XIAOQIAN ZHANG

PhD Student in Economics
Georgia State University

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November 9, 2026

Selection Committee
NBER PhD Student Workshop on Microeconomic Policies and Economic Growth

Dear Selection Committee:

I am writing to apply for the NBER PhD Student Workshop on Microeconomic Policies and Economic Growth. I am a PhD student in Economics at Georgia State University, specializing in health economics, and I am currently at the dissertation-writing stage of my program. I expect to complete my degree in August 2028.

The paper I am submitting, The Impact of Title X Policy Changes on Teen Birth Rates, examines how changes in the federal family-planning safety net affect adolescent fertility. I construct a county-month dataset that links restricted-use National Vital Statistics System natality records with Office of Population Affairs Title X clinic locations, SEER population estimates, and local economic conditions. The analysis uses policy-induced changes in geographic access to Title X providers and applies continuous-treatment difference-in-differences, event-study, and regression-discontinuity designs.

More broadly, my current research examines how public policies shape healthcare access, health behavior, and maternal and child health. My dissertation projects also study postpartum Medicaid coverage extensions, Medicaid expansion and late-postpartum maternal health, and the relationship between local economic conditions, fertility, and birth outcomes.

My future research will continue to investigate how microeconomic policies affect healthcare markets, family decision-making, and disparities in health. I am particularly interested in how insurance eligibility, the availability of safety-net providers, and reproductive-health policy influence access to care, fertility, and maternal and infant outcomes, including differences across geographic and socioeconomic groups.

My faculty advisers, Professor James Marton and Professor Lauren Velasco, can serve as references for my application. I would welcome the opportunity to participate in the workshop, receive feedback on my research, and engage with other scholars studying microeconomic policy and economic growth.

Sincerely,

Xiaoqian Zhang

Xiaoqian Zhang (Shirley)

4703095792 | xzhang99@gsu.edu | Atlanta
Current Phd Student | Georgia State University

EDUCATION

Georgia State University Aug 2023 - Present
Ph.D. in Economics Atlanta, GA
University of Southern California Aug 2020 - May 2022
M.S. in Applied Economics and Econometrics Los Angeles, CA
Tianjin University of Finance and Economics Sep 2015 - Jul 2019
B.S. in Accounting Tianjin,China

SPECIALTY AREAS

Health Economics and Environmental Economics

RESEARCH EXPERIENCE

Research Assisitance Sep 2024 - May 2025

- Analyzed the impact of NYC's traffic congestion tax by evaluating changes in average travel times across zones for four taxi types (e.g., yellow cabs, ride-share vehicles).
- Conducted difference-in-differences and spatial regression analysis in Stata to isolate the policy's effect, controlling for confounding factors (e.g., weather, events).
- Automated data pipelines to aggregate results by time-of-day and zone, generating visualizations (maps, trend plots).

Research Assistance Sep 2023 - Aug 2024

- Investigated the relationship between solar energy adoption and new construction trends using geospatial and permitting data across California.
- Developed and implemented R scripts to process and analyze large-scale construction permit records, identifying and classifying solar installation permits and new construction permits at the city level.
- Cleaned, merged, and validated datasets using Stata to ensure consistency and accuracy for regression analysis

Research Assistance Oct 2021 - May 2023

- Used ArcGIS and VIIRS nightlight data to construct monthly flood maps for Wuhan (China) and Chennai (India).
- Analyzed changes in nightlight intensity before, during, and after floods to measure the extent of damage and recovery over time.
- Demonstrated that nightlight-based maps provide a useful proxy for tracking urban resilience and disaster impact.

MISCELLANEOUS

- **Skills:** Excel,Stata,R,ArcGIS,LaTeX

WORK EXPERIENCE

Georgia State University Aug 2023 - Present
Research Assistent Atlanta, GA

- Collaborated with faculty on multiple research projects, including data acquisition, cleaning, and documentation.
- Wrote and executed Stata and R code to analyze large datasets, producing statistical tables, regression outputs, and publication-quality figures.
- Drafted clear, concise text to summarize empirical findings and support integration of results into papers and reports.

IQHi Inc. Sep 2022 - Jan 2023
Financial Analyst Irvine, CA
This firm is in the new energy industry. It aims at improving the efficiency of the battery, thus expanding the use of batteries in new energy vehicles. My job's obligations are as follows:

- Conduct financial analysis on IQHi's internal and external operations and advise management about strategies to reduce costs and expenses and improve the company's performance and profitability.
- Prepare and present financial reports and develop corporate financial strategies.
- Develop financial projection models specifically for our business model to improve the accuracy of financial forecasting.
- Study economic and industry trends and new regulations and policies and devise business decisions that help IQHi stay ahead of its competitors.

HONORS & AWARDS

The E.D. "Jack" Dunn Fellowship March 2025
The E.D. "Jack" Dunn Fellowship March 2024

The impact of Title X rule changes on teen birth rate

Xiaoqian Zhang

November 2, 2026

Abstract

U.S. teen birth rates have declined substantially over the past three decades, yet the effects of recent reproductive healthcare policies on adolescent fertility remain uncertain. This paper examines the impact of the 2019 Title X Final Rule and its 2021 restoration on teen fertility. I construct a monthly county-level dataset integrating restricted-use National Vital Statistics System (NVSS) natality records, Title X clinic availability data from the Office of Population Affairs (OPA), SEER population estimates, and county-level unemployment rates. Using Poisson Pseudo-Maximum Likelihood (PPML) Difference-in-Differences models, I exploit county-level variation in changes in geographic access to Title X clinics, complemented by event-study and Regression Discontinuity analyses. The results indicate that counties experiencing reduced Title X access following the 2019 Final Rule had approximately 1.8% lower expected teen conception rates relative to comparison counties. However, significant differential pre-policy trends limit the causal interpretation of this finding. Following the 2021 restoration, improved geographic access is associated with a small, statistically insignificant increase in teen conceptions, with no consistent evidence of persistent dynamic effects. Planned Parenthood exposure specifications yield similar results, while Regression Discontinuity estimates are sensitive to bandwidth selection. Overall, this study contributes new evidence on recent Title X policy reforms and adolescent fertility, highlighting the distinction between changes in geographic healthcare access and measurable fertility outcomes, as well as the challenges of identifying causal effects of nationwide reproductive healthcare policies.

1 Introduction

Teen birth remains an important public health concern in the United States despite substantial declines over the past several decades. Compared with other high-income countries, the United States continues to experience one of the highest teen birth rates, with persistent disparities across racial, ethnic, and geographic groups (Kearney and Levine, 2012). Although national teen birth rates have fallen considerably, unintended adolescent pregnancy continues to impose significant health, educational, and economic costs for both young mothers and their children. Understanding which public policies have contributed to these declines, and whether recent policy changes have altered this trajectory, remains an important question for both researchers and policymakers.

Figure 1 illustrates the long-run decline in the U.S. teen birth rate between 2007 and 2023 using CDC WONDER natality data. During this period, the birth rate among females aged 15–19 fell from nearly 40 births per 1,000 females to just above 20. This sustained decline likely reflects the combined influence of multiple factors, including improvements in contraceptive use, expanded access to reproductive healthcare, changes in adolescent sexual behavior, and broader public health initiatives. Although the overall downward trend is clear, it remains uncertain how individual policy interventions have contributed to this decline. In particular, the extent to which changes in federal family planning policy have influenced teen birth rates has received relatively limited empirical attention. This study addresses that question by examining the effects of the 2019 and 2021 Title X policy changes.

There is a long-standing policy effort in the United States aimed at reducing unintended pregnancies, particularly among teens. Kearney and Levine (2009) study state Medicaid expansions in the 1990s and 2000s that extended subsidized family planning services to low-income individuals not eligible for full Medicaid. They find that these changes reduced the teen birth rate by 4%, driven by increased contraceptive use. Bailey (2012) demonstrates that implementing federal family planning programs during the 1960s and 1970s led to a 2.3% reduction in teen birth rates within six to ten years of their introduction.

Among the policies designed to improve reproductive health access, the Title X Family Plan-

ning Program occupies a unique role because it is the only federal program dedicated exclusively to providing comprehensive family planning and preventive reproductive health services. Established in 1970, Title X supports clinics that offer confidential, low- or no-cost contraceptive counseling, contraceptive methods, pregnancy testing, sexually transmitted infection screening and treatment, cancer screening, and related preventive services, regardless of a patient's income or age. Although federal law prohibits the use of Title X funds to directly pay for abortion services, participating providers have historically been permitted to offer nondirective pregnancy counseling and abortion referrals consistent with professional medical standards. These services are particularly important for adolescents, who often face financial, transportation, confidentiality, and informational barriers when seeking reproductive healthcare. Consequently, changes in the availability of Title X providers have the potential to influence teen birth rates by altering access to effective contraception as well as pregnancy counseling and referral services.

The Title X provider network experienced two major federal policy changes during the study period. In July 2019, the Trump administration implemented the Title X Final Rule, commonly referred to as the "domestic gag rule." The regulation prohibited Title X-funded providers from referring patients for abortion and imposed new physical and financial separation requirements between Title X-funded services and abortion-related activities. As a result, many organizations withdrew from the Title X program, most notably Planned Parenthood, which had historically been one of the largest Title X providers nationwide. Planned Parenthood served a disproportionately young and low-income patient population and provided a substantial share of publicly funded contraceptive services. Its withdrawal therefore represented a major reduction in reproductive healthcare access in many communities. In November 2021, the Biden administration reversed these regulations, allowing providers, including Planned Parenthood affiliates, to reenter the Title X network and restoring access to comprehensive counseling and referral services. These two policy changes created substantial variation in the geographic availability of Title X-funded care across the United States, providing a natural setting for evaluating how changes in family planning access affect teen birth rates.

Figure 2 illustrates the geographic distribution of counties according to how their access to Title X services changed following the 2019 and 2021 policy reforms. Counties shown in yellow maintained at least one Title X clinic throughout both policy periods, whereas counties shown in white never had a Title X clinic during the study period. The remaining counties experienced policy-induced changes in provider availability. Counties highlighted in blue lost their Title X clinic after implementation of the 2019 Final Rule but regained access following the 2021 restoration, while counties shown in orange lost access after the 2019 policy change and had not regained a Title X clinic by the end of the study period. Together, these counties represent the areas directly affected by the Title X policy changes and provide the primary source of identifying variation in the empirical analysis. The geographic distribution also shows that policy exposure was widespread, affecting counties across multiple regions of the United States rather than being concentrated in a single state or region. This substantial cross-county variation in changes in clinic availability forms the basis for estimating the effects of the Title X policy changes on teen birth rates.

In this paper, I aim to estimate the impact of Title X policy changes in July 2019 and November 2021 on teen birth rates. Compared with the previous reseach, this paper adds to the reproductive health policy literature by providing new evidence on how recent federal changes to the Title X program affected teen birth rates. While prior work has focused on earlier expansions (e.g., Bailey, 2012; Kearney and Levine, 2012), relatively little is known about how the 2019 restrictions and the 2021 reversal affected teen fertility.

To answer this question, I construct a monthly county-level panel covering September 2017 through December 2024 by integrating several administrative and public data sources. Teen birth outcomes are measured using restricted-use National Vital Statistics System (NVSS) natality records, which provide complete county-level birth data and allow births to be assigned to the month of conception using information on the mother's last normal menstrual period. Birth counts are combined with county-level population estimates from the Surveillance, Epidemiology, and End Results (SEER) Program to calculate monthly teen birth rates. I further merge these data with a newly constructed Title X clinic availability database based on the Office of Population Affairs

(OPA) Title X Clinic Locator and Directory, county-level unemployment rates from the Bureau of Labor Statistics, and geographic measures of access that capture changes in the distance from each county centroid to the nearest Title X clinic following provider exits and reentry.

My primary empirical strategy is a Difference-in-Differences (DID) design that exploits policy-induced changes in geographic access to Title X services across counties. Because county-month observations frequently contain zero teen births, the DID models are estimated using a Poisson specification with the population of females aged 15–19 included as an exposure term. The analysis separately estimates the effects of the 2019 Final Rule and the 2021 Title X restoration. To examine the robustness of the findings, I additionally implement a time-based Regression Discontinuity (RD) design that estimates whether teen birth rates changed discontinuously at each policy implementation date using local linear regressions with data-driven optimal bandwidth selection. Finally, I extend the DID framework into an event-study specification to examine the dynamic evolution of policy effects and to assess whether pre-policy trends are consistent with the identifying assumptions.

The empirical results reveal mixed evidence on the relationship between Title X policy changes and adolescent fertility. The DID estimates indicate that counties experiencing reduced geographic access to Title X clinics following the 2019 Final Rule had approximately 1.8% lower teen conception rates relative to comparison counties, contrary to the expected direction of the policy effect. However, the event-study analysis identifies significant differential pre-policy trends, limiting the causal interpretation of this estimate. Following the 2021 restoration, improved geographic access is associated with a small and statistically insignificant increase of approximately 1.1% in teen conceptions. Additional analyses focusing on Planned Parenthood exposure yield similar findings, while the RD estimates are sensitive to bandwidth selection. Overall, the results do not establish a consistent causal relationship between changes in the Title X provider network and adolescent fertility, highlighting the challenges of translating changes in geographic healthcare access into measurable fertility outcomes.

The remainder of the paper is organized as follows. Section 2 reviews the literature on fam-

ily planning policies, Title X, and adolescent fertility. Section 3 describes the construction of the monthly county-level dataset and the measurement of teen fertility and geographic access to Title X clinics. Section 4 presents the empirical methodology, including the DID framework, RD design, and event-study specifications. Section 5 reports the empirical findings and discusses their implications for reproductive healthcare policy evaluation. Section 6 outlines planned extensions, including heterogeneity analyses and additional robustness checks.

2 Literature Review

There are some previous literatures that evaluate trends in teen birth rates over the past decades, emphasizing how policy interventions have influenced those trends.Kearney and Levine (2012) document the long-run decline in U.S. teen birth rates and investigates the extent to which this trend can be attributed to changes in sexual activity, contraceptive use, and policy interventions. They conclude that 65% of the decline in teen childbearing between 1991 and 2008 is due to increased contraceptive use, and 35% is due to reduced sexual activity. However, the effect of policy changes is limited. All targeted policies combined (e.g., welfare reforms, contraception access, sex ed mandates) explain only about 12% of the overall decline in teen births from 1991 to 2008. Kearney and Levine (2015) further explore the attributes that affect teen birth rate. They highlight the role of media exposure in shaping teen fertility decisions. This paper provides compelling evidence that the MTV show 16 and Pregnant led to around one-third of the overall decline in the teen birth rate between 2009 and 2010. Besides, other papers also focus on the factors lead to the decline in teen birth rate. Boonstra (2014) summarizes a broad set of contributing factors behind the national decline in teen pregnancy and birth rates, highlighting that 86% of the decline from 1995 to 2002 can be attributed to improved contraceptive use, while only 14% was due to reduced sexual activity. Above literatures all analyze the decline teen birth rate and the attributes of it.

