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September 3, 2026

Workshop on Development Economics and Networks for Early-Career Researchers
National Bureau of Economic Research, Cambridge, MA

Dear Professors Breza, Chandrasekhar, and Viviano,

I am writing to apply to the NBER Workshop on Development Economics and Networks for Early-Career Researchers on December 5, 2026. I am a fifth-year Ph.D. candidate at Harvard's Department of Global Health and Population, and I would like to present my job market paper, "Can Adolescent-Friendly Health Services Lower Teen Birth Rates? Evidence from a Staggered Roll-Out in the Dominican Republic."

This application includes, in order, a statement of motivation and research interests, my curriculum vitae with a list of my completed and current doctoral courses, and my job market paper.

Recommendation: Jessica Cohen, Professor of Health Economics, Department of Global Health and Population, Harvard T.H. Chan School of Public Health (cohenj@hsph.harvard.edu), is my dissertation advisor and committee member and can provide a recommendation if I am shortlisted.

Thank you for your consideration.

Sincerely,

Marie Chantel Montás

Workshop on Development Economics and Networks - Statement of Motivation

Marie Chantel Montás | Ph.D. Candidate in Health Systems | mariemontas@fas.harvard.edu

I grew up in the Dominican Republic (DR), where near-universal skilled birth attendance coexists with high maternal and neonatal mortality and exceptionally high c-section rates. Moreover, my experience working within the Dominican public health and social security system taught me that access does not necessarily translate to effective care: whether care reaches people depends on the quality and organization of services, what people know, and whom they trust. My research examines how the quality and organization of health-service delivery shape fertility, health, and human capital in LMICs.

In my job market paper, I evaluate the DR's staggered national expansion of adolescent-friendly health services (AFHS) inside public hospitals. I assembled opening dates from government records and field visits, linked 1.44 million registered births to census and geolocated facility data, and built small-area population denominators. Using heterogeneity-robust DiD estimators, I find that a municipality's first unit reduces the adolescent birth rate by 6.4 births per 1,000 women aged 15–19, or 12 percent from the baseline mean. The decline begins after a nine-month gestational lag, reaches 17 percent by the second year, and persists as exposed cohorts enter ages 20 and 21. Independent hospital-production registries of pregnancies and abortion-related care also show a similar decline. Secondary school enrollment also increases by 1.5 points overall, with gains for both boys and girls, and the largest proportional increase among overage students in the terminal grade. These results suggest broader human-capital responses beyond the adolescents directly at risk of pregnancy.

I also explore spatial patterns that naturally point toward network questions. When I exclude untreated municipalities within 10, 20, and 30 kilometers of an adolescent unit, the estimated fertility reduction strengthens from 6.55 to 9.92 births per 1,000. This pattern is consistent with nearby controls being partially exposed through cross-border service use. Some adolescents may travel farther to seek anonymity, but the current data cannot distinguish that mechanism from other forms of cross-border use. These are not direct estimates of network transmission, but they suggest that information, service use, and behavioral responses may extend beyond municipal boundaries. Together with the schooling outcomes, this evidence motivates a next study of peer effects in fertility and school attachment within school networks. Overall, I want to move from these reduced-form patterns to designs that measure how information, beliefs, service use, and human-capital responses travel through geographic and social ties.

Furthermore, my broader research agenda examines quality and access at different points in the delivery chain. In urban Malawi, I use randomized trial data to isolate the causal effect of counseling quality on family planning outcomes. In Nigeria, I estimate the relation between price and quality of pharmaceuticals. Currently, I am working with Family Empowerment Media on a cluster-randomized trial of radio-delivered family-planning information in Nigeria. As we enter baseline fieldwork, I am building the geospatial sampling frame around health-facility catchment areas. For me, the central question is not only whether radio broadcasts impact contraceptive knowledge and behavior, but how messages move through social networks, whether social ties reinforce or weaken them, and how the design should account for spillovers and interference.

The NBER workshop would help me strengthen my ability to measure social ties in LMICs, differentiate direct effects from network transmission, and draw valid inferences when treatment effects propagate across connected individuals and communities. I would present new population-level evidence from the DR showing that a nationwide expansion of AFHS reduced adolescent fertility, alongside spatial and educational patterns that point to effects extending beyond treated municipalities and adolescents directly at risk of pregnancy. I would apply the training immediately to develop extensions of my job market paper to investigate these patterns; and to incorporate diffusion, spillovers, and interference into the design and analysis of the radio-based trial. Together, these projects form my post-Ph.D. research agenda on how health information and services move through networks, whom they reach, and when social connections amplify their benefits.

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RESEARCH INTERESTS

Development economics; health economics; global health; impact evaluation and causal inference; networks; health systems and service delivery; sexual and reproductive health; maternal and neonatal health; drug quality; human capital. Geographic focus: Latin America and the Caribbean, sub-Saharan Africa.

TEACHING INTERESTS

Quantitative research methods; introduction to economics; development economics; microeconomics; econometrics for health policy; comparative health systems; global health; public policy analysis.

EDUCATION

HARVARD UNIVERSITY, DEPARTMENT OF GLOBAL HEALTH AND POPULATION Boston, MA
Ph.D. Candidate in Population Health Sciences (Health Systems), 2022 – 2027 (expected)
Minors: Health Economics and Development Economics. **Advisor:** Jessica Cohen; committee: Jessica Cohen, Margaret McConnell and Kevin Croke.
Dissertation: "Salient Markers of Health Systems Quality: Evidence from Low- and Middle-Income Countries"
Doctoral coursework (completed and current):
Economics: Microeconomic Theory I and II (ECON 2020A/2020B); Development Economics: Microeconomic Issues (MIT 14.771; Profs. Esther Duflo and Benjamin Olken); Health Economics (ECON 2465); Research in Health Economics (ECON 3017); Behavioral Economics and Global Health (GHP 237).
Econometrics and methods: Econometric Methods in Impact Evaluation (GHP 228); Quantitative Research Methods in Population Health Sciences I and II (PHS 2000A/2000B); Multilevel and Longitudinal Models (STAT 151); Introduction to Epidemiology: Methods I with laboratory (EPI 201/EPI 201LAB); Measuring Population Health (GHP 506); Modeling for Health System Analysis and Priority Setting (GHP 501).
Global health and population: Concepts and Methods for Global Health and Population Studies (GHP 210); Comparative Health Systems I (GHP 202) and Part II (GHP 203); An Introduction to History, Politics, and Public Health: Theories of Disease Distribution and Health Inequities (SBS 506); Field Trip: Health Reform and Community Medicine in Chile (GHP 297); Responsible Conduct of Research (HPM 548).
Current (Fall 2026): Research (GHP 350, dissertation).

UNIVERSITY OF MICHIGAN, DEPARTMENT OF HEALTH MANAGEMENT AND POLICY Ann Arbor,MI
Master of Public Health, Health Management and Policy (GPA 4.0), 2020-2022
Concentration: Global Health Policy. **Advisors:** Elisa Maffioli and Scott Greer
Other activities: Research Assistant, Center for Evaluating Health Reform (Prof. Andrew Ryan) and to Prof. Elisa Maffioli; Professional Development Chair, Health Policy Student Association.

UNIVERSITAT POMPEU FABRA Barcelona, Spain
Postgraduate Diploma in Pharmacoeconomics, 2019

PONTIFICIA UNIVERSIDAD CATÓLICA MADRE Y MAESTRA Santo Domingo, DR
Bachelor of Arts in Economics, (GPA 3.6, Magna Cum Laude), 2013-2017
Other activities: Teaching Assistant for Public Policy Analysis, Development Economics, and Macroeconomics.

JOB MARKET PAPER

"Can Adolescent-Friendly Health Services Lower Teen Birth Rates? Evidence from a Staggered Roll-Out in the Dominican Republic" (2026). Read here.

PEER-REVIEWED PUBLICATIONS

"Is price associated with the quality of medicines? Evidence from active pharmaceutical ingredient testing in Nigeria" (2025). *PLOS ONE*, 20(12): e0338739. with C. Anyakora and E.M. Maffioli. doi:10.1371/journal.pone.0338739

"Excessive active pharmaceutical ingredients in substandard and falsified drugs should also raise concerns in low-income countries" (2024). *Journal of Global Health*, 14: 03029. with E.M. Maffioli and C. Anyakora. doi:10.7189/jogh.14.03029

"Usability and cultural relevance of an mHealth app for Hispanic/Latino individuals living with rheumatoid arthritis: Protocol for a mixed methods study" (2026). *JMIR Research Protocols*, 15: e88401. with T.F. Alves, R. Horner, and M. Cozad. doi:10.2196/88401

"Strategic purchasing and health systems resilience: Lessons from COVID-19 in six European countries" (2022). *Health Policy*. with K. Klasa, E. van Ginneken, and S.L. Greer. doi:10.1016/j.healthpol.2022.06.005

"Hispanic–White differences in double bonuses for quality of care in Medicare Advantage" (2022). *JAMA Health Forum*, 3(3): e215281. with A.A. Markovitz, A. Warrier, J.Z. Ayanian, and A.M. Ryan. doi:10.1001/jamahealthforum.2021.5281

WORKING PAPERS

"COVID-19 and Teen Birth Rates: Comparative Evidence from Ecuador and the Dominican Republic" (2026). with O. Galárraga. *Working paper.* Read draft here.

"The Causal Effects of Counseling Quality: Experimental Evidence from Urban Malawi" (2026). with S. Vincent and M. Karra. *Working paper.* Read draft here.

"Family Planning and Unintended Pregnancy: Experimental Evidence from Urban Malawi" (2026). with M. Karra and S. Vincent. *Working paper.* Read draft here.

"Chatbots as Digital Health Interventions for Adults with Chronic Conditions: A Scoping Review" (2026). with M. Mamello and T.F. Alves. *Working paper.*

"Inter-Sectoral Budgeting in Health: A Case-Study Analysis for Low-, Middle-, and High-Income Countries" (2026). with P. Vissapragada and S.L. Greer. *Under review, Global Public Health.*
"Biomedical Capacity, Governance, and Health Security: A Dominican Republic Analysis of Stakeholder Perspectives" (2026). with A. Baez, A. Schad, and W. Malamud. *Under review, PLOS Global Public Health.*

Conference Presentations

"Can Adolescent-Friendly Health Services Lower Teen Birth Rates? Evidence from a Staggered Roll-Out in the Dominican Republic." **Population Association of America (PAA) 2027**, Seattle, WA.

"The Causal Effects of Counseling Quality: Evidence from a Family Planning Intervention in Urban Malawi." **ASHEcon Annual Conference 2026**, Minneapolis, MN (Contraception and Development session).
"Is price a predictor of the quality of medicines? Quantitative evidence from Nigeria." **Science for Health Systems 2025**, St. Louis, MO.
"Strategic purchasing and health systems resilience: Lessons from COVID-19 in selected European countries." **Health Systems Global (HSR 2022)**, Bogotá, Colombia; **APHA 2022**; University of Michigan Global Public Health Poster Session.

Research Experience

HARVARD UNIVERSITY Boston, MA
Doctoral Researcher, 2022 – present
Dissertation research on health system quality in LMICs: a national quasi-experimental evaluation of adolescent health services in the Dominican Republic (universe of registered births linked to census and geolocated facility data; original treatment-timing record built from government documents and field visits); an instrumental-variables study of contraceptive counseling quality using randomized trial data from urban Malawi (with Prof. Mahesh Karra); and medicine-quality research in Nigeria based on laboratory testing of active pharmaceutical ingredients (with Prof. Elisa Maffioli). Designed and managed primary data collection, including a hospital data-collection operation with the Dominican Ministry of Health.
Co-investigator, cluster-randomized trial of radio-delivered family planning information in Nigeria, with Family Empowerment Media (PIs: M. Karra, D. Groves, J. Marshall, M. McConnell), 2024 – present
Working on full RCT design and implementation: survey design and instrument development, including contraceptive-method visuals and translation review; enumerator manual and training of trainers; geospatial sampling frame around health-facility catchment areas; baseline fieldwork planning and overall project management.
UNIVERSITY OF NEBRASKA MEDICAL CENTER Omaha, NE
Visiting Scholar, Health Services Research, 2025 – present
Mixed-methods research on mHealth for Hispanic/Latino populations living with rheumatoid arthritis.
UNIVERSITY OF MICHIGAN Ann Arbor, MI
Research Assistant, Center for Evaluating Health Reform, 2020 – 2022
Alternative and value-based payment models; co-authored a national study of Medicare Advantage quality bonuses and equity (*JAMA Health Forum*). With Prof. Elisa Maffioli: pre-analysis plans, IRB protocols, and survey instruments for an AEA-registered RCT on drug quality in Nigeria.

Professional Experience

SUPERINTENDENCE OF HEALTH AND LABOR RISKS (SISALRIL) Santo Domingo, DR
Health Policy Researcher, Special Studies Unit, 2017 – 2020
Analyzed the national health insurance system's administrative enrollment and paid-claims databases (7.7 million affiliates) inside the regulator. Co-authored four studies in the agency's Technical Report Series (below), including a national profile of the uninsured built by cross-linking enrollment, household-survey, poverty-registry, and tax records, and a claims-based analysis of utilization and spending by income decile.
Consultant and applied research positions, 2016 – 2025: Economic Analysis Intern, CARE (2025): impact of U.S. funding terminations across the global portfolio. Survey-instrument translation for a cash-transfer study, UMass Chan Medical School (2024). Health Financing Consultant, WHO (2022): cross-sectoral budgeting across country income levels. Consultant, Inter-American Development Bank, Social Protection and Health (2022): quality-amenable mortality in Latin America and the Caribbean. Consultant, PAHO (2021): public financial management in the COVID-19 response. Principal Collaborator, Bloomberg Philanthropies Data for Health (2022): led an AI-enhanced epidemiological surveillance dashboard for Santo Domingo. Policy Analyst, Dominican Ministry of Economy, COVID-19 unit (2020). Short-Term Consultant, World Bank/IFC, Macroeconomics and Fiscal Management (2017). Research Intern, European Observatory on Health Systems and Policies (2021). Research Assistant, Plenitud Foundation / Dominican Health System Observatory (2019). Research Assistant to Prof. Arachu Castro (2019). Research Intern, J-PAL Dominican Republic (2017). Junior Economist, Fundación Economía y Desarrollo (2016–2017).

Research Grants

$4,200 Clarence James and Sarah Bradley Gamble Scholarship, Harvard, 2025
$2,000 Hansen and Vogelheim Research Fund for Africa, Harvard, 2024
$4,500 David Rockefeller Center for Latin American Studies, Harvard, 2023
$69,300 Bloomberg Philanthropies Data for Health Global Grants Program (co-developed with Amado Báez, Augusta University), 2022
$12,500 University of Michigan fellowships and grants (four awards), 2021 – 2022
$306,120 USAID Development Innovation Ventures (DIV, Award 7200AA21FA00006), randomized evaluation of drug-quality technology in Nigeria (PI: Elisa Maffioli, University of Michigan; role: co-investigator), 2021

Awards and Fellowships

J. William Fulbright Foreign Scholarship, U.S. Embassy in the Dominican Republic, 2020
Winner, Junior Category, World Bank/IFC Call for Analytical Work on Tax Reforms and Equitable Growth, 2017

Technical Reports

"The alternative health plans of the Dominican Social Security System (SDSS) and the insurance market in the Dominican Republic, 2014–2019" (2019). *Technical Report Series, SISALRIL.* with M. Martínez, J. Rodríguez, and P. Ramírez.
"Use of services in the family health insurance of the contributory regime, 2018" (2019). *Technical Report Series, SISALRIL.* with J. Rodríguez, M. Martínez, and P. Ramírez.
"What happens when a member reaches the age of majority in family health insurance? 2014–2019" (2019). *Technical Report Series, SISALRIL.* with M. Martínez, J. Rodríguez, and P. Ramírez.
"Family health insurance affiliation coverage and profile of the unaffiliated population in the Dominican Republic, 2018" (2019). *Technical Report Series, SISALRIL.* with M. Martínez, J. Rodríguez, and P. Ramírez.
The four studies appear in *Boletín Serie de Informes Técnicos SISALRIL No. 2* (2019). Read the bulletin here.
"Fiscal policy and regional disparities in the Dominican Republic" (2018). with V. Díaz and E. Rancier.

Manuscript Reviews and Editorial Roles

Reviewer and GBD Collaborator, Global Burden of Disease (GBD) Study, Institute for Health Metrics and Evaluation (IHME).
Advisor and Reviewer, Dominican Journal of Public Health (*Revista Dominicana en Salud Pública*), Research Division, Ministry of Public Health of the Dominican Republic.

Teaching Experience

Head Teaching Fellow, Harvard University, 2023 – present
MPH 103: Leadership and Communication (Master's, Fall 2025; Fall 2026)
MPH 102: Health Systems (Master's, Fall 2025)
ID 100: Foundations for Public Health (Master's, Spring 2025)
GHP 525: Econometrics for Health Policy (Master's, Spring 2025)
MPH 105: Public Health Policy and Politics (Master's, Fall 2024)
GENED 1063: World Health: Challenges and Opportunities (Undergraduate, Spring 2024; Spring 2027)
GHP 230: Introduction to Economics with Health and Development Applications (Master's, Fall 2023)
GHP 297: Health Reform and Community Medicine in Chile (Master's, Winter 2023)
Instructor, Pontificia Universidad Católica Madre y Maestra: Research Skills Seminar, Master of Applied Economics, Summer 2022.
Teaching Assistant, Pontificia Universidad Católica Madre y Maestra: Public Policy Analysis (2018, 2019); Development Economics (2017); Macroeconomics I and II (2017).
Curriculum Designer, University of Michigan / University of Puerto Rico, 2021: designed a K–12 curriculum for the "Global Health in Puerto Rican Classrooms" program on the role of government in public health crises.

Affiliations and Service

Harvard affiliations: Institute for Quantitative Social Science (IQSS); Center for International Development (CID); Harvard Center for Population and Development Studies (HCPDS); Global Health Education and Learning Incubator (GHELI).
Service: Lead Investigator, "Frontline Voices" community disaster-risk program, TECHO Dominican Republic (2018 – 2020).
Memberships: American Society of Health Economists; AcademyHealth; Health Systems Global; Mixed Methods International Research Association.

Other

Languages: Spanish (native), English (fluent), Portuguese (advanced), French (intermediate).
Methods: randomized and quasi-experimental designs (staggered difference-in-differences, event studies, instrumental variables); administrative and claims data; geospatial analysis; survey design and field data collection.
Software: Stata, R, Python, LaTeX, SurveyCTO, QGIS, Tableau.
Additional training: J-PAL Africa, Evaluating Social Programs (2022); Inter-American Development Bank/edX, What to Finance in Health and at What Price? (2022).

References

Jessica Cohen, Professor of Health Economics, Harvard T.H. Chan School of Public Health, cohenj@hsph.harvard.edu
Margaret McConnell, Associate Professor, Harvard Pilgrim Health Care Institute, mmcconne@hsph.harvard.edu
Sue Goldie, Roger Irving Lee Professor of Public Health, Harvard T.H. Chan School of Public Health, sue_goldie@harvard.edu
Elisa Maffioli, Associate Professor, Health Management and Policy, University of Michigan School of Public Health, elisamaf@umich.edu
Scott L. Greer, Professor of Health Management and Policy, University of Michigan School of Public Health, slgreer@umich.edu.

Can Adolescent-Friendly Health Services Lower Teen Birth Rates? Evidence from a Staggered Roll-Out in the Dominican Republic

By Marie C. Montás*

Job Market Paper: September 2, 2026

Abstract

Adolescent-friendly health services are the World Health Organization's global standard for adolescent care, yet national-scale causal evidence on their effects on fertility remains scarce. I evaluate the Dominican Republic's staggered expansion of dedicated adolescent units inside public hospitals using a newly assembled record of opening dates, 1.44 million registered births from 2016 to 2025, and heterogeneity-robust difference-in-differences models across 146 municipalities. Opening a municipality's first adolescent unit reduces the birth rate among women aged 15–19 by 6.4 births per 1,000, or 12 percent of the pre-opening mean. The effect appears only after a nine-month gestational lag, reaches 17 percent by the second year of exposure, begins with conceptions at age 17, and extends into ages 20 and 21 as treated cohorts age out of adolescence. Independent hospital-production registries show a 17 percent decline in adolescent pregnancy events, while abortion-related attendances fall in proportion to births, consistent with fewer pregnancies rather than a change in reporting or substitution toward termination. Effects are larger where adolescents live closer to the public hospitals that host the adolescent units, and the estimates are stable across alternative estimators, samples, treatment dates, and population denominators. Across treated municipalities, the estimates imply around 3,300 averted adolescent births, and benchmark break-even calculations suggest that the benefits of the program are large relative to plausible operating costs. (*JEL* I12, J13, I18, I15)

Keywords: adolescent fertility; adolescent-friendly health services; reproductive-health service delivery; contraceptive access; staggered difference-in-differences; Dominican Republic

*Department of Global Health and Population, Harvard T.H. Chan School of Public Health (mariemontas@fas.harvard.edu).
I am grateful to Dr. Jael Garrido, Viceminister Dr. Eladio Pérez, Darwind De la Cruz, and Dr. Ruth Ramírez, at the Dirección de Análisis de Situación de Salud, Monitoreo y Evaluación de Resultados (DASIS), Ministry of Public Health (MoH; *Ministerio de Salud Pública*), for informational interviews, access to the birth-registration data, and fieldwork support for adolescent-unit visits. At the Programa de Adolescentes of the National Health Service (NHS; *Servicio Nacional de Salud*), Dr. Luz Fermín and Dr. Luisa Medrano provided the initial unit records, facilitated site visits, and shared institutional and clinical context. I also thank Dr. Carmen Taveras at the Ministry of Education (MoE; *Ministerio de Educación*), Madeline Martínez at the Superintendencia de Salud y Riesgos Laborales (SISALRIL), and analysts at the National Statistics Office (NSO; *Oficina Nacional de Estadística*) for providing administrative data. Interviews with Vice President Raquel Peña and representatives of the Consejo Nacional para la Niñez y la Adolescencia (CONANI), Seguro Nacional de Salud (SeNaSa), MoE's sexual education program, the Centro de Promoción de Salud Integral de Adolescentes at the Ministerio de la Mujer, PROFAMILIA, Plan International, and UNICEF informed the design of this study. I thank my dissertation committee, Jessica Cohen, Margaret McConnell, and Kevin Croke, as well as Omar Galárraga and Ben Olken, for guidance and comments. Fieldwork was supported by the Clarence James and Sarah Bradley Gamble Scholarship at Harvard T.H. Chan School of Public Health, and early-stage research by Harvard's David Rockefeller Center for Latin American Studies (DRCLAS). This research was conducted under Harvard data use agreement DUA24-1170. I am solely responsible for the paper's content, which does not necessarily reflect the views of any government agency.

1 Introduction

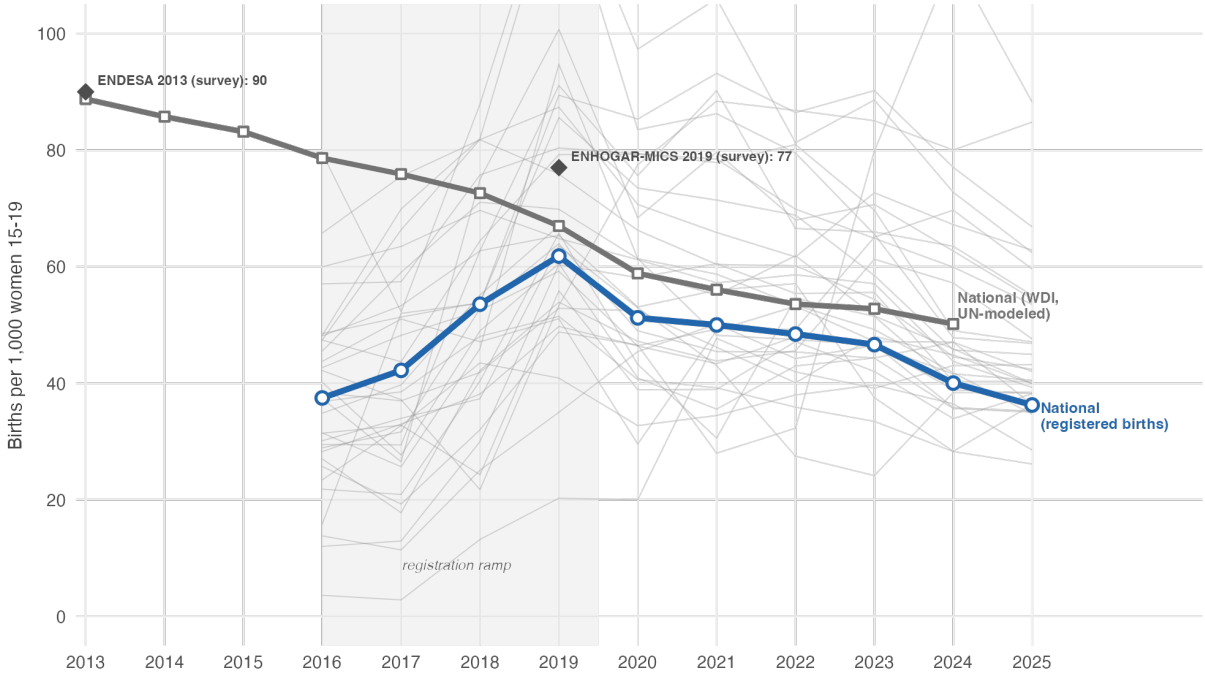
Adolescent childbearing remains a persistent challenge in Latin America, and the Dominican Republic (DR) has long had one of the region's highest teen birth rates (World Bank, 2026, Appendix Figure B1). Survey evidence points to both unintendedness and sharp socioeconomic disparities. Two-thirds of adolescent mothers reported that their pregnancy occurred earlier than desired, while 29.7 percent of women aged 15–19 in the poorest wealth quintile had already given birth, compared with 4.3 percent in the richest quintile.[1] The World Health Organization's adolescent-friendly health services (AFHS) model responds by offering confidential, youth-oriented care intended to reduce barriers to contraception and counseling (World Health Organization, 2012; World Health Organization and UNAIDS, 2015). Governments across the region are investing in this model, yet causal evidence on whether it reduces adolescent fertility remains limited.

In the DR, adolescent pregnancy and unmet need for contraception remain substantial and costly for the government. In the 2013 Demographic and Health Survey (ENDESA-DHS), 27.3 percent of women aged 15–19 had an unmet need for contraception.[2] The United Nations' MILENA costing exercise estimates the annual opportunity cost of adolescent pregnancy at US$245 million, equivalent to 0.29 percent of GDP, including losses from foregone education, earnings, fiscal revenue, and additional health spending (UNFPA República Dominicana, 2021). Adolescent birth rates also changed markedly over the study period. As Figure 1 shows, the registered rate peaked at 61.8 births per 1,000 women aged 15–19 in 2019 and fell to 36.3 by 2025, with declines observed in nearly every province. Because treated and untreated municipalities shared this national decline, an evaluation must distinguish the effect of adolescent units from contemporaneous changes in fertility.

