Preventing Teen Pregnancies at Scale: School-Based Access to Contraceptives and Reproductive Health

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February, 2026

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Abstract

This paper studies whether delivery architecture shapes the effective take-up of reproductive health services among adolescents. I examine Argentina's National Plan for the Prevention of Unintended Pregnancy in Adolescence (ENIA), a large-scale reform that integrated confidential school-based counseling with direct referrals to long-acting reversible contraceptives (LARCs). Exploiting the staggered rollout of the program across counties and administrative birth records, I estimate intent-to-treat effects on adolescent fertility. ENIA reduced adolescent birth rates by 7 to 10 percent, with effects emerging at the biologically plausible nine-month horizon and concentrated in first births. Mechanism evidence points to school-based counseling as a key gateway into the health system. Fertility declines coincide with sharp increases in the provision of subdermal implants, and within treated counties, counseling strongly predicts subsequent contraceptive uptake. Effects are substantially larger in socially conservative areas, consistent with the program relaxing binding gatekeeping constraints. Using 2022 Census tabulations, I document modest increases in secondary school completion among exposed cohorts. The findings highlight how redesigning delivery systems—by shifting access points and reducing non-price barriers—can determine whether formal rights translate into effective access.

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1 Introduction

Nearly a third of women in low- and middle-income countries give birth before age 20 (UNFPA, 2022). Teenage childbearing carries substantial costs for both mothers and the next generation, significantly impacting health, educational, and labor market trajectories across the life cycle (Aizer, Devereux, & Salvanes, 2022; Levine & Painter, 2003).[1] Yet, a large share of these pregnancies are unintended, reflecting a persistent gap between reproductive intentions and outcomes (Sully et al., 2020). This gap suggests that adolescent fertility reflects not only preferences, but binding constraints on effective access to contraception.

This puzzle is especially salient in Latin America. Despite near-universal secondary school enrollment and relatively progressive legal frameworks for sexual and reproductive health, adolescent birth rates remain among the highest outside Sub-Saharan Africa. A key feature of the region is the wedge between de jure entitlements and de facto access: contraception is often formally free, yet confidentiality concerns, provider discretion, and social stigma can impose a high non-monetary "price" on minors. These barriers are particularly binding where local norms restrict adolescents' autonomy. Understanding whether reducing these non-price barriers can shift adolescent fertility is therefore a first-order development and human-capital question.

This paper studies whether redesigning delivery architecture can relax these constraints and translate formal rights into effective access. I examine Argentina's National Plan for the Prevention of Unintended Pregnancy in Adolescence (ENIA), a multi-sectoral reform that embedded confidential counseling in public schools and created fast-track referrals to free long-acting reversible contraceptives (LARCs), especially subdermal implants. By shifting the primary access points from traditional clinics to schools, ENIA effectively bypassed local gatekeeping and provided adolescents with a confidential channel to initiate the use of highly effective contraception. In doing so, this approach builds on recent evidence demonstrating that schools can serve as powerful, scalable platforms for delivering norm-shifting interventions and services to adolescents (Dhar, Jain, & Jayachandran, 2022).

ENIA rolled out in 2018 through staggered adoption across counties. I leverage multiple sources of quasi-experimental variation—staggered county rollout, age-specific exposure, and within-county differences across cohorts and school types—to estimate intent-to-treat effects and to isolate the role of the school-based delivery channel. I combine restricted-access birth records from Argentina's Ministry of Health with detailed administrative data on ENIA's monthly implementation, encompassing school-based counseling sessions and LARC provision. To estimate the impact of the program, I exploit its staggered rollout in a difference-in-differences framework.[2]

[1] A large literature documents the negative life-cycle impacts of early childbearing on both the offspring and other family members (see, e.g., Ashcraft, Fernández-Val, & Lang, 2013; Heissel, 2017; Kingston et al., 2012).

[2] I estimate heterogeneity-robust event-study DiD models for a staggered, binary, and absorbing treatment (Borusyak,

ENIA generated a persistent decline in adolescent fertility: birth rates fell by 7 to 10 percent from a baseline of 55.1 births per 1,000 females aged 15–19. In the preferred population-weighted specification, the program reduced the adolescent birth rate by 4.0 births per 1,000 (s.e. = 1.14), or approximately 1,425 averted births per year across treated counties. Consistent with the biological lag between conception and birth, effects emerge nine months after implementation. The decline is driven almost entirely by a 8.2 percent reduction in first births, indicating that the program primarily reduce entry into teenage motherhood. I find no evidence of differential pre-trends in fertility or contraceptive supply, and the fertility declines coincide with sharp increases in school-based counseling and LARC provision in treated counties. The estimated impacts are in the upper range of prior estimates from comparable policies (Lindo & Packham, 2017; Miller, 2010).[3] Relative to these settings, ENIA is distinctive in its population-wide targeting of adolescents and its school-anchored delivery architecture.

To understand the mechanisms, I use detailed implementation data. The evidence points to school-based counseling as the key gateway into the health system: within treated counties, counseling strongly predicts subsequent adoption of subdermal implants, whereas broader sex education alone displays weaker associations with short-run uptake. I also find larger effects in more socially conservative areas, consistent with the program relaxing binding gatekeeping constraints in restrictive environments.

Complementary evidence suggests these demographic shifts translated into broader human capital gains. Within-school survey data replicate the fertility decline, showing larger reductions in parenthood in the public schools where ENIA's counseling component was implemented. Furthermore, using 2022 Census tabulations, I find evidence that cohorts exposed to the program experienced modest increases in secondary school completion, consistent with delayed childbearing improving educational trajectories.

This paper contributes to the literature on the demographic and economic impacts of contraceptive access. Evidence from high-income settings shows that modern contraception can alter fertility and life-cycle investments (Bailey, 2006; Goldin & Katz, 2002), but evidence from low- and middle-income countries remains mixed (Dupas et al., 2024; Miller, 2010). Moreover, adolescent-focused interventions in LMICs are often small-scale and NGO-led (Banerjee et al., 2017). I provide national-scale evidence that a government-led, school-anchored reform can generate first-order reductions in adolescent fertility when it expands confidential access to highly effective methods.

Second, the paper speaks to work on institutional frictions, social norms, and last-mile delivery. Even when formal rights exist, stigma, misinformation, and provider discretion can act as gatekeep-

Jaravel, & Spiess, 2024; Callaway & Sant'Anna, 2021; de Chaisemartin & d'Haultfoeuille, 2020; Sun & Abraham, 2021).

[3]For example, an experimental evaluation of an 18-month program facilitating access to contraceptive services to adolescent mothers led to a 17.4% reduction in repeated pregnancy (Luca et al., 2021).

ing constraints that limit effective access for adolescents (Chandra-Mouli et al., 2014; Marie & Zwiers, 2023). I link these ideas to demographic outcomes by showing that ENIA's school-based counseling and referral pathway increased uptake of highly effective contraception and reduced births, with larger impacts in areas where restrictive norms are more salient. The results highlight how redesigning delivery architecture can relax norm-mediated constraints in morally contested domains.

Finally, this study contributes to research on schools as delivery platforms and adolescent human capital. Much of the policy toolkit to reduce teen fertility in LMICs has relied on demand-side incentives (Behrman, Parker, & Todd, 2011; Olson, Clark, & Reynolds, 2019), while evidence on standalone sex education often finds limited behavioral change (Paton, Bullivant, & Soto, 2020). ENIA illustrates a scalable public-sector alternative: bundling education with confidential school-based counseling and access to highly effective contraception. These results highlight how delivery systems—not only policies—determine whether formal rights translate into effective access.

2 Institutional Context and the ENIA Program

2.1 Adolescent Fertility and Institutional Barriers in Latin America

Despite global declines in adolescent fertility, Latin America and the Caribbean have experienced one of the slowest reductions in teen birth rates and still account for a large share of early motherhood worldwide (Caffe et al., 2017).[4] In fact, in 2013 it was the only region with a rising trend in pregnancies among mothers younger than 15 years old.

In response to the lack of progress in LAC, PAHO/WHO, UNFPA and UNICEF concluded that there is no single portrait of teenage mothers. For some, pregnancy may represent an opportunity to gain adult status and upward social mobility, while for others pregnancy may result from sexual abuse, lack of knowledge about their sexual and reproductive health and rights, poor access or improper use of contraceptives, barriers to abortion even in post-rape cases, or restrictive laws, policies, or practices despite the presence of enabling laws (Caffe et al., 2017). For instance, even successful programs in Argentina and Colombia, such as comprehensive sex education (CSE) and adolescent-friendly health services, lacked the necessary intensity and sustainability to have lasting impacts (Cano and Sanabria, 2020). In the case of Argentina, in 2012 the CSE national law mandated that all schools include the curriculum in every course (a transversal approach). However, lack of enforcement and monitoring, combined with low support from principals, parents, and provincial leaders, led to scatter and limited reach.[5] This example is not uncommon in LAC, since the region had some of the world's most conservative laws on abortion (United Nations, 2014).

Historical religious influence has shaped reproductive norms and policies across Latin Amer-

[4] Appendix Figure A1 shows birth trends for every 1,000 women aged 15 to 19 years old for each continent.

[5] The education and health system is decentralized according to the federal organization of the country, and some provinces favor religious education in public schools.

ica. Catholic pressure (or the fear of it) has thus delayed support for family planning initiatives. Consistent with institutional persistence and models of cultural transmission across generations, the Church's influence has shaped current norms and policies (Bisin, 2000; Bisin and Verdier, 2001). For example, until 1994 Argentina's president was required to be Catholic (as is also the case in some other countries in the region), and numerous demonstrations protested the flow of reproductive health information—i.e., the supply of CSE.[6] Therefore, in this paper I explore the impact of a public policy directly designed to address issues related to reproductive health and contraception, which are at the core of the religious sentiment in many parts of Latin America and the Caribbean. I do so by exploiting the religious heterogeneity that exists across region in Argentina, while capitalizing on county-level differences in treatment status brought by ENIA.

Argentina exhibits substantial regional variation in adolescent fertility rates and social norms. Figure A3 illustrates the heterogeneity across provinces and countries in LAC. Further, the evolution and levels of the women's rights index calculated by Tertilt et al. (2022) depict a homogeneous image of LAC, wherein Argentina appears to be a representative country.[7]

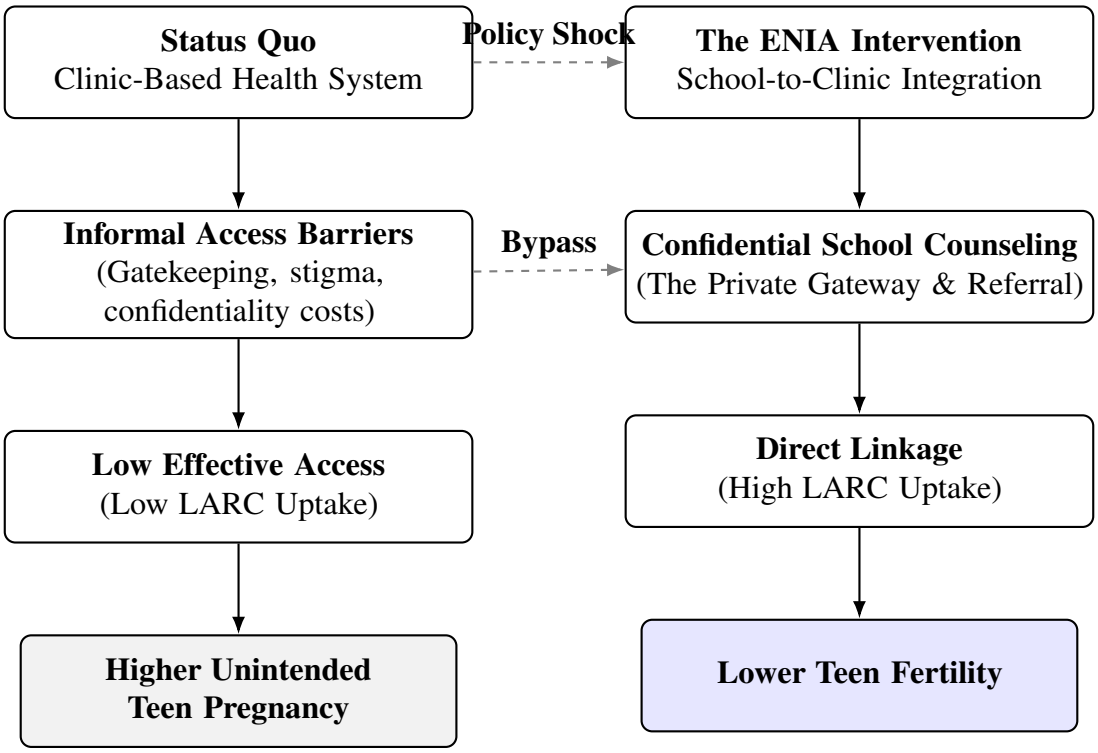


Figure 1: Conceptual Framework: How Confidential Counseling Bypasses Gatekeeping to Reduce Teen Fertility

[6] *Con mis hijos no te metas* ("Don't mess with my kids") is a social movement that opposes the inclusion of comprehensive sex education in national curricula. Over the years, several thousands of families in Peru, Bolivia, Paraguay, and Argentina have participated in such demonstrations.

[7] Even though Argentina has a larger GDP per capita than other countries in LAC, such as Peru or Brazil, its PISA tests scores in 2018 were significantly below the median of the 10 participating countries.

2.2 The ENIA Program

Argentina's National Plan for the Prevention of Unintended Teen Pregnancies (ENIA) is a large-scale interministerial program launched in 2018 to reduce adolescent pregnancy by expanding access to sexual and reproductive health services. It involved the Ministries of Social Protection, Health and Education, and its design started during 2017 as part of the 2030 Agenda for Sustainable Development. ENIA poses an integral and intersectorial intervention using an approach based on rights, gender, and social equity. Its main goal is to prevent and reduce unintended teen pregnancies by increasing the number of adolescents effectively and appropriately protected by contraceptives (De León and Thourte, 2019). The focus is on promoting access to public services that strengthen the autonomy of adolescents to make informed decisions about their sexuality and facilitate access to relevant information and appropriate protection methods.

ENIA's main objectives are (1) to raise awareness of the importance of preventing unintended pregnancy in adolescence; (2) to promote access to sexual and reproductive rights; (3) to provide information on sexual and reproductive health as well as contraceptive methods free of charge; and (4) to strengthen the prevention of abuse and sexual violence and increase access to the legal interruption of pregnancy, according to the regulatory framework in Argentina, in cases of rape or when the mother's or newborn's life is in danger.

The programs can be roughly categorized in three main components:

1. *Education*: This strategy (in public schools) strengthens CSE, so that adolescents can access information on sexual and reproductive health, gender, diversity, affectivity, and rights and in-school, walk-in counseling with private and confidential referrals to healthcare providers.

2. *Health*: Comprehensive health consulting consists of listening, counseling, and consultation in middle schools and health centers. This facilitates and orients adolescents' approach to the health system and strengthens the contraceptive supply by providing a variety of LARC and consultation.

3. *Community-based*: Community awareness strategies using adults and role models encourage informed decisions. These mainly target teenagers outside the educational system to facilitate access to health services.

2.3 Implementation and Staggered Rollout

Implementation. ENIA targeted the whole population of adolescents in 36 treated counties in 12 prioritized provinces—i.e., subnational states[8]. Prioritized provinces had in between 2 to

[8]Much like in the United States, Argentina is a federal country where subnational entities—provinces—hold significant autonomy. Provinces have their own constitutions and are responsible for key policy areas such as education, healthcare, and social protection. While provinces play a central role in service provision and administration, they rely

6 selected counties. The selection criteria for provinces relied on estimates of the incidence of unintended teen pregnancies in 2017. This data is not fully representative as it covers less than 50% of all pregnancies and is subject to incomplete reporting. Then, counties were selected based on adolescent population size, to add up to at least 30% of public schools in the province. In selected counties, they worked with all the *public* secondary schools (SENAF, Secretaría Nacional de Niñez, Adolescencia y Familia, 2019).

Staggered adoption. The program was implemented in three stages during 2018, creating staggered exposure across counties. My first identification of the timing of the program is based on official reports of adoption (e.g., the time at which at least two of the planned devices started operations). There are three stages, early (January-April), intermediate (May-August), and late (September-December). The corresponding proportions of treated counties are $\frac{1}{12}$, $\frac{1}{4}$, and $\frac{2}{3}$, respectively.[9] The spatial distribution of treated counties, located primarily in northern Argentina, and staggered adoption is summarized in Figure 2. Note that every province, including has most of its counties as control regions.

Potential concerns. A potential concern is that ENIA may have interacted with other reproductive health programs. That is the case if there was a reassignment of resources from control counties towards treated counties. On the contrary, the plan had its own public budget, managed its purchases of LARC, and designed curricular material and training in the context of the national CSE curricula, rather than increasing the demand for ongoing national pieces of training.[10]

On the other hand, there are reasons to believe that the plan may have had different kinds of spillovers on control counties. For example, as part of the community outreach, *influencers* campaigns discussing contraceptives methods, double barrier contraception, consent, a challenge on how to put on a condom in 20 seconds, among others, had a reach of at least 2.8 million views. Also, In July 2019 through a sanitary train, the plan delivered about 690 subdermal implants among different towns and cities including control counties (ENIA, 2020). Hence, I expect to recover a lower bound of the impact of the plan on adolescent birth rates in case of a predicted decrease, or an upper bound in case of an increase.

Another concern is related to Argentina's lockdown due to the COVID pandemic that could potentially affect county's birth rates in a way that is related to the target of ENIA, however I restrict my attention to births conceived before the pandemic.

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heavily on federal transfers for funding, whereas in the U.S., states have broader taxation powers and greater fiscal autonomy.

[9]Adoption timing for the province Formosa is omitted mainly due to its lack of implementation of the school-based programs. I code it as a late adopter acknowledging the intent-to-treat nature of my analysis and further providing sensitivity analysis excluding this province.

