loreto and Ucayali and markedly lower rates in Lima and other urban areas (Instituto Nacional de Estadística e Informática (INEI), 2022). These patterns suggest that aggregate progress in contraceptive coverage has not fully addressed the specific institutional and social barriers faced by adolescents.

This paper evaluates a major reform to Peru's public family planning system that explicitly targeted such barriers. Studying this reform provides new evidence on a policy that specifically focused on adolescents and aimed to reduce barriers related to provider discretion, trust in confidential care, and adolescent autonomy. In 2017, the Ministry of Health implemented the Family Planning Technical Guideline (hereafter, FPTG), which standardised a rights-based approach to service delivery for adolescents and introduced protocols for adolescent-friendly family planning services for individuals under 18. In particular, the guideline strengthened protections around confidentiality, privacy, and voluntary decision-making, and promoted differentiated delivery conditions for adolescents (e.g., dedicated spaces and service hours beyond school time). A subset of existing public health centres meeting minimum infrastructure and staffing requirements was designated for upgrade to deliver adolescent-friendly services (hereafter, HCA), rather than expanding supply through new facilities. This feature generates quasi-experimental variation in access across districts and communities, against a backdrop of historically contested and uneven family planning provision in Peru (Aramburú, 2014; Defensoría del Pueblo, 2005).

I combine administrative and survey data with a difference-in-differences design to estimate the impact of this reform on adolescent fertility and related life transitions. The main analysis uses the universe of birth records (Certificado de Nacido Vivo, CNV) from 2013 to 2019 to construct age-specific fertility rates by district, age group, and year for girls aged 13–17, which I link to information on whether the district contains at least one HCA centre. I estimate a two-way fixed effects model and an event-study specification around the 2017 reform to trace dynamic effects. I complement this with individual-level evidence from the Peruvian Demographic and Health Surveys (DHS) for 2013–2019, exploiting georeferenced cluster locations to compare adolescents living within a short distance of an HCA centre to those whose nearest public centre did not receive the adolescent-friendly upgrade. Together, these data allow me to document changes in fertility, early union formation, schooling, contraceptive use, and reproductive health knowledge.

The results show that increasing access to adolescent-friendly services substantially reduced teenage fertility. In the population-level CNV birth records, exposure to an HCA centre is associated with a persistent decline in fertility among girls aged 13–17, amounting to roughly a 15% reduction relative to comparable districts without an HCA centre. The estimated effects are larger for older adolescents and in areas located closer to upgraded centres. Evidence from the individual-level DHS is consistent with this pattern: the reform reduces the probability that a girl has given birth before age 18 by around 1.5–2 percentage points (approximately 13–17% of the baseline), with stronger effects in urban areas.

The benefits of the reform, however, were not evenly distributed: fertility reductions were concentrated among adolescents from relatively more advantaged backgrounds, those living in wealthier, male-headed, and larger households, underscoring the persistence of inequalities in programme uptake despite the policy's universal design. Event-study estimates indicate that the impact strengthens in the years following implementation; however, attenuates in 2020, coinciding with Peru's COVID-19 lockdown and the temporary suspension of non-urgent reproductive health services, temporarily nullifying the gains. Notably, fertility declines resume once services are restored, reinforcing the importance of continuity in adolescent reproductive health services. This COVID-period disruption provides an unexpected stress test, highlighting that the reform's gains are sensitive to interruptions in service availability.

The reform also shifted other key life outcomes. Adolescents in treated areas are less likely to enter early cohabiting unions and are less likely to interrupt or leave school because of pregnancy, childcare responsibilities, or marriage/cohabitation, consistent with greater control over the timing of family formation and reduced role conflicts between schooling and family duties. Across outcomes, the estimates indicate effects not only on adolescent fertility, but also on early union formation and schooling, consistent with improvements in adolescents' ability to access services under conditions of confidentiality, discretion, and trust. Analysis of mechanisms suggests that these changes are driven by shifts in contraceptive practices and information sources: adolescents in treated areas move away from traditional methods

toward more autonomous methods such as condoms, and they become less reliant on family members and peers for family planning information, consistent with increased engagement with confidential, institution-based counselling.

This study contributes to several strands of the literature on contraceptive access, teenage fertility behavior, family planning policies, and service design and delivery. First, it adds to a robust body of work studying how access to modern contraception shapes fertility decisions and broader life-cycle outcomes. Vast research in high-income settings has shown that contraceptive access can dramatically alter women's educational, marital, and labor market trajectories[1] (Bailey, 2010; Bailey and Lindo, 2017; Goldin and Katz, 2002; Guldi, 2008; Lindo et al., 2020; Ragan, 2025). In their seminal paper, Goldin and Katz (2002) provided foundational evidence that access to oral contraception delayed marriage and childbearing among young women in the 1970s, potentially enhancing higher educational attainment and increasing labor market participation[2].

In contrast to high-income settings, evidence from developing countries, though extensive, has focused more on fertility outcomes, and shows mixed results. For instance, Dupas et al. (2024) find that free provision of medical contraception in Burkina Faso was insufficient to alter reproductive behavior, even when accompanied by interventions aimed at addressing potential misperceptions. This finding aligns with Ashraf et al. (2013) who report similarly limited effects in Zambia following an intervention that provided vouchers for free and immediate access to long-acting contraceptive methods. On the other hand, Miller (2010) shows that the expansion of family planning services in Colombia significantly reduced fertility and improved women's educational attainment. Similarly, Pörtner et al. (2011) find that access to family planning in Ethiopia significantly reduced fertility in women without education, but had no effect on women with some schooling, suggesting that family planning and formal education may act as substitutes in this setting.

Second, this paper advances the literature on fertility policy by focusing on adolescents, an empirically understudied population in causal work on reproductive health in LMICs, who face distinct institutional and social constraints, including concerns around confidentiality and third-party involvement, provider discretion, and stigma around sexuality[3] (Colman et al., 2013; Guldi, 2008; Myers and Ladd, 2020; Sabia and Anderson, 2016). For instance, in the United

[1]Notably, prior research on these dynamics has largely centered on the birth control pill, given its importance as the most transformative breakthrough in contraceptive technology since the 1960s (Doepke et al., 2023).

[2]Extensive complementary research examining socioeconomic outcomes finds that access to contraception and family planning programs significantly increases years of education and the likelihood of high school graduation, while also contributing to a reduction in the gender gap and the likelihood of intergenerational poverty risk (Ananat and Hungerman, 2012; Bailey et al., 2012, 2019; Lindo et al., 2020; Myers, 2017; Stevenson et al., 2021)

[3]From a policy perspective, these constraints matter not only for immediate access to services, but also due to the strong association between early childbearing and lower educational attainment and socioeconomic mobility (Bailey, 2025; Heath and Jayachandran, 2016; Herrera Almanza and Sahn, 2018; Todd and Wolpin, 2006).

States, [4] several studies demonstrate that expanding access to contraception and abortion services can significantly reduce teenage fertility and improve educational trajectories (Fischer et al., 2018; Kelly et al., 2020; Lindo and Packham, 2017; Stevenson et al., 2021). Yet, most of this evidence comes from high-income settings, where legal and health system contexts differ markedly from those in middle-income countries. Although limited, prior causal studies in LMICs find that integrated family planning programs, which combine contraceptive access (Ferre et al., 2023; Galárraga and Harris, 2021; Kaytan et al., 2025), along with counseling and information campaigns (Branson and Byker, 2018; Kaytan et al., 2025; Strupat, 2017), can have a significant impact on addressing teenage pregnancies and early childbearing. This paper advances this literature by studying the effects of a nationwide policy reform that specifically targeted adolescent access and discretion, in a setting where adolescents' access to contraceptive services had previously been more contingent on discretionary gatekeeping and concerns about confidentiality.

Third, this study speaks to broader debates on how the source and framing of reproductive health information influence behavior. While supply-side policies may increase availability, their effectiveness will often depend on service delivery and perceived accessibility, and the extent to which demand is constrained by knowledge gaps, perceived barriers, or financial costs (Bailey et al., 2023; Branson and Byker, 2018; Grossman et al., 2025; Kearney and Levine, 2015). Strong evidence from a range of settings particularly suggests that autonomy, discretion, and provider interaction can shape both take-up and outcomes (Ashraf et al., 2014, 2013; Grossman et al., 2025; Jones and Pineda-Torres, 2024; Myers, 2022). By documenting shifts not only in fertility but also in information sources, knowledge, and downstream outcomes like marriage and schooling, this paper highlights the importance of service design and user autonomy as key mechanisms in shaping adolescent reproductive behavior. Specifically, this paper contributes to the existing body of work by providing new causal evidence from a middle-income country context, showing that alleviating supply-side barriers may not be sufficient in the presence of societal and institutional frictions.

The remainder of the paper is structured as follows: Section 2 provides background on teenage fertility in Peru and the 2017 Family Planning Technical Guideline. Section 3 describes the administrative and survey data used in the analysis. Section 4 outlines the empirical strategy and identification approach. Section 5 presents the main results on adolescent fertility, including heterogeneity analyses, complementary outcomes, and COVID-19 dynamics. Section 6 reports robustness checks. Section 7 examines underlying mechanisms using individual-level data, focusing on changes in contraceptive use, sources of reproductive health information, and knowledge of contraceptive methods. Section 8 concludes.

[4] By the 1970s, the United States had a notably higher teen pregnancy rate compared to other developed nation (Kearney and Levine, 2012).

2 Background

2.1 Adolescent Fertility in Peru

Despite decades of shifting political and ideological approaches to family planning in Peru, adolescent fertility has remained a persistent policy challenge.[5] Although Peru's total fertility rate declined from 6.7 in 1970 to just over 2.2 in recent years (World Bank, 2025b), nearly 13% of girls aged 15 to 19 have begun childbearing, with substantial disparities across regions, income levels, and education (Instituto Nacional de Estadística e Informática (INEI), 2022). In rural areas, adolescent motherhood reaches 22%, compared to 10% in urban zones, and exceeds 20% in Amazonian regions such as Loreto and Ucayali. These figures contrast with Lima, where the rate falls below 8%. Peru's adolescent fertility rate is more than twice the OECD average and remains significantly higher than other upper-middle-income countries (OECD, 2023). This persistence suggests that national progress in contraceptive coverage has not fully addressed the distinct institutional and social barriers faced by adolescents.

The institutional constraints that shape access to contraception in Peru, and the 2017 reform studied in this chapter, are defined around the legal threshold of adulthood. The FPTG explicitly introduced adolescent-friendly protocols and removed age-based restrictions for individuals under the legal age, 18. Accordingly, the empirical analysis focuses on fertility among minors, where confidentiality, provider discretion, and stigma are most likely to bind, and where the reform was intended to operate. In the administrative CNV data, this corresponds to restricting the main fertility outcomes to births to mothers aged 13–17.

Legal and economic constraints further reinforce these gaps. While comprehensive sex education was introduced in 2012, and emergency contraception was legalized for public distribution in 2019, abortion remains illegal in nearly all circumstances.[6] This restrictive environment is compounded by inconsistencies in legal norms governing adolescent sexual and

[5] Initial efforts in the 1960s–early 1980s were weak and largely symbolic, but the first national population policy in 1985 broadened access to publicly funded contraceptives, heavily supported by international cooperation. Under President Fujimori (1996–2000), the program expanded aggressively, prioritising sterilisation and reaching nearly 200,000 procedures, but allegations of coercion and reported deaths triggered a major scandal that discredited public family planning and fuelled conservative opposition (Defensoría del Pueblo, 2005; Valdivia, 2005). After 2000, family planning was downgraded and integrated into maternal–child health, with growing emphasis on "natural" methods, often promoted by religious actors, reducing access particularly for rural and less-educated women. Even after regime changes, family planning budgets and political support remained limited, while Peru's upper-middle-income status reduced external aid. By 2013, nearly 40% of contraceptive users relied on private providers (Aramburú, 2014), reflecting persistent gaps in public provision. These historical legacies help explain why, despite increased contraceptive coverage, adolescents continued to face institutional gatekeeping, provider discretion, and stigma in accessing modern methods.

[6] According to the Center for Reproductive Rights, in Peru, therapeutic abortion is permitted under Article 119 of the Penal Code; it is not punishable when performed by a physician to save the life of the pregnant woman or to prevent serious harm to her physical or mental health. Nonetheless, access remains highly restricted in practice due to regulatory barriers, the lack of standardized protocols, and providers' use of conscientious objection. In 2025, the Ministry of Health revised the Technical Guidelines for therapeutic abortion, explicitly removing considerations related to mental health risk, with experts have describing it as a setback, particularly in cases of adolescent pregnancy resulting from sexual violence.

reproductive rights. The age of sexual consent in Peru is 14, and until a legislative reform in 2023, adolescents could legally marry below the age of 18 with judicial authorization.[7] Although the law thus granted adolescents a degree of sexual autonomy, institutional barriers often limited their effective access to reproductive services.