The Title X family planning program has long served as a critical source of confidential reproductive health care, and numerous studies have examined its impact on access to contraception

and reproductive outcomes. One influential study in this area is Bailey (2012), which reexamines the early rollout of federally funded family planning programs, including the early years of Title X. Using county-level variation in program funding from the 1960s and 1970s, she finds that access to subsidized family planning services led to significant reductions in fertility, especially among lower-income women. While early research such as Bailey (2012) documents fertility reductions following Title X expansions, more recent work examines the implications of regulatory retrenchment. Smith et al. (2022) show that the Trump-era rule changes to Title X ed to the expansion of "contraception deserts," or geographic areas with limited access to publicly funded reproductive health care. Using spatial access models across 10 states, they estimate that millions of low-income individuals and people of color lost potential access to Title X services. Together, these studies highlight how both the expansion and contraction of Title X services can significantly shape access to reproductive care and influence fertility outcomes, particularly for disadvantaged populations.

Access to abortion services has also been shown to play a critical role in shaping birth rates, particularly among teenagers and other populations at heightened risk of unintended pregnancies. Myers (2017) reexamines the impact of young women's access to abortion and contraception by leveraging state-level policy variation from the 1960s and 1970s. She finds that legal access to abortion reduced birth rates among teens by 34 births per 1,000 women aged 15–19, and among women aged 20–24 by 15–20 births per 1,000, with the largest effects concentrated among young, low-income, and minority women. Drawing on policy shifts in Eastern Europe in the late 1980s and 1990s, Levine and Staiger (2004) find that legalizing abortion in contexts where it had previously been severely restricted led to birth rate reductions of over 10%. Donohue III and Levitt (2001)) provide indirect but compelling evidence that abortion access influences fertility patterns by linking the legalization of abortion in the 1970s to significant long-run declines in crime during the 1990s. They argue that this relationship is driven in part by reductions in unwanted births following Roe v. Wade, particularly among young, poor, and high-risk women.

A growing body of research examining the effects of reproductive health policies on fertility

outcomes has employed quasi-experimental econometric methods to establish causal relationships. Many studies exploit policy variation across time and geography, using difference-in-differences (DiD) frameworks to compare changes in outcomes between treated and control groups (e.g., Bailey (2012); (Levine and Staiger, 2004)). Others apply event study designs or instrumental variables to account for endogeneity concerns, especially when examining access to services like contraception or abortion (e.g., Myers (2017)). Some studies, such as Donohue III and Levitt (2001), use long-run panel data to trace downstream effects of earlier policy shifts, while others incorporate spatial access models (e.g., Smith et al. (2022)) to measure geographic variation in service availability. These methodological approaches collectively allow researchers to isolate the impact of policy changes from confounding trends in fertility and reproductive behavior.

This paper contributes to the growing body of research on reproductive health policy by providing new evidence on the impact of recent federal-level changes to the Title X program on teen birth rates. While much of the existing literature has focused on earlier expansions of Title X or broader family planning interventions (e.g., Bailey (2012); Kearney and Levine (2012)), fewer studies have examined the consequences of recent regulatory rollbacks and reinstatements, particularly their effects on adolescents. By exploiting the sharp timing of the 2019 and 2021 Title X policy shifts and employing a regression discontinuity framework, this study offers credible identification of short-run and lagged impacts. It also expands on prior work by conducting dynamic event-study analyses to assess the presence of delayed behavioral responses, an area that has received limited attention in the context of teen fertility.

3 Data

This study constructs a monthly county-level panel dataset covering from September 2017 through December 2024 to examine how changes in Title X policy affected teen birth rates. The unit of observation is the county-month. The analytic dataset integrates multiple administrative and publicly available data sources, including restricted-use National Vital Statistics System (NVSS) natality

records, monthly Title X clinic availability database derived from the Office of Population Affairs (OPA) Title X Clinic Locator and Directory, county-level population estimates from the Surveillance, Epidemiology, and End Results (SEER) Program, monthly county unemployment rates from the Bureau of Labor Statistics (BLS), and geospatial measures of county centroid. Together, these data provide comprehensive information on reproductive health service availability, demographic characteristics, local economic conditions, and birth outcomes across U.S. counties over time.

3.1 Birth outcomes (NVSS Restricted Natality Data)

Birth outcomes are constructed from the restricted-use National Vital Statistics System (NVSS) Natality Files, which contain individual birth certificate records for all live births occurring in the United States. Unlike publicly available NVSS natality files, restricted data provide complete county identifiers for maternal residence and retain all counties, including those suppressed or aggregated in public-use files to protect confidentiality. The restricted files also contain detailed information on maternal demographic characteristics, pregnancy history, birth outcomes, and the timing of pregnancy, allowing births to be aggregated to the monthly county level. These features make restricted NVSS data particularly suitable for examining local changes in Title X clinic availability in reproductive health policies and birth outcomes over time.

A key feature of this study is that monthly teen birth rates are measured based on the month of conception rather than the month of delivery. Births are assigned to the conception month using the reported month and year of the mother's last normal menstrual period (LMP) recorded on the birth certificate. Aligning births with the timing of conception provides a more appropriate measure of exposure to changes in Title X policy because contraceptive access influences pregnancy initiation rather than the timing of delivery. Approximately 94 percent of birth records contain valid LMP information. For observations with missing LMP values, the conception month is imputed using the reported birth month together with the combined gestational age measured in completed weeks. This imputation procedure reduces the proportion of missing conception dates from approximately 6 percent to only 0.1 percent, substantially improving data completeness while preserving the tim-

ing of pregnancy for the analytic sample. County-level birth rates are assigned according to the mother's county of residence rather than the county in which the delivery occurred. Maternal county of residence more accurately reflects the local reproductive health care environment and the population exposed to Title X services, whereas the county of delivery may differ because pregnant individuals frequently travel across county boundaries to give birth. Births are therefore aggregated to monthly county-level counts based on maternal residence before merging with county population estimates and other county-level measures.

The analytic sample is restricted to live births to mothers aged 15–19 years, consistent with the study's focus on adolescent fertility. Monthly births are first aggregated to the county level based on the estimated month of conception and the mother's county of residence. To construct monthly teen birth rates, these birth counts are merged with county-level population estimates from the Surveillance, Epidemiology, and End Results (SEER) Program. SEER provides annual population estimates by age, sex, and geographic unit based on U.S. Census Bureau population estimates. I use the annual female population aged 15–19 years in each county as the denominator to calculate monthly teen birth rates per 1,000 females aged 15–19. Because county population estimates are available annually rather than monthly, the annual population is assumed to remain constant within each calendar year. The analysis is limited to September 2017 through December 2024, corresponding to the period for which complete monthly Title X provider data are available and ensuring a balanced county-month panel across all data sources.

3.2 Title X Clinic Availability Database

Information on Title X providers is obtained from the Office of Population Affairs (OPA) Title X Clinic Locator and Directory, the official public directory of Title X-funded providers in the United States. The directory classifies organizations into three administrative levels: grantees, sub-recipients, and service sites. Grantees receive Title X funding directly from OPA, sub-recipients administer funding through grantees, and service sites are the physical clinics where patients receive family planning and related reproductive health services. Because the objective of this study

is to measure local access to Title X services, the analysis uses service sites as the unit of observation. For each clinic, the directory reports the clinic name, street address, city, state, ZIP code, and organizational affiliation.

Monthly snapshots of the OPA directory were collected for every month from September 2017 through December 2024, creating a longitudinal clinic-level database that tracks changes in the Title X provider network over time. Each service site was assigned a county Federal Information Processing Standards (FIPS) code using its ZIP code and street address to ensure accurate geographic assignment. Geographic coordinates (latitude and longitude) were then added to each clinic through geocoding, allowing clinics to be linked to county centroids and enabling the construction of measures of geographic access, including the distance from each county without Title X clinics to their nearest active one. Validation checks comparing the constructed database with the published Family Planning Annual Reports indicate that the monthly number of service sites closely matches the official national counts reported by OPA, supporting the reliability of the constructed provider database.

The clinic-level database is aggregated to the county-month level to construct measures of local Title X availability. For each county and month, the database identifies whether at least one active Title X clinic is present and records the total number of active Title X service sites. To account for differences in county population size, these measures are merged with annual county population estimates from the SEER Program to calculate the number of Title X clinics per 100,000 women aged 15–44, the primary population eligible for Title X family planning services. These measures capture both the extensive margin of access (whether any Title X services are available) and the intensive margin (the density of providers relative to the population).

Planned Parenthood played a central role in the Title X policy changes examined in this study. The 2019 Title X Final Rule required physical and financial separation between Title X-funded family planning services and abortion-related services, leading Planned Parenthood to withdraw from the program in August 2019. Following the reversal of the 2019 rule, Planned Parenthood was permitted to rejoin the Title X program beginning in late 2021. Because Planned Parenthood

has historically served a large share of low-income and adolescent patients seeking confidential contraceptive and reproductive health services, its withdrawal and subsequent reentry substantially altered access to Title X care for many teenagers, particularly in counties where Planned Parenthood clinics represented a major share of the local provider network. To capture this mechanism, Planned Parenthood-affiliated Title X service sites are identified from the OPA directory and aggregated to the county-month level. Similar measures are then constructed, including an indicator for the presence of any Planned Parenthood Title X clinic, the number of Planned Parenthood Title X clinics, and the number of Planned Parenthood Title X clinics per 100,000 women aged 15–44.

Aggregating the clinic-level information produces a monthly county-level Title X availability dataset spanning September 2017 through December 2024. For each county-month, the dataset records whether the county contains any active Title X clinic, the total number of Title X clinics, and the number of Title X clinics per 100,000 women aged 15–44. Parallel measures are constructed for Planned Parenthood-affiliated Title X clinics, including indicators for the presence of any Planned Parenthood clinic, the total number of Planned Parenthood clinics, and their population-adjusted availability. These measures provide a comprehensive description of how access to Title X-funded family planning services evolved across counties before, during, and after the two major federal policy changes.

Figures 3 and 4 illustrate the evolution of the Title X provider network over the study period. Figures 3 plots the monthly number of active Title X clinics nationwide. Prior to the implementation of the 2019 Title X Final Rule, the size of the provider network remained relatively stable at approximately 3,600 clinics. Immediately following the policy change, the number of active clinics declined sharply by nearly one-quarter, reflecting the withdrawal of a substantial number of providers from the Title X program. Following the 2021 rule restoration, the network expanded as providers reentered the program, although the total number of clinics never returned to its pre-2019 level. Beginning in mid-2023, the network experienced another noticeable decline, coinciding with the period following the Supreme Court's Dobbs v. Jackson Women's Health Organization decision. Although this decline may reflect broader changes in the reproductive health care landscape

after Dobbs, the present analysis does not attempt to identify its causal effect.

Figure 4 demonstrates that these changes were driven primarily by Planned Parenthood. Before the 2019 Final Rule, approximately 380 Planned Parenthood clinics participated in the Title X program. Following Planned Parenthood's withdrawal from the network in August 2019, the number of affiliated Title X clinics fell to nearly zero and remained at that level throughout the duration of the 2019 rule. After the 2021 rule restoration, Planned Parenthood clinics gradually reentered the network, with participation recovering to approximately 300 clinics by 2023. The sharp decline and subsequent recovery closely align with the timing of the two federal policy changes, providing descriptive evidence that the 2019 and 2021 Title X rules substantially altered the availability of family planning services through changes in Planned Parenthood participation.

Figures 5 and 6 examine how the geographic coverage of the Title X provider network changed over time by plotting the monthly number of counties containing at least one Title X clinic. Figure 4 presents the monthly number of counties with at least one active Title X clinic. The pattern closely mirrors that shown in Figure 3. Prior to the 2019 Final Rule, approximately 1,850 counties contained at least one Title X clinic. Following the implementation of the rule, the geographic coverage of the Title X network contracted sharply, falling to roughly 1,450 counties. After the 2021 rule restoration, the number of counties served increased as providers reentered the network. However, despite this recovery, the network did not regain its pre-2019 geographic coverage. Similar to the trend in the total number of clinics, the number of counties with Title X providers declined again beginning in mid-2023 and remained below its earlier peak through the end of the study period.

Figure 6 focuses specifically on counties containing at least one Planned Parenthood Title X clinic. The pattern closely aligns with the trend in the total number of Planned Parenthood clinics shown in Figure 4. Before the 2019 policy change, approximately 230 to 240 counties were served by at least one Planned Parenthood Title X clinic. Following Planned Parenthood's withdrawal from the Title X program, this number fell to nearly zero and remained at that level throughout the duration of the 2019 rule. After the 2021 policy reversal, Planned Parenthood gradually reentered

the Title X network, expanding coverage to nearly 200 counties by 2023. Although the geographic reach of Planned Parenthood recovered substantially after reentry, it remained below its pre-2019 level through December 2024.

3.3 County Population Estimates (SEER)

The study uses county-level population estimates from the Surveillance, Epidemiology, and End Results (SEER) Program, which publishes annual population estimates by age, sex, race, and geographic area based on U.S. Census Bureau population estimates. The SEER population files provide consistent county-level demographic measures for all U.S. Annual estimates are available for every county throughout the study period, allowing population denominators to be measured consistently across space and time.

The SEER population estimates are incorporated into the analysis in two ways. First, the annual female population aged 15–19 is used as the denominator to calculate monthly county-level teen birth rates. Monthly birth counts derived from the restricted NVSS natality files are divided by the corresponding county-year population estimate and expressed as births per 1,000 females aged 15–19. Second, the female population aged 15–44 is used to construct population-adjusted measures of Title X service availability. Specifically, the number of Title X clinics and Planned Parenthood Title X clinics in each county-month is normalized by the population of women of reproductive age, producing measures of clinic availability per 100,000 females aged 15–44. These population-adjusted measures account for differences in county population size and provide a more comparable indicator of access to Title X family planning services across counties.

3.4 Local Economic Conditions (BLS Unemployment Rates)

To account for changes in local economic conditions, this study incorporates county-level unemployment rates from the Local Area Unemployment Statistics (LAUS) program published by the U.S. Bureau of Labor Statistics (BLS). The LAUS program provides monthly estimates of labor force participation, employment, unemployment, and unemployment rates for counties throughout

the United States.

Monthly county unemployment rates are merged with the county-month panel for the period from September 2017 through December 2024 using county FIPS codes and calendar month. The unemployment rate serves as a time-varying control variable in all empirical analyses to account for changes in local economic conditions that may independently influence fertility behavior. Controlling local labor market conditions helps isolate the effects of changes in Title X service availability from broader economic fluctuations, including those associated with the COVID-19 pandemic and subsequent economic recovery.

3.5 Geographic Measures

Because many U.S. counties do not contain a Title X service site, measuring geographic access to family planning services requires more than a simple indicator of whether a county has a clinic. Residents living in counties without a Title X clinic may still obtain services by traveling to neighboring counties, but greater travel distances increase the cost of accessing care and may reduce utilization, particularly for teenagers who often face transportation and financial constraints. To capture geographic accessibility, this study constructs a measure of the distance from each county without a Title X clinic to its nearest Title X service site. Because the Title X clinic directory is available monthly, the nearest clinic and the corresponding travel distance are recalculated for every county-month, allowing geographic access to vary over time as clinics enter or exit the Title X network following policy changes.