[10]Former public servants working on the plan acknowledged the significance of having other ongoing social protection policies to reach higher effectiveness, however other social protection policies were also present in the rest of Argentina.

The staggered rollout across counties provides the basis for the empirical strategy described in the next section.

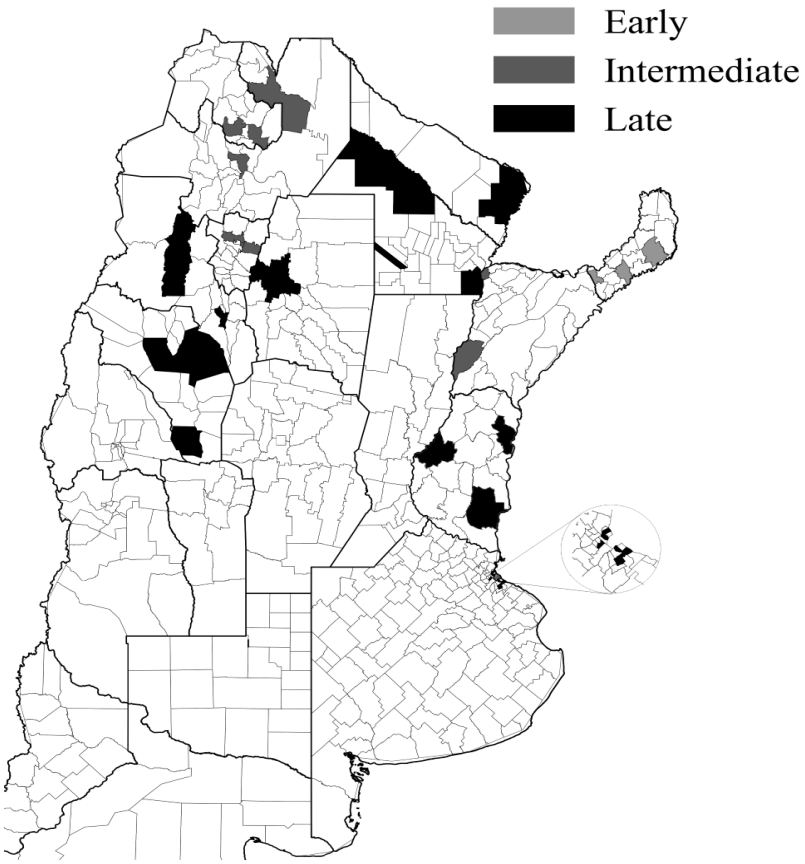


Figure 2: Target and Timing of ENIA Implementation

Notes: Spatial distribution of ENIA-treated counties and staggered rollout in 2018. Shading denotes the month of program adoption: early (January–April), intermediate (May–August), and late (September–December). Provincial borders are shown with darker outlines. Southern provinces are omitted as they contain no treated counties. The inset shows Greater Buenos Aires.

3 Data and Empirical Strategy

This section describes the data and empirical strategy used to estimate the impact of ENIA on adolescent fertility. I combine two main sources of variation. First, I exploit the staggered rollout of ENIA across counties to estimate intent-to-treat effects on birth rates using administrative vital statistics. Second, I use school census data to examine whether the school-based components of the program amplified these effects, exploiting variation across counties, cohorts, and public versus private schools. Throughout, the main outcome of interest is the birth rate per 1,000 females aged 15–19.

3.1 Data Sources and Outcomes

Birth records. The main data source is the universe of official live birth records from Argentina's Ministry of Health, covering January 2012 to September 2020.[11] Registries have mother characteristics, including the age, county of residence, and month of birth.[12]

Birth rates are constructed as the number of births to mothers aged 15–19 divided by the number of women aged 15–19 in the county, expressed per 1,000 women. Population counts for women aged 15–19 are obtained from tabulations of single-year age distributions from the 2010 and 2022 population censuses. County-level population counts for intermediate years are constructed by linearly interpolating age-specific female population counts between the two censuses.

I undertake most analyses at the quarter level to balance two considerations: monthly data provide sharper timing but more erratic paths, while annual data mask the dynamics of pregnancy prevention. Quarters provide a natural compromise, given that they correspond to roughly one-third of a full-term pregnancy.

For analyses that distinguish outcomes by maternal age (e.g., the age-specific difference-in-differences estimates), birth rates are computed separately for each single year of age using the corresponding age-specific female population in the county.

Appendix Figure A13 shows that pre-treatment birth-rate distributions in treated and control counties are broadly similar, although treated counties had somewhat higher adolescent fertility levels on average, consistent with program targeting.

School censuses. To provide complementary *within-school* evidence on ENIA's school-based margin, I use Aprender, Argentina's national school census administered to students in the last year of secondary school in 2017 and 2019. The survey includes student and principal questionnaires covering school trajectory, sociodemographic characteristics, learning environment, and self-reported parenthood.

Aprender outcomes are measured only among students who are enrolled and attending school in the surveyed grade/year. Consequently, estimates using Aprender identify an *in-school* estimand: the effect of ENIA on adolescent parenthood among students who remain enrolled at the relevant age/grade. This is a policy-relevant object because ENIA's counseling and referral device operates through schools, and Aprender provides individual-level measures of parenthood and exposure to program components within that setting.

A key caveat is that ENIA may affect school enrollment and thus the composition of the Apren-

[11] While data for the last semester of 2020 was available I discarded it from my analysis for mainly two reasons: (1) based on estimations from the data provider the reliability of that period is typically low (due to births being reported in the following year), (2) Argentina's lockdown due to the COVID pandemic started on March 2020 making births in the last quarter being potentially biased by a major shock to fertility.

[12] While migration can be a concern, I estimate that in roughly 97% of records, mothers' province of residence corresponds with the location of the facility at which the birth took place.

der sample. If ENIA reduces pregnancies, it may increase retention among girls who would otherwise drop out, mechanically making the observed in-school sample more negatively selected; this would tend to attenuate estimated reductions in in-school parenthood (bias toward zero). More generally, Aprender estimates should not be interpreted as a direct replication of population-wide birth-record effects. Instead, I use Aprender as supporting evidence on the school-based delivery channel and as a complement to administrative birth records.

I distinguish between public and private schools and construct measures of exposure to comprehensive sex education (CSE). At the student level, I define indicators for exposure to any of 14 CSE topics (*Any CSE*) and to any of the three ENIA-related modules (*Any ENIA*). At the school level, I use principals' reports to construct analogous measures, a standardized ratio of ENIA-targeted modules over total CSE (*Ratio*), and an agreement measure (*Agree*) that equals one when the principal and at least half of surveyed students report exposure to at least one ENIA-related module.

3.2 Estimation Sample and Common Support

The main estimation sample is restricted to counties whose adolescent female population lies within the support of treated counties. This improves comparability by excluding very small counties, often rural, where adolescent births are infrequent and measured with substantial noise. Appendix results using the full sample are quantitatively similar.

The resulting panel includes 328 counties observed quarterly between 2012 and September 2020, corresponding to 35 quarters of data. The common-support sample accounts for approximately 86.2% of births observed in the full dataset. The main results are estimated on this common-support sample. For transparency, I also report results using the full sample of counties in Appendix Tables and Figures, which produce quantitatively and qualitatively similar estimates.

3.3 County-Level Identification

To estimate the effect of ENIA on adolescent birth rates, I exploit the staggered rollout of the program across counties in a difference-in-differences framework. Identification comes from comparing changes in birth rates in treated counties before and after rollout with contemporaneous changes in counties that were not yet treated or never treated. Because childbearing is biologically delayed, any effect of the program should emerge only after the pregnancies affected by the intervention would have come to term, that is, roughly three quarters after implementation.

Difference-in-Differences. As a benchmark summary specification, I estimate the following difference-in-differences model:

$$B_{it}^{19} = \alpha + \gamma ENIA \times POST_{it} + X_{it}'\phi + \mu_i + \delta_t + \varepsilon_{it}, \tag{1}$$

where B_{it}^{19} denotes the birth rate among females aged 15–19 in county i and quarter t. The

treatment indicator $ENIA \times POST_{it}$ switches on three quarters after implementation in treated counties, reflecting the earliest period in which effects on births can plausibly be observed. County fixed effects absorb time-invariant differences across counties, while time fixed effects absorb aggregate shocks common to all counties. The vector X_{it} includes time-varying controls, namely province-specific seasonality and a proxy for local economic activity based on night-time lights.

A standard concern in staggered DiD designs is that two-way fixed effects estimates may be difficult to interpret under treatment-effect heterogeneity across cohorts or over time (Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021). I therefore complement the single-coefficient specification with dynamic event-study estimates and heterogeneity-robust estimators.[13]

Event study. To trace the dynamic response of adolescent birth rates to ENIA, I consider the following event-study representation:

$$B_{it}^{19} = \alpha + \sum_{j=-K}^{J} \beta_j D_{it}^{j} + X_{it}'\phi + \mu_i + \delta_t + \varepsilon_{it}, \tag{2}$$

where D_{it}^{j} indicates that county i is j quarters away from the policy start. The event-study specification traces treatment effects relative to the period immediately before the expected impact window. Leads allow me to assess the plausibility of parallel trends and the absence of anticipatory responses, while lags recover the dynamic response of birth rates after implementation. Because counties adopt the program at different dates, the relative-time panel is unbalanced, so I bin distant pre- and post-treatment periods.

The identifying assumption is that, absent ENIA, treated counties would have followed the same trend in adolescent birth rates as comparison counties, conditional on fixed effects and controls. A second assumption is the absence of anticipatory responses. The event-study estimates provide an empirical check on both.

In the empirical analysis, I estimate dynamic treatment effects using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2024). This approach recovers cohort-specific treatment effects by imputing counterfactual outcomes for treated units using untreated observations and avoids the negative weighting issues that may arise in staggered adoption designs. Let τ_k denote the average treatment effect k quarters after the policy start. In the empirical analysis, particular attention is given to the τ of the first period after the biological onset of the fertility response in births (nine months to twelve months after implementation).

Because treatment effects may differ across adoption cohorts, I also report interaction-weighted

[13]To assess the potential severity of this concern, I also document the decomposition of $\hat{\gamma}$ into a series of 2×2 difference-in-differences models using the package proposed by Goodman-Bacon, Goldring, and Nichols (2022).

estimates following Sun and Abraham (2021), which are robust to heterogeneity across cohorts. Additional estimators commonly used in staggered DiD designs are reported as robustness checks in the Appendix.

3.4 School-Level Evidence

The second empirical exercise exploits variation within counties, between schools and across cohorts. It evaluates the role of public schools in ENIA's impacts, builds on the school censuses data. I start by replicating the population results using a difference-in-difference estimator. I compare responses in 2019 and 2017 among students in treated and control counties. Then, I exploit the fact that only public schools in treated counties were targeted by the school-based device of ENIA and estimate a triple differences model (DDD) through the following specification:

$$\begin{aligned} y_{asy} = \alpha + \beta_{DD}\mathbb{1}(Post_y \wedge Treat_{sy}) + \beta_{DDD}\mathbb{1}(Post_y \wedge Treat_{sy}) \times Public_{asy} \\ + \gamma_1 Public_{asy} \times Post_y + X'_{asy}\rho + \alpha_s + \delta_y + \varepsilon_{asy} \end{aligned} \tag{3}$$

where y_{asy} is the outcome variable of interest (i.a., parenthood) for student a in school s at year y. $Treat_a$ takes value of 1 if school a is in a treated county. $Public_{asy}$ indicates whether school s is public that is constant across both time periods by construction. $Post_y$ takes value of one in 2019 and 0 in 2017. X is a set of controls that includes gender, mother's education and the student and mothers' nationality. α_s and δ_y are school and year fixed effects, respectively. The parameters of interest are β_{DD} and β_{DDD} that capture the DD and triple differences impacts. While β_{DD} requires a parallel trends assumption to identify the causal impacts, β_{DDD} does not. It assumes that in the absence of the treatment the evolution of parenthood in public schools relative to private would behave in a similar manner across treated and control counties.

Sample criteria. For the purpose of this study, to estimate a triple difference estimator, I restricted the sample in three ways. First, I kept schools present in both 2017 and 2019 in the same public or private offering.[14] Second, I excluded counties that lack schools in both private and public sector. Finally, I restricted the sample to urban schools due to limitations of the data, that is, rural schools were easier to be identified and I was not provided with information on county for most of them.

4 The Effects of ENIA on Entry into Teenage Motherhood

ENIA leads to a sharp and sustained decline in entry into teenage motherhood. Figure 3 presents event-study estimates of first births per 1,000 women aged 15–19. The pre-treatment (lead) coefficients are small and statistically indistinguishable from zero, and I fail to reject their joint equality to zero (p-value = 0.736), consistent with parallel trends between treated counties and

[14]There is a reduction of only 570 students ($< 1\%$) in schools that 'switched' from public to private, or vice versa.

their imputed counterfactual outcomes. Following implementation, the first-birth rate begins to decline approximately three quarters later—consistent with the biological lag between conception and birth—and quickly stabilizes at approximately 5 fewer births per 1,000 females relative to the counterfactual. These estimates correspond to a reduction of approximately 10.6 percent relative to the pre-treatment mean of 47.25 in treated counties.

This timing suggests that the program reduced fertility by preventing pregnancies rather than affecting outcomes conditional on conception, such as abortion or miscarriage. The absence of immediate effects and the delayed response further rule out explanations based on contemporaneous shocks affecting fertility behavior. This pattern is consistent with the view that adolescent fertility is constrained by barriers to effective contraceptive access, and that reducing these frictions lowers the likelihood of early entry into motherhood.

The dynamic estimates also provide insight into the magnitude of steady-state effects. The event-study estimates indicate that the decline stabilizes at approximately 5 to 6 fewer first births per 1,000 females ages 15–19 relative to the counterfactual path. This magnitude is somewhat larger than the average effects reported in Table 1, reflecting that the latter average over early post-treatment periods when exposure is still partial. As a result, the estimates in Table 1 should be interpreted as intention-to-treat effects.

Magnitude and margins. Table 1 reports average treatment effects across specifications. In the preferred population-weighted specification (column 2), ENIA reduces the adolescent birth rate by 4.00 births per 1,000 females ages 15–19 (s.e. = 1.14), corresponding to a 7.3 percent decline relative to the pre-treatment mean of 55.13. Because this specification weights counties by baseline adolescent population, the estimate is interpreted as the effect for the average adolescent female rather than the average county. Translating this estimate into levels implies approximately 1,425 fewer adolescent births per year across treated counties (95% CI: [629, 2,222]).

The unweighted estimate in column (1) is larger in magnitude, at 5.43 births per 1,000 females (8.7 percent relative to the pre-treatment mean), indicating that the decline is stronger in smaller counties than in more populous ones. Estimates are similar when using the full sample (column 3) and when employing the Callaway and Sant'Anna estimator (column 4), confirming that the results are not driven by sample selection or estimator choice.

To understand the economic margin driving this response, I decompose the total effect into first births and higher-order births. The decline is primarily driven by first births, which fall by 3.88 births per 1,000 females (column 5), corresponding to an 8.2 percent reduction relative to the pre-treatment mean of 47.25. In contrast, higher-order births decline by 1.55 per 1,000 females (column 6), a 10.4 percent reduction relative to a lower baseline of 14.80, but the estimates are less precisely estimated, reflecting the lower baseline frequency of higher-order births in this age group, and are not consistently distinguishable from zero across specifications. While the point estimates

are negative, the evidence is less precise, and the results indicate that the program primarily reduced entry into motherhood.

The magnitude of the estimated effects is large relative to existing evidence on adolescent fertility interventions, which typically report reductions in the range of 3–6 percent for contraceptive supply or education programs (e.g., Mark and Wu (2022); Lindo and Packham (2017); Miller (2010)).

Interpretation as intention-to-treat. The estimates should be interpreted as intention-to-treat effects based on the timing of program rollout. Because implementation was heterogeneous across provinces and the post period includes early phases with gradual take-up, these estimates likely understate the effects among adolescents fully exposed to the school-based delivery model.

Age-specific patterns. A key concern is that unobserved aggregate shocks—such as macroeconomic fluctuations or broader secular changes in fertility preferences—coinciding with the ENIA rollout could explain the observed fertility decline. To address this, I exploit the targeted nature of the intervention. Because the program operated through high schools, its effects should be concentrated among adolescents and absent among older cohorts.

Figure 4 reports age-specific estimates. The decline in fertility is concentrated among women aged 15–19. I also observe smaller negative effects for women aged 20–21, a group that partially overlaps with recent exposure to the school-based delivery system. In contrast, estimates for older age groups (e.g., ages 23 and above, and particularly 25–29) are close to zero and statistically insignificant. The absence of effects among older women in the same treated counties provides evidence against explanations based on aggregate shocks or generalized changes in reproductive behavior. Consistent with this interpretation, the share of births to younger women declines alongside the level of adolescent fertility, indicating a shift in fertility behavior rather than aggregate demographic trends (Appendix Figure X).

Robustness. These patterns are robust across alternative specifications and samples. Estimates using the full set of counties yield similar magnitudes and dynamics, and results are stable across modern difference-in-differences estimators[15]. Additional results show limited and less precisely estimated effects on higher-order births, reinforcing that the program primarily operated through the margin of entry into motherhood.

Back-of-the-envelope calculations based on the average post-treatment effect imply about 1,425 averted births. Using the stabilized dynamic effects yields larger implied reductions.

[15]Appendix Figure A14 shows that these results are robust across alternative estimators commonly used in staggered difference-in-differences settings, including the methods proposed by Sun and Abraham (2021), Callaway and Sant'Anna (2021), and de Chaisemartin and d'Haultfoeuille (2020). Across all estimators, the estimated effects exhibit similar magnitudes and dynamic patterns.

