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Does Increasing Financial Access to Contraception in the U.S. Reduce Undesired Pregnancies? Evidence from the M-CARES Randomized Control Trial at Two Years

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ABSTRACT

We use a randomized controlled trial to examine how out-of-pocket costs affect contraceptive method choice, pregnancy, abortion, and childbirth among uninsured U.S. women. The study recruited women seeking care through Title X—a national family planning program subsidizing reproductive health services for low-income Americans—and randomized vouchers, making the full spectrum of contraception highly discounted or free. We find that subsidizing contraception has large and persistent effects on the choice of contraceptive method, resulting in significantly fewer pregnancies and abortions, but no effect on births, at 26 months.

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Each year, roughly 40% of the more than 6 million pregnancies in the United States are unintended (Kost et al. 2023), which the Centers for Disease Control defines as occurring earlier than intended or when no birth was wanted in the future.[1] A large descriptive and quasi-experimental literature links unintended pregnancies to poorer health and worse psychosocial and economic outcomes among women and children (Mohllajee et al. 2007; Bailey 2013; Bailey et al. 2018; Guzzo and Hayford 2020; Miller et al. 2023; Londoño-Vélez and Saravia 2025; Bailey 2025), and they are significantly more common among younger, low-income, and minority women. These patterns suggest that unintended pregnancies exacerbate a cycle of disadvantage and inequality.

Contraceptive use is the most important proximate determinant of unintended pregnancy, which raises the question: could policies expanding access to contraception reduce unintended pregnancy? Although high-quality randomized controlled trials (RCTs) have begun to shed light on this question in low- and middle-income countries (Ashraf et al. 2014; Dupas et al. 2025; Bau et al. 2025), this gold standard of evidence has been limited for high-income countries. The literature on the U.S., by necessity, has relied on uncontrolled or quasi-experimental research designs, evaluated interventions that bundle several policy changes, or focused on specific contraceptive methods—features that have sometimes led to conflicting conclusions. Moreover, data constraints have limited the extent to which credible quasi-experimental studies can link policy changes to individual choices about contraceptive use or pregnancy outcomes, leaving considerable uncertainty about the magnitude of treatment effects.[2]

[1] Recent research has moved away from using the term "intention" and shifted toward the language of "desire," which aligns more closely with survey questions (Kost and Zolna 2019; Potter et al. 2019; Maddow-Zimet and Kost 2020). We acknowledge this important shift in language but adopt the consistent terminology of "unintended" because it is widely understood and aligns with the language used in most of the literature.

[2] The St. Louis Contraceptive CHOICE Project provided free access to contraception but lacked a control group, limiting causal conclusions (Birgisson et al. 2015; Bailey and Lindo 2018). Quasi-experimental work improves on CHOICE's design, but the evidence is mixed. Colorado's Family Planning Initiative (CFPI), which made LARCs free at federally funded clinics, reduced teen births (Lindo and Packham 2017; Kelly, Lindo, and Packham 2020), but providing free condoms in high schools tended to raise teen births unless paired with counseling (Buckles and Hungerman 2018). Reductions in funding for family planning in Texas increased teen birth rates (Packham 2017). Medicaid family-planning expansions in the 1990s lowered birth rates, with larger effects for teens (Lindrooth and McCullough 2007; Kearney and Levine 2009), but the post-ACA Medicaid expansion appears to have had little impact on birth rates, despite affecting contraceptive use (Becker 2018; Dalton et al. 2020; Gartner et al. 2022). The Delaware Contraceptive Access Now program increased LARC use in Title X clinics (Boudreaux et al. 2020) and among teens in Medicaid claims (Boudreaux et al. 2022) but produced little to no change in unintended pregnancies resulting in childbirth (Hurtado-Acuna and Rendall 2025) or abortion rates (Kim et al. 2023). By contrast, Virginia's 2018–2023 Contraceptive Access Initiative, which funded Title X and other clinics to provide no-cost LARCs and other methods, reduced birth rates but by around

The Michigan Contraceptive Access, Research, and Evaluation Study (M-CARES) uses a large-scale RCT to examine how out-of-pocket costs shape choices about contraceptive use and pregnancy outcomes. Between 2018 and 2023, M-CARES randomized vouchers that made *any* method of contraception free or sharply discounted for 100 days at Title X health centers—federally funded clinics providing reproductive care to millions of low-income Americans. Although the Affordable Care Act (ACA) eliminated cost-sharing for contraception for Americans with health insurance (including Medicaid) (Becker 2018; Dalton et al. 2020), its provisions did not apply to cost-sharing among those without insurance. M-CARES vouchers reduced or eliminated these remaining out-of-pocket costs.

This paper reports the effects of this intervention on the choice of contraception and pregnancy outcomes at 26 months. Our first set of findings shows that using the voucher increased the likelihood of buying contraception by 25 percentage points (68%) and raised spending on contraception by $334 (288%) within the first 100 days of the trial. In addition, voucher users switched to methods with 24 percentage-point-lower failure rates (70%) and raised the use of long-acting reversible contraceptives (LARCs, which refers to intrauterine devices, IUDs, and implants) by 11 percentage points (213%). In addition, voucher users purchased methods covering an average of 254 more days (167%), increasing the likelihood of consistent method use and reducing the need for return visits. While the intervention was temporary, these large treatment effects persisted for at least 26 months, suggesting that contraceptive costs constrain method choice in the long run.

Changes in contraception method may not reduce pregnancies, because women using less effective contraceptives may adjust their behaviors (e.g., reducing the frequency of intercourse, using withdrawal; Pennington and Venator 2024). In addition, switching to new method may cause discontinuation due to side effects or less consistent use. Using detailed administrative and survey data on pregnancy, abortion, and childbirth, our second set of findings shows that, by 26 months, voucher use reduced pregnancies by 16%

half the amount of CFPI (Kiser et al. 2024). Other randomized trials in the U.S have been limited to teens, and most have found limited effects of family-planning interventions on childbearing (Kirby 1997; DiCenso et al. 2002). However, the Teen Options to Prevent Pregnancy program, an 18-month intervention that consisted of personalized contraceptive counseling, facilitated access to contraceptive services, and referrals to social services, found large and significant increases in the use of LARCs and substantial reductions in repeat and unintended pregnancies among teen mothers (Luca et al. 2021).

and abortions by 12%. Our follow-up survey suggests that 76% of the reduction in pregnancies and 92% of the reduction in abortions would have been unintended. We find little evidence that the voucher reduced births within 26 months.

