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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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Does Increasing Financial Access to Contraception in the U.S. Reduce Undesired Pregnancies? Evidence from the M-CARES Randomized Control Trial at Two Years Martha J. Bailey, Emilia Brito Rebolledo, Deniz Gorgulu, Kelsey Figone, Vanessa W. Lang, Alexa Prettyman, and Vanessa Dalton NBER Working Paper No. 34400 October 2025

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ABSTRACT

We use a randomized controlled trial to examine how the costs of contraception affect method choice, pregnancy, abortion, and childbirth among 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 available 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 within two years. Subsidizing contraception negatively affected births, but the effect was not significant at two years.

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Each year, around forty percent of over 6 million U.S. pregnancies are unintended, either occurring too soon or when no child was wanted at any point in the future (Kost et al. 2023). Unintended pregnancies are both concentrated among socially and economically disadvantaged groups and also amplify these inequalities, by worsening health, psycho-social, and economic outcomes of women and children (Mohllajee et al. 2007; Guzzo and Hayford 2020; Londoño-Vélez and Saravia 2025; Bailey 2025). The 2022 *Dobbs* decision has increased the potentially negative consequences of unintended pregnancies by allowing states to restrict or ban abortion. Yet evidence regarding how policy could prevent unintended pregnancies remains limited.

The Michigan Contraceptive Access, Research, and Evaluation Study (M-CARES) is a randomized controlled trial (RCT) that examines how subsidizing the full spectrum of contraceptives for low-income women affects method choice and pregnancy outcomes. Between 2018 and 2023, M-CARES randomly assigned vouchers to study participants making *any* method of contraception free or sharply discounted at Title X providers—a federal program providing reproductive health care to millions of low-income Americans. Before the intervention, Title X subsidies made contraception more affordable, but some methods cost over \$1,000 out of pocket for uninsured women. The goal of M-CARES was to remove cost barriers for *all* methods available at Title X providers, allowing women to choose methods best suited to their preferences and circumstances.

This paper documents how reducing the costs of contraception for low-income women affects their choice of method and pregnancy outcomes up to two years later. Our first set of findings shows that the intervention increased the likelihood of buying contraception by 25 percentage points (69%) and raised spending on contraception by \$331 (286%) (local average treatment effects, LATEs, reported). In addition, expected failures among contraceptives purchased fell by 24 percentage points (37%), reflecting both increases in the likelihood of using contraception as well as transitions to more effective methods. Among many changes in method choice, the voucher raised the use of long-acting reversible contraceptives (LARCs, which refers to intrauterine devices, IUDs, and implants) by 12 percentage points (217%). Voucher users purchased methods covering an average of 256 more days, increasing the likelihood of

consistent method use and reducing the need for return visits. These large treatment effects persisted for 26 months, suggesting that contraceptive costs meaningfully constrained method choice in the long run.

Changes in contraceptive choice need not translate into changes in pregnancies, because women without access to their preferred methods may adjust their behavior in other ways (e.g., reducing the frequency of intercourse, using withdrawal). In addition, new method users may discontinue use due to side effects or use them less consistently (e.g., new Depo users may forget follow up injections). Using detailed data on pregnancy, abortion, and childbirth from a follow-up survey and administrative health records, our second set of findings show that, by 26 months, receiving the voucher reduced the cumulative incidence of pregnancy by 16% and the cumulative incidence of abortion by 12%, implying that many of the pregnancies prevented by changes in contraception were unwanted. Although the voucher negatively affected the incidence of births, the effects at endline are not statistically significant at conventional levels.