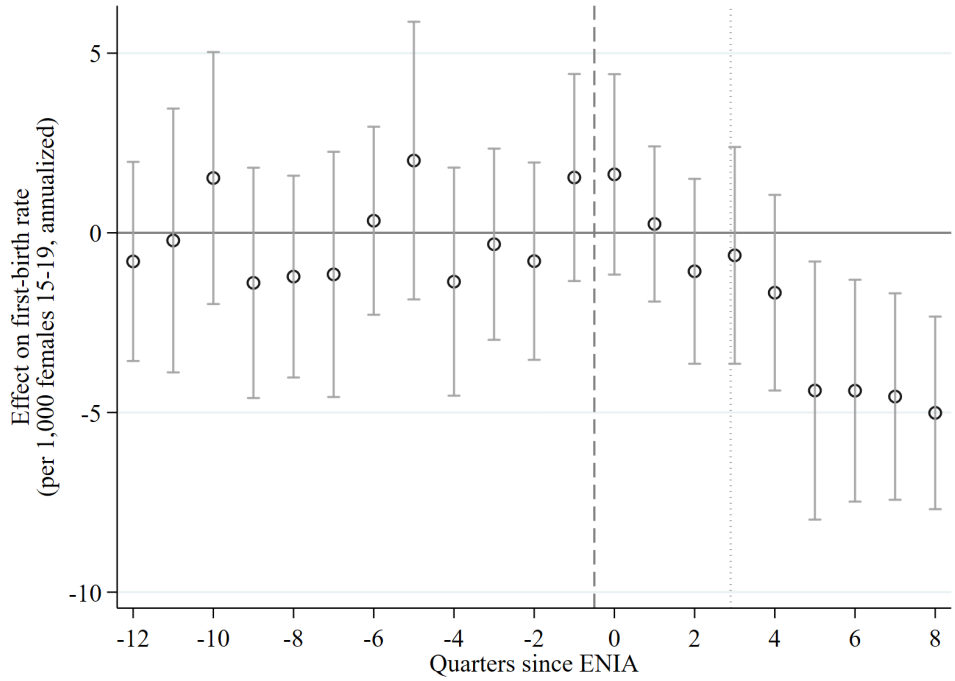


Figure 3: ENIA Effects on Teenage Motherhood

Notes: Event-study estimates of the impact of ENIA on teenage motherhood. The outcome variable is the entry into motherhood per 1,000 women aged 15–19 at the county–quarter level. Coefficients are estimated using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2024). Event time is measured in quarters relative to the start of ENIA implementation ($t = 0$). The dashed vertical line marks the beginning of the program, while the dotted line indicates the earliest period in which births can plausibly respond to the policy through pregnancy prevention (three quarters after implementation, corresponding to the median biological lag between conception and birth). The estimation sample is restricted to counties within the common support of treated counties based on adolescent population size (Appendix Figure A4 uses the full sample). Each point reports the estimated treatment effect at a given event-time horizon, and bars denote 95% confidence intervals with standard errors clustered at the county level.

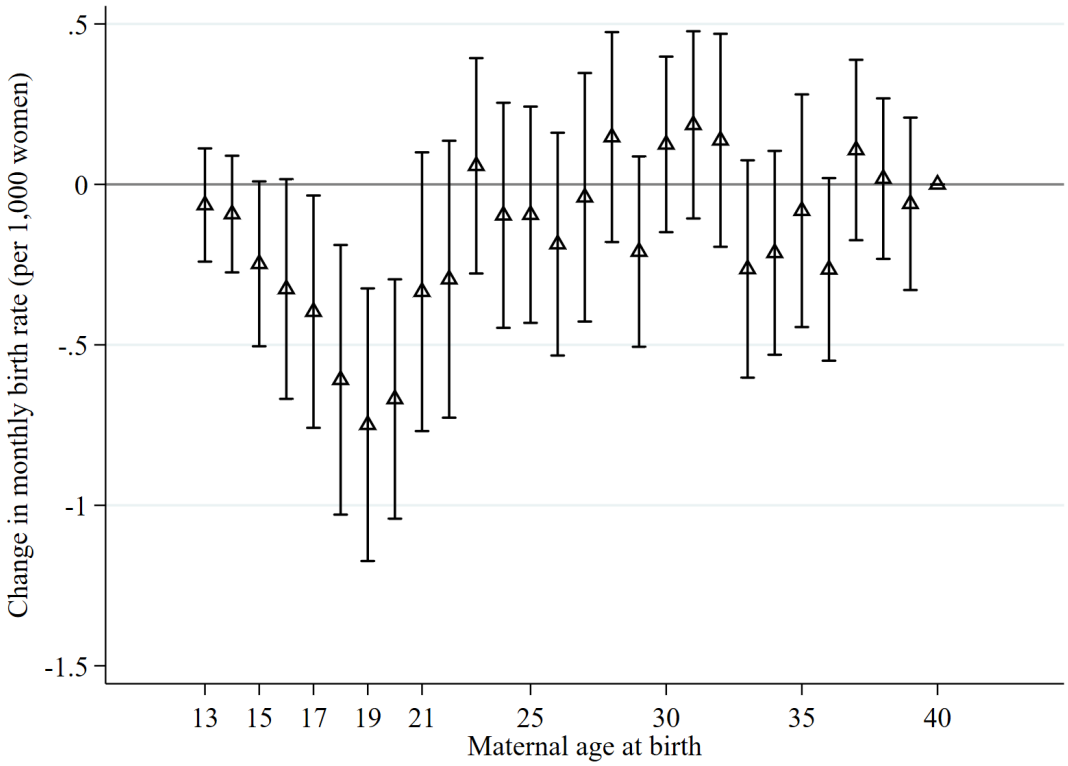


Figure 4: Age-Specific Effects of ENIA on Birth Rates

Notes: Difference-in-differences estimates of ENIA's effects on monthly birth rates by maternal age. The outcome variable is the birth rate per 1,000 women at the county–month level for each age. The sample is restricted to counties within the common support of treated counties based on adolescent population size (Appendix Figure A6 uses the full sample). The specification estimates age-specific treatment effects using a linear fixed-effects model that includes interactions between age, month, and county. The post-policy period begins nine months after the earliest ENIA implementation, reflecting the biological lag between conception and birth. Each point shows the estimated treatment effect for a given maternal age with 95% confidence intervals. Standard errors are clustered at the county level.

Table 1: ENIA Effects on Teenage Birth Rates

	Total Births				First Births	Higher-Order
	(1)	(2)	(3)	(4)	(5)	(6)
ENIA × Post	-5.428***	-4.003***	-4.198***	-3.868***	-3.883***	-1.545**
	(1.466)	(1.141)	(1.151)	(1.107)	(0.991)	(0.670)
Mean Dep. Var.	62.05	55.13	55.13	55.13	47.25	14.80
Observations	11480	11480	18410	11480	11480	11480
Counties (Clusters)	328	328	526	328	328	328
Estimator	BJS	BJS	BJS	CS	BJS	BJS
Common Support	Yes	Yes	Full	Yes	Yes	Yes
Pop. Weights (15–19)	No	Yes	Yes	Yes	No	No
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports difference-in-differences estimates of the effect of ENIA on adolescent fertility (births per 1,000 females ages 15–19). The treatment indicator equals one in periods three or more quarters after program implementation, accounting for the biological lag between conception and birth. Columns (1)–(3) and (5)–(6) use the imputation estimator of Borusyak, Jaravel, and Spiess (2024); Column (4) uses the Callaway and Sant'Anna (2021) estimator. The "Common Support" sample excludes counties outside the support of the treated population. Weighted columns use the baseline (2017) female population ages 15–19. The pre-treatment mean is computed among treated counties over event times -4 to -1. Standard errors are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5 Mechanisms: Delivery Architecture and Effective Contraceptive Access

The results in Section 4 show that ENIA generated a sharp decline in entry into teenage motherhood. In this section, I examine whether this response is consistent with a mechanism in which the program reduced access frictions to effective contraception through its school-based delivery model.

Because component-level monitoring data on counseling, sex education, and contraceptive provision are observed only in ENIA-treated counties, I do not attempt to separately identify the causal effect of each program component. Instead, I assess whether the program's internal dynamics are consistent with a coherent prevention chain: school-based counseling facilitates access to highly effective contraception, particularly subdermal implants; contraceptive uptake rises among adolescents; and, with a biological lag, conceptions and births decline.[16] These patterns closely match the program's theory of change, which explicitly aimed to reduce access frictions by linking confidential school-based counseling to the provision of highly effective contraception.

Targeted contraceptive uptake. I begin by examining whether ENIA expanded access to effective contraception for its intended population. Panel A of Figure 5 reports event-study estimates of the differential effect of ENIA on subdermal implant insertions among females aged 15–19 relative to

[16]This strategy is designed to document a coherent delivery chain rather than to causally decompose ENIA into separable components.

those aged 20–24 within treated counties. Prior to implementation, the age differential is small and statistically indistinguishable from zero. Following rollout, implant insertions rise sharply among adolescents relative to older women, with effects emerging immediately at implementation and persisting thereafter. Appendix Figures A10 and A16a provide complementary evidence on the supply side. They show that ENIA was accompanied by a sharp increase in the availability of LARCs—particularly subdermal implants—at both the provincial and county levels, with increases concentrated in the target adolescent population. These patterns support the interpretation that the program operated by expanding effective contraceptive access rather than merely shifting demand across existing supply.

This age-differential design differences out common shocks to contraceptive supply and provides evidence that ENIA disproportionately expanded effective access to highly effective contraception for adolescents. The immediacy of the response suggests that the relevant constraint was not information alone, but access to effective methods.

Timing and fertility responses. If expanded access to effective contraception is the primary mechanism, changes in contraceptive uptake should precede changes in fertility outcomes with biologically plausible timing. Panel B of Figure 5 examines this prediction by plotting event-study estimates of first-birth conceptions inferred from birth records using gestational timing.

The temporal sequence is sharp. Implant uptake increases immediately after ENIA rollout (Panel A), while conceptions begin to decline shortly thereafter, with effects becoming negative within one to two quarters. The decline in births documented in Section 4 follows with an additional delay consistent with gestation. This sequencing—uptake first, conceptions next, births last—aligns closely with the biological process of pregnancy formation and supports the view that the program operated through pregnancy prevention rather than changes in outcomes conditional on conception.

While these patterns are not by themselves sufficient to uniquely identify the mechanism, they are difficult to reconcile with alternative explanations that do not operate through pregnancy prevention, such as changes in reporting, compositional shifts, or contemporaneous shocks to fertility preferences.

School-based counseling as the gateway. To further characterize the program's internal delivery dynamics, Table 3 reports regressions of LARC adoption on contemporaneous and lagged measures of school-based counseling and comprehensive sex education within treated counties.

School-based counseling is strongly associated with subsequent increases in LARC uptake, particularly at short lags. In the preferred specification, the cumulative elasticity of LARC adoption with respect to counseling is approximately 0.13.[17]

[17]The cumulative elasticity corresponds to the sum of contemporaneous and one-period lagged coefficients. Results are robust to including counties with incomplete implementation of the school-based components (e.g., Formosa and San Isidro); see Appendix Table A5.

In contrast, once counseling is accounted for, the association with broader curricular exposure is substantially smaller, yielding cumulative effect close to zero (0.02) and statistically indistinguishable from zero. These patterns are consistent with counseling acting as the primary gateway into the health system by reducing confidentiality and access frictions, whereas information provision alone appears insufficient to generate comparable changes in contraceptive behavior in the short run.

Scale of exposure. The magnitude of ENIA's effects is consistent with the scale at which the program expanded access. Table 2 shows that, during the first full year of implementation, subdermal implant insertions averaged 88.9 per 1,000 females aged 15–19, with some counties exceeding 200 per 1,000. This corresponds to approximately 9 percent of adolescent females receiving implants on average, with substantially higher coverage in the upper tail.

At the same time, school-based counseling reached over 300 consultations per 1,000 adolescent females on average. These figures indicate that ENIA operated at a scale sufficient to affect a nontrivial share of the at-risk population, consistent with the first-order reductions in adolescent fertility documented in the main results.

School-based delivery and within-county evidence. Finally, I examine whether ENIA's school-based delivery channel amplified its effects using individual-level data from Argentina's school census (*Aprender*). Because *Aprender* is administered in schools, outcomes are observed only for students who remain enrolled at the relevant age and grade. Estimates therefore identify the effect of ENIA on adolescent parenthood among in-school individuals, rather than in the full population.[18]

This distinction is critical because adolescent motherhood is strongly associated with school dropout. If ENIA reduces pregnancies and/or increases school retention among higher-risk adolescents, the treated in-school sample may become more negatively selected, mechanically attenuating the estimated decline in parenthood.[19]

Table 4 reports difference-in-differences and triple-differences (DDD) estimates comparing changes in adolescent parenthood across cohorts, counties, and school sectors. Because ENIA's counseling and referral components were implemented only in public schools, private schools within treated counties provide a within-county comparison group.

The estimates reveal a reduction in self-reported parenthood of approximately 0.3 percentage points relative to a baseline of 2.99 percent, corresponding to a decline of roughly 10 percent. This magnitude closely matches the reduction in adolescent birth rates estimated using administrative vital statistics, despite relying on a distinct data source and outcome definition. This convergence across independent data sources provides complementary evidence on the role of the school-based delivery channel.

[18]These estimates should be interpreted as effects on in-school adolescents, a policy-relevant group given that ENIA's counseling and referral components operate exclusively through schools.

[19]Under this interpretation, the Aprender estimates are likely conservative relative to the effect among school-attending adolescents.

Supporting evidence: intensity and dose–response. Figure 6 provides additional descriptive evidence by relating county-level increases in LARC supply at program onset to subsequent changes in adolescent birth rates. Counties experiencing larger spikes in LARC provision tend to exhibit larger fertility declines. While this relationship is not causal and may reflect differences in implementation intensity or underlying demand, it is consistent with a dose–response pattern linking expanded access to effective contraception to reductions in adolescent fertility.

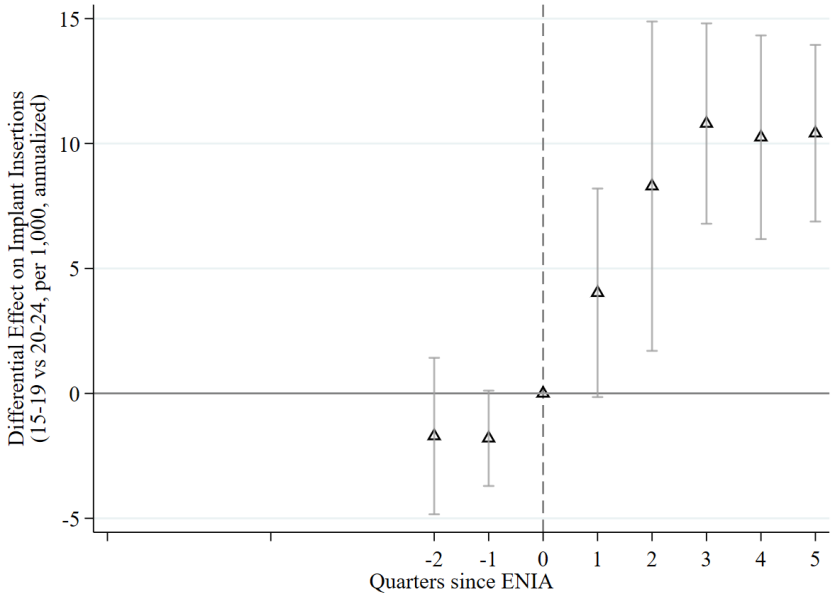
Interpretation. Taken together, the evidence supports a mechanism in which ENIA reduced adolescent fertility by reconfiguring access to reproductive health services. By embedding confidential counseling within schools and creating direct referral pathways to providers, the program appears to have relaxed binding access constraints that previously limited adolescents' ability to use highly effective contraception.

Rather than operating primarily through information provision alone, ENIA's effectiveness appears to derive from its delivery architecture: bringing confidential access to effective contraception into the school environment and lowering the non-monetary costs of use. More broadly, the findings highlight how institutional design can determine whether formal reproductive rights translate into effective contraceptive access in practice. More generally, they suggest that delivery models of this kind may be particularly consequential in settings where adolescents face substantial barriers to accessing reproductive health services despite formal legal availability.

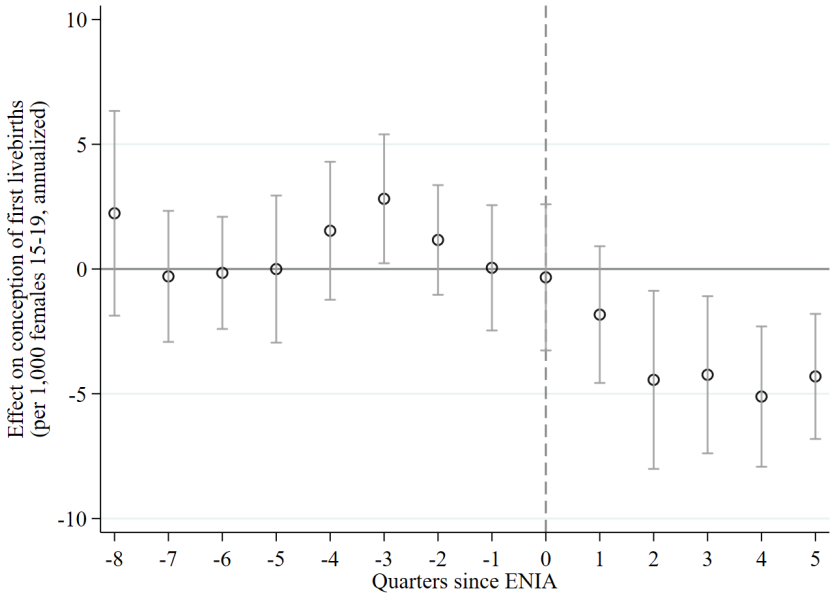
Table 2: ENIA Program Reach and Contraceptive Uptake (Full Implementation, 2019)

Outcome	**Mean**	**SD**	**P25**	**Median**	**P75**	**Max**
Panel A. Contraceptive Access (annual totals per 1,000 females ages 15–19)						
Subdermal implants	88.9	39.4	65.8	80.3	98.3	225.7
IUDs	1.5	1.6	0.2	0.8	2.0	5.9
LARCs	90.3	39.5	66.6	82.8	103.4	225.7
Females 15–19 receiving implants (%)	8.9	3.9	6.6	8.0	9.8	22.6
Panel B. Program Reach (annual totals per 1,000 females ages 15–19)						
School-based counseling	303.1	199.1	136.1	263.0	426.5	760.0
Comprehensive sex education	460.7	329.2	230.7	360.0	643.8	1730.0

Notes: This table summarizes ENIA program intensity across treated counties during 2019, the first full year of implementation. The sample is restricted to ENIA-treated counties and excludes Formosa and San Isidro, where the school-based delivery model was not implemented as designed. For each county, I compute the total monthly flow of each activity during 2019 and express it per 1,000 females ages 15–19. Contraceptive access measures include subdermal implants, intrauterine devices (IUDs), and total long-acting reversible contraceptives (LARCs). Program reach measures include school-based counseling and comprehensive sex education exposure. Because counseling and sex education activities may include some individuals outside ages 15–19 or males, these rows should be interpreted as normalized indicators of program intensity relative to the adolescent female population, rather than as literal participation rates for females ages 15–19 alone.