County geographic coordinates are obtained from the U.S. Census Bureau County Gazetteer Files, which provide the latitude and longitude of each county's geographic centroid. The OPA Title X Clinic Locator and Directory was previously geocoded using each clinic's street address to obtain precise latitude and longitude coordinates for every Title X service site. For each county-month in which a county contains no Title X clinic, the great-circle distance is calculated between the county centroid and every active Title X clinic using the haversine formula, and the minimum distance is retained as the distance to the nearest Title X clinic. Counties containing at least one

Title X clinic are assigned a distance of zero. This measure provides a continuous indicator of geographic access to Title X services and captures changes in travel burden as clinics exit or reenter the Title X network over time.

3.6 Descriptive Statistics

Table 1 presents summary statistics for the outcome variables across the three Title X policy periods. Both the average number of teen births and the teen birth rate exhibit a steady decline over the sample period. Before the implementation of the 2019 Final Rule (September 2017–July 2019), counties averaged 4.55 teen births per month and a teen birth rate of 1.84 births per 1,000 females aged 15–19. During the 2019 Final Rule period (August 2019–November 2021), these averages declined to 3.93 births and 1.64 births per 1,000, respectively. Following the 2021 restoration of the Title X program (December 2021–December 2024), the averages decreased further to 2.81 births and 1.15 births per 1,000. The downward trend is consistent with the long-run decline in U.S. teen fertility documented in Figure 1, highlighting the importance of accounting for underlying time trends in the empirical analysis. County unemployment averaged 4.40 percent over the full sample period, although substantial variation remained across counties and over time.

Table 2 summarizes changes in the availability of Title X services across the three policy periods. Before the 2019 Final Rule, approximately 58.4 percent of county-month observations contained at least one Title X clinic, with an average of 1.13 clinics per county and 20.05 clinics per 100,000 females aged 15–44. Following implementation of the 2019 Final Rule, these measures declined markedly: the share of county-months with a Title X clinic fell to 45.7 percent, the average number of clinics decreased to 0.84 per county, and the population-adjusted clinic availability declined to 17.58 clinics per 100,000 women of reproductive age. After the 2021 policy reversal, Title X availability recovered only partially. Although the average number of clinics increased modestly, all measures remained below their pre-2019 levels. Geographic access also deteriorated over time, as the average distance from counties without a Title X clinic to the nearest Title X service site increased from 26.2 kilometers before 2019 to 66.4 kilometers during the 2019 Final

Rule period before improving to 46.9 kilometers following the 2021 restoration. Planned Parenthood experienced even larger changes. The proportion of county-months containing a Planned Parenthood Title X clinic declined from 7.4 percent before the 2019 rule to less than 0.5 percent during the rule's implementation, reflecting Planned Parenthood's withdrawal from the Title X network. Although Planned Parenthood reentered the network after the 2021 policy reversal, its geographic presence and clinic counts remained substantially below pre-2019 levels. Together, these descriptive statistics indicate that the 2019 policy substantially reduced both the supply and geographic accessibility of Title X services, while the 2021 restoration only partially rebuilt the provider network.

Taken together, these data provide a comprehensive monthly county-level panel that links changes in teen birth rates with detailed measures of Title X service availability, local demographic characteristics, economic conditions, and geographic access to family planning services. The dataset captures both the extensive margin of access through indicators of whether a county contains a Title X provider and the intensive margin through clinic counts, population-adjusted provider availability, and travel distance to the nearest Title X clinic. Because the panel spans the implementation of both the 2019 Title X Final Rule and the 2021 policy restoration, it provides substantial temporal and geographic variation in access to publicly funded family planning services. The next section describes the empirical strategies used to exploit this variation and estimate the causal effects of changes in Title X availability on county-level teen birth rates.

4 Methodology

This section outlines the empirical methods used to examine the 2019 and 2021 Title X policy changes. First, I employ a Difference-in-Differences (DID) design that compares changes in teen birth rates between counties experiencing policy-related changes in distance to the nearest active Title X clinic and counties with unchanged distance. Second, I use a time-based Regression Discontinuity (RD) design to examine whether teen birth rates changed discontinuously around each

policy implementation date. Finally, I apply an event-study framework to examine the dynamic effects of the reforms and assess differential pre-policy trends between treated and comparison counties. Together, these approaches provide evidence on how changes in geographic access to Title X services are associated with teen fertility.

4.1 Main Approach: Difference-in-Differences

My primary empirical strategy estimates the effects of the 2019 and 2021 Title X policy changes using a Difference-in-Differences (DID) design. The analysis exploits cross-county variation in changes in geographic access to Title X-funded clinics following each policy reform. Geographic access is measured by the distance from each county centroid to the nearest active Title X clinic for the counties without Title X access, the distance will equal to zero if the county has any Title X clinics. Rather than using the magnitude of distance changes as a continuous treatment measure, I classify counties into treatment and control groups based on whether their distance to the nearest active Title X clinic changed in the direction associated with each reform.

For the 2019 Final Rule, I compare the distance to the nearest active Title X clinic in June 2019, before the policy change, with the corresponding distance in September 2019. Counties experiencing an increase in distance are classified as treated, reflecting reduced geographic access following the reform. All remaining counties constitute the control group, including those with unchanged distance and those experiencing a decrease in distance. For the 2021 Rule Restoration, I compare clinic distances in October 2021 and May 2022. Counties experiencing a decrease in distance are classified as treated, reflecting improved geographic access, while counties with unchanged or increased distance constitute the control group. The longer measurement interval for the 2021 reform accommodates the gradual recovery of Title X provider participation following the restoration. Thus, treatment is determined by the direction of the distance change rather than its magnitude.

The treatment-group indicators are defined as

$$T_c^{2019} = \mathbb{1}\left(Distance^{TX}_{c,\text{Sep2019}} > Distance^{TX}_{c,\text{Jun2019}}\right),$$

and

$$T_c^{2021} = \mathbb{1}\left(Distance^{TX}_{c,\text{May2022}} < Distance^{TX}_{c,\text{Oct2021}}\right),$$

where $Distance^{TX}_{c,m}$ denotes the distance from the centroid of county c to the nearest active Title X clinic in month m. For each reform, $T_c^p = 1$ identifies treated counties, while $T_c^p = 0$ identifies all other counties, regardless of whether their distance remained unchanged or changed in the opposite direction.

The DID models are estimated separately for the two reforms. The 2019 analysis covers September 2017 through November 2021, while the 2021 analysis covers August 2019 through April 2023. The latter window is restricted to limit exposure to subsequent disruptions in reproductive healthcare provision. In particular, the number of counties with active Title X clinics declined sharply in 2023, as illustrated in Figure 5. This decline may reflect broader changes in the reproductive healthcare environment following the June 2022 *Dobbs v. Jackson Women's Health Organization* decision, although the decline cannot be attributed to that decision alone. Treatment status remains fixed at the county level based on the two selected clinic-distance measurements for each reform.

The DID design compares changes in adolescent fertility outcomes between treated and control counties before and after each policy reform. Its central identifying assumption is that, in the absence of the policy-related changes in Title X clinic access, treated and control counties would have followed parallel trends in teen live-birth conception rates. County fixed effects account for time-invariant differences across counties, while month-year fixed effects absorb common temporal shocks. The model also controls for county-level unemployment rates to account for observed changes in local economic conditions.

Given the discrete nature of birth counts and the presence of county-month observations with

zero births, I estimate the DID models using Poisson Pseudo-Maximum Likelihood (PPML). Following Lindo and Packham (2017), I model the expected number of conceptions resulting in live births and include the population of females aged 15–19 as an exposure term. The logarithm of this population enters the model as an offset with its coefficient constrained to one. This approach accommodates zero counts and allows the treatment coefficient to be interpreted as a proportional change in the expected live-birth conception rate.

The preferred DID specification is

$$E(Y_{ct} \mid D^p_{ct}, X_{ct}, \alpha_c, \lambda_t) = \exp\left(\beta_p D^p_{ct} + \gamma X_{ct} + \alpha_c + \lambda_t + \ln(P_{ct})\right),$$

where Y_{ct} denotes the number of estimated conceptions resulting in live births among mothers aged 15–19 in county c and conception month t. The variable P_{ct} is the corresponding population of females aged 15–19, included as a logarithmic offset. The vector X_{ct} contains time-varying county characteristics, specifically the county unemployment rate. County fixed effects (α_c) account for time-invariant county characteristics, while month-year fixed effects (λ_t) absorb common temporal shocks. Standard errors are clustered at the county level to account for within-county serial correlation.

For each policy reform, the DID treatment indicator is constructed as

$$D^p_{ct} = T^p_c \times Post^p_t, \qquad p \in \{2019, 2021\},$$

where T^p_c identifies treated counties according to the policy-specific distance comparisons described above. The indicator $Post^p_t$ equals one beginning in July 2019 for the Final Rule and November 2021 for the restoration, and zero before the respective cutoff.

The coefficient β_p measures the difference in the proportional change in expected live-birth conception rates between treated and control counties, conditional on the fixed effects, observed controls, and population offset. The corresponding percentage change is calculated as $100[\exp(\beta_p) - 1]$. Because treatment is binary, the coefficient does not represent the effect of an additional kilo-

meter of travel distance.

To examine whether the estimated associations differ for counties exposed to Planned Parenthood (PP) Title X providers, I additionally estimate separate DID specifications using a more restrictive treatment definition. These specifications retain the same binary classification based on changes in distance to the nearest active Title X clinic. They do not measure changes in distance to the nearest PP clinic as a separate continuous treatment variable.

For the 2019 analysis, PP exposure is measured using the provider network in June 2019, before implementation of the Final Rule. A county is classified as PP-exposed if it contains an active PP Title X clinic or, among counties without an in-county Title X clinic, if its nearest active Title X provider is operated by PP. The PP-specific treatment group consists of counties satisfying this exposure criterion that also experienced an increase in distance to the nearest active Title X clinic between June and September 2019.

For the 2021 analysis, PP exposure is measured using the provider network in May 2022, following the restoration. The PP-specific treatment group consists of counties satisfying the same exposure criterion in May 2022 that also experienced a decrease in distance to the nearest active Title X clinic between October 2021 and May 2022.

The PP-specific analysis builds on the overall Title X analysis by applying an additional PP exposure requirement. A county is classified as PP-treated only if it experiences a policy-relevant change in distance to the nearest active Title X clinic and meets one of two conditions: (1) it has an active PP Title X clinic within the county, or (2) it has no Title X clinic within the county and its nearest active Title X clinic is operated by PP. Therefore, the PP-treated counties are a subset of the overall Title X treated counties. All counties that do not meet both requirements serve as controls, including counties experiencing distance changes without PP exposure.

The timing of PP exposure is selected to reflect PP's participation in the Title X network under each reform. For the 2019 Final Rule, PP exposure is measured in June 2019, before PP withdrew from the program, to identify counties that previously relied on PP Title X services and could be affected by its withdrawal. For the 2021 restoration, PP exposure is measured in May 2022 because

PP providers gradually reentered the Title X network after the policy reversal in November 2021. This later measurement captures counties with PP participation following the restoration. Thus, the PP-specific analysis examines whether counties experiencing policy-related changes in Title X access and meeting the PP exposure criteria show different fertility outcomes relative to other counties.

4.2 Regression Discontinuity Design

To complement the main Difference-in-Differences analysis, I employ a time-based Regression Discontinuity (RD) design as a robustness check to examine whether the 2019 Title X Final Rule and the 2021 Rule Restoration generated immediate changes in county-level teen birth rates. Whereas the DID framework exploits cross-county variation in policy-induced changes in geographic access to Title X services, the RD design exploits only the timing of the federal policy implementation. Specifically, it compares county-level teen birth rates immediately before and after each policy cutoff to identify any discontinuous changes occurring at the implementation date. The primary estimation equation for each policy cutoff (July 2019 and November 2021) is specified as:

$$TBR_{ct} = \beta_0 + \beta_1 Post_t + \beta_2 Run_t + \beta_3 (Post_t \times Run_t) + \gamma X_{ct} + \alpha_c + \delta_m + \varepsilon_{ct}, \tag{1}$$

where TBR_{ct} denotes the county-level teen birth rate in county c during month t, measured as the number of births per 1,000 females aged 15–19. $Post_t$ is an indicator variable equal to one for observations occurring after the policy implementation date and zero otherwise. Run_t is the running variable, defined as the number of months relative to the policy cutoff, with negative values representing months before the policy and positive values representing months after the policy. The interaction term, $Post_t \times Run_t$, allows the slope of the running variable to differ on either side of the cutoff. The vector X_{ct} includes time-varying county-level control variables, including the county unemployment rate. County fixed effects (α_c) account for time-invariant differences

across counties, while calendar month fixed effects (δ_m) control for seasonality in teen birth rates. Standard errors are clustered at the county level to account for serial correlation within counties over time.

The coefficient of primary interest is β_1, which captures the discontinuous change in teen birth rates at the policy implementation date after controlling for the underlying local time trend. Because the RD analysis is intended as a robustness check, the estimates are interpreted as evidence of whether the main DID results are supported by an alternative identification strategy based solely on the timing of the policy changes.

Bandwidth selection follows the data-driven procedure proposed by Calonico, Cattaneo, and Titiunik (CCT) and implemented in the rdrobust package. Specifically, the CCT mean squared error (MSE)-optimal bandwidth is used in the primary analysis to determine the estimation window around each policy cutoff. Because the optimal bandwidth differs across policy changes, the estimation sample is determined separately for the 2019 and 2021 analyses. As a robustness check, the RD models are re-estimated using fixed symmetric bandwidths of 12, 18, and 24 months on either side of each policy cutoff while maintaining the same county fixed effects, calendar month fixed effects, and unemployment controls. Comparing results across alternative bandwidths provides evidence on the sensitivity of the estimated policy effects to the choice of estimation window.

The identifying assumption of the RD design is that, absent the Title X policy changes, county-level teen birth rates would have evolved smoothly over time around the policy implementation dates. Under this assumption, any discontinuous change in teen birth rates at the cutoff can be attributed to the implementation of the Title X policy rather than to underlying temporal trends or other confounding factors.

4.3 Event-Study Analysis

To examine the dynamic effects of the 2019 Title X Final Rule and the 2021 Rule Restoration, I extend the baseline Difference-in-Differences framework to an event-study specification estimated using Poisson Pseudo-Maximum Likelihood (PPML). Unlike the baseline DID model, which esti-

mates the average effect of changes in geographic access following each policy reform, the event-study model estimates separate treatment effects for each month relative to the policy implementation date. This approach serves two purposes. First, it allows an assessment of the parallel trends assumption by examining whether counties experiencing different policy-induced changes in access exhibited similar pre-policy trends in teen conception rates. Second, it traces the evolution of treatment effects over time, allowing the analysis to distinguish immediate from delayed responses to changes in Title X availability.

The following PPML specification is estimated separately for the 2019 and 2021 policy changes:

$$E(TBR_{ct} \mid Treat_c, \alpha_c, \alpha_t, X_{ct}) = \exp\left(\sum_{k \neq -1} \beta_k \left[\mathbf{1}(EventMonth_t = k) \times Treat_c \right] + \alpha_c + \alpha_t + \gamma X_{ct} \right), \tag{2}$$

where TBR_{ct} denotes the teen conception rate per 1,000 females aged 15–19 in county c during month t. The variable $EventMonth_t$ represents the number of months relative to the policy implementation date, with $k = -1$ serving as the omitted reference month. The binary treatment indicator $Treat_c$ identifies counties experiencing the policy-relevant change in geographic access to Title X clinics. For the 2019 Final Rule, treated counties are those whose distance to the nearest Title X clinic increased between June and September 2019. For the 2021 Rule Restoration, treated counties are those whose distance decreased between October 2021 and May 2022. Counties without the corresponding directional change serve as the comparison group.