These results may be surprising for those familiar with the literature concluding that contraceptive costs are a minor barrier to method choice among teens or poor and near-poor populations in the U.S. (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 recent high-quality, randomized controlled trial in Burkina Faso, which finds that offering vouchers for free contraception had a negligible effect on the use of contraception and birth rates over three years (Dupas et al. 2025). Key to understanding these results is that detailed microdata on contraceptive use and pregnancy outcomes allow us to estimate the effects of reducing the costs of contraception for "compliers"—individuals who were financially constrained in their contraceptive method choice who changed their choices due to the intervention. In addition, high-quality administrative microdata reveal large changes in pregnancies that end in abortion, which are severely underreported in surveys (Hood et al., 2022; Kissling and Jackson, 2022; Lindberg et al., 2020), and proved essential to understanding behavioral responses within two years of the intervention.

¹ Within a reproductive justice framework, "compliers" may connote coercion or external control over individual choices. This is not the meaning here. "Compliers" is from the statistical literature referring to individuals who take-up the treatment they are assigned within an instrumental-variables framework (Angrist, Imbens, and Rubin 1996).

This study makes several contributions. First, it provides the first experimental evidence evaluating the importance of financial access to contraception for a broad population of women in the U.S., 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.² Second, M-CARES follows individuals' outcomes using survey and administrative microdata, providing a more comprehensive picture of contraceptive use and pregnancy outcomes. Although quasi-experimental studies have been forced to examine outcomes in different aggregated data or unlinked survey (Kearney and Levine 2009; Boudreaux et al. 2020, 2022; Kim et al. 2023; Hurtado-Acuna and Rendell 2025), this study connects financial access, contraceptive choice, and pregnancy outcomes directly in an instrumental variables framework. Third, M-CARES examines a scalable intervention that is highly relevant for millions of U.S. Title X patients currently receiving subsidized reproductive health services—a low-income population seeking reproductive health care who face significant out-of-pocket costs (Bailey 2024). Accordingly, this trial's results are directly relevant to understanding the effects of eliminating the Title X program (proposed in the 2026 Congressional budget) and provide direct evidence regarding how subsidies for contraception (like those through Title X) affect contraceptive use and pregnancy outcomes. Our experimental design and the richness of our data allow us to provide rigorous evidence on a highly relevant and scalable policy intervention that could reduce unintended pregnancies in the U.S.

² Prior studies have mostly by necessity relied on available quasi-experimental research designs, evaluated interventions that bundle cost changes with other treatments, or focused on specific methods or populations. The St. Louis Contraceptive CHOICE Project provided free access to contraception but lacked a control group limiting causal conclusions. 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, 2020), but providing free condoms in high schools tended to raise teen births unless paired with counseling (Buckles and Hungerman 2018). 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 (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 also produced little to no change in unintended births 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 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).

I. Background and Study Design

In the U.S., the rate of unintended pregnancy is more than five times higher among women in poverty than women with incomes at least two times the poverty level (Guttmacher 2019). One potential contributor is the high cost of reliable contraception. While the Affordable Care Act (ACA) eliminated out-of-pocket costs for contraception for women with health insurance (Becker 2018; Dalton et al. 2020), women without health insurance continue to face prohibitive costs for many methods.

M-CARES's goal is to support participants' reproductive autonomy by providing vouchers making any desired contraceptive lower cost or free, without promoting any particular method. 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 following sections describe the M-CARES trial, which was approved by the Institutional Review Board (IRB-24-5355) at the University of California, Los Angeles, and registered at the American Economic Association RCT Registry (Bailey et al. 2020).

Recruitment

From August 20, 2018, to February 28, 2023, M-CARES recruited participants from 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, which was compensated with \$10.³ If a patient met the inclusion criteria and was willing to participate, a tablet led her through the informed consent, with optional assistance from the NORC 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 at risk of unintended pregnancy, with costs that the intervention could reduce, and whose outcomes could be followed over time. Eligibility required that participants (1) be between the ages of 18 to 35 (inclusive); (2) be physically capable and at risk of having a pregnancy (biologically female and fecund); (3) not be pregnant nor wish to become pregnant in the next 12 months;

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

and (4) face out-of-pocket costs for contraceptives, which the study's intervention could affect. This final criterion excluded patients with health insurance (who the ACA mandates should pay the entire cost of contraception), and patients with incomes below the federal poverty line who faced no costs for contraception for most of the recruitment period. (The latter group briefly faced out-of-pocket costs during our study period, which we later describe.)