These results may be surprising given conclusions that contraceptive costs pose a minor barrier to method choice among teens or poor and near-poor U.S. populations (DiCenso et al. 2002; Edin et al. 2007; Silverman et al. 1987; Reed et al. 2014). These results are also seemingly at odds with a high-quality RCT in Burkina Faso, which finds that offering free contraception had a negligible effect on its use and birth rates over three years (Dupas et al. 2025). Key to understanding these apparent contradictions is that we study a population seeking contraceptive care whose choice of method was constrained by costs. In addition, high-quality administrative microdata allow us to observe pregnancies that end in abortion, which are severely underreported in surveys (Hood et al., 2022; Kissling and Jackson, 2022; Lindberg et al., 2020) and have not been available for other studies.

This study makes several contributions. *First*, it provides the first experimental evidence regarding the importance of financial access to contraception for a broad U.S. population, helping settle questions of long-standing academic and policy interest. This research design improves upon both observational and quasi-experimental studies, which have come to differing conclusions, and expands experimental evidence to non-teens. *Second*, M-CARES follows individuals longitudinally using survey and administrative *micro*data, providing a comprehensive and dynamic picture of contraceptive use and pregnancy outcomes. Whereas quasi-experimental studies have been forced to examine outcomes in aggregated data or cross-sectional surveys (Kearney and Levine 2009; Boudreaux et al. 2020, 2022; Kim et al. 2023; Hurtado-Acuna and Rendell 2025), this study links individuals' financial access, contraceptive choice, and pregnancy outcomes over time in an instrumental variables framework. *Third*, M-CARES examines a scalable intervention that could affect millions of Title X patients—a low-income population seeking reproductive health care who face significant out-of-pocket costs (Bailey 2024). Study findings are directly relevant to understanding the effects of eliminating the Title X program (proposed in recent Congressional budgets) and provide novel evidence regarding Title X's effects on contraceptive use and pregnancy outcomes. Our

experimental design and the richness of our data provide rigorous evidence on a highly relevant and scalable policy intervention that could enable more U.S. women to use their preferred contraceptive method and reduce unintended pregnancies.

I. Background and Study Design

At the outset of the trial, 1.6 million Americans served through Title X (40% of all Title X clients) were uninsured (Fowler 2019) and faced out-of-pocket costs that could reach over $1,000 for LARCs. The Title X program's sliding scale reduced these costs: patients with incomes at 201-250% of the federal poverty line (FPL) paid 75% of these costs; at 151-200% of the FPL paid 50%; and at 101-150% of the FPL paid 25%. But even with the sliding scale, paying over $250 ($1,000 x 25%) for contraception could compete with basic expenses such as rent or groceries. Financing out-of-pocket costs of contraception is not available through any public program or market mechanism; costs are paid upfront.

The M-CARES trial hypothesized that out-of-pocket costs could pose a large barrier to using a preferred contraceptive method and, by extension, raise unintended pregnancies. The study aimed to make *any desired* contraceptive lower cost or free, while not promoting any particular method. To implement the trial, M-CARES partnered with Planned Parenthood of Michigan (PPMI), Michigan's largest Title X provider serving 70% of the state's clients (Compton et al. 2025). The trial was approved by the Institutional Review Boards at the University of Michigan (HUM00132900) and the University of California-Los Angeles (IRB-24-5355) and registered at the American Economic Association RCT Registry (Bailey et al. 2020).

Recruitment

Between August 20, 2018, and February 28, 2023, M-CARES recruited participants in the waiting rooms of 13 PPMI health centers. Professionally trained NORC field interviewers asked prospective recruits to complete a 5-minute, self-administered screening survey on a tablet, which was compensated with $10.[3] If a patient met the inclusion criteria and was willing to participate, a tablet led her through the

[3] NORC is a non-partisan research organization at the University of Chicago that specializes in survey research.

informed consent, with optional assistance from the interviewer. Participation required that the patient agree to (1) be contacted to complete follow-up surveys and (2) release her administrative data to the study.

Inclusion Criteria and Final Sample

M-CARES recruited individuals who were (1) between the ages of 18 to 35 (inclusive); (2) physically capable and at risk of having a pregnancy (biologically female and fecund); (3) not pregnant nor wishing to become pregnant in the next year; and who (4) faced out-of-pocket costs for contraceptives, which the study's intervention could reduce. This final criterion largely excluded patients with incomes below the federal poverty line, who received free care under the Title X sliding scale for most of the trial and always excluded individuals with health insurance (e.g., Medicaid) for whom contraception was free under the ACA.

As the national policy context and PPMI's policies changed, the study adapted its recruitment. Initially, the study restricted eligibility by visit type, including only patients with clinician visits based on the advice of PPMI. However, as M-CARES learned more about the different groups visiting PPMI, we collaborated with PPMI to relax this restriction and, on May 13th, 2019, began to recruit patients with non-clinician visits (e.g., contraceptive supply pick-ups, lab services such as fluid samples or blood work). On August 24th, 2020, M-CARES gained PPMI's permission to recruit patients who came to health centers seeking abortions. Our analysis pools these different study populations and models changes in participant composition in our first stage.

Over four years of recruitment, Figure 1 shows that 6,037 patients met the inclusion criteria, of which 4,192 consented and were randomly assigned: 2,063 received vouchers, and 2,129 were assigned to the control group. After randomization, 27 participants opted to withdraw from the study, which any participant could do by sending an email. 76% of those randomized responded to the year-two follow-up survey (Y2FU), a rate that does not differ for voucher recipients and non-recipients (-0.0057, s.e.=0.022). Our analysis sample includes 3,172 individuals who could be followed in (1) the Y2FU survey and (2) PPMI medical records (99%).