[1] The exact share reporting that they wanted their pregnancy later is 67.7 percent (Oficina Nacional de Estadística (ONE), 2019, Cuadro 14). The wealth-quintile estimates are survey-weighted calculations from the Oficina Nacional de Estadística (ONE) and UNICEF (2022) microdata.

[2] Computed from the ENDESA-DHS 2013 microdata (CESDEM and ICF International, 2014).

Figure 1: Teen birth rates in the Dominican Republic



Notes: Registered adolescent birth rates by province, 2013–2025, with the Ministry of Health national series, the World Bank's World Development Indicators (WDI) series, and survey benchmarks (ENDESA-DHS 2013, ENHOGAR-MICS 2019). Grey lines are the 32 provinces with the national series. Registration completeness rises from roughly 48 percent in 2016 to 92 percent by 2019. The 2016–2018 run-up in registered rates reflects the birth-registration ramp (Section 3.1).

The existing AFHS literature provides its evidence on service utilization rather than fertility outcomes. Early reviews of youth-friendly and adolescent-friendly health services found that multicomponent interventions, including provider training, facility improvements, and community outreach, could increase adolescents' use of care, whereas stand-alone youth centers were generally less effective (Ross et al., 2006; Denno et al., 2015). At the same time, implementation fidelity has often been limited: the full package of adolescent-friendly services is rarely delivered consistently, and in settings where fidelity was assessed, fewer than one-third of facilities reported implementing key standards (Chandra-Mouli et al., 2015). More importantly, the evidence has largely stopped upstream, focusing on access and utilization rather than downstream fertility outcomes. A Lancet review identified rigorous evaluations of youth-friendly services on health outcomes as a leading research priority (Tylee et al., 2007). Recent evidence from low- and middle-income countries shows that adjacent interventions can reduce adolescent fertility: Kaytan et al. (2026) find that a teen-pregnancy initiative combining free access to long-acting reversible contraceptives with school-based information campaigns reduced teen births in Costa Rica, while Angrist and Anabwani (2026) shows that near-peer delivery of school-based sex education reduced adolescent pregnancy in Botswana. But these studies identify the effects of contraceptive provision and information delivery rather than adolescent-friendly service delivery itself, which operates by changing the accessibility and mode of care within the health system. To my knowledge, no study has estimated the effects of a

nationwide roll-out of dedicated AFHS services on adolescent fertility at the population level in a developing country.

This paper asks whether expanding adolescent-friendly health services through dedicated public-hospital units reduces adolescent fertility, and through which margins. This intervention margin differs from programs that expand contraceptive access alone. In the context I study, the adolescent units aim to change how adolescents enter the health system by offering confidential, youth-oriented care inside existing public hospitals. I evaluate the national expansion of an AFHS program in the Dominican Republic: the *Unidades de Atención Integral para Adolescentes* (Adolescent Units, AUs), which are dedicated spaces inside public hospitals that provide adolescents with multiple services such as sexual and reproductive health counseling, contraceptive services — including oral and progestin-only pills, injectable contraceptives, intrauterine devices, and condoms — and psychological and gynecological care.[3] Between 2020 and 2025, the expansion introduced the first AU in 20 municipalities that previously lacked adolescent-friendly services, through six annual waves.

The primary estimand is the effect of a municipality's first AU opening on the municipality-level adolescent birth rate, measured as births per 1,000 women aged 15–19. I exploit geographic variation in the AU roll-out and employ a differences-in-differences design. Because the expansion proceeded gradually across municipalities while many municipalities remained untreated or not yet treated during the study period, the design compares changes in adolescent fertility in municipalities that open a first AU with changes in municipalities not yet exposed to a first AU. I reconstruct the timing of these openings from National Health Service (NHS) records, press documentation, and field visits.

I combine this staggered roll-out with the Ministry of Health (MoH)'s universe of registered live births from 2016 to 2025, comprising 1.44 million births assigned to the mother's municipality of residence, of which 219,068 were to mothers aged 15–19, and with municipality-level population denominators constructed for the post-2020 period. In addition to birth records, I assemble independent hospital-production registries that measure adolescent pregnancies and abortion-related attendances, allowing the analysis to test whether the birth results are mirrored on the broader pregnancy margin. The data therefore allow me to trace whether any decline in births is mirrored in the broader pregnancy margin and whether the response is concentrated at ages and locations most consistent with voluntary use of adolescent reproductive health services. The main specification applies the heterogeneity-robust staggered difference-in-differences estimator of Callaway and Sant'Anna (2021) across 146 municipalities. A complementary within-municipality triple-difference design compares adolescents with women aged 30–34, the youngest cohort that could not have received AU services as adolescents during the study period.

The rollout occurred within a broader effort to reduce adolescent pregnancy. Earlier plans

[3]The methods listed are those the units report dispensing on the NHS monthly AU reporting form (*Métodos de Planificación Familiar entregados por el servicio*), which also records post-obstetric contraception.

had called for differentiated adolescent services, but a 2018 diagnosis described implementation as fragmented and reliant on pilots (Gabinete de Coordinación de Políticas Sociales, 2019). A 2019 multisectoral plan and clinical guidance then established new targets and a rights-based model of comprehensive adolescent care (Gabinete de Coordinación de Políticas Sociales (GCPS), 2019; Ministerio de Salud Pública, 2019). Other changes overlapped with the study period, including the 2021 prohibition of marriage before age 18, a policy addressing early unions and adolescent pregnancy, expansion of the extended school day, and the COVID-19 pandemic (Congreso Nacional de la República Dominicana, 2021; Gabinete de Niñez y Adolescencia (GANA-RD) and CONANI, 2021; Garganta et al., 2026). Section 2 describes this institutional setting in detail. The empirical design must therefore separate AU openings from both the national fertility decline and these concurrent policies.

Opening a municipality's first AU reduces the adolescent birth rate by 6.4 births per 1,000 women aged 15–19 ($p = .002$), or 12 percent of the pre-opening mean. The response follows gestational timing: births remain unchanged during the first nine months and then decline, reaching 17 percent of the baseline rate by the second year of exposure. Conceptions begin to fall at age 17, birth rates fall most at ages 18 and 19, and the response extends into ages 20 and 21 as exposed adolescents age beyond formal eligibility. Independent hospital registries show a 17 percent decline in adolescent pregnancy events, while abortion-related attendances also fall.[4] This pattern is more consistent with fewer conceptions than with substitution toward recorded termination.

The effects are largest where adolescents live closer to the public hospitals that host the AUs. Other baseline moderators produce smaller and less precise differences, suggesting that the existing health system complements the new units. Schooling outcomes also move in a favorable direction, although those estimates are exploratory. Applying the fertility estimate to treated municipalities' exposure implies approximately 3,300 averted registered adolescent births. Because program costs are unavailable, I use the United Nations' MILENA valuations (UNFPA República Dominicana, 2021) to report break-even cost thresholds rather than a cost-effectiveness ratio.

The program's placement rules and the timing and age profile of the estimated effects support the identification strategy. Government planning documents prioritized municipalities with larger adolescent populations, and the data show that actual placement was driven by population scale rather than by adolescent fertility levels: after conditioning on population, the baseline adolescent birth rate does not predict treatment. Among treated municipalities, baseline fertility also does not predict opening order. Because population is already the denominator of the outcome, the design addresses scale through within-municipality comparisons and complementary weighting and matching exercises rather than by conditioning on log population in the main rate regression.

[4] Abortion is prohibited under all circumstances in the DR (Ishola et al., 2025). The hospital series records abortion-related attendances, including miscarriage care and complications of clandestine terminations, rather than all induced abortions. Clandestine terminations that do not reach public facilities are unobserved, so the comparison is informative about the direction of recorded care, not the level of pregnancy termination.

Event-study estimates show no differential trends before an opening, and a placebo based entirely on pre-rollout treatment dates yields a null estimate. The within-municipality comparison group is women aged 30 to 34, the youngest cohort that could not have received the program as adolescents during the study period. Consistent with that choice, the estimated effect is largest among women aged 15 to 19, declines through their twenties, and reaches zero at ages 30 to 34. The extended school day program studied by Garganta et al. (2026) also expanded similarly in treated and untreated municipalities and shows no discrete change around AU openings.

This paper makes three contributions. First, it provides, to my knowledge, the first quasi-experimental, population-level evaluation of a national AFHS rollout on adolescent fertility. Reviews of youth-friendly services find that multicomponent interventions can increase care use, but implementation is often incomplete and evidence on downstream health outcomes remains scarce (Ross et al., 2006; Denno et al., 2015; Chandra-Mouli et al., 2015; Tylee et al., 2007). The closest antecedent, Branson and Byker (2018), finds delayed first births among South African women exposed to accredited adolescent-friendly clinics. I instead estimate the population-level fertility effect of dedicated units embedded in public hospitals and show that births, pregnancies, and age-specific fertility move together after exposure.

Second, the paper identifies a distinct supply-side margin in a developing-country setting. Existing quasi-experimental evidence often studies contractions in reproductive-health access or expansions in high-income settings (Packham, 2017; Fischer et al., 2018; Galárraga and Harris, 2021; Lindo and Packham, 2017; Lovenheim et al., 2016). Evidence from low- and middle-income countries more often concerns schooling, income, information, or combined contraceptive and education programs (Duflo et al., 2015; Baird et al., 2011; Dupas, 2011; Berthelon and Kruger, 2011; Kearney and Levine, 2015; Kaytan et al., 2026; Angrist and Anabwani, 2026). The AU rollout isolates a different intervention by changing the accessibility and mode of reproductive-health care within the public hospital system in an LMIC setting.

Third, the paper shows how complementary administrative systems can strengthen fertility research when no source is complete. I combine MoH birth records with independent NHS registries of pregnancy and abortion-related care, construct small-area population denominators anchored to official data (Hamilton and Perry, 1962; Smith et al., 2013), and address incomplete birth registration explicitly (González-Rodríguez et al., 2024). I also translate the estimated reduction into break-even program budgets of approximately US$48,000 to US$560,000 per municipality-year. These thresholds provide benchmarks for future cost data; they do not establish that benefits exceed costs.

The remainder of the paper proceeds as follows. Sections 2 and 3 describe the program and data. Section 4 presents the empirical strategy. Sections 5 and 6 report the results and mechanism evidence, and Section 7 assesses robustness. Sections 8 and 9 discuss economic value, interpretation, and policy implications. Section 10 concludes.

2 The Unidades de Atención Integral para Adolescentes (AU) Program

An AU is a dedicated service located within an existing public hospital. Each AU occupies a separate space, operates during regular weekday hours, and serves adolescents aged 10 to 19 of both sexes through a multidisciplinary team. The NHS standardizes the model nationally following the WHO's AFHS framework (World Health Organization and UNAIDS, 2015; Ministerio de Salud Pública (PRONAISA), 2019). Services include sexual and reproductive health education and counseling, pregnancy and STI prevention, obstetrics-gynecology care, health education, and psychological support. Care is confidential and free at the point of use.[5]

The AU comprehensive model is a recent development within the public health system, even though adolescent-focused initiatives also emerged through other institutions before the roll-out studied here. A national prevention plan for 2011–2016 had called for differentiated adolescent services, but a 2018 situational assessment found implementation weak, fragmented across institutions, and reliant on pilots rather than universal provision (Gabinete de Coordinación de Políticas Sociales, 2019). Program officials likewise describe the services adolescents encountered in that earlier period as primarily contraception-focused.[6]

Outside the MoH/NHS hospital system, the Ministerio de la Mujer opened in Santo Domingo the CentroPSIA in 2015, with KOICA support, to provide group-based education and counseling on sexual and reproductive health, violence, family dynamics, and life planning (Ministerio de la Mujer de la República Dominicana, 2020). That initiative broadened the policy environment around adolescent well-being, but it was not the hospital-embedded AU service model analyzed in this paper. Because CentroPSIA was already operating in Santo Domingo before the roll-out studied here, it does not generate the staggered municipal treatment variation used in this paper.

The multisectoral PREA-RD plan marked the next step in the development of the AU model. Adopted in 2019, it established targets under a multisectoral and comprehensive approach to reducing adolescent pregnancy (Gabinete de Coordinación de Políticas Sociales (GCPS), 2019). In the same year, the Ministry of Public Health codified a rights-based model of comprehensive adolescent care, with particular emphasis on sexual and reproductive health, through a ministerial resolution (Ministerio de Salud Pública, 2019). The guide defines a differentiated service with exclusive space, signage, and hours inside an existing health facility, staffed by a multidisciplinary team of trained providers operating under WHO-derived quality standards. The 2020–2025 AU roll-out that generates the identifying variation in this paper represents the operational expansion of that comprehensive model.

[5] Free provision at the point of use is documented through fieldwork rather than the press record.

[6] I conducted interviews with officials from the adolescent programs of the MoH and the NHS. They characterized earlier services as focused primarily on family planning, described the current AUs as the first model to provide comprehensive adolescent care and to treat adolescents explicitly as rights holders, and dated provider training on WHO-derived quality standards to 2019.

The AUs' physical design and intake procedures indicate how they may reduce barriers to care. Figure 2 shows the entrance to an AU at Hospital Regional Docente Juan Pablo Pina, with clear adolescent-specific signage and a posted intake notice. The AUs are visibly marked and organized as adolescent-specific spaces rather than generic hospital rooms, consistent with the WHO framework's emphasis on making services more accessible to adolescents. At this site, the notice at the entrance states that patients do not require an appointment (*no requieren citas*) and are served in order of arrival during morning and afternoon shifts. This walk-in arrangement reduces the advance scheduling required to obtain care and provides the institutional basis for interpreting the AUs as lowering the fixed costs of, and barriers to entry into, seeking reproductive health services.

Figure 2: Entrance to an Adolescent Unit inside a public hospital



Notes: Entrance to the AU at Hospital Regional Docente Juan Pablo Pina, San Cristóbal. The signage identifies the AU as a dedicated service for adolescents inside an existing public hospital. The posted notice states that patients do not require an appointment and are served in order of arrival during morning and afternoon shifts. Appendix Figure B6 provides additional images of the AUs' physical spaces, and public presentation.

Two additional institutional features matter for the analysis. First, the AUs use space within existing hospitals and draw their personnel from the public hospital system, so their incremental costs consist primarily of staffing and service organization rather than capital expenditures on new construction. This institutional structure motivates the break-even calculations in Section 8. Second, locating the AUs inside hospitals links access to adolescents' proximity to and prior contact with the public health system. The larger estimated effects among adolescents living closer to public

hospitals, reported in Section 6, are consistent with this access channel.

Dedicated AUs predate the comprehensive model analyzed in this paper. The first AU opened in Santo Domingo Este in 1993, followed by AUs in Baní and Santo Domingo Oeste in 1998. By 2016, nine municipalities, primarily large metropolitan areas, already had at least one AU. The identifying variation in this paper therefore does not come from the existence of AUs per se, but from the subsequent expansion of first AUs into municipalities without previous adolescent-friendly services. Between 2020 and 2025, under the national adolescent-pregnancy agenda described in the introduction and with public sponsorship from the Office of the First Lady and the NHS, the government introduced the first AU in 20 additional municipalities through six annual waves.

Government planning documents describe site selection primarily in terms of demographic scale and recorded adolescent births, subject to the availability of an NHS hospital. The 2019 national plan identified territorial coverage gaps in provinces and municipalities with the largest adolescent populations, specifically naming Santo Domingo and Santiago (Gabinete de Coordinación de Políticas Sociales (GCPS), 2019). The December 2021 policy then identified 20 priority municipalities using population and registered adolescent-birth counts, explicitly assigning weight to population density and noting that this criterion could underrepresent low-density rural communities (Gabinete de Niñez y Adolescencia (GANA-RD) and CONANI, 2021). In practice, however, the priority list did not map mechanically into treatment. Of the 20 priority municipalities, seven already had AUs before 2016, eight received their first AU during the 2020–2025 expansion, and five remained untreated during the study period. Conversely, 12 of the 20 newly treated municipalities do not appear on the priority list. Hospital availability imposed an additional constraint because AUs operate inside existing hospitals: all 20 treated municipalities have an NHS hospital, compared with 78 percent of never-treated municipalities. Hospital presence was therefore necessary but not sufficient for treatment. Section 4.6 shows formally that actual placement was predicted by population scale rather than by adolescent fertility rates.

3 Data

I construct a municipality-by-year panel for the Dominican Republic covering 2016–2025. The panel combines the paper's primary outcome, the adolescent birth rate, with municipality-level population denominators and AU opening dates. I then describe the treatment coding and the municipality characteristics used in the descriptive, adjusted, and heterogeneity analyses. I also assemble two complementary administrative sources: NHS hospital-production registries, which provide independent measures of adolescent pregnancies, abortion-related attendances, and prenatal consultations, and MoE school records, which provide enrollment and dropout outcomes. The section concludes with the estimation sample and descriptive statistics.

3.1 Birth Records and Population Denominators

Birth counts come from the Ministry of Public Health's national live-birth registration database (BDNV). Each record reports the mother's age, municipality of residence, municipality and facility of delivery, nationality, insurance status, and delivery method, together with the newborn's birth weight, gestational age, and size for gestational age.[7] I assign births to the mother's municipality of residence, so the outcome measures fertility among residents rather than hospital caseloads. The main outcome is the number of births to mothers aged 15–19 per 1,000 resident women in that age group. Exact birth dates also permit monthly aggregation for the gestational-timing analysis using the 17 first openings with a documented month (Section 5.2).

Birth registration was incomplete early in the study period but reached approximately 92 percent in 2019 and remained stable thereafter.[8] Because all treatment cohorts in the primary design open in 2020 or later, the 2016–2018 increase in registration completeness occurred before treatment. Restricting the analysis to 2019–2025 yields the same main estimate (Section 7). Incomplete registration therefore affects measured levels more directly than the identifying variation. If registration completeness is unaffected by treatment, the estimated number of averted registered births is a conservative estimate of total averted births.

No official source publishes municipality populations by age and sex after 2020. I therefore construct the main denominator series, Series A, by combining NSO's published municipality estimates for 2016–2020 with its province projections through 2025. I allocate the province projections across municipalities using Hamilton–Perry cohort-change ratios and then rake the resulting municipality estimates to the published province totals (Hamilton and Perry, 1962; Smith et al., 2013). A validation exercise that predicts the 2022 census from the 2015–2020 series yields mean absolute percentage errors of 8.1 percent across national age-sex cells and 14.7 percent across municipality cells. Series A modestly overstates the adolescent population relative to the 2022 census and therefore understates birth-rate levels. As an alternative, I construct Series B by interpolating between the 2010 and 2022 censuses. Estimated treatment effects are insensitive to the choice of denominator (Appendix Table A19). The projections cover every five-year age group from 10–14 through 30–34, as required for the age-gradient and triple-difference analyses. Appendix Appendix C details both constructions, and the separate supplement with complete municipality-by-age-by-year projections accompanies the replication package.

[7] The availability of maternal characteristics varies over time. Insurance status is recorded beginning in 2018, education in 2020, and union status and prenatal care in 2021. These variables enter only the descriptive and composition analyses.

[8] The World Bank's World Development Indicators series `SP.REG.BRTH.ZS` reports 92.2 percent completeness in 2019.

3.2 Treatment Definition and Timing

The primary treatment measure records the opening of a municipality's first AU. A municipality becomes treated in the year its first AU opens and remains treated thereafter; subsequent openings do not change its treatment status. I compile opening dates from official NHS records, national and local press reports, and field visits. These sources identify 39 AUs in 29 municipalities. Twenty-eight AUs provide the comprehensive service package, while 11 offer specialized adolescent consultations. Because the available documentation does not support a consistent measure of service intensity, the primary treatment indicator records any first AU opening regardless of its reported service mix.

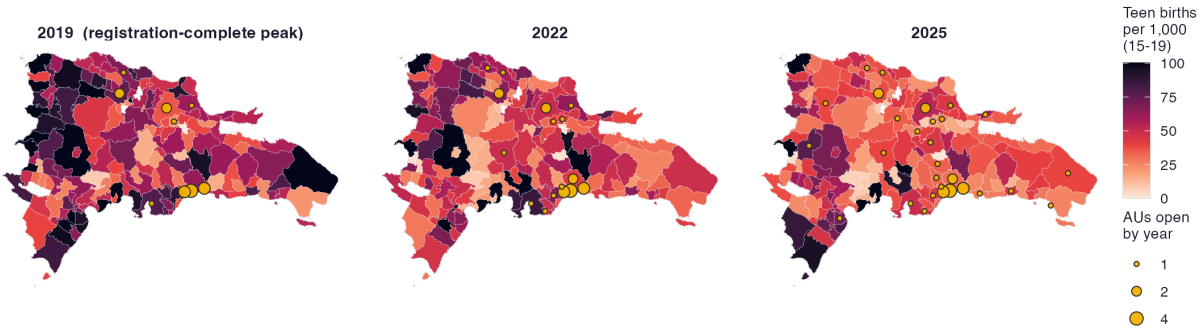
Nine municipalities received their first AU before 2016 and are excluded from the primary design because they have no untreated observations. The identifying variation therefore comes from first openings in 20 municipalities during 2020–2025, with annual cohort sizes of 2, 2, 3, 6, 6, and 1. Seventeen opening dates are documented by the NHS or verified through fieldwork. Dates are unavailable for La Romana, San Pedro de Macorís, and Yamasá, which I assign to 2023, the midpoint of the expansion. Dropping these municipalities or varying their assigned dates does not materially change the estimates (Section 7). Figure 3 summarizes the expansion. Panel A maps open AUs against adolescent birth rates over 2019–2025, while Panel B shows the timing of first openings. The figure makes clear that AUs cluster in large urban municipalities even as adolescent birth rates decline nationally, and that many municipalities with the highest rates never receive an AU.

As a complementary specification, I also define treatment using the arrival of the contemporary comprehensive-service model. A qualifying modernization event is either the opening of a new comprehensive AU or a documented modernization or remodeling of a pre-existing AU that expanded care beyond contraception-focused services. A municipality's modernization year is the earliest qualifying event among its AUs, after which treatment remains absorbing. This modernization design covers 2017–2025 and includes 29 treated municipalities, of which 22 enter through new openings and seven through upgrades. All nine municipalities served before 2016 experienced a qualifying modernization during the study period and therefore re-enter this complementary design. The resulting sample therefore contains the full study geography of 155 municipalities: 29 modernized municipalities and 126 municipalities that never received an AU. For each AU, I use the documented modernization date when available and otherwise use its recorded opening year. Of the 29 cohort-defining dates, 24 are supported by a documented modernization event and five rely on a recorded but independently unverified opening year; none requires midpoint imputation.[9] The full AU-level record, listing each AU's opening and modernization date together with its source tier, appears in Appendix Table A21.

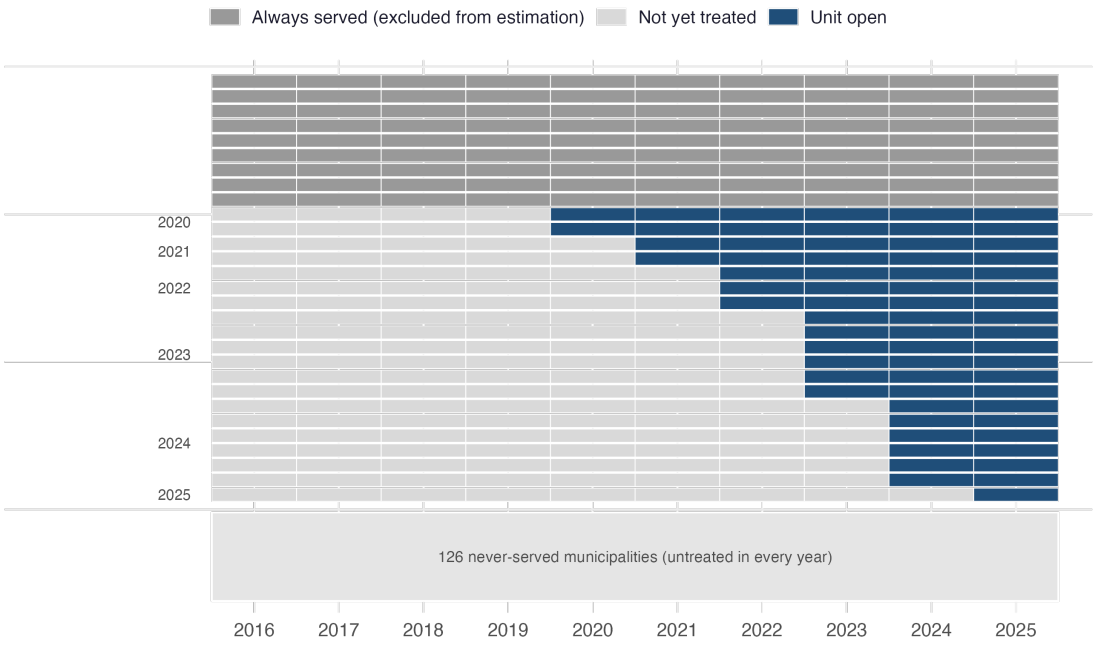
[9]The five dates based on recorded but independently unverified opening years are Santiago (2017), Castillo (2021), Las Matas de Farfán (2024), Pedro Brand (2024), and San Ignacio de Sabaneta (2025).

Figure 3: The AUs roll-out

(a) Openings and the teen birth rate, 2019–2025



(b) Treatment timing



Notes: Panel (a): municipalities are shaded by their birth rate per 1,000 women aged 15–19; yellow markers denote municipalities with a AU open in that year, sized by the number of AUs; the fill is winsorized at the 95th percentile. The panels start in 2019 because 2016–2018 are a birth-registration ramp (Appendix C.2). Panel (b): each row is one of the 29 municipalities that ever has a AU, ordered by the year its first AU opened; gray cells are years in which a AU is open, and the 126 never-treated municipalities are collapsed into the band at the bottom. The expansion into twenty previously unserved municipalities over 2020–2025 provides the identifying variation.

3.3 Municipality Characteristics

Municipality characteristics serve three purposes in this study: they describe pre-existing differences between treated and never-treated municipalities, enter the covariate-adjusted robustness specifications, and define several heterogeneity splits. The baseline specification does not condition on covariates. When I use them in adjusted specifications, I enter them as time-invariant municipality characteristics rather than annually updated measures that could themselves respond to treatment (Caetano and Callaway, 2026).

The characteristics include a wealth index, female secondary education, urbanization, public health facilities per 10,000 residents, affiliation with the subsidized public insurer SeNaSa in 2013, and the share of households in the bottom two national income quintiles. Wealth, education, and urbanization come from the 2022 census obtained from NSO. Facility density is constructed from the NHS registry of 1,588 licensed public hospitals and first-level care centers. The share of households in the bottom two national income quintiles comes from the 2018 Survey of Income and Expenses (ENIGH) household budget survey and is measured at the province level.