Each coefficient β_k captures the differential change in the log expected teen conception rate between treated and comparison counties in event month k, relative to the month immediately preceding policy implementation. The coefficients can be transformed into percentage changes using $100(e^{\beta_k} - 1)$. County fixed effects (α_c) account for time-invariant county characteristics, while calendar month-year fixed effects (α_t) absorb nationwide shocks and common temporal trends. The vector X_{ct} contains time-varying county characteristics, including the county unemployment rate. Standard errors are clustered at the county level.

The event-study model is estimated separately for the 2019 Final Rule and the 2021 Rule Restoration. For the 2019 analysis, event month is centered on July 2019, while for the 2021 analysis it is centered on November 2021. Estimating the two policy changes separately allows the dynamic responses to provider exits and provider reentry to differ without imposing a common adjustment path across the two policy reforms.

Using monthly rather than annual event time is particularly important in this study because the outcome is measured according to the estimated month of conception rather than the month of birth. Changes in contraceptive access may affect conception rates relatively soon after changes in Title X availability, whereas the resulting births typically occur several months later. Monthly event time therefore provides greater temporal precision for identifying when policy effects begin to emerge. In addition, estimating month-specific treatment effects allows the event-study framework to detect both short-run and longer-run responses that would be obscured under annual aggregation.

The event-study estimates serve two important purposes. First, the pre-policy coefficients provide evidence on whether treated and comparison counties followed similar trends in teen conception rates before the policy changes. A joint test of the pre-policy coefficients evaluates whether these differential changes are statistically distinguishable from zero, although failure to reject the null hypothesis does not establish that parallel trends holds. Second, the post-policy coefficients characterize the dynamic evolution of treatment effects following each policy reform. Together, these estimates help determine whether the effects of changes in Title X access emerge immediately, accumulate gradually over time, or dissipate as counties adjust to changes in the provider network.

5 Analysis of Teen Birth Rate

The expected effects of the Title X policy changes follow directly from the program's role in expanding access to publicly funded family planning services. The July 2019 Title X Final Rule imposed restrictions that prohibited Title X-funded providers from referring patients for abor-

tion services and required physical and financial separation between Title X-funded activities and abortion-related care. These requirements prompted many providers, most notably Planned Parenthood, to withdraw from the Title X network, reducing the availability of contraceptive and reproductive health services in many communities. Consequently, the 2019 policy is expected to reduce access to effective contraception, increase barriers to family planning services, and ultimately increase teen birth rates, particularly in counties experiencing the largest reductions in Title X access.

In contrast, the November 2021 Title X Rule Restoration reinstated the pre-2019 funding requirements and permitted providers that had previously withdrawn to rejoin the Title X program. The restoration of the provider network is therefore expected to improve access to comprehensive family planning services and reduce teen birth rates, especially in counties where access to Title X services improved following provider reentry.

The remainder of this section evaluates these hypotheses using the three empirical approaches described in the previous section. The Difference-in-Differences analysis estimates the average effects of policy-induced changes in geographic access to Title X services, the Regression Discontinuity analysis examines whether the policy implementation dates generated immediate discontinuities in teen birth rates, and the event-study analysis evaluates the dynamic evolution of policy effects over time while testing the parallel trends assumption.

5.1 Difference-in-Differences Estimates

Table 3 presents the Difference-in-Differences (DID) estimates of the associations between the 2019 Title X Final Rule, the 2021 policy restoration, and county-level teen birth rates. The table reports four specifications: overall Title X exposure and Planned Parenthood (PP)-specific exposure for each policy reform.

The proportion of treated counties differs across the two reforms and treatment definitions. In the overall Title X analysis, 19.66% of counties were classified as treated following the 2019 Final Rule, compared with 11.54% following the 2021 restoration. In the PP-specific analysis,

the corresponding proportions were 12.25% and 7.30%, respectively. The smaller PP-specific treatment groups reflect the additional requirement that counties have relevant exposure to a PP Title X provider.

Columns (1) and (2) report the estimates for the 2019 Final Rule. In the overall Title X specification, the estimated DID coefficient is -0.0179 (SE = 0.0081), statistically significant at the 5% level. Under the PPML log-link specification, this coefficient corresponds to an approximately 1.77% lower expected teen live-birth conception rate in treated counties relative to control counties following the reform. The PP-specific specification produces a similar coefficient of -0.0174 (SE = 0.0089), also statistically significant at the 5% level, corresponding to an approximately 1.72% lower expected rate. Although these estimates are negative and statistically significant, their causal interpretation depends on the validity of the DID identifying assumptions, particularly parallel trends.

Columns (3) and (4) report the results for the 2021 restoration. The overall Title X coefficient is 0.0109 (SE = 0.0125), corresponding to an approximately 1.10% higher expected teen live-birth conception rate in treated counties relative to control counties. The PP-specific coefficient is 0.0071 (SE = 0.0156), corresponding to an approximately 0.71% higher expected rate. Neither estimate is statistically significant at conventional levels. Thus, the baseline DID results do not provide statistically significant evidence of changes in teen live-birth conception rates associated with improved geographic access following the 2021 restoration.

To assess the plausibility of the parallel-trends assumption, I estimate PPML event-study specifications for both reforms, separately examining overall Title X exposure and PP-specific exposure. The event-study analysis examines dynamic differences between treated and control counties, using the month immediately preceding each policy cutoff as the reference period. Table 4 reports joint Wald tests of the pre-policy coefficients within a 12-month window before each reform.

For the 2019 Final Rule, the null hypothesis that the pre-policy coefficients are jointly equal to zero is rejected in both the overall Title X specification ($p = 0.0059$) and the PP-specific specification ($p = 0.0448$). These findings indicate statistically significant differential pre-policy dynamics

between treated and control counties, raising concerns about the parallel-trends assumption and limiting the causal interpretation of the negative 2019 DID estimates.

For the 2021 restoration, the corresponding tests fail to reject the null hypothesis in both the overall Title X specification ($p = 0.4239$) and the PP-specific specification ($p = 0.6146$). Although these results do not reveal statistically significant differential pre-policy dynamics, they do not establish that the parallel-trends assumption holds. Together with the statistically insignificant DID estimates, the event-study evidence suggests that the available data do not provide conclusive evidence of a change in teen live-birth conception rates following the 2021 restoration.

5.2 Event Study Analysis

To further investigate the relationship between Title X policy changes and adolescent fertility, I examine the dynamic estimates from the event-study specifications described in the methodology section. Table 6 reports the pre-policy coefficients and joint tests of pre-policy trends, while Table 7 presents the post-policy coefficients. Figures 9–12 illustrate the corresponding dynamic estimates and their 95% confidence intervals. Together, these results provide evidence on the plausibility of the parallel-trends assumption and whether changes in teen live-birth conceptions emerged immediately or gradually following each reform.

Table 6 presents the event-study estimates for the months preceding the 2019 Final Rule and the 2021 restoration. The most important distinction between the two reforms is the strength of the evidence of differential pre-policy trends.

For the 2019 Final Rule, the joint Wald test rejects the null hypothesis that all pre-policy coefficients are zero in the overall Title X specification ($\chi^2(17) = 31.12$, $p = 0.0193$). Although most individual coefficients are not statistically significant, the joint test indicates that the pre-policy differences between treated and control counties are collectively significant. This distinction matters because examining individual coefficients alone could suggest that pre-policy trends are relatively similar, while the joint test reveals systematic differences that may undermine the DID identifying assumption. Figure 9 further illustrates that the estimated coefficients are generally positive in sev-

eral earlier pre-policy months but become negative closer to implementation, suggesting that the relative outcomes of treated and control counties were already changing before the Final Rule.

The PP-specific specification in column (2) provides similar, although somewhat weaker, evidence of differential pre-policy dynamics. The joint Wald test yields $\chi^2(17) = 27.34$ ($p = 0.0532$), which is significant at the 10% level but not at the conventional 5% level. Notably, the estimated coefficient for month -11 is positive and statistically significant in both the overall and PP-specific specifications. The similarity between these two specifications suggests that restricting treatment to counties with PP exposure does not eliminate concerns about pre-existing differences in fertility trends. Consequently, the negative DID estimates associated with the 2019 Final Rule should be interpreted cautiously rather than as definitive evidence of a causal policy effect.

In contrast, the 2021 restoration exhibits less evidence of differential pre-policy dynamics. The joint Wald tests fail to reject the null hypothesis in both the overall Title X specification ($\chi^2(16) = 17.20$, $p = 0.3726$) and the PP-specific specification ($\chi^2(16) = 14.91$, $p = 0.5313$). Although the PP-specific coefficient at month -11 is individually significant, the pre-policy coefficients are not jointly significant. This result is important because it indicates that isolated statistically significant coefficients do not necessarily imply systematic differences in pre-policy trends. Compared with the 2019 analysis, the 2021 specifications provide less statistical evidence against the parallel-trends assumption. Nevertheless, failure to reject the joint null does not establish that parallel trends holds.

Table 7 reports the event-study estimates following the implementation of each reform. These coefficients help distinguish short-run changes from more persistent differences in adolescent fertility outcomes.

For the 2019 Final Rule, neither the overall Title X nor the PP-specific specification exhibits a consistent post-policy response. In the overall Title X specification, the estimated coefficients are initially close to zero, followed by negative estimates in several subsequent months. However, only a few individual coefficients are statistically significant, including months $+3$, $+7$, and $+17$. The PP-specific estimates also fluctuate between positive and negative values, with limited evidence of

statistically significant changes. Importantly, the negative estimates do not persist throughout the post-policy period, and some later coefficients become positive. These patterns suggest that the negative average DID estimates are not accompanied by a stable or consistently negative dynamic response. Combined with the evidence of differential pre-policy trends, this weakens the interpretation that the 2019 Final Rule generated a sustained reduction in teen live-birth conceptions.

The 2021 restoration exhibits a different short-run pattern. In the overall Title X specification, the coefficient for the implementation month is negative and statistically significant ($\beta = -0.0839$, $p < 0.05$), corresponding to an approximately 8.05% lower expected live-birth conception rate relative to the omitted pre-policy month, conditional on the model specification. The PP-specific specification also produces a negative implementation-month coefficient ($\beta = -0.0862$, $p < 0.10$). Additional negative coefficients appear in months $+2$ and $+3$ for the PP-specific analysis and month $+3$ for the overall Title X analysis.

These initial negative estimates are noteworthy because they do not indicate an immediate increase in teen live-birth conceptions following improved geographic access to Title X services. However, the short-run coefficients should not be interpreted as evidence of an immediate causal effect of the restoration. Changes in clinic participation occurred gradually, and the treatment groups are defined using changes in access measured through May 2022. Therefore, the implementation-month estimates may reflect differences associated with counties that subsequently experienced changes in clinic access, rather than an immediate response to restored services.

Furthermore, the negative estimates are not sustained. In later post-policy months, the coefficients fluctuate around zero, and most are not statistically significant. Some estimates become positive, particularly around months $+8$ and $+9$, but these changes are also statistically insignificant. This pattern suggests that the initial negative associations are temporary or imprecisely estimated rather than part of a persistent post-restoration trend. The absence of a stable dynamic response is consistent with the statistically insignificant average DID estimates reported in Table 3.

The event-study results highlight two distinct limitations of the baseline DID findings. For the

2019 Final Rule, statistically significant pre-policy differences raise concerns about the parallel-trends assumption, limiting the causal interpretation of the negative DID estimates. For the 2021 restoration, the pre-policy tests do not reveal systematic differential trends, but the post-policy coefficients provide little evidence of a sustained change in teen live-birth conception rates. Across both reforms, the overall Title X and PP-specific specifications exhibit broadly similar patterns, suggesting that the main findings are not driven solely by the choice of exposure definition. However, the PP-specific estimates reflect a more restrictive classification of Title X access changes and should not be interpreted as isolating the effects of individual PP clinic exits or reentries.

5.3 Regression Discontinuity (RD) Estimates

As a robustness check to the Difference-in-Differences analysis, I estimate a time-based Regression Discontinuity (RD) design that exploits the exact implementation dates of the two major Title X policy changes. Unlike the DID approach, which identifies average changes over the post-policy period, the RD design examines whether teen birth rates exhibit an immediate discontinuity at the policy implementation dates. If the policy changes had an instantaneous effect on access to family planning services, a corresponding jump or decline in teen birth rates would be expected immediately after the policy cutoff. The RD specifications include county fixed effects, calendar month fixed effects, and county unemployment rates as controls. Following the optimal bandwidth selection procedure implemented in `rdrobust`, the preferred specification uses a symmetric bandwidth of seven months around each policy cutoff. Table 3 presents the main RD estimates for the July 2019 Title X Final Rule and the November 2021 Title X Rule Restoration. For the July 2019 policy change, the coefficient on the post-policy indicator is positive and statistically significant, suggesting a discrete increase in teen birth rates immediately following implementation of the Final Rule. In contrast, the November 2021 policy restoration is also associated with a positive and statistically significant post-policy coefficient. Although this result is inconsistent with the expected long-run effect of restoring access to Title X services, it likely reflects short-run adjustment dynamics immediately surrounding the policy implementation rather than the policy's

ultimate impact on reproductive behavior.

For both policy changes, the interaction between the running variable and the post-policy indicator is statistically insignificant, indicating that the local trend in teen birth rates does not differ substantially on either side of the policy threshold within the optimal bandwidth. The estimated discontinuities therefore primarily reflect shifts in the intercept at the policy cutoff rather than changes in the local slope of the outcome trend. Figures 6 and 7 provide graphical evidence for the regression discontinuity analyses. Figure 6 plots monthly county-level teen birth rates around the July 2019 policy implementation date. Each point represents the average teen birth rate within a running-variable bin, while the solid curve represents the fourth-order polynomial fitted separately on each side of the policy threshold. The figure shows a modest upward discontinuity at the July 2019 cutoff, consistent with the positive post-policy coefficient reported in Table 3. However, the discontinuity is relatively small compared with the overall downward trend in teen birth rates observed throughout the study period.

Figure 7 presents the corresponding RD plot for the November 2021 Title X Rule Restoration. Similar to Figure 6, the fitted regression functions suggest a small discontinuity at the policy threshold. The graphical evidence indicates that teen birth rates did not immediately decline following the restoration of the Title X regulations. Instead, the observed pattern suggests that any policy effects may have emerged gradually over time rather than occurring instantaneously at the implementation date.

Table 4 evaluates the sensitivity of the RD estimates to alternative bandwidth selections by re-estimating the models using symmetric windows of ±12, ±18, and ±24 months around each policy cutoff. Across the July 2019 specifications, the estimated effect of the running variable remains consistently negative and statistically significant, while the post-policy coefficient is generally sensitive to bandwidth choice. In particular, the positive post-policy effect identified in the preferred specification is not consistently observed across narrower and wider bandwidths, suggesting that the magnitude of the estimated discontinuity depends on the choice of estimation window.