(a) Implant Insertions (15-19 vs 20-24)



(b) First-Birth Conceptions (ages 15-19 at birth)

Figure 5: Timing of ENIA Effects on Conceptions and Births

Notes: Panel A reports event-study estimates of the differential effect of ENIA on subdermal implant insertions among females aged 15–19 relative to those aged 20–24 within treated counties (2018–2019). Panel B reports event-study estimates of ENIA's effect on conceptions of live births to mothers aged 15–19, constructed using gestational timing. The outcome measures conceptions of births to mothers aged 15–19 at the time of birth. Event time is measured in quarters relative to program onset and normalized to the pre-treatment period; the vertical dashed line indicates implementation. The sample is restricted to counties within common support based on adolescent population size. Formosa is excluded, as it did not implement the full ENIA delivery model. 95% confidence intervals are shown with standard errors clustered at the county level.

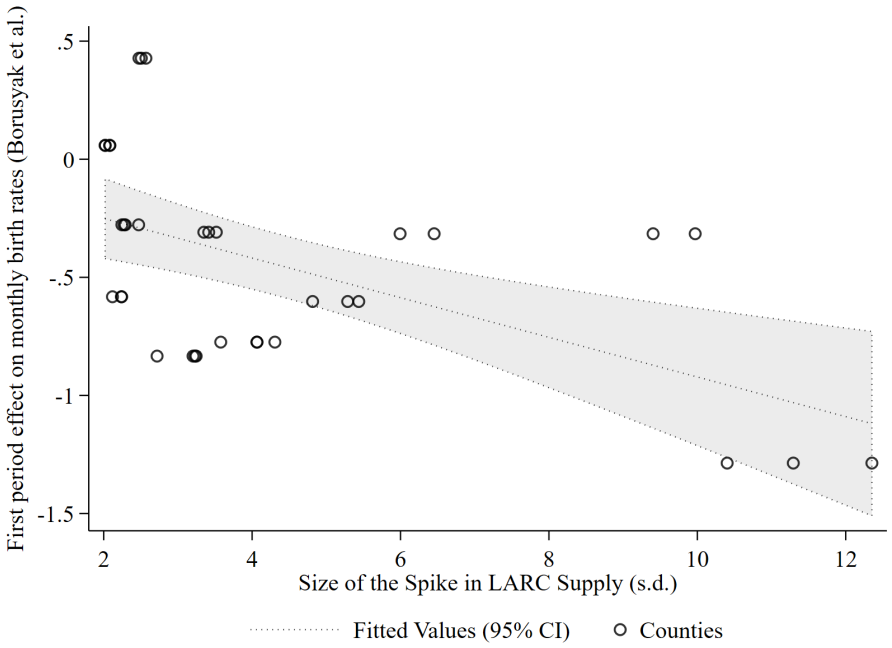


Figure 6: LARC Supply Expansion and Fertility Effects

Notes: The figure plots county-level estimates of ENIA's effect on adolescent birth rates against the magnitude of the increase in LARC supply during program implementation. The supply shock is defined as the first month in which LARC deliveries exceed the county-specific baseline by more than two standard deviations. Treatment effects correspond to the first post-treatment period (τ_0) estimated using the imputation estimator of Borusyak, Jaravel, and Spiess (2024). Each point corresponds to a county. The fitted line and shaded region represent predicted values and 95% confidence intervals. It captures the relationship between county-level increases in LARC provision at program onset and immediate subsequent changes in adolescent birth rates.

Table 3: School-Based Counseling and the Uptake of LARCs

	(1)	(2)	(3)
	Dependent variable: $\log(LARC_{it})$		
School-Based Counseling (SBC)			
log(SBC_t)	0.180***	0.094**	0.043
	(0.045)	(0.042)	(0.034)
log(SBC_t-1)	0.108**	0.128***	0.087***
	(0.043)	(0.041)	(0.032)
Comprehensive Sex Ed. (CSE)			
log(CSE_t)	0.061***	0.047**	0.023
	(0.018)	(0.020)	(0.016)
log(CSE_t-1)	0.033**	0.017	-0.010
	(0.016)	(0.016)	(0.015)
Cumulative Effects ($\beta_t + \beta_{t-1}$)			
Sum: SBC	0.288	0.221	0.130
Sum: CSE	0.094	0.064	0.013
Obs.	759	759	759
R2	0.524	0.621	0.776
Time FE	N	Y	Y
County FE	N	N	Y

Notes: This table reports regressions of LARC adoption on contemporaneous and lagged measures of ENIA program components within treated counties during 2018–2019. The dependent variable is the log number of LARC insertions per 1,000 females aged 15–19. Explanatory variables include school-based counseling (SBC) and comprehensive sex education (CSE), both expressed in logs. The sample excludes Formosa and San Isidro, where the school-based delivery model was not implemented as designed. Results including all treated counties are qualitatively similar but less precisely estimated. All specifications include time and/or county fixed effects as indicated. Standard errors are clustered at the county level. The bottom panel reports the sum of coefficients at time t and $t-1$. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: School-Based Delivery and Adolescent Parenthood: Triple-Differences Estimates

	(1)	(2)	(3)	(4)	(5)
(β_{DD}) $\mathbb{1}\{ENIA \times POST_{it}\}$	-0.004***	-0.004***	0.000	-0.001	-0.002**
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
(β_{DDD}) $\mathbb{1}\{ENIA \times POST_{it}\}$ × Public			-0.008***	-0.005***	-0.003**
			(0.002)	(0.001)	(0.002)
$\mathbb{1}\{POST\}$	-0.008***	-0.004***	-0.008***	-0.004***	-0.003***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Female		-0.001*		-0.001*	-0.001*
		(0.001)		(0.001)	(0.001)
Mother Secondary Education		-0.001**		-0.001**	-0.001**
		(0.001)		(0.001)	(0.001)
Mother Higher Education		-0.002***		-0.002***	-0.002***
		(0.001)		(0.001)	(0.001)
Control Y-mean (pre-period)					
All	.0299	.0154	.0299	.0154	.0154
Public			.0432	.0236	.0236
Private			.0135	.0097	.0097
R2	0.057	0.052	0.057	0.052	0.052
N. of obs.	521,907	300,905	521,907	300,905	300,905
N. of clusters	419	182	419	182	182
School FE	Y	Y	Y	Y	Y
Restricted sample		Y		Y	Y
Individual controls		Y		Y	Y
Public × Post					Y

Notes: This table reports difference-in-differences and triple-differences estimates using individual-level data from Argentina's school census complementary questionnaire (Aprender) for 2017 and 2019. The dependent variable is an indicator for whether the student reports having children. The triple-differences specification compares changes over time across cohorts, counties, and school types (public vs. private). Because ENIA's counseling and referral components were implemented in public schools, private schools within treated counties provide a within-county comparison group. All specifications include school and year fixed effects. Standard errors are clustered at the county level. Estimates indicate larger reductions in adolescent parenthood in public schools relative to private schools within treated counties, consistent with the importance of the school-based delivery channel.

6 Social Norms and Access Frictions

In this section, I examine whether ENIA's effects varied with local social norms regarding adolescent sexual and reproductive autonomy. Social norms can raise the reputational cost of adolescent sexual activity (Bénabou & Tirole, 2011) and induce local gatekeepers—such as teachers or healthcare providers—to restrict access to contraception. As a result, adolescents may face substantial non-monetary barriers to accessing reproductive services, even when these are formally available.

Measurement of the normative environment.

To capture local variation in these constraints, I construct an index of reproductive conservatism combining two sources. First, I use province-level voting behavior during the 2018 congressional debate on the Voluntary Interruption of Pregnancy Bill (IVE). Legislative voting on this moral issue exhibited substantial within-party heterogeneity and strong geographic clustering, making it a useful proxy for underlying constituency preferences rather than strict party alignment.[20]

Second, I complement this proxy with a hand-collected database of municipalities and provinces that publicly declared themselves as "pro-life." Because these formal declarations intensified during the abortion debate and may partly reflect contemporaneous political mobilization, I treat this measure as capturing active local conservative gatekeeping rather than a purely pre-treatment baseline of preferences.

I combine these two measures into a standardized index of reproductive conservatism and classify counties into terciles. This grouping allows for a flexible, non-parametric characterization of heterogeneous effects across different normative environments.

Heterogeneous effects.

I estimate heterogeneous treatment effects by interacting ENIA exposure with the terciles of the conservatism index. Figure 7 summarizes the results.

The pattern is non-monotonic. The estimated effect on adolescent fertility is small and statistically indistinguishable from zero in the least conservative tercile, while it becomes larger in magnitude in the remaining, more conservative groups. This pattern is inconsistent with mechanisms based on uniform shifts in demand or preferences, and instead points to margins that are more likely to be binding in socially restrictive environments.

This heterogeneity directly tests the mechanism proposed in Section 5. If ENIA operated by relaxing access frictions through confidential, school-based delivery, its effects should be larger in settings where adolescents face stronger informal barriers to accessing reproductive health services.

While these patterns do not isolate the role of social norms from all correlated local characteris-

[20] Appendix Figure A18 documents the distribution of legislative votes across provinces, showing substantial geographic variation. The voting-based measure is also positively correlated with alternative proxies of local conservatism, including self-declared pro-life municipalities and religiosity measures from the INADI survey (2013).

tics, they are difficult to reconcile with explanations based solely on aggregate shocks or uniform changes in fertility preferences.

Taken together, these results suggest that the effectiveness of reproductive health policies depends critically on their ability to relax access frictions created by local social constraints. Programs that operate through institutional channels capable of bypassing gatekeeping—such as school-based delivery—may be particularly effective in environments where formal rights do not translate into effective access in practice.

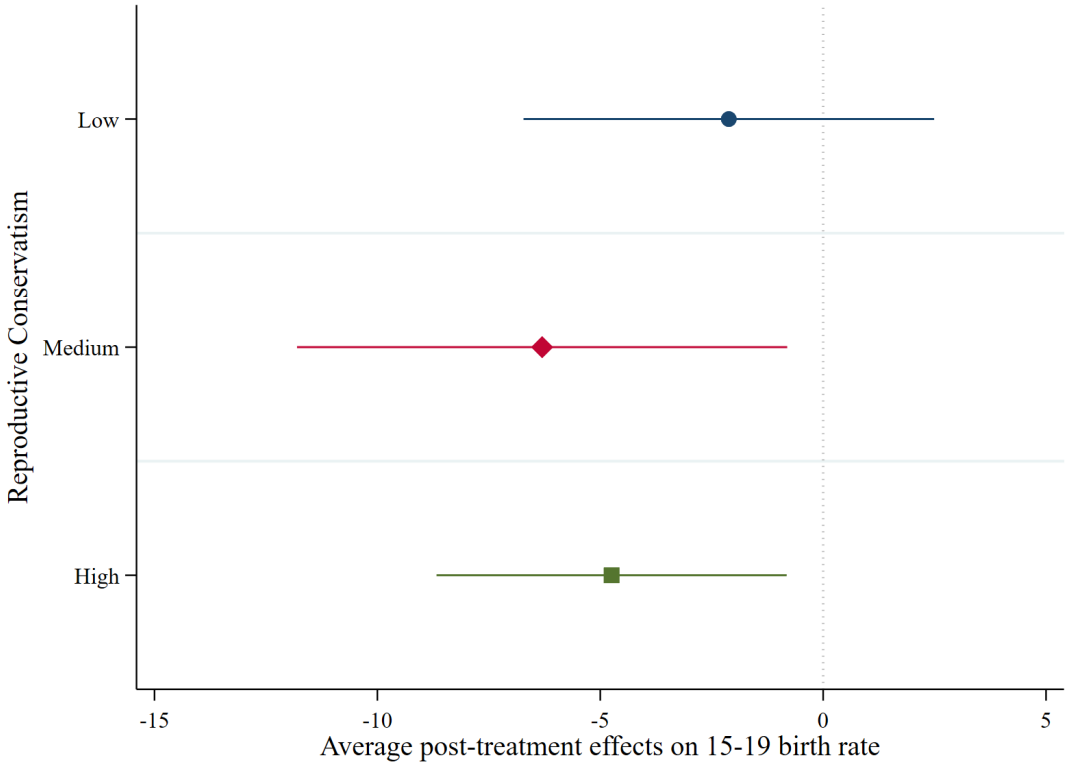


Figure 7: Heterogeneous Effects by Reproductive Conservatism Terciles

Notes: The figure reports average post-treatment effects of ENIA on adolescent birth rates (per 1,000 females aged 15–19), estimated separately by terciles of reproductive conservatism. Estimates correspond to the contemporaneous treatment effect (τ_0) from the imputation estimator of Borusyak, Jaravel, and Spiess (2024), using the same specification as Column (1) of Table 1. Reproductive conservatism is measured using a pre-program index combining legislators' votes against the Voluntary Interruption of Pregnancy Bill (IVE) and an indicator for pro-life localities. Terciles are defined based on the distribution of the conservatism index among ENIA-treated counties, and the resulting cutoffs are then applied to the full estimation sample. As a result, treated counties are evenly distributed across terciles, while the number of observations differs across groups due to variation in the distribution of control counties. The estimation sample includes 7,070 observations in the lowest tercile and 2,205 observations in each of the medium and high terciles. Point estimates are -2.11, -6.30, and -4.74 births per 1,000 females aged 15–19 for the low, medium, and high conservatism terciles, respectively. Vertical lines denote 95% confidence intervals.

Table 5: Impacts by Congress Votes Against Abortion and Pro-life Cities

	(1)	(2)	(3)
	Y=Birth rates (females aged 15-19)		
Z:	Senators (votes against IVE)	Senators and Deputies (quartiles against IVE)	Pro-life
$\mathbb{1}\{ENIA \times POST\} \wedge Z=0$	0.000		
	(.)		
$\mathbb{1}\{ENIA \times POST\} \wedge Z=1$	-0.515	4.254**	
	(2.765)	(2.153)	
$\mathbb{1}\{ENIA \times POST\} \wedge Z=2$	-7.692***	-7.726***	
	(2.799)	(2.542)	
$\mathbb{1}\{ENIA \times POST\} \wedge Z=3$	-4.558**	-6.278**	
	(2.105)	(2.105)	
$\mathbb{1}\{ENIA \times POST\} \wedge Z=4$		-4.541**	
		(2.105)	
$\mathbb{1}\{ENIA \times POST\}$			-5.308**
			(2.575)
$\mathbb{1}\{ENIA \times POST\} \times Pro\text{-}life=1$			-1.687
			(3.392)
Avg. Nightlight Intensity	-6.511***	-4.364***	-5.142***
	(1.570)	(1.511)	(1.510)
N. of obs.	18,375	18,375	18,375
Counties	525	525	525
Y-variable Mean	70.79	70.79	70.79
R2	0.63	0.64	0.63
County× FE	Y	Y	Y
Province Seasonality	Y	Y	Y
Time × Category FE	Y	Y	Y

Notes: All models have birth rates (per 1,000 females aged 15-19) as the dependent variable and follow Equation 1. Column (1) uses the intersection of treatment and number of senators' votes against the Voluntary Interruption of Pregnancy Bill as independent variables. Column (2) presents the share of votes against IVE, averaged across deputies and senators and split into quartiles, with each intersection with treatment presented. Column (3) interacts treatment and a dummy variable for a county having a self-proclaimed pro-life urban settlement. Robust standard clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7 Human Capital Consequences of Reduced Adolescent Fertility

The previous sections show that ENIA substantially reduced adolescent fertility. A natural next question is whether these fertility declines translated into improvements in young women's human capital. Early childbearing is strongly associated with school dropout and lower educational attainment, but identifying the causal effect of fertility timing is challenging because adolescents who become mothers early differ systematically from those who delay childbearing. [21]

To examine medium-run outcomes, I use tabulations from the 2022 Argentine Population Census. The analysis is conducted at the county × age × gender level and focuses on cohorts who were adolescents during ENIA's rollout and are observed several years later, when secondary-school completion can be measured. The main outcome is an indicator for completed secondary school; years of education are used as a complementary continuous measure.

