SOCIAL SCIENCES

The impact of contraceptive access on high school graduation

Amanda J. Stevenson[1,2]*, Katie R. Genadek[2,3], Sara Yeatman[2,4], Stefanie Mollborn[1,2], Jane A. Menken[2]

Does access to the full range of contraceptive methods increase young women's educational attainment? Family planning programs are often justified by claims that it does, but contemporary evidence is unexpectedly weak. We use a natural experiment afforded by a 2009 Colorado policy change to assess the impact of expanded access to contraception on women's high school graduation. Linking survey and Census data, we follow a population-representative U.S. sample, including large subsamples of young women living in Colorado in 2010 and in comparison states. Using a difference-in-differences design, we find expansion of access to contraception was associated with a statistically significant 1.66 percentage-point increase in high school graduation. This increase in graduation represents a 14% decrease in the baseline percentage not graduating high school before the policy change. Results are robust to a variety of sensitivity tests. Our findings indicate that improving access to contraception increases young women's human capital formation.

INTRODUCTION

In her dissent in *Gonzales* v. *Carhart* (2007), Justice Ruth Bader Ginsburg wrote that women's "ability to realize their full potential … is intimately connected to 'their ability to control their reproductive lives,'" quoting *Casey* v. *Planned Parenthood* (1992). This claim, that access to contraception has beneficial indirect effects, including improving women's educational attainment, is also used as rationale to support the U.S. Title X program, which funds access to sexual and reproductive health services for low-income and uninsured residents (*1*, *2*). While arguments for access to contraception are based on a host of principles, including justice (*3*), human rights (*4*), the promotion of women's health (*5*), and the right to privacy (*6*), justifications for publicly funded family planning programs in the U.S. often rely on a narrow and testable version of Ginsburg's claim: that access to contraception has beneficial indirect effects, including improving women's educational attainment (*1*, *2*). Despite the ubiquity of this claim, there is unexpectedly little rigorous contemporary evidence to support it, so arguments in support of Title X in particular and publicly subsidized family planning in general have rested on the observation that young women who bear children as teenagers are less likely to complete high school (*2*, *7*, *8*). This reliance weakens the rationale for funding because it rests a causal argument about the impact of access to contraception on partial evidence (*2*, *8*–*12*). It assumes that when contraceptive access improves, a significant population of women will increase their contraceptive use and thereby avert teen births, which would otherwise have prevented them from graduating high school. Decades of evidence indicate that women who have teen births and subsequently do not graduate high school are disproportionately subject to social and economic disadvantage before they have children (*7*, *13*, *14*), so their nongraduation may not have been caused by their childbearing.

This reliance on evidence about the impact of teen births also assumes that the only pathway between access to contraception and human capital is through fertility. Yet the capacity to reliably plan one's childbearing may itself increase human capital formation even among young women who would not otherwise have ended up having teen births. Alternative causal pathways have been hypothesized to operate through increased confidence that investments in education will yield benefits, mental health improvements associated with control over fertility, or labor market changes associated with employers' reduced concern that young women will leave work due to unplanned or unwanted births (*15*–*17*). For these reasons, directly assessing the relationship between access to contraception and education is essential both for understanding human capital formation and for providing a more robust scientific evidence base for policy.

Stronger evidence establishing the impact of access to contraception on subsequent outcomes requires both an identifiable change in access to contraception and data over time for a sufficiently large representative sample of individuals exposed and not exposed to that change. In the United States, family planning policy changes most often occur at the state level. But individual longitudinal samples are typically available only for nationally representative cohorts, which are not representative at the state level. Our study overcomes these hurdles by harnessing an abrupt expansion of access to contraception in Colorado and restricted individually linked survey and decennial census data that are sufficiently large for state-specific analysis.

The initial introduction of oral contraceptive pills in the 1960s and the advent of the federal Title X family planning funding program in the 1970s provided the most recent nationwide changes in family planning access. These expansions, 50 years ago and more, affected women's fertility, education, and employment (*15*–*18*), but the question of the effect of introducing contraception where no modern methods had previously been available is not analogous to the question of whether changes to existing modern family planning programs affect women's lives. In addition, historical evaluations do not identify how impacts of policies vary by race/ethnic categories in the contemporary context. The inability to disaggregate

[1]Department of Sociology, University of Colorado Boulder, Boulder, CO 80309, USA. [2]Institute of Behavioral Science, University of Colorado Boulder, Boulder, CO 80309, USA. [3]U.S. Census Bureau, Suitland-Silver Hill, MD 20746, USA. [4]Department of Health and Behavioral Sciences, University of Colorado Denver, Denver, CO 80217, USA.
*Corresponding author. Email: amanda.stevenson@colorado.edu

effects by race and ethnicity leaves programs open to critiques that they are racist and antinatalist. Concerns about racist contraceptive coercion are particularly salient to family planning programs that modify access to permanent or long-acting reversible contraceptive (LARC) methods (*11*). Overall, the absence of evidence leaves a dominant rationale for the public subsidy of contraception untested at a time when funding for and scope of family planning programs are increasingly contested.