For the November 2021 policy restoration, a similar pattern emerges. Although some coeffi-

cients remain statistically significant under alternative bandwidths, both the magnitude and statistical significance of the post-policy indicator vary across specifications. In contrast, the interaction between the running variable and the post-policy indicator remains generally small and statistically insignificant throughout the robustness analyses. Overall, the qualitative conclusions remain unchanged, indicating that the RD results are not driven by a single bandwidth selection, while also highlighting the sensitivity of the estimated discontinuities to the local estimation window.

Taken together, the regression discontinuity analyses provide limited evidence that the Title X policy changes generated immediate shifts in county-level teen birth rates. While statistically significant discontinuities are observed in some specifications, the estimated effects are not consistently robust across alternative bandwidths and do not uniformly align with the expected direction of the policy changes. These findings are not unexpected because the mechanisms through which Title X policies affect fertility—including changes in provider participation, contraceptive access, and individuals' reproductive decisions—are unlikely to operate instantaneously. Instead, the effects of the policy changes are expected to accumulate over time as provider networks adjust and individuals respond to changes in service availability. Consequently, the RD analysis is best interpreted as evidence regarding short-run discontinuities, while the subsequent Difference-in-Differences and event-study analyses are better suited for evaluating the medium- and long-term impacts of the 2019 and 2021 Title X policy changes.

5.4 Discussion and Contributions

The empirical findings provide a nuanced picture of the relationship between Title X policy changes, geographic access to publicly funded family planning services, and adolescent fertility. Although the 2019 Final Rule and the 2021 restoration substantially altered the regulatory environment for Title X providers, the estimated changes in teen conceptions do not consistently follow the directions predicted by the conventional relationship between contraceptive access and fertility. The results suggest that changes in the availability of Title X-funded clinics may not translate directly or immediately into measurable changes in adolescent fertility outcomes.

The 2019 Difference-in-Differences estimates indicate that counties experiencing an increase in distance to the nearest Title X clinic exhibited approximately 1.8% lower expected teen conception rates relative to comparison counties. This negative association is contrary to the initial hypothesis that reduced access to publicly funded contraceptive services would increase adolescent fertility. However, the event-study results reveal statistically significant differential pre-policy dynamics, raising concerns about the parallel-trends assumption. Consequently, the negative estimates cannot be confidently attributed to the 2019 Final Rule. Rather than suggesting that restrictions on Title X services reduced teen fertility, these findings highlight the importance of accounting for underlying differences between counties experiencing changes in clinic access and those maintaining relatively stable provider networks.

For the 2021 restoration, the DID estimates are positive but statistically insignificant, providing no clear evidence that improvements in geographic access were associated with reductions in teen conceptions. Although the event-study specifications identify several negative coefficients immediately following the policy change, these estimates do not persist across subsequent months. One possible explanation is that regulatory restoration did not immediately translate into a full recovery of clinical capacity, patient utilization, or contraceptive service provision. Clinics returning to the Title X network may have required time to restore services, while adolescents may have continued relying on alternative providers or existing contraceptive methods.

The broadly similar findings from the overall Title X and Planned Parenthood exposure specifications further suggest that the observed patterns are not unique to a single definition of provider exposure. Nevertheless, the PP-specific estimates should be interpreted as associations among counties exposed to PP providers rather than as causal effects attributable exclusively to PP withdrawals or reentry. Taken together, the findings emphasize that the relationship between regulatory changes, provider availability, actual service utilization, and reproductive outcomes is potentially complex.

The estimated effects can be compared with evidence from other family planning policy interventions. Lindo and Packham (2017) examine Colorado's Family Planning Initiative, which

expanded access to long-acting reversible contraceptives (LARCs), and find that the program reduced teen birth rates by approximately 6.4% over five years in counties with funded clinics. In contrast, Packham (2017) investigates substantial reductions in publicly funded family planning services in Texas and finds that funding cuts increased teen birth rates by approximately 3.4% over four years. Both studies identify effects that emerge primarily several years after implementation, suggesting that fertility responses to changes in contraceptive access may be delayed. Compared with these findings, the present study estimates a smaller reduction in teen conceptions of approximately 1.8% following the 2019 Title X restrictions and a statistically insignificant increase of approximately 1.1% following the 2021 restoration. However, the significant pre-policy trend differences in the 2019 analysis and the statistical uncertainty surrounding the 2021 estimates limit the extent to which these differences can be interpreted as differences in causal policy effects. Furthermore, the interventions differ in their mechanisms and intensity: Colorado directly subsidized access to effective contraceptive methods, Texas substantially reduced family planning funding, and the Title X reforms changed provider participation in a federal funding network. These distinctions, together with differences in outcome measurement and post-policy horizons, may help explain the variation in estimated magnitudes across studies.

This study contributes to the literature and policy evaluation in several respects. First, it examines two major regulatory changes within the same federal family planning program: the 2019 restrictions and the 2021 restoration. Evaluating these reforms separately provides an opportunity to investigate whether contractions and expansions of the provider network are associated with symmetric changes in adolescent fertility. The absence of a clear reversal in the estimated outcomes suggests that such symmetry should not be assumed, although the current estimates do not establish the causal effects of either reform.

Second, the paper constructs a monthly county-level dataset linking restricted-use NVSS natality records to a historical database of Title X clinic availability. By assigning live births to their estimated conception months, the analysis more closely aligns reproductive outcomes with the timing of policy changes than conventional birth-month measures. This approach also permits the

examination of short-run dynamics that may be obscured by annual aggregation. Moreover, the use of county-level geographic access measures captures variation in exposure to a federal policy that was implemented nationwide, providing a framework for examining the heterogeneous local consequences of national regulatory changes.

Third, the findings offer lessons for the design and evaluation of reproductive healthcare policy. Changes in formal program participation should not be treated as sufficient evidence of changes in effective contraceptive access or fertility outcomes. Policymakers evaluating Title X reforms should consider multiple dimensions of program performance, including clinic participation, service capacity, patient volume, contraceptive utilization, and reproductive health outcomes. In particular, the absence of a statistically significant fertility response following the 2021 restoration should not be interpreted as evidence that restoring Title X services was ineffective, since the program provides benefits extending beyond fertility outcomes, including preventive care, counseling, and screening services.

Finally, the analysis illustrates the importance of explicitly evaluating identifying assumptions when studying nationwide policy changes through local variation in exposure. The evidence of differential pre-policy dynamics in the 2019 specifications demonstrates that counties experiencing provider-network disruptions may differ systematically from comparison counties. Recognizing these limitations is essential for distinguishing descriptive associations from causal policy effects. Overall, the paper contributes new county-level evidence on the relationship between recent Title X regulatory reforms and adolescent fertility, while highlighting the need for further research on the mechanisms connecting provider-network changes to actual reproductive healthcare utilization.

6 Plans for Next Step

The empirical results presented in this paper provide an initial evaluation of the effects of the 2019 and 2021 Title X policy changes on teen fertility using Difference-in-Differences, Regression Discontinuity, and event-study analyses. Future research will focus on two extensions. First, I

will conduct heterogeneity analyses to examine whether the estimated effects differ across demographic groups and geographic areas, particularly among populations that may rely more heavily on publicly funded family planning services. Second, I will perform additional robustness checks to assess the sensitivity of the findings to alternative treatment definitions, sample restrictions, and model specifications. These extensions will help identify potential differences in policy exposure across populations, evaluate the reliability of the empirical results, and provide a more comprehensive understanding of how changes in the Title X provider network are associated with adolescent fertility outcomes.

6.1 Heterogeneity Analysis

The baseline Difference-in-Differences estimates identify the average effect of changes in Title X access on county-level teen birth rates. However, the impacts of the 2019 and 2021 policy changes are unlikely to be uniform across all adolescents. Access to reproductive healthcare, contraceptive use, and reliance on publicly funded family planning services differ substantially across demographic and socioeconomic groups. To better understand which populations were most affected by changes in the Title X provider network, the final stage of the analysis will estimate treatment effects separately by age, race and ethnicity, and maternal educational attainment.

6.1.1 Urban and Rural Analysis

To examine whether the effects of the 2019 and 2021 Title X policy changes differ between urban and rural areas, I conduct a heterogeneity analysis based on county urban–rural classification. Geographic access to reproductive healthcare services may be particularly important in rural counties, where healthcare providers are often more geographically dispersed and alternative sources of affordable contraceptive services may be limited. Consequently, changes in the Title X provider network may have different implications for adolescent fertility in urban and rural areas.

I estimate the baseline Difference-in-Differences (DID) specification separately for urban and rural counties, maintaining the same treatment definitions, estimation periods, control variables,

and fixed effects as in the main analysis. Specifically, I examine both overall Title X clinic access and Planned Parenthood (PP)-specific exposure for the 2019 Final Rule and the 2021 restoration. Comparing the estimated coefficients across urban and rural counties allows me to assess whether the associations between changes in geographic access to Title X services and teen live-birth conception rates vary by geographic setting. This analysis provides additional evidence on whether the consequences of Title X policy changes are concentrated in areas with potentially greater barriers to reproductive healthcare access.

6.1.2 Age-specific Analysis

The second heterogeneity analysis will estimate separate treatment effects for each individual age between 15 and 19 years. Adolescents at different ages may respond differently to changes in Title X availability because their reproductive health needs, contraceptive use, and ability to access healthcare vary substantially. Younger teenagers may rely more heavily on confidential, publicly funded family planning services because they have fewer financial resources, less independent transportation, and greater concerns about confidentiality. Older teenagers, in contrast, may have greater access to alternative healthcare providers or private insurance. Estimating age-specific effects will therefore help identify whether changes in Title X access have larger impacts on particular age groups within the teenage population.

6.1.3 Race and Ethnicity

The analysis will also estimate separate effects by race and ethnicity, including non-Hispanic White, non-Hispanic Black, Hispanic, and other racial and ethnic groups. Because Title X primarily serves low-income and medically underserved communities, reductions in provider availability may disproportionately affect populations that rely more heavily on publicly funded reproductive health services. Examining heterogeneous effects across racial and ethnic groups will therefore provide insight into whether the recent Title X policy changes contributed to existing disparities in adolescent reproductive health outcomes.

6.1.4 Baseline Title X Provider Network

A final heterogeneity analysis will examine whether the effects of the 2019 and 2021 Title X policy changes differ according to the structure of the local Title X provider network before the policy changes. Unlike previous studies, the monthly Office of Population Affairs (OPA) Title X database constructed for this project provides detailed information on the number and type of Title X providers operating in each county over time. This information allows counties to be classified according to their baseline reliance on the Title X safety-net system prior to implementation of the 2019 Final Rule.

Specifically, counties will be classified into four mutually exclusive groups based on the composition of their Title X provider network immediately before the policy change: (1) counties served by a single non-Planned Parenthood Title X clinic; (2) counties served by multiple Title X clinics; (3) counties served by a single Planned Parenthood Title X clinic; and (4) counties without a Title X clinic. The Difference-in-Differences model will then be estimated separately for each group to examine whether the effects of the policy changes differ across local healthcare markets.

This classification is motivated by the expectation that the consequences of provider exits depend not only on changes in travel distance but also on the availability of alternative providers. Counties served by only a single Title X clinic may lose nearly all publicly funded family planning services if that provider exits the network, whereas counties with multiple Title X clinics may continue to maintain access through remaining providers. Similarly, counties whose only Title X provider is a Planned Parenthood clinic may experience particularly large disruptions because Planned Parenthood historically served a substantial share of Title X patients and has been one of the nation's largest providers of confidential contraceptive and reproductive health services. By contrast, counties without a Title X clinic are not expected to experience direct changes in local Title X availability following the federal policy reforms and therefore provide a useful comparison group.

Comparing treatment effects across these four county types will provide insight into whether the impacts of the 2019 and 2021 policy changes depended on the baseline structure of the local

Title X provider network. In particular, this analysis will help distinguish whether policy effects were driven primarily by the loss of any nearby Title X provider, by the withdrawal and subsequent reentry of Planned Parenthood clinics, or by the availability of alternative Title X providers within a county. These results will provide additional evidence on which communities are most vulnerable to changes in the federal family planning safety net and may help inform future decisions regarding the allocation and retention of Title X providers.

6.2 Additional Robustness Checks

An important robustness analysis will evaluate whether the estimated policy effects are sensitive to the method used to construct the month of conception. The primary analysis measures teen birth rates according to the estimated month of conception rather than the month of delivery. The main specification combines birth records with valid reported last normal menstrual period (LMP) information and birth records for which the conception month is recovered using gestational age and birth month. This approach substantially reduces missing information while preserving nearly the full analytic sample.

To assess the sensitivity of the results to this measurement strategy, two additional robustness specifications will be estimated and compared with the main analysis. The first robustness specification will restrict the sample to births with valid reported LMP information only, excluding all observations requiring imputation. This specification evaluates whether the estimated policy effects are driven by the inclusion of imputed conception dates. The second robustness specification will instead estimate the conception month for all birth records using only gestational age and birth month, regardless of whether a valid LMP was reported. Applying the same estimation procedure to every observation eliminates any potential differences arising from combining reported and imputed conception dates within the same analysis.

The Difference-in-Differences and event-study analyses will be re-estimated under each of these alternative specifications, and the resulting estimates will be compared with those obtained from the primary analysis. If the estimated treatment effects remain similar across all three spec-

ifications, it would provide strong evidence that the empirical findings are robust to alternative methods of measuring conception timing. Such consistency would increase confidence that the estimated effects reflect changes in teen fertility associated with the Title X policy reforms rather than artifacts of the procedure used to recover missing LMP information. Conversely, meaningful differences across specifications would suggest that the measurement of conception timing influences the estimated policy effects and would motivate further investigation into alternative approaches for constructing conception dates.

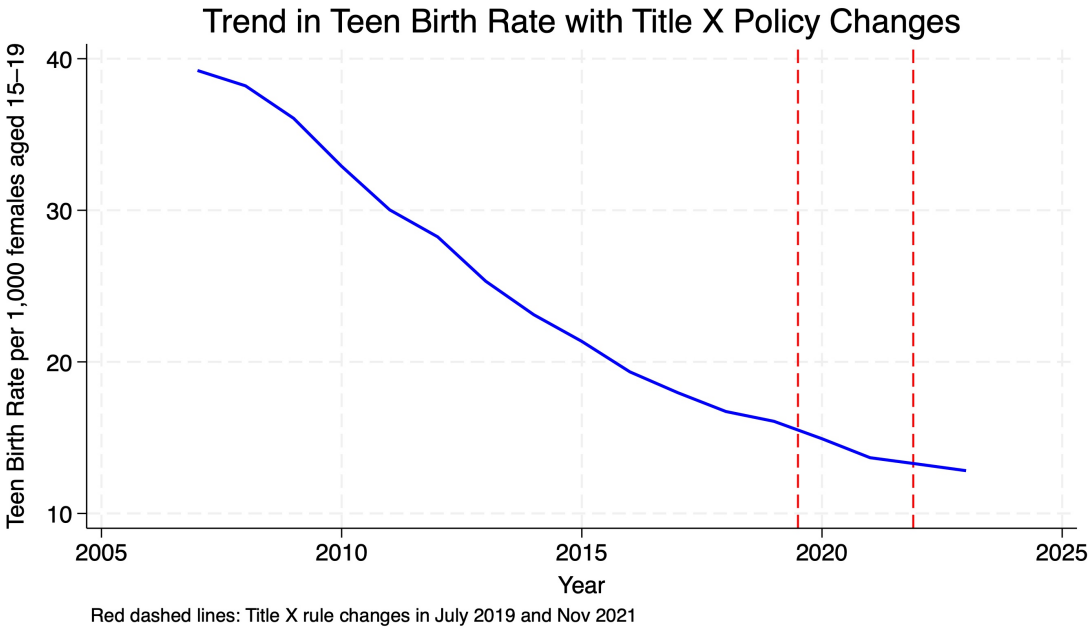


Figure 1: Trend in U.S. Teen Birth Rates and Major Title X Policy Changes

Notes: The figure illustrates the annual U.S. teen birth rate (births per 1,000 females aged 15–19) from 2007 to 2023 using data from the CDC WONDER Natality Files. The two vertical dashed lines indicate the implementation of the July 2019 Title X Final Rule and the November 2021 Title X Rule Restoration.