Because ENIA was followed by major shocks—most notably COVID-19 school closures and other region-specific disruptions—simple comparisons across places or cohorts are unlikely to isolate the effect of delayed fertility. I therefore estimate a triple-difference design exploiting variation across counties, cohorts, and gender:

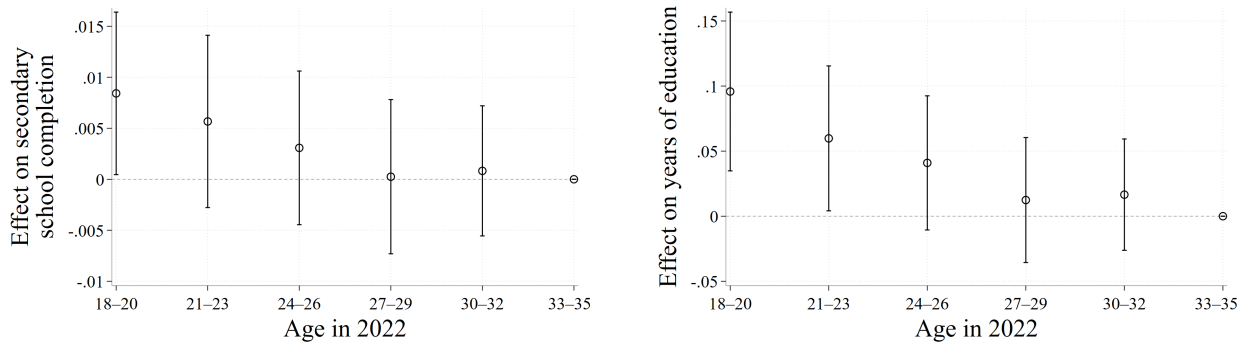
$$Y_{cag} = \beta \left(ENIA_c \times Exposed_a \times Female_g \right) + \lambda_{ca} + \lambda_{cg} + \lambda_{ag} + \varepsilon_{cag}, \tag{4}$$

where Y_{cag} is the education outcome in county c, cohort a, and gender g; $ENIA_c$ indicates counties exposed to the program; and $Exposed_a$ identifies cohorts aged 14–19 during ENIA's rollout in 2018. The fixed effects absorb county-by-cohort, county-by-gender, and cohort-by-gender differences. The identifying assumption is that, absent ENIA, contemporaneous shocks would have affected girls and boys similarly within county-cohort cells. Standard errors are clustered at the county level.

Figure 8 shows that the estimated education gains are concentrated among cohorts exposed to ENIA during adolescence. For older placebo cohorts, the estimated gender gaps between treated and control counties are small and statistically indistinguishable from zero. The ATT estimated indicates an increased in secondary school completion among exposed women of 0.54 percentage points relative to men in the same cohorts and counties. Years of education move in the same direction.

Figure 10 presents the county-level relationship between fertility reduction and human capital. Counties where ENIA generated larger fertility declines also tend to exhibit larger education gains. The relationship is broadly consistent with a positive association between fertility reductions and education gains, consistent with delayed childbearing being an important mechanism behind secondary education completion.

[21] A large literature documents substantial education and labor-market penalties of childbearing for women, with much smaller effects for men; see, for example, Bronars and Grogger (1994) and Kleven, Landais, and Søgaard (2019).



(a) Secondary school completion

(b) Years of education

Figure 8: Human capital effects by exposure cohort.

Notes: Estimates of ENIA's effect on human capital using the 2022 Argentine Population Census. Each point reports the coefficient on the interaction between county ENIA exposure, female, and cohort from a triple-difference specification estimated on county × age × gender Census-2022 cells. Individuals aged 18-23 in 2022 were adolescents during ENIA's rollout with varying intensity. Regressions are weighted by the number of census respondents with valid education information in each cell and include county × age, county × gender, and age × gender fixed effects. Standard errors are clustered at the county level.

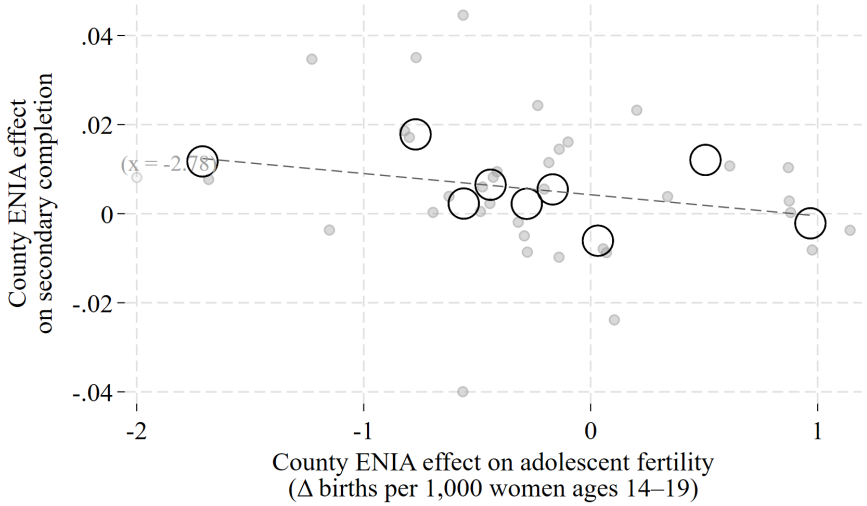


Figure 10: Counties with larger fertility declines exhibit larger education gains.

Notes: Light gray markers show county-specific estimates for the 36 treated counties. Dark markers show means within bins of counties ranked by the magnitude of the ENIA fertility effect. County fertility effects correspond to the county-specific post-ENIA change in monthly births per 1,000 females ages 15–19 estimated from the birth-rate panel. County education effects correspond to the county-specific triple-difference coefficient for secondary-school completion estimated from Census-2022 county × age × gender cells. The horizontal axis is reported so that larger values denote larger fertility declines.

A simple back-of-the-envelope calculation, combining the fertility and education estimates indicates a decline of 10 births per 1,000 adolescents corresponds to roughly a 1 percentage point increase in secondary-school completion. These estimates should be interpreted as medium-run intent-to-treat effects of ENIA exposure.

8 Conclusion

This paper studies whether redesigning delivery architecture can translate formal reproductive rights into effective access for adolescents. Exploiting the staggered rollout of Argentina's ENIA program, I find that a school-anchored reform combining confidential counseling with facilitated access to long-acting reversible contraceptives reduced adolescent birth rates by around 7 to 10 percent, with effects emerging at the biologically plausible nine-month horizon. The decline is primarily driven by reductions in first births, indicating lower entry into teenage motherhood.

Multiple pieces of evidence are consistent with improved access to highly effective contraception as the main channel. Fertility declines coincide with sharp increases in LARC provision, particularly subdermal implants, and are closely aligned with changes in contraceptive uptake. Within treated counties, school-based counseling is the strongest predictor of subsequent LARC adoption, consistent with counseling functioning as a confidential gateway into the health system in a setting where provider discretion and stigma can raise the effective cost of access.

The effects are larger in more conservative areas, suggesting that the program is particularly effective where access frictions are more likely to be binding. This pattern highlights the role of local social norms in shaping the effectiveness of reproductive health policies.

I validate the fertility declines using school census data, which show similar reductions in self-reported parenthood and larger impacts in public schools where the counseling component was implemented. Using tabulations from the 2022 Population Census, I also find suggestive evidence of modest gains in secondary completion among exposed cohorts, though these estimates should be interpreted as medium-run intent-to-treat effects rather than structural returns to delaying fertility.

These findings point to a broader lesson: expanding formal access to reproductive health services may be insufficient if nonprice barriers and gatekeeping constraints limit effective use. Policies that reduce these frictions through integrated and accessible delivery platforms—such as school-based entry points—can substantially increase the take-up of existing rights. An important direction for future research is to examine longer-run human capital and labor market effects as exposed cohorts age.

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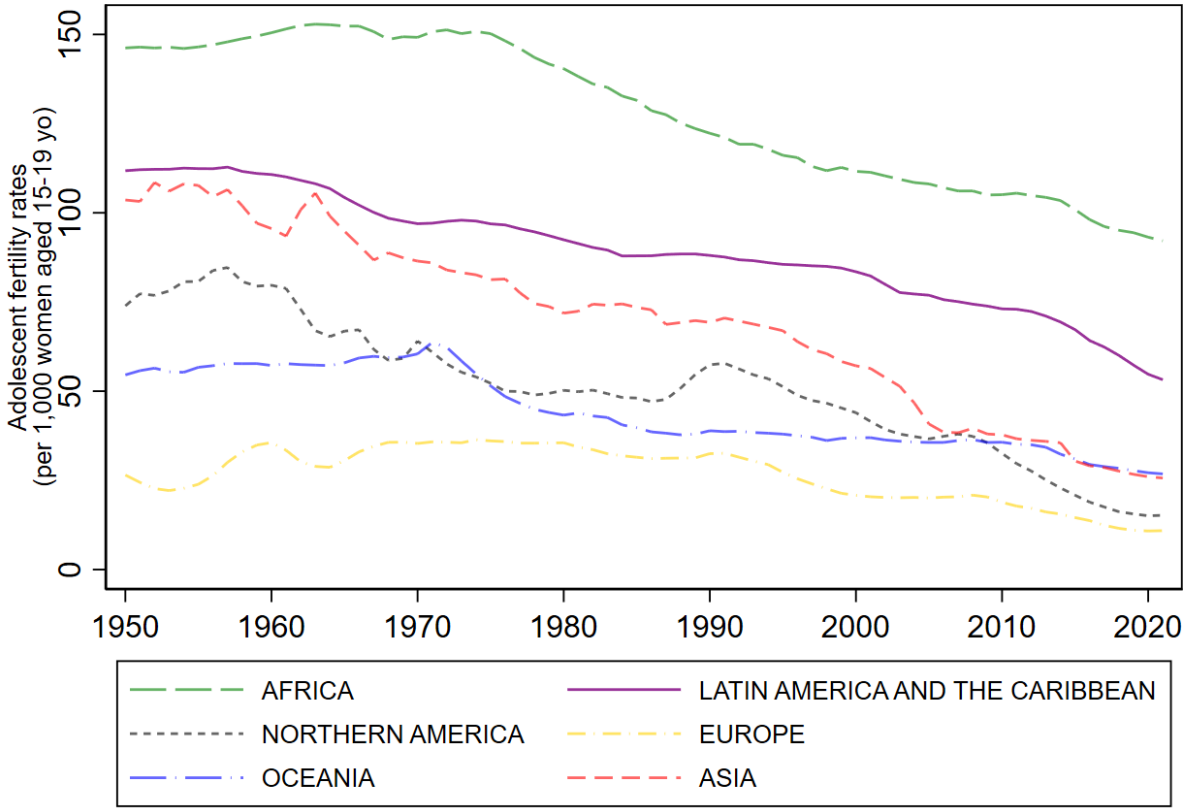
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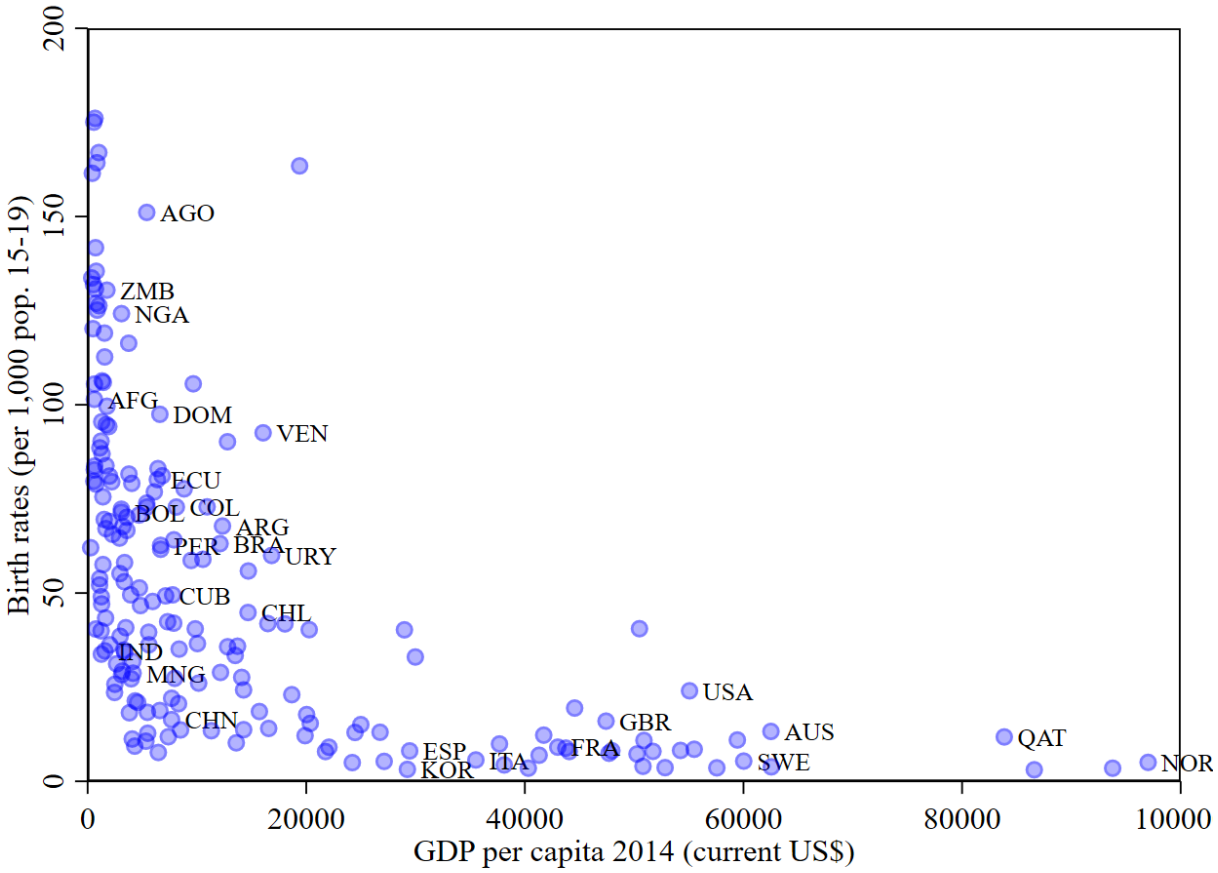
A Appendix

Figure A1: Adolescent Fertility Trends by Continent



Own calculations from World Population Prospects 2022, United Nations.

Figure A2: Adolescent Birth Rates and GDP at the Country-level



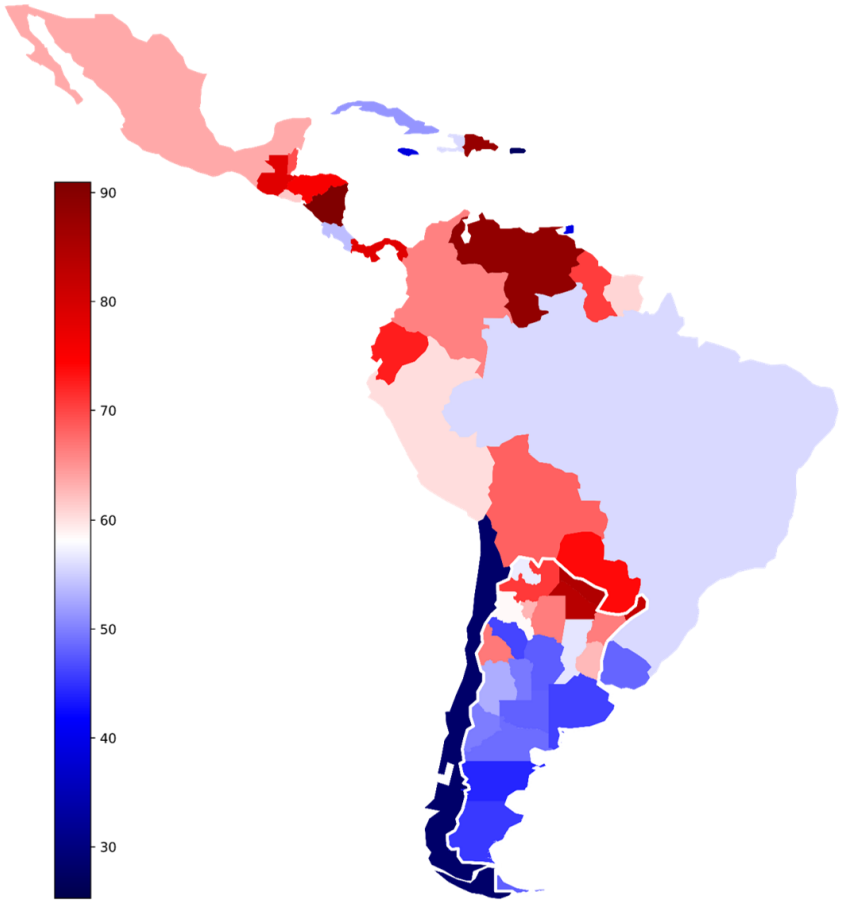


Figure A3: Adolescent Fertility: Argentine Provinces and Latin American Countries

Notes: Births per 1,000 women aged 15 to 19 during 2017. The map uses a color scale ranging from low fertility (30) to high fertility (90). Source: Own calculations based on the United States World Population Prospects database and official birth records from the Argentine Ministry of Health.