The wealth index is the first principal component of 29 census indicators covering household assets, housing conditions, crowding, and access to basic services. Table A15 describes its construction. Its municipality mean correlates at -0.91 with the independently measured SIUBEN quality-of-life index. The sign is negative because the two scales run in opposite directions, while the magnitude indicates that the index captures the expected socioeconomic gradient.

Because the 2022 census was conducted during the roll-out, some of its measures could partly reflect post-treatment conditions. I assess this concern using analogous wealth and female-education measures from the 2010 census, collected a decade before the first in-window opening. The 2010 measures correlate at 0.95 and 0.92, respectively, with their 2022 counterparts, suggesting that the 2022 variables primarily capture persistent cross-sectional differences rather than treatment-induced changes (Appendix Table A9).

3.4 Additional Outcomes and Heterogeneity Measures

3.4.1 Hospital-Production Registries

Monthly hospital-production registries provide independent administrative measures of adolescent pregnancies and health-service use. Every public facility files Form 67-A to the NHS, reporting deliveries, abortion-related attendances, and prenatal consultations by maternal age group and attending facility. I digitize all published reports from 2015 through early 2026 and assign approximately 210 reporting facilities to municipalities. More than 99 percent of adolescent events are successfully assigned.

Over 2015–2025, the registries contain 262,428 deliveries and 29,508 abortion-related attendances among women aged 15–19, for a total of 291,936 adolescent pregnancy events. Because abortion is criminalized without exception in the DR, recorded attendances combine miscarriage

care with complications of clandestine terminations and cannot measure the incidence of induced abortion (Ishola et al., 2025). Stillbirth reporting begins in 2022 and contains 1,017 adolescent events. The hospital series correlates at 0.96 with MoH registry births aggregated by municipality of delivery. Because Form 67-A is collected through a separate administrative channel, it provides an independent validation source rather than a mechanical re-expression of the birth registry.[10] Section 5.3 uses these data to examine pregnancy and abortion-related events, while Section 6 uses prenatal consultations as a proxy for service use.

3.4.2 Schooling Outcomes

Schooling outcomes come from the Ministry of Education (MoE)'s school-level administrative records for academic years 2015–2016 through 2024–2025. The records report enrollment by grade and sex, within-year dropout, and each school's schedule regime. I aggregate them to municipalities using school identifiers. Section 5.5 uses these records to study secondary enrollment, dropout, and examination outcomes, while Section 7.3 uses the schedule data to measure the expansion of the extended school day program, *Jornada Escolar Extendida*. Because the records do not report student age, I calculate gross secondary enrollment as total secondary enrollment divided by the population ages 12–17. School performance comes from the *Pruebas Nacionales*, which is the national examination taken at the end of secondary school, providing center-level first-convocatoria results for 2016–2024, with no score reported in 2020 and no examination administered in 2021. The 2024 wave is constructed from first-convocatoria student microdata provided directly by MoE and linked to municipalities through MoE's official directory of education centers.

3.4.3 Heterogeneity Measures

The heterogeneity analysis in Section 5.4 divides the 20 treated municipalities at the median of prespecified baseline moderators. For annually measured moderators, I use the value in the year before the municipality's first AU opened; cross-sectional census and survey measures are held fixed at their observed values. Several moderators use the municipality characteristics described above. Additional measures capture proximity to public health services, baseline contraceptive access, adolescent vulnerability and school attachment.

To measure proximity, I geolocate the 1,588 facilities in the NHS registry, comprising 1,391 first-level centers, 147 intermediate-complexity hospitals, and 50 high-complexity hospitals. For each municipality-year, I calculate the straight-line distance from the municipality centroid to the nearest facility operating in that year. I also construct a two-step floating catchment access measure that summarizes congestion-adjusted facility access within overlapping 20-km catchments. The national median distance to the nearest facility is 2.8 kilometers, and the median among treated

[10]The NHS confirmed in writing that AU-level utilization statistics do not exist and that Form 67-A is the available source for hospital-recorded pregnancies.

municipalities is 3.0 kilometers.

Baseline contraceptive access comes from the 2013 ENDESA-DHS microdata. I construct province-level measures of unmet need for family planning, contraceptive prevalence, and satisfied demand among women aged 15–49 and assign them to municipalities within each province (CESDEM and ICF International, 2014). For context, the national unmet-need rate among women aged 15–19 is 27.3 percent. Because these measures vary only across provinces, they enter the heterogeneity analysis but not the municipality-level main specification. The 2019 ENHOGAR-MICS microdata provide the complementary national benchmarks reported in the descriptive statistics (Oficina Nacional de Estadística (ONE) and UNICEF, 2022).

Finally, I construct four measures of adolescent vulnerability and school attachment from the 2019 *Evaluación Diagnóstica* family questionnaire collected by MoE, which includes the share of students frequently absent from school, the share whose parents participate in school activities, and the share whose families expect them to complete university. I aggregate these measures to the municipality level using family weights and use them in the heterogeneity analysis.

3.5 Estimation Sample and Descriptive Statistics

The estimation sample contains 146 municipalities observed annually from 2016 through 2025, yielding 1,460 municipality-year observations. It includes 20 municipalities that receive their first AU during the study period and 126 that never receive an AU. Nine municipalities served before 2016 are excluded because they have no untreated observations in the primary design.

Table 1 reports sample characteristics and pre-treatment outcomes. Panel A describes individual birth records in 2019, the final year before any in-window opening and the first year after the registration ramp had stabilized. Panel B reports municipality-level fertility outcomes, population scale, schooling, and socioeconomic characteristics. Panel C provides ENDESA-DHS 2013 and ENHOGAR-MICS 2019 benchmarks for contraceptive access and characteristics unavailable in the pre-treatment birth registry.

The main difference between treated and never-treated municipalities is population scale rather than baseline adolescent fertility. Although treated municipalities represent only 13.7 percent of the estimation sample, they account for 47.1 percent of registered adolescent births in 2019. They averaged 6,687 women aged 15–19, compared with 1,281 in never-treated municipalities ($p = .003$), and recorded 461 versus 82 adolescent births ($p = .004$). The same scale difference appears in the hospital-production data: treated municipalities recorded substantially more adolescent pregnancy events and abortion-related attendances at baseline. By contrast, their 2019 adolescent birth rates were 66.1 and 60.9 per 1,000, respectively ($p = .327$), and the corresponding 2016–2019 averages were 50.1 and 45.5 ($p = .303$). Province-level contraceptive-access measures also do not differ precisely across groups. At the birth-record level, estimated differences in maternal age, age composition, nationality, delivery method, and facility type are small or imprecise. Treated municipalities are wealthier and more educated and have greater household internet access. They also have lower

public-facility density per 10,000 residents, despite being larger and more urban. These socioeconomic differences motivate the covariate-adjusted and matched-control analyses in Section 7.2. Table 1 reports standardized differences so that balance can be compared across counts, rates, and indices; Appendix Table A10, Panel B uses the same metric before and after matching.

Table 1: Baseline characteristics of treated and comparison municipalities

	Treated (20)	Never-treated (126)	*p*-value	Std. difference
Panel A. Birth-record level: teen (15–19) births, 2019 (N = 19,580)				
Number of births	9,223	10,357		
Mother's age (mean)	17.6	17.6	.517	
% mothers aged 15–17	40.5	42.0	.406	
% mothers aged 18–19	59.5	58.0	.406	
% Haitian mothers	11.6	10.3	.325	
% without health insurance (N = 17,029)[a]	62.4	75.5	.162	
% c-section	48.7	50.3	.617	
% public-facility births	85.8	83.8	.658	
Panel B. Municipality level (N = 146)				
Teen birth rate, 2019 (per 1,000 women 15–19)	66.1	60.9	.327	0.19
Teen birth rate, 2016–19 average	50.1	45.5	.303	0.23
Teen births, 2019 (mean count)	461	82	.004	1.01
Teen pregnancy events, 2019 (mean count)[h]	513	69	.001	1.18
Teen abortion-related attendances, 2019[h]	50	7	.001	1.15
Women aged 15–19 (2016)	6,687	1,281	.003	1.05
Gross enrollment ratio, secondary (2018–19)	85.2	82.4	.390	0.20
Dropout rate (pp of enrolled, 2018–19)	2.9	3.9	<.001	-0.74
Grade-6 (terminal) enrollment ratio (2018–19)	10.1	9.2	.175	0.32
Wealth index (2022 census, 29-item PCA)	-0.19	-0.58	<.001	1.33
% women 20–49 with secondary or more	77.4	67.3	<.001	1.33
% households with internet	44.9	36.7	.003	0.65
% urban	64.4	55.5	.086	0.44
% in bottom two income quintiles (2018)	40.9	49.2	.007	-0.65
Child-woman ratio, 2010 census	374	419	<.001	-0.63
Public health facilities per 10,000 residents	1.9	3.3	<.001	-0.95
% enrolled in SeNaSa (DHS 2013)	35.9	37.8	.559	-0.14
Panel C. Survey benchmarks (ENDESA 2013; ENHOGAR-MICS 2019)				
% unmet need for contraception, women 15–49 (N = 146)[p]	9.8	11.0	.224	-0.31
% modern contraceptive prevalence, women 15–49 (N = 146)[p]	71.6	69.8	.201	0.28
% women 15–19 ever given birth (N = 3,827)		16.1		
poorest wealth quintile (N = 1,061)		29.7		
Teen mothers: % partnered (N = 646)		63.6		
Teen mothers: years of education (N = 646)		10.0		
Teen mothers: prenatal visits (N = 599)		9.2		

Notes: All quantities are pre-treatment unless noted otherwise; the first cohort opens in 2020. Sample sizes appear in row labels when they differ from the panel total. Reported *p*-values come from Panel A regressions on 2019 records with municipality-clustered SEs and Panel B Welch *t*-tests across municipalities, excluding the nine served before 2016. [a]Insurance is recorded from 2018 (2,551 records lack it); education, union status, and prenatal care begin in 2020–21 and therefore appear only in Panel C. [h]Hospital-registry (Form 67-A) events at the attending facility's municipality; municipalities without a reporting facility are zeros. Pregnancy events = deliveries + abortion-related attendances, ages 15–19. The standardized difference divides the mean difference by the pooled standard deviation across municipalities, $\sqrt{(s_T^2 + s_C^2)/2}$. Large values for counts and population reflect the documented scale-based placement criterion (Table 2); identification relies on parallel trends rather than baseline balance. Appendix Table A10, Panel B reports covariate balance before and after matching in the same metric. Schooling rows are for the school year ending in 2019; gross enrollment is any-age secondary enrollment per 100 population aged 12–17. The wealth index is the first principal component of 29 census items (Appendix Table A15). [p]ENDESA province measures for women aged 15–49 are assigned to municipalities; tests cluster at the province. MICS quantities are national, weighted, and admit no arm comparison; teen mothers are women aged 15–19 with a birth in the 24 months before the interview. *Sources:* BDNV; Form 67-A registries; MoE; 2022 census; NSO; NHS registry; SIUBEN; ENDESA 2013; ENHOGAR-MICS 2019.

4 Empirical Strategy

4.1 Staggered difference-in-differences design

The primary design estimates the effect of a municipality's first AU opening on its adolescent birth rate by comparing each treated cohort with municipalities that remain untreated at time t, including both later-treated municipalities that have not yet opened an AU and municipalities that never open one during the study period. Treatment is binary and absorbing: once a municipality opens its first AU, it remains treated thereafter. The primary specification is implemented on the 146 municipalities in the first-opening sample: 20 municipalities treated during 2020–2025 and 126 never-treated municipalities. I implement this design using the heterogeneity-robust estimator of Callaway and Sant'Anna (2021), which is well suited to staggered adoption when treatment effects may vary across cohorts and over time.

Let Y_{mt} denote births to women aged 15–19 per 1,000 resident women aged 15–19 in municipality m and year $t \in \{2016, \ldots, 2025\}$. Let G_m denote the year in which municipality m receives its first AU, with $G_m = \infty$ for never-treated municipalities. The primary group-time estimand is

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0) \mid G_m = g], \tag{1}$$

where $Y_t(g)$ is the potential outcome when treatment begins in year g and $Y_t(0)$ is the potential outcome without treatment. Thus, $ATT(g,t)$ is the effect in year t for municipalities receiving their first AU in year g.

I report an overall aggregation across treated cohorts and observed post-treatment periods, together with event-study averages

$$\theta(e) = \sum_{g:\, 2016 \le g+e \le 2025} \mathbb{P}(G_m = g \mid 2016 \le G_m + e \le 2025)\, ATT(g, g+e), \tag{2}$$

where $e = t - g$ denotes event time. The event-study estimates cover $e = -5, \ldots, 4$, with cohort shares renormalized among cohorts observed at each event time. Percent effects in the primary design are expressed relative to the treated cohorts' mean adolescent birth rate in the year before opening, 55.3 births per 1,000.

As a complementary specification, I replace the first-opening cohort G_m with the modernization cohort G_m^M defined in Section 3.2. Replacing G_m with G_m^M in equations (1) and (2) defines $ATT^M(g,t)$ and the corresponding dynamic effects. I apply the same Callaway and Sant'Anna (2021) estimator, comparison-group rule, and event-time aggregation to the full study geography of 155 municipalities. This modernization specification includes 29 municipalities modernized during 2017–2025 and 126 municipalities that never received an AU. It estimates a broader treatment margin than the primary design: for the 20 municipalities first treated during 2020–2025, it captures the arrival of a new comprehensive AU, while for the nine municipalities already served before 2016,

it captures the first documented arrival of the comprehensive model through either a new AU or an upgrade of existing services. I report this estimate alongside the primary first-opening result in Section 5, but interpret it as a complementary result rather than as the basis of the paper's main identification argument.

4.2 Triple-Difference Design and Comparison Age Group

As a complementary design, I estimate a triple difference that compares the adolescent birth rate with the birth rate among women aged 30–34 within the same municipality-year. The intuition is that municipality-level shocks, such as changes in local economic conditions, health-system capacity, or registration completeness, may affect fertility across age groups. Differencing the adolescent birth rate from the rate among women aged 30–34 removes shocks common to both groups within a municipality-year. I implement this design for both treatment definitions: the primary first-opening design and the complementary modernization design.

Following Ortiz-Villavicencio and Sant'Anna (2025), I use a doubly robust staggered-DDD estimator that avoids aggregating contaminated pairwise comparisons under staggered adoption. Under this implementation, each treated cohort is compared separately with the cohorts that are still untreated at that date, and those valid comparisons are then combined by the estimator rather than pooled naively into a single control group (cf. Olden and Møen, 2022):

$$\begin{aligned} ATT^{DDD}(g,t) = \mathbb{E}\Big[\Delta_t^{g-1}Y^{15\text{–}19} - \Delta_t^{g-1}Y^{30\text{–}34} \;\Big|\; G_m = g\Big] \\ - \mathbb{E}\Big[\Delta_t^{g-1}Y^{15\text{–}19} - \Delta_t^{g-1}Y^{30\text{–}34} \;\Big|\; G_m > t\Big], \end{aligned} \tag{3}$$

where $\Delta_t^{g-1}Y^a$ is the change in the birth rate for age group a between $g-1$ and t.

Replacing G_m with G_m^M yields the corresponding modernization-based triple-difference estimand. The DDD is estimated without covariates. With cohorts containing only one to six treated municipalities in the primary design, and with limited overlap in the modernization design, covariate-adjusted DDD specifications either fail numerically or fail their overlap diagnostics. I therefore report the unconditional estimator rather than extrapolate beyond the common support.

I choose the comparison age group using a rule fixed before estimation: it is the youngest age group that could not have been exposed to an AU as an adolescent. This rule balances similarity to adolescents against the requirement of no possible treatment exposure. AUs serve ages 10–19. Because the first opening in the primary design occurred in 2020, the oldest woman who could have been exposed as an adolescent is 24 by 2025. Under the broader modernization definition, which begins in 2017, the oldest potentially exposed woman is 27. Ages 20–24 may therefore include former adolescent users or births postponed beyond age 19, while ages 25–29 cannot be ruled out as exposed under the modernization definition. Women aged 30–34 are thus the youngest group that is mechanically unexposed under both treatment definitions.

The estimated age profile supports this choice, but does not determine it. In the results,

the response declines beyond adolescence and reaches an estimated zero at ages 30–34, with no statistically distinguishable effect for that group (Section 5). Any negative spillover to women aged 30–34 would attenuate the DDD estimate toward zero, making this comparison conservative under that potential violation.

4.3 Identifying assumptions and estimation

Identification of the primary group-time effects requires parallel trends relative to the untreated comparison cohorts used by the estimator:

$$\mathbb{E}[Y_t(0) - Y_{g-1}(0) \mid G_m = g] = \mathbb{E}[Y_t(0) - Y_{g-1}(0) \mid G_m > t], \qquad t \geq g, \tag{4}$$

That comparison set consists of municipalities untreated at time t, including later-treated and never-treated municipalities. Absent the program, municipalities receiving their first AU in year g must therefore have experienced the same change in adolescent birth rates as municipalities still untreated through year t. The modernization specification imposes the corresponding condition relative to G_m^M: absent modernization, municipalities modernized in year g would have followed the same outcome path as municipalities not yet modernized or never served.

The DDD requires that in the absence of treatment, the difference between the adolescent birth rate and the rate among women aged 30–34 must have evolved similarly in treated municipalities and the valid comparison cohorts. Shocks common to both age groups within a municipality cancel by construction, but age-specific shocks that differentially affect adolescent fertility can still confound the DDD estimate.

Both preferred staggered estimators use municipalities untreated at time t, including later-treated and never-treated municipalities. Specifications that restrict comparisons to never-treated municipalities appear in the robustness analysis.

I do not use two-way fixed effects as the primary estimator because, with staggered adoption and heterogeneous effects, TWFE can combine valid comparisons with comparisons in which already treated municipalities serve as controls (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). TWFE and the heterogeneity-robust estimators of Sun and Abraham (2021), Borusyak et al. (2024), and Gardner et al. (2024) are reported as alternative specifications and yield similar estimates (Appendix Table A1).

Estimation follows Callaway and Sant'Anna (2021) using a universal base period, so each group-time effect is measured relative to the year immediately before treatment, $g-1$. The headline estimates are unconditional. Covariate-adjusted specifications use regression adjustment based on the five time-invariant municipality characteristics described in Section 3.3 (Sant'Anna and Zhao, 2020). I do not condition on annually updated covariates because variables that respond to treatment can bias staggered difference-in-differences estimands (Caetano and Callaway, 2026). Inference uses the multiplier bootstrap clustered at the municipality, the level of treatment assignment.

4.4 Treatment allocation and population scale

The allocation analysis examines selection into the primary first-opening treatment. Government planning documents prioritized municipalities with large adolescent populations and large numbers of registered adolescent births, with explicit weight assigned to population density (Gabinete de Coordinación de Políticas Sociales (GCPS), 2019; Gabinete de Niñez y Adolescencia (GANA-RD) and CONANI, 2021). Table 2 evaluates this count-based allocation rule using the decomposition proposed by Duflo (2001).

Column 1 shows that the log number of adolescent births predicts whether a municipality received its first AU. Because the log birth count equals log adolescent population plus the log adolescent birth rate, up to a constant, allocation based on birth counts implies equal coefficients on the two components. The estimates reject this restriction. In column 3, the coefficient on log adolescent population is an order of magnitude larger than the coefficient on the adolescent birth rate, which is near zero. Equality of the coefficients is strongly rejected ($F = 15.4$, $p < .001$). Hospital presence also does not predict placement after conditioning on population size (column 4). Panel B examines the complementary timing margin: among the 20 municipalities that eventually received an AU, earlier openings did not go to higher-fertility places. The opening year is unrelated to the baseline adolescent birth rate (OLS coefficient 0.04, $p = .97$; Spearman rank correlation -0.03, $p = .89$), and the earliest opening cohorts (2020–2021) had essentially the same baseline rate as the latest (52.6 versus 50.2 per 1,000, $p = .90$).

Actual first-AU placement therefore followed population scale rather than adolescent fertility intensity, on both the extensive and the timing margins. This finding does not establish parallel trends by itself, but it reduces concern that AUs were placed in response to unusually high adolescent birth rates. It also clarifies how scale should be handled in the design. Population is already the denominator of the outcome, and municipality fixed effects absorb time-invariant differences in scale levels. The remaining concern is therefore not level differences in population, but whether municipalities of different scale would have followed different fertility trends absent treatment.

For that reason, the paper addresses scale through within-municipality comparisons, the event-study evidence, and complementary weighting and matching exercises rather than by conditioning mechanically on log population in the main rate regression. In this setting, adding log population as a covariate would answer the scale-selection question by extrapolation, even though the treated municipalities include the largest urban areas and many lack close population counterparts among controls. The matching analysis reported later conditions on population directly on common support, which is the more credible way to assess sensitivity to scale. Because the modernization treatment also includes upgrades to pre-existing AUs, its allocation process need not follow the first-opening rule. I therefore interpret modernization as a complementary specification rather than as the basis of the paper's primary identification argument.

Table 2: Allocation and timing of first Adolescent Unit openings

	Received a first AU, 2020–2025[a]			
	(1)	(2)	(3)	(4)
Panel A. Which municipalities received a first AU (linear probability model)				
Log of the number of adolescent births[b]	0.120***			
	(0.025)			
Log of the number of adolescent women aged 15–19 in the municipality		0.170***	0.169***	0.167***
		(0.028)	(0.028)	(0.030)
Log of the adolescent birth rate[b]			0.012	0.011
			(0.028)	(0.029)
Public hospital in the municipality (0/1)				0.012
				(0.042)
Number of observations	146	146	146	146
R^2	0.20	0.26	0.26	0.26
Panel B. Timing of adoption among the 20 treated municipalities[c]				
Opening year on log baseline birth rate (OLS)	0.04 (1.01), $p = 0.97$			
conditional on log adolescent population	0.33 (1.11), $p = 0.77$			
Spearman rank correlation, opening year × baseline rate	-0.03, $p = 0.89$			
Mean baseline rate, 2020–21 vs. 2023–25 cohorts	52.6 vs. 50.2, $p = 0.90$			

Notes: Heteroskedasticity-robust standard errors are in parentheses. [a]The dependent variable is an indicator equal to one if the municipality received its first AU between 2020 and 2025; Panel A reports linear probability models for the 146 estimation municipalities. [b]Births per 1,000 women aged 15–19; all regressors are 2016–2019 pre-roll-out averages. The number of adolescent births, the documented allocation criterion, is the product of the adolescent population and the birth rate, so allocation on counts implies that the two components of column (3) enter with equal coefficients. [c]Panel B tests whether earlier openings went to higher-fertility municipalities among the 20 treated municipalities. Opening year is regressed on the log baseline rate (HC1 standard errors), the rank correlation is Spearman's ρ, and the last row compares mean baseline rates of the earliest (2020–2021) and latest (2023–2025) opening cohorts (Welch test). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

4.5 Pre-trend, placebo, and sensitivity evidence

I assess the plausibility of the identifying assumptions using event-study leads, placebo exercises, and sensitivity analysis. Appendix Table A4 collects the corresponding results.

First, the dynamic pre-treatment estimates are small and individually statistically insignificant. Joint tests fail to reject that the leads are zero for both the first-opening design ($p = .68$) and the modernization specification ($p = .85$; Appendix Table A3). The modernization evidence should be interpreted somewhat more cautiously because its earliest treated cohort enters in 2017 and therefore contributes a shorter pre-treatment window.

Second, a fake-timing placebo assigns first-opening cohorts counterfeit treatment dates three years before their true opening dates and estimates the effect using only observations before actual treatment. The placebo estimate is -4.84 with a standard error of 7.06 ($p = .49$), indicating no

detectable effect when treatment is known not to have occurred.

Third, I estimate the full first-opening event study using the birth rate among women aged 30–34 as the outcome. The estimated effect is -0.70 with a standard error of 2.40 ($p = .77$), and the dynamic estimates remain flat. This placebo is especially relevant for the triple-difference design because it tests whether the comparison age group itself exhibits a spurious break around AU openings.

Failure to reject pre-trends is not sufficient to establish parallel trends, particularly with a small number of treated municipalities (Roth, 2022). I therefore report sensitivity bounds following Rambachan and Roth (2023). For the primary level estimate, the confidence set excludes zero when post-treatment deviations from parallel trends are allowed to reach 10 percent of the largest pre-treatment deviation ($\bar{M} = 0.1$). For the triple difference, whose pre-period is flatter, the confidence set excludes zero up to $\bar{M} = 0.2$.

Taken together, these exercises do not prove the identifying assumptions, but they show that the results are not driven by visible pre-trends, arbitrary treatment timing, or a break in the untreated comparison age group. The broader identification argument therefore rests on the combined evidence from the event-study leads, the timing placebo, the unaffected 30–34 comparison group, the treatment-allocation analysis, and the independent administrative measures of pregnancies and abortion-related attendances discussed later in the paper.

4.6 Threats to identification

Several potential threats to identification arise in this setting: concurrent policies and shocks, measurement of treatment timing, limited numbers of treated municipalities, spillovers, and anticipation. This subsection explains how I address each in turn.

A first concern is that other policies affecting adolescent fertility changed during the study period. Policies implemented nationally are differenced out through contemporaneous comparisons with untreated municipalities. The most important municipality-varying policy is the extended school day program, *Jornada Escolar Extendida*, which may independently affect adolescent fertility (Garganta et al., 2026). Section 7.3 therefore tests directly whether its expansion is correlated with AU timing and whether controlling for it changes the estimates.

A second concern is measurement and background data quality. Birth-registration completeness increased during 2016–2018 (Section 3.1), but this ramp predates every treatment event in the primary design, whose first cohort opens in 2020. Dropping the ramp years leaves the primary estimate materially unchanged (Section 7). The modernization specification includes a 2017 cohort and is therefore more exposed to differential changes in registration completeness during the ramp. I consequently treat modernization as a complementary specification and rely on the primary first-opening design for the paper's main identifying variation. The roll-out also overlaps the COVID-19 pandemic, but excluding the pandemic years produces a similar primary estimate (Section 7).

Treatment-date measurement presents a related concern. In the primary design, 17 of the

20 first-opening dates are documented by the NHS or verified through fieldwork. The dates for La Romana, San Pedro de Macorís, and Yamasá are unavailable and are assigned to 2023. The primary estimate is stable when these municipalities are excluded or their assigned dates are varied (Section 7). In the modernization design, 24 of the 29 cohort-defining dates are supported by documented modernization events and five rely on recorded but independently unverified opening years; none is assigned through midpoint imputation (Section 3.2).

A third concern is inference with a limited number of treated municipalities. The primary design contains 20 treated municipalities, and the modernization specification 29. In addition to the municipality-clustered multiplier bootstrap used throughout, I compare analytic cluster-robust inference with wild-cluster bootstrap-t inference for the TWFE benchmark (Cameron et al., 2008; MacKinnon and Webb, 2018). The resulting inference is similar, indicating that the main significance levels are not driven by cluster-count distortions (Section 7.1).

A fourth concern is that treatment may affect AUs outside its formal target. Effects among women aged 20–24 may reflect adolescents aging out of eligibility or births postponed beyond age 19. This makes that age group unsuitable as a DDD comparison group, but it is informative about the timing of the response (Section 6). Spatial spillovers to nearby untreated municipalities would attenuate the estimated effects. A donut specification that excludes control municipalities near an AU assesses this possibility (Section 7).