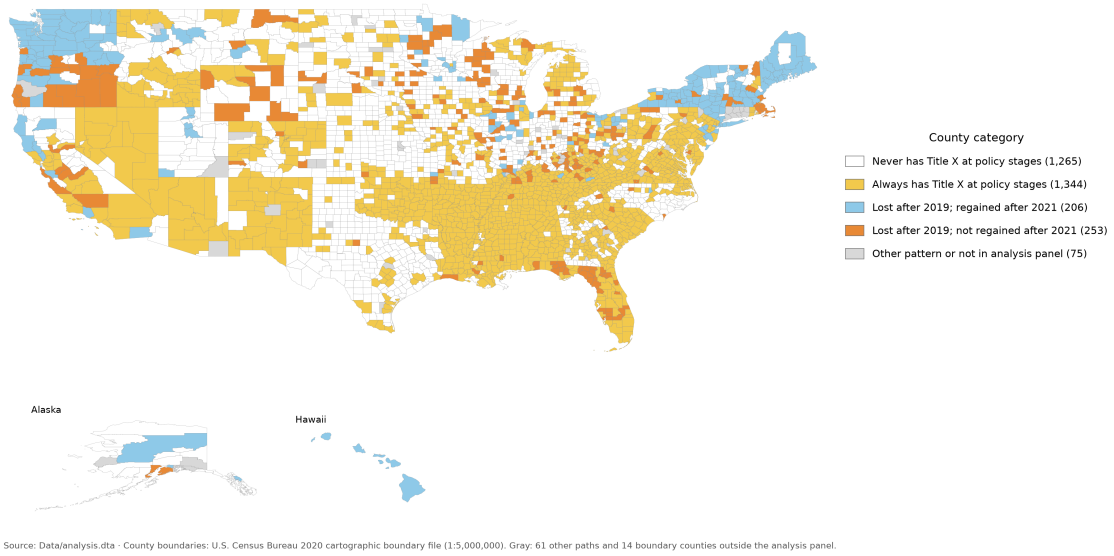


Figure 2: County-Level Changes in Title X Clinic Availability Following the 2019 and 2021 Policy Changes

Notes: The figure classifies U.S. counties according to changes in Title X clinic availability following the July 2019 Final Rule and the November 2021 Rule Restoration. Yellow counties continuously had at least one Title X clinic throughout analysis periods. White counties never had a Title X clinic during the study period. Blue counties lost access to at least one Title X clinic after the 2019 policy change and regained access following the 2021 policy reversal. Orange counties lost Title X access after the 2019 policy change but had not regained access by the end of the study period. Gray counties represent other transition patterns or counties excluded from the analysis panel. County boundaries are based on the 2020 U.S. Census Bureau cartographic boundary files.

Table 1: Summary Statistics of Teen Birth Outcomes by Policy Period

Variable	N	Mean	SD	Min	Max
Panel A: Pre-2019 Final Rule (September 2017–July 2019)					
Teen conceptions	71,967	4.55	13.90	0	402
Female population age 15–19	71,967	3,326.07	10,526.24	3	319,116
Teen birth rate (per 1,000)	71,967	1.84	2.58	0	90.91
Panel B: 2019 Final Rule Period (August 2019–November 2021)					
Teen conceptions	87,612	3.93	11.71	0	334
Female population age 15–19	87,612	3,331.48	10,432.25	1	309,126
Teen birth rate (per 1,000)	87,612	1.64	2.62	0	200.00
Panel C: Post-2021 Rule Restoration (December 2021–December 2024)					
Teen conceptions	115,761	2.81	9.63	0	312
Female population age 15–19	115,761	3,416.67	10,639.82	1	302,187
Teen birth rate (per 1,000)	115,761	1.15	2.31	0	250.00
Panel D: Full Sample					
County unemployment rate (%)	275,264	4.40	2.26	0.90	36.10

Notes: The unit of observation is the county-month. The sample covers September 2017 through December 2024. Teen birth rates are measured as the number of teen conceptions ends in live birth per 1,000 females aged 15–19 in each county-month. Policy periods correspond to the implementation of the 2019 Title X Final Rule and the 2021 restoration of Title X regulations.

Table 2: Summary Statistics of Title X Availability by Policy Period

Variable	N	Mean	SD	Min	Max
Panel A: Pre-2019 Final Rule (September 2017–July 2019)					
Any Title X clinic	71,967	0.584	0.493	0	1
Number of Title X clinics	71,967	1.125	3.293	0	114
Title X clinics per 100,000 females age 15–44	71,967	20.05	42.47	0	680.27
Any Planned Parenthood clinic	71,967	0.074	0.262	0	1
Number of Planned Parenthood clinics	71,967	0.118	0.663	0	25
Planned Parenthood clinics per 100,000 females age 15–44	71,967	0.396	2.749	0	107.87
Distance to nearest Title X clinic (km)	71,967	26.19	73.59	0	3789.30
Panel B: 2019 Final Rule Period (August 2019–November 2021)					
Any Title X clinic	87,612	0.457	0.498	0	1
Number of Title X clinics	87,612	0.836	2.569	0	88
Title X clinics per 100,000 females age 15–44	87,612	17.58	41.52	0	719.42
Any Planned Parenthood clinic	87,612	0.004	0.064	0	1
Number of Planned Parenthood clinics	87,612	0.006	0.102	0	9
Planned Parenthood clinics per 100,000 females age 15–44	87,612	0.016	0.534	0	107.87
Distance to nearest Title X clinic (km)	87,612	66.44	266.55	0	4422.14
Panel C: Post-2021 Rule Restoration (December 2021–December 2024)					
Any Title X clinic	115,761	0.433	0.495	0	1
Number of Title X clinics	115,761	0.940	3.385	0	134
Title X clinics per 100,000 females age 15–44	115,761	16.10	45.87	0	1449.28
Any Planned Parenthood clinic	115,761	0.049	0.215	0	1
Number of Planned Parenthood clinics	115,761	0.081	0.652	0	33
Planned Parenthood clinics per 100,000 females age 15–44	115,761	0.239	2.430	0	123.76
Distance to nearest Title X clinic (km)	115,761	46.86	140.33	0	4368.30

Notes: The unit of observation is the county-month. Statistics are calculated from the monthly county-level Title X availability dataset constructed from the Office of Population Affairs (OPA) Title X Clinic Locator and Directory. Clinic locations were geocoded and aggregated to the county-month level. "Any Title X clinic" and "Any Planned Parenthood clinic" are indicator variables equal to one if at least one clinic of the corresponding type is located in the county during a given month. Clinic rates are measured per 100,000 females aged 15–44. Distance is measured as for the counties without Title X clinics the distance from the county centroid to the nearest active Title X clinic and equals zero for counties containing at least one Title X clinic. The three policy periods correspond to the pre-2019 Title X Final Rule (September 2017–July 2019), the 2019 Final Rule period (August 2019–November 2021), and the post-2021 rule restoration period (December 2021–December 2024).

Table 3: Difference-in-Differences Estimates of Title X Policy Changes on Teen Birth Rates

	(1)	(2)	(3)	(4)
	July 2019		November 2021	
	All Title X	PP Exposure	All Title X	PP Exposure
Outcome: Teen birth rate				
Post-policy × Treated (2019)	-0.0179**			
	(0.0081)			
Post-policy × PP Treated (2019)		-0.0174**		
		(0.0089)		
Post-policy × Treated (2021)			0.0109	
			(0.0125)	
Post-policy × PP Treated (2021)				0.0071
				(0.0156)
Unemployment rate (%)	-0.0077***	-0.0076***	-0.0021	-0.0019
	(0.0015)	(0.0016)	(0.0013)	(0.0013)
County fixed effects	Yes	Yes	Yes	Yes
Month-year fixed effects	Yes	Yes	Yes	Yes
Population offset	Yes	Yes	Yes	Yes
Observations	157,998	144,636	138,645	132,255

Notes: This table reports PPML difference-in-differences estimates of the 2019 Title X Final Rule and 2021 restoration. The dependent variable is the monthly count of conceptions resulting in live births among females aged 15–19, with the log female population aged 15–19 as an offset. Columns (1) and (3) define treatment by policy-relevant changes in distance to the nearest active Title X clinic; columns (2) and (4) additionally require Planned Parenthood (PP) exposure. Control counties are those not meeting the corresponding treatment criteria. All specifications include county and month-year fixed effects and county unemployment controls. County-clustered standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: Joint Tests of Pre-Policy Trends

	(1)	(2)	(3)	(4)
	July 2019		November 2021	
	All Title X	PP Exposure	All Title X	PP Exposure
Joint pretrend p-value	0.0059	0.0448	0.4239	0.6146
Reject at 5%	Yes	Yes	No	No
Observations	76,100	69,625	76,075	72,525

Notes: This table reports joint Wald tests of the pre-policy coefficients from Poisson Pseudo-Maximum Likelihood (PPML) event-study regressions.
Each specification includes monthly event-time indicators interacted with treatment status, using the month immediately preceding the policy change as the reference period. The event-study window covers 12 months before and 12 months after each policy change.
The null hypothesis is that all pre-policy interaction coefficients are jointly equal to zero.
All specifications include county and month-year fixed effects, control for county-level unemployment rates, and include the logarithm of the female population aged 15–19 as an offset. Standard errors are clustered at the county level.
PP denotes Planned Parenthood.

Table 5: Regression Discontinuity Estimates Around the 2019 and 2021 Title X Policy Changes

	(1) July 2019	**(2)** November 2021
Running variable	-0.220^{***}	-0.169^{**}
	(0.050)	(0.078)
Post-policy indicator	2.114^{***}	2.323^{***}
	(0.559)	(0.824)
Running variable $\times$ Post	0.055	-0.045
	(0.044)	(0.044)
Unemployment rate (%)	-0.015	0.024
	(0.018)	(0.028)
County fixed effects	Yes	Yes
Month fixed effects	Yes	Yes
Bandwidth	$\pm$7 months	$\pm$7 months
Bandwidth selection	CCT MSE-optimal	CCT MSE-optimal
Observations	46,935	46,929
R^2	0.21	0.16

Notes. The table reports local linear regression discontinuity estimates around the July 2019 Title X Final Rule and the November 2021 Title X Rule Restoration. The running variable is measured as the number of months relative to each policy implementation date. Bandwidths are selected using the Calonico–Cattaneo–Titiunik (CCT) mean squared error (MSE)-optimal procedure implemented in `rdrobust`. All specifications include county fixed effects, calendar month fixed effects, and county unemployment rates. Robust standard errors are clustered at the county level.
Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: Event-Study Estimates of Title X Policy Changes: Pre-Policy Trends in Teen Live-Birth Conception Rates

	2019 Final Rule		2021 Restoration	
Event Month	(1) Overall	(2) PP Exposure	(3) Overall	(4) PP Exposure
Month −18	0.0083	0.0285	—	—
	(0.0268)	(0.0308)		
Month −17	-0.0333	-0.0314	-0.0307	-0.0429
	(0.0330)	(0.0378)	(0.0277)	(0.0326)
Month −16	-0.0078	0.0385	0.0168	-0.0114
	(0.0327)	(0.0357)	(0.0382)	(0.0444)
Month −15	0.0273	0.0509	-0.0207	-0.0253
	(0.0317)	(0.0355)	(0.0406)	(0.0476)
Month −14	-0.0031	0.0387	-0.0308	-0.0281
	(0.0309)	(0.0340)	(0.0370)	(0.0429)
Month −13	0.0124	0.0406	-0.0037	0.0083
	(0.0310)	(0.0342)	(0.0365)	(0.0418)
Month −12	0.0110	0.0308	-0.0426	-0.0415
	(0.0333)	(0.0374)	(0.0402)	(0.0471)
Month −11	0.0535*	0.0953***	-0.0575	-0.0929**
	(0.0318)	(0.0354)	(0.0357)	(0.0416)
Month −10	0.0425	0.0531	-0.0107	-0.0141
	(0.0340)	(0.0379)	(0.0357)	(0.0432)
Month −9	0.0125	0.0439	-0.0410	-0.0367
	(0.0305)	(0.0332)	(0.0373)	(0.0444)
Month −8	0.0251	0.0464	-0.0125	-0.0309
	(0.0299)	(0.0330)	(0.0417)	(0.0524)
Month −7	0.0079	0.0317	-0.0460	-0.0452
	(0.0293)	(0.0328)	(0.0368)	(0.0423)
Month −6	0.0296	0.0588*	-0.0536	-0.0709
	(0.0315)	(0.0356)	(0.0394)	(0.0457)
Month −5	-0.0310	0.0024	-0.0162	-0.0431
	(0.0304)	(0.0336)	(0.0405)	(0.0471)
Month −4	-0.0355	-0.0103	-0.0088	-0.0581
	(0.0314)	(0.0361)	(0.0417)	(0.0492)
Month −3	-0.0443	0.0083	0.0256	0.0026
	(0.0299)	(0.0321)	(0.0376)	(0.0447)
Month −2	-0.0282	0.0137	0.0213	-0.0073
	(0.0303)	(0.0324)	(0.0361)	(0.0409)
Month −1 (Reference)	Omitted	Omitted	Omitted	Omitted
Observations	157,998	144,636	138,645	132,255
Joint test of pre-policy coefficients				
χ^2	31.12	27.34	17.20	14.91
Degrees of freedom	17	17	16	16
p-value	0.0193	0.0532	0.3726	0.5313

Notes: PPML event-study estimates of teen live-birth conception rates. Columns distinguish overall Title X access and Planned Parenthood (PP) exposure. Month −1 is the reference period. All models include county and month-year fixed effects, unemployment controls, and a female population offset. County-clustered standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Event-Study Estimates of Title X Policy Changes: Post-Policy Trends in Teen Live-Birth Conception Rates