Title X grantees are required to offer a broad range of Food and Drug Administration (FDA)–approved contraceptive methods, but insufficient levels of funding have led to rationing of more expensive methods (e.g., intrauterine devices and implants), leaving unmet demand for these more effective methods (*19*, *20*). The Colorado Family Planning Initiative (CFPI), introduced late in 2009, abruptly expanded contraceptive access by providing funding that made every FDA-approved contraceptive method available to every client in every Colorado Title X family planning clinic at low or no cost (*21*). In addition, it provided training in the administration of specialized methods (particularly LARCs) for Colorado Title X providers and a family planning social marketing campaign (*21*). The peak period of CFPI implementation (2009–2014) saw an increase of 16.5 percentage points in utilization of LARCs (*21*) and saw fertility and abortion rates fall for 15- to 24-year-olds (*22*–*24*). CFPI, a statewide program, definitively demonstrated that increasing Title X funding and capacity at the population level was followed by increased use of the most expensive and most effective contraceptive methods and declines in fertility and abortion rates (*22*–*24*). It confirms earlier evidence from clinical-scale interventions (*25*–*27*) that has been considered biased because it was based only on women who self-selected into presenting for services rather than all women who could have accessed services.

We build on this research by assessing the impact of CFPI on receipt of high school diplomas by young women who lived in Colorado at the time of CFPI onset. We focus on receipt of a high school diploma by young adulthood (ages 20 to 22) because of its known association with improved later life outcomes (*28*, *29*) and because this is where early impacts on life course development of human capital may first be apparent. Our approach yields an estimate of the impact of CFPI on high school graduation among young women who lived in Colorado at the time of program implementation, thus generating evidence of the impact of the program on high school graduation at the population level. This approach is agnostic regarding mechanisms through which access to contraception shapes human capital. Taking an intent-to-treat approach in which all Colorado-resident women at program outset are considered exposed to CFPI, we estimate statewide impacts, not impacts on women who sought Title X services. We first estimate the percentage point change in high school graduation, and then we assess the degree to which CFPI affected the adverse outcome (not graduating high school) by expressing the impact of CFPI as a percentage of baseline levels of nongraduation.

RESULTS

Difference-in-differences estimates of the effect of CFPI

To estimate change over time in women's high school graduation associated with exposure to CFPI, we use difference-in-differences (DID) to compare change between cohorts of Colorado residents and change between cohorts of residents of other states. Specifically, we calculate the percentage of women by cohort who had earned at least a high school diploma by the time they were observed at age 20 to 22 in the 2009 to 2017 American Community Survey (ACS). See Fig. 1 for an illustration of our cohorts. After linking ACS respondents to their 2010 Census responses, we compute these percentages separately for those who lived in Colorado at the 2010 Census and for those who lived in our 17 comparison parallel trends states: Those states that did not differ statistically in level or trend of high school diploma receipt in the period before CFPI and that, like Colorado, had expanded Medicaid by 2015. We then compute the difference in the percentage completing high school between the pre- and post-CFPI cohorts in Colorado and parallel trends states, the first differences. Subtracting the first difference in parallel trends states from the first difference in Colorado yields an estimate of additional change in Colorado or the DID. We calculate these percentages and differences for all women and separately for non-Hispanic White women and Hispanic women. Because Colorado has relatively small Black and Asian populations, the ACS samples for these groups were not large enough to yield stable estimates. As shown in table S2, our Colorado cohorts are only 3.35% Black and 2.56% Asian.

Table 1 displays percentages of women aged 20 to 22 with a high school diploma or greater (high school graduation) by 2010 residence, cohort, and race/ethnicity. It also displays the difference between post-CFPI and pre-CFPI cohorts in these percentages and the DID. For all groups, Colorado residents experienced significantly greater gains in the percent graduating high school than the comparison group. Compared with parallel trends states, the Colorado difference is 1.66 percentage points greater for all women (from an initial Colorado level of 88.15%, $P = 0.001$), 2.16 percentage points greater for non-Hispanic White women (from an initial Colorado level of 92.26%, $P < 0.001$), and 4.86 percentage points greater for Hispanic women (from an initial Colorado level of 77.71%, $P < 0.001$).

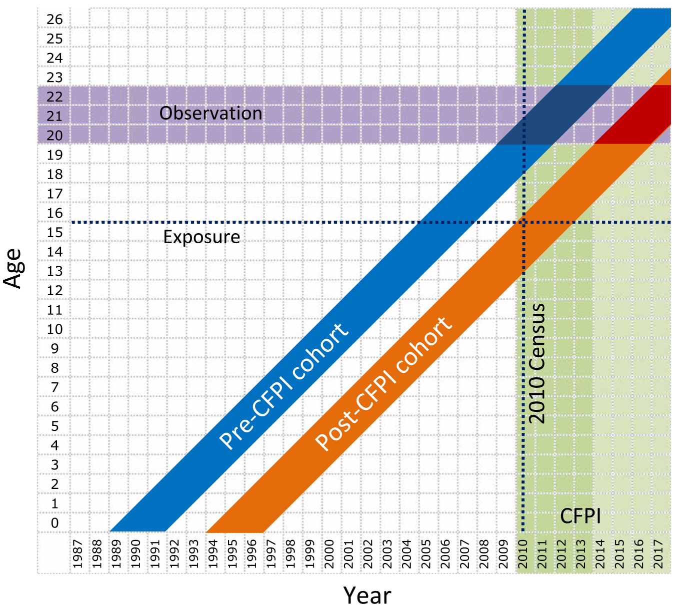

Fig. 1. Exposure to the CFPI by birth cohort.

Table 1. Percentage with high school diploma by 2010 residence, cohort, and race/ethnicity: women aged 20 to 22. Source: 2010 Decennial Census and 2009 through 2017 1-year ACS data. Results were approved for release by the U.S. Census Bureau's Disclosure Review Board, authorization number CBDRB-FY20-ERD002-010. *P* value indicates statistical significance of DID estimates reported based on significance of two-way interaction between Colorado residence in 2010 and being in the post-CFPI cohort in regression models including state fixed effects and clustered SEs.