Out-of-pocket contraceptive costs further limit access. When not publicly subsidized, a monthly pack of oral contraceptives can cost around USD 10 (PROMSEX, 2021), exceeding the daily minimum wage. In contrast, IUDs may cost between USD 160 and 320, making them largely inaccessible for low-income and adolescent users. These affordability issues highlight the urgent need for reforms that expand both access and autonomy in reproductive healthcare.

2.2 Family Planning Technical Guideline (FPTG)

In 2016, the Peruvian Ministry of Health approved the FPTG, officially enacted through Ministerial Resolution No. 652-2016/MINSA and updated in 2017.[8] The guideline, released and implemented in 2017, established standardized protocols for the delivery of contraceptive services across all public health centres.[9] The FPTG aimed to improve the quality and safety of family planning services by unifying procedures across different levels of care. It emphasized a rights-based approach to reproductive health, with a focus on gender equity, cultural inclusion, and non-discrimination.[10]

A key innovation of the 2017 reform was to formalise a rights-based approach to adolescent access within public family planning services by explicitly codifying safeguards for confidentiality, privacy, and voluntary decision-making. In doing so, it reduced a long-standing source of institutional ambiguity in Peru's legal environment: although the age of sexual consent is 14, adolescents' ability to seek reproductive care in practice has often been constrained by uncertainty and discretionary gatekeeping. The guideline therefore emphasises protocols that protect adolescents' autonomy during consultations, limit disclosure of health information to third parties, and prohibit pressure or coercion in contraceptive choices.[11]

Implementation was selective. The guideline defines adolescent "differentiated services"

[7] The Peruvian Civil Code previously permitted marriage from age 16 for boys and 14 for girls with judicial consent. Law No. 31839, enacted in 2023, eliminated these exceptions and established 18 as the minimum legal age for marriage.

[8] While compulsory for public health institutions, the FPTG served as a reference for the private sector.

[9] The FPTG functions as a legally binding technical guideline, comparable to implementation directives in the U.S. Department of Health and Human Services or the UK's NHS. It regulates how national reproductive health policies are operationalized.

[10] Specifically, the law stated: (i) that any consultation or mention of your case is carried out discreetly and that there are no persons present who are not directly involved in your care without your consent; (ii) no information should be given to third parties or family members about the reasons for care, diagnoses or treatment, unless authorized by the user, except as provided by law; (iii) to make decisions related to his/her health care; (iv) to be free from any pressure or coercion to choose a contraceptive method. More detail on the materials and service implementation can be found at Figure C4, C5, C6.

[11] For instance, the guideline requires privacy during counselling and examinations, establishes confidentiality of communications and clinical records, restricts disclosure of information without the user's authorisation, and explicitly prohibits coercion or inducements in method choice.

as continuous, quality comprehensive sexual and reproductive health and family planning care, delivered in *differentiated hours* and *exclusive environments* for adolescents, and aligned with national standards for adolescent services. It also notes that adolescent attention may be provided through youth development centres (CDJ), combining counselling, training workshops, socio-cultural activities, and clinical care. In practice, this service model requires minimum infrastructure and staffing capacity, so only a subset of public health centres were authorised to deliver adolescent-friendly family planning services (HCA).

While the reform did not introduce a national outreach strategy exclusively targeted to adolescents, it set out general dissemination guidelines for sexual and reproductive health and family planning. Communication efforts were expected to combine mass media (press, radio, television) with alternative media and local campaigns, including posters and printed materials, public murals and advertisements in public transport, and community-based channels such as meetings with local authorities and loudspeaker announcements (perifoneo). When adolescents were the intended audience, coordination with the Ministry's Adolescent Life Stage Unit (Etapa de Vida Adolescente) was explicitly encouraged.[12]

Throughout the paper, I define the start of the reform period as January 2017, when the Family Planning Technical Norm became operational and adolescent-focused services began to be offered in public health centres. All conceptions (and survey observations) from 2017 onwards are therefore treated as post-reform.

3 Data

3.1 Data sources

To examine the effects of FPTG on outcomes for women, I use administrative birth records covering the universe of live births (CNV), population estimates from the Peruvian Ministry of Health, and microdata sourced from the Peruvian Demographic and Health Survey (DHS). The main pre-pandemic analysis uses data from 2013–2019 (CNV and DHS). For the event-study analysis of COVID-19 disruptions, I extend the CNV sample to 2013–2023.

3.1.1 Birth records data

I use an administrative dataset from the Peruvian Ministry of Health (MINSA), based on the *Certificado de Nacido Vivo (CNV)*.[13] The system was launched in March 2012 and operates nationwide, incorporating data from all healthcare providers, both public and private.

[12] MINSA (2017), diffusion and communication guidelines; implementation is coordinated across national and regional health authorities, and adolescent-oriented campaigns are coordinated with the Etapa de Vida Adolescente.

[13] Data can be accessed upon request from the Ministry of Health. Detailed information available at: https://www.minsa.gob.pe/cnv/

It records every live birth at the moment of delivery. It collects detailed information on the mother (such as age and educational level), the newborn (including sex and birthweight), and the pregnancy (such as gestational age).

This information allows me to construct the universe of registered births in Peru, disaggregated by year, mother's age, and district of residence, covering the period 2013 to 2019.[14] I restrict the sample to births to mothers aged 13 to 17, in line with the specific target population of the FPTG. Using census-based population estimates for these age groups, I construct a district-by-age-group-by-year panel of age-specific fertility rates, which serves as the main outcome variable in the first part of the analysis.

Because the FPTG reform targets access and service protocols for minors, I align the main administrative outcome with this policy-relevant cutoff and construct fertility measures for girls below age 18.

3.1.2 Individual data

I also use data from the Peruvian Demographic and Health Surveys (DHS, or ENDES in its local designation), a nationally representative survey conducted by the National Institute of Statistics and Informatics (INEI). The DHS has been fielded continuously since 2004, with quarterly data collection that supports the construction of annual cross-sectional estimates. For this study, I use waves from 2013–2019. The DHS adopts a repeated cross-sectional design to ensure national and regional representativeness. Using stratified, multi-stage cluster sampling, the survey selects clusters of households within strata defined by geography and urban/rural status, and then randomly samples households within each selected cluster.

The survey collects detailed demographic and health information for women aged 15 to 49, including fertility histories, marital and employment status, household decision-making, and socio-economic characteristics. In this study, I focus on adolescent childbearing and related life transitions. Because the DHS interviews women aged 15–49, adolescents aged 13–14 are not consistently observed in the repeated cross-sections. I therefore restrict the DHS analysis sample to girls aged 15–17 at the time of interview, so that all respondents are minors (i.e., under 18) in the pre-reform and/or implementation period. To maintain a policy-relevant estimand tied to the reform's legal cutoff and to ensure comparability with the administrative measures, I define DHS outcomes using pre-18 thresholds (e.g., having given birth before age 18, and entering marriage/cohabitation before age 18).

While the CNV covers the universe of registered births, the DHS provides rich information on behaviours, knowledge, and schooling for a representative sample of adolescents. Although the two sources cannot be linked at the individual level, they offer complementary evidence

[14] The CNV records all centres-based births; although home or unregistered births may be more common in remote areas, there is no evidence that their incidence changed differentially across districts with and without an HCA during the study period, so any resulting undercoverage is likely time-invariant and absorbed by district fixed effects.

on the reform's fertility and behavioural impacts.

The inclusion of GPS coordinates for DHS household clusters enables me to compute each cluster's proximity to the nearest health centre.[15] However, because the survey does not report the name of each cluster (*centro poblado*), district is the smallest administrative unit at which I can condition the analysis (e.g., through district fixed effects).

3.1.3 Health centres location data

I complement this information with the GPS locations of all public health centres providing differentiated services to adolescents (*HCA centres*). This information is publicly available upon request from the Ministry of Health in Peru (*MINSA*). As illustrated in Figure C1, HCA centres are present in the 25 regions of Peru but not in all districts, only in 72.63% of the universe of Peruvian provinces.[16]

3.2 Measuring exposure to HCA

To capture exposure to HCA centres, I employ two complementary measures, tailored to the structure of each dataset. First, in the administrative birth records, geographic precision is limited to the district level. I therefore define exposure based on whether at least one health centre in the district was accredited as an HCA under the reform period. The HCA list was determined prior to the 2017 reform and remained unchanged over my study period, so this indicator is time-invariant at the district level. Treatment variation then comes from interacting this fixed district characteristic with the common post-reform period.

Second, using the DHS, which provides georeferenced coordinates for survey clusters, I construct a more granular measure of exposure based on proximity to HCA-accredited centres. Specifically, I calculate the Euclidean distance between each DHS household cluster and the nearest HCA centre from the pre-reform list. A household cluster is considered exposed if an HCA centre falls within a given distance radius of its location, irrespective of administrative boundaries. Because HCA status is fixed over time, this exposure measure is time-invariant at the cluster level and is only activated in the post-2017 period of the analysis.[17] The main results section examines how varying the buffer size affects the estimates and demonstrates that the findings are robust across alternative thresholds.[18] This individual-level exposure definition is applied consistently in both difference-in-differences and event study models, allowing for greater precision while maintaining alignment with the district-level analysis.

[15] In the Peruvian DHS, clusters contain on average 25 households, ranging from 1 to 50.

[16] Region corresponds to the first administrative level of the country, province to the second, and district to the third administrative level.

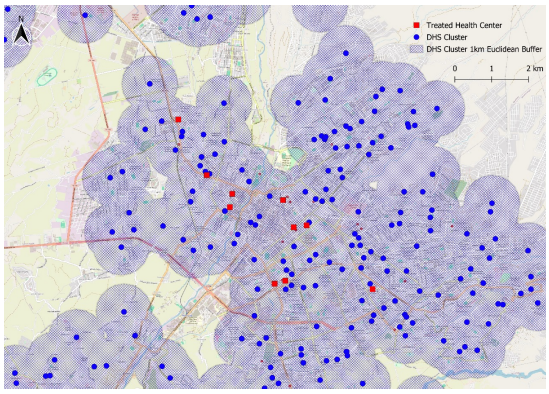
[17] Importantly, no additional centres were accredited as HCA during 2013–2019, so the reform did not generate staggered adoption across districts.

[18] See Section 5 for robustness checks using different distance buffers (e.g., 2–5 km). Results remain stable across specifications.

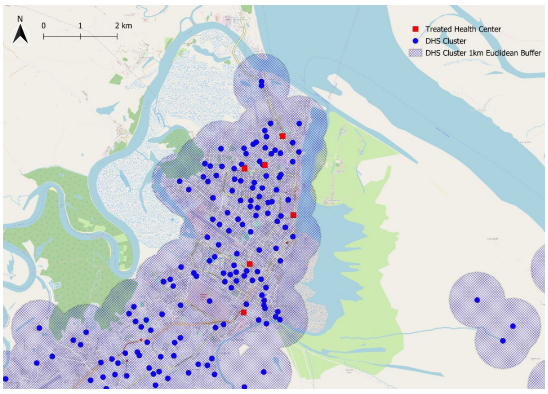
Figure 1 illustrates this spatial approach by mapping the Euclidean buffers for four selected regions in Peru: Arequipa, Iquitos, Lima, and Trujillo.

FIGURE 1 – EUCLIDEAN DISTANCE BUFFERS AND HCA (DHS CLUSTERS OF HOUSEHOLDS)

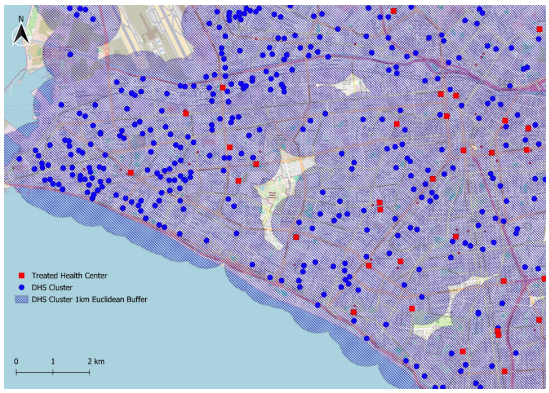
(a) AREQUIPA



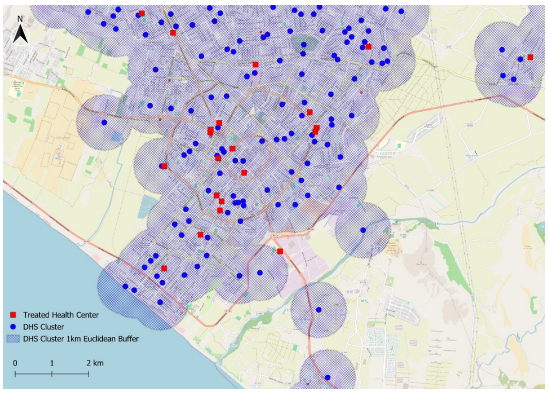
(b) IQUITOS



(c) LIMA



(d) TRUJILLO



Notes: This graph illustrates the location coordinates of the household DHS clusters, using a 1km buffer for illustrative purposes, and the HCA centres in 4 different locations of Peru.