	2019 Final Rule		2021 Restoration	
Event Month	(1) Overall	(2) PP Exposure	(3) Overall	(4) PP Exposure
Month 0	0.0250	0.0569	-0.0839**	-0.0862*
	(0.0316)	(0.0346)	(0.0401)	(0.0472)
Month +1	0.0298	0.0554	-0.0260	-0.0809
	(0.0317)	(0.0342)	(0.0419)	(0.0500)
Month +2	-0.0003	0.0343	-0.0584	-0.0787*
	(0.0319)	(0.0343)	(0.0377)	(0.0424)
Month +3	-0.0506*	-0.0150	-0.0758*	-0.0984*
	(0.0299)	(0.0321)	(0.0417)	(0.0509)
Month +4	-0.0496	-0.0184	-0.0106	-0.0085
	(0.0312)	(0.0330)	(0.0424)	(0.0513)
Month +5	-0.0107	0.0207	-0.0161	-0.0110
	(0.0334)	(0.0366)	(0.0364)	(0.0435)
Month +6	-0.0226	-0.0043	-0.0287	-0.0289
	(0.0301)	(0.0329)	(0.0428)	(0.0506)
Month +7	-0.0538*	-0.0387	-0.0049	-0.0404
	(0.0311)	(0.0350)	(0.0417)	(0.0446)
Month +8	-0.0597	-0.0267	0.0609	0.0501
	(0.0378)	(0.0440)	(0.0398)	(0.0467)
Month +9	-0.0084	0.0129	0.0505	0.0591
	(0.0350)	(0.0372)	(0.0380)	(0.0437)
Month +10	-0.0279	0.0033	-0.0154	-0.0515
	(0.0310)	(0.0329)	(0.0395)	(0.0476)
Month +11	0.0295	0.0523	-0.0686	-0.0879*
	(0.0317)	(0.0352)	(0.0440)	(0.0521)
Month +12	0.0424	0.0680*	0.0101	0.0002
	(0.0339)	(0.0377)	(0.0337)	(0.0393)
Month +13	0.0362	0.0353	-0.0243	-0.0412
	(0.0317)	(0.0345)	(0.0374)	(0.0436)
Month +14	0.0141	0.0413	-0.0235	-0.0386
	(0.0336)	(0.0379)	(0.0405)	(0.0478)
Month +15	-0.0148	0.0146	-0.0310	-0.0251
	(0.0322)	(0.0364)	(0.0408)	(0.0471)
Month +16	-0.0206	-0.0037	0.0233	-0.0075
	(0.0307)	(0.0331)	(0.0358)	(0.0422)
Month +17	-0.0656**	-0.0417	-0.0062	-0.0312
	(0.0325)	(0.0361)	(0.0389)	(0.0487)
Month +18	-0.0103	0.0195	—	—
	(0.0251)	(0.0274)		
Observations	157,998	144,636	138,645	132,255
County fixed effects	Yes	Yes	Yes	Yes
Month-year fixed effects	Yes	Yes	Yes	Yes
Population offset	Yes	Yes	Yes	Yes

Notes: PPML event-study estimates of teen live-birth conception rates. Event month 0 denotes July 2019 or November 2021. Month -1 is the omitted reference period. Columns distinguish overall Title X access and Planned Parenthood (PP) exposure. Models include county and month-year fixed effects, unemployment controls, and a female population offset. County-clustered standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

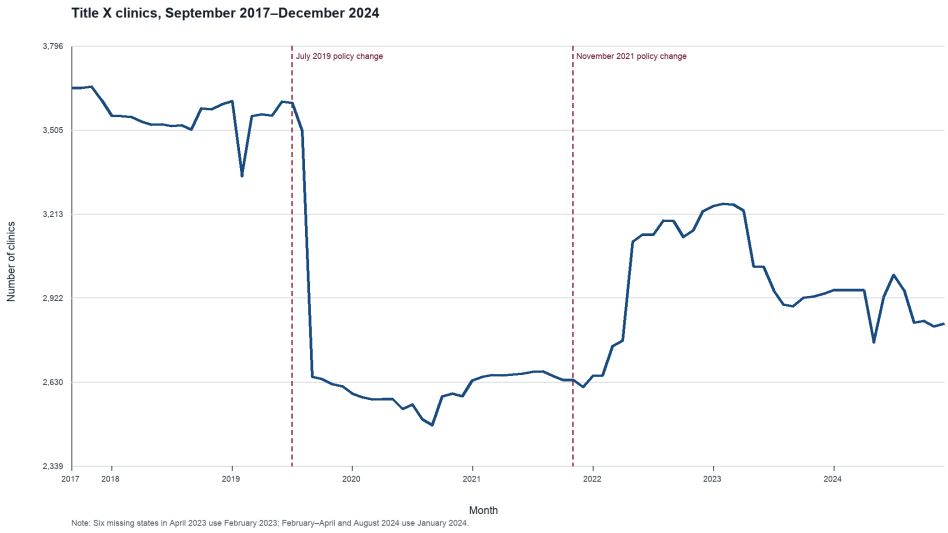


Figure 3: Trend of Monthly Number of Title X Clinics

Notes: The figure plots the monthly number of active Title X clinics nationwide from September 2017 through December 2024. Clinic information is obtained from the Office of Population Affairs (OPA) Title X Clinic Locator and Directory and compiled into a monthly clinic-level dataset. The dashed vertical lines indicate the implementation of the 2019 Title X Final Rule (July 2019) and the restoration of Title X regulations (November 2021). Temporary data gaps in several states were addressed by carrying forward the nearest available monthly clinic listings, as described in the data construction process.

Table 8: Robustness Check: Regression Discontinuity Estimates with County Fixed Effects

	July 2019 Cutoff			November 2021 Cutoff		
	±24 m	±18 m	±12 m	±24 m	±18 m	±12 m
Running variable	-0.0093***	-0.0045**	-0.0058*	-0.0057***	-0.0059*	-0.0130**
	(0.0015)	(0.0018)	(0.0034)	(0.0015)	(0.0034)	(0.0057)
Post-policy	-0.0335	-0.1132***	-0.1007***	0.0002	0.0088	0.0438
	(0.0237)	(0.0255)	(0.0330)	(0.0270)	(0.0330)	(0.0416)
Running × Post	0.0050***	0.0084***	0.0086	0.0003	0.0000	0.0075
	(0.0019)	(0.0026)	(0.0054)	(0.0019)	(0.0037)	(0.0062)
Unemployment rate	-0.0155***	-0.0211***	-0.0191***	-0.0129***	-0.0103	-0.0131
	(0.0029)	(0.0030)	(0.0039)	(0.0031)	(0.0070)	(0.0167)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth (months)	24	18	12	24	18	12

Notes: Each column reports a separate regression discontinuity specification estimated using county-level monthly data. All regressions include county fixed effects and cluster standard errors at the county level. The running variable is the number of months relative to the policy implementation date. The post-policy indicator equals one for months after the policy cutoff. Narrower bandwidths are reported as robustness checks.

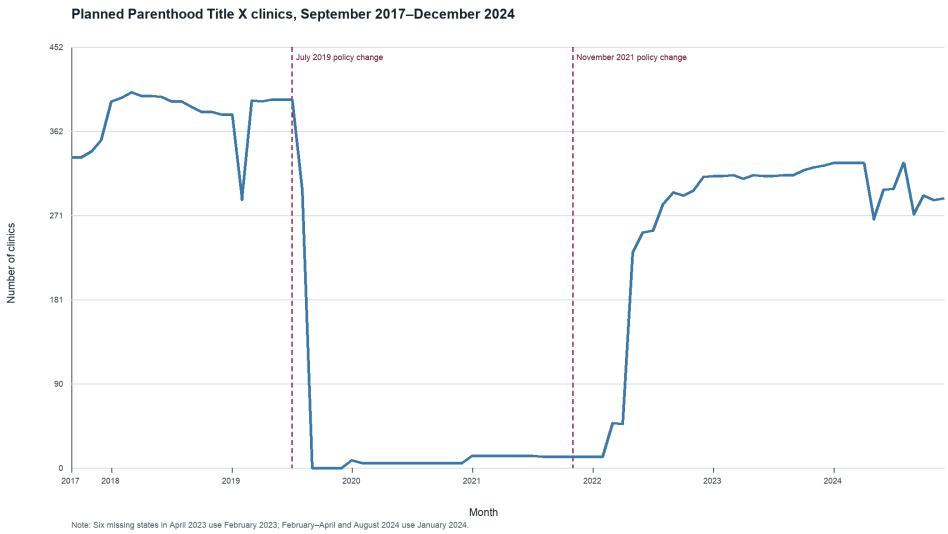


Figure 4: Trend of Monthly Number of Planned Parenthood Title X Clinics

Notes: The figure plots the monthly number of active Planned Parenthood clinics participating in the Title X program from September 2017 through December 2024. Clinic information is obtained from the Office of Population Affairs (OPA) Title X Clinic Locator and Directory. Planned Parenthood clinics are identified by searching the clinic name for the term "Planned Parenthood" and aggregating the identified clinics to the monthly national level. The dashed vertical lines indicate the implementation of the 2019 Title X Final Rule (July 2019) and the restoration of Title X regulations (November 2021).

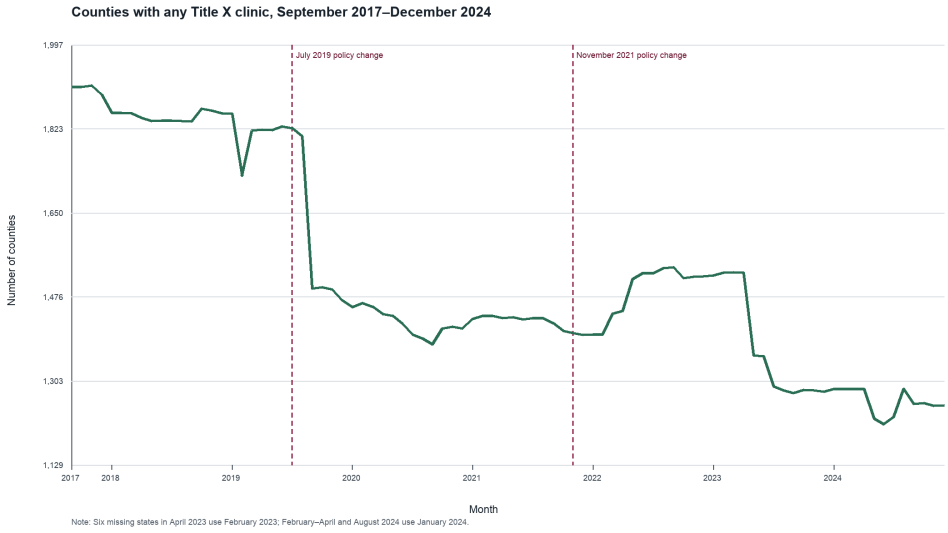


Figure 5: Trend of Monthly Number of Counties with any Title X Clinic

Notes: The figure plots the monthly number of U.S. counties containing at least one active Title X clinic from September 2017 through December 2024. Clinic information is obtained from the Office of Population Affairs (OPA) Title X Clinic Locator and Directory. A county is classified as having Title X availability if at least one active Title X clinic is located within the county in a given month. The dashed vertical lines indicate the implementation of the 2019 Title X Final Rule (July 2019) and the restoration of Title X regulations (November 2021).

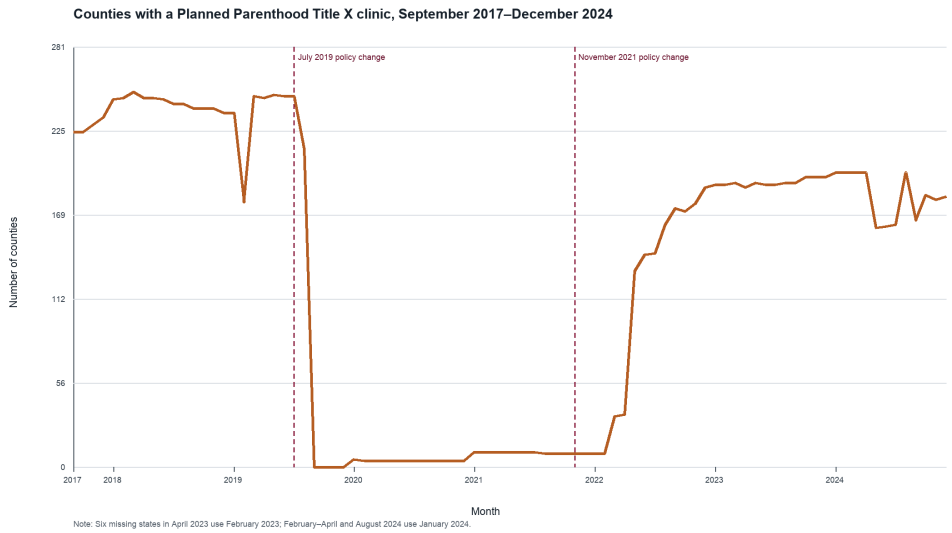


Figure 6: Trend of Monthly Number of Counties with any Planned Parenthood Title X Clinic

Notes: The figure plots the monthly number of U.S. counties containing at least one active Planned Parenthood clinic participating in the Title X program from September 2017 through December 2024. Clinic information is obtained from the Office of Population Affairs (OPA) Title X Clinic Locator and Directory. Planned Parenthood clinics are identified based on whether the clinic name contains the term "Planned Parenthood." A county is classified as having Planned Parenthood Title X availability if at least one active Planned Parenthood Title X clinic is located within the county in a given month. The dashed vertical lines indicate the implementation of the 2019 Title X Final Rule (July 2019) and the restoration of Title X regulations (November 2021).

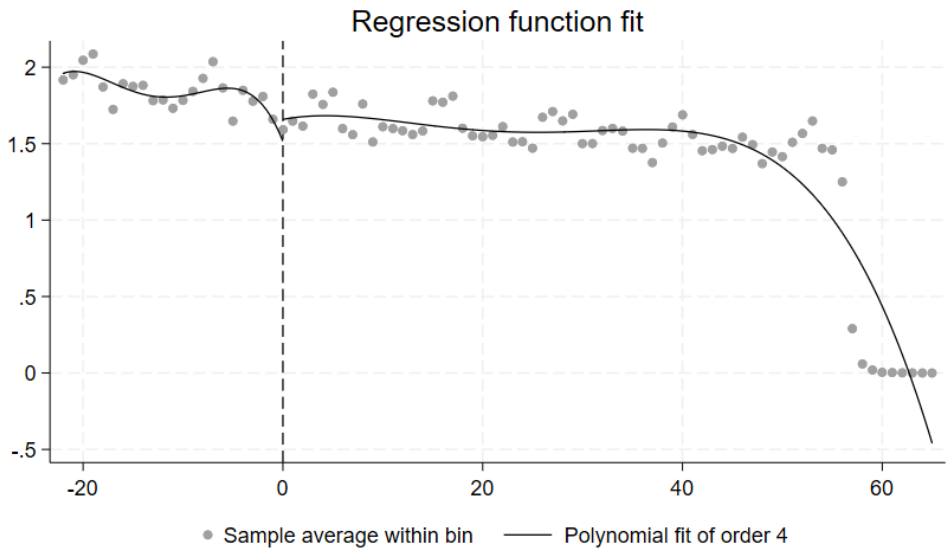


Figure 7: Regression Discontinuity Plot at the July 2019 Title X Final Rule Cutoff

Notes: The figure illustrates the relationship between the county-level teen birth rate (per 1,000 females aged 15–19) and time relative to the implementation of the July 2019 Title X Final Rule. The vertical dashed line marks the policy cutoff in July 2019. Gray dots represent average outcomes within bins of the running variable, while the solid curves display fourth-order polynomial fits estimated separately on either side of the cutoff for visualization purposes. The main RD estimates are obtained using local linear regressions, with the mean squared error (MSE)-optimal bandwidth selected using the Calonico–Cattaneo–Titiunik (CCT) procedure implemented in `rdrobust`. The fitted curves shown in the figure are descriptive and do not represent the local linear estimates reported in the regression tables.