	Pre-CFPI cohort		Post-CFPI cohort		Difference (Post-pre)	DID (Colorado difference–comparison difference)	*P* value
	Percent	SE	Percent	SE			
COLORADO							
All women	88.15	0.7738	92.10	0.7667	3.95		
White, non-Hispanic	92.26	0.7367	95.28	0.6206	3.02		
Hispanic	77.71	2.086	87.24	2.044	9.53		
PARALLEL TRENDS STATES							
All women	90.70	0.1873	92.99	0.245	2.29	1.66	0.001
White, non-Hispanic	93.35	0.1206	94.21	0.1623	0.86	2.16	<0.001
Hispanic	85.71	0.2956	90.38	0.3661	4.67	4.86	<0.001

In addition to our main comparison described above, we also compared Colorado with all non-Colorado U.S. states and with states with similar trends in graduation among 20- to 22-year-old women in the pre-CFPI period but lower initial levels of graduation, providing a comparison group with higher potential growth in the outcome. Results of these comparisons are presented in table S3. Both yield DID estimates of CFPI's effect that are smaller than those generated in the comparison to our parallel trends states, but direction and significance of the estimated effect of CFPI are the same.

Magnitude of the effect of CFPI

Populations with higher baseline levels of nongraduation from high school require greater absolute declines in nongraduation to achieve full graduation. For that reason, Fig. 2 illustrates the magnitude of the estimated decline due to CFPI, or the DID estimate, as a percentage of the pre-CFPI cohort fraction without high school graduation. It illustrates this separately for all women, non-Hispanic White women, and Hispanic women. We estimate that CFPI reduced the percentage of Colorado women without a high school diploma by 14.0% for all women, by 27.9% for non-Hispanic White women, and by 21.8% for Hispanic women.

The post-CFPI Colorado cohort population (female Colorado residents born 1994 to 1996 in the 2010 Census) comprised approximately 97,500 women. Applying the effects shown in Table 1, we estimate that CFPI resulted in an additional 3800 Colorado women born 1994 to 1996 receiving high school diplomas by age 20 to 22.

Robustness of findings

In addition to the models using the two alternative comparison groups discussed above, we further tested the robustness of our findings using four types of sensitivity tests, a falsification test, and an alternative analytic design, all of which yielded results similar to the primary analyses. (See the Supplementary Materials for more information.) First, to test whether results were sensitive to model specification, we performed our analyses with a variety of DID model estimation strategies. Second, we tested whether results were sensitive to the definition of our outcome by varying whether we considered holders of General Educational Development (GED) certificates to be high school graduates. Third, we assessed whether our reliance on linked data introduced bias by performing a variety of alternative analyses for which no linkage was required. Fourth, we performed our analysis with a variety of additional alternative comparison groups to assess further whether results were sensitive to our selection of comparison states. We conducted a falsification test by shifting the entire analysis back in time to assure that no effect is identified before CFPI could have had an impact. Last, we tested whether our results were robust to an alternative analytic design by performing synthetic control analyses.

DISCUSSION

That family planning programs reduce fertility is well-established. This fact, however, is insufficient as evidence that family planning programs positively affect women's socioeconomic opportunities. We now provide that crucial evidence. Before CFPI, Colorado residents had access to standard family planning services through the Title X clinic network, and high school graduation among young women was near 90%. Even in this context, improving access to the full range of contraceptive methods reduced young women's fertility (*22*–*24*). We show that CFPI is also associated with significant population-level improvement in high school graduation among young women in Colorado. This improvement is important: Failure to graduate from high school sets individuals on a path of reduced lifetime educational attainment that has become increasingly associated with poor life chances, driving inequalities in lifetime earnings (*30*, *31*) and mortality (*28*, *32*).

Our research improves on earlier approaches to examining the impact of access to contraception on young women's lives. Studies of fertility outcomes have usually compared outcomes in nonrepresentative clinical samples of individuals who self-select into contraceptive use instead of the population-representative sample we have constructed (*25*, *27*). Studies assessing impacts on educational attainment have usually used ecological comparisons in which exposures and outcomes are measured simultaneously (*33*). We improve on these