One potential concern related to the DHS GPS locations is the precision of this information, since all GPS points provided in the DHS dataset are displaced before public disclosure of the datasets to preserve individuals' confidentiality. The GPS displacement is done randomly, with differences between urban and rural clusters. Urban clusters are uniformly displaced up to 2 kilometres, and rural clusters are displaced up to 5 kilometres. Additionally, all the displacement points are restricted, so they stay within the province level (second administrative unit). To address any concerns related to the displacement of GPS locations, I established a reasonable buffer distance to the HC, rather than the closest distance to the HC centre.[19] In that sense, I provide evidence that the main results are not sensitive to different groups of Euclidean buffer distances. Additionally, since the displacement is less pronounced in urban

[19]Because DHS cluster coordinates are randomly displaced within provinces and treatment is defined using relatively broad distance buffers (2–5 km), any misclassification arising from GPS displacement is likely to attenuate estimated effects toward zero rather than generate spurious impacts.

locations than in rural areas, I present my results using both the full sample and the urban cluster sample.

3.3 Descriptive statistics

I restrict the sample to women under 18 years old; this cut-off corresponds to how the FPTG reform targets adolescents under age of 18 in Peru. In the case of the birth records dataset, the analysis sample comprises a balanced panel of 1872 districts (out of 1890 districts in the country) spanning the 2013-2019 conception period. Figure C2 plots the distribution of teenage fertility rates across districts with and without an HCA.[20] It is essential to mention that the treated districts are considerably smaller than the comparison districts. Only 27% of provinces have an HCA.

In the case of the Peruvian DHS, I restrict the individual dataset to women aged 15, 16 or 17 during the reform period or survey year and who are not sterilised.[21] Table A1 provides information about the sample distribution for the DHS 2013-2019 period. Table A2 presents descriptive statistics for the pre-reform period (2013–2016) at the individual and household level for the full sample and by treatment status, where treatment is defined using the baseline measure of living within a 4-kilometre Euclidean buffer of an HCA centre (results are robust to alternative buffer sizes).

34% of the sample falls within a 4km distance to their closest health centre. Moreover, it is observed that there are some slight differences between treatment and control at the household characteristics measuring proxy for wealth and assets accumulation. Regarding the characteristics of women, there are not many differences between the two groups, except for the variable of urban residence. Due to the location of the existing HCA, the treatment group primarily resides in urban areas.

4 Methodology

To estimate the effect of increasing access to adolescent-friendly contraceptive (HCA) services, I use a difference-in-differences framework estimated with a two-way fixed effects (TWFE) specification (Roth et al., 2023). The empirical strategy exploits the sharp timing of the 2017 reform, the age cut-off that defines adolescents, and the fact that HCA services were introduced in a fixed set of pre-existing public health centres (Figure C1). I first present the specification using district-level data and then the specification using individual-level data.

[20] Calculated as the number of births divided by the estimated population per 1000 people in a given district, year and age group (15, 16 and 17).

[21] The Peruvian DHS only includes women under the age of 15 if they are already mothers in the households selected for interviews. However, women under 15 are not included in the sample design.

4.1 Identification and event-study design

The centres that implemented the adolescent-friendly family planning protocol were not randomly selected. Prior to the reform, HCA centres tended to be larger centres with more infrastructure and personnel than other public health centres, reflecting the requirement to allocate dedicated space and staff for adolescent services. These level differences motivate the inclusion of rich fixed effects and controls in the empirical analysis.

The identifying assumption is that, conditional on these time-invariant differences and common shocks, adolescent outcomes in areas served by HCA centres and other centres would have followed parallel trends in the absence of the reform. Consistent with this assumption, the event-study estimates presented below show no systematic differences in pre-2017 trends between areas served by adolescent-friendly centres and areas served by other centres.

4.2 District-level specification

I estimate a two-way fixed effects (TWFE) model that compares the fertility rate of 15–17-year-olds before and after the introduction of the family planning reform across districts with and without an HCA facility. The list of HCA centres is fixed over my study period: all treated centres were already constructed and accredited before the 2017 reform, and no new centres gained HCA status during 2013–2019. Thus, the design combines a single, common policy date (2017) with pre-existing cross-sectional variation in the presence of an HCA across districts. The key identifying assumption is that, in the absence of the reform, the change in the average teenage birth rate observed in comparison districts would have provided a good counterfactual for the change in treated districts.

In this approach, I construct a panel at the district level and define the outcome of interest and the relevant age group accordingly. The equation can be formalised as:

$$Y_{dty} = \beta_0 + \beta_1(Treated_d \times Post_t) + \theta_{r(d)t} + \delta_d + \epsilon_{dty}, \tag{1}$$

where Y_{dty} is the fertility rate for age group y in district d and conception year t.[22] $Post_t$ indicates whether the conception year is after the reform became operational, that is, for conceptions occurring in 2017, 2018, and 2019. $Treated_d$ indicates whether there is at least one HCA facility in district d. The coefficient β_1 captures the change in adolescent fertility in treated districts after the reform, relative to the change in comparison districts, under the assumption of parallel trends. The terms $\theta_{r(d)t}$ and δ_d are region-by-year fixed effects and

[22] The main outcome is an age-specific fertility rate at the district-by-age-group-by-year level. I first use the information in the birth records to assign each live birth to an approximate month and year of conception, and then aggregate all conceptions that occur between January and December of calendar year t for each district–age cell. The fertility rate for district d, age group y and conception year t is defined as the number of births conceived in year t to women aged y residing in district d, divided by the corresponding female population in that district–age group and multiplied by 1,000.

district fixed effects, respectively.[23] Finally, ϵ_{dty} is an error term. In this specification, I cluster the standard errors at the district level, i.e., the level of treatment (Abadie et al., 2023). [24]

To examine how the impact of the 2017 family planning reform evolved in the years before and after its implementation, I estimate an event-study specification of the following form (Miller, 2023):

$$Y_{dt} = \sum_{j \in \{-4,\ldots,-1,1,\ldots,4\}} \beta_j \left(\text{Treated}_d \times \mathbf{1}\{t - 2017 = j\}\right) + \alpha_d + \delta_t + \varepsilon_{dt}, \tag{2}$$

Here, Y_{dt} denotes the outcome of interest (e.g., the adolescent fertility rate for 15–17-year-olds) in district d and year t. The indicator Treated_d equals one for districts that contain at least one HCA facility in the pre-reform accreditation list, and zero otherwise. The term $\mathbf{1}\{t-2017 = j\}$ is an indicator for calendar years relative to the first post-reform year (2017), so that $j < 0$ indexes leads and $j > 0$ indexes lags. The coefficients β_j measure how the difference in outcomes between treated and comparison districts evolves j years before and after the reform, relative to the year just before implementation ($j = -1$), which is omitted from the summation. As before, α_d and δ_t denote district and year fixed effects, respectively, and ε_{dt} is an error term.

Estimates for $j < 0$ capture pre-reform trends and allow for tests of the parallel trends assumption, while those for $j > 0$ quantify the policy's effects in the years following implementation. As shown in later sections, there is no statistical evidence that pre-treatment trends differ between treated and comparison districts, supporting the parallel trends assumption.

4.3 Individual-level specification

To complement the district-level estimates in section 4.2 based on population birth records, I additionally exploit individual-level DHS microdata to estimate the reform's effects on adolescent childbearing and related behaviours. The DHS adds value for two reasons. First, it allows me to construct a more precise measure of exposure based on residential proximity to upgraded HCA centres (using DHS cluster coordinates), rather than relying solely on an indicator for whether an HCA centre operates within the district. Second, the DHS contains individual outcomes and mediating behaviours (e.g., sexual activity, contraceptive knowledge and use) that are not observable in administrative birth records, providing complementary evidence on mechanisms.

Using repeated cross-sectional DHS surveys from 2013 to 2019, I implement a standard difference-in-differences (DiD) design to estimate the effects of the reform on adolescent (under-18) fertility outcomes and related behaviours, including sexual activity, contraceptive

[23] In Peru, regions (or departments) correspond to the first administrative level below the national government. Districts are grouped into provinces, and provinces are grouped into regions/departments. There are 25 regions in total.

[24] Standard errors are clustered at the district level in all district-level regressions.

practices, and reproductive health knowledge. This approach relies on the accessibility and distribution of services within regions, which can vary depending on proximity to households, particularly in smaller districts. In order to estimate the impact of FPTG reform on adolescents' fertility decisions, the following specification is employed:

$$(Treated_{id}^{(R)} \times Post_t) + \beta_2 X_{it} + \gamma_d + \delta_t + \epsilon_{idt}, \tag{3}$$

where Y_{idt} represents the outcome of interest for woman i, residing in district d in year t. treatment is defined based on residential proximity to HCA services. Let $Treated_{id}^{(R)}$ be an indicator equal to one if woman i's DHS cluster lies within R kilometres of the nearest HCA facility, and zero otherwise.[25] $Post_t$ takes the value 1 if the survey year is 2017, 2018, or 2019. The vector X_{it} includes individual covariates: age (and age squared), an indicator for mother tongue different from Spanish, and the DHS wealth index. γ_d and δ_t are district-of-residence fixed effects and survey-year fixed effects, respectively. Finally, ϵ_{idt} is the error term. I cluster the standard errors at the district level.

Because the DHS is a repeated cross-section, the set of observed ages varies by survey year and the number of very young adolescents is limited. I therefore align the DHS analysis with the population of interest in this chapter—minors—by restricting the estimation sample to girls who are at most 17 years old at interview.[26] To maintain comparability across survey years and avoid differential exposure windows by interview age, I focus on outcomes defined strictly before age 18 (e.g., having had a birth before age 18), so that the outcome period remains within the policy-relevant age range for all respondents.

5 Main Results

This section presents the primary findings on the effects of the 2017 FPTG reform on adolescent fertility in Peru. I begin by assessing pre-trends and reporting estimates from district-level specifications using administrative birth records. I then turn to individual-level results from the DHS survey and explore heterogeneity by subgroups and age cohorts. I complement the main results with additional outcomes related to adolescent cohabitation and schooling. Finally, I conclude with the results, including the COVID-19 disruption.

5.1 Effects on teenage fertility rates

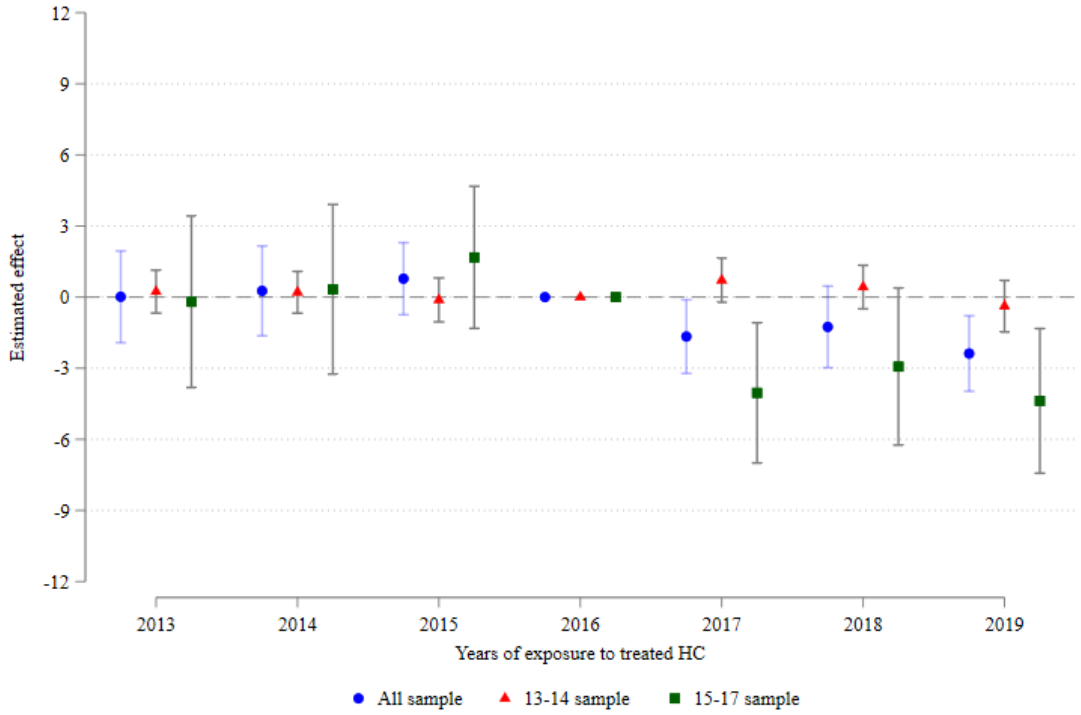
To verify the plausibility of the identifying assumptions, I start by examining whether adolescent fertility trends differed between treated and untreated areas before the reform.