Study Intervention

After walking study participants through the screening and consent, the tablet randomized the assignment of vouchers in a 1:1 ratio within health centers. The voucher amount was determined by a patient's out-of-pocket costs according to the sliding scale (described previously). However, the cost of services and the sliding scale shifted over time, due to unanticipated changes in PPMI pricing and the national policy environment. Consequently, M-CARES adjusted the value of the voucher to reflect these changes (see Appendix Table C1).

Phase 1. The voucher covered up to the out-of-pocket cost of a Liletta IUD (the least expensive IUD, which cost 50% of name-brand devices like Skyla, Paragard, or Mirena) and all related medical costs.[4] However, vouchers could be used to cover the costs of *any contraceptive and related costs* over multiple visits for up to 100 days after enrollment. For instance, the voucher could be used to select a Mirena IUD, but participants would pay out of pocket for costs exceeding the voucher. The voucher could also be used to purchase multiple packs of birth control pills (up to 12 packs), receive two Depo injections, or purchase any other kind of birth control.[5] Consistent with 42 U.S.C. § 300a-6, vouchers could not be used for abortion services.

Phase 2. After six months, we learned the Liletta was rarely stocked or used, so the intervention had only halved the price of available IUDs, rather than making them free. We, therefore, increased voucher generosity to fully cover the out-of-pocket cost of the commonly stocked name-brand IUDs, effective March 4, 2019.

Phase 3. Following the federal Title X rule changes on November 4, 2019, PPMI withdrew from Title X.[6] During this period, PPMI began stocking and inserting Lilettas and reduced the generosity of its

[4] Related medical costs are those required to use a contraceptive method. For example, getting an IUD requires a pregnancy test and the procedure to have the IUD inserted.

[5] The 100-day period applies to all trial phases and allowed women to return to PPMI, for example, to get two shots of Depo-Provera (each lasting 90 days) or an IUD. The 100-day deadline also helped minimize procrastination, which could lead participants to forget about or lose the voucher (Ariely and Wertenbroch 2002; O'Donoghue and Rabin 1999).

[6] New 2019 Trump Administration restrictions prohibited Title X providers from referring patients for abortion or providing abortion and family planning services at the same location. Refusing to operate under these restrictions, Planned Parenthood withdrew from Title X.

sliding-fee scale. For the first time since 1970, PPMI charged patients with incomes lower than the federal poverty line (as long as they were 22 or older) and adjusted cost-sharing for other income groups. The study continued funding vouchers up to a name-brand IUD and expanded recruitment to those below the poverty line, adapting voucher amounts to the new sliding scale. Recruitment was interrupted by COVID-19 clinic closures.

Phase 4. When Planned Parenthood rejoined Title X on September 14, 2021, it modestly changed its pricing for most groups but eliminated out-of-pocket costs for individuals below the poverty line. M-CARES continued to make any contraceptive free up to the cost of a name-brand IUD for all groups with out-of-pocket costs and updated voucher amounts to reflect the updated sliding scale.

To account for these unanticipated changes in patient composition and the relative generosity of the voucher (due to changes in PPMI pricing and national policy), our analysis models heterogeneity in voucher use by study phase and patient population.

Final Sample Characteristics

Consistent with randomization, the voucher (N=1,567) and control groups (N=1,605) are well balanced in pre-specified characteristics, including contraceptive methods used before enrollment, age, race/ethnicity, marital/cohabitation status, education, income as a percent of the federal poverty line, and previous childbearing (Table 1, columns 3-4). Although the voucher and control groups exhibit a handful of differences, these characteristics do not jointly predict voucher receipt (F-statistic=0.62, p=0.93).[7] At recruitment, 72% of M-CARES participants report using some form of contraception, with birth control pills (26%), nonprescription methods (e.g., condoms or withdrawal; 22%), and LARCs (15%) being the most commonly used. Relative to all Title X clients nationwide from the 2018 Health and Human Services (HHS) Annual Report (column 1), the M-CARES sample is less likely to have income below the federal poverty line, owing to the fact that M-CARES only recruited individuals in poverty during phase 3. In addition, the M-CARES sample is less likely to identify as Hispanic/Latina and Black, owing to this group's

[7] This test excludes observations with missing values.

underrepresentation in Michigan and in the areas served by Planned Parenthood health centers participating in M-CARES.

II. Data Sources and Primary Outcomes

M-CARES tracks participants' outcomes in survey and administrative data from the following three sources. First, we link participants to their Y2FU survey responses that occurred at least 25 months after participants enrolled. We observe 100% of our survey respondents for 25 months and 99.8% for 26 months, which we use as the endpoint of our analysis. This survey asked respondents about their use of reproductive health care and contraceptives and the dates of pregnancies, births, abortions, and miscarriages.

Second, we link participants to billing and medical records from PPMI for 26 months after enrollment. This information contains spending on contraception, pregnancy tests and diagnoses, whether an abortion was obtained, the date of PPMI services, and payment information (amount and source). All but 25 individuals were linked to PPMI billing records (99%=3,147/3,172).

Third, we link participants to their birth records, which are available from the Michigan Department of Health and Human Services (MDHHS) through December 31, 2023, which is at least 26 months after enrollment for 79% of our sample. Although 21% of our sample does not have 26 months of data from MDHSS records, a comparison of MDHHS records to the Y2FU for respondents with both sources of information suggests that 95% of births are reported in the survey.

We reconciled these different sources of data to create a comprehensive individual panel up to 26 months after recruitment for our outcomes of interest: (1) contraceptive use based on purchases at PPMI; (2) pregnancy, defined by the date the pregnancy ended and including all births, abortions, and miscarriages;[8] (3) abortion (date of occurrence); and (4) birth (date of occurrence). For (2)-(4), we treated events as the same if they matched in both administrative and survey data. Because survey responses contain errors, we reconciled events that did not match exactly on dates (see Appendix A).

[8] In our data, 6.3% of pregnancies after recruitment end in miscarriages. 5.8% of miscarriages come from the PPMI medical records and the remaining come from the Y2FU.