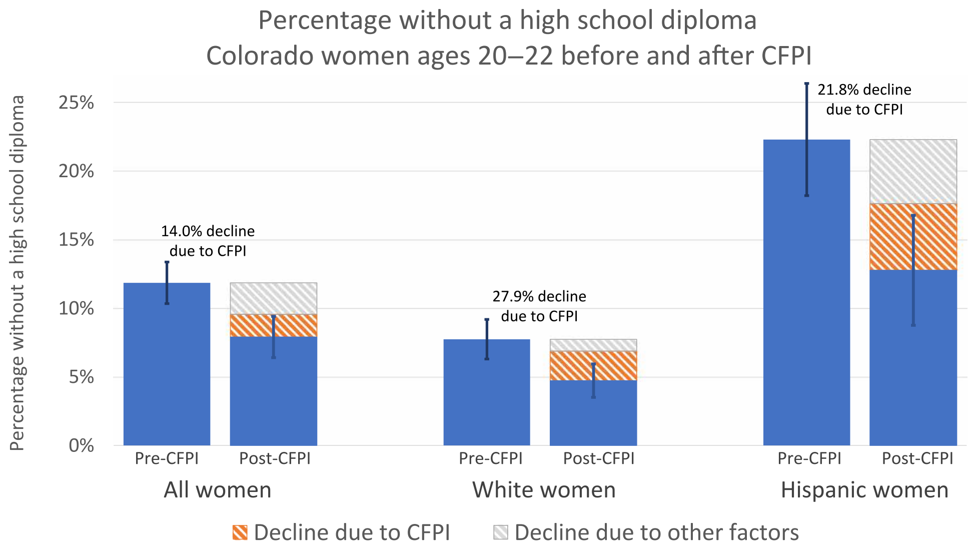

Fig. 2. Magnitude of effect of CFPI. All estimates from individual-level models for the DID in high school graduation among Colorado women. Blue lines indicate 95% confidence intervals around point estimates. Source: 2010 Decennial Census and 2009 through 2017 1-year ACS data. Results were approved for release by the U.S. Census Bureau's Disclosure Review Board, authorization number CBDRB-FY20-ERD002-010.

kinds of studies by using linked 2010 Census and ACS data, which enable us to measure a woman's exposure to the program based on her place of residence at program outset and allow us to observe educational outcomes years after exposure even when people moved.

Our results are robust to the extensive tests we conducted but are still limited. While CFPI was a particularly large and effective statewide intervention (*21*), Colorado has a higher proportion of people of Hispanic ethnicity than the U.S. as a whole and lower proportions of other non-White race/ethnic groups, so our estimates of impact may not be generalizable to other states. In particular, our results cannot be generalized to describe possible impacts of programs like CFPI on Black or Asian populations or states with substantial populations from these groups. Like all DID analyses, our results rely on the assumption that Colorado would have followed a similar trajectory in young women's high school graduation as comparison places did. Our robustness analyses showed that our main results are not sensitive to our approach to selecting comparison states. Our reliance on linked data could introduce bias into our findings because some populations are more difficult to link across time. However, our sensitivity analyses using contemporaneous exposure and not relying on linkage generated similar results. Last, our analytic approach does not illuminate the pathways through which access to contraception affects high school graduation. While the effect may operate through decreased fertility, it may also operate through increased confidence that human capital investments will yield benefits and improved mental health associated with control over fertility (*15*, *17*).

Our study assessed the population-level impact of access to the full range of contraceptive methods in Title X clinics on educational attainment in the contemporary United States. It provides much-needed estimates of the effects of a contraceptive program expansion on education in the general population and the first estimates by race/ethnicity of the population-level socioeconomic impacts of increasing access to contraception. Funding for Title X has consistently fallen well below the level required to provide care to all who are in need (*34*, *35*). CFPI fully funded Colorado's Title X program with the goal of facilitating access to the full range of contraceptive methods for all clients. Our analysis demonstrates that this expansion of service intervention, designed to ensure that every Title X client could freely choose from all FDA-approved methods of contraception with minimal barriers to initiation, significantly increased statewide high school graduation among young women.

MATERIALS AND METHODS

Experimental design

Our DID design compares Colorado birth cohorts whose graduation could not and could have been affected by the policy and then compares this difference to the difference between the same birth cohorts in other states. It therefore provides estimates of the intent-to-treat impact of the program, describing the impact on the population at large—not just those who visited Title X clinics. To do this, we construct two cohorts of women who were aged 20 to 22 (2 to 4 years past the typical age of high school diploma receipt) when they responded to an ACS between 2009 and 2017. One cohort comprises women who were too old at the time of CFPI implementation to have had their high school graduation affected (pre-CFPI cohort: born 1989 to 1991), and a second cohort comprises women who were just young enough that CFPI could have affected their high school graduation if they resided in Colorado (post-CFPI cohort: born 1994 to 1996).

Data

Longitudinal data from a large enough sample of the U.S. population by state, including Colorado, are necessary to allow for analyses of long-term outcomes like educational attainment. Thus, we cannot rely on typical vital registration data, cross-sectional survey data, or small nationally representative longitudinal survey data. Instead, we used secondary restricted-use data from the U.S. Census Bureau.

Two datasets were used for the analyses: the 2010 Census and the 2009–2017ACS. The 2010 Census covers all residents of the United States in April 2010. Its questionnaire is similar to previous "short form" decennial census questionnaires; microdata include householder status, relationship to household head, age, race/ethnicity, sex, and latitude and longitude of residence. The ACS, which went into full production in 2006, interviews a probability sample of all U.S. residents in each calendar year. Fielded throughout the year, it surveys 2.5% of households annually, covering about 1.5% of that year's U.S. population. It is representative at the state level for single years and the county level for pooled year ranges. The ACS uses a questionnaire modeled after the "long form" of Census 2000. It provides rich household and individual-level demographic and socioeconomic information including education, employment, household structure, marriage, and fertility.

To construct our longitudinal dataset including outcomes from ACS responses and exposure from the 2010 Census response, we linked members of our ACS cohorts of interest to their individual 2010 Census responses via Census Bureau assigned individual-level Protected Identification Key (PIK). The Census Bureau uses personally identifying information to assign a unique identification number, or PIK, to improve data collection and measurement of the population. PIKs are assigned using probabilistic record linkage techniques through a process called the Person Identification Validation System, in which the identified information in the data is used to link individuals to an administrative records composite file (*36*). Respondents linked to the composite file are assigned a PIK from the file, and the personally identifying information is then removed. PIKs facilitate linkage to other files that have been assigned PIKs. The rates at which the files are assigned PIKs are generally high, with the 2010 Census having 91% of respondents with PIKs and the ACS files having around 94%. The PIK assignment is subject to error, but error is small (with observed error generally well below 1%) (*36*–*38*). However, failure to match is unequally distributed, because of established limitations of probabilistic record matching processes (*39*). See the "Robustness of Findings" section and the Supplementary Materials for more information on tests of sensitivity to match failure in our analysis.