A final concern is differential pre-treatment trends or anticipation. I evaluate this possibility using event-study leads, joint tests of the pre-treatment coefficients, the fake-treatment-timing exercise, and sensitivity analysis allowing controlled violations of parallel trends. The universal base period localizes potential anticipation bias to behavior affecting the year immediately before treatment. Section 7 also reports specifications that allow one year of anticipation.

5 Main Results

5.1 Effects of Opening a First AU on Adolescent Births

Table 3 reports the main estimates, and Figure 4 shows the corresponding event-study evidence for the level and triple-difference designs. Opening a municipality's first AU lowers the adolescent birth rate by 6.43 births per 1,000 women aged 15–19 (SE 2.10, $p = .002$), or 11.6 percent relative to the treated municipalities' mean rate in the year before opening (55.3 per 1,000). Adding baseline controls for municipality wealth, urbanicity, educational attainment, health-system supply, and insurance coverage yields a similar estimate of -5.44 ($p = .028$), indicating that the result is not driven by the observed dimensions along which treated and untreated municipalities differ most.

The within-municipality triple-difference design provides similar evidence for the first-opening specification. The estimate is -7.48 ($p < .001$) under not-yet-treated comparisons and -5.76 under never-treated comparisons only. This agreement is substantively important because, in the first-opening sample, the comparison age group behaves as required by the design: the birth rate

among women aged 30–34 shows no detectable response to treatment (−0.70, SE 2.40, $p = .77$; Appendix Table A8). In both the level and triple-difference designs, the event-study coefficients show no evidence of differential pre-trends, and the decline appears only after an AU opens.

The estimated magnitude is also similar across alternative functional-form specifications (Appendix Table A18). Taken together, these estimates indicate that opening a municipality's first AU reduced adolescent births, and that this conclusion does not depend on a particular comparison group or outcome scaling.

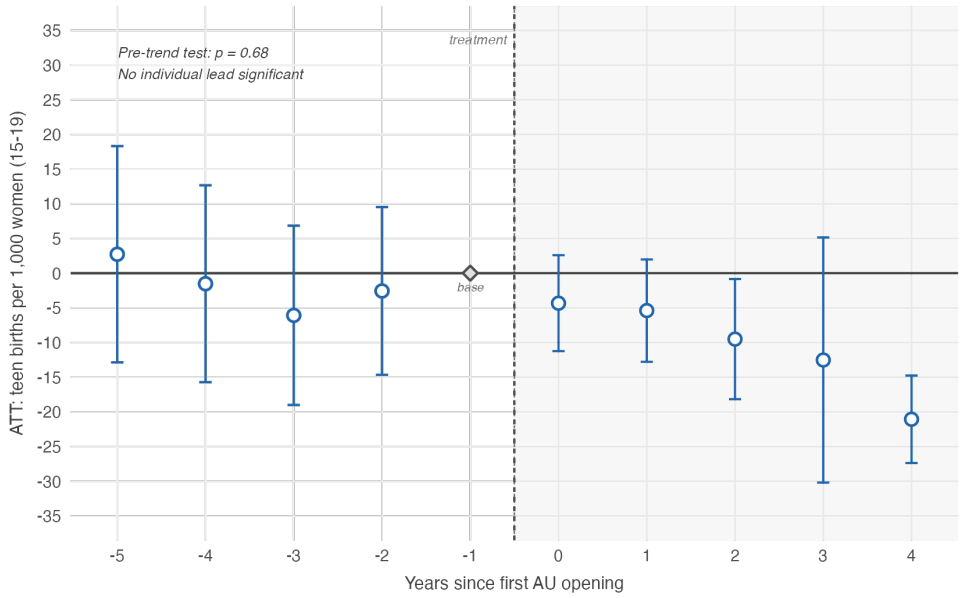
Table 3: Effect of AU openings on the adolescent birth rate

	First AU opening	First AU opening or modernization
Adolescent births per 1,000 women aged 15–19	−6.43*** (2.10) [−10.55, −2.31]	−3.95** (1.96) [−7.79, −0.11]
+ baseline controls	−5.44** (2.48) [−10.30, −0.58]	−3.10 (2.41) [−7.82, 1.62]
Triple difference (15–19 vs. 30–34)	−7.48*** (1.68) [−10.77, −4.19]	−7.29*** (1.31) [−9.86, −4.72]
never-treated comparisons only	−5.76** (2.45) [−10.56, −0.96]	−7.23*** (1.88) [−10.91, −3.55]
Baseline mean	55.3	52.4
Treated municipalities	20	29
Municipalities (clusters)	146	155
Observations (municipality-years)	1,460	1,550

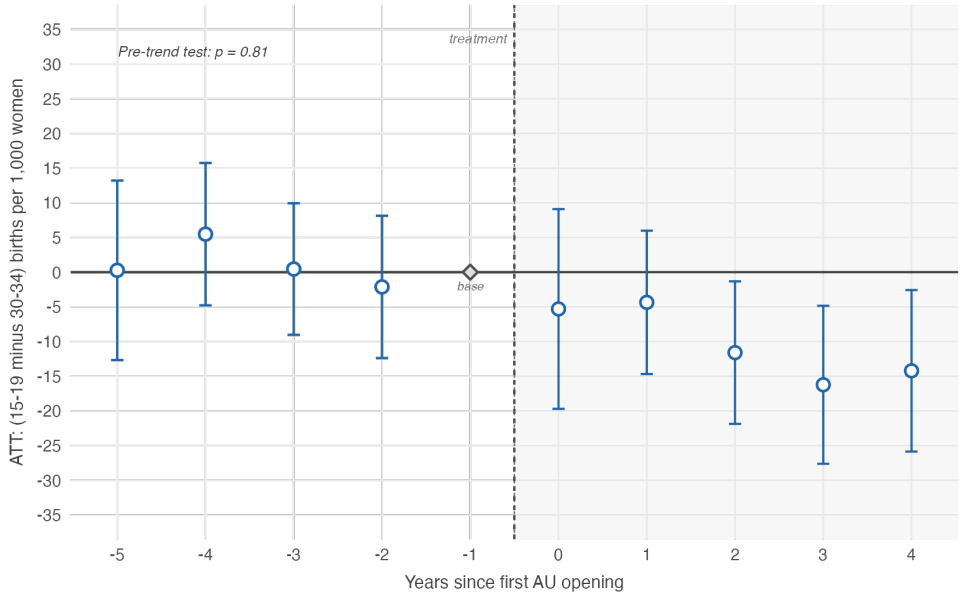
Notes: Callaway–Sant'Anna group-time ATT; dependent variable is the adolescent (15–19) birth rate per 1,000 women. Municipality-clustered standard errors are in parentheses; 95% confidence intervals are in brackets. The first column defines treatment as a municipality's first AU opening; the second also includes modernizations of pre-existing units. Triple differences use the tripledif estimator with cohort-specific not-yet-treated comparisons combined by GMM; the indented row restricts the comparison group to never-treated municipalities. Controls: baseline wealth index, public health facilities per 10,000 residents, % urban, % secondary-or-more, % SeNaSa 2013 (time-invariant). The triple-difference nets the 30–34 birth rate and stacks both age groups (2,920 / 3,100 observations). Estimator robustness and year-by-year dynamics appear in the Appendix. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Figure 4: Effect of opening an AU on teen birth rates

(a) Level: teen births per 1,000 women aged 15–19



(b) Within-municipality triple difference vs. ages 30–34



Notes: Panel (a) is the paper's primary level design (Callaway–Sant'Anna dynamic ATT, no covariates); panel (b) is the complementary within-municipality triple difference (Ortiz-Villavicencio and Sant'Anna). Both panels: not-yet-treated controls, uniform 95% bands, municipality-clustered multiplier bootstrap standard errors, and a shared y-axis scale. Event time -1 is the omitted reference period (grey diamond); the shaded region begins at treatment. Pre-trend joint tests of the plotted leads: average -1.9, $p = .68$ (panel a) and $+1.0$, $p = .81$ (panel b); no individual lead is significant in either panel. The far horizons are identified by few municipalities (e=+3: 7; e=+4: 4), so readers should interpret the group ATT rather than the tail.

The complementary modernization specification is best read as a boundary exercise rather than as a second main design. When the treatment definition is broadened to include modernizations of pre-existing AUs, the level estimate falls to −3.95 ($p = .043$) and becomes less precise under covariate adjustment. Appendix Table A17, Panel A shows that this broader level estimate is driven by municipalities receiving their first AU during 2020–2025, while municipalities that already had adolescent services before the study period show no detectable reduction in adolescent births, whether their documented event is a newly built AU or an upgrade of existing services. Panel B, in turn, shows that the large triple-difference coefficient is mechanically driven by the comparison band rather than by a larger response among adolescents: under the modernization design, the birth rate among women aged 30–34 rises by +3.27 overall and by +6.27 ($p = .031$) in the nine recovered municipalities alone, so subtracting that positive response mechanically enlarges the difference. I therefore interpret the paper's main result as evidence that first access to the comprehensive AU model reduces adolescent births, whereas additional or upgraded AUs in already-served municipalities do not generate the same effect. Which components of an upgrade, including infrastructure, dedicated personnel, service mix, and amenities, matter cannot be separated with the documentation available here and is a natural question for future work with facility-level service records.

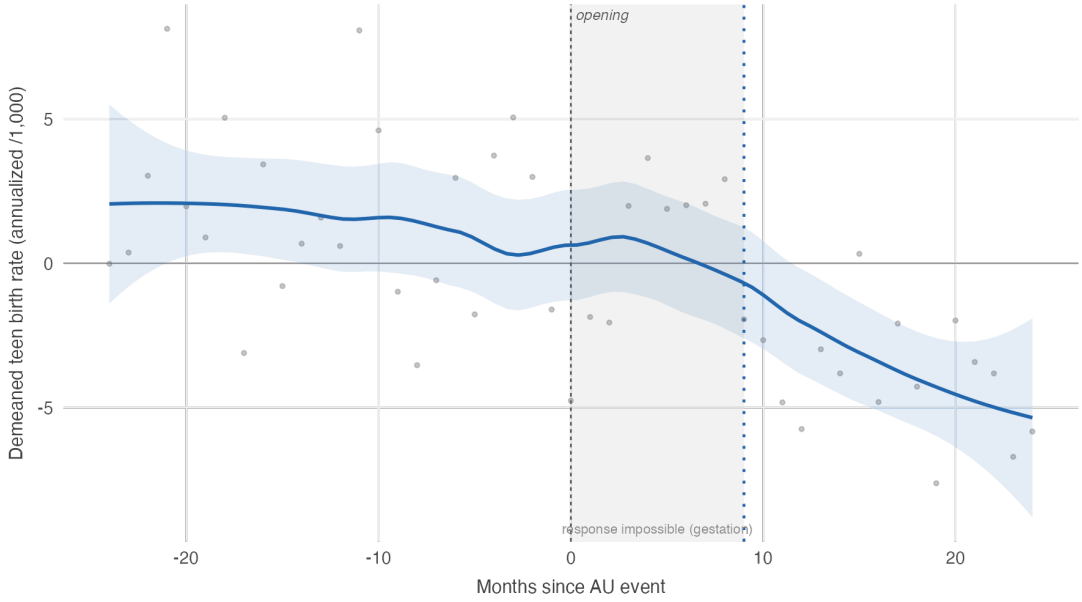
5.2 Dynamic Timing and Age Pattern of the Response

The dynamic estimates are consistent with the timing implied by the outcome. As Figure 4 shows, the estimated effect in the opening year is small and statistically indistinguishable from zero (−4.33, SE 2.69), which is what one would expect if many births observed in that year were conceived before the AU began operating. The effect becomes larger in the first full year after opening and reaches −9.51 ($p < .01$) by the second, equivalent to 17 percent of the pre-opening baseline (Appendix Table A2). Later post-treatment coefficients continue to grow in absolute value, but they are identified by only seven and then four treated municipalities and are therefore best interpreted with caution. The same exposure gradient appears across rollout cohorts, with earlier opening waves showing larger declines than later cohorts observed for shorter post-treatment periods (Appendix Table A16 and Appendix Figure B2). The especially large estimate for the 2021 cohort should also be read against the post-pandemic rebound in adolescent births documented in Montás and Galárraga (2026). A higher comparison path during the rebound may amplify treated-versus-comparison gaps for that cohort, although the cohort-specific estimate cannot establish this explanation.

Monthly event-study evidence based on each AU's opening month points in the same direction. Figure 5 aligns births to the month in which each AU opened and shows little change during the first eight months after opening, followed by a decline beginning only once the gestational window allows conceptions prevented after the opening to appear in the birth data. Relative to months 0 through 8, the average birth rate in months 9 through 24 is 4.37 births per 1,000 lower ($p = .005$).

The quarterly facility-based pregnancy series displays the same pattern: pregnancy events remain flat in the first two quarters after opening and decline from the third quarter onward. Taken together, these timing patterns are more consistent with fewer conceptions than with a change in registration, reporting, or other contemporaneous shocks.

Figure 5: Timing of the birth response around AU openings



Notes: Stacked monthly event windows (±24 months) around 25 month-dated AU events: 17 first openings and 8 month-precision modernization events in previously served municipalities (21 municipalities in total). The unit of observation is the municipality-month; the outcome is the annualized teen birth rate, demeaned within event window. Points are event-month means and the solid line is a loess fit. Standard errors are clustered at the municipality level. The average difference between months 9–24 and months 0–8 is −4.37 (SE 1.57, $p = .005$); the difference between months 0–8 and the pre-opening period is −1.09 ($p = .38$). The absence of an immediate decline and the emergence of the effect only after roughly nine months are consistent with fewer conceptions rather than changes in measurement.

Furthermore, I decompose the main effect by the mother's single year of age at birth, holding the common 15–19 denominator so that the rows sum to the total effect (Table 4). The decline is concentrated among older adolescents, since ages 18 and 19 account for −2.84 ($p < .01$) and −2.62 ($p < .05$) of the total −6.43 effect, corresponding to 18 and 14 percent of their respective baselines, whereas ages 15 to 17 contribute a combined −0.97 that is not statistically distinguishable from zero. The reduction in adolescent births therefore does not occur uniformly across the age range served by the program, but is concentrated where voluntary demand for reproductive-health services is most plausible. Ages 18 and 19 also lie at or above the DR's legal age of majority, which may affect adolescents' ability to seek care independently.

Table 4: Effect by mother's single year of age (decomposition of the teen birth rate)

Mother's age	First AU opening	First AU opening or modernization
Single years		
15	0.11 (0.48) [−0.83, 1.05]	−0.14 (0.34) [−0.81, 0.53]
16	−0.67 (0.47) [−1.59, 0.25]	−0.64 (0.47) [−1.56, 0.28]
17	−0.41 (0.44) [−1.27, 0.45]	−0.16 (0.44) [−1.02, 0.70]
18	−2.84*** (0.98) [−4.76, −0.92]	−1.97** (0.79) [−3.52, −0.42]
19	−2.62** (1.05) [−4.68, −0.56]	−1.03 (0.75) [−2.50, 0.44]
Subtotals		
15–17	−0.97 (0.80) [−2.54, 0.60]	−0.95 (0.81) [−2.54, 0.64]
18–19	−5.45*** (1.65) [−8.68, −2.22]	−3.01** (1.30) [−5.56, −0.46]

Notes: Callaway–Sant'Anna group ATT; outcome is births to mothers of the given age per 1,000 women aged 15–19 (the *common* 15–19 denominator), so each row is that age's *contribution* to the overall teen-rate effect; the single-year ATTs sum to the 15–19 total (−6.43). These are contributions, not single-year fertility rates (no single-year denominator exists). Municipality-clustered standard errors are in parentheses; 95% confidence intervals are in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Reclassifying births by the mother's age at conception strengthens that interpretation. Appendix Table A7 shows that when births are dated back to conception, the estimated effect rises to −8.22 per 1,000 ($p = .001$), again about 12 percent of baseline. The response begins at age 17 (−2.73, $p < .01$), remains negative at ages 18 and 19, and is essentially zero for conceptions at ages 10 to 14. The birth-based and conception-based results therefore tell a consistent story: the AU expansion reduces pregnancies beginning around age 17, and that decline appears in the birth data primarily as fewer births at ages 18 and 19.

The same logic extends beyond the AU program's formal eligibility range of 10 to 19 years of age. Appendix Table A8 shows that birth rates also decline among women aged 20–24, with the effect concentrated at ages 20 and 21. This pattern is consistent with adolescents exposed at ages 18 or 19 carrying altered contraceptive behavior into their early twenties, so it supports an interpretation based on changed pregnancy timing and service use rather than a one-year mechanical displacement of births. The estimated response then becomes smaller through ages 25–29 and reaches a precise zero at ages 30–34 (−0.70, $p = .77$; Appendix Table A8), which is the youngest age band that could not have been exposed under either treatment definition. The event studies by non-teen age group in Appendix Figure B5 show the same gradual attenuation with age.

Taken together, these results show that the response follows the timing implied by conception and is concentrated in the age groups most likely to use adolescent-targeted reproductive-health services. The effect emerges only after the gestational lag and fades to zero at ages that could not have been exposed to the program.

5.3 Effects on Pregnancies and Abortion-Related Attendances

Births capture only one observable consequence of adolescent pregnancy, so an important next question is whether the estimated decline in births reflects fewer pregnancies or instead a change in where births occur or how they are recorded. Table 5 addresses this question by holding the design fixed and changing the outcome. Each row re-estimates the first-opening specification on a different administrative measure: births assigned to the mother's municipality of residence, births assigned to the municipality of delivery, pregnancy-related events in public-facility registries, and abortion-related attendances. Comparing these outcomes helps distinguish a genuine reduction in pregnancies from alternative explanations based on sectoral reallocation or measurement.[11]

When births are assigned to the municipality of delivery rather than the mother's municipality of residence, the estimated effect is -7.42 per 1,000 ($p = .026$). Using the hospital-production registries, pregnancy-related events decline by 11.98 per 1,000, or 16.7 percent relative to baseline ($p < .001$). Because these registries do not pass through the MoH registration system, the fact that they move together with the birth records makes a pure registration or reporting explanation difficult to sustain. Pre-treatment coefficients are also small across outcomes, which supports interpreting these declines as arising around AU openings rather than from differential pre-existing trends.

The facility breakdown suggests that part of the response may reflect a modest shift across delivery sectors, but this channel is small relative to the overall decline in births, and because the AUs operate inside existing public hospitals rather than adding new delivery capacity, the intervention itself creates little scope for mechanical substitution across sectors. Public-facility births fall by 16.6 percent, while births in private or other facilities rise by 1.37 per 1,000 ($p = .044$). This offset is not large enough to alter the main conclusion, but it does reinforce the choice of births by mother's residence as the primary outcome, since a public-facility measure alone would confound changes in adolescent childbearing with changes in where those births take place.

[11] Nizao is excluded from Table 5 because its municipal hospital experienced a sharp increase in obstetric volume in 2024, which mechanically inflates facility-based rates. This restriction has little effect on the residence-based estimate: the main effect is -6.43 in the full sample and -6.04 in the common sample used in the table.

Table 5: Effects of AU openings on births and pregnancy-related events

	Estimate (SE) [95% CI]	Baseline ($g-1$)	%	Avg. pretrend
Civil registry (BDNV), 2016–2025				
Births, mother's residence (main estimate)	-6.04^{***} (2.25) [−10.45, −1.62]	54.01	−11.2%	−1.30
Births, delivery municipality, all facilities	-7.42^{**} (3.33) [−13.96, −0.89]	59.64	−12.5%	−0.25
public facilities	-8.79^{***} (3.34) [−15.35, −2.24]	52.87	−16.6%	−0.77
private/other facilities (substitution)	1.37^{**} (0.68) [0.04, 2.70]	6.77	+20.2%	+0.90
Hospital-production registries (Form 67-A), 2015–2025				
Pregnancy events, public facilities	-11.98^{***} (2.69) [−17.26, −6.70]	71.70	−16.7%	−0.58
Abortion-related attendances, public facilities	-1.15^{**} (0.56) [−2.25, −0.05]	6.21	−18.5%	+0.36
Stillbirths, public facilities (descriptive)†	−0.14 (0.46) [−1.04, 0.76]	0.76	−18.1%	+0.92

Notes: Callaway–Sant'Anna group ATTs, identical specification across rows (first AU opening, not-yet-treated controls, universal base period, municipality-clustered SEs in parentheses, 95% confidence intervals in brackets); outcomes per 1,000 women 15–19 (denominator A); % relative to the treated mean in the year before opening ($g-1$). "Avg. pretrend" is the precision-weighted average of the dynamic pre-treatment coefficients ($e \in [-5, -2]$); no lead is individually significant for any outcome. Facility outcomes assign events to the municipality of the attending facility (public-sector Form 67-A registries; validated against NSO province aggregates, corr 0.99, and BDNV, corr 0.96). Abortion-related attendances include spontaneous losses, induced abortions, and post-abortion care. All rows exclude Nizao (145 municipalities), whose municipal hospital roughly quadrupled obstetric volume in 2024 (deliveries to women 20+ rose from 85 to 353; confirmed in the civil registry), a facility-supply change that mechanically inflates facility-based rates; the main estimates elsewhere in the paper use the full sample. †Stillbirths are recorded only from 2022, leaving pre-periods for the 2023+ cohorts only; the estimate fails the pre-trend diagnostic and is shown only as descriptive context; the raw decline is −36% from 2022 to 2025. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The broader pregnancy margin moves in the same direction as births. Abortion-related attendances decline by 1.15 per 1,000, or 18.5 percent relative to baseline ($p = .040$). If the AU expansion reduced births mainly by shifting pregnancies toward termination, one would expect this margin to rise rather than fall. Instead, births, pregnancy-related events, and abortion-related attendances all decline together, which is more consistent with fewer pregnancies occurring in the first place. Given the country's total criminalization of abortion, recorded attendances necessarily omit clandestine terminations that do not reach public facilities, so this test relies on the direction of recorded care rather than its level (Ishola et al., 2025). Stillbirths are reported descriptively because the series begins only in 2022 and the estimate fails the pre-trend diagnostic.

These results are reported as average effects rather than dynamic event-study plots because the facility-based outcomes become harder to interpret at longer horizons. The facility-based series declines clearly in the first two years after opening, but later coefficients are identified by only a small number of municipalities and become harder to interpret as patient flows across municipalities and facility types accumulate over time. The residence-based birth outcome does not present these

concerns, which is why I rely on Figure 4 for the dynamic analysis and use Table 5 to show that the broader pregnancy margin moves in the same direction.

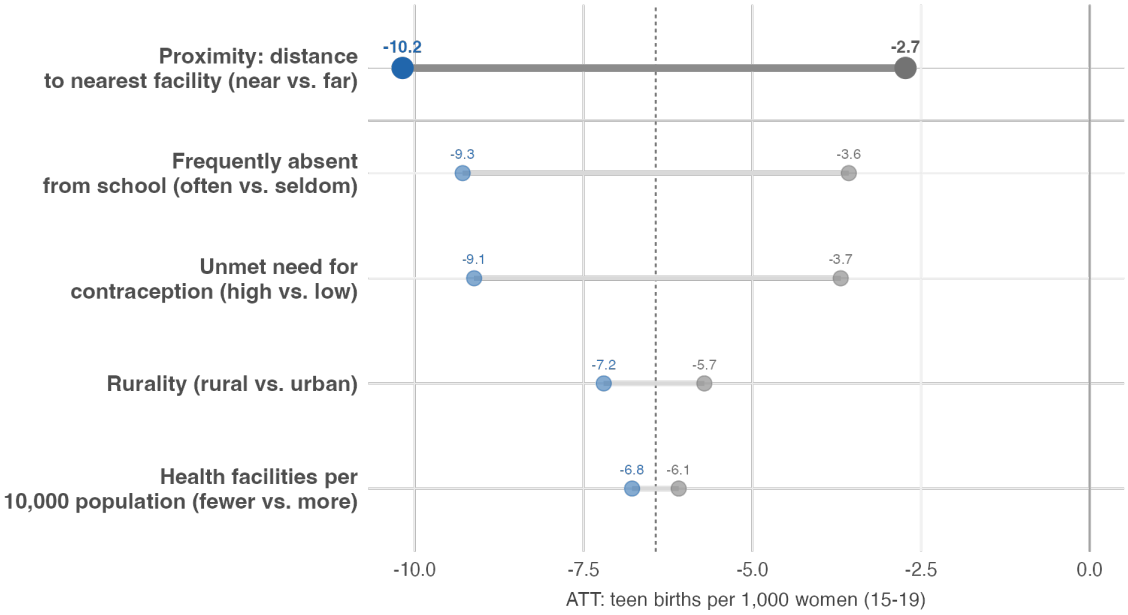
Taken together, these results indicate that the AU expansion reduced pregnancies rather than simply shifting births across reporting systems or facility types.

5.4 Heterogeneity by Access to the Health System

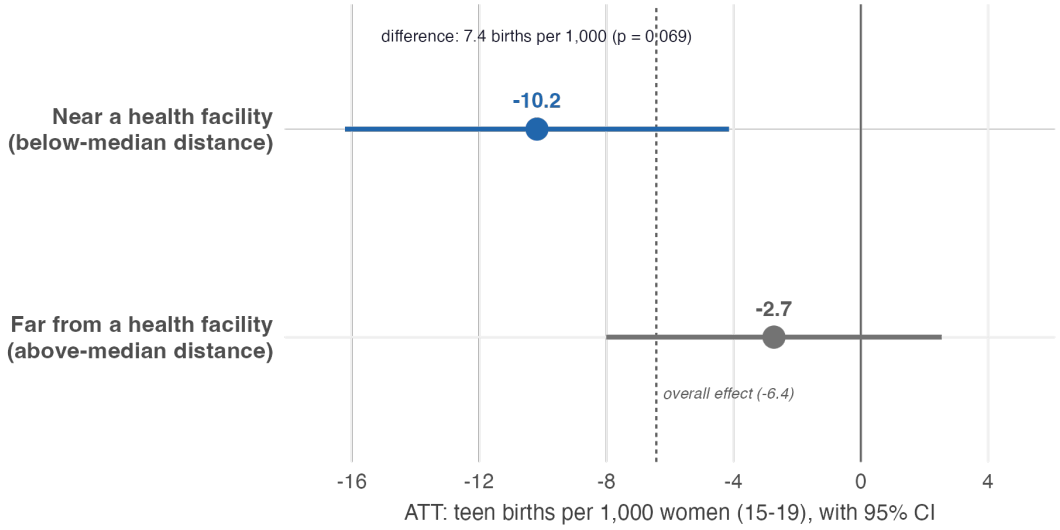
The next question is whether the effect of AU openings differs systematically across municipalities. The heterogeneity exercise considers a broader set of baseline moderators spanning contraceptive access, socioeconomic conditions, geography, health-system supply, and adolescent vulnerability. Figure 6 summarizes the main contrasts, and Appendix Table A6 reports the full set of subgroup estimates. Across these splits, the clearest pattern is still proximity to the public health system: the effect is substantially larger in municipalities whose population centers lie closer to an NHS facility, while most other contrasts show smaller and less precisely estimated differences. This is the margin one would expect to matter most, since the AUs operate inside existing public hospitals. This is even more relevant outside the capital. Excluding the Santo Domingo area, the estimated effect is -12.2 per 1,000 in municipalities whose population centers lie close to a public facility and near zero in those farther away (Appendix Figure B4), underscoring how basic physical access conditions what the model delivers.