[25] In robustness checks, I consider alternative radius $R \in \{1, 2, 3, 4, 5, ...\}$ to assess how the estimated effects vary with the size of the buffer area.

[26] The DHS's questionnaire sample design only includes women aged 15 and older; consequently, the DHS cannot capture consistently outcomes for ages 13–14, which are observed in the CNV population birth records.

Using administrative data, I estimate event study models that flexibly allow for dynamic effects before and after the policy rollout, as shown in Equation 2. Figure 2 shows no statistically significant differences in fertility trends between treatment and comparison groups prior to 2017, consistent with the parallel trends assumption. The event-study estimates show that treated and comparison districts follow parallel trends prior to 2017 and that adolescent fertility declines sharply in treated districts immediately after the reform, with effects persisting over subsequent post-reform years.

FIGURE 2 – EVENT STUDY FOR TEENAGE FERTILITY RATES – *CERTIFICADO NACIDO VIVO* (2013–2019)



Notes: This graph presents summary difference-in-differences coefficients of Equation 1, estimated by Ordinary Least Squares (OLS) using the national birth records dataset. All regressions include district and year-department fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 95% level are plotted.

I estimate a difference-in-differences specification using administrative birth records covering the universe of registered births in Peru between 2013 and 2019. Table A3 reports the results for adolescents aged 13–17 (Column 1), as well as for the 13–14 (Column 2) and 15–17 (Column 3) age cohorts separately. The reform is associated with a statistically significant reduction of about 2.1 births per 1,000 adolescents (around 13% relative to the control mean) in the full sample. When disaggregating the analysis by age group, the effect is concentrated among older adolescents (ages 15–17), for whom the reform led to a reduction of about 4.3 births per 1,000 (around 14–15% relative to the control mean).[27] These findings highlight

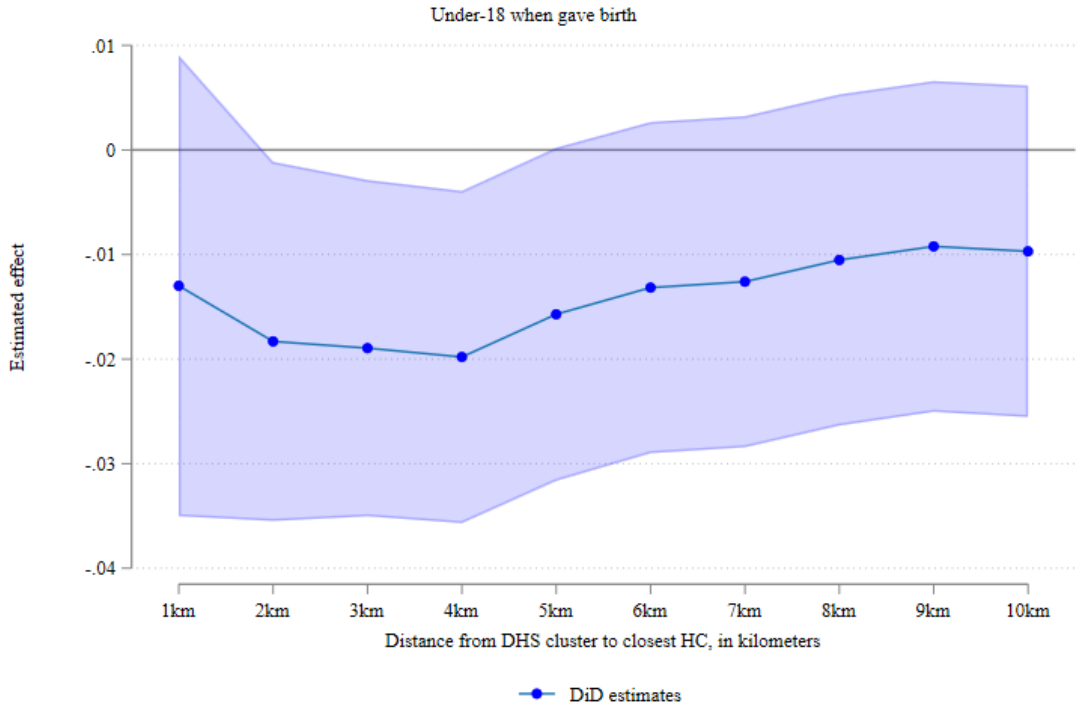
[27] This is consistent with very low baseline fertility among ages 13–14 in the control group (about 2.2 births per 1,000), which limits the scope for large absolute reductions and reduces precision in this subgroup.

the demographic significance of removing discretion-based barriers to contraceptive access for adolescents, particularly among those nearing the legal age of adulthood.

In addition to adolescent fertility rates, I also examine whether the reform influenced outcomes related to birth characteristics, utilising the administrative CNV dataset. Specifically, I examine average birth weight, birth height, and gestational age among newborns of adolescent mothers. Across all specifications, I find no statistically significant effects of the reform on these indicators (Table A4). These null results suggest that while the reform influenced fertility behavior, such as the timing and likelihood of adolescent births, it did not substantially alter maternal health or birth outcomes in the short term.

While the district-level approach provides broad coverage, it relies on administrative boundaries that may mask local variation in exposure. To address this, I implement an individual-level difference-in-differences model using DHS data, where treatment is defined based on Euclidean proximity for a given buffer to a health centres providing adolescent-friendly services. A key empirical question is how far these services can realistically influence adolescent reproductive behavior. To address this, I estimate a series of difference-in-differences regressions using concentric distance buffers around each treated facility, ranging from 1 km to 10 km in radius. Figure 3 plots the estimated treatment effects across distance bands, along with 95% confidence intervals.

FIGURE 3 – DIFFERENT DISTANCE BUFFERS FOR TEENAGE PREGNANCY – DHS (2013–2019)



Notes: Each point represents the coefficient from a separate difference-in-differences regression estimating the effect of exposure to an HCA following the 2017 FP reform, following Equation 2 and using the DHS dataset. All regressions include covariates: age, age squared, Indigenous native tongue (=1), and wealth index. All regressions include district and survey year fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 95% level are plotted.

The results indicate that the reform significantly reduced adolescent fertility among girls living in close proximity to treated centres. The most significant and robust effects are concentrated within 2 to 4 kilometres, where the estimated declines are both statistically significant and economically meaningful. Beyond 4 km, the effects attenuate and become statistically indistinguishable from zero. This pattern suggests that geographic proximity is an important determinant of practical exposure, likely reflecting reduced time and travel costs, increased likelihood of contact with youth-focused providers, and higher take-up of confidential services.

The results, reported in Table 1, indicate that the reform significantly reduced adolescent fertility among girls living near treated centres, corroborating the district-level findings. All specifications between 2 and 5 kilometres lead to negative and statistically significant coefficients, with the largest effects concentrated within the 3–4 km range. Specifically, the reform reduced the probability of having given birth before age 18 by around 1.5–2 percentage points (roughly 13–17 percent of the baseline). The magnitude and consistency of these results suggest that geographic proximity is a key determinant of program effectiveness, likely reflecting lower access costs, greater familiarity with services, and increased confidentiality for adolescents. Importantly, this pattern is consistent with the administrative birth records results, reinforcing the robustness of the evidence across data sources.

TABLE 1 – IMPACT OF FP REFORM ON TEENAGE PREGNANCY RATES - DHS (2013-2019)

Specification	Coefficient	Std. Error	Mean Dep. Var.	N
Panel A. Post × HCA within 2km				
All sample	-0.018**	(0.009)	0.1150	22,819
Only urban clusters	-0.016*	(0.009)	0.0921	14,976
Panel B. Post × HCA within 3km				
All sample	-0.019**	(0.008)	0.1170	22,819
Only urban clusters	-0.015*	(0.009)	0.0926	14,976
Panel C. Post × HCA within 4km				
All sample	-0.020**	(0.008)	0.1166	22,819
Only urban clusters	-0.019**	(0.009)	0.0889	14,976
Panel D. Post × HCA within 5km				
All sample	-0.016*	(0.008)	0.1176	22,819
Only urban clusters	-0.015	(0.009)	0.0886	14,976

Notes: This table presents summary difference-in-differences coefficients of Equation 2, estimated by Ordinary Least Square (OLS) using the DHS dataset. The outcome is a dichotomical variable taking the value of 1 if the individual gave birth at age 17 or younger. All regressions include age, age square, an indigenous native tongue (=1), and wealth index as well as district and survey year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

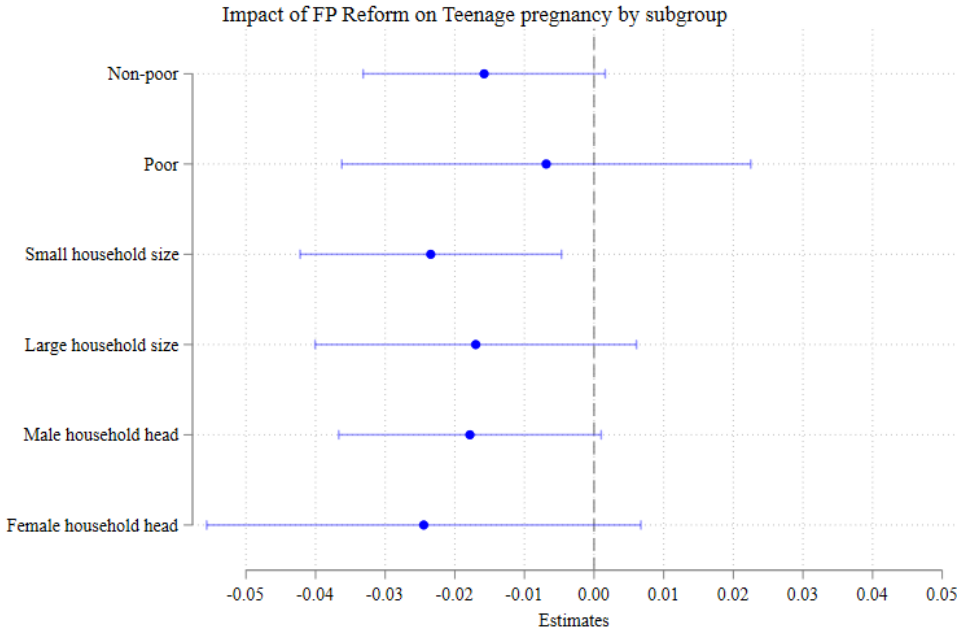
In light of this evidence, subsequent individual-level analyses restrict the sample to adolescents residing within 4 kilometres of a treated health facility, which captures the zone where the policy's impact is most pronounced. To assess the validity of the identifying assumptions

in this setting, I also estimate an event study specification using the DHS sample restricted to girls living within 4 kilometres of a treated facility. As shown in Figure C3, the pre-reform coefficients are small in magnitude and not statistically significant, providing no evidence of differential trends before the reform. The post-reform trajectory mirrors the pattern found in the administrative data, with a clear and sustained decline in teenage pregnancy following the roll-out of adolescent-friendly services. These results reinforce the consistency and robustness of the main findings across data sources and identification strategies.

5.2 Heterogeneity Analysis

To better understand who benefited most from the reform, I explore heterogeneous effects across key socioeconomic and demographic dimensions. Using the DHS sample, I estimate subgroup-specific difference-in-differences models by wealth status, household size, and the gender of the household head. The results are presented in Figure 4.

FIGURE 4 – IMPACT OF FPTG REFORM ON TEENAGE PREGNANCY RATES, HETEROGENEITY ANALYSIS – *DHS* (2013–2019)



Notes: This graph presents summary difference-in-differences coefficients of Equation 1, estimated by Ordinary Least Squares (OLS) using the birth records dataset, separately for each of the following subgroups: individuals in the upper half of the wealth index distribution, those in households below the median household size, and those living in male-headed households. All regressions include district and survey year-department fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 95% level are plotted.

Consistent with earlier findings, the reform's impact is concentrated among adolescents from relatively more advantaged backgrounds (see Table A5). Non-poor adolescents experience a statistically significant reduction in teenage pregnancy rates of approximately 1.6 percentage points compared to an insignificant effect among poorer peers.