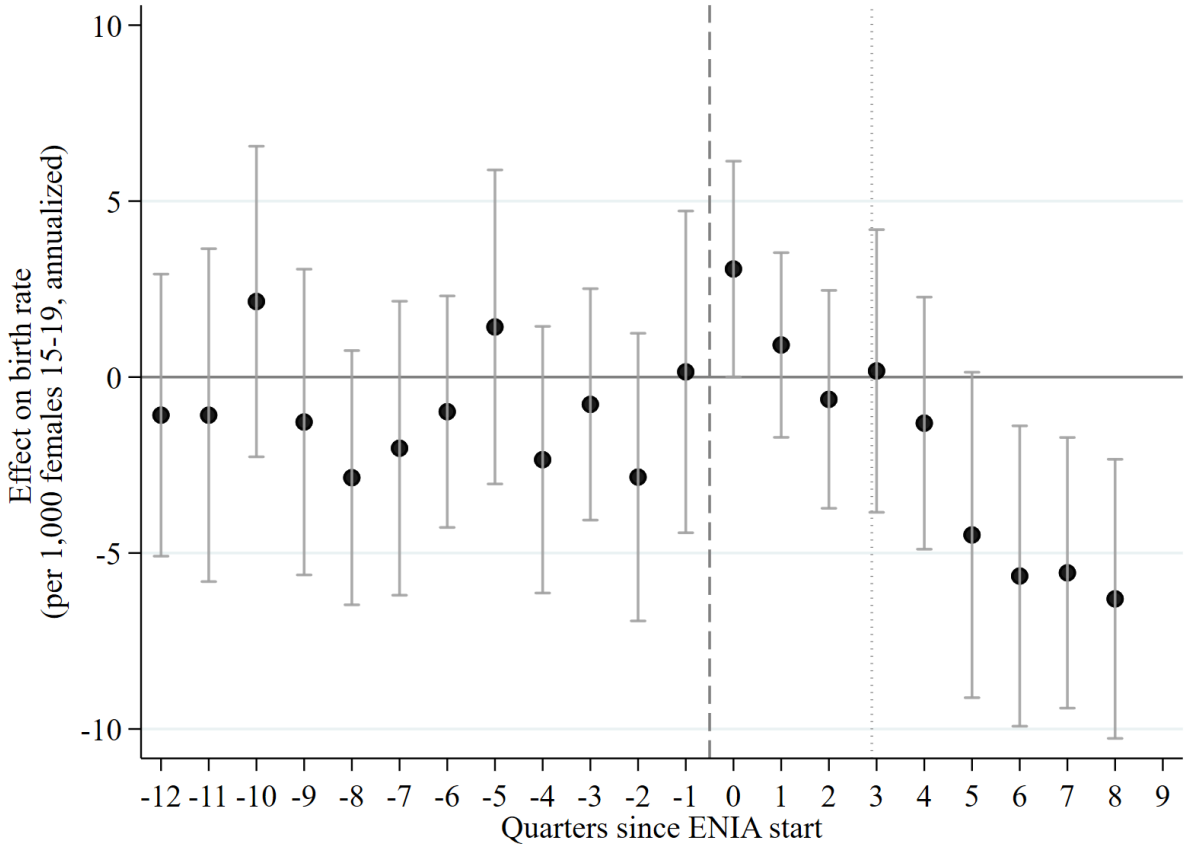


Figure A4: Event-Study Estimates Using the Full Sample of Counties

Notes: Event-study estimates of the impact of ENIA on adolescent birth rates using the full sample of counties. The dashed series corresponds to estimates using all counties, while the solid series reproduces estimates for the common-support sample used in the main analysis. Estimates are obtained using the imputation estimator of Borusyak, Jaravel, and Spiess (2024). The outcome variable is the birth rate per 1,000 women aged 15–19 at the county–quarter level. Bars denote 95% confidence intervals clustered at the county level.

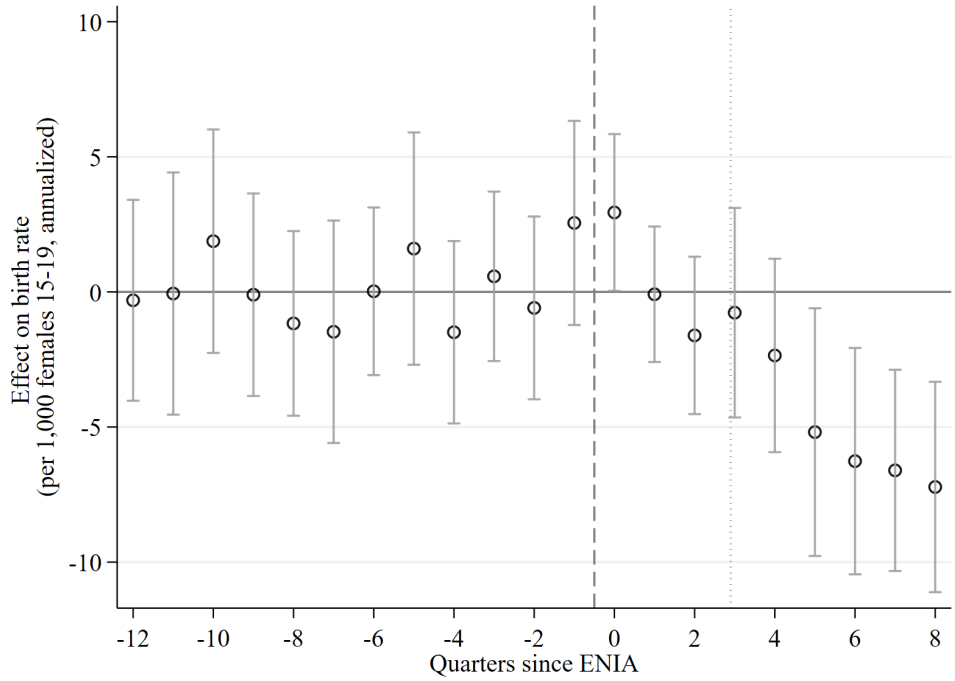


Figure A5: ENIA Effects on Births Rates (15-19)

Notes: Event-study estimates of the impact of ENIA on adolescent birth rates. The outcome variable is the birth rate per 1,000 women aged 15–19 at the county–quarter level. Coefficients are estimated using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2024). Event time is measured in quarters relative to the start of ENIA implementation ($t = 0$). The dashed vertical line marks the beginning of the program, while the dotted line indicates the earliest period in which births can plausibly respond to the policy through pregnancy prevention (three quarters after implementation, corresponding to the median biological lag between conception and birth). The estimation sample is restricted to counties within the common support of treated counties based on adolescent population size (Appendix Figure A4 uses the full sample). Each point reports the estimated treatment effect at a given event-time horizon, and bars denote 95% confidence intervals with standard errors clustered at the county level.

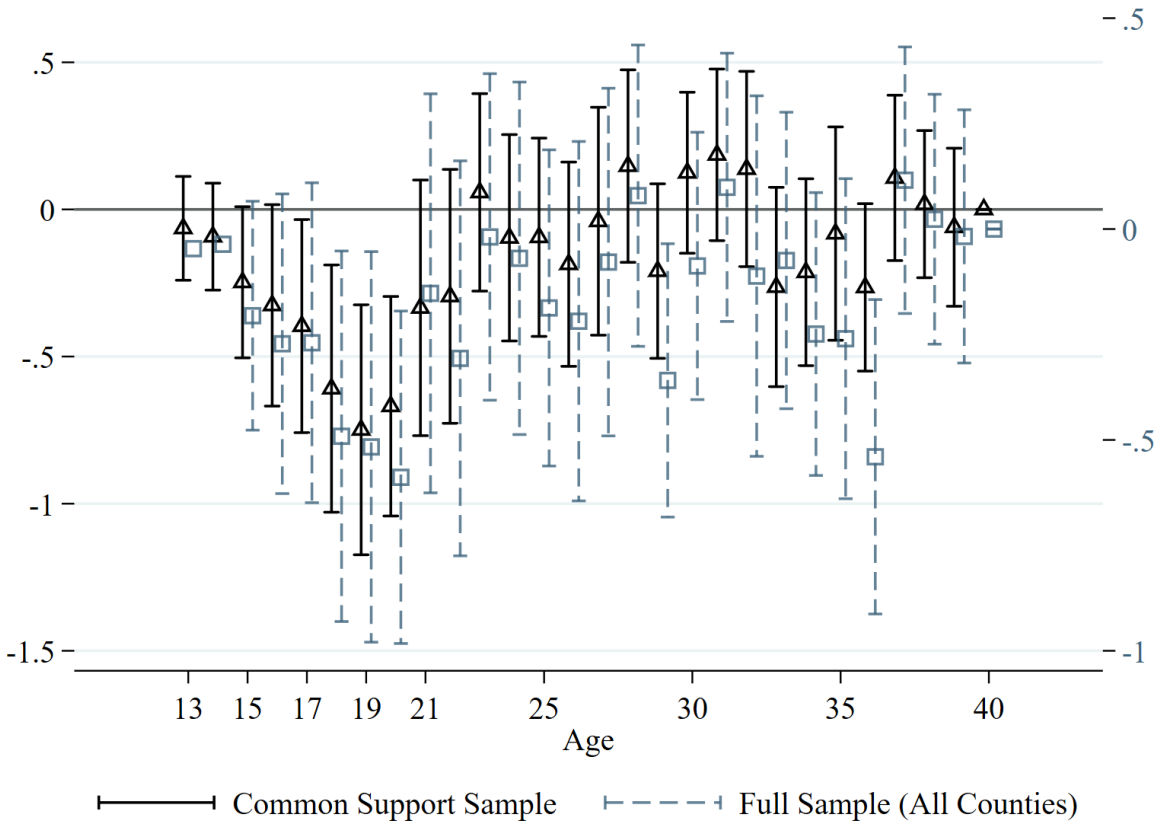
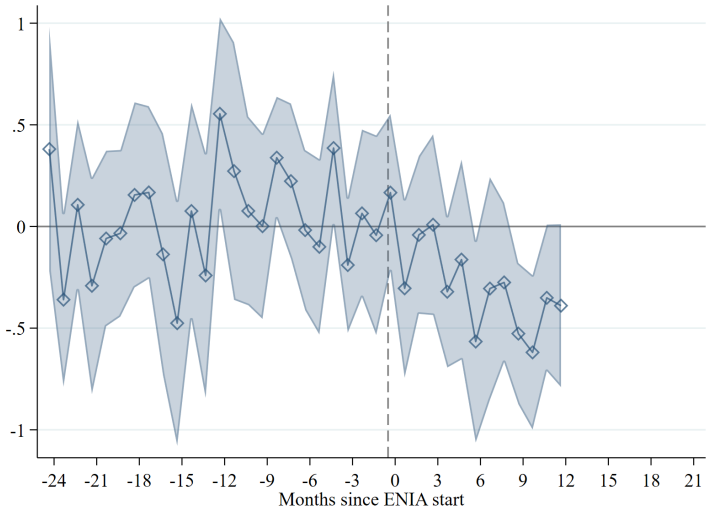


Figure A6: Age-Specific Difference-in-Differences Estimates: Full Sample

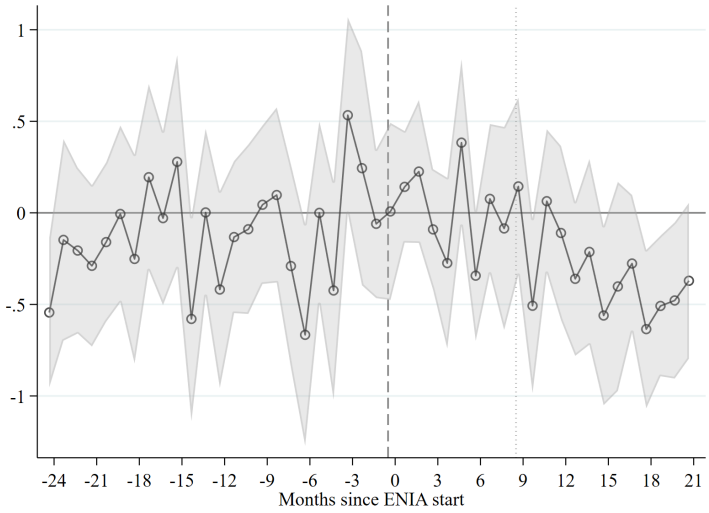
Notes: Age-specific difference-in-differences estimates of the impact of ENIA on monthly birth rates using the full sample of counties. The figure replicates the specification used in the main text but removes the common-support restriction. Coefficients correspond to the interaction between the ENIA treatment indicator and the post-policy period for each single year of maternal age. Bars denote 95% confidence intervals with standard errors clustered at the county level.

Figure A7: Timing of ENIA Effects on Conceptions and Births: Full Sample

(a) Conception Timing

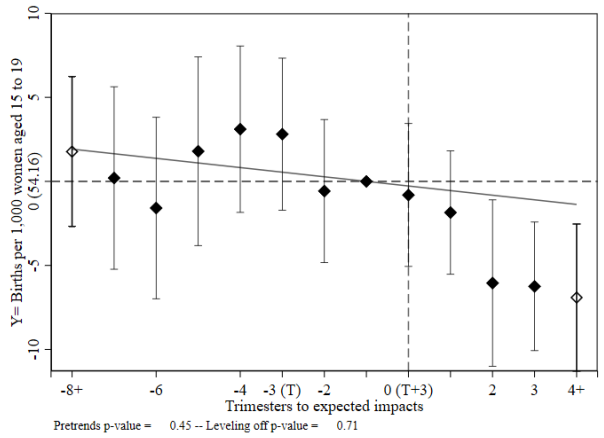


(b) Birth Timing

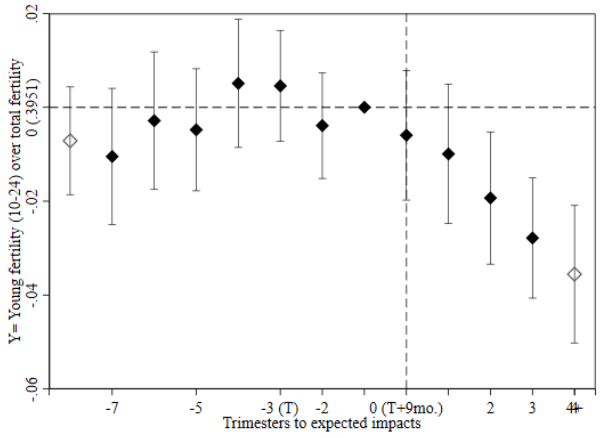


Notes: Monthly event-study estimates of ENIA's effects on conceptions and births among adolescents. Both panels report estimates obtained using the imputation estimator proposed by Borusyak, Jaravel, and Spiess (2024). The outcome variable is the rate per 1,000 women aged 15–19 at the county–month level. The estimates use the full sample. Shaded areas represent 95% confidence intervals with standard errors clustered at the county level.

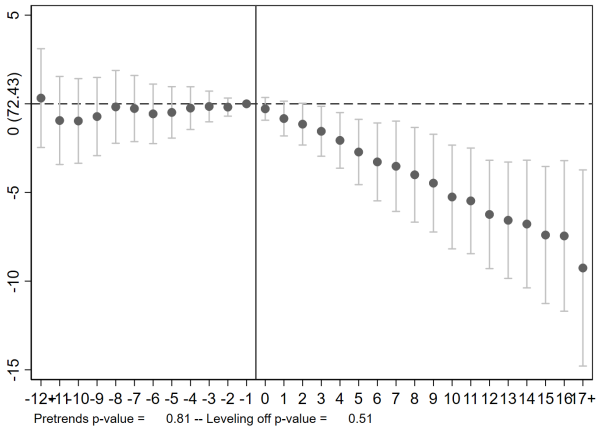
Figure A8: Robustness: Birth Ratio, Time at Conception and Placebo



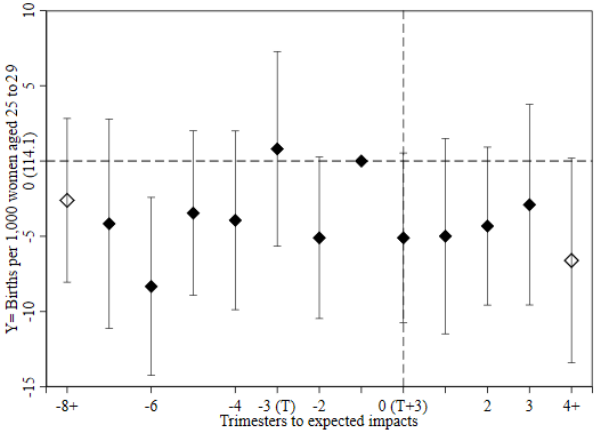
(a) Event study (TWFE)



(b) Birth ratio (young to adults)



(c) Monthly estimates using conception time



(d) Placebo analysis

Notes: In all but Figure (B), the vertical line stands for the expected time of impact (9 months after the implementation) period at which treatment impacts can be plausibly driven by preventive choices. Figure (A) adds a linear smoothest path through confidence region as suggested by Freyaldenhoven et al. (2021). Figure (C) recovers the conception time by subtracting the gestational period, hence time is presented relative to the implementation. In Figure (B) the outcome variable is the ratio of births to young mothers (aged 10 to 24) over total births. Figure (D) presents the estimates for the age group aged 25 to 29.