Figure 6: Heterogeneity of the effect: proximity to the health system is the operative margin

(a) Effects across baseline moderators



(b) Near versus far from a public health facility



Notes: Subgroup Callaway–Sant'Anna ATTs using median splits of baseline moderators among treated municipalities and the shared never-treated control pool. The dashed line marks the overall estimate (−6.43). Panel (b) compares treated municipalities near versus far from the nearest SNS facility. Event studies by proximity subgroup appear in Appendix Figure B4; the full subgroup estimates and difference tests appear in Appendix Table A6.

Splitting treated municipalities at the median distance from their population center to the nearest public health facility, the estimated effect is −10.2 births per 1,000 ($p = .001$) in municipalities close to the system, compared with −2.7 in municipalities farther away. The difference between

the two is marginally significant in the full sample ($p = .069$) and somewhat stronger when the Santo Domingo metropolitan area is excluded ($p = .044$). This is the most substantively informative heterogeneity margin in this study, and because the AUs are embedded in public hospitals, their effects should be larger where adolescents can more readily reach those facilities. The pattern therefore, suggests complementarity with the existing public health system. The subgroup event studies in Appendix Figure B4 point in the same direction.

The broader heterogeneity exercise provides suggestive, though not definitive, evidence that the program also benefits adolescents facing greater disadvantage. Effects are larger in municipalities with higher unmet need for contraception (-9.1 versus -3.7), which is suggestive and consistent with a contraceptive-access channel. Similarly, the estimated effect is larger in municipalities where students were more frequently absent from school before the rollout (-9.3 versus -3.6, difference $p = .13$), which is consistent with the program mattering more where adolescents were already at greater risk of disengagement. Estimates are also larger where parental school participation and expectations of university completion were lower, but these gaps are imprecise. By contrast, municipality-level splits by wealth, rurality, insurance coverage, facility density, and mothers' secondary completion show little differential response; for the last measure, the estimates are -6.2 and -6.7 ($p = .89$). Complementary evidence in Appendix Table A13 shows that births decline across both maternal education groups, but the proportional reduction is larger among mothers with less than secondary education (14.6 versus 9.5 percent). Because the difference between the absolute effects is imprecise ($p = .43$), this pattern is suggestive of greater proportional benefits among less-educated adolescents rather than distinct subgroup effects. Overall, proximity to the hospital system remains the strongest heterogeneity result, with additional suggestive evidence that the program reaches adolescents facing greater vulnerability.

5.5 Schooling Outcomes

Because the AUs provide broader adolescent services rather than only reproductive-health care, their effects may extend beyond fertility to schooling outcomes. Table 6 reports estimates from the first-opening design for municipality-level measures of secondary-school progression and performance. Panel A reports enrollment and dropout by sex, Panel B decomposes enrollment by grade group, and Panel C reports participation and performance in the *Pruebas Nacionales*, the national terminal-secondary examination. The main enrollment outcome is the secondary gross enrollment ratio, defined following the World Development Indicators as total secondary enrollment divided by the population of official secondary-school ages. This normalization is important because it avoids mechanically conflating changes in enrollment with changes in cohort size.[12]

[12]The 12–17 denominator interpolates the projection's five-year age groups assuming uniform ages within group. A net enrollment ratio cannot be constructed because the administrative enrollment data do not record student ages; the overage margin in panel (b) of Table 6 is the closest available analog.

Table 6: Effects of AU openings on secondary-school outcomes

(a) Enrollment and dropout, by sex

	Male	Female	Total	Baseline	Pre-trend p
Gross enrollment ratio (ages 12–17)	$+1.67^{***}$ (0.54) [0.61, 2.72]	$+1.28^{**}$ (0.53) [0.24, 2.31]	$+1.48^{***}$ (0.44) [0.61, 2.35]	82.1	0.21
Dropout rate (pp of enrolled)	+0.02 (0.43) [−0.83, 0.87]	+0.08 (0.23) [−0.37, 0.54]	+0.05 (0.29) [−0.51, 0.62]	4.4	0.11

(b) Enrollment by grade group, by sex

Gross enrollment ratio (ages 12–17)	Male	Female	Total	Baseline	Pre-trend p
Grades 4–5 (ages ≈15–17)	+0.15 (0.28) [−0.40, 0.69]	+0.20 (0.40) [−0.59, 0.99]	+0.18 (0.29) [−0.40, 0.76]	24.3	0.70
Grade 6, terminal (ages ≈17–18)	$+0.44^{*}$ (0.23) [−0.01, 0.88]	+0.23 (0.17) [−0.11, 0.57]	$+0.33^{*}$ (0.18) [−0.02, 0.69]	9.5	0.34
of which overage (3+ yrs behind, both sexes)			$+0.07^{**}$ (0.03) [0.01, 0.13]	0.37	0.12

(c) School performance, Pruebas Nacionales

	Estimate (SE) [95% CI]	Baseline	% of base	Pre-trend p
Test score, all four subjects (SD)	+0.00 (0.07) [−0.14, +0.14]	N/A	N/A	0.73
Test score, mathematics (SD)	+0.06 (0.07) [−0.08, +0.20]	N/A	N/A	0.67
Test-takers per 100 population 17–18	−1.23 (1.68) [−4.52, +2.06]	24.78	−5.0	0.72
Test-takers who pass, per 100 population 17–18	−0.89 (1.28) [−3.40, +1.62]	18.79	−4.7	0.90

Notes: Callaway–Sant'Anna group ATT on municipality-level secondary-schooling outcomes (146 municipalities, school years 2015–16 to 2024–25); not-yet-treated controls, universal base period, municipality-clustered multiplier-bootstrap standard errors in parentheses, 95% confidence intervals in brackets. The gross enrollment ratio is all secondary enrollment, regardless of age, per 100 population of the official secondary ages 12–17; the dropout rate is the share of enrolled students leaving during the school year. Baseline = treated mean in the year before opening (total column); pre-trend p is the joint test of the leads $e \in [-5, -2]$. In panel (b) the male and female columns use per-sex enrollment over the per-sex official-age population, so the sex-specific ratios do not average to the total. Panel (b) omits grades 1–3 because that outcome fails its pre-trend test; the nested row isolates students at least three years behind grade. Panel (c) uses center-level results from the Pruebas Nacionales, the national terminal-secondary examination (first convocatoria; school years 2016–2024, with the assessment scoreless in 2020 and cancelled in 2021; the 2024 wave uses first-convocatoria student microdata provided by MoE), aggregated to municipalities; 16 treated municipalities (2020 and 2021 openers are excluded because their base school year is unobservable). Scores are standardized within year × subject × modality × scoring regime; test-takers and passers are scaled per 100 population aged 17–18, approximated as 0.4× the population aged 15–19. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The estimates are suggestive of a positive schooling response. The secondary gross enrollment ratio rises by 1.5 points after an AU opening, about 2 percent of baseline, with no evidence of differential pre-trends. By contrast, the estimated effect on the within-year dropout rate is close to zero and imprecisely estimated. This pattern is not inconsistent with the fertility results, because even a meaningful reduction in adolescent childbearing would imply only a modest short-run change in dropout at the municipality level, and the confidence interval is wide enough that such effects would be difficult to detect precisely in these data.

The enrollment response is present for both sexes and is somewhat larger for males. Across grade groups, the increase is concentrated in the terminal secondary grade, where total enrollment rises by 0.33 points, about 3.5 percent of baseline, with acceptable pre-treatment fit. Within that grade, the clearest response appears among overage students, whose enrollment rises by 0.07 points, or about 19 percent of baseline. This is the subgroup most likely to be at risk of disengaging from school, so the pattern is consistent with the AUs helping older, more vulnerable adolescents remain enrolled.

Panel C indicates that these margins do not extend to measurable short-run gains in examination performance. Average scores in the *Pruebas Nacionales* are precise zeros, both overall and in mathematics, and neither examination taking nor passing increases detectably when expressed relative to the population ages 17–18. This pattern is consistent with the broader interpretation that the AU expansion appears to affect school attachment and progression at the extensive margin, especially near the end of secondary school, but not learning or certification outcomes within the observation window.

These schooling results are best viewed as supporting evidence that the AU model may generate broader human-capital benefits, rather than as definitive estimates of long-run educational attainment. The cohorts exposed to the program are still too young for completed schooling to be observed directly. Taken together, the schooling evidence suggests that the AU expansion may have benefits beyond fertility, particularly in keeping older adolescents attached to school, but those effects are estimated less precisely and should be interpreted as exploratory. The next section turns from outcomes to interpretation and asks what the available evidence suggests about how the AUs produced these fertility effects.

6 Mechanisms

The main results indicate that opening a municipality's first AU reduced adolescent births, with patterns that are difficult to reconcile with changes in registration, reporting, or delivery location alone. This section asks what the available evidence suggests about how that decline occurred. Because direct AU-level utilization data do not exist, the mechanism analysis is necessarily indirect. I therefore combine the timing, age-profile, pregnancy-margin, and proximity evidence from the previous section with the auxiliary administrative measures that can speak to how adolescents

entered care and whether the decline reflects fewer pregnancies or instead compositional changes in the births that remained.

6.1 Utilization and Entry into Care

The evidence is most consistent with the AUs reducing adolescent fertility by lowering non-price barriers to reproductive-health care. The timing results in Section 5.2 show that the decline appears only after the gestational lag, while Section 5.3 shows that pregnancy-related events and abortion-related attendances fall alongside births. Section 5.4 further shows that the effect is largest where adolescents live closer to the public hospitals that host the AUs. Taken together, these patterns point toward fewer conceptions arising from greater access to confidential, adolescent-oriented services, rather than from changes in measurement, registration, or the location of delivery.

Direct utilization statistics for the AUs are not available, so the mechanism evidence is necessarily incomplete. Table A14 therefore turns to the hospital-production registries for the limited utilization signals they can provide. The consultation levels themselves are too noisy to be very informative: prenatal consultations to adolescents are imprecisely estimated, and the counseling and family-planning series is available only from 2023, so it covers only the late opening cohorts and does not provide a precise signal of service use. The most useful margin is instead the adolescent share of prenatal consultations, which rises by 3.4 percentage points ($p = .092$) after a municipality's first AU opens among cohorts opening from 2021 onward. Adolescents therefore account for a larger share of antenatal care even as adolescent pregnancies decline. This pattern is consistent with the AUs drawing adolescents into earlier or more sustained contact with the health system, but it does not identify AU use directly.

This interpretation is also consistent with the age profile of the response documented in Section 5.2. The effect is concentrated among older adolescents, begins at conception age 17, and extends into ages 20 and 21, which is what one would expect if exposure to the AUs changed contraceptive behavior rather than merely shifting births across reporting categories. The utilization evidence should nonetheless be interpreted cautiously, since no direct AU-level records exist and the available proxy measures are limited. I therefore read the prenatal-consultation result as suggestive supporting evidence, not as a standalone demonstration of the mechanism.

Taken together, the available evidence supports an interpretation in which the AUs worked by making adolescents more willing and able to enter care within the public health system. The next subsection asks whether the decline reflects fewer pregnancies occurring in the first place, rather than a change in the composition of the births that remained.

6.2 Quantity, Not Composition

The evidence shows that the decline in adolescent births reflects fewer pregnancies occurring in the first place, rather than a change in the composition of the births that remain. Section 5.3

shows that births, pregnancy-related events, and abortion-related attendances all decline together, which is difficult to reconcile with a story based primarily on reclassification or substitution across pregnancy outcomes. The composition evidence points in the same direction: the observed decline does not appear to be driven by a sharp change in which adolescents give birth.

Appendix Table A13 shows that the reduction in births is broad-based across maternal education groups. Birth rates fall for mothers with less than secondary education (-1.93, $p = .074$) and for those with secondary or more (-3.18, $p = .039$), and the difference between the two estimates is small and imprecisely estimated ($p = .43$). The decline therefore does not appear to be concentrated in only one observed educational segment of adolescent mothers. In proportional terms, however, the decline is somewhat larger among mothers with less than secondary education (14.6 versus 9.5 percent of the respective baselines), which provides suggestive but imprecise evidence that the program also reaches less-advantaged adolescents. This pattern is consistent with the broader heterogeneity evidence in Section 5.4, which also suggests that the program's reach is shaped more by access to the health system than by baseline socioeconomic status.

Newborn outcomes provide an additional check on composition. Appendix Table A5, Panel A shows that low birthweight, preterm birth, and small-for-gestational-age births all decline in proportion to the overall fall in adolescent births, with estimated reductions of roughly 12 to 17 percent, though none is individually precise. Measured per 1,000 adolescent women, this pattern is more consistent with fewer births occurring than with a marked improvement in the health composition of surviving births. Panel B reaches a similar conclusion: when outcomes are expressed as shares of teen births, most margins change little. The main exception is the c-section share, which rises by 3.5 percentage points ($p = .003$). Because this share conditions on the post-treatment set of births, it cannot distinguish selection among remaining births from a change in delivery practice. It is nevertheless a margin worth monitoring: in a setting where half of adolescent births are already surgical, an increase associated with greater facility contact could represent an unintended cost.

Taken together, the composition evidence is limited but coherent. The decline in adolescent births is not confined to a narrow maternal education group, and newborn-health outcomes move broadly in proportion to births rather than suggesting strong positive selection among the births that remain. Combined with the fall in pregnancy-related events and abortion-related attendances, these patterns are most consistent with the AUs reducing the number of pregnancies rather than merely changing their composition.

7 Robustness

7.1 Estimators and Inference

The main result is not specific to one estimator. Appendix Table A1 reports estimates from several approaches designed for staggered treatment timing. The Callaway–Sant'Anna estimate of -6.43 births per 1,000 lies within a narrow range of heterogeneity-robust alternatives: -8.01 un-

der Sun and Abraham (2021), −5.57 under the imputation estimator of Borusyak et al. (2024), numerically identical here to the two-stage estimator of Gardner et al. (2024), and −5.00 under the static two-way fixed-effects benchmark. The similarity of these estimates indicates that the paper's conclusion does not depend on a particular estimator for staggered adoption. A Goodman-Bacon decomposition further shows that the TWFE benchmark in this setting places 95.7 percent of its weight on treated-versus-untreated comparisons and only 1.3 percent on later-versus-earlier treated comparisons, which helps explain why it remains close to the heterogeneity-robust estimators (Goodman-Bacon, 2021).

Throughout the paper, standard errors are computed using the municipality-clustered multiplier bootstrap, and the event-study figures report uniform rather than pointwise confidence bands. Because the design includes only twenty treated municipalities, Appendix Table A1 also reports alternative inference for the TWFE benchmark. The analytic cluster-robust p-value (0.116) and the wild-cluster bootstrap-t p-value (0.127) are very similar. This comparison supports the TWFE benchmark but does not directly validate inference for the preferred Callaway–Sant'Anna estimator (Cameron et al., 2008; MacKinnon and Webb, 2018).

7.2 Alternative Samples, Comparison Groups, and Treatment Timing

Appendix Table A9 examines how the main result changes when individual ingredients of the design are varied one at a time. The estimate is very similar when the comparison group is restricted to never-treated municipalities (−6.45, $p = .002$), when the Santo Domingo metropolitan area is excluded (−6.69, $p = .007$), and when the three municipalities with imputed opening dates are dropped (−5.02, $p = .023$). A log specification yields −0.13 log points ($p = .006$), which closely matches the implied percentage effect in the baseline linear model. Population weighting attenuates the estimate to −4.47 ($p = .096$), consistent with the weaker response observed in the largest cities, while allowing one year of anticipation reduces precision by construction and yields −3.78 ($p = .26$). Taken together, these checks indicate that the negative effect is not sensitive to a particular comparison group, functional form, or sample restriction.

The within-municipality triple-difference estimates are similarly stable. Restricting the comparison group to never-treated municipalities yields −5.76, population weighting yields −6.63, and excluding the imputed-date municipalities yields −8.68. By contrast, replacing the 30–34 comparison band with women aged 20–24 or 25–29 drives the estimate to +0.48 and +0.34, respectively, both statistically insignificant. This is the expected pattern, since those younger adult groups can themselves be exposed to the program and therefore do not provide valid untreated comparison bands.

Treatment timing is documented rather than assumed, and the estimates are stable to the remaining ambiguities in dating. Seventeen of the twenty first-opening dates are verified directly from health-service records or fieldwork. Excluding the three municipalities with imputed dates yields −5.02 ($p = .023$), while recoding La Vega's opening to its earlier alternative date yields −5.90.

Dropping Constanza, whose 2019 baseline appears affected by a local registration shift, strengthens the estimate to -7.47 ($p < .001$), so retaining it is the conservative choice. Across these timing variants, the estimate remains between roughly -5.0 and -7.5 births per 1,000 (Appendix Table A9).

Matching addresses the placement imbalance directly, because the full never-treated pool includes many small municipalities unlike the treated ones. As Appendix Table A10 shows, matching each treated municipality to its nearest structural neighbors (wealth, urbanity, education, health supply, insurance, and log population, but never the baseline outcome) yields estimates between -2.9 and -4.1 births per 1,000 across five schemes, always negative though individually imprecise.[13] Because matching discards roughly 90 of the 126 never-treated municipalities and cannot fully eliminate the structural differences between treated municipalities and their potential controls, I interpret these estimates as a sensitivity exercise rather than as a competing main design.

Spatial spillovers are the final sample concern. If adolescents near an AU but resident in an untreated municipality use it, the never-treated controls are contaminated and the main estimate is biased toward zero. Two checks bound this possibility. Assigning treatment by proximity instead of municipal ownership yields a small and insignificant estimate (-2.52, $p = .46$), while removing never-treated controls within 10, 20, and 30 km of an AU leaves the estimate negative and statistically significant throughout (Appendix Table A9). These checks therefore suggest that cross-border use, if present, attenuates rather than generates the main result. The estimate strengthens as the exclusion radius widens, from -6.55 at 10 km to -9.92 at 30 km, consistent with wider rings removing more partially treated controls. One possible explanation is that some adolescents travel farther for anonymity, but the evidence cannot distinguish that mechanism from other forms of cross-border use.

7.3 Measurement and Contemporaneous Confounds

A first concern is whether the estimated decline in adolescent births could be generated by changes in measurement rather than by changes in fertility itself. Birth-registration completeness rises over 2016–2018, but every first AU opening occurs in 2020 or later. Re-estimating the main specifications on 2019–2025 alone leaves the results unchanged: the level, covariate-adjusted, and triple-difference estimates are numerically identical to the baseline ones (Appendix Table A12). The early registration ramp is therefore unlikely to confound the primary design.

Further, there are two compositional shifts on treatment: the public-facility share of teen births falls by 4.3 percentage points ($p = .001$), and the Haitian-mother share falls by 3.5 points ($p = .045$) after openings. Appendix Table A12, Panel B reports three patterns that limit the importance of those shifts. First, the public-facility and Haitian-mother shares show similar patterns among births to women aged 25–44, indicating broader municipality-level changes rather than teen-specific responses to the program. Second, leave-one-out exercises show that neither pattern is driven by a single municipality. Third, conditioning on the baseline values of those shares strengthens rather

[13] The matched municipalities are listed in Appendix Table A11.

than weakens the main estimate (-7.69, $p = .001$), whereas conditioning on contemporaneous values would amount to conditioning on post-treatment outcomes and is reported only as a bound. More importantly, a pure registration explanation is difficult to sustain in light of the main results already presented. Section 5.2 shows that the decline appears only after the gestational lag, and Section 5.3 shows that pregnancy-related events fall in independent NHS hospital-production registries that never pass through the MoH birth registrations. A registration story would therefore have to make two distinct reporting systems move together at each municipality's opening date while also respecting the gestational window.

A third measurement concern is the denominator of the adolescent birth rate. Official municipality population counts by the NSO are available only through 2020, so the main outcome necessarily relies on projected post-2020 populations. Panel A of Appendix Table A19 therefore treats denominator construction as a robustness exercise in its own right. Replacing the main denominator with an intercensal series anchored to the 2010 and 2022 censuses leaves the level estimate essentially unchanged at -6.42 ($p = .002$), which indicates that the main result is not sensitive to the particular population series used to scale births. The triple-difference estimate is smaller under the intercensal construction (-4.37, $p = .002$), but it remains negative and statistically significant. That attenuation is mechanical rather than substantive: the intercensal series shifts the 15–19 and 30–34 denominators differentially in pre-treatment years, which the triple difference absorbs by construction. Panel B of Appendix Table A19 shows more formally that significance at the five-percent level survives a differential post-treatment denominator error of six percent, while the point estimate reaches zero only under an error of fifteen percent. The realized differential drift between the two independently constructed denominator series is only -0.37 percent, with a confidence interval bounded below $+0.6$, so the error required to overturn the result is substantially larger than the disagreement the data actually show. A specification that may look natural in this context is not: conditioning directly on log municipality population attenuates the estimate to -1.46, and to -2.48 in the covariate-adjusted specification, because population is the denominator of the outcome and the regression is then conditioning on a function of the dependent variable rather than on an independent confound.

A second class of concerns is that other national shocks and policies overlapped with the AU rollout. Table 7 reports the relevant checks. The most obvious confound candidate is the COVID-19 pandemic, since the rollout begins in 2020 and adolescent fertility was changing nationally throughout this period. The most direct test drops the pandemic calendar years while retaining all treated municipalities, and it reproduces the main estimate. By contrast, restricting attention only to post-pandemic opening cohorts, or combining both restrictions, attenuates the estimate. That attenuation is not diagnostic of confounding, however, because excluding the 2020–21 opening cohorts leaves later-opening municipalities observed only at short exposures, when the dynamic effect is still building. The relevant comparison is therefore the specification that removes the pandemic years while preserving the original treatment cohorts, and that specification leaves the

result intact.

Table 7: Sensitivity to concurrent policies and shocks

	Estimate (SE) [95% CI]	% of baseline	Munis	Treated
Main estimate, for reference	-6.43^{***} (2.10) [−10.55, −2.31]	−11.6	146	20
Panel A. COVID-19				
Drop pandemic years (2020–2021)	-6.26^{**} (2.84) [−11.83, −0.69]	−11.0	146	20
Post-pandemic opening cohorts only (2022+)	−3.91 (2.86) [−9.52, 1.70]	−7.6	142	16
Both restrictions	-4.77^{*} (2.82) [−10.30, 0.76]	−8.8	142	16
Panel B. Jornada Escolar Extendida (JEE)				
No covariates (common JEE sample)	-6.42^{***} (2.12) [−10.58, −2.26]		145	20
Main-specification covariates	-5.39^{**} (2.68) [−10.64, −0.14]		145	20
+ baseline JEE coverage (2018–2019)	-5.02^{**} (2.55) [−10.02, −0.02]		145	20
Panel C. Child-marriage ban (Law 1-21, effective January 2021)				
Ages 15–17 (ban-bound)	−0.97 (0.81) [−2.56, 0.62]	−5.0	146	20
Ages 18–19 (not directly bound)	-5.45^{***} (1.64) [−8.66, −2.24]	−16.0	146	20

Notes: Each row reports a Callaway–Sant'Anna group ATT for the adolescent birth rate using not-yet-treated controls, a universal base period, and municipality-clustered multiplier-bootstrap standard errors. "% of baseline" is relative to the treated mean in the year before opening. *Panel A* examines pandemic calendar years and opening cohorts. Dropping 2020–2021 while retaining all treated municipalities is the most direct pandemic check. Restricting the sample to later opening cohorts also removes cohorts observed at longer exposures, so the resulting attenuation is not a clean test of pandemic confounding. *Panel B* examines sensitivity to the extended school day. All rows use the 145 municipalities with JEE data; the last adds baseline JEE coverage to the main covariate set. Appendix Figure B7 shows that JEE coverage evolves similarly in treated and never-treated municipalities around AU openings. *Panel C* splits the teen birth rate by age band using the common denominator of women aged 15–19. A response to the January 2021 child-marriage ban should be concentrated below age 18; instead, the decline is concentrated at ages 18–19. Births at ages 10–14 appear in Appendix Table A8. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The other overlapping policy most relevant for identification is the *Jornada Escolar Extendida* (JEE), whose municipal expansion independently reduces teen fertility and is not differenced out by the age-based triple difference (Garganta et al., 2026). The identifying requirement is therefore not that JEE be absent, but that its expansion be orthogonal to AU timing. Appendix Figure B7 shows that treated and never-treated municipalities expanded JEE similarly across the study period and that an event study of JEE coverage around AU openings is flat (+0.91, $p = .32$). Conditioning on baseline JEE coverage also barely changes the covariate-adjusted estimate (Table 7, Panel B). These patterns indicate that AU openings occurred against essentially the same JEE environment

in treated and untreated municipalities.

Finally, the January 2021 child-marriage ban does not match the age profile of the estimated response (Congreso Nacional de la República Dominicana, 2021). If the ban, rather than the AUs, were driving the decline, the effect should be concentrated among the under-18 age groups that the law directly constrains. Instead, Table 7, Panel C shows that the response is carried by ages 18–19 (-5.45, $p = .001$), while ages 15–17 show no significant decline (-0.97, $p = .23$). That pattern is difficult to reconcile with the ban as the primary explanation, but it is consistent with the broader age-profile evidence in Section 5.2.

Taken together, these checks indicate that the main result is not an artifact of registration improvements, projected denominators, or overlapping national policies. The remaining measurement concerns are real but bounded, and they do not overturn the conclusion that opening a municipality's first AU reduced adolescent births.

8 Economic Value and Break-Even Costs

The estimated reduction in adolescent births is large enough to raise an immediate policy question: even without observing the program's actual budget, is the effect plausibly large enough to justify the cost of operating the AUs? Because no public budget line for the AU program is reported, I cannot compute a conventional cost-effectiveness ratio. What the estimates do support is a break-even calculation: how much could an AU cost per treated municipality-year and still generate benefits at least as large as its operating cost? Table 8 answers that question in three steps by converting the estimated reduction in births into implied births averted, assigning each a monetary value, and then dividing the resulting benefit by the number of treated municipality-years.

Multiplying the main ATT by treated municipalities' post-opening exposure implies 3,333 averted registered adolescent births over 2016–2025, with a 95 percent confidence interval of 1,199 to 5,466. Valuing each averted birth using the United Nations' MILENA costing exercise for the Dominican Republic (UNFPA República Dominicana, 2021) yields US$935 in health-system savings alone, or US$10,910 when foregone education, labor-market earnings, fiscal revenue, and related costs are included (2025 US dollars). These values imply total benefits of US$3.1 million under the health-only valuation and US$36.4 million under the full opportunity-cost valuation.