Although 95% of births in MDHHS are reported in the survey, 46% of abortions observed at PPMI were omitted from the survey, which is consistent with previous studies of abortion underreporting (Hood et al. 2022; Kissling and Jackson 2022; Lindberg et al. 2020). Our administrative data miss abortions that occurred outside of PPMI. Similarly, miscarriages are underreported in surveys (Yan and Tourangeau 2022). While this is helped somewhat by miscarriages observed in PPMI records, our data likely underrepresent pregnancies resulting in miscarriage. In short, our data capture a large number of pregnancies, births, and abortions. To the extent that missing abortions from private providers or underreported births and miscarriages occur disproportionately in the control group, both sources of measurement error will lead us to understate the effects of the intervention.

Primary Outcomes

Our research question relates to how financial access to contraception affects pregnancies and pregnancy outcomes. A key first step is understanding how financial access affects the choice of contraceptive method. To test the effects on different dimensions of contraceptive choice, we prespecified the following outcomes measured cumulatively at 100 days (the last day the voucher could be used) and from enrollment through month 26 (inclusive): (1) the total dollar value of all contraception purchased at PPMI; (2) whether any contraceptives were ever purchased; (3) whether a LARC was ever purchased; (4) the contraceptive efficacy of the most effective method purchased (efficacy defined by the CDC failure rate over one year for typical use, Trussell 2011); and (5) the number of days covered by all contraceptives purchased (e.g., one pack of birth control pills covers 28 days, whereas an injection covers 90 days). Appendix A provides additional detail regarding the construction of all variables used in the analysis.

We also pre-specified pregnancy outcomes, including (1) whether a pregnancy occurred and ended by date t (including all births, abortions, and miscarriages occurring after enrollment); (2) whether an abortion ever occurred by date t; and (3) whether a child had been born by date t. These pregnancy outcomes deviate from our pre-analysis plan in that they are measured as binary variables, rather than as counts. In addition, we omit reporting several pre-specified outcomes in the main text because they added no

additional information. We explain the rationale for these deviations in Appendix B and present all pre-specified analyses in Appendix Tables C2-C3 for the interested reader.

III. Research Design and Statistical Framework

In the absence of an experiment, measures of financial access to contraception will be correlated with a variety of individual characteristics as well as contraceptive choice and pregnancy outcomes. More educated women are significantly more likely to use IUDs today (Kavanaugh and Jerman 2017), and they have had fewer children than those with less education for at least 100 years (Bailey 2025). The advantage of randomizing vouchers allows the analysis to isolate the causal role of financial access on contraceptive method choices and pregnancy outcomes.

Our analysis uses a standard intention-to-treat (ITT) and three instrumental variables (IV) estimators. The ITT specification is

$$Y_{it} = \pi_1 Voucher_i + \boldsymbol{W}_i'\pi_2 + \varepsilon_{1it}, \quad (1)$$

where Y_{it} is a primary outcome measured for individual i at date t after the intervention, where t is measured in time from the date of enrollment and summarized in our tables at 100 days or 26 months. $Voucher_i$ is a binary variable equal to 1 if i is randomly assigned to receive a voucher and 0 otherwise. To increase precision, the vector $\boldsymbol{W}$ includes a constant and indicator variables for income categories, prior childbearing, age, education, the most effective contraceptive method used in the month before recruitment, trial phase, visit type on the day of enrollment to capture changes in participant population (non-clinician, clinician, and abortion visits), and health center of recruitment. π_1 captures the net, causal effect of providing a voucher on contraceptive use or pregnancy outcomes.

We also use a generalized method-of-moments two-step instrumental variables (IV) estimator, which is more efficient than two-stage least squares under heteroskedasticity:

$$VoucherUse_i = \delta_1 Voucher_i + \boldsymbol{W}_i'\delta_2 + \varepsilon_{2ai} \quad (2a)$$

$$Y_{it} = \delta_3 VoucherUse_i + \boldsymbol{W}_i'\delta_4 + \varepsilon_{2bit} \quad (2b)$$

where $VoucherUse_i$ is a binary variable equal to 1 for individuals who used their voucher to purchase contraception and 0 otherwise, and other variables are as previously defined. Under standard assumptions, δ_3 identifies a causal effect of voucher use on Y_{it} (Wooldridge 2002).[9]

Over the course of the trial, the cost of services, the population recruited, and the sliding scale shifted due to unanticipated changes in PPMI pricing and the national policy environment. We pursue several approaches to modeling these unanticipated changes, which deviate from our pre-analysis plan. Our first and preferred approach models heterogeneity in the first stage by leveraging institutional knowledge and accounts for changes in national and PPMI policies, recruitment population, natural variation across sites in the study's implementation, and the most effective pre-recruitment method of birth control. This last category captures women's baseline contraceptive needs, which influence both the demand for contraception and the "dose" of the intervention. For example, the 15% of women using a LARC before enrollment are less likely to need contraception, whereas the effect of voucher use on individuals who were not using contraception before enrollment could be much larger. Appendix Table C4 shows that the data strongly reject homogenous first-stage effects across these groups with an F-statistic of 149. We model this first-stage heterogeneity, which allows take-up responses to differ across groups, as follows:

$$VoucherUse_i = Voucher_i \boldsymbol{Z}_i'\theta_1 + \boldsymbol{W}_i'\theta_2 + \varepsilon_{3i}, \tag{3}$$

where $\boldsymbol{Z}$ is a subvector of $\boldsymbol{W}$ and contains a constant and characteristics that capture variation in the intervention's intensity, including indicators for trial phase, visit type on the day of recruitment, health center, and most effective pre-recruitment contraceptive method. The second stage remains as previously specified, and the only excluded instruments are interactions of $\boldsymbol{Z}$ with *Voucher* because $\boldsymbol{Z}$ enters the second stage as a subvector of $\boldsymbol{W}$.

[9] The causal interpretation turns on several important assumptions. First, financial barriers are relevant to women's decisions about which contraceptive method to use, which is directly tested. Second, voucher assignment is exogenous, which is consistent with randomization and balance tests in Table 1. Third, voucher assignment is excludable, that is only affects outcomes by increasing the affordability of desired contraceptives. This assumption seems plausible as the voucher can only be used for contraceptives and is not bundled with any additional intervention. Fourth, the monotonicity assumption rules out the case where receiving a voucher reduces the use of contraception. While this is hard to test in practice, there is little theoretical reason that providing a voucher to reduce the price of contraception would reduce its use.