After linking the 2010 Census and the ACS using the assigned PIKs, we checked the links by comparing basic demographic information available in the surveys. We removed duplicates and excluded respondents who had clearly erroneously been assigned the same PIK. In total, we linked 71.4% of the ACS respondents in our cohorts of interest living in any U.S. state at the time of ACS response to their 2010 Census response (N = 361,550 linked). (See Fig. 1 for the illustration of cohorts.) A similar percentage (72.2%) of ACS respondents living in Colorado at the time of ACS response was linked to their 2010 Census response.

All data we analyzed were de-identified before we gained access (*36*). These data were made available to us through an approved project at the U.S. Census Bureau, and all analyses were performed on secure Census Bureau servers. Our results were reviewed by Census Bureau staff to prevent unlawful disclosure of individual-level information, and all numbers and percentages are rounded in accordance with Census Bureau disclosure review guidelines. This study has been determined exempt from human subjects review by the University of Colorado Institutional Review Board. Researchers can access these data via the Federal Statistical Research Data Center system, as we did. Our study materials are available to approved researchers within the secure environment.

Our primary variables of interest were constructed as follows.

Highest educational attainment was reported in the ACS. Rather than capturing lifetime educational attainment, this outcome considers whether women attained high school graduation by young adulthood. We consider a woman a high school graduate if she had attained high school graduation or gone on to college. We do not consider respondents with GED certificates who had not attended college as high school graduates in our primary analyses because of evidence that their life outcomes are more similar to those who did not graduate (*29*, *40*). GED recipients who had not attended college were included as graduates in sensitivity analyses.

State of residence at the 2010 Census (April 2010) is used as a proxy for residence at CFPI onset (late 2009), an intent-to-treat definition of exposure.

Race and ethnicity were reported in the ACS. ACS respondents can select one or more races from check boxes, and there is a race write-in option. Hispanic ethnicity is a separate question with check boxes to indicate being of Hispanic origin or not. We coded our race and ethnicity variable to be the race of the respondents if non-Hispanic was selected, and Hispanic for respondents answering yes to the ethnicity question of any race. Individuals not selecting Hispanic and selecting more than one race were coded as "other." Our analyses focus on all women together, non-Hispanic White women, and Hispanic women because Colorado samples for other race/ethnic groups were too small to yield reliable results in pre-analysis power calculations. See table S2 for Colorado sample sizes by race/ethnicity.

Our treatment variable is exposure to CFPI before age 16, when a difference in contraceptive access could affect high school graduation, considering waiting time to conception and gestation. To operationalize this exposure, we identified each woman's state of residence in 2010 as above and constructed two cohorts of women defined by age at CFPI implementation (see Fig. 1):

1) The pre-CFPI cohort (blue diagonal): too old for CFPI to affect high school graduation. These women, born 1989–1991, were 18 to 20 at the end of 2009 and 20 to 22 in ACS 2009–2014.

2) The post-CFPI cohort (orange diagonal): earliest cohort whose high school graduation could have been affected by CFPI if Colorado residents. These women, born 1994–1996, were 13 to 15 at the end of 2009 and 20 to 22 in ACS 2014–2017.

Thus, our population of interest includes women in these cohorts who resided in a U.S. state during 2009 to 2017 and at the time of the 2010 Census (see Fig. 1). Our sample is all such women who were aged 20 to 22 when they responded to an ACS in 2009 to 2017 and who were matched to their 2010 Census response. In our main analysis, we restricted our sample to women who resided in Colorado or one of our parallel trends states at the 2010 Census.

Identification of parallel trends states

A key assumption of DID methods, our analytic approach, is parallel trends between the intervention group and the comparison group (*41*). DID methods also require similar trajectories in other salient policies. Therefore, we limited our parallel trends comparison group to states that met two criteria: the state had expanded Medicaid by 2015 (because Colorado expanded Medicaid under the Affordable Care Act during our study period), and its pre-CFPI state-specific levels and time trends in high school graduation did not differ significantly from Colorado's for all women, non-Hispanic White women, and Hispanic women. Our team also systematically reviewed Colorado education legislation from 2009 to 2017 to verify that no substantial policy changes occurred during the period of our study. We identified one policy, Colorado's extension of in-state tuition to undocumented students who graduated on or after 1 September 2013. However, the policy's impact appears to have been modest in size and concentrated among undocumented high school students who already had or would graduate high school (*42*).

We consider 2008 as our first pre-CFPI year because the ACS did not distinguish between GED and high school diplomas before 2008. Year 2012 is the last pre-CFPI year because women at ages 20 to 22 in that year were at least age 17 at CFPI's onset, and thus, with waiting time to conception and gestation, their on-time high school graduation experience (typically between the ages of 17 and 19) would have been subject to the pre-CFPI regime of access to contraception.

We used individual-level linear probability models (*43*) of high school graduation to assess differences in state level or trend in young women's high school graduation before CFPI. We estimated these models using 2008–2012 ACS microdata for the three race/ethnic groups of women at ages 20 to 22. All models include a constant term for Colorado residence, a Colorado linear slope, fixed effects (difference in level) for each other state and linear slopes (differences in slope) for each other state.