Notably, additional heterogeneity is observed in household-level characteristics. Adolescents living in larger households and those in households headed by men exhibit significantly stronger fertility declines. The fertility rate among girls in large households drops by 2.3 percentage points, and the effect for those living in male headed households is approximately 1.8 percentage points; both estimates are statistically significant at conventional levels. These patterns likely reflect deeper structural advantages: adolescents in wealthier, male-headed, or larger households may face fewer mobility constraints, benefit from stronger linkages to the formal health system, or be better positioned to navigate newly expanded service options. These households may also be more responsive to policy reforms, especially when services require navigating institutional norms or procedures.

These results reveal the importance of intersectional barriers in shaping the impact of adolescent-focused policy reforms. Although the reform eliminated legal age-based restrictions and introduced adolescent-friendly services, its benefits were unevenly distributed. Targeted outreach or complementary support strategies may be necessary to ensure that adolescents from poorer, indigenous, or female-headed households are equally able to access and benefit from expanded reproductive health services.

5.3 Effects on Cohabitation, Marriage and Education

I investigate whether the reform has more wide-ranging effects beyond its effects on teenage pregnancy, the reform also appears to shift family formation and schooling trajectories. As shown in Table 2, adolescent girls living close to health centres that offered adolescent-friendly services were significantly less likely to report being married or cohabiting before age 18. The estimates imply declines of about 2.5–3 percentage points from pre-reform means of 11–13 percent, i.e. roughly a one-fifth reduction in early unions. At the same time, the reform reduced the total number of children ever born by around 0.03 among adolescents (column 3), corresponding to a sizeable decline relative to the baseline mean. These patterns suggest that access to confidential, adolescent-centered services not only lowers the probability of any teen birth but also dampens the intensity of early childbearing and delays union formation.

Schooling outcomes show a consistent picture. Table 3 documents that girls exposed to the reform are less likely to report leaving school because of childbearing or marriage: the probability of citing pregnancy/childbearing as the main reason for stopping school falls by about 1.7 percentage points (almost one-third of the pre-reform mean), with a smaller decline for marriage-related reasons. By contrast, there is little evidence that the reform affected the likelihood of stopping school for economic reasons, because the respondent had already completed her studies or academic disengagement, suggesting that the policy mainly altered fertility-related causes of dropout rather than broader schooling constraints. Taken together, these additional results indicate that the declines in early cohabitation, marriage, and fertility operate through a re-timing of key life-course transitions, allowing adolescents to remain

in school and accumulate human capital for longer. These patterns are consistent with a broader literature documenting the interplay between reproductive autonomy, early marriage, and human capital investment (Bailey, 2012; Goldin and Katz, 2002; Stevenson et al., 2021).

TABLE 2 – IMPACT OF FPTG REFORM ON COHABITATION AND MARRIAGE - DHS (2013-2019)

	(1) Cohabitation-18	(2) Marriage-18	(3) Total Children
Panel A. All sample			
4km HC × Post	-0.025***	-0.029***	-0.035***
	(0.008)	(0.008)	(0.009)
Observations	22,819	22,819	22,819
Mean Dep. Var.	0.11	0.13	0.14
Panel B. Only urban			
4km HC × Post	-0.016*	-0.024***	-0.023**
	(0.008)	(0.009)	(0.010)
Observations	14,976	14,976	14,976
Mean Dep. Var.	0.08	0.10	0.11

Notes:This table reports difference-in-differences estimates from Equation 3, estimated by ordinary least squares (OLS) using DHS data. The outcomes are: (i) an indicator equal to 1 if the individual is cohabiting (living with a partner) before age 18, (ii) an indicator equal to 1 if the individual married before age 18, and (iii) total children ever born (continuous). All regressions include age, age square, an indigenous native tongue (=1), and wealth index as well as district and survey year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

TABLE 3 – IMPACT OF FPTG REFORM ON PATHWAYS OUT OF SCHOOLING: FERTILITY, MARRIAGE, AND SCHOOL PROGRESSION - DHS (2013-2019)

	(1) Childbearing	(2) Marriage	(3) Economic	(4) Graduated or short-course	(5) Academic disengagement
Panel A. All sample					
4km HC × Post	-0.017**	-0.008**	0.012	-0.011	0.000
	(0.008)	(0.003)	(0.008)	(0.007)	(0.005)
Observations	22,819	22,819	22,819	22,819	22,819
Mean Dep. Var.	0.06	0.03	0.08	0.04	0.03
Panel B. Only urban					
4km HC × Post	-0.015*	-0.007*	0.011	-0.015	-0.002
	(0.009)	(0.004)	(0.008)	(0.009)	(0.005)
Observations	14,976	14,976	14,976	14,976	14,976
Mean Dep. Var.	0.06	0.01	0.07	0.06	0.02

Notes: This table presents summary difference-in-differences coefficients of Equation 3, estimated by Ordinary Least Square (OLS) using the DHS dataset. The outcomes are binary indicators for the main reason the respondent stopped attending school: (i) childbearing/pregnancy, (ii) marriage, (iii) economic constraints, (iv) already graduated or enrolled in a short course/academy, and (v) academic disengagement (did not want to continue studying). All regressions include age, age square, an indigenous native tongue (=1), and wealth index as well as district and survey year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

5.4 Post-Reform Dynamics and the COVID-19 Disruption

An essential aspect of evaluating the reform's effectiveness is understanding how its impacts evolved over time, particularly in response to external disruptions. Using CNV data extended to 2023, Figure 5 presents the dynamic treatment effects by year since exposure to a treated health facility, disaggregated by adolescent age group. The reform begins to show statistically significant reductions in fertility for the older cohort (15–17) in the years immediately following its roll-out. However, these effects appear to dissipate in 2020, coinciding with the onset of the COVID-19 pandemic.

This change in trend coincides with Peru's national lockdown and the temporary suspension of non-urgent health services, including family planning. Public health centres were repurposed to manage COVID-19 cases, and in-person services were restricted, plausibly limiting adolescents' access to contraceptive counselling and supplies. While this narrative is consistent with administrative reports documenting service disruptions, these patterns should be interpreted cautiously. The pandemic generated multiple simultaneous shocks, including economic, social, behavioural, and institutional changes, that are difficult to separate empirically in this context.[28]

Interestingly, fertility declines among the 15–17 cohort reappear and strengthen from 2021 onward, once services began to resume. This trend may reflect the reinstatement of adolescent-focused service delivery, but it may also capture other post-pandemic dynamics. In contrast, the 13–14 age group consistently shows null effects throughout the period, consistent with earlier findings that the reform primarily benefited older adolescents closer to reproductive maturity.

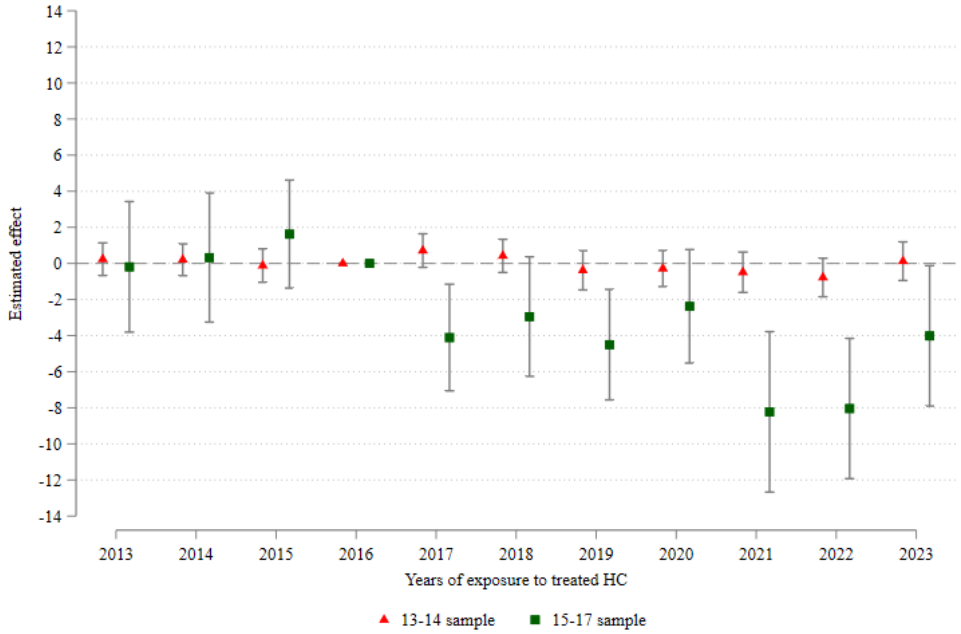
Overall, these patterns suggest that institutional design is critical, but sustained service delivery is also key to long-term behavioral change. At the same time, they underscore the vulnerability of adolescent reproductive services to systemic shocks, and how crises like COVID-19 can complicate efforts to measure and sustain policy impact in real time.

6 Robustness

I perform several robustness checks to verify the consistency and reliability of the main findings. First, a key identifying assumption is that, in the absence of the FPTG, the teenage fertility rates among districts with and without health centres would have followed parallel trends. Following standard practice in the literature (Roth et al., 2023), I statistically test for differences in trends before the implementation of the FPTG. Results are presented in Table A7, and I cannot reject the null hypothesis that the two groups were on parallel trends before 2017.

[28] As a result, the empirical design does not permit a causal attribution of the 2020 attenuation to pandemic-related service disruptions, given the presence of concurrent unobserved shocks.

FIGURE 5 – EVENT STUDY FOR FERTILITY RATES – *CERTIFICADO NACIDO VIVO* (2013–2023)



Notes: This graph presents summary difference-in-differences coefficients of Equation 1, estimated by Ordinary Least Squares (OLS) using the national birth records dataset for the period 2013–2023. All regressions include district and survey year-department fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 95% level are plotted.

To assess the validity of the identifying assumption underlying the difference-in-differences design, I examine whether the timing of the reform is correlated with pre-existing trends in adolescent fertility. Specifically, I test whether districts that gained exposure to adolescent-friendly services already exhibited differential fertility trajectories prior to the reform. Using the universe of administrative birth records, I estimate regressions of pre-reform changes in fertility rates on indicators for the year a treatment facility became available in a given district. The results presented in Table A8 show no evidence of differential trends in treated versus untreated areas before the reform was implemented. This suggests that the timing of the adolescent-focused family planning rollout was not systematically related to prior changes in fertility, supporting the plausibility of the parallel trends assumption. Overall, event study graphs and formal tests of pre trends (Table A7 and Table A8) show no evidence that treated and comparison districts differed in their adolescent fertility trajectories prior to the reform, supporting the parallel trends assumption. Consistent patterns also emerge in the DHS sample (see Table A9 and Table A10).

Second, the results are not driven by any particular region nor the metropolitan area of the capital city Lima (Table A11 and Table A12). The estimated coefficients remain stable when I omit one region at a time from the database. Third, to account for potential spatial correlation in treatment exposure, I also compute Conley standard errors (Conley, 1999), which allow for spatial autocorrelation based on geographic distance. Results remain robust and statistically significant across a range of distance cut-offs, indicating that spatial dependence does not

drive the main findings.

Additionally, I re-estimate the district-level DiD specifications using the CNV data aggregated at the quarter and semester level instead of annually. The estimated effect of exposure to an adolescent-friendly health facility remains negative and statistically significant in both panels (Table A13), with magnitudes similar to the baseline results. This confirms that the estimated reduction in teenage fertility is not driven by the particular choice of temporal aggregation and that the reform's impact is robust to using finer time variation in births data. Finally, I present individual-level results estimated using an alternative functional form, a probit regression model, in Table A14. The results remain consistent with the baseline findings across the different distance thresholds.

Overall, these robustness checks collectively reinforce the main conclusion: proximity to HCA facility after the FPTG reform is consistently associated with decreased teenage pregnancy rates.