Spread over the 65 treated municipality-years that generated the estimated effect, the implied break-even budget is approximately US$48,000 per municipality-year under the health-only valuation and US$560,000 under the full social valuation. Even the more conservative health-only figure is informative as a benchmark. Because an AU is a staffed service embedded in an existing public hospital rather than a new stand-alone facility (Section 2), some capital costs are shared with the host hospital. The administrative data do not record AU staffing composition, wages, commodities, or overhead, however, so the exercise is expressed as a break-even budget per municipality-year rather than as a cost estimate built from observed inputs.

Table 8: Break-even value of opening a first AU: births averted, valuation, and implied budget ceiling

	2018 US$	2025 US$
Panel A. Estimated effect		
Adolescent births averted, 2016–2025 [95% CI]	3,333 [1,199–5,466]	
Exposed teen-woman-years	518,291	
Treated municipality-years	65	
Panel B. Value per averted adolescent birth		
Health-system savings only	729	926
Full opportunity-cost value	8,510	10,807
Panel C. Implied benefits and break-even budget ceiling		
Total benefit: health only	2,431,072	3,087,462
Total benefit: full opportunity cost	28,362,509	36,020,387
95% CI, full opportunity cost	10,203,015–46,513,494	
Break-even cost per municipality-year: health only	37,401	47,499
Break-even cost per municipality-year: full opportunity cost	436,346	554,160

Notes: Treatment is a municipality's first AU opening (20 municipalities). Births averted = Callaway–Sant'Anna ATT (−6.43, SE 2.10) × exposed teen-women-years; brackets = 95% CI propagated from the ATT. Registration-based birth counts undercount true births (completeness 92.2% in 2019, World Bank SP.REG.BRTH.ZS). Because registration-based birth counts understate true births, the estimated births averted and all benefit figures are conservative lower bounds. Per-birth values divide MILENA's annual national totals (UNFPA 2021, data year 2018) by the 28,791 adolescent births reported for 2019, an annual-flow approximation. 'Total opportunity cost' adds foregone education, labor-force participation, earnings, and fiscal revenue to health spending. 2025 US$ uses the ratio of the BLS CPI-U annual averages for 2025 (321.943) and 2018 (251.107). The break-even budget is the maximum annual cost an AU could incur per treated municipality-year while still generating benefits at least as large as costs. The calculation applies the average ATT to all post-treatment years and counts only treated municipalities' own exposure. For context, the MILENA exercise estimates the national annual opportunity cost of adolescent pregnancy at roughly US$245 million (0.29 percent of GDP), reports a 33:1 ratio between treatment costs and the prevention plan, and is consistent with broader evidence that contraceptive services in LMICs yield roughly 3:1 returns.

There are three considerations that are important when interpreting these calculations. First, if birth registration completeness is unaffected by treatment, incomplete registration makes the averted-birth count and associated benefits conservative, and therefore the estimated births averted and all associated benefit calculations are considered lower bounds. Second, the calculation applies the average ATT to all post-opening years, even though the dynamic estimates show that the effect builds from near zero in the opening year. Finally, it counts only treated municipalities' own exposure and does not incorporate the responses at ages 20–21 or possible neighboring-municipality spillovers documented elsewhere in the paper. The full MILENA valuation treats the observed education and earnings penalties associated with adolescent childbearing as causal, whereas a long literature argues that selection explains part of those gaps (Geronimus and Korenman, 1992; Hotz et al., 2005; Ashcraft et al., 2013). For that reason, the health-only valuation is the more conser-

vative benchmark.

Taken together, these calculations suggest that the AU model is likely to be economically attractive in the DR even under conservative assumptions. In this context, where the AUs operate inside existing public hospitals and adolescent pregnancy remains a stated policy priority, the relevant margin is less whether the program can plausibly pay for itself than whether the public system can expand and sustain it where adolescents can actually reach it. The heterogeneity results imply that this condition is most likely to hold in municipalities with effective access to hospital-based services, while extending similar benefits to more remote areas may require adapting the model through outreach or other delivery channels rather than relying on hospital-based provision alone.

9 Discussion

The estimated 12 percent reduction in adolescent births is substantial and lies within the range of evidence on reproductive-health access and pregnancy prevention. Adolescent fertility rises when family-planning funding or clinics contract (Packham, 2017; Fischer et al., 2018; Galárraga and Harris, 2021), while subsidized contraception, school-based health centers, and combined contraceptive and information programs reduce births or pregnancies (Lindo and Packham, 2017; Lovenheim et al., 2016; Kaytan et al., 2026; Angrist and Anabwani, 2026). What distinguishes the present setting is not only the magnitude of the response, but also the policy margin under study. The estimate captures first access to a dedicated, adolescent-friendly point of entry inside the public hospital system. It therefore speaks to service delivery, not only to the price of contraception or the provision of information.

The age and outcome patterns add more meaning to the interpretation, as births decline most at ages 18 and 19, conceptions begin to fall at age 17, and effects extend into ages 20 and 21 as exposed cohorts leave adolescence. Births, pregnancy events, and abortion-related attendances also move in the same direction. These patterns suggest fewer pregnancies during late adolescence rather than a short displacement within the teen years or substitution across recorded pregnancy outcomes. They do not reveal whether completed fertility eventually changes, because the observation window cannot follow exposed cohorts far enough into adulthood. Even so, postponing births beyond late adolescence may matter for schooling, labor-market entry, and maternal and child health.

The mechanism evidence is also coherent, because AUs are embedded in public hospitals, and the largest effects appear where adolescents live closer to those hospitals, while most socioeconomic moderators show weaker differences. This pattern suggests complementarity between the units and existing health-system access. A dedicated service can reduce non-price barriers within a hospital, which implies that a hospital-based model is likely to be most effective where the public health system is already within reach. Municipalities with hospital capacity but no adolescent-friendly services are the most direct candidates for additional units, whereas remote municipalities may

require outreach, school linkage, transport support, or mobile provision (cf. Galárraga et al., 2024).

The contrast between first openings and modernizations also matters for scale-up. Municipalities receiving first access drive the broader modernization estimate; upgrades in municipalities with pre-existing adolescent services show no detectable fertility response. This does not imply that upgrades have no value, since they may improve privacy, quality, capacity, or outcomes not observed here. It does imply that the marginal fertility benefit of establishing an initial point of access may exceed the benefit of improving an existing one. Facility-level data on staffing, service mix, commodities, and utilization would be needed to identify which upgrade components are productive.

The economic evidence should be interpreted as a decision threshold. Embedding AUs within hospitals may allow them to share infrastructure, but the available data omit staffing, wages, contraceptive commodities, and overhead. The break-even budgets in Section 8 show how much the program could cost before estimated benefits are exhausted. Collecting comparable operating-cost data across units would allow policymakers to compare first openings, upgrades, and alternative delivery models on the same basis.

Several limitations define the scope of the findings. The estimates are municipality-level intention-to-treat effects of access, not treatment-on-the-treated effects of AU use, because unit-level utilization data are unavailable. Only twenty municipalities identify the first-opening effect, so subgroup comparisons are much less precise than the average effect. Registered births undercount true births, so the averted-birth calculations are conservative, but differential registration changes could bias the ATT. Hospital records capture abortion-related attendances rather than all induced abortions. The estimates also describe marginal expansion municipalities in a middle-income setting with relatively dense facility delivery, not metropolitan areas served before 2016, or settings with much thinner infrastructure. Spatial spillovers could attenuate comparisons with nearby controls, but the evidence cannot establish a lower bound. Finally, the schooling estimates are exploratory because exposed cohorts are too young for completed schooling or labor-market outcomes.

These limitations also point to several priorities for future research. First, administrative utilization data, if collected in the future, would allow the municipality-level effects estimated here to be linked more directly to service uptake and would sharpen the mechanism analysis. Second, the SNS hospital-registry pregnancy series, because it is available ahead of complete MSP birth registrations, it could support closer-to-real-time monitoring of adolescent pregnancy trends and of the health-system response to new interventions. Third, the longer-run consequences of the AU expansion remain an important open question. As the treated cohorts age, this setting should make it possible to study whether the AUs increase completed schooling, later earnings, and next-generation health. The suggestive rise in secondary enrollment documented here is consistent with the beginning of that human-capital pathway, but recovering completed attainment and labor-market effects will require a cohort-exposure design once the adolescents exposed during 2020–2025 have

fully aged out of school. For now, the evidence supports a narrower but policy-relevant conclusion, which is that expanding adolescent-friendly service delivery through dedicated AUs inside public hospitals can meaningfully reduce adolescent fertility, especially where adolescents can realistically access the health system.

10 Conclusion

This paper asks whether AFHS can reduce adolescent fertility when they are scaled through the public health system. Using the Dominican Republic's staggered roll-out of dedicated adolescent units inside public hospitals, I find that opening a municipality's first AU reduces the adolescent birth rate by about 12 percent of the pre-opening mean. The response follows the gestational lag, appears in independent pregnancy records, and is concentrated at ages and locations consistent with adolescents gaining access to confidential reproductive-health care. Taken together, the results indicate that expanding confidential, youth-oriented care can prevent pregnancies rather than merely shifting where births are recorded or delivered.

I show that a staffed adolescent service embedded in an existing public hospital can generate meaningful reductions in teen births at costs that appear modest relative to the benefits. At the same time, the results make clear that access remains geographic, because findings suggest that the model works best where adolescents can reach the health system. In that sense, the paper supports both further expansion of the AU model in underserved municipalities with hospital capacity and complementary approaches for adolescents who remain beyond the reach of hospital-based care. More broadly, the findings provide population-level evidence that the AFHS model can affect not only service use but fertility itself when it is implemented as a real point of entry into care.

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Appendix A Additional Tables

Table A1: Robustness to estimator choice

	First AU opening	First AU opening or modernization
Callaway–Sant'Anna	-6.43^{***} (2.08) [−10.51, −2.35]	-3.95^{**} (1.95) [−7.77, −0.13]
Sun–Abraham	-8.01^{***} (2.07) [−12.07, −3.95]	-5.17^{**} (2.45) [−9.97, −0.37]
Borusyak et al. (imputation)	-5.57^{*} (2.97) [−11.39, 0.25]	-5.64^{*} (2.87) [−11.27, −0.01]
TWFE (static benchmark)	−5.00 (3.16) [−11.19, 1.19]	-4.73^{*} (2.68) [−9.98, 0.52]
Municipalities (clusters)	146	155
Observations (municipality-years)	1,460	1,550

Notes: Overall ATT on the adolescent (15–19) birth rate, level outcome, no controls; municipality-clustered standard errors in parentheses. Gardner (2022) two-stage omitted because it is numerically identical to Borusyak imputation. Controls are omitted here because, being time-invariant, they are absorbed by municipality fixed effects in every estimator except Callaway–Sant'Anna. The Callaway–Sant'Anna row is re-estimated within the estimator-comparison script; its bootstrap standard error can differ trivially from Table 3 across bootstrap runs. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A2: Effect by year since first AU opening

	Year 0	Year 1	Year 2
Callaway–Sant'Anna	−4.33 (2.69)	−5.39* (2.87)	−9.51*** (3.37)
	[−9.60, 0.94]	[−11.02, 0.24]	[−16.12, −2.90]
TWFE distributed-lag	−4.13 (2.57)	−4.11 (2.85)	−9.64*** (3.46)
	[−9.17, 0.91]	[−9.70, 1.48]	[−16.42, −2.86]
Treated municipalities identifying horizon	20	19	13

Notes: Dynamic ATT on the adolescent (15–19) birth rate after a first AU opening; full panel of 146 municipalities, 1,460 municipality-years. Municipality-clustered standard errors in parentheses. Capped at Year 2: Years 3–4 are identified off only 7 and 4 municipalities. The small, statistically insignificant Year-0 effect is consistent with the ∼9-month gestational lag. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A3: Event-time (lead and lag) estimates

Years since opening (e)	First AU opening	First AU opening or modernization
Leads (pre-treatment)		
-5	2.73 (6.07) [−9.17, 14.63]	1.71 (5.67) [−9.40, 12.82]
-4	−1.52 (5.53) [−12.36, 9.32]	−1.30 (4.16) [−9.45, 6.85]
-3	−6.08 (5.03) [−15.94, 3.78]	0.31 (4.60) [−8.71, 9.33]
-2	−2.57 (4.71) [−11.80, 6.66]	2.03 (3.22) [−4.28, 8.34]
-1 (reference)	0.00	0.00
Lags (post-treatment)		
0	−4.33 (2.69) [−9.60, 0.94]	−2.42 (1.96) [−6.26, 1.42]
1	−5.39 (2.87) [−11.02, 0.24]	−3.12 (2.19) [−7.41, 1.17]
2	−9.51 (3.37) [−16.12, −2.90]	−4.05 (3.31) [−10.54, 2.44]
$3^{\dagger}$	−12.52 (6.89) [−26.02, 0.98]	−8.85 (4.51) [−17.69, −0.01]
$4^{\dagger}$	−21.06 (2.46) [−25.88, −16.24]	−11.81 (5.58) [−22.75, −0.87]
Joint pre-trend test ($e \in [-5, -2]$), p	0.68	0.85

Notes: Callaway–Sant'Anna dynamic ATT on the adolescent (15–19) birth rate, universal base period $e = -1$; municipality-clustered standard errors in parentheses. The joint pre-trend test averages the plotted leads $e \in [-5, -2]$. †Years 3–4 are identified off only 7 and 4 treated municipalities in the first-opening design and are imprecise. $^{***}p < 0.01$, etc.

Table A4: Placebo tests and sensitivity to parallel-trends violations

Panel A. Placebo tests

	Estimate (SE) [95% CI]	p
Fake-timing placebo (openings recoded 3 years early)[a]	−4.84 (7.06) [−18.68, 9.00]	0.49
Average pre-treatment cell, level[b]	3.35 (3.69) [−3.88, 10.58]	0.36
Average pre-treatment cell, triple difference[b]	0.68 (2.24) [−3.71, 5.07]	0.76

Panel B. Rambachan–Roth confidence sets for the average post-treatment effect

	Level (15–19 rate)	Triple difference
$\bar{M} = 0$ (parallel trends exact)	$[-14.30, -1.58]^{\dagger}$	$[-11.67, -1.98]^{\dagger}$
$\bar{M} = 0.1$	$[-15.92, -0.71]^{\dagger}$	$[-12.62, -1.53]^{\dagger}$
$\bar{M} = 0.2$	[−17.74, 0.45]	$[-14.00, -0.64]^{\dagger}$
$\bar{M} = 0.3$	[−19.69, 1.88]	[−15.68, 0.54]

Notes: [a]Each cohort is assigned a counterfeit opening three years before its true one and the design is re-estimated on 2016–2019 data only, before any true opening; a null says the design does not manufacture effects from trends alone. [b]Influence-function average of the raw pre-treatment $ATT(g,t)$ cells, the complement to the dynamic-lead averages printed on the event-study figures; both should be (and are) indistinguishable from zero. Placebos on the birth rates of unexposable age groups appear in Appendix Table A8. Panel B reports Rambachan and Roth (2023) C-LF confidence sets allowing post-treatment parallel-trends violations up to $\bar{M}$ times the largest pre-treatment violation; †marks sets that exclude zero. The level estimate survives violations up to 10 percent of the maximal pre-treatment trend and the triple difference, with its flatter pre-period, up to 20 percent. The pre-period is estimated on twenty treated clusters, so this yardstick is itself noisy; the identification case rests on the convergence of leads, placebos, timing, and the age gradient rather than on any single bound.

Table A5: Newborn outcomes: population burden and birth composition

Outcome	Baseline	Estimate (SE) [95% CI]	% of baseline	p-value
Panel A. Population burden (per 1,000 women aged 15–19)				
Low birthweight (<2,500g)	8.2	−1.03 (0.72) [−2.44, 0.38]	−12.5%	0.15
Preterm (<37 weeks)	3.8	−0.48 (0.36) [−1.18, 0.23]	−12.5%	0.18
C-section births	26.4	−0.83 (1.11) [−3.00, 1.33]	−3.2%	0.45
Small for gestational age	6.1	−1.06 (0.69) [−2.42, 0.30]	−17.3%	0.13
Panel B. Share of teen births (descriptive; selection-confounded)				
Low birthweight (<2,500g)	15.5	−1.05 (1.67) [−4.32, 2.22]	−6.8%	0.53
Preterm (<37 weeks)	7.1	0.02 (0.67) [−1.28, 1.33]	+0.3%	0.97
C-section share	50.6	3.48*** (1.18) [1.16, 5.80]	+6.9%	0.00
Small for gestational age	11.9	−1.71 (1.39) [−4.43, 1.01]	−14.4%	0.22

Notes: Each row reports a Callaway–Sant'Anna group ATT using the first-opening design, not-yet-treated controls, and municipality-clustered multiplier-bootstrap standard errors. Panel A expresses newborn outcomes per 1,000 women aged 15–19 and is the appropriate intention-to-treat scale for asking whether the population burden of adverse newborn outcomes changes after an AU opening. Panel B re-expresses the same outcomes as shares of teen births and is descriptive only, because conditioning on births is post-treatment and therefore selection-confounded. Baseline is the treated mean in the year before opening. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A6: Heterogeneity of the effect by baseline municipality characteristics

	More underserved	Less underserved	Gap	p_{diff}
Health access (distance)				
Distance to nearest facility (km)	-2.73 (2.69) [-8.00, 2.54]	-10.18*** (3.08) [-16.22, -4.14]	7.44	0.069
Distance to nearest facility (km, excl. SD)	-1.39 (3.67) [-8.58, 5.80]	-12.16*** (3.88) [-19.76, -4.56]	10.78	0.044
Two-step floating catchment access	-6.65*** (1.92) [-10.41, -2.89]	-6.17* (3.18) [-12.40, 0.06]	-0.47	0.899
Contraceptive access				
Unmet need for contraception	-9.12*** (2.46) [-13.94, -4.30]	-3.69 (2.56) [-8.71, 1.33]	-5.43	0.126
Contraceptive prevalence (any method)	-7.18*** (1.96) [-11.02, -3.34]	-5.68 (3.48) [-12.50, 1.14]	-1.50	0.707
Satisfied demand for contraception	-7.64*** (1.43) [-10.44, -4.84]	-5.20 (3.52) [-12.10, 1.70]	-2.43	0.522
Health supply (density)				
All facilities per 10,000	-6.78*** (1.41) [-9.54, -4.02]	-6.09 (4.35) [-14.62, 2.44]	-0.69	0.880
Primary-care facilities per 10,000	-6.78*** (1.30) [-9.33, -4.23]	-6.09 (4.33) [-14.58, 2.40]	-0.69	0.879
Hospitals per 10,000	-6.73*** (1.81) [-10.28, -3.18]	-6.15* (3.17) [-12.36, 0.06]	-0.58	0.874
Socioeconomic and geography				
Urban share (rural vs. urban)	-7.20** (3.21) [-13.49, -0.91]	-5.71*** (1.95) [-9.53, -1.89]	-1.49	0.692
Wealth index (census)	-6.11* (3.55) [-13.07, 0.85]	-6.78*** (1.73) [-10.17, -3.39]	0.67	0.866
Subsidized insurance (%)	-6.15* (3.25) [-12.52, 0.22]	-6.73*** (1.42) [-9.51, -3.95]	0.58	0.870
Adolescent vulnerability and school attachment				
Frequently absent from school	-9.29*** (3.06) [-15.29, -3.29]	-3.57 (2.20) [-7.88, 0.74]	-5.72	0.129
Parental school participation	-9.44*** (1.74) [-12.85, -6.03]	-3.45 (3.53) [-10.37, 3.47]	-5.99	0.128
Family expects university completion	-8.33** (3.56) [-15.31, -1.35]	-4.53* (2.32) [-9.08, 0.02]	-3.80	0.372
Mother completed secondary	-6.19** (2.69) [-11.46, -0.92]	-6.69*** (2.44) [-11.47, -1.91]	0.49	0.892

Notes: Subgroup Callaway–Sant'Anna ATTs, plotted in Figure 6: the 20 treated municipalities are split at each baseline's median ($n = 10$ per half, at $g-1$) against the shared never-treated controls (municipality-clustered bootstrap; single seed, fixed estimation order). The excl.-SD row repeats the proximity split among the 16 treated municipalities outside the Santo Domingo metropolitan provinces ($n = 8$ per half). The two-step floating catchment (2SFCA) index measures facility supply within overlapping 20-km catchments per 10,000 women, discounting each facility by the population competing for it, so it captures congestion-adjusted access rather than simple distance. p_{diff} tests the underserved−served gap (conservative: shared controls) and is unadjusted for the number of contrasts; proximity in the full sample ($p = .069$) has a Holm-adjusted p of 0.83, so the splits are read as descriptive. Overall ATT $= -6.43$. Sources: NHS registry (1,588 facilities; distance and density), ENDESA 2013 (contraception and subsidized insurance, province level), 2022 census (wealth, urbanization); the school-attachment panel uses the 2019 Evaluación Diagnóstica family questionnaire (MoE; family-weighted municipality shares, school year 2018–19, pre-dating every opening in the sample). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A7: Effect by mother's age: age at birth vs. age at conception

	Age at birth	Age at conception
15–19 (main estimate)	-6.43^{***} (2.12)	-8.22^{***} (2.47)
% of baseline	*−12%*	*−12%*
Single years		
15	0.11 (0.51)	-0.74^{**} (0.37)
16	−0.67 (0.49)	−0.17 (0.46)
17	−0.41 (0.45)	-2.73^{***} (0.97)
18	-2.84^{***} (0.97)	-2.33^{**} (0.95)
19	-2.62^{**} (1.04)	-2.25^{**} (0.93)
Subtotals		
15–17	−0.97 (0.81)	-3.63^{***} (1.32)
18–19	-5.45^{***} (1.59)	-4.58^{***} (1.46)

Notes: Callaway–Sant'Anna group ATT, first AU opening (20 treated municipalities), not-yet-treated controls, municipality-clustered SEs in parentheses; panel years 2018–2025 for both columns (the mother's date of birth, needed for age at conception, is recorded from 2018; the age-at-birth main estimate is identical on 2016–2025 and 2018–2025, −6.43, as all openings occur 2020+). Age at conception = age at (birth date − recorded gestational age). Outcome = births to mothers of the given age per 1,000 women 15–19 (common denominator), so single-year rows sum to the 15–19 total. Records with inconsistent date-of-birth/gestation combinations (≈2.4%) excluded. The two classifications tell one story at different clocks: conceptions are prevented from age 17 up, which appears as fewer births at 18–19. Sensitivity: dating conception by a fixed 40 weeks instead of recorded gestation gives −8.42; extending to 2016–2025 by allocating pre-2018 births in expectation by their actual gestational age gives −8.04 (−12%). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A8: The age gradient: effects by the mother's age band

	Estimate (SE) [95% CI]	Baseline	% of baseline	Avg. lead (p)
Panel A. Age-band birth rates (own denominators, 2016–2025)				
10–14	0.01 (0.30) [−0.58, 0.60]	2.1	0.3%	0.15 (.56)
15–19	−6.43*** (2.10) [−10.55, −2.31]	55.3	−11.6%	−1.86 (.68)
20–24	−8.19*** (2.77) [−13.62, −2.76]	110.2	−7.4%	−4.34 (.45)
25–29	−6.33 (4.21) [−14.58, 1.92]	103.8	−6.1%	−4.03 (.52)
30–34	−0.70 (2.40) [−5.40, 4.00]	71.2	−1.0%	−2.87 (.47)
Panel B. Single years 20–24 (per 1,000 women 20–24, 2018–2025)				
20	−2.42*** (0.67) [−3.73, −1.11]	21.4	−11%	
21	−2.01** (0.83) [−3.64, −0.38]	22.1	−9%	
22	−0.93 (1.04) [−2.97, 1.11]	22.5	−4%	
23	−2.12 (1.32) [−4.71, 0.47]	23.1	−9%	
24	−0.71 (0.82) [−2.32, 0.90]	21.1	−3%	

Notes: Callaway–Sant'Anna group ATT, first AU opening (20 treated municipalities), not-yet-treated controls, municipality-clustered SEs in parentheses; baseline = treated mean in the year before opening. The last column reports the average of the dynamic leads $e \in [-5, -2]$ with its influence-function p-value, the same lead test printed on the event-study figures. Cohort arithmetic predicts the ordering ex ante: the units serve ages 10–19 and the first in-sample opening is 2020, so treated adolescents can age at most to 24 by 2025; ages 20–24 are mechanically reachable (aged-out treated teens; postponed births), 25–29 only marginally (modernization margin, to age 27), and 30–34 cannot be exposed in any study year and serves as the comparison group. The response concentrates at ages 20–21, just past the eligibility boundary, and fades to zero by 24; births at 10–14 are too rare (2.1 per 1,000) for the design to detect changes, and the 30–34 band is a precise zero. A correlated municipality-level shock would not decay in age exactly where program exposure does. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A9: Sensitivity to alternative specifications, samples, and comparison groups

	Level effect Estimate (SE) [95% CI]	Triple difference Estimate (SE) [95% CI]
Main estimate (not-yet-treated comparisons)	−6.43*** (2.10) [−10.55, −2.31]	−7.48*** (1.68) [−10.77, −4.19]
Never-treated comparisons only	−6.45*** (2.05) [−10.47, −2.43]	−5.76** (2.45) [−10.56, −0.96]
Population-weighted	−4.47* (2.68) [−9.72, 0.78]	−6.63*** (1.17) [−8.92, −4.34]
One year of anticipation allowed	−3.78 (3.36) [−10.37, 2.81]	N/A
Drop imputed-date municipalities	−5.02** (2.21) [−9.35, −0.69]	−8.68*** (1.93) [−12.46, −4.90]
Intercensal population denominators	−6.42*** (2.02) [−10.38, −2.46]	−4.37*** (1.41) [−7.13, −1.61]
Drop the Santo Domingo metropolitan area	−6.69*** (2.49) [−11.57, −1.81]	N/A
Log specification (log points)	−0.13*** (0.05) [−0.23, −0.03]	N/A
DDD comparison age 20–24 (exposed; invalid)	N/A	0.48 (1.46) [−2.38, 3.34]
DDD comparison age 25–29 (exposed; invalid)	N/A	0.34 (2.79) [−5.13, 5.81]
Treatment dating		
La Vega recoded to 2022	−5.90*** (2.06) [−9.94, −1.86]	N/A
Drop Constanza	−7.47*** (2.07) [−11.53, −3.41]	N/A
Covariate vintage (covariate-adjusted specification)		
Covariates from the 2022 census	−5.44** (2.55) [−10.44, −0.44]	N/A
Covariates from the 2010 census	−4.33* (2.62) [−9.48, 0.81]	N/A
Spatial spillovers		
Treatment by proximity (unit within 20 km)	−2.52 (3.43) [−9.24, 4.20]	N/A
Donut: drop controls within 10 km of a unit	−6.55*** (2.21) [−10.88, −2.22]	N/A
Donut: drop controls within 20 km	−7.16*** (2.38) [−11.82, −2.50]	N/A
Donut: drop controls within 30 km	−9.92*** (2.91) [−15.62, −4.22]	N/A

Notes: Each row varies one ingredient of the design and re-estimates the level effect and the triple difference (15–19 versus 30–34). Standard errors are municipality-clustered multiplier-bootstrap standard errors. Triple-difference estimates use the same tripledif estimator as the main design, combining cohort-specific not-yet-treated comparisons by GMM. The anticipation and drop-metropolitan-area variants are unavailable for the triple difference because the corresponding restricted designs leave comparison cohorts too thin for stable GMM aggregation. The intercensal triple difference is smaller because that denominator construction shifts the two age bands differentially before treatment, which the triple difference absorbs. The log specification is reported in log points; −0.13 corresponds closely to a 12 percent decline. The comparison-age rows replace ages 30–34 with age groups that can themselves be exposed to the program, so the collapse of the triple difference is mechanically expected. La Vega's unit existed by mid-2022 but its documented event is a 2023 reinauguration, which is the coded date in the main analysis; Constanza's 2019 baseline appears inflated by a local registration shift, so retaining it is conservative. The covariate-vintage rows re-estimate the covariate-adjusted specification and are therefore not directly comparable to the unconditional rows. Spatial spillovers would bias the main estimate toward zero; deleting controls near treated municipalities strengthens the estimate, consistent with limited cross-border contamination. Matching estimates are reported separately in Appendix Table A10. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A10: Matched-control estimates

Panel A. Effect of AU openings on the teen birth rate in matched samples

Matching scheme	Controls	Estimate (SE) [95% CI]	% of baseline
Structural + log population (primary)	34	−3.67 (2.50) [−8.57, 1.23]	−6.6%
+ 2019 birth composition	33	−4.07* (2.28) [−8.54, 0.40]	−7.4%
Eligible-hospital pool + structural	33	−2.95 (2.42) [−7.69, 1.79]	−5.3%
+ baseline fertility and proximity	29	−3.08 (2.61) [−8.20, 2.04]	−5.6%
Propensity score (logit)	23	−2.89 (2.81) [−8.40, 2.62]	−5.2%

Panel B. Covariate balance under the primary scheme (standardized differences)

	Before matching	After matching
Matched on:		
Wealth index	1.33	0.98
% urban	0.44	0.50
% women 20–49 with secondary or more	1.32	0.95
NHS facilities per 10,000	-0.94	-0.56
% enrolled in SeNaSa (DHS 2013)	-0.14	-0.12
Log population (2019)	1.53	0.93
Not matched on:		
Teen birth rate, 2019	0.19	0.24
NHS hospitals (count)	0.86	0.70
Mother's age, 2019 births	0.32	0.17
% Haitian mothers	-0.03	0.24
% without insurance	-0.48	-0.20
% c-section	0.20	-0.18

Notes: Each estimate in Panel A re-estimates the Callaway–Sant'Anna first-opening specification on a matched sample consisting of the 20 treated municipalities and the listed matched controls, using not-yet-treated comparisons within the matched pool and municipality-clustered multiplier-bootstrap standard errors. Schemes 1–4 use nearest-neighbor Mahalanobis matching with replacement (1:3) on the listed covariates; the final scheme matches on a logit propensity score. The baseline adolescent birth rate is never included as a matching variable, so its post-match balance in Panel B is informative rather than imposed mechanically. Residual imbalance on wealth, education, and population reflects limited overlap between treated municipalities and the never-treated pool. The matched estimates are therefore reported as a sensitivity exercise rather than as a competing main design. Percent effects are relative to the treated mean in the year before opening ($g-1$), equal to 55.28 births per 1,000 women aged 15–19. Matched municipalities are listed in Appendix Table A11. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.10$.