Seventeen states met the parallel trends and Medicaid expansion criteria: Arizona, California, Connecticut, Hawaii, Illinois, Iowa, Maryland, Massachusetts, Michigan, Minnesota, Montana, New Hampshire, New Mexico, New York, North Dakota, Pennsylvania, and Rhode Island. See table S1 for sample sizes for Colorado, parallel trends states, and all non-Colorado U.S. states. The annual estimated percentage with a high school diploma among women at ages 20 to 22 in these states in the aggregate is displayed in fig. S1. Note that the data in this figure and used in the models to identify comparison states are single-year ACS estimates for all women at ages 20 to 22 based on residence at time of survey. In our main analysis, we use linked ACS and decennial census data to classify individuals based on past residence and restrict our sample to specific birth cohorts.

By definition, no individual parallel trends state was statistically different from Colorado in level or trend, but in the aggregate, the group appears to display a higher initial level and greater increase during the pre-CFPI period when compared to Colorado. This is likely due to our reliance on statistical significance tests to exclude states individually; smaller states are less likely to be rejected because their samples in the ACS are smaller and thus their estimates are less precise. To address any concerns arising from this, we also constructed a "higher growth potential" comparison group relying on point estimates from the linear probability models described above instead of relying on statistical tests of difference. Specifically, these states were lower than Colorado at the start of the pre-CFPI period (as measured by intercept) and had slopes close to Colorado's within this period. We expect states selected in this way to provide a more conservative test of CFPI's effect because initial graduation rate may be negatively associated with potential for growth (because graduation cannot rise above 100%). We selected comparison groups separately for our three groups of women. The slopes were within 0.01 for all and non-Hispanic White women and within 0.04 for Hispanic women (because their small sample sizes yielded much more variable slope estimates). All non-Colorado states (i.e., the rest of the United States) comprise a third comparison group whose results are presented to illustrate robustness to alternative comparisons. Aggregate annual estimates for each of these alternative comparison groups are also displayed in fig. S1.

Statistical analysis

Our empirical approach is a DID comparison of the difference in the percentage of women who graduated high school between pre- and post-CFPI cohorts for women residing in Colorado at CFPI onset compared to the same difference for women living in other states.

We first calculated the percentage of women who graduated high school in the pre- and post-CFPI cohorts in 12 groups defined by 2010 residence (Colorado, parallel trends states, non-Colorado U.S. states, and high growth potential U.S. states) and race/ethnicity (all women of any race or ethnicity, only non-Hispanic White, and only Hispanic). For each group, we calculated the cohort difference as the percentage who graduated high school in the post-CFPI cohort minus percentage in the pre-CFPI cohort.

We compared Colorado first to parallel trends states, then to non-Colorado U.S. states, and to the higher growth potential states. For each race/ethnicity group, we calculated the DID by subtracting the cohort difference in the comparison place from the cohort difference in Colorado. To test whether these DID estimates were statistically significant, we estimated individual-level linear probability models of high school graduation for members of pre-CFPI and post-CFPI cohorts, using separate models to compare Colorado to each comparison group. Models included an indicator of Colorado cohort membership (1 if Colorado resident at the 2010 Census, 0 otherwise), an indicator of post-CFPI cohort membership (1 if member of post-CFPI cohort, 0 if member of pre-CFPI cohort), the interaction of these two indicators (our DID estimator), and state fixed effects. To assess whether we should test effects of CFPI separately by race/ethnicity group, we also fitted models that included binary indicators of race/ethnicity and the interactions of these indicators with Colorado residence, with post-CFPI cohort, and with the interaction of Colorado and post-CFPI. The coefficient on the three-way interaction of race/ethnicity, Colorado residence, and post-CFPI cohort was significant; therefore, we present separate models for all women, non-Hispanic White women, and Hispanic women.

Because our race/ethnicity groups had different initial levels of high school graduation, and because nongraduation from high school is the adverse outcome that a positive effect of CFPI could potentially avert, we also calculated the DID as a percentage of young women who did not graduate high school in the pre-CFPI group [calculated as (DID estimate)/(percentage who did not graduate in pre-CFPI cohort)]. This provides an estimate of the relative effect of CFPI on the probability that a member of the population would not graduate from high school by our age of observation.

All analyses used ACS weights and two sets of weights we generated to account for our analytic design: First, weights to adjust for rates of 2010 Census-ACS linkage by state at ACS, birth cohort, and race/ethnicity category (as defined above); and second, weights to adjust for the difference in age structure between pre- and post-CFPI cohorts. The differences in age structure were generated by censoring in the post-CFPI cohort (see red section of orange diagonal in Fig. 1). SEs were adjusted for clustering within states. All analyses were performed using Stata 16.

SUPPLEMENTARY MATERIALS

Supplementary material for this article is available at http://advances.sciencemag.org/cgi/content/full/7/19/eabf6732/DC1