6.1 Age-gradient analysis

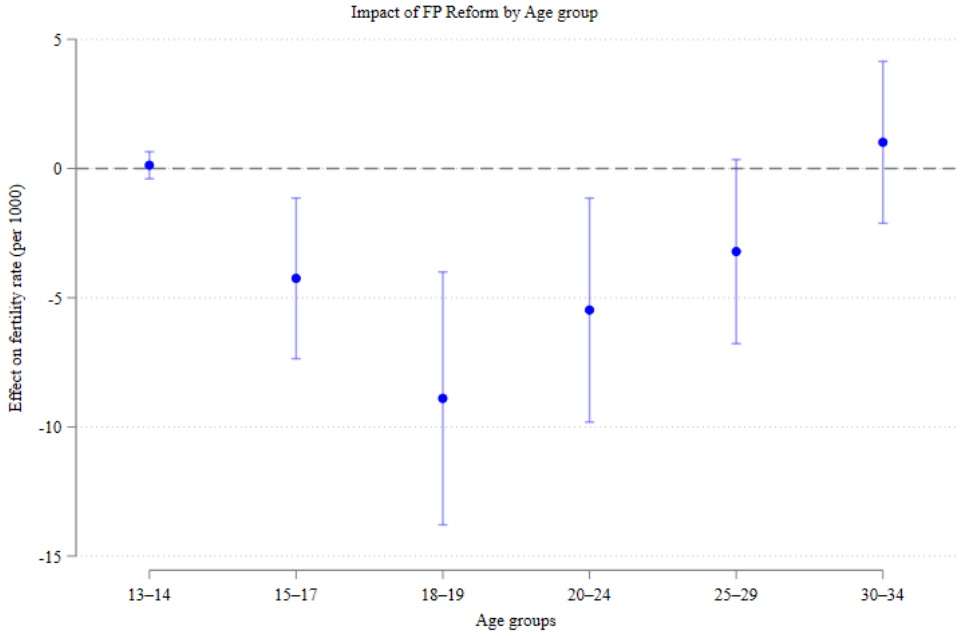
As an additional robustness exercise, I examine whether the FP reform affected women beyond the formally targeted group. I estimate separate difference-in-differences models for six age cohorts: 13–14, 15–17, 18–19, 20–24, 25–29, and 30–34. Each model follows the main district-level specification but is estimated independently for each age group, allowing me to trace how the reform's impact varies across the age distribution.

The results, presented in Figure 6, reveal no significant change in fertility among girls aged 13–14, consistent with their low baseline fertility and limited demand for reproductive health services at that age. Fertility declines significantly among 15–17-year-olds, which is the group formally targeted by the reform, and even larger reductions are observed among women aged 18–19 and 20–24. These patterns suggest that improved access to and quality of contraceptive services at HCA centres also benefited slightly older women who use the same centres and providers. Fertility also falls modestly among women aged 25–29, with no discernible effect for those aged 30–34. Together, these results indicate a clear age gradient in treatment effects, with the largest declines concentrated among adolescents and younger adults.

This pattern has direct implications for identification. The presence of substantial treatment effects among 18–24-year-olds undermines their validity as an untreated comparison group in a triple-difference (DDD) design, since they are also exposed to the upgraded centres and the broader improvements in service delivery triggered by the reform. As a result, DDD estimates that use older women as a control group are likely to be biased toward zero and understate the true effect on the intended teenage population. The age-gradient analysis therefore supports interpreting the reform as a broader improvement in local reproductive health services, and it justifies relying on age-specific DiD estimates rather than DDD as the

preferred identification strategy.[29]

FIGURE 6 – FERTILITY RESPONSES TO FPTG REFORM ACROSS AGE COHORTS - CNV (2013–2019)



Notes: Each point represents the coefficient from a separate difference-in-differences regression estimating the effect of exposure to an HCA following the 2017 FP reform. The outcome is the annual fertility rate (births per 1,000 women) for each age cohort. All regressions include district and survey year-department fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 90% level are plotted. Sample period: 2013–2019.

7 Potential mechanisms

To better understand the mechanisms behind the observed decline in adolescent fertility, I use the DHS microdata to examine changes across three domains: (i) contraceptive use and knowledge, (ii) family planning communication channels, and (iii) sexual behavior.

Panel A of Table A6 shows that adolescents living near centres offering adolescent-friendly services were significantly less likely to use traditional methods (e.g., withdrawal or rhythm) and more likely to report current condom use. The estimated 4–5 percentage-point increase in condom use represents about a 20–25% rise relative to the pre-reform mean, consistent with the magnitude of the fertility decline.

While there is no statistically significant evidence of an overall rise in modern contraceptive uptake requiring clinical involvement (such as hormonal methods or IUDs), the increase in

[29]One potential alternative would be to exploit the DHS, which includes a large cross-sectional sample of women aged 15–49. However, the DHS is a repeated cross-section, not a panel, making it difficult to track exposure consistently across cohorts over time. Since different individuals are observed in each wave and assignment to treatment relies on age at exposure, comparisons between adjacent cohorts (e.g., 15–17 vs. 18–19) risk conflating age, period, and sample-composition effects. In contrast, the national births dataset provides a complete panel of fertility outcomes by district and age group over time, allowing for cleaner implementation of cohort-based DiD and robustness analyses.

condom use suggests a shift toward less provider-dependent and more accessible methods. This behavioral shift is accompanied by a modest rise in contraceptive knowledge, especially regarding modern methods, indicating improved awareness and a greater ability to make informed choices.

These findings suggest that the reform enhanced access to methods that better align with adolescents' preferences for autonomy and discretion. A plausible interpretation is that the rise in condom use reflects greater comfort with over-the-counter options and improved privacy, facilitated by confidential service delivery. Consistent with this view, Panel B shows a reduction in reported discussions about family planning with partners, relatives, and peers. This decline should not necessarily be read as disengagement, but may instead indicate a shift away from informal and potentially judgemental sources of advice toward professional guidance offered in adolescent-friendly clinics. Although the DHS does not directly record whether health providers were the primary source of information, the simultaneous decline in reliance on family and peers, together with increased contraceptive knowledge and condom use, is suggestive of a shift toward more formal channels.

Panel C finds no significant changes in sexual behavior indicators, such as age at first intercourse or partner age gaps, which suggests that fertility reductions were not driven by delayed sexual debut but rather may have resulted from safer and more autonomous contraceptive practices. Taken together, these patterns imply that the reform's main contribution was less about altering sexual behavior per se and more about enabling adolescents to manage reproductive decisions with greater privacy, reliability, and control.[30]

Together, these findings suggest that the reform primarily operated by enhancing access to reliable contraception and improving the informational environment.[31] Adolescents responded by switching to more effective and accessible methods like condoms, reducing reliance on informal advice, and making more autonomous reproductive decisions.[32] While changes in reported sexual behaviour were limited, the increase in contraceptive knowledge and condom use points to a meaningful shift in how adolescents engage with family planning in settings where stigma and institutional barriers have historically limited their agency.

[30] I also explore changes in fertility preferences, as measured by the number of children adolescents report wanting in the future. I find no statistically significant change associated with the reform, suggesting that shifts in long-term fertility goals do not drive the observed fertility effects.

[31] The available data only allow me to observe women's fertility outcomes and contraceptive behaviour. I do not have comparable information on men's contraceptive use or service utilisation, either in the birth records or in the DHS. As a result, the mechanism analysis focuses on women's side of contraceptive decision-making.

[32] Because contraceptive use and sexual behaviour are self-reported and potentially subject to social desirability bias, these mechanism estimates should be interpreted cautiously; nonetheless, the patterns are internally consistent with the fertility results.

8 Conclusion

This study provides causal evidence that institutional design plays a critical role in shaping adolescent reproductive behaviour. The 2017 Peruvian family planning reform reform reduced institutional ambiguity and discretionary barriers around adolescent access, and introduced adolescent-focused protocols in a subset of public health centres. These changes led to sustained reductions in adolescent fertility, particularly among older girls and those living closer to centres offering the upgraded services. The findings demonstrate the importance of proximity and the quality of service delivery in determining programme effectiveness.

The reform also influenced other key life transitions. Adolescents exposed to the policy were less likely to enter early unions and less likely to leave or interrupt school because of pregnancy, childcare responsibilities, or marriage/cohabitation. These results point to broader improvements in reproductive autonomy and decision-making, suggesting that access to adolescent-centred services can delay family formation and reduce role conflicts between schooling and family duties. Such changes represent important investments in human capital during a critical period of development.

Analysis of potential mechanisms reveals that the reform shifted contraceptive behaviour toward more autonomous and accessible methods. Adolescents in treated areas reported lower use of traditional methods and a modest increase in condom use, consistent with a preference for discreet options that do not require clinical intervention. In parallel, adolescents became less likely to rely on family members or peers for reproductive health information. While the data do not directly record whether health providers were the primary source of information, the simultaneous decline in reliance on family and peers, together with increased contraceptive knowledge and condom use, is suggestive of a shift toward more formal channels for contraception and counselling.

Despite these gains, the benefits of the reform were unevenly distributed. Fertility reductions were significantly larger among adolescents from wealthier households, non-indigenous backgrounds, urban areas, male-headed families, and larger households. These disparities reveal the persistence of structural and cultural barriers that continue to shape access, even after the reform strengthened confidentiality protections and standardised adolescent-friendly service protocols, and highlight how ostensibly universal policies can disproportionately benefit more advantaged groups.

The qualitative evidence discussed next helps interpret why the reform's effects are larger in urban and more advantaged settings, and why effective access may not improve uniformly across space. In many regions, adolescent-friendly services remained scarce: for instance, fewer than ten percent of centres in Piura offered differentiated services in 2017, and even in Arequipa only a minority provided private, adolescent-appropriate spaces (PROMSEX, 2017). These services were concentrated in urban centres, leaving rural and peripheral areas underserved. Adolescents also faced stigma when seeking contraception, including concerns about

confidentiality, fear of judgement by providers, and community disapproval. Reports from peri-urban areas describe moralising behaviour and denial of services (PROMSEX, 2023), while household dynamics, particularly in conservative or indigenous families, further constrained adolescents' autonomy to make confidential reproductive health decisions.

Taken together, these findings suggest that while the 2017 reform expanded institutional opportunities and formal protections, cultural, social, and operational barriers limited its reach. The distinction between rights on paper and effective access is critical. Closing this gap requires adapting implementation to local conditions, fostering community engagement, training providers in non-discriminatory practices, and monitoring service quality.

This study contributes to the broader literature on reproductive health policy by showing that adolescent outcomes can improve in middle-income contexts when legal reforms are combined with trusted and confidential service delivery. In high-income countries, access to contraception has been linked to delayed fertility, higher educational attainment, and improved labour market outcomes (Bailey, 2012; Goldin and Katz, 2002). In Latin America, the findings complement Miller (2010) on Colombia, which documents fertility and education gains following family planning expansion. The Peruvian case adds new evidence by focusing on adolescents and highlighting mechanisms related to discretion, trust, and autonomy. By comparison, studies from Zambia and Burkina Faso emphasise that interventions which expand supply alone often yield more limited results because of demand-side frictions and institutional resistance (Ashraf et al., 2013; Dupas et al., 2024). The results therefore suggest that policies prioritising confidentiality and adolescent agency may be better positioned to overcome stigma and increase contraceptive uptake in settings where adolescent fertility remains high.

The temporary reversal of gains during the COVID-19 pandemic further highlights the vulnerability of adolescent reproductive services to external shocks. Event-study estimates show that the fertility reductions associated with the reform were effectively nullified in 2020, when non-urgent reproductive health services were suspended, but resumed once services were restored. This interruption illustrates the need for resilient systems that can preserve access during public health emergencies; continuity of care must be a core priority in future policy planning.

Overall, the findings underscore that expanding access to modern contraceptive methods is not sufficient on its own; the design of services and the conditions under which adolescents seek care are central to reducing early childbearing. In the Peruvian context, this suggests that scaling up adolescent-friendly models such as HCA to underserved and rural areas, ensuring sustained training and supervision of providers, and monitoring stigma and confidentiality breaches should be policy priorities. More broadly, the results speak to other middle-income settings where adolescent fertility remains high despite nominal contraceptive availability: reforms that combine method access with confidential, judgement-free counselling and clear, age-appropriate protocols may yield sizable gains in delaying first births and supporting girls' educational trajectories.

While the analysis is specific to Peru's health system and legal framework, the patterns documented here are likely relevant for other Latin American countries with persistently high adolescent fertility, such as Colombia, Ecuador, and Bolivia. Many of these settings share features such as relatively high contraceptive prevalence among adults, decentralised public health systems, and strong social norms around sexuality and motherhood. At the same time, cross-country differences in the scope of reproductive rights, the organisation of primary care, and the extent of opposition to comprehensive sexuality education may shape both the feasibility and the impact of adolescent-friendly service models. As such, the estimates in this paper should be interpreted as evidence of what can be achieved when discretionary constraints and confidentiality-related barriers are reduced, and adolescent-friendly delivery conditions are implemented, rather than as mechanical predictions for other contexts.

In summary, this study offers clear lessons for governments seeking to reduce adolescent fertility within restrictive reproductive environments. Legal reforms are necessary, but not sufficient. To be both effective and equitable, reforms must be accompanied by institutional investments that expand real access, build trust, and promote adolescent autonomy. Ensuring that services are confidential, culturally appropriate, and physically accessible is critical to empowering young people to make informed decisions about their futures.