Table A11: Match assignments: treated municipalities and their nearest structural neighbors

Treated municipality	First AU	Matched never-treated controls
NIZAO	2020	JIMA ABAJO, GASPAR HERNÁNDEZ, EL FACTOR
SANTO DOMINGO NORTE	2020	BONAO, MOCA, SAN JUAN
CASTILLO	2021	SÁNCHEZ, CEVICOS, SABANA GRANDE DE PALENQUE
SAN CRISTÓBAL	2021	BONAO, BAJOS DE HAINA, SAN JUAN
CONSTANZA	2022	VILLA RIVA, VILLA LA MATA, EL FACTOR
IMBERT	2022	ALTAMIRA, SAN GREGORIO DE NIGUA, CAMBITA GARABITOS
LOS ALCARRIZOS	2022	AZUA, BONAO, SAN JUAN
BARAHONA	2023	AZUA, SABANA GRANDE DE BOYÁ, BAYAGUANA
COTUÍ	2023	BONAO, HATO MAYOR, VILLA LA MATA
LA ROMANA	2023	VILLA HERMOSA, BISONÓ, MAO
LA VEGA	2023	MOCA, BONAO, SAN JUAN
SAN PEDRO DE MACORÍS	2023	MAO, VILLA HERMOSA, AZUA
YAMASÁ	2023	MONTE PLATA, PERALVILLO, SABANA GRANDE DE BOYÁ
BOCA CHICA	2024	BAJOS DE HAINA, BONAO, AZUA
FANTINO	2024	SABANA GRANDE DE PALENQUE, SÁNCHEZ, CABRERA
HIGÜEY	2024	ESPERANZA, MAO, VILLA HERMOSA
LAS MATAS DE FARFÁN	2024	PADRE LAS CASAS, VILLA RIVA, LAS YAYAS DE VIAJAMA
LAS TERRENAS	2024	GUAYACANES, LAS GUÁRANAS, SABANA IGLESIA
PEDRO BRAND	2024	AZUA, BAJOS DE HAINA, ESPERANZA
SAN IGNACIO DE SABANETA	2025	DAJABÓN, PARTIDO, PEPILLO SALCEDO

Notes: Primary matching scheme: each of the 20 in-window treated municipalities is matched with replacement to its three nearest never-treated neighbors by Mahalanobis distance on the wealth index, the urban share, women's secondary education, NHS facilities per 10,000, SeNaSa affiliation, and log 2019 population; the baseline adolescent birth rate is never a matching variable. Because matching is with replacement, a municipality can serve as a control for more than one treated municipality; the union of listed controls (34 municipalities) plus the 20 treated forms the estimation sample of the primary row in Appendix Table A10.

Table A12: Registration completeness and birth composition

	Full window 2016–2025	2019–2025 only
Panel A. Dropping the registration-ramp years (2016–2018)		
Level	-6.43^{***} (2.10) [−10.55, −2.31]	-6.43^{***} (2.11) [−10.57, −2.29]
Level, covariate-adjusted	-5.44^{**} (2.48) [−10.30, −0.58]	-5.44^{**} (2.49) [−10.32, −0.56]
Triple difference	-7.48^{***} (1.46) [−10.34, −4.62]	-7.48^{***} (1.67) [−10.75, −4.21]
Panel B. Compositional shifts after openings and their bounds		
% public-facility births, all births (pp)	-4.26^{***} (1.31) [−6.83, −1.68]	
% public-facility births, mothers 25–44 (pp)	-2.94^{**} (1.43) [−5.74, −0.13]	
leave-one-out, dropping each treated municipality	range [−4.90, −3.51]	
% Haitian mothers, all births (pp)	-3.49^{**} (1.74) [−6.90, −0.08]	
% Haitian mothers, mothers 25–44 (pp)	-3.07^{*} (1.65) [−6.30, 0.15]	
leave-one-out, dropping each treated municipality	range [−4.03, −2.47]	
Teen rate, conditioning on baseline shares	-7.69^{***} (2.27) [−12.14, −3.25]	
Teen rate, conditioning on contemporaneous shares[c]	−2.59 (2.55) [−7.59, 2.42]	

Notes: Panel A re-estimates the main design on the 2019–2025 window only. Because every first opening occurs in 2020 or later, the 2016–2018 registration ramp is entirely pre-treatment, so dropping those years leaves the estimates unchanged apart from trivial differences in standard errors. Panel B reports the two compositional shifts that accompany openings and the associated bounding exercises. Both shifts also appear among births to mothers aged 25–44, indicating that they reflect broader municipality-level changes in where births are recorded and which mothers appear in the registry, rather than teen-specific responses to the program. The leave-one-out rows re-estimate each shift dropping one treated municipality at a time (all-births specification); both ranges remain negative, so neither pattern is driven by a single municipality (the largest single-municipality movers are San Cristóbal for the facility share and Santo Domingo Norte for the Haitian share). Conditioning on the baseline values of the two shares strengthens the teen estimate. Conditioning on their contemporaneous values instead ([c]) conditions on post-treatment outcomes and is therefore reported only as a bad-control bound. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A13: Composition of the response by maternal education

	Estimate (SE) [95% CI]	Baseline	% of baseline
Birth rates by maternal education (per 1,000 women aged 15–19)			
Less than secondary	−1.93* (1.08) [−4.05, 0.19]	13.2	−14.6%
Secondary or more	−3.18** (1.54) [−6.20, −0.16]	33.5	−9.5%
Difference: less than secondary − secondary or more	1.25 (1.58) [−1.85, 4.35]		

Notes: Each row reports a Callaway–Sant'Anna group ATT using the first-opening design, not-yet-treated controls, and municipality-clustered multiplier-bootstrap standard errors. Outcomes are birth rates per 1,000 women aged 15–19, split by the mother's registered education. Maternal education is observed beginning in 2020, so these estimates are identified from the shorter 2020–2025 panel and the reported baselines are treated-group means over that period. The final row reports the difference between the two education-group estimates. The estimates indicate declines in both education groups and do not reject equality across them. The analogous split by age bands affected by the 2021 child-marriage ban appears in Table 7, Panel C. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A14: Utilization signals from hospital-production registries

	Estimate (SE) [95% CI]	Opening cohorts
Prenatal consultations to adolescents per 1,000 women 15–19	−15.0 (41.0) [−95.4, 65.5]	2021+
Adolescent share of prenatal consultations (pp)	3.35* (1.99) [−0.55, 7.25]	2021+
Counseling and family-planning visits per 1,000 women 15–49	−21.7 (24.0) [−68.8, 25.5]	2023+

Notes: Each row reports a Callaway–Sant'Anna group ATT using outcomes constructed from the public-facility Form 67-A hospital-production registries and assigned to municipalities as in Section 5.3. Standard errors are municipality-clustered multiplier-bootstrap standard errors. The consultation series begin in 2020, and counseling/family-planning visits are observed only from 2023, so only the listed opening cohorts contribute to each estimate. The prenatal-consultation share is the most informative utilization margin because it is less sensitive than consultation levels to changes in the number of pregnancies and is available for more cohorts. The Servicio Nacional de Salud confirmed in writing that no unit-level utilization records exist. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A15: Components of the household wealth index

Component	Share of households (%)	PC1 loading	Sampling adequacy
Panel A. Durable assets			
Cistern	24.10	+0.206	0.950
Internet subscription	51.60	+0.236	0.949
Radio or music system	35.30	+0.128	0.948
Television	75.60	+0.208	0.942
Rooftop water tank	44.40	+0.218	0.942
Landline telephone	17.90	+0.204	0.942
Private car	24.20	+0.213	0.941
Cell phone	87.60	+0.162	0.935
Air conditioning	18.40	+0.224	0.931
Laptop computer	23.30	+0.198	0.930
Power inverter	15.30	+0.153	0.925
Desktop computer	8.80	+0.144	0.922
Washing machine	78.60	+0.220	0.921
Refrigerator	83.50	+0.227	0.914
Tablet	25.50	+0.139	0.911
Electricity generator	3.10	+0.094	0.887
Panel B. Dwelling and services			
Household refuse collected	87.20	+0.134	0.938
Walls of block or concrete	82.10	+0.215	0.929
Piped water inside the dwelling	62.70	+0.181	0.929
Flush toilet	87.00	+0.225	0.924
Lighting from the electricity grid	98.30	+0.097	0.922
Kitchen inside the dwelling	88.80	+0.186	0.921
Finished floor (mosaic, granite, marble, ceramic)	46.60	+0.267	0.920
Connected to the public sewer network	24.10	+0.127	0.904
Dwelling is an apartment	18.30	+0.159	0.900
Cooks with gas or electricity	90.80	+0.201	0.899
Concrete roof	50.80	+0.253	0.898
Marginal dwelling (room, barracks, unfinished)	3.10	-0.072	0.889
More than two persons per bedroom	17.40	-0.076	0.850
Panel C. Specification search			
	Items	KMO	Corr. with ICV
Durable assets only (initial index)	18	0.896	-0.834
Drop motorbike	17	0.900	-0.837
Add dwelling and service items	31	0.907	-0.902
Index used	29	0.925	-0.906

Notes: The index is the first principal component of the 29 binary items in Panels A and B, estimated on 3,722,404 households of the 2022 census that report every item, standardized to mean zero and unit variance, then averaged to the municipality. Codings were verified against the census codebook; shares are unweighted incidence. Sampling adequacy is the item Kaiser-Meyer-Olkin measure. The overall KMO is 0.925 and the lowest item value is 0.85, both above the conventional .80 threshold. A stove and a motorbike were dropped from the initial specification because their adequacy fell below .85. Panel C reports the search: adding dwelling and service items and pruning weak ones raises the KMO from 0.896 to 0.925 and strengthens the correlation between the municipality index and the SIUBEN quality-of-life index from -0.834 to -0.906. That correlation is negative because the ICV scale runs in the opposite direction, so a larger magnitude means better validation.

Table A16: Effect by opening cohort

Cohort	Municipalities	Estimate (SE) [95% CI]	Baseline ($g-1$)	%	Max. exposure (yrs)
2020	2	-11.16^{***} (2.70) [−16.45, −5.87]	71.3	−16%	6
2021	2	-21.85^{***} (5.62) [−32.87, −10.83]	68.9	−32%	5
2022	3	1.97 (5.61) [−9.03, 12.97]	43.2	5%	4
2023	6	-7.88^{**} (3.82) [−15.37, −0.39]	55.4	−14%	3
2024	6	−4.84 (5.00) [−14.64, 4.96]	54.4	−9%	2
2025	1	7.82^{***} (1.47) [4.94, 10.70]	37.1	21%	1

Notes: Callaway–Sant'Anna cohort-level ATTs (group aggregation), first AU opening, not-yet-treated controls, municipality-clustered SEs in parentheses; overall group ATT = −6.43. Baseline = cohort mean teen rate in the year before its opening. Later cohorts are observed only at short exposures, where the dynamic effect is still small (Year 0 ≈ -4, building to ≈ -9.5 by Year 2), so small late-cohort ATTs are expected, not anomalous. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A17: New units versus upgrades: decomposing the modernization estimate

Panel A. New units versus upgrades

	Estimate (SE) [95% CI]	Baseline	% of base	Treated
First openings, 2020–2025 (main design; 20)	-5.72^{***} (1.90) [−9.44, −2.00]	55.6	−10.3	20
Recovered: first modern unit is new (3)	0.22 (1.21) [−2.15, 2.59]	45.8	+0.5	3
All new modern units (23)	-4.96^{**} (1.92) [−8.72, −1.20]	54.3	−9.1	23
Recovered: upgrades of pre-existing units (6)	0.05 (0.87) [−1.66, 1.76]	45.2	+0.1	6
Pooled modernization design (29)	-3.95^{**} (1.96) [−7.79, −0.11]	52.4	−7.5	29

Panel B. The 30–34 comparison band (placebo)

	First AU opening	First AU opening or modernization
Effect on the 30–34 birth rate	−0.70 (2.39) [−5.38, +3.98]	+3.27 (2.03) [−0.70, +7.24]
recovered nine municipalities alone	N/A	$+6.27^{**}$ (2.91) [+0.56, +11.98]
Implied triple difference (Table 3 level − band)	−5.73	−7.22

Notes: Callaway–Sant'Anna group ATTs; not-yet-treated comparisons, universal base period, municipality-clustered multiplier bootstrap, seed reset before each estimate. *Panel A*: the teen birth rate, estimated separately for each subset of the modernization design's treated municipalities against the shared never-treated comparisons, with modernization-cohort timing throughout. *Recovered* municipalities are the nine served before 2016, entering through their first modern-unit event: a newly built full-service unit (new) or a documented remodel (upgrade). Baseline = treated mean in the year before the modernization event. The pooled row reproduces the modernization column of Table 3; the upgrade subset is small and its estimate correspondingly imprecise. *Panel B*: the 30–34 birth rate, the triple difference's comparison band; women aged 30–34 cannot be exposed (the units serve ages 10–19, and the oldest woman exposable as an adolescent is 24 by 2025). The near-zero response under the first-opening design validates the comparison band; under the modernization design the band *rises*, driven entirely by the nine recovered municipalities, consistent with urban fertility postponement rather than the program. Subtracting the band effect from the level estimates of Table 3 reproduces its never-treated triple differences (−5.76, −7.23): the larger modernization triple difference is the comparison band moving, not a larger effect on teens. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A18: Functional form and births averted

	First AU opening	First AU opening or modernization
Implied % effect on the teen birth rate		
Callaway–Sant'Anna, linear (main estimate)	$-11.6\%^{***}$ (3.8) [−19.1%, −4.2%]	$-7.5\%^{**}$ (3.7) [−14.9%, −0.2%]
Callaway–Sant'Anna, log-rate	$-12.2\%^{***}$ (3.9) [−19.4%, −4.3%]	$-7.8\%^{*}$ (3.9) [−15.2%, 0.3%]
TWFE, Poisson (count)	$-15.5\%^{**}$ (6.3) [−27.0%, −2.3%]	$-9.6\%^{**}$ (4.6) [−18.3%, −0.1%]
Teen births averted (treated, 2016–25) [95% CI]	3,333 [1,199–5,466]	7,025 [193–13,857]

Notes: Implied % = effect relative to the pre-treatment baseline rate (linear: coefficient/baseline; log/Poisson: $e^{\beta}-1$; the log outcome is log(1+rate)); delta-method standard errors on the percent scale in parentheses, exact-transform 95% confidence intervals in brackets. The CS log-rate row is the heterogeneity-robust proportional effect (Callaway–Sant'Anna has no count/Poisson family). The Poisson row is a static two-way fixed-effects count model with a log-population offset and is the literal analog of Packham's (2017) ln-rate specification, population-weighted via the birth count. **Births averted** = CS ATT × exposed teen-women-years in treated municipalities after opening; brackets = 95% CI propagated from the ATT (normal approx.). Rough welfare approximation: it applies the *average* ATT to *all* post-treatment years (the effect actually builds from ≈ 0 in Year 0, so it is a total, not year-accurate), counts treated municipalities' OWN exposure only (no spillovers), and uses denominator A. Sanity check: treated municipalities recorded 21,569 teen births in these post-opening cells, so the first-opening counterfactual is ≈24,902; AUs prevented ≈13% of the would-be teen births. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

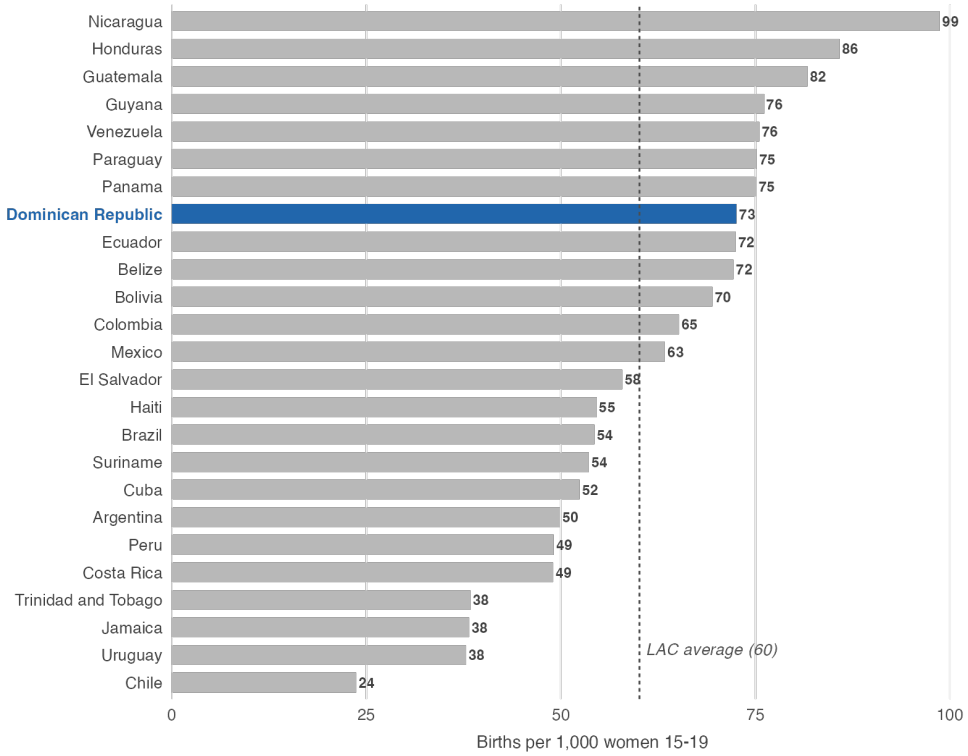
Table A19: Sensitivity to the population denominator

	Estimate (SE) [95% CI]	% of baseline
Panel A. Alternative denominator constructions		
A: Hamilton–Perry projection anchored on NSO estimates (main series)		
Teen birth rate	-6.43^{***} (2.10) [−10.55, −2.31]	−11.6
Triple difference	-7.48^{***} (1.68) [−10.77, −4.19]	N/A
B: Intercensal series based on the 2010 and 2022 censuses		
Teen birth rate	-6.42^{***} (2.03) [−10.39, −2.44]	−11.3
Triple difference	-4.37^{***} (1.41) [−7.13, −1.61]	N/A
Panel B. Differential post-treatment denominator error		
Over-projection $\delta = 0\%$ (main estimate)	-6.43^{***} (2.10) [−10.55, −2.31]	
$\delta = 3\%$	-5.10^{**} (2.10) [−9.22, −0.99]	
$\delta = 6\%$	-3.78^{*} (2.09) [−7.88, 0.32]	
$\delta = 9\%$	−2.46 (2.06) [−6.50, 1.59]	
$\delta = 12\%$	−1.14 (2.07) [−5.20, 2.93]	
$\delta = 15\%$	0.19 (2.06) [−3.85, 4.23]	
Realized differential drift, A vs. B (%)	−0.37 (0.47) [−1.29, 0.55]	

Notes: Each cell reports a Callaway–Sant'Anna group ATT for the first AU opening using not-yet-treated controls, a universal base period, and municipality-clustered multiplier-bootstrap standard errors. NSO publishes municipality population only through 2020, so post-2020 denominators must be projected. Series A is the paper's main Hamilton–Perry projection and does not use the 2022 census. Series B is an intercensal construction based on the 2010 and 2022 censuses and uses NSO only through province growth rates. A back-test against the held-out 2022 census implies that the official series over-projects women aged 15–19 by about 10 percent; removing that bias uniformly rescales the level estimate and the baseline rate together, leaving the implied percentage effect essentially unchanged. The smaller triple-difference estimate under series B reflects that the intercensal construction shifts the 15–19 and 30–34 denominators differentially in pre-treatment years, which the triple difference absorbs mechanically. Panel B rescales the treated post-opening rate by $1 + \delta$, which is algebraically equivalent to a differential over-projection of δ in the treated post-opening denominator. The final row estimates the same specification on the ratio of the two independently constructed denominator series, bounding the differential drift actually present in the data. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

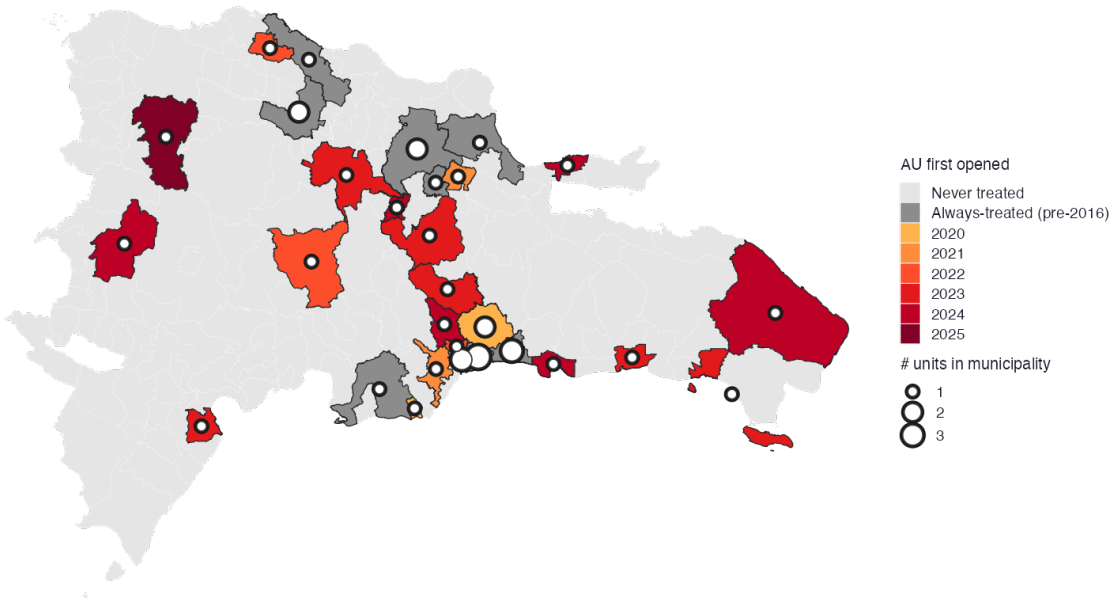
Appendix B Additional Figures

Figure B1: Births per 1,000 women aged 15–19, World Bank WDI (SP.ADO.TFRT, current revised vintage), 2018.