REFERENCES AND NOTES

1. Institute of Medicine, A. S. Butler, E. W. Clayton, *A Review of the HHS Family Planning Program: Mission, Management, and Measurement of Results* (National Academies Press, Washington D.C., 2009).
2. U.S. Department of Health and Human Services, Office of Disease Prevention and Health Promotion, Healthy People 2020 - Topics and Objectives - Family Planning (2016); www.healthypeople.gov/2020/topics-objectives/topic/family-planning.
3. L. Ross, E. Derkas, W. Peoples, L. Roberts, P. Bridgewater, *Radical Reproductive Justice: Foundation, Theory, Practice, Critique* (Feminist Press at CUNY, 2017).
4. Z. Luna, *Reproductive Rights as Human Rights: Women of Color and the Fight for Reproductive Justice* (NYU Press, 2020).
5. Institute of Medicine, Committee on Preventive Services for Women, Board on Population Health and Public Health Practice, *Clinical Preventive Services for Women: Closing the Gaps* (National Academies Press, Washington D.C., 2011).
6. *Griswold v. Connecticut* (1965).
7. L. Schulkind, D. H. Sandler, The timing of teenage births: Estimating the effect on high school graduation and later-life outcomes. *Demography* **56**, 345–365 (2019).
8. A. T. Geronimus, S. Korenman, The socioeconomic costs of teenage childbearing: Evidence and interpretation. *Demography* **30**, 281–290 (1993).
9. L. L. Wu, N. D. E. Mark, Could we level the playing field? Long-acting reversible contraceptives, nonmarital fertility, and poverty in the United States. *RSF Russell Sage Found. J. Soc. Sci.* **4**, 144–166 (2018).
10. A. C. Gubrium, E. S. Mann, S. Borrero, C. Dehlendorf, J. Fields, A. T. Geronimus, A. M. Gómez, L. H. Harris, J. A. Higgins, K. Kimport, K. Luker, Z. Luna, L. Mamo, D. Roberts, D. Romero, G. Sisson, Realizing reproductive health equity needs more than long-acting reversible contraception (LARC). *Am. J. Public Health* **106**, 18–19 (2016).
11. A. M. Gomez, L. Fuentes, A. Allina, Women or LARC first? Reproductive autonomy and the promotion of long-acting reversible contraceptive methods. *Perspect. Sex. Reprod. Health* **46**, 171–175 (2014).
12. I. V. Sawhill, *Generation Unbound: Drifting into Sex and Parenthood without Marriage* (Brookings Institution Press, 2014).
13. F. F. Furstenberg, *Destinies of the Disadvantaged: The Politics of Teen Childbearing* (Russell Sage Foundation, 2007).
14. C. J. Diaz, J. E. Fiel, The effect(s) of teen pregnancy: Reconciling theory, methods, and findings. *Demography* **53**, 85–116 (2016).
15. M. J. Bailey, More power to the pill: The impact of contraceptive freedom on Women's life cycle labor supply. *Q. J. Econ.* **121**, 289–320 (2006).
16. M. J. Bailey, J. M. Lindo, in *Oxford Handbook on the Economics of Women*, S. L. Averett, L. M. Argys, S. D. Hoffman, Eds. (Oxford Univ. Press, New York, 2018).
17. C. Goldin, L. F. Katz, The power of the pill: Oral contraceptives and Women's career and marriage decisions. *J. Polit. Econ.* **110**, 730–770 (2002).
18. M. J. Bailey, Reexamining the impact of family planning programs on US fertility: Evidence from the war on poverty and the early years of Title X. *Am. Econ. J. Appl. Econ.* **4**, 62–97 (2012).
19. S. Wood, T. Beeson, B. Bruen, D. Goetz Goldberg, H. Mead, P. Shin, S. Rosenbaum, Scope of family planning services available in federally qualified health centers. *Contraception* **89**, 85–90 (2014).
20. J. E. Potter, K. Hopkins, A. R. A. Aiken, C. Hubert, A. J. Stevenson, K. White, D. Grossman, Unmet demand for highly effective postpartum contraception in Texas. *Contraception* **90**, 488–495 (2014).
21. "Taking the Unintended Out of Pregnancy: Colorado's Success with Long-Acting Reversible Contraception" (Colorado Department of Public Health and Environment, 2017).
22. J. M. Lindo, A. Packham, How much can expanding access to long-acting reversible contraceptives reduce teen birth rates? *Am. Econ. J. Econ. Policy.* **9**, 348–376 (2017).
23. S. Ricketts, G. Klingler, R. Schwalberg, Game change in Colorado: Widespread use of long-acting reversible contraceptives and rapid decline in births among young, low-income women. *Perspect. Sex. Reprod. Health* **46**, 125–132 (2014).
24. A. Kelly, J. M. Lindo, A. Packham, The power of the IUD: Effects of expanding access to contraception through Title X clinics. *J. Public Econ.* **192**, 104288 (2020).
25. G. M. Secura, T. Madden, C. McNicholas, J. Mullersman, C. M. Buckel, Q. Zhao, J. F. Peipert, Provision of no-cost, long-acting contraception and teenage pregnancy. *N. Engl. J. Med.* **371**, 1316–1323 (2014).
26. G. M. Secura, J. E. Allsworth, T. Madden, J. L. Mullersman, J. F. Peipert, The contraceptive CHOICE project: Reducing barriers to long-acting reversible contraception. *Am. J. Obstet. Gynecol.* **203**, 115.e1-7 (2010).
27. C. C. Harper, C. H. Rocca, K. M. Thompson, J. Morfesis, S. Goodman, P. D. Darney, C. L. Westhoff, J. J. Speidel, Reductions in pregnancy rates in the USA with long-acting reversible contraception: A cluster randomised trial. *The Lancet.* **386**, 562–568 (2015).
28. R. A. Hummer, E. M. Hernandez, The effect of educational attainment on adult mortality in the United States. *Popul. Bull.* **68**, 1–16 (2013).
29. P. M. Krueger, M. K. Tran, R. A. Hummer, V. W. Chang, Mortality attributable to low levels of education in the United States. *PloS One.* **10**, e0131809 (2015).
30. P. M. Krueger, I. A. Dehry, V. W. Chang, The economic value of education for longer lives and reduced disability. *Milbank Q.* **97**, 48–73 (2019).
31. S. Lamb, E. Markussen, R. Teese, N. Sandberg, J. Polesel, *School Dropout and Completion: International Comparative Studies in Theory and Policy* (Springer Science & Business Media, 2010).
32. I. Sasson, Trends in life expectancy and lifespan variation by educational attainment: United States, 1990–2010. *Demography* **53**, 269–293 (2016).
33. K. Hicks-Courant, A. L. Schwartz, Local access to family planning services and female high school dropout *Rates*. *Gynecologie* **127**, 699–705 (2016).
34. E. M. August, E. Steinmetz, L. Gavin, M. I. Rivera, K. Pazol, S. Moskosky, T. Weik, L. Ku, Projecting the unmet need and costs for contraception services after the Affordable Care Act. *Am. J. Public Health* **106**, 334–341 (2016).
35. J. J. Frost, M. R. Zolna, L. F. Frohwirth, A. Douglas-Hall, N. Blades, J. Mueller, Z. H. Pleasure, S. Kochhar, "Publicly supported family planning services in the United States: Likely need, availability and impact, 2016" (Guttmacher Institute, 2019); www.guttmacher.org/report/publicly-supported-FP-services-US-2016.
36. D. Wagner, M. Layne, *The Person Identification Validation System (PVS): Applying the Center for Administrative Records Research and Applications'(CARRA) Record Linkage Software* (Center for Economic Studies, US Census Bureau, 2014).
37. M. Layne, D. Wagner, C. Rothhaas, *Estimating Record Linkage False Match Rate for the Person Identification Validation System*, vol. 2 (Center for Administrative Records Research and Applications, Washington, D. C., 2014).
38. E. Mulrow, A. Mushtaq, S. Pramanik, A. Fontes, Final report: Assessment of the U.S. Census Bureau's Person Identification Validation System (NORC at the University of Chicago, 2011); www.norc.org/PDFs/May%202011%20Personal%20Validation%20and%20Entity%20Resolution%20Conference/PVS%20Assessment%20Report%20FINAL%20JULY%202011.pdf.
39. 2010 Census Planning Memoranda Series No. 247 (2012); www.census.gov/content/dam/Census/library/publications/2012/dec/2010_cpex_247.pdf.
40. S. V. Cameron, J. J. Heckman, The nonequivalence of high school equivalents. *J. Labor Econ.* **11**, 1–47 (1993).
41. A. Abadie, Semiparametric difference-in-differences estimators. *Rev. Econ. Stud.* **72**, 1–19 (2005).
42. M. Grosz, A. Hines, In-state tuition policies and the college decisions of undocumented students: Evidence from Colorado. *Educ. Finance Policy*, 1–49 (2020).
43. S. G. Donald, K. Lang, Inference with difference-in-differences and other panel data. *Rev. Econ. Stat.* **89**, 221–233 (2007).
44. A. Abadie, A. Diamond, J. Hainmueller, Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *J. Am. Stat. Assoc.* **105**, 493–505 (2010).
45. A. Abadie, A. Diamond, J. Hainmueller, Synth: An R Package for synthetic control methods in comparative case studies. *J. Stat. Softw.* **42**, 1–17 (2011).