A second approach to modeling first-stage heterogeneity uses post-double selection for the instruments following Belloni, Chernozhukov, and Hansen (2014) and Chernozhukov, Hansen, and Spindler (2015), which is implemented using ivlasso in Stata (Ahrens et al. 2018). This procedure uses LASSO to select from $\boldsymbol{W}$ excluded instruments among 40 possible *Voucher*–covariate interactions.

Both approaches to modeling first-stage heterogeneity increase the first-stage fit by allowing take-up to vary across sites, subgroups, and policy environment. The resulting GMM estimand is a weighted average over take-up margins shifted by the excluded instruments, where weights reflect both the magnitude of the first-stage relationship and the GMM weighting matrix.

IV. Results: How Financial Constraints Affect Contraceptive Choice, Pregnancy, Childbirth, and Abortion

A central question of the study is whether out-of-pocket costs for contraception constrain choices of birth control method or affect the risk of unintended pregnancy. If their contraceptive choices are not driven by financial constraints, receiving a voucher may simply crowd out spending rather than altering choices. However, if financial constraints play a role, receiving the voucher should alter recipients' contraceptive choices by making more methods affordable.

Table 2A reports statistics for the first stage: the average partial effect of receiving the voucher on voucher use; the effective F-statistic based on the Montiel Olea and Pflueger (2013) weak instrument test; and instruments assuming no first-stage heterogeneity (column 2,6), theoretically motivated first-stage instruments (column 3,7), and LASSO-selected first-stage instruments (column 4,8). Voucher take-up is 51%, and the randomized offer of the voucher easily passes the weak instrument test. Moreover, LASSO selects a similar set of instruments as the theoretically motivated specification, but it omits some of the within variable group categories that are not strongly associated with variation in the first stage (e.g., it includes a handful of site interactions rather than all site interactions with the voucher as in columns 3 and

7).[10] Our discussion focuses on the results for theoretically motivated instruments, but the LASSO instrument selection generally yields similar results.

We find that relaxing financial constraints led to large changes in contraceptive purchases in the short run.[11] Within 100 days of study enrollment when the voucher could be used, its use raised the purchase of any contraception by at least 25 percentage points, or 68% (Table 2B); increased dollars spent on contraceptives by $334, or 288% (Table 2C); and increased the efficacy of the most effective method purchased by 24 percentage points, or 70% (Table 2D), relative to the control group. Importantly, out-of-pocket expenditures fell by only $51 (s.e.=4.0, not shown for brevity) versus the $334 increase, implying that receiving the voucher had small crowd-out effects.

The increase in method efficacy partially reflects that voucher use boosted the use of LARCs by 11 percentage points, or 213% (Table 2E), which are some of the most expensive and effective methods.[12] But this is only part of the story. Appendix Table C5 shows much more contraceptive method switching across all methods in the treatment group than in the control group: 28% of voucher recipients switched to a more effective method versus 18% in the control group; 70% of women in the voucher group stayed on the same method or did not purchase any contraceptives versus 80% in the control group. Only 2-3% switched to less effective methods, which does not differ between the voucher and control groups (-0.0038, s.e.=0.0025).

Appendix Table C6 explores how variation in the generosity of the voucher, which coincided with trial phases, affected contraceptive choice. When the voucher was less generous and covered only 50% of the value of a LARC (phase 1), voucher use did not affect switching to more effective methods or using LARCs, but increased the use of birth control pills by 19 percentage points. In phase 2, when the voucher

[10] The decrease in the effective F-statistic between columns (2) and (3) reflects the fact that column (3) is a joint test over 22 excluded instruments. The increase in the effective F-statistic between columns (3) and (4) reflects the fact that LASSO selects a subsample of instruments with the strongest first stage. For LASSO, this statistic is not a valid test of the instrument selection, so its inclusion is only for comparison.

[11] The results differ from Bailey et al. (2023) due to using a different sample: individuals recruited across all phases (not just phases 1 and 2) and in the Y2FU.

[12] Among those purchasing LARCs within 100 days, 88% continued use at 26 months. Voucher use has a small positive effect on LARC removal at 26 months (1.5 percentage points, s.e.=0.67), because LARC use is higher in the treatment group.

doubled in value to cover 100% of the value of a LARC, voucher use increased transitions to more effective methods by 12 percentage points and LARC use by 17 percentage points, with no effect on the use of birth control pills. In phases 3-4, when the voucher value continued to cover the cost of a LARC but PPMI altered its pricing of other methods and offered generic LARCs for half price, voucher use again had smaller effects on switching to more effective methods (7 percentage points), inducing smaller increases in LARCs (10 percentage points) with some increase in birth control pill use (10 percentage points).

A comparison of the effects of the 50% and 100% vouchers sheds light on a highly relevant public policy question: how much of a subsidy is required to enable women to use their preferred method? Doubling the voucher subsidy from 50% to 100% more than quadrupled the treatment effect of voucher use on LARC use, raising the relative effect from 49% to 460%. These estimates imply a demand elasticity which exceeds 4 for the 100% subsidy (phase 2). That is, LARCs remain prohibitively expensive for uninsured women, even when reducing the already discounted Title X price by 50%. Making all tiers of contraceptives free, as in phase 2, allows significantly more women to use their preferred method.

Financial constraints may also affect the consistency of method use. For example, being priced out of longer-acting methods like an IUD could lead to using similarly effective, but shorter-acting, methods potentially requiring more follow-up visits, more time off work, higher child-care costs, and higher transportation costs. Financial constraints could also imply more frequent returns to clinics to pick up supplies, such as birth control pills, rather than purchasing multiple months of supplies at once. If receiving the voucher relaxes financial constraints, this could allow more women to switch to methods requiring less effort for consistent use (e.g., LARCs) or lead to more upfront purchases of supplies (e.g., buying 12 months of pills rather than returning monthly). Consistent with this hypothesis, voucher use increased the coverage of methods purchased by 254 days (Table 2F).