9 Appendix

1. Tables

TABLE A1 – SAMPLE DISTRIBUTION AND HEALTH CENTER EXPOSURE – DHS (2013–2020)

Survey year	All	Urban	Rural
2013	2587	1598	989
2014	2759	1744	1015
2015	3272	2124	1148
2016	3017	1982	1035
2017	2944	1944	1000
2018	3854	2598	1256
2019	4668	3157	1511

Notes: This table summarizes the DHS sample distribution for the analytical sample of women aged 17 or younger during the survey year or at the implementation of the FPTG reform.

TABLE A2 – WOMEN'S DESCRIPTIVE STATISTICS, ONLY URBAN SAMPLE, FOR THE PRE-REFORM PERIOD (2013-2016)

	Total		Treated		Control	
	Mean	SD	Mean	SD	Mean	SD
Panel A. Treatment variable						
HCA within 4 km	0.34	(0.47)	-	-	-	-
Panel B. Household characteristics						
Has electricity (%)	0.91	(0.29)	0.99	(0.12)	0.87	(0.33)
Wealth (standardised) index	-0.10	(1.00)	0.46	(0.87)	-0.39	(0.93)
Age of head of household	45.88	(12.08)	46.68	(12.33)	45.48	(11.94)
Head of household is male (%)	0.75	(0.43)	0.71	(0.45)	0.78	(0.42)
Panel C. Women characteristics						
Age in years	15.98	(0.81)	16.03	(0.82)	15.95	(0.81)
Residence is in urban area (%)	0.64	(0.48)	0.97	(0.18)	0.47	(0.50)
Highest level of education is high school (%)	0.90	(0.30)	0.94	(0.25)	0.89	(0.32)
Under-18 when gave birth (%)	0.08	(0.27)	0.07	(0.25)	0.09	(0.28)
Total children ever born	0.09	(0.29)	0.07	(0.26)	0.09	(0.31)

Notes: This table presents descriptive statistics using the DHS dataset for the pre-FPTG period between treated and control districts. The sample corresponds to women aged 17 or younger. The pre-FPTG reform period includes the years 2013, 2014, 2015 and 2016. Treatment districts contain an HCA center within a 4km buffer distance from the DHS household cluster.

TABLE A3 – THE EFFECT OF FPTG REFORM ON TEENAGE FERTILITY RATES - *CERTIFICADO NACIDO VIVO* (2013-2019)

	All sample	13-14	15-17
HCA in district x Post	-2.066**	0.128	-4.250***
	(0.817)	(0.268)	(1.586)
Observations	26,173	13,076	13,097
Mean Dep. Var.	15.80	2.17	29.41
Year - Region FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes

Notes: This table presents summary difference-in-difference (DiD) coefficients of Equation 1, estimated by Ordinary Least Squares (OLS) using the universe of birth records dataset. The outcome is the number of births per 1000 women in each age group, weighted by the population in 2019 in the corresponding age group. Column 1 presents estimates for all women teens (13 to 17 years old). Column 2 shows the results for women aged 13 to 14, and Column 3 shows the results for women aged 15 to 17. All regressions include district and survey year-department fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 per cent levels are denoted by ***, **, and *, respectively.

TABLE A4 – IMPACT OF FPTG REFORM ON OTHER OUTCOMES - *CERTIFICADO NACIDO VIVO* (2013-2019)

	Weight at birth	Pregnancy duration	Height at birth
	(1)	(2)	(3)
HCA in district x Post	12.321	-0.045	6.046
	(14.608)	(0.054)	(8.540)
Observations	10,221	10,221	10,221
Mean Dep. Var.	3075.52	38.59	58.28
Year-Region FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes

Notes: This table presents summary difference-in-difference (DiD) coefficients, estimated by Ordinary Least Squares (OLS) using the universe of birth records dataset. The outcomes are at the individual level: weight at birth in grams, dichotomous for more than one child, pregnancy duration in weeks and height at birth in centimetres. Column 1 presents estimates for all women teens (13 to 17 years old). Column 2 shows the results for women aged 15 to 17 years old. All regression includes district, year-department fixed effects and sex of the child as controls. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

TABLE A5 – IMPACT OF FPTG REFORM ON TEENAGE PREGNANCY RATES, HETEROGENEITY ANALYSIS – DHS (2013–2019)

	Native	Non-native	Poor	Non-poor	Small hhsize	Large hhsize	Female head	Male head
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
4km HCA x Post	-0.010	-0.021**	-0.007	-0.016*	-0.017	-0.023**	-0.024	-0.018*
	(0.060)	(0.008)	(0.015)	(0.009)	(0.012)	(0.010)	(0.016)	(0.010)
Observations	3,387	19,220	12,837	9,813	10,102	12,439	5,542	16,952
Mean Dep. Var.	0.13	0.11	0.14	0.06	0.11	0.12	0.10	0.12
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents summary difference-in-difference coefficients of Equation 2, estimated by Ordinary Least Squares (OLS) using the DHS dataset. The subsample analysis corresponds to the native mother tongue, wealth index, household size, and sex of household head. All regressions include covariates. Covariates include age, age squared, an indigenous native tongue (=1), and wealth index, as well as district and survey-year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

TABLE A6 – MECHANISMS – DHS (2013-2019)

	(1)	(2)	(3)
Panel A. Contraceptive use			
	Use of traditional method	Currently uses condom	Knowledge of modern methods
	(1)	(2)	(3)
4km HC x Post	-0.034**	0.043*	0.037**
	(0.017)	(0.023)	(0.017)
Observations	6,124	5,294	22,819
Mean Dep. Var.	0.12	0.19	0.47
Panel B. Family Planning services			
	FP with partner	FP with family	FP with friends
4km HC x Post	-0.027***	-0.033**	-0.026*
	(0.009)	(0.014)	(0.014)
Observations	22,819	22,819	22,819
Mean Dep. Var.	0.12	0.26	0.23
Panel C. Sexual behaviour			
	Under-18 first intercourse	Age at first intercourse	Age difference with first partner
4km HC x Post	-0.014	0.068	-0.237
	(0.013)	(0.069)	(0.230)
Observations	22,819	6,124	6,084
Mean Dep. Var.	0.27	14.94	4.18
Covariates	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes

Notes: This table presents summary difference-in-differences coefficients of Equation 2, estimated by Ordinary Least Square (OLS) using the DHS dataset. Covariates include age, age square, an indigenous native tongue (=1), and wealth index. All regression includes district and survey year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

TABLE A7 – PRE-TRENDS - *CERTIFICADO NACIDO VIVO* (2013-2016)

	All sample (1)	13–14 (2)	15–17 (3)
HCA in district × 2014	0.249	-0.030	0.526
	(0.663)	(0.332)	(1.272)
HCA in district × 2015	0.762	-0.351	1.875
	(0.999)	(0.409)	(1.860)
HCA in district × 2016	-0.013	-0.232	0.198
	(0.986)	(0.461)	(1.844)
Observations	14,938	7,470	7,468
Pre-trends testing pvalue	0.745	0.796	0.655
Mean Dep. Var. (control)	17.71	6.22	28.20

Notes: Each row reports OLS estimates from separate regressions of the district-level teenage fertility rate (births per 1,000 girls aged 13–17) on an indicator for whether an adolescent health center (HCA) was present in the district in a specific pre-reform year. These regressions test for differential trends before the implementation of the 2017 family planning reform. All regressions include district and survey year-department fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

TABLE A8 – PLACEBO YEARS - CNV (2013-2016)

	2014 (1)	2015 (2)	2016 (3)
HCA in district × Post=2014	-0.725		
	(0.762)		
HCA in district × Post=2015		-1.044	
		(0.802)	
HCA in district × Post=2016			-1.677
			(0.665)
Observations	26,145	26,145	26,145
Mean Dep. Var.	15.20	15.20	15.20
Year - Department FE	Yes	Yes	Yes
Age group - Department FE	Yes	Yes	Yes

Notes: Each column reports OLS estimates from separate regressions that assign placebo implementation dates for the 2017 reform. The outcome is the district-level teenage fertility rate (births per 1,000 girls aged 13–17). In each specification, the treatment indicator equals 1 if an HCA is located in the district and the placebo reform is assumed to begin in the specified pre-reform year. All specifications include region fixed effects to absorb time-invariant regional heterogeneity. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

TABLE A9 – PRE-TRENDS - DHS (2013-2016)

	All sample (1)	Only urban (2)
HCA within 4 km × 2014	0.013	0.024
	(0.014)	(0.016)
HCA within 4 km × 2015	-0.012	-0.021
	(0.014)	(0.017)
HCA within 4 km × 2016	0.007	0.006
	(0.016)	(0.017)
Observations	11,428	7,333
Pre-trends testing pvalue	0.340	0.061
Year - Department FE	Yes	Yes
Age group - Department FE	Yes	Yes

Notes: Each colum reports separate OLS estimates from difference-in-difference coefficients of Equation 2 using the DHS dataset. These regressions test for differential trends before the implementation of the 2017 family planning reform. All regressions include covariates. Covariates include age, age squared, an indigenous native tongue (=1), and wealth index, as well as district and survey-year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

TABLE A10 – PLACEBO YEARS - DHS (2013-2016)

	All sample			Only urban		
	2014	2015	2016	2014	2015	2016
	(1)	(2)	(3)	(4)	(5)	(6)
HCA within 4km × Post=2014	-0.007			-0.007		
	(0.010)			(0.011)		
HCA within 4km × Post=2015		-0.011			-0.013	
		(0.008)			(0.009)	
HCA within 4km × Post=2016			-0.009			-0.007
			(0.008)			(0.008)
Observations	22,819	22,819	22,819	14,976	14,976	14,976
Mean Dep. Var.	0.12	0.12	0.12	0.09	0.09	0.09
Year - Department FE	Yes	Yes	Yes	Yes	Yes	Yes
Age group - Department FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports OLS estimates from separate regressions that assign placebo implementation dates for the 2017 reform using the DHS. The outcome is whether the individual gave birth before the age of 18. In each specification, the treatment indicator equals 1 if an HCA is located within a 4km distance and the placebo reform is assumed to begin in the specified pre-reform year. All regressions include covariates. Covariates include age, age squared, an indigenous native tongue (=1), and wealth index, as well as district fixed effects. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

TABLE A11 – EXCLUDING METROPOLITAN LIMA - *CERTIFICADO NACIDO VIVO* (2013-2019)

	All sample	13-14	15-17
	(1)	(2)	(3)
HCA in district x Post	-1.770***	0.255	-3.801***
	(0.681)	(0.276)	(1.293)
Observations	25,543	12,772	12,771
Mean Dep. Var.	15.32	2.08	28.56
Year - Department FE	Yes	Yes	Yes
Age group - Department FE	Yes	Yes	Yes

Notes: This table presents summary difference-in-difference (DiD) coefficients, estimated by Ordinary Least Squares (OLS) using the universe of birth records dataset. Column 1 presents estimates for all women teens (13 to 17 years old). Column 2 shows the results for women aged 15 to 17 years old. All regression includes district, year-department fixed effects and sex of the child as controls. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

TABLE A12 – EXCLUDING ONE REGION AT A TIME - *CERTIFICADO NACIDO VIVO* (2013-2019)

Excluded Region	Coef. (SE)	R-squared
Amazonas	-2.406** (1.123)	0.419
Ancash	-2.369* (1.220)	0.415
Apurimac	-2.504** (1.108)	0.411
Arequipa	-2.504** (1.108)	0.416
Ayacucho	-2.604** (1.145)	0.420
Cajamarca	-2.205* (1.169)	0.411
Callao	-2.571** (1.126)	0.413
Cusco	-2.615** (1.062)	0.404
Huancavelica	-2.162* (1.133)	0.418
Huanuco	-1.981* (1.100)	0.410
Junin	-2.504** (1.108)	0.410
Ica	-3.209*** (1.127)	0.412
La Libertad	-2.326** (1.175)	0.411
Lambayeque	-2.331** (1.132)	0.411
Lima	-3.515*** (1.269)	0.424
Loreto	-2.612** (1.107)	0.411
Madre de Dios	-2.481** (1.116)	0.413
Moquegua	-2.504** (1.108)	0.413
Pasco	-2.378** (1.128)	0.413
Piura	-2.487** (1.139)	0.409
Puno	-2.504** (1.108)	0.408
San Martin	-2.623** (1.156)	0.410
Tacna	-2.504** (1.108)	0.415
Tumbes	-2.547** (1.117)	0.412
Ucayali	-2.331** (1.106)	0.411

Notes: This table presents summary difference-in-difference (DiD) coefficients, estimated by Ordinary Least Square (OLS) using the universe of births records dataset. Column 1 presents estimates for all women teens (13 to 17 years old). Column 2 shows the results for women aged 15 to 17 years old. All regression includes district, survey year-department fixed effects and sex of the child as controls. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

TABLE A13 – MAIN RESULTS USING QUARTERLY AND SEMESTER DATA - *CERTIFICADO NACIDO VIVO* (2013-2019)