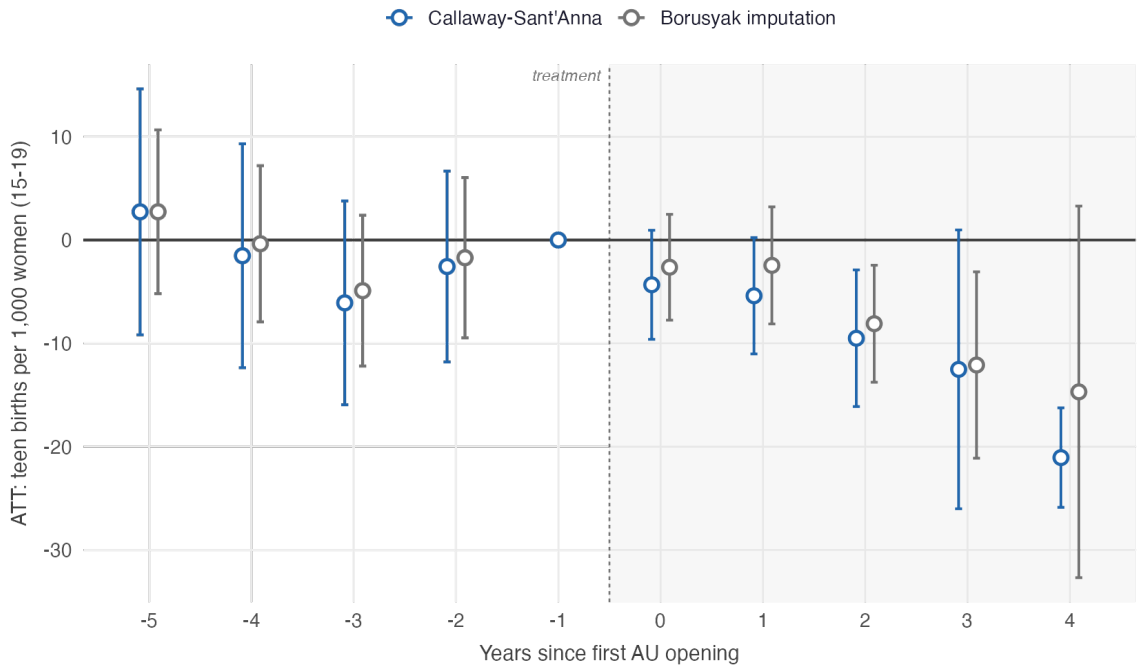
Notes: The DR ranks eighth of 25 countries; assessments naming the DR the region's highest used the earlier, pre-revision series.

Figure B2: The AUs roll-out by opening cohort.



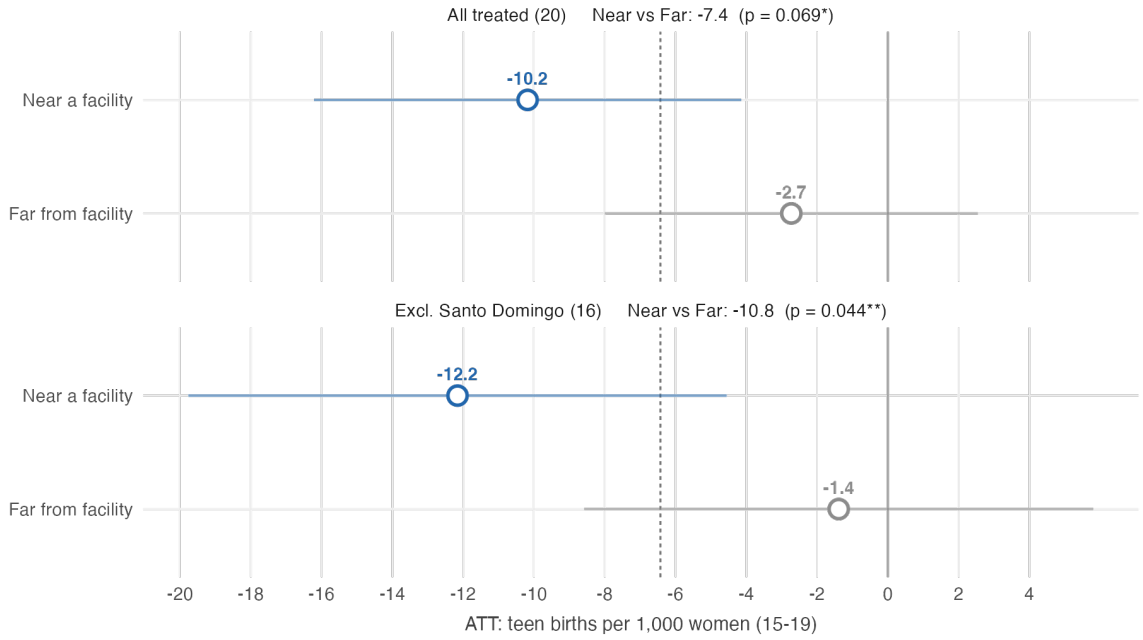
Notes: Grey denotes the nine always-served municipalities, which have no pre-period and are excluded from estimation; shaded municipalities are the 2020–2025 opening cohorts that identify the effect. Points are sized by the number of AUs in the municipality (39 AUs across 29 municipalities). Source: MoH/NHS AU inventory and field collection; ADM3 boundaries.

Figure B3: Heterogeneity-robust event studies across multiple estimators.



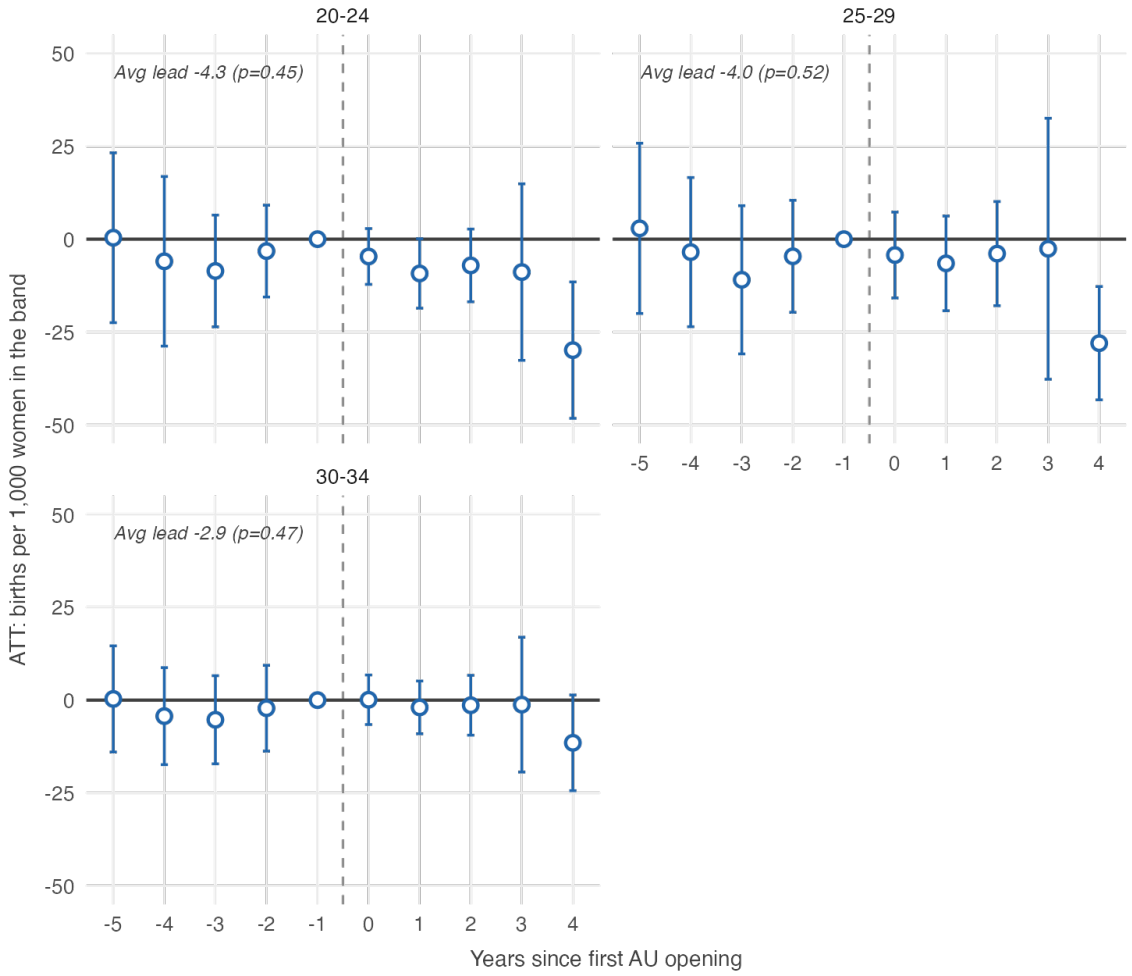
Notes: Dynamic ATTs on the 15–19 birth rate under alternative heterogeneity-robust estimators. Treatment is the first AU opening. The figure reports 95 percent pointwise confidence intervals and municipality-clustered standard errors using Series A denominators; points are offset horizontally for visibility.

Figure B4: The effects by proximity to public hospitals.



Notes: Callaway–Sant'Anna subgroup ATTs for treated municipalities split at the median distance from the population center to the nearest public health facility in the year before opening, shown for all 20 treated municipalities and excluding the Santo Domingo area; each facet header reports the near-minus-far difference and its p-value. AUs operate inside second-level hospitals, so proximity measures access to the existing system. Estimates use not-yet-treated controls and municipality-clustered standard errors; the dashed line marks the overall effect.

Figure B5: Effects on birth rates at ages 20–24, 25–29, and 30–34



Notes: Callaway–Sant'Anna dynamic ATT on each band's own birth rate; uniform 95 percent bands; municipality-clustered standard errors; Series A denominators; each panel reports the average of its dynamic leads $e \in [-5, -2]$ with the influence-function p-value.

Figure B6: AUs as physical spaces.



(a)



(b)



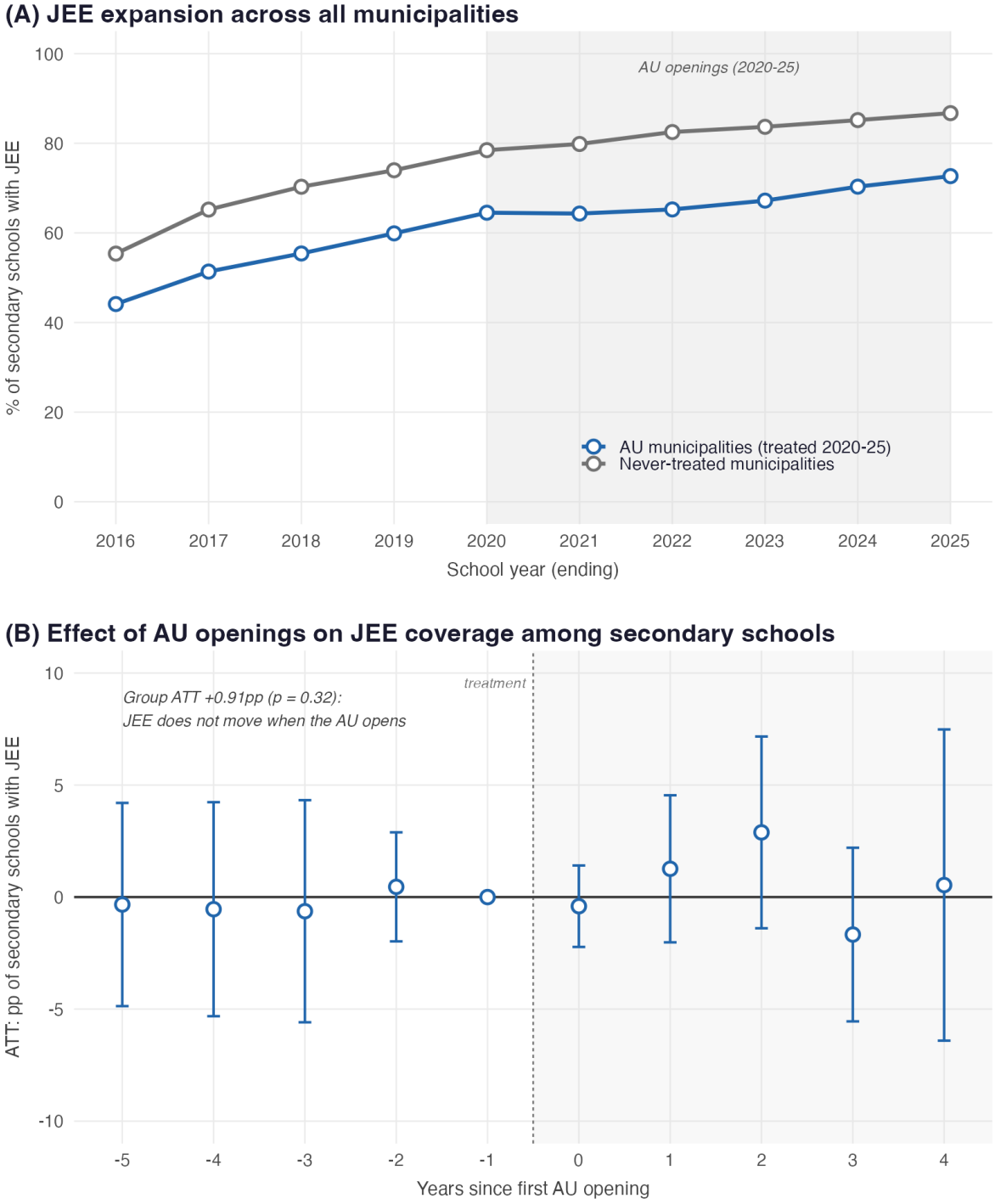
(c)



(d)

Notes: (a) Entrance of an AU at Hospital Regional Docente Juan Pablo Pina, San Cristobal. The posted notice states that the service requires no appointment (*no requieren citas*), with tickets issued by order of arrival at 7:15 a.m. and 12:00 p.m. (b) Interior corridor and consulting room, showing an AU as a delimited space inside an existing hospital. (c) Inauguration of an AU at Hospital de Engombe, Santo Domingo Oeste. (d) Official Servicio Nacional de Salud post listing the service package as general and specialized pediatrics, family medicine, family therapy, clinical psychology, and gynecology. Panels (a) to (c) are field-visit photographs; (d) reproduces NHS public communication material.

Figure B7: The extended school day (JEE) and AU openings.



Notes: Panel (A): share of secondary-offering public schools operating extended-day, treated versus never-treated municipalities, school years 2015–16 to 2024–25. Panel (B): effect of AU openings on JEE coverage among secondary schools, a Callaway–Sant'Anna event study of the JEE share re-centered on each municipality's opening (not-yet-treated controls; formal test +0.91pp, $p = .32$). MoE per-center registries (OAI-0790-2026), public and semiofficial sectors; always-treated municipalities excluded.

Appendix C Data Construction and Measurement

This appendix documents the constructed inputs used to build the estimation panel: population denominators (Section Appendix C.1), AU opening dates (Section Appendix C.3), and birth-registration completeness (Section Appendix C.2).

Appendix C.1 Population projection method

No official source publishes municipality populations by age and sex after 2020, so I construct the post-2020 denominators using the Hamilton–Perry cohort-change-ratio method (Hamilton and Perry, 1962; Smith et al., 2013). For each municipality and five-year age-sex group, the cohort-change ratio is defined as the size of a cohort at the end of an observed interval relative to the size of the corresponding cohort five years earlier, using NSO's municipal estimates for 2015–2020. An open-interval ratio is used for the 80-and-over group. To reduce instability in small cells, the ratios are winsorized to the interval $[0.3, 3]$.

I then project each cohort forward five years using these ratios, linearly interpolate intermediate years, and rake the resulting municipality-by-age-by-sex cells to NSO's official province-level projections by iterative proportional fitting. This final step ensures that the municipal series aggregates exactly to the published province totals in every year.

The main denominator series (Series A) relies only on official NSO inputs: municipal estimates for 2016–2020 and province projections for 2021–2025. It therefore does not require interpolation using the 2022 census and is fully reproducible from public data. I construct two alternatives for robustness. Series B is an intercensal series based on interpolation between the 2010 and 2022 censuses, and a third series launches directly from the 2022 census. As shown in Appendix Table A19, the causal estimates are not sensitive to the denominator construction.

A validation exercise assesses the accuracy of Series A by holding out the 2022 census and predicting it using only the 2015–2020 inputs. The resulting mean absolute percentage error is 8.1 percent across national age-sex cells and 14.7 percent across municipality cells. Relative to the 2022 census, Series A somewhat overstates the adolescent population, with the gap rising from 2.5 to 9.6 percent over the projection window. This implies that the series slightly understates the level of the adolescent birth rate. A smooth scaling error common to treated and comparison municipalities is differenced out, but differential error could bias the estimates. Appendix Table A19 therefore compares independently constructed denominators and quantifies the differential error required to overturn the result.

Table A20 illustrates the constructed series for women aged 15–19 in Santo Domingo province and its municipalities under both Series A and Series B. The complete projections, for every municipality by year, sex, and five-year age group, are provided in the supplementary volume *Population Projections for the Municipalities of the Dominican Republic, 2016–2025* and deposited in machine-readable form with the replication package.

Table A20: Projected population of women aged 15–19: Santo Domingo province, both denominator series

	2016	2017	2018	2019	2020	2021	2022	2023	2024	2025
Panel A. Series A (NSO estimates 2016–2020; Hamilton–Perry extension 2021–2025)										
Santo Domingo (province)	130,124	131,833	133,531	135,227	136,918	137,657	138,393	139,132	139,867	140,602
Boca Chica	8,214	8,329	8,444	8,559	8,673	8,728	8,783	8,838	8,892	8,947
Los Alcarrizos	15,332	15,552	15,770	15,987	16,204	16,310	16,416	16,522	16,627	16,733
Pedro Brand	4,173	4,235	4,295	4,356	4,416	4,446	4,477	4,507	4,538	4,568
San Antonio De Guerra	2,345	2,375	2,406	2,437	2,467	2,480	2,493	2,507	2,520	2,533
Santo Domingo Este	51,034	51,659	52,281	52,901	53,521	53,763	54,005	54,247	54,488	54,729
Santo Domingo Norte	29,817	30,219	30,619	31,018	31,416	31,596	31,776	31,956	32,136	32,315
Santo Domingo Oeste	19,209	19,464	19,716	19,969	20,221	20,333	20,444	20,555	20,666	20,777
Panel B. Series B (intercensal interpolation, 2010 and 2022 censuses)										
Santo Domingo (province)	121,138	119,979	118,819	117,660	116,501	115,341	114,182	114,792	115,398	116,005
Boca Chica	7,610	7,537	7,463	7,389	7,316	7,242	7,168	7,181	7,195	7,209
Los Alcarrizos	14,770	14,727	14,684	14,640	14,597	14,553	14,510	14,633	14,755	14,876
Pedro Brand	4,020	4,010	4,000	3,989	3,979	3,968	3,958	3,929	3,902	3,875
San Antonio De Guerra	2,366	2,375	2,385	2,394	2,404	2,413	2,423	2,442	2,460	2,478
Santo Domingo Este	45,706	44,924	44,142	43,360	42,579	41,797	41,015	41,359	41,698	42,035
Santo Domingo Norte	28,742	28,647	28,553	28,459	28,365	28,271	28,177	28,213	28,250	28,290
Santo Domingo Oeste	17,924	17,758	17,593	17,427	17,262	17,096	16,931	17,035	17,138	17,241

Notes: Illustrative extract of the municipality-level population projections used as denominators; the province row sums its municipalities (2010 municipal division: the three post-2010 municipalities are folded into their parent municipalities). The complete projections, every municipality by five-year age group, sex, year, and series, appear in the supplementary volume *Population Projections for the Municipalities of the Dominican Republic, 2016–2025* and as machine-readable files in the replication package.

Appendix C.2 Registration completeness

Birth registration was incomplete early in the study period but improved rapidly. The registered national adolescent birth rate rises from 37.5 per 1,000 in 2016 to a peak of 61.8 in 2019 and then falls to 36.3 by 2025. The increase over 2016–2018 reflects a registration ramp rather than a true increase in fertility. The relevant decline in the registration-complete period is therefore the 41 percent fall between 2019 and 2025. The World Bank's birth-registration completeness indicator (`SP.REG.BRTH.ZS`) reports Dominican completeness at 92.2 percent in 2019, which matches the completeness calculated internally by comparing registered birth counts across data vintages.

This ramp predates all first openings in the main design, whose treatment cohorts begin in 2020. Consistent with that timing, re-estimating the analysis on 2019–2025 reproduces the level, covariate-adjusted, and triple-difference estimates reported in the paper (Section 7), weighing against the early registration change as an explanation for the results.

Two compositional shifts accompany treatment and are disclosed in the robustness section as limitations rather than interpreted as mechanisms: the public-facility share of births declines, and the Haitian-mother share declines. As shown there, both patterns also appear among births to women aged 25–44, so they reflect municipality-wide changes rather than a teen-specific registration artifact; neither is driven by a single municipality; and conditioning on their baseline values strengthens rather than weakens the main estimate. Registered births nevertheless provide incom-

plete levels. If registration completeness is unaffected by AU openings, the implied number of averted registered births is a conservative estimate of total averted births.

Appendix C.3 AUs opening dates

The treatment record was assembled from NHS documentation, national and local media reports, and field visits. Each AU's opening year is assigned a source tier reflecting the strength of the underlying documentation. Most in-window openings are documented by an NHS press release or another dated web source. A second set appears in an internal NHS AU spreadsheet but lacks independent web confirmation. Three AUs (Las Terrenas, Higüey, and Nizao) rely in part on fieldwork and facility-identity checks. Three municipalities (La Romana, San Pedro de Macorís, and Yamasá) are undated in all available sources and are therefore imputed to 2023, the midpoint of the national roll-out. Seventeen of the twenty first-opening cohort dates in the main design are thus directly documented or field-verified. Section 7 shows that dropping the imputed municipalities, or varying the small number of contestable dates, leaves the estimates within a narrow range.

Two dates warrant additional discussion. La Vega is coded to 2023, the year of its NHS-documented reinauguration. An NHS article confirms that the AU existed by mid-2022, but no original opening date appears in the available sources, so a 2022 coding would itself be a constructed bound rather than a documented fact. Nizao is coded to 2020, its recorded opening year. The hospital director reported that the AU began receiving patients in January 2021, so the 2020 coding is conservative: it assigns some untreated months to the treated period and therefore attenuates the estimated effect in the opening year.

One duplicate record was also removed. The NHS AU list includes the Boca Chica hospital twice, once under a generic name and once under its full name, Hospital Dr. Francisco Vicente Castro Sandoval. The NHS establishments registry confirms that these entries refer to a single second-level hospital in the municipality. After deduplication, the treatment file contains 39 distinct AUs drawn from 40 documented records.

Table A21: Treatment-timing provenance: the 39 adolescent units

Province	Municipality	Unit (hospital)	Opening	Modernization	Source
Panel A. First openings during the roll-out (20 municipalities, 21 units): the identifying variation					
Peravia	Nizao	Bajos de Nizao	2020-01	2021-01 (new)	Field
Santo Domingo	Santo Domingo Norte	Dr. Hugo Mendoza	2020-10	2020-09 (new)	Web/press
Santo Domingo	Santo Domingo Norte	Materno Infantil Villa Mella	2020†	2022 (new)	Interim
San Cristóbal	San Cristóbal	Juan Pablo Pina	2021-03	2020 (new)	NHS document
Duarte	San Francisco de Macorís	de Castillo	2021-12	None	NHS list
La Vega	Constanza	Dr. Pedro Antonio Céspedes	2022-07	2022-07 (new)	Web/press
Puerto Plata	Imbert	de Imbert	2022-06	2021 (new)	NHS document
Santo Domingo	Los Alcarrizos	Vinicio Calventi	2022-06	2023 (new)	NHS document
Barahona	Barahona	Universitario Jaime Mota	2023-01	2023-01 (new)	Web/press
Sánchez Ramírez	Cotuí	Inmaculada concepción	2023-09	2023-09 (new)	Web/press
La Romana	La Romana	Francisco Gonzalvo	2023†	2019 (new)	Web/press
La Vega	La Vega	Universitario Dr. Luis Manuel Morillo King	2023-09	2023-09 (upgrade)	NHS document
San Pedro de Macorís	San Pedro De Macorís	Antonio Musa	2023†	2025 (new)	Web/press
Monte Plata	Yamasá	de Yamasa	2023†	2020 (new)	Web/press
Santo Domingo	Boca Chica	Boca Chica Dr. Francisco Vicente Castro Sandoval	2024-04	2024-04 (new)	Web/press
Sánchez Ramírez	Fantino	Dr. Sigifredo Alba Dominguez	2024-10	2024-10 (new)	Web/press
La Altagracia	Higuey	Universitario Maternidad Nuestra Señora de la Altagracia	2024-07	2024-07 (new)	Field
San Juan	Las Matas de Farfán	Dr. Federico Armando Aybar Las Matas de Farfan	2024-04	None	NHS list
Samaná	Las Terrenas	Licenciado Pablo Antonio Paulino	2024-12	2024-12 (new)	Field
Santo Domingo	Pedro Brand	Rodolfo de la Cruz	2024-07	None	NHS list
Santiago Rodríguez	Sabaneta	Santiago Rodriguez	2025-04	None	NHS list
Panel B. Municipalities served before 2016 (9 municipalities, 18 units): excluded from the first-opening design					
Santo Domingo	Santo Domingo Este	Villa Duarte	1993-07	2022 (new)	Interim
Santo Domingo	Santo Domingo Este	San Lorenzo de Los Mina	1997-01	None	Field
Peravia	Baní	Nuestra Señora de Regla	1998-01	2021 (upgrade)	Field
Santo Domingo	Santo Domingo Oeste	de Las Caobas	1998-07	2022 (new)	Interim
Santo Domingo	Distrito Nacional	Dra. Evangelina Rodriguez	2001-01	2024-03 (upgrade?)	Web/press
Puerto Plata	Puerto Plata	Dr. Ricardo Limardo	2002-06	2022 (upgrade)	Field
Santo Domingo	Santo Domingo Este	El Almirante	2005-12	2017 (upgrade)	Field
Duarte	Pimentel	Dr. Felipe J. Achercar	2006-04	2025 (upgrade)	Field
Santiago	Santiago	Regional Universitario Dr. Arturo Grullón	2006-06	2023 (upgrade)	Field
Santo Domingo	Distrito Nacional	Maternidad Nuestra Señora de La Altagracia	2009	2024-07 (upgrade)	Web/press
María Trinidad Sánchez	Nagua	Dr. Antonio Yapor Heded	2009	2024 (upgrade)	Field
Duarte	San Francisco de Macorís	San Vicente de Paúl	2013-05	2019 (upgrade)	Field
Santiago	Santiago	Presidente Estrella Ureña	2017-02	None	NHS list
Santo Domingo	Distrito Nacional	Dr. Robert Reid Cabral	2020-11	2020-11 (new)	Web/press
Duarte	San Francisco de Macorís	de CENOVI	2021-12	2021-12 (new)	Web/press
Santo Domingo	Distrito Nacional	Ciudad Sanitaria, Luis Eduardo Aybar	2023-06	2023-06 (new)	Web/press
Santo Domingo	Distrito Nacional	Marcelino Vélez Santana	2024-07	2024-07 (new)	Web/press
Santo Domingo	Santo Domingo Oeste	de Engombe	2025-11	2025-11 (new)	Web/press

Notes: One row per unit; the treatment date of a municipality with several units is its earliest. Panel A municipalities enter the first-opening design at their opening year; Panel B municipalities are excluded from that design and enter the modernization design at their modernization year, alongside Panel A. *Source* is the strongest evidence behind the unit's dates: NHS document > dated web or press record > author fieldwork > NHS internal list (recorded date without independent confirmation) > interim placeholder. †Opening year imputed to the roll-out midpoint (three municipalities; dropping them: Appendix Table A9) or, for Villa Mella, to its municipality's documented first opening. *Modernization* is the documented arrival of the comprehensive-service model (new full-service unit or upgrade of an existing one); *None* indicates no documented modernization event. La Vega and Nizao coding choices are discussed in Appendix C.3.