One interpretation of these findings is that the voucher hastened purchases, allowing women to switch methods a few months earlier, while not altering their method use at endline. But an examination of the effects of the voucher on contraceptive purchases over two years shows the effects persisted across the five dimensions of contraceptive choice for at least 26 months (Table 2, columns 5-8; Appendix Figure C1).

Slight reductions in the treatment effects indicate that voucher use hastened some contraceptive purchases, but the high persistence reveals that financial constraints were binding in the longer term. Differences in the treatment effects of voucher generosity also persist over time (Appendix Table C6, columns 11-16), suggesting that the high out-of-pocket costs of LARCs present binding constraints in the short and long run for low-income women.

These large effects on method choice differ from other studies (Dupas et al. 2025) and suggest that there may be effects on pregnancy as well, albeit with smaller magnitudes than implied by differences in method failure rates, due to inconsistent use, nonadherence, or other pregnancy risk mitigation strategies in the control group. The results also support this hypothesis. Because pregnancies take time to resolve, Figure 2A shows little effect on their incidence in the short term. However, the effects of voucher use grow to 3.3 percentage points at 26 months, a 16% reduction relative to the control group (Table 3A, column 3). A large share of this reduction in pregnancies is attributable to a reduction in abortions, which fell by 2.1 percentage points, or 12% (Table 3B, column 3). If missing miscarriages and underreported abortions or abortions at other providers are more prevalent in the control group (because the voucher group prevents more pregnancies), the treatment effects on pregnancies and abortions will be understated. Based on pregnancy intentions reported in Y2FU, 76% of pregnancies and 92% of abortions prevented by the voucher would have been unintended. With the caveat that births are rarer and so the analysis has less power, we find little effect on childbirth at 26 months (Table 3C, column 3).

Modelling substantively important first-stage treatment-effect heterogeneity resulting from unexpected changes in external policies (i.e., 2019 Title X changes) and conditions (i.e., the COVID-19 pandemic) proved important for revealing substantively large effects on pregnancy and abortion. Similarly, high-quality, administrative data on abortions revealed sizable behavioral responses within two years of the intervention, which are not evident in the survey. Using the Y2FU alone shows that the effect of voucher use on pregnancies is around ⅔ as large and on abortion is only ⅓ as large at 26 months, with neither estimate statistically significant. Similarly, had we used childbirth as an outcome (Table 3C)—the best information available for most quasi-experimental studies—we would have concluded that the intervention

had little effect at 2 years as found in other studies (Kearney and Levine 2009; Bailey 2012; Lindo and Packham 2017; Kelly, Lindo, and Packham 2020; Gartner et al. 2022; Kiser et al. 2024; Hurtado-Acuna and Rendell 2025).

V. Subsidizing Contraception Reduces Unintended Pregnancies and Abortions

Our experimental design and rich data provide the most rigorous evidence to date that the high costs of contraception prevent many uninsured women from using their preferred method and increase unintended pregnancies and abortions in the United States. Subsidizing the full spectrum of contraceptive methods raises the likelihood that low-income women purchase contraception, choose more effective methods, and purchase methods covering more days, reducing the need for frequent follow-up visits for refills. We also find that uninsured women are particularly sensitive to the prices of highly effective contraceptives, with the reduced-form price elasticity for LARCs exceeding 4 for the 100% subsidy. The treatment effects of the intervention's temporary reduction in the costs of contraception persist for over two years, which implies that these costs pose longer-term barriers to care. Finally, subsidizing the full spectrum of contraception also results in sizable reductions in unintended pregnancies and abortions among uninsured women for up to 26 months. We conclude that making contraception more affordable would allow more uninsured women to use their preferred methods, reduce unintended pregnancies, and help break the cycle of disadvantage in the U.S.