	Quarterly (1)	Semester (1)
HC in district × Post	-0.376**	-0.802**
	(0.157)	(0.323)
Observations	51,182	25,593
Mean Dep. Var.	3.73	7.83
Survey year FE	Yes	Yes
District FE	Yes	Yes

Notes: This table presents summary difference-in-difference (DiD) coefficients, estimated by Ordinary Least Squares (OLS) using the universe of birth records dataset. Column 1 presents estimates for all women teens (13 to 17 years old). Column 2 shows the results for women aged 15 to 17 years old. All regression includes district, year-department fixed effects and sex of the child as controls. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

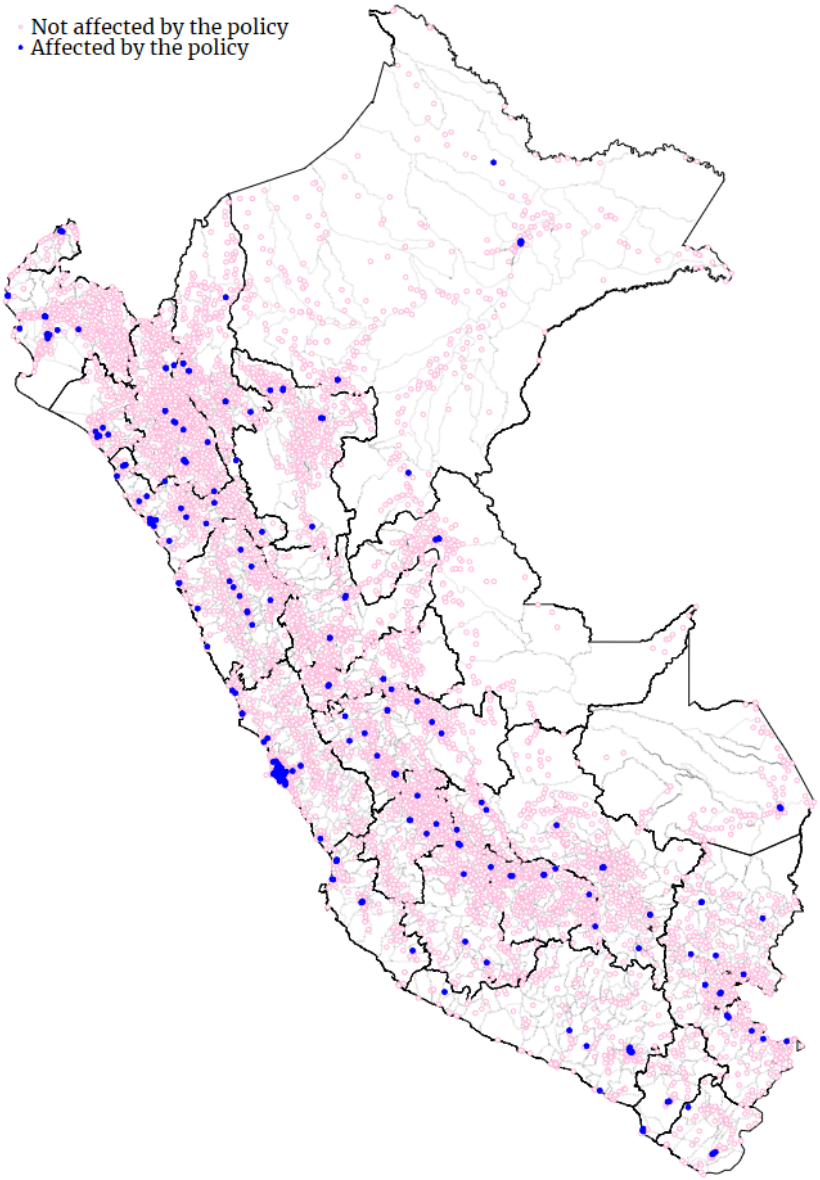
TABLE A14 – PROBIT ANALYSIS - DHS (2013-2019)

Specification	Coefficient	Std. Error	Mean Dep. Var.	N
Panel A. Post × HCA within 2km				
All sample	-0.010	(0.009)	0.1446	19,726
Only urban clusters	-0.009	(0.008)	0.1074	13,400
Panel B. Post × HCA within 3km				
All sample	-0.014*	(0.008)	0.14466	19,726
Only urban clusters	-0.014*	(0.007)	0.1074	13,400
Panel C. Post × HCA within 4km				
All sample	-0.015*	(0.008)	0.1446	19,726
Only urban clusters	-0.015*	(0.009)	0.1074	13,400
Panel D. Post × HCA within 5km				
All sample	-0.016*	(0.009)	0.1446	19,726
Only urban clusters	-0.017*	(0.010)	0.1074	13,400

Notes: This table presents summary difference-in-difference (DiD) coefficients, estimated by Ordinary Least Square (OLS) using the universe of births records dataset. Column 1 presents estimates for all women teens (13 to 17 years old). Column 2 shows the results for women aged 15 to 17 years old. All regression includes district, survey year-department fixed effects and sex of the child as controls. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by ***, **, and *, respectively.

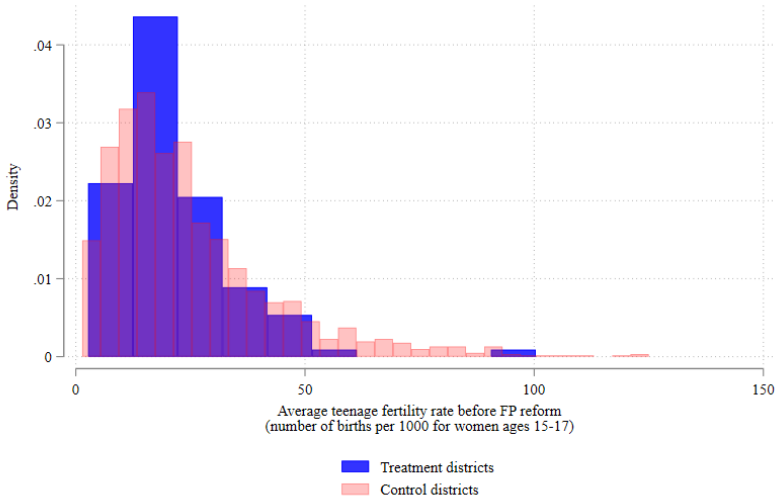
2. **Figures**

FIGURE C1 – LOCATION OF HEALTH CENTERS PROVIDING A FAMILY PLANNING SERVICE IN PERU (DHS)



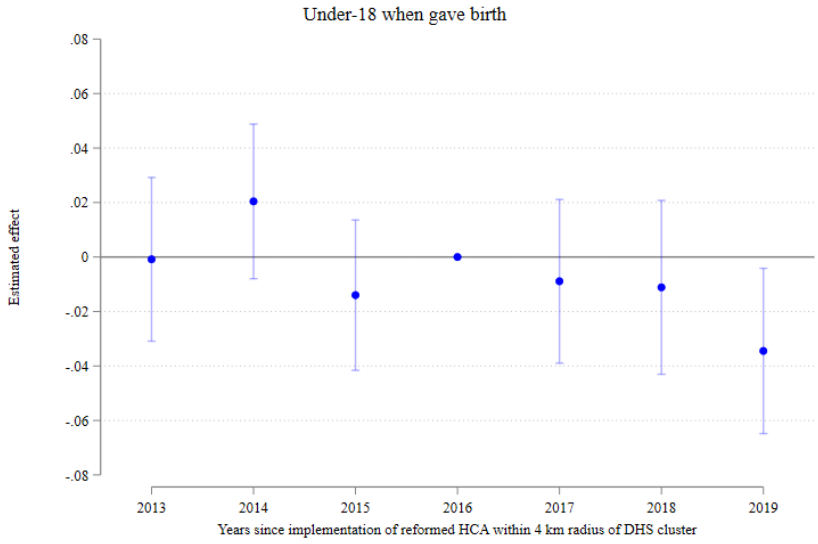
Notes: This graph illustrates the location coordinates of the HCA centers and other health centers at the national level using data from the Ministry of Health.

FIGURE C2 – DISTRIBUTION OF TEEN FERTILITY RATES BEFORE 2017



Notes: This figure shows the distribution of average teenage fertility rates (age groups 15, 16, and 17) in the pre-FPTG reform period for the 1,872 districts in the main sample, separately for treatment and comparison districts. The pre-FPTG reform period includes the years 2013, 2014, 2015, and 2016. Treatment districts contain HCA centers, while comparison districts have other health centers.

FIGURE C3 – EVENT STUDY FOR TEENAGE PREGNANCY RATES – DHS (2013–2019)



Notes: This graph presents summary difference-in-difference coefficients of Equation 2, estimated by Ordinary Least Squares (OLS) using the DHS dataset. All regressions include covariates: age, age squared, indigenous native tongue (=1), and wealth index. All regressions include district and survey year fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 90% level are plotted.

FIGURE C4 – FLOWCHART FOR COUNSELING IN FAMILY PLANNING CONSULTATIONS

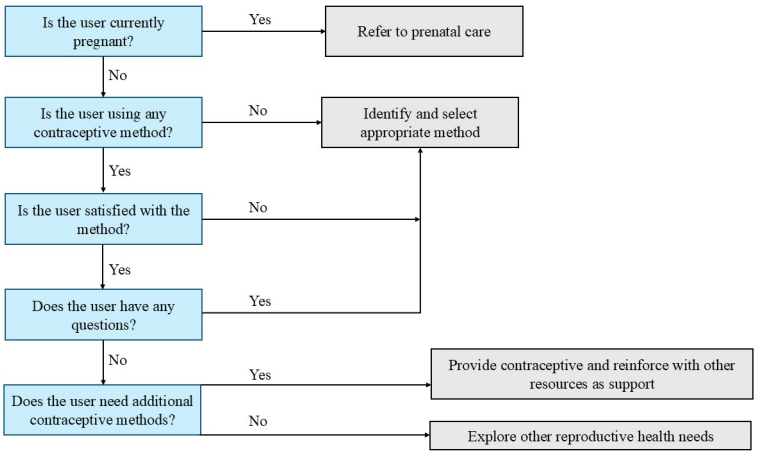


FIGURE C5 – SELECTING A FAMILY PLANNING METHOD

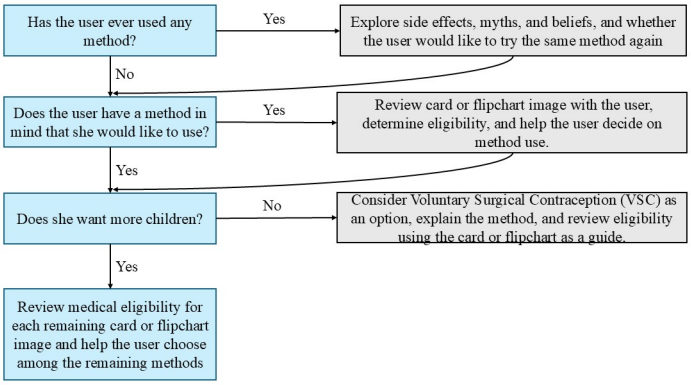


FIGURE C6 – SERVICE IMPLEMENTATION

(a) COUPLES SERVICES



(b) INFORMATION CAMPAIGNS



Notes: Photograph taken during monitoring visit by mass media. No identifiable information is disclosed.

TABLE A15 – MATERIALS, EQUIPMENT, AND FURNISHINGS REQUIRED FOR SERVICE PROVISION

2 **Materials**

- Pamphlets on contraceptive methods, STI prevention, breast self-exams, HIV/AIDS.
- Violence screening form.
- Flip charts or visual cards of contraceptive methods.
- Anatomical models (female and male reproductive tracts).
- Displays of contraceptive methods.

Contraceptive Methods and Supplies

- Contraceptive methods.
- Microscope slides.
- Ayre spatulas.
- Cytobrushes.
- 70 alcohol.
- Sterile gloves.
- Lidocaine 2% without epinephrine (5 cm syringe, 21G needle).
- Rapid HIV tests.

Equipment

- Vaginal speculums (small, medium, large).
- IUD insertion/removal equipment.
- Female genital exam equipment.
- Male genital exam equipment.

Information Records

- Medical record.
- Standard prescription form.
- Lab test request form.
- Imaging request form.
- Appointment control card.
- SIS Unique Care Format.
- Family Planning follow-up form.
- FP Activity Report (SIS 240–D).
- Daily Services Log (HIS).

Furnishings

- Desk.
- Chairs.
- Gynecological exam table.
- Rotating stool.
- Gooseneck lamp.
- Screen divider.
- Metal shelves.
- Scale.
- Stethoscope.
- Aneroid sphygmomanometer.

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