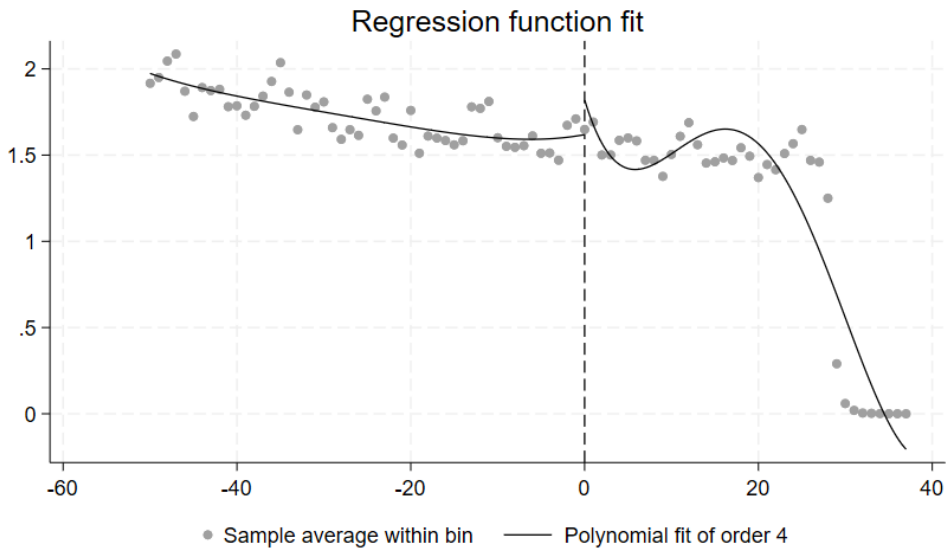


Figure 8: Regression Discontinuity Plot at the November 2021 Title X Restoration Cutoff

Notes: The figure illustrates the relationship between the county-level teen birth rate (per 1,000 females aged 15–19) and time relative to the implementation of the November 2021 Title X Rule Restoration. The vertical dashed line marks the policy cutoff in November 2021. Gray dots represent average outcomes within bins of the running variable, while the solid curves display fourth-order polynomial fits estimated separately on either side of the cutoff for visualization purposes. The main RD estimates are obtained using local linear regressions, with the mean squared error (MSE)-optimal bandwidth selected using the Calonico–Cattaneo–Titiunik (CCT) procedure implemented in `rdrobust`. The fitted curves shown in the figure are descriptive and do not represent the local linear estimates reported in the regression tables.

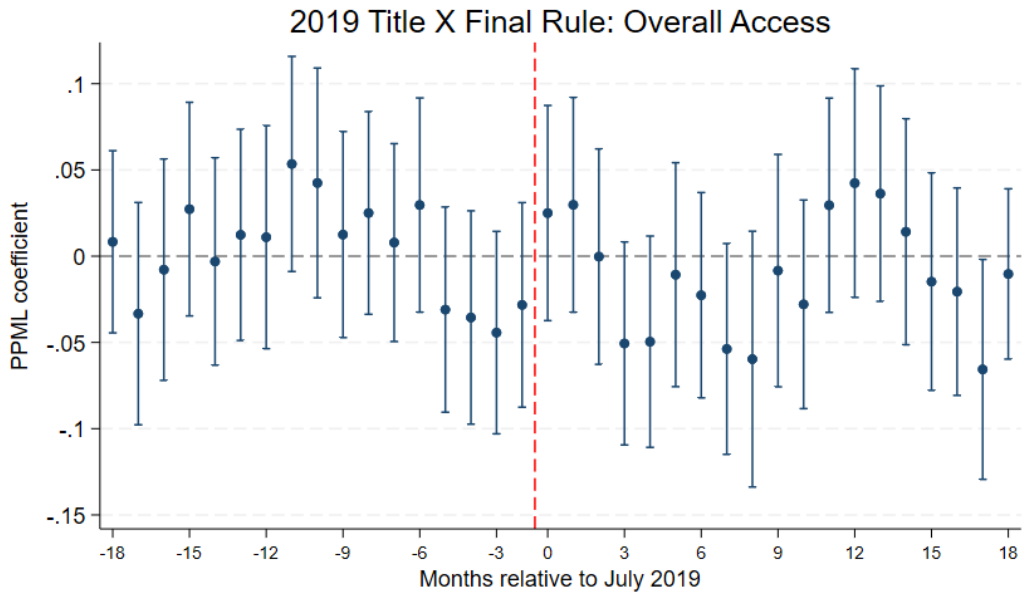


Figure 9: Event-Study Estimates of the July 2019 Title X Final Rule: Overall Title X Clinic Access

Notes: This figure presents monthly event-study estimates of the association between the July 2019 Title X Final Rule and county-level teen births. The estimates are obtained from Poisson Pseudo-Maximum Likelihood (PPML) regressions, with the number of teen conceptions as the dependent variable and the logarithm of the female population aged 15–19 included as an offset.
Treated counties are those that experienced an increase in distance to the nearest active Title X clinic between June and September 2019. The comparison group consists of counties without an increase in distance.
The horizontal axis indicates months relative to July 2019, with June 2019 (month -1) omitted as the reference period. The vertical dashed line marks the policy cutoff. Points represent estimated event-time coefficients, and vertical bars indicate 95% confidence intervals.
All specifications include county and month-year fixed effects and control for county-level unemployment rates. Standard errors are clustered at the county level. Event months outside the interval $[-18, 18]$ are grouped into the corresponding endpoint categories.

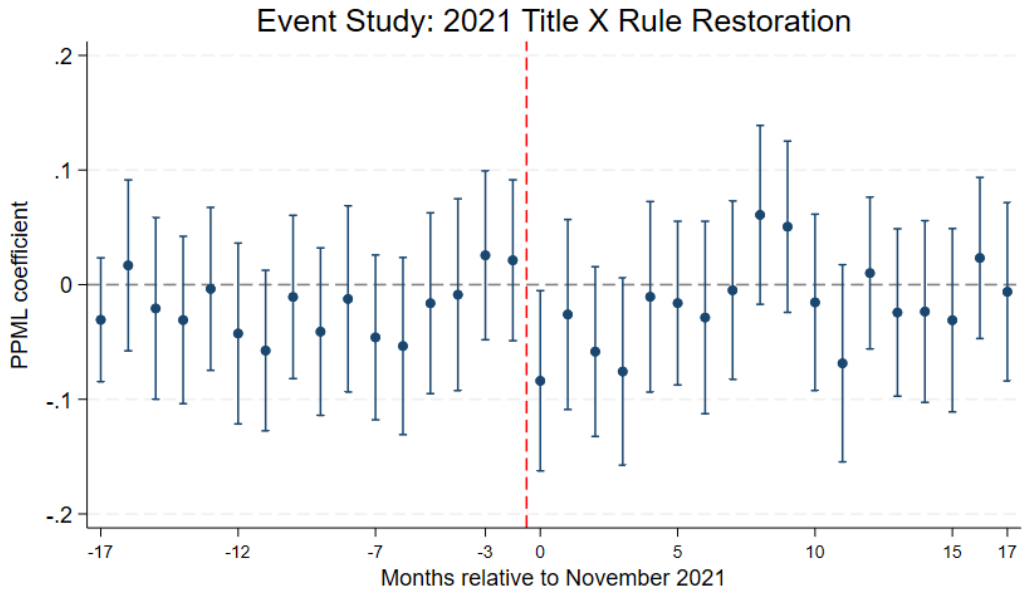


Figure 10: Event-Study Estimates of the November 2021 Title X Policy Restoration: Overall Title X Clinic Access

Notes: This figure presents monthly event-study estimates of the association between the November 2021 Title X policy restoration and county-level teen conceptions. Estimates are obtained using Poisson Pseudo-Maximum Likelihood (PPML) regression, with the number of teen conceptions as the dependent variable and the logarithm of the female population aged 15–19 included as an offset. Treated counties are those that experienced a decrease in distance to the nearest active Title X clinic between October 2021 and May 2022. The comparison group consists of counties without a decrease in distance.

The horizontal axis represents months relative to November 2021, with October 2021 (month -1) omitted as the reference period. The vertical dashed line indicates the November 2021 policy cutoff. Points represent estimated event-time coefficients, while vertical bars indicate 95% confidence intervals.

The specification includes county and month-year fixed effects and controls for county-level unemployment rates. Standard errors are clustered at the county level. Event months outside the interval $[-17, 17]$ are grouped into the corresponding endpoint categories.

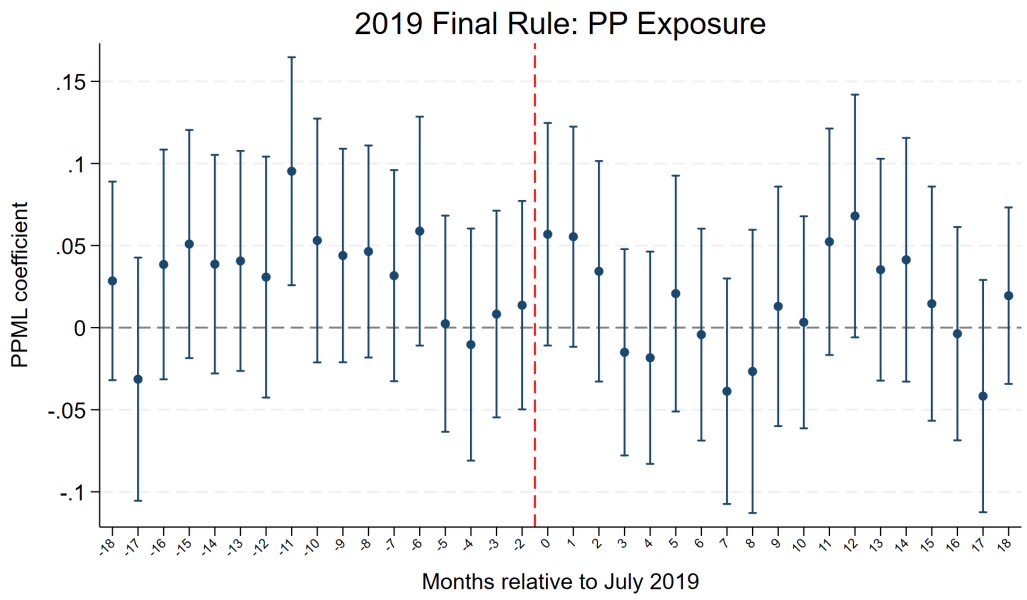


Figure 11: Event-Study Estimates of the July 2019 Title X Final Rule: Planned Parenthood Clinic Access

Notes: This figure presents monthly event-study estimates of the association between the July 2019 Title X Final Rule and county-level teen conceptions among counties classified according to changes in Planned Parenthood (PP) Title X exposure. Estimates are obtained using Poisson Pseudo-Maximum Likelihood (PPML) regression, with the number of teen conceptions as the dependent variable and the logarithm of the female population aged 15–19 included as an offset.
Treated counties are those that experienced an increase in distance to the nearest active Title X clinic between June and September 2019 and had PP Title X exposure in June 2019. PP exposure is defined as having at least one active PP Title X clinic within the county or, for counties without an in-county Title X clinic, having PP as the nearest active Title X provider. The comparison group consists of counties whose distance to the nearest active Title X clinic remained unchanged.
The horizontal axis represents months relative to July 2019, with June 2019 (month -1) omitted as the reference period. The vertical dashed line indicates the policy cutoff. Points represent estimated event-time coefficients, and vertical bars indicate 95% confidence intervals.
The specification includes county and month-year fixed effects and controls for county-level unemployment rates. Standard errors are clustered at the county level. Event months outside the interval $[-18, 18]$ are grouped into the corresponding endpoint categories.

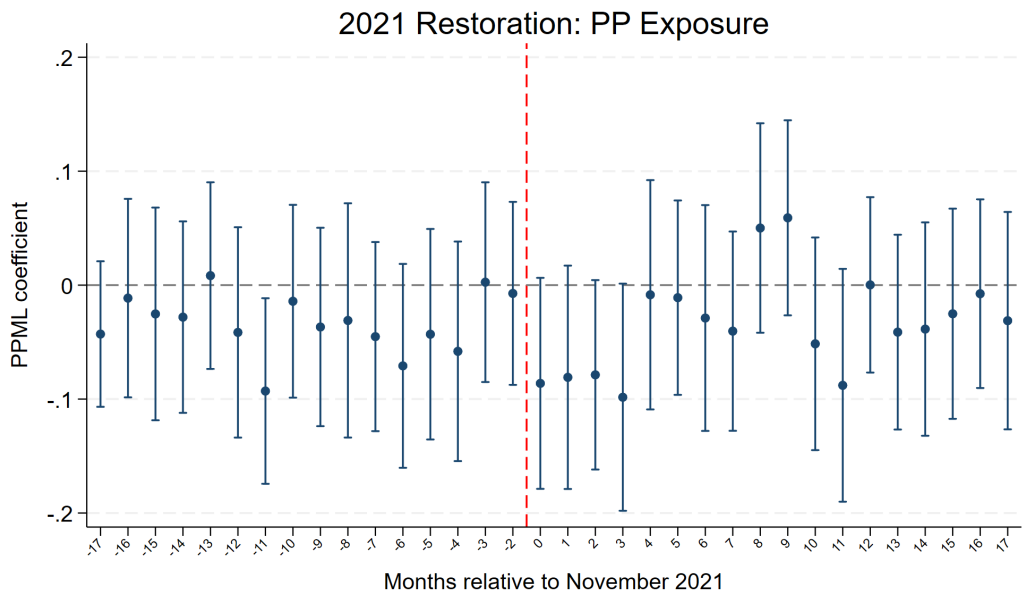


Figure 12: Event-Study Estimates of the November 2021 Title X Policy Restoration: Planned Parenthood Clinic Access

Notes: This figure presents monthly event-study estimates of the association between the November 2021 Title X policy restoration and county-level teen conceptions among counties classified according to changes in Planned Parenthood (PP) Title X exposure. Estimates are obtained using Poisson Pseudo-Maximum Likelihood (PPML) regression, with the number of teen conceptions as the dependent variable and the logarithm of the female population aged 15–19 included as an offset.
Treated counties are those that experienced a decrease in distance to the nearest active Title X clinic between October 2021 and May 2022 and had PP Title X exposure in May 2022. PP exposure is defined as having at least one active PP Title X clinic within the county or, for counties without an in-county Title X clinic, having PP as the nearest active Title X provider. The comparison group consists of counties whose distance to the nearest active Title X clinic remained unchanged.
The horizontal axis represents months relative to November 2021, with October 2021 (month -1) omitted as the reference period. The vertical dashed line indicates the policy cutoff. Points represent estimated event-time coefficients, and vertical bars indicate 95% confidence intervals.
The specification includes county and month-year fixed effects and controls for county-level unemployment rates. Standard errors are clustered at the county level. Event months outside the interval $[-17, 17]$ are grouped into the corresponding endpoint categories.

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