VI. References

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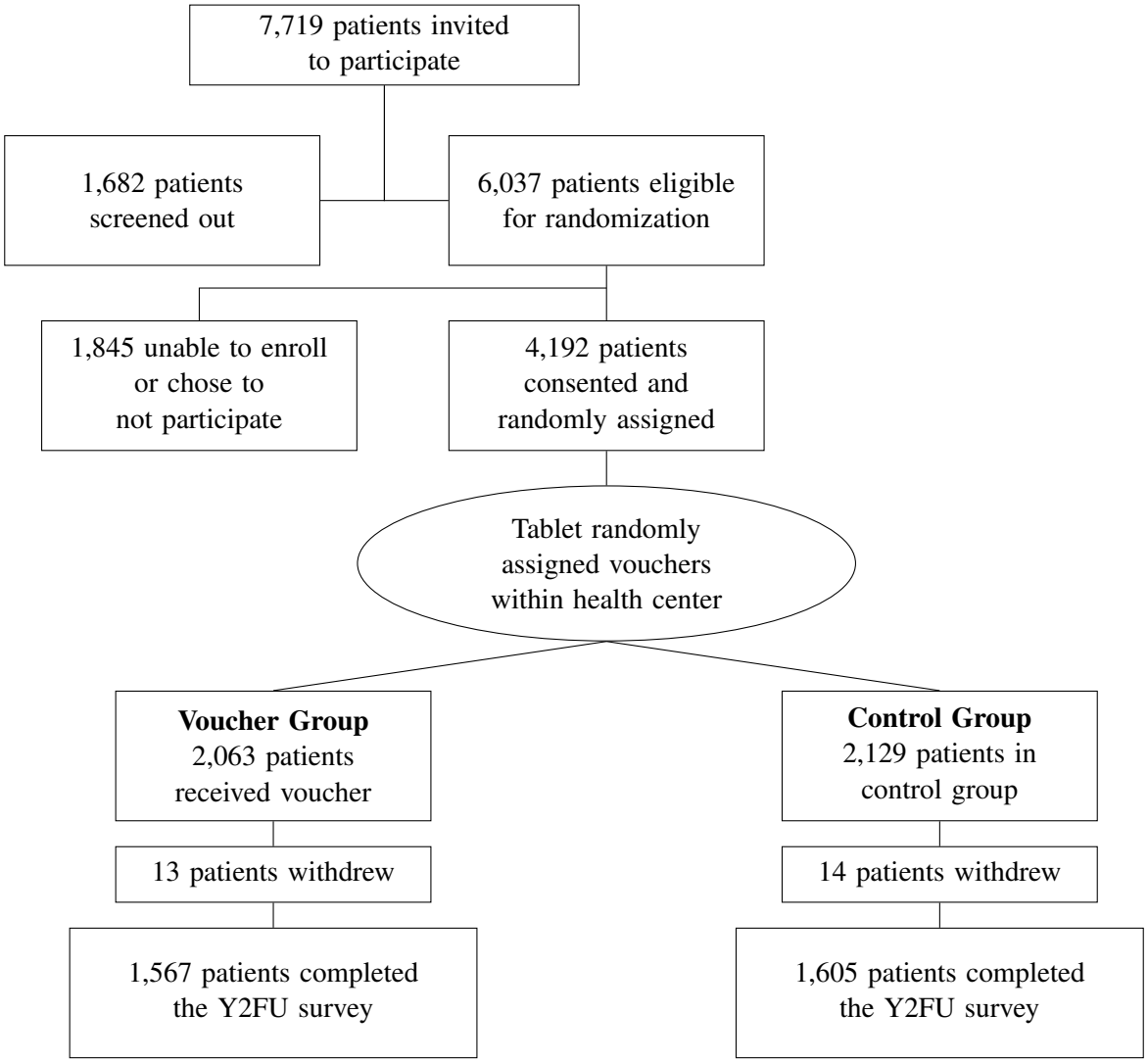
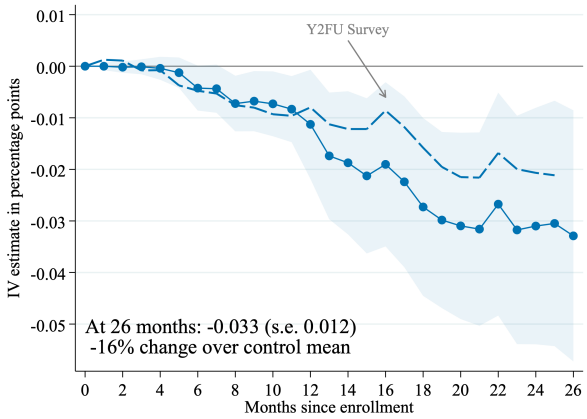
Figure 1. M-CARES Enrollment and Randomization of Patients



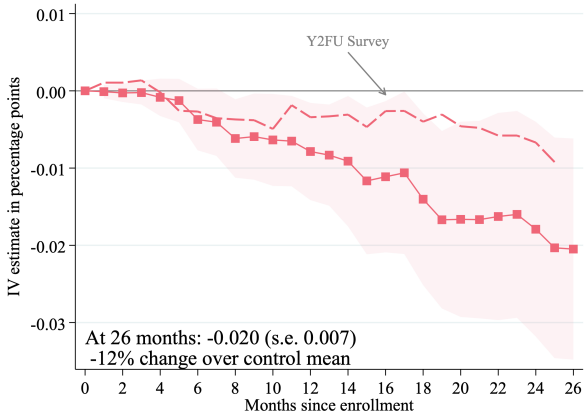
Notes: All participants were recruited between August 20, 2018, and February 28, 2023, from the waiting rooms of 13 Title X clinics. See text for more details.

Figure 2. Treatment Effects of Voucher Use on the Cumulative Incidence of Pregnancy, Abortion, and Childbirth, by Months since Enrollment

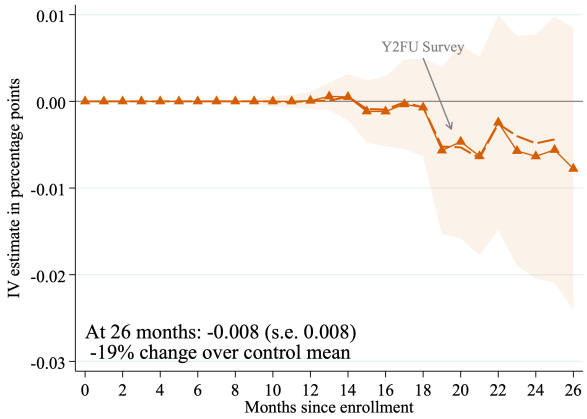
A. Cumulative Incidence of Pregnancy



B. Cumulative Incidence of Abortion



C. Cumulative Incidence of Childbirth



Notes: All individuals are observed through 25 months and 3,159 individuals are observed through 26 months. Y2FU stands for year 2 follow-up survey. Each point estimate is from a separate IV regression that models first-stage heterogeneity as in equation (3). Standard errors are corrected for heteroskedasticity (Huber 1967; White 1980).

Table 1. Characteristics of Title X Clients, M-CARES Participants, and Balance in the Treatment and Control Group

	(1) National Title X population	(2) All M-CARES participants	(3) Voucher group	(4) Control group	(5) Test of difference between (3)&(4) (p-values)
Observations	3,446,504	3,172	1,567	1,605	3,172
Age					
Age 18-19	0.10	0.08	0.08	0.08	0.74
Age 20-24	0.25	0.39	0.38	0.39	0.58
Age 25-29	0.21	0.32	0.32	0.32	0.98
Age 30-34	0.15	0.19	0.19	0.18	0.68
Age 35+	0.21	0.02	0.02	0.02	0.82
Race					
Non-Hispanic White	0.33	0.67	0.66	0.68	0.22
Non-Hispanic Black	0.19	0.13	0.14	0.13	0.75
Hispanic any race	0.34	0.11	0.11	0.10	0.42
Other	0.13	0.09	0.10	0.09	0.41
Income as % of federal poverty line (FPL)					
Up to 100%	0.65	0.08	0.07	0.08	0.46
101-150%	0.14	0.37	0.39	0.35	0.03
151-200%	0.07	0.24	0.22	0.26	0.01
201-250%	0.03	0.12	0.12	0.12	0.80
251+%	0.07	0.19	0.19	0.19	0.64
Birth control use[1]					
Any birth control	-	0.72	0.71	0.73	0.23
Birth control pills	-	0.26	0.26	0.26	0.87
LARC (IUD, implant)	-	0.15	0.15	0.15	0.82
Injection	-	0.06	0.06	0.06	0.73
Other prescription method[2]	-	0.03	0.03	0.03	0.84
Nonprescription method[3]	-	0.22	0.21	0.23	0.29
No method	-	0.28	0.29	0.27	0.23
Marital status					
Single	-	0.52	0.53	0.51	0.22
Cohabiting	-	0.23	0.23	0.24	0.47
Married	-	0.07	0.07	0.07	0.96
Education					
Less than high school	-	0.02	0.02	0.02	0.92
High school degree	-	0.15	0.16	0.14	0.14
Some college	-	0.42	0.42	0.43	0.43
College degree or more	-	0.23	0.23	0.23	0.87
Previous childbearing					
0 births	-	0.82	0.81	0.84	0.06
1 birth	-	0.10	0.10	0.09	0.29
2 births	-	0.05	0.06	0.05	0.43
3+ births	-	0.02	0.03	0.02	0.17