Figure A9: Estimated Treatment Effects and LARC Uptake in Teenagers

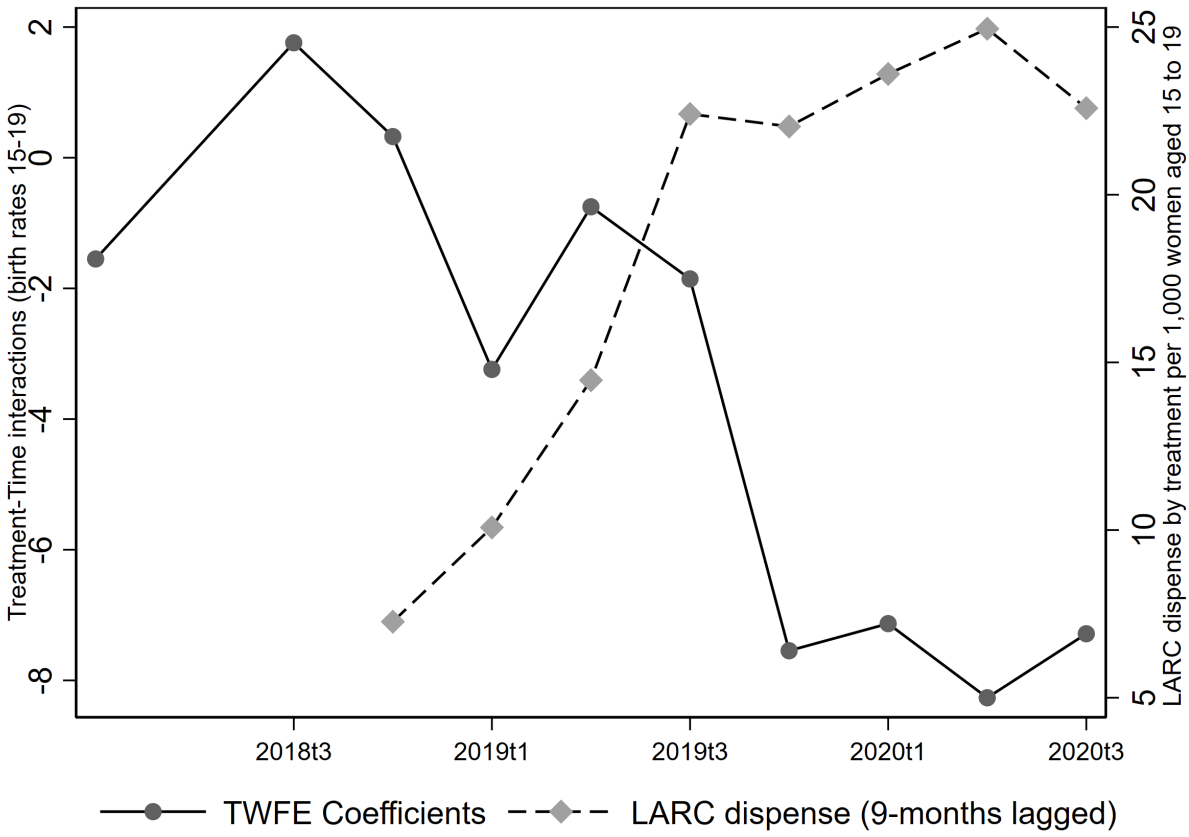
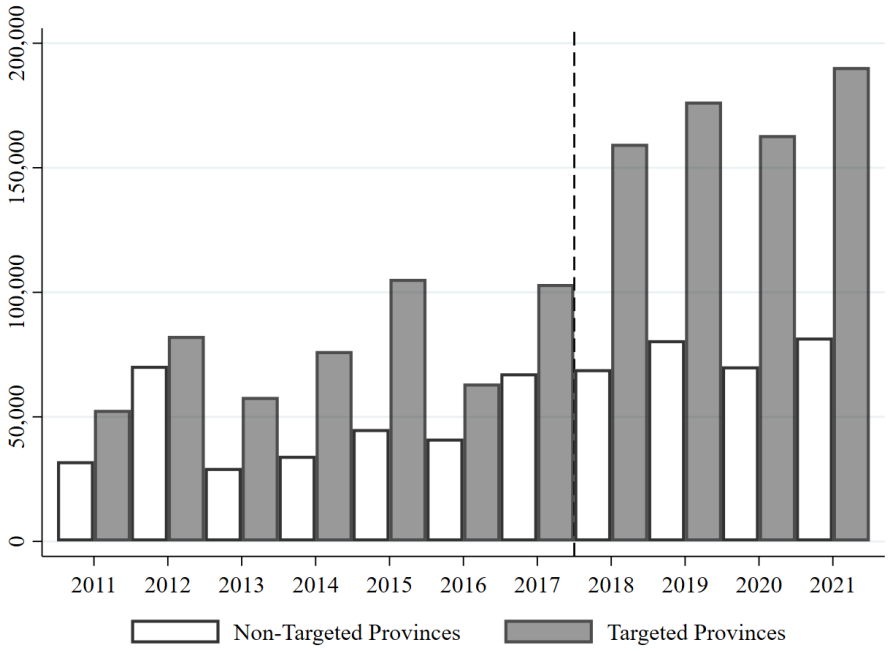
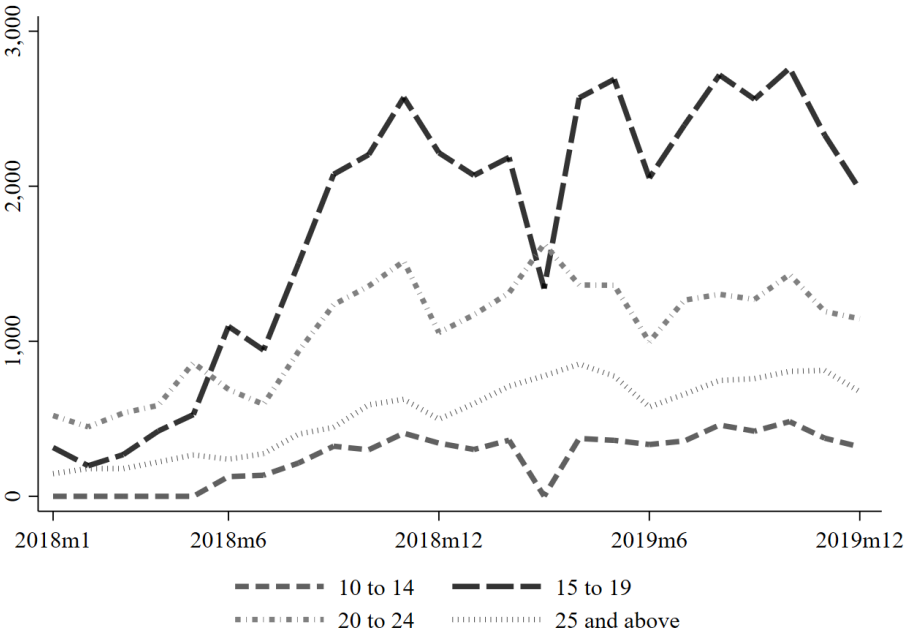


Figure A10: Variation in LARC Supply and Uptake

(a) LARC Inventories at Provincial Level



(b) Monthly LARC Uptake in ENIA Counties by Age Group



Notes: Panel (A) shows total LARC inventories at the provincial level, distinguishing provinces with ENIA-targeted counties from non-targeted provinces. Panel (B) shows monthly LARC uptake in ENIA counties by age group. The vertical line indicates program rollout. Both panels show a sharp increase in LARC availability and uptake following ENIA implementation, with increases concentrated among adolescents aged 15–19 relative to older groups.

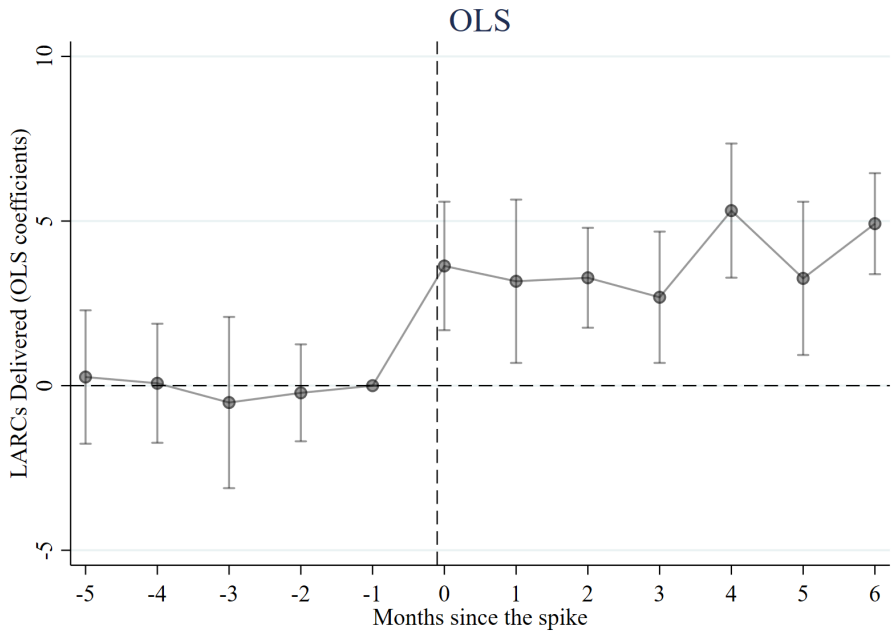


Figure A11: Event Study Around the First LARC Supply Spike

Notes: The figure reports coefficients from an event-study specification centered on the first month in which LARC deliveries in a county exceed the county-specific baseline by more than two standard deviations. The outcome is the number of LARC devices delivered. The vertical dashed line denotes the spike month. Confidence intervals are at the 95% level.

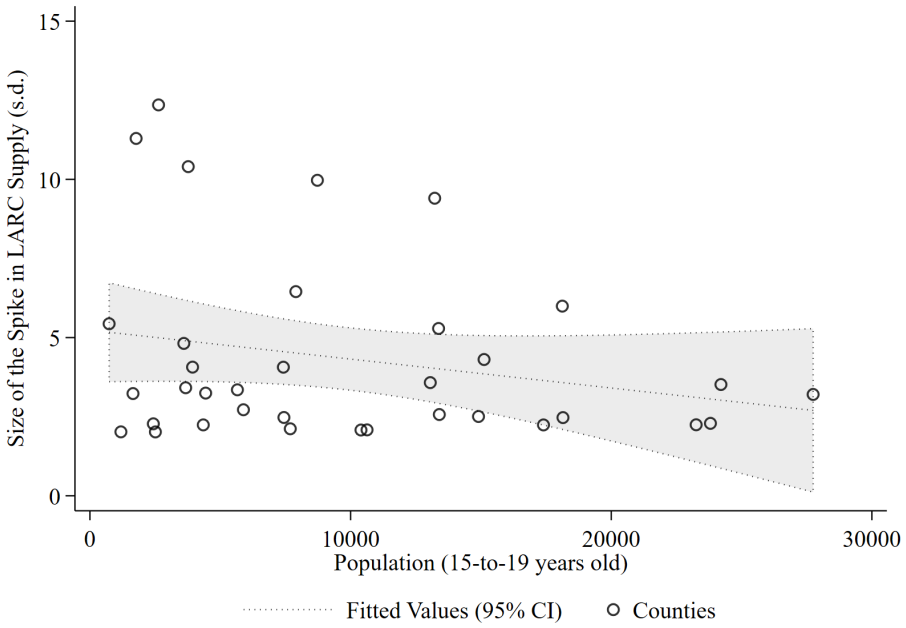
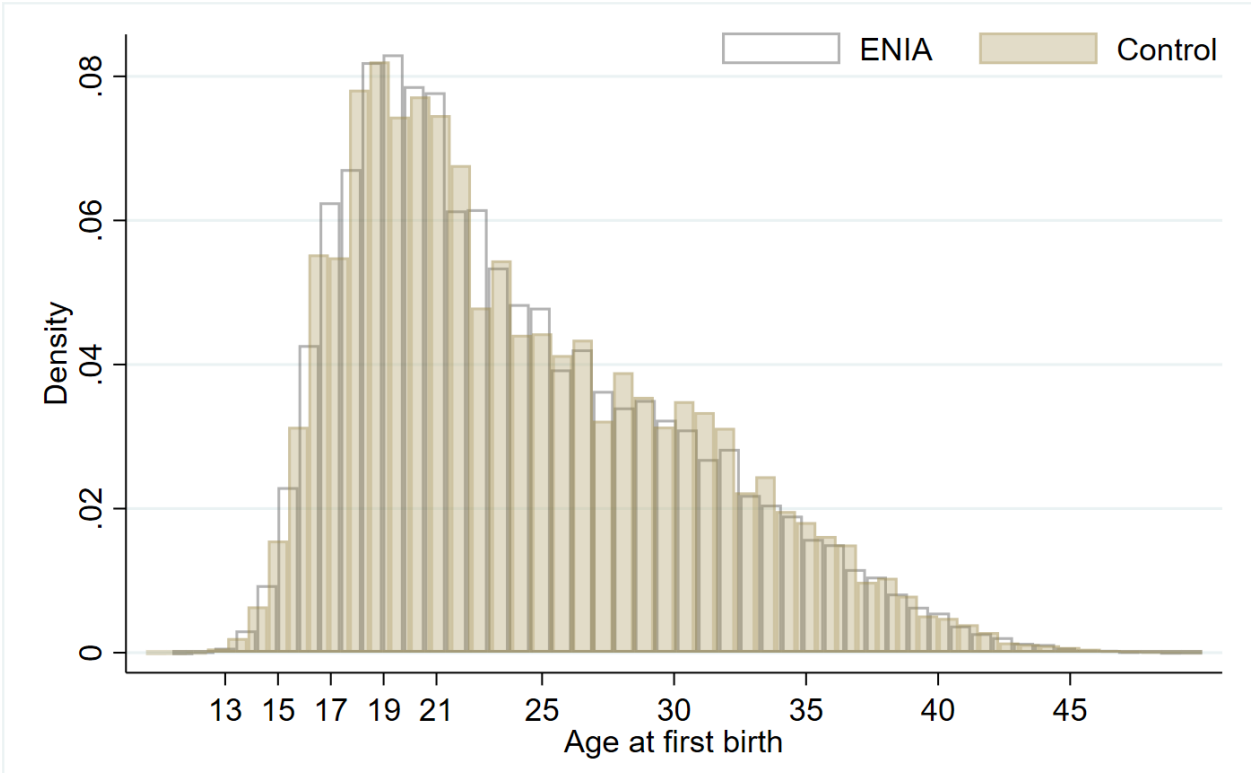


Figure A12: Size of LARC Supply Spikes and County Population

Notes: The figure plots the magnitude of the first LARC supply spike in each county against the size of the adolescent female population (ages 15–19). The spike is defined as the first month in which deliveries exceed the county baseline by more than two standard deviations. The fitted line corresponds to an OLS regression with 95% confidence intervals.

Figure A13: Distribution of First Births in 2017



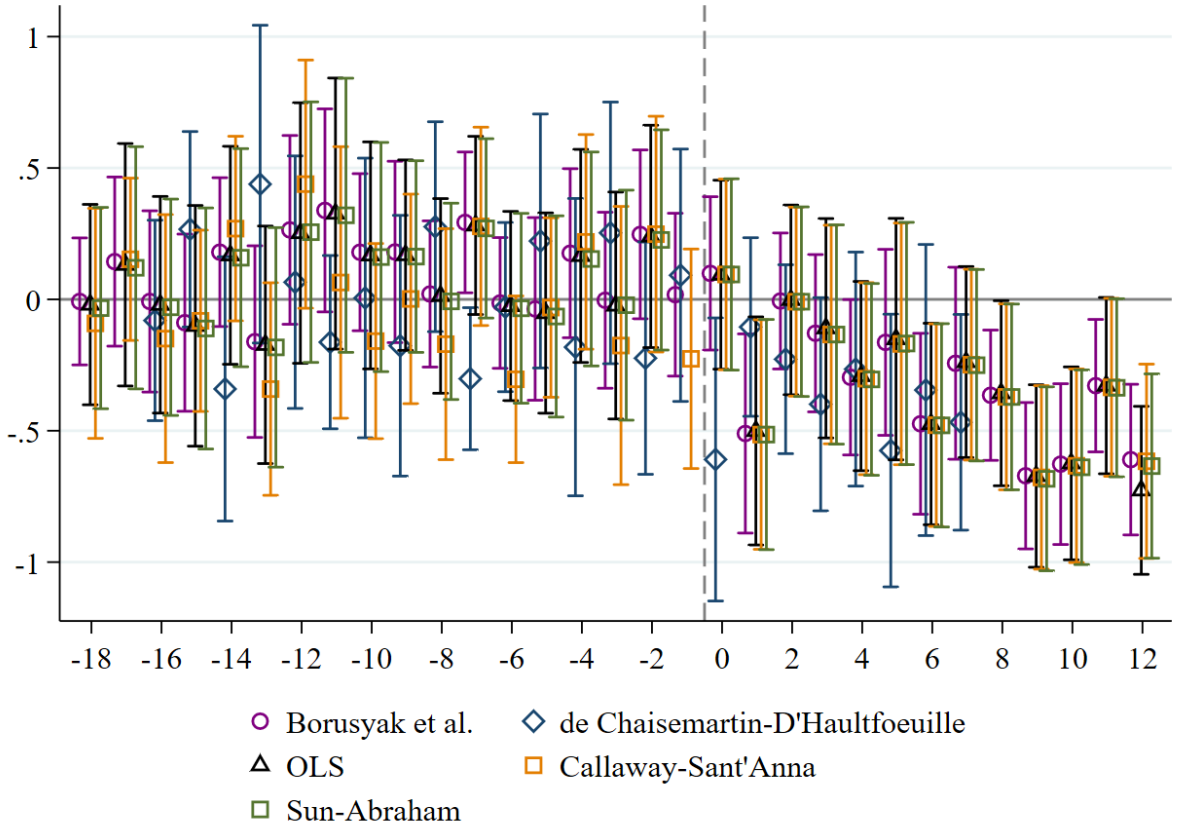


Figure A14: Dynamic Treatment Effects Across Alternative Difference-in-Differences Estimators

Notes: Event-study estimates of the impact of ENIA on adolescent birth rates using alternative staggered difference-in-differences estimators. The figure compares estimates obtained using the imputation estimator of Borusyak, Jaravel, and Spiess (2024), the interaction-weighted estimator of Sun and Abraham (2021), the group-time average treatment effects estimator of Callaway and Sant'Anna (2021), the estimator of de Chaisemartin and d'Haultfoeuille (2020), and the conventional two-way fixed-effects (TWFE) estimator. The outcome variable is the birth rate per 1,000 women aged 15–19 at the county–quarter level. Event time is measured relative to the quarter of ENIA implementation. The sample corresponds to the common-support sample used in the main analysis. Bars denote 95% confidence intervals with standard errors clustered at the county level.