Acknowledgments: We would like to thank J. Sanders and T. Streng-Schroeter for research assistance and C. Medalia and three anonymous reviewers for helpful suggestions and feedback. **Funding:** This research was supported by the William and Flora Hewlett Foundation (grants 2017-6333 and 2019-9738), by the Society of Family Planning Research Fund (award SFPRF11-08, project no. 1555575), by the Eunice Kennedy Shriver National Institute of Child Health and Human Development (grant 1R01HD101480-01), and by seed funding from the University of Colorado Boulder Office of Research and Innovation and the University of Colorado Population Center. This work has also benefited from research, administrative, and computing support provided by the University of Colorado Population Center, funded by the Eunice Kennedy Shriver National Institute of Child Health and Human Development (project 2P2CHD066613-06). **Author contributions:** A.J.S.: Conceptualization, methodology, validation, formal analysis, writing the original draft, and funding acquisition. K.R.G.: Conceptualization, methodology, validation, formal analysis, data curation, writing (review and editing), and funding acquisition. S.Y.: Conceptualization, methodology, writing (review and editing), and funding acquisition. S.M.: Conceptualization, methodology, writing (review and editing), and funding acquisition. J.A.M.: Conceptualization, methodology, writing (review and editing), and funding acquisition. **Competing interests:** The authors declare that they have no competing interests. **Data and materials availability:** We used secondary data sources that the research team is approved to use through the Census

Bureau's restricted use program. Other researchers can access these data via the Federal Statistical Research Data Center system, as we did. Our study materials, including data curation software and analysis code, are available to approved researchers within the secure environment. **Disclaimer:** Any views expressed are those of the authors and not necessarily those of the U.S. Census Bureau. Results were approved for release by the Census Bureau's Disclosure Review Board, authorization numbers CBDRB-FY20-ERD002-010 and CBDRB-FY21-ERD002-012.

Submitted 11 November 2020
Accepted 17 March 2021
Published 5 May 2021
10.1126/sciadv.abf6732

Citation: A. J. Stevenson, K. R. Genadek, S. Yeatman, S. Mollborn, J. A. Menken, The impact of contraceptive access on high school graduation. *Sci. Adv.* **7**, eabf6732 (2021).