Notes: Estimates of the 2018 Title X participants are derived from Fowler et al. (2019), Exhibit 4, Exhibit 7, and Exhibit 15. We use the 2018 Title X population because this is the most recent year reported before Trump Administration rules restructured the program. [1]For M-CARES participants, birth control use is constructed from screening survey and PPMI billing data for the month prior to enrollment. [2]Other prescription method includes ring, patch, and diaphragm. [3]Nonprescription methods include condoms, withdrawal, and natural family planning, with a small number of reports of partner vasectomy at recruitment.

Table 2. Treatment Effects of Voucher Use on Contraceptive Choice

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	At 100 days				At 26 months			
		IV				IV		
	ITT	No first-stage heterogeneity	Theoretical first-stage heterogeneity	LASSO first-stage heterogeneity	ITT	No first-stage heterogeneity	Theoretical first-stage heterogeneity	LASSO first-stage heterogeneity
A. First stage								
Effect		0.512	0.513	0.510		0.512	0.513	0.510
		(0.0120)	(0.0113)	(0.0109)		(0.0120)	(0.0113)	(0.0109)
Effective F[1]		1,810	126	322		1,810	126	322
Instruments		Voucher	Voucher × P, V, S, C	Voucher × P, V, S, C, E		Voucher	Voucher × P, V, S, C	Voucher × P, V, S, C, E
B. Any Birth Control Purchase		*Control mean: 0.369*				*Control mean: 0.446*		
Effect	0.159	0.311	0.252	0.274	0.124	0.243	0.188	0.206
	(0.0154)	(0.0261)	(0.0234)	(0.0237)	(0.0158)	(0.0280)	(0.0232)	(0.0239)
% change	43.2%	84.2%	68.3%	74.3%	27.9%	54.4%	42.2%	46.1%
C. PPMI Charges on Birth Control in Dollars		*Control mean: 116*				*Control mean: 266*		
Effect	192	375	334	344	142	277	249	259
	(12.2)	(20.5)	(17.6)	(18.0)	(16.0)	(28.8)	(26.7)	(27.0)
% change	166%	324%	288%	297%	53.4%	104%	93.5%	97.2%
D. Contraceptive Efficacy		*Control mean: 0.343*				*Control mean: 0.417*		
Effect	0.152	0.297	0.240	0.261	0.119	0.232	0.182	0.196
	(0.0145)	(0.0245)	(0.0218)	(0.0221)	(0.0149)	(0.0264)	(0.0216)	(0.0224)
% change	44.4%	86.7%	69.9%	76.2%	28.6%	55.7%	43.5%	47.0%
E. LARC Insertion		*Control mean: 0.0530*				*Control mean: 0.0928*		
Effect	0.0856	0.167	0.113	0.127	0.0667	0.130	0.0934	0.102
	(0.0103)	(0.0193)	(0.0163)	(0.0169)	(0.0117)	(0.0221)	(0.0193)	(0.0199)
% change	162%	315%	213%	240%	71.8%	140%	101%	109%
F. Temporal Coverage in Days		*Control mean: 152*				*Control mean: 304*		
Effect	200	390	254	275	156	304	221	239
	(21.9)	(40.3)	(28.0)	(30.1)	(26.2)	(49.2)	(39.7)	(41.5)
% change	132%	257%	167%	181%	51.2%	99.9%	72.9%	78.5%

Notes: [1]Effective F is from the Montiel Olea and Pflueger (2013) test. P, V, S, C, E stand for interactions of the voucher with phase, visit type, health center site, pre-enrollment contraception and education, respectively. Each point estimate is from a separate regression. Columns 2-4 and 6-8 model first-stage heterogeneity as described in the text. Results are either at 100 days (columns 1-4) or at 26 months (columns 5-8). Standard errors are corrected for heteroskedasticity (Huber 1967, White 1980).

Table 3. Treatment Effects of Voucher Use on the Cumulative Incidence of Pregnancy, Abortion, and Childbirth

	(1)	(2)	(3)	(4)
	At 26 months			
		IV		
	ITT	No first-stage heterogeneity	Theoretical first-stage heterogeneity	LASSO first-stage heterogeneity
A. Pregnancy		*Control mean: 0.208*		
Effect	-0.00956	-0.0186	-0.0329	-0.0274
	(0.00936)	(0.0181)	(0.0124)	(0.0151)
% change	-4.60%	-8.95%	-15.8%	-13.2%
B. Abortion		*Control mean: 0.173*		
Effect	-0.0107	-0.0208	-0.0205	-0.0250
	(0.00693)	(0.0134)	(0.00730)	(0.00999)
% change	-6.16%	-12.0%	-11.8%	-14.4%
C. Birth		*Control mean: 0.0414*		
Effect	0.00798	0.0155	-0.00778	-0.00731
	(0.00735)	(0.0142)	(0.00830)	(0.00947)
% change	19.3%	37.6%	-18.8%	-17.7%

Notes: Each point estimate is from a separate regression. Columns 2-4 model first-stage heterogeneity as described in the text. Standard errors are corrected for heteroskedasticity (Huber 1967, White 1980).