Figure A15: Goodman-Bacon (2021) Decomposition

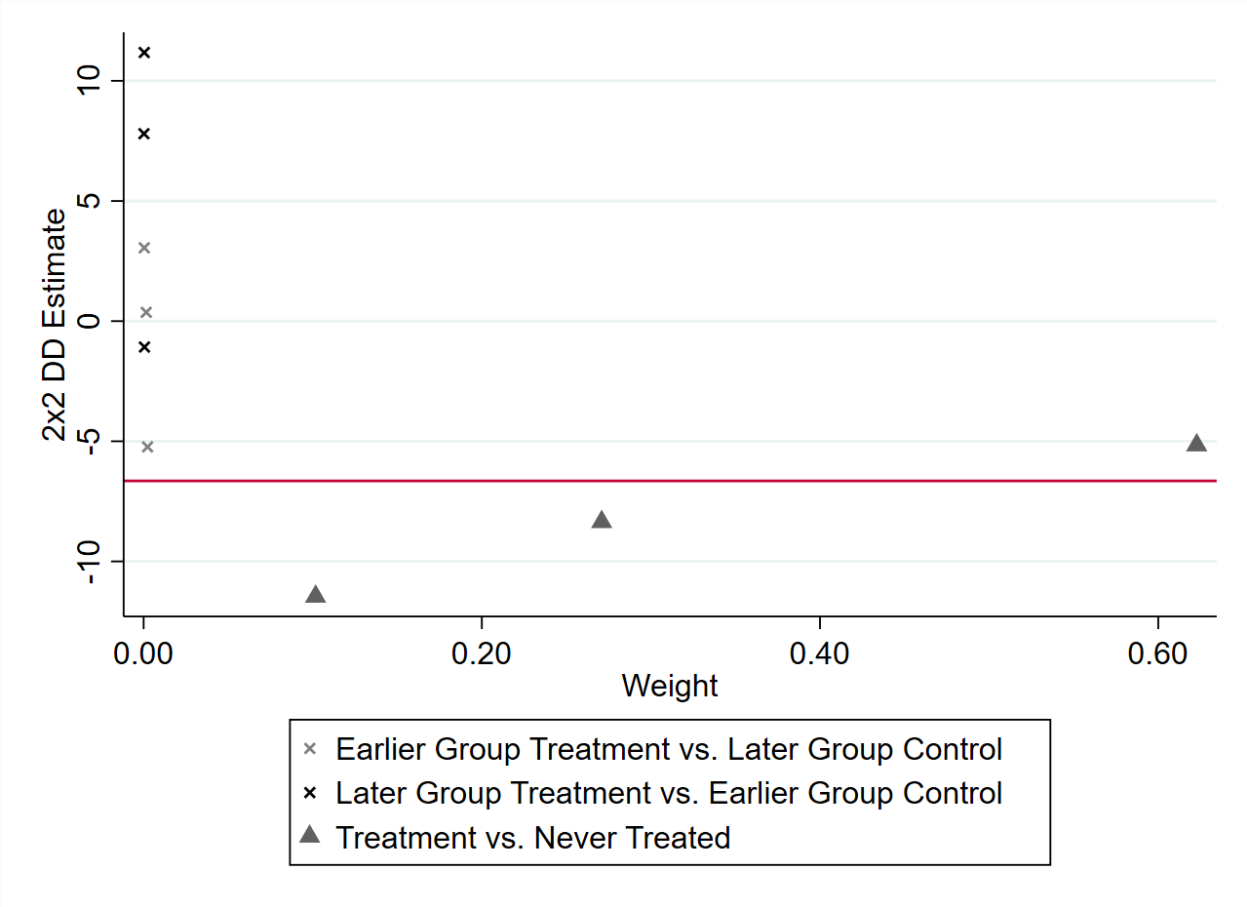
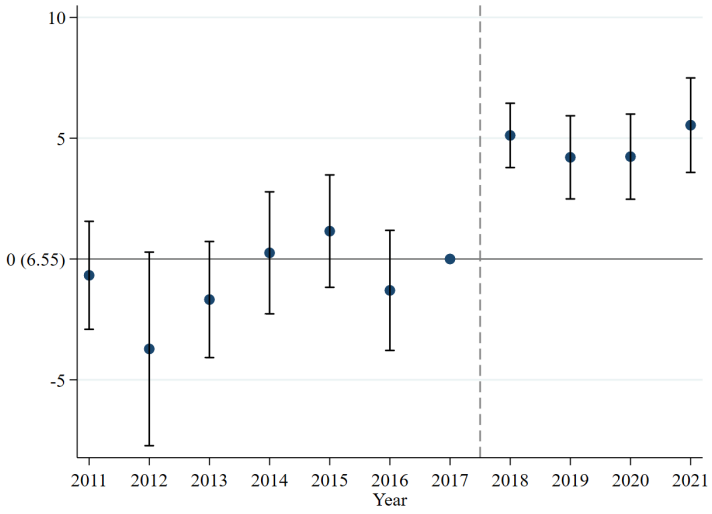
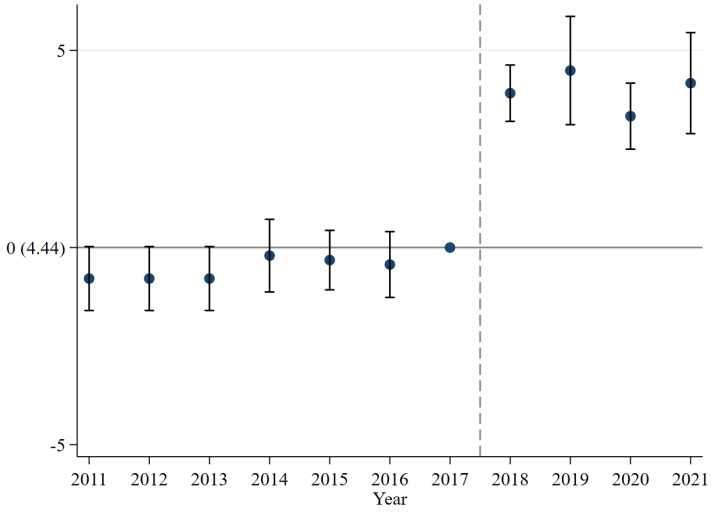


Figure A16: Impacts of ENIA on LARC Inventories

(a) LARC (all) per 1,000 Females Aged 13-19



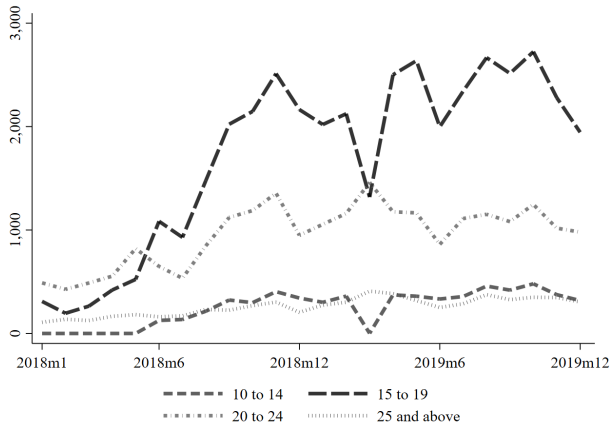
(b) Subdermal Implants per 1,000 Females Aged 13-19



Notes: The figure reports event-study estimates of ENIA's impact on LARC inventories at the provincial level. Outcomes are expressed per 1,000 females and estimated using a TWFE specification where provinces with ENIA counties are treated starting in 2018. The results show a discrete and persistent increase in LARC availability following program implementation, driven primarily by subdermal implants among adolescents.

Figure A17: LARC Uptake on ENIA Counties by Type and Age Groups

(a) Subdermal Implants



(b) IUD (intrauterine device)

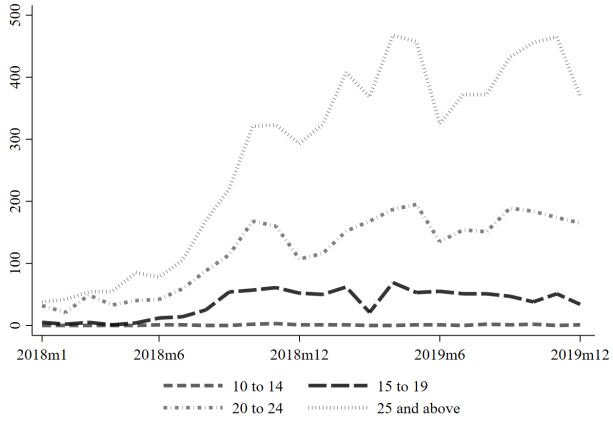
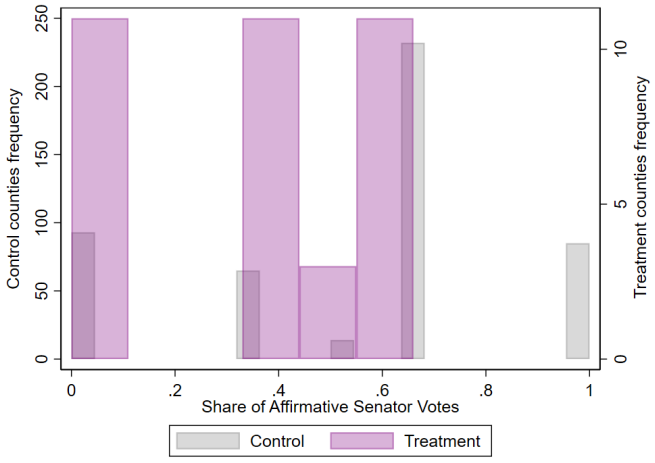
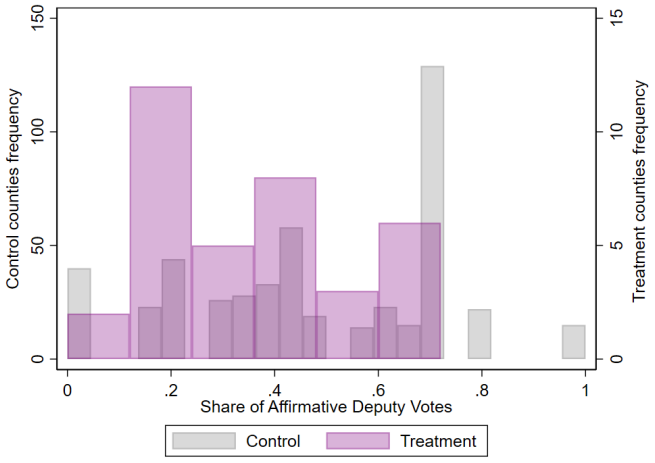


Figure A18: Distribution of Votes on Voluntary Interruption of Pregnancy Bill



Upper Legislative Chamber



Lower Legislative Chamber

Table A1: First-Birth Effects: Alternative Specifications

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se
tau	-3.883***	-3.851***	-2.623***	-2.732***	-2.907***	-0.059***
	(0.991)	(0.978)	(0.828)	(0.820)	(0.909)	(0.020)
Mean Dep. Var.	47.25	47.25	42.37	42.37	42.37	103.91
Observations	11480	11480	11480	18410	11480	11480
Counties (Clusters)	328	328	328	526	328	328
Estimator	BJS	TWFE	BJS	BJS	CS	Poisson
Sample	Com. Support	Com. Support	Com. Support	Full	Com. Support	Com. Support
Weights/Exposure	No	No	Yes	Yes	Yes	2017 Baseline
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table replicates the main specification using first births as the outcome. The dependent variable is first births per 1,000 females ages 15–19. The treatment indicator equals one in periods three or more quarters after program implementation. All specifications include county and quarter fixed effects, and standard errors are clustered at the county level. The pre-treatment mean is computed among treated counties over event times -4 to -1.

Table A2: Main Effects: Alternative Sample Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	b/se	b/se	b/se	b/se	b/se	b/se	b/se
tau	-5.428***	-4.626***	-4.744***	-5.102***	-3.635***	-4.819***	-5.089***
	(1.466)	(1.473)	(1.493)	(1.507)	(1.119)	(1.543)	(1.435)
Mean Dep. Var.	62.05	62.05	67.15	62.05	60.19	61.17	63.05
Observations	11480	18410	13685	12075	14105	18095	18375
Counties (Clusters)	328	526	391	345	403	517	525
Estimator	BJS	BJS	BJS	BJS	BJS	BJS	BJS
Sample	Com. Support	Full	-Buenos Aires	ENIA Provinces	Fertility Support	-Formosa	-San Isidro
Weights/Exposure	No	No	No	No	No	No	No
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the main specification under alternative sample restrictions. The dependent variable is total births per 1,000 females ages 15–19. The sample varies across columns as indicated. All specifications include county and quarter fixed effects, and standard errors are clustered at the county level. The pre-treatment mean is computed among treated counties over event times -4 to -1.

Table A3: Main Effects: Alternative Population Weights

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	b/se	b/se	b/se	b/se	b/se	b/se	b/se
tau	-4.003***	-4.626***	-5.542***	-4.291***	-3.359***	-4.199***	-4.619***
	(1.141)	(1.473)	(1.067)	(1.269)	(1.108)	(1.179)	(1.115)
Mean Dep. Var.	55.13	55.13	61.61	55.13	54.99	54.52	55.97
Observations	11480	18410	13685	12075	14105	18095	18375
Counties (Clusters)	328	526	391	345	403	517	525
Estimator	BJS	BJS	BJS	BJS	BJS	BJS	BJS
Sample	Com. Support	Full	-Buenos Aires	ENIA Provinces	Fertility Support	-Formosa	-San Isidro
Weights/Exposure	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the main specification under alternative weighting schemes. The dependent variable is total births per 1,000 females ages 15–19. Columns differ in the use of population weights based on the female population ages 15–19. All specifications include county and quarter fixed effects, and standard errors are clustered at the county level. The pre-treatment mean is computed among treated counties over event times -4 to -1.

Table A4: Comprehensive Sex Education

Panel A: Principal Reports				
	(1)	(2)	(3)	(4)
	Any CSE	Any ENIA	Ratio ENIA (*sd.*)	Agree
$\mathbb{1}\{ENIA\} \times$ Public	0.033	0.117***	0.019	0.125***
	(0.035)	(0.039)	(0.084)	(0.041)
Public	-0.024	0.095***	0.090**	0.086***
	(0.021)	(0.019)	(0.042)	(0.018)
Y-variable Mean	0.698	0.397	0.001	0.372
R2	0.062	0.072	0.054	0.073
N. of obs.	4,314	4,314	3,983	4,314
N. of Clusters	181	181	181	181
County FE	Y	Y	Y	Y
Individual controls	Y	Y	Y	Y
School controls	Y	Y	Y	Y
Panel B: Students Reports				
	(1)	(2)	(3)	(4)
	Any CSE	Any ENIA	Ratio	Agree
$\mathbb{1}\{ENIA\} \times$ Public	0.016***	0.067***	0.145***	0.107**
	(0.006)	(0.020)	(0.032)	(0.046)
Public	-0.026***	-0.005	0.087***	0.109***
	(0.002)	(0.008)	(0.014)	(0.021)
Y-variable mean	0.945	0.792	-0.005	0.381
R2	0.018	0.024	0.019	0.092
N. of obs.	159,696	159,696	150,851	159,696
N. of counties	181	181	181	181
County FE	Y	Y	Y	Y
Individual controls	Y	Y	Y	Y
School controls	Y	Y	Y	Y

Notes: This table presents OLS estimates based on Aprender 2019 using both student and principals reports. The dependent variable in (3) represents the standardized share of ENIA-targeted CSE modules over total modules. Columns (1), (2), and (4) are dummy variables where *Agree* takes a value of 1 if both principals and at least half of respondent students report any ENIA-targeted CSE module. Individual controls are included for students' gender, age, nationality, and mother's education and for principals' gender, age, age squared, and university attainment. School controls include school infrastructure, number of student respondents in the school, and female ratio. Robust standard errors clustered at county level are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: School-Based Counseling and the Uptake of Subdermal Implants: Including outliers

	$log(LARC_t)$	
$log(CSE_t)$	0.028*	0.028*
	(0.015)	(0.015)
$log(CSE_{t-1})$	-0.003	-0.002
	(0.015)	(0.015)
$log(SBC_t)$	0.071**	0.071**
	(0.033)	(0.033)
$log(SBC_{t-1})$	0.110***	0.111***
	(0.031)	(0.031)
$log(CBC_t)$		-0.032
		(0.030)
$log(CBC_{t-1})$		-0.002
		(0.032)
$log(CSE_t \times CSE_{t-1})$	0.025	0.026
$log(SBC_t \times SBC_{t-1})$	0.181	0.182
$log(CBC_t \times CBC_{t-1})$		-0.034
Obs.	828	828
R2	0.791	0.791
Time FE	Y	Y
County FE	Y	Y

Notes: This table reports regressions of LARC adoption on contemporaneous and lagged measures of ENIA program components within treated counties during 2018–2019. The dependent variable is the log number of LARC insertions. Explanatory variables include participation in comprehensive sex education (CSE), school-based counseling (SBC), and community-based counseling (CBC), all expressed in logs per 1,000 adolescents. All specifications include county and time fixed effects. Standard errors are clustered at the county level. School-based counseling is positively associated with subsequent increases in LARC uptake, particularly at short lags, while associations with broader curricular exposure (CSE) are smaller and less precisely estimated once counseling is accounted for.

Table A6: Impacts by Interaction with Social Norms

	(1)	(2)	(3)	(4)
$\mathbb{1}\{ENIA \times POST\}$	-3.450	-5.924**	-5.076**	-8.711*
	(2.211)	(2.422)	(2.508)	(4.643)
$\mathbb{1}\{ENIA \times POST\} \times Index$	-5.173**	-13.269***	-10.568***	-10.713***
	(2.086)	(2.374)	(2.562)	(3.411)
$\mathbb{1}\{ENIA \times POST\} \times Index^2$		10.440***	9.104**	21.011*
		(3.350)	(3.618)	(11.764)
$\mathbb{1}\{ENIA \times POST\} \times Index^3$				-8.516
				(8.306)
Avg. Nightlight Intensity	-5.220***	-5.254***	-5.029***	-5.274***
	(1.499)	(1.495)	(1.641)	(1.495)
N. of obs.	18375	18375	18375	18375
Counties	525	525	525	525
Y-variable Mean	70.79		70.79	
R2	0.63	0.63	0.63	0.63
County× FE	Y	Y	Y	Y
Province Seasonality	Y	Y	Y	Y
Time × Category FE			Y	
Restricted Sample			Y	

Notes: The regression results presented in this table are based on Equation 1 with birth rates (15-19) as the dependent variable. The index used in the models is a proxy for restrictive norms, constructed as the principal factor of legislators (senators and deputies) voting against the Voluntary Interruption of Pregnancy Bill and a dummy for self-proclaimed pro-life counties. Treatment effects are allowed to vary in different ways across the models: column (1) uses a linear relationship, while columns (2) and (3) use a quadratic relationship. Column (4) allows for a cubic relationship. The index was chosen based on having an eigenvalue greater than 1. Robust standard clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.