As the national policy context and PPMI's policies changed, the study adapted recruitment. Initially, the study restricted eligibility by visit type, including only patients with clinician visits (PPMI advised these were the only patients in the waiting room long enough to complete the screening and enrollment process without disrupting clinic flow). However, as M-CARES learned more about the different groups visiting PPMI, the study team 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 seeking abortions. Our analysis pools these different study populations and models these changes in participant composition.

Over four years of recruitment, Figure 1 shows that 6,037 patients met the inclusion criteria, and 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), which does not differ for voucher recipients and non-recipients (-0.0055, s.e=0.0132). The final sample contains 1,567 participants in the voucher group and 1,605 in the control group for a total of 3,172 individuals.

Study Intervention

Consenting participants were randomly assigned to receive a voucher in a 1:1 ratio, with the voucher amount determined by a patient's out-of-pocket costs. Prior to the intervention, PPMI offered services on a sliding scale with funding from Title X. Patients with incomes at 101-150% of the federal

7,719 patients invited to participate 1,682 patients 6,037 patients eligible for randomization screened out 1,845 unable to enroll 4,192 patients consented and or chose to not participate randomly assigned 2,129 patients 2,063 patients in control group received vouchers 13 patients withdrew 14 patients withdrew **Voucher Group Control Group** Analytic Sample Analytic Sample N = 1,567 (76%)N = 1,605 (76%)completed Y2FU fertility module completed Y2FU fertility module

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.

poverty line (FPL) paid 25% of the costs of services; with incomes at 151-200% of the FPL paid 50%; with incomes at 201-250% of the FPL 75%; and with incomes at 251% or above the FPL paid full price. Due to changes in PPMI pricing and the national policy environment, the cost of services and the sliding scale changed over the course of the study. In each study phase, the M-CARES voucher reduced these out-of-pocket costs as follows.

Phase 1. The voucher covered up to the out-of-pocket cost of a Liletta IUD (the lowest cost IUD which cost about 50% of name-brand devices like Skyla, Paragard, or Mirena) and the medically required pregnancy test and insertion appointment. However, vouchers could be used to cover the costs of any contraceptive for up to 100 days after enrollment over multiple visits. 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 multiple Depo injections, or purchase any other kind of birth control.⁴ 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 effectively 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. ⁵ During this period, PPMI began stocking and inserting Lilettas and reduced the generosity of its 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 (Bailey et al. 2022).

Phase 4. When Planned Parenthood rejoined Title X on September 14, 2021, it again changed its pricing. 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, updating voucher amounts to reflect the reinstated sliding fee scale.

Appendix Table A1 provides more details about each of these phases. To account for the changing patient composition, PPMI pricing, national policy, and the relative generosity of the voucher, our analysis models heterogeneity in voucher effects by study phase.

⁵ 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.

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⁴ The 100-day period allowed returns to PPMI, for example, to get two shots of Depo-Provera (each lasting 90 days) or an IUD. Deadlines also help minimize procrastination, which could lead participants to forget about or lose the voucher (Ariely and Wertenbroch 2002; O'Donoghue and Rabin 1999).

II. Data Sources, Analysis Sample, 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 end point 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); the missing 25 very likely did not purchase services at PPMI.

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 Y2FU 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; (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 allowed some tolerance in matching events without exact matches. For births, we matched events if dates were within 10 days or matched on at least two date components (e.g., month and year matched but the day was different; month and day matched exactly but the year was different) as long as the recording

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⁶ 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 two-year follow-up survey.

aligned with the recorded total number of children born. Miscarriages reported in the survey were recoded as abortions if they occurred in the same month and year as an abortion in the administrative data. In a handful of cases, we manually updated this reconciliation when other information suggested a reporting error in one source (e.g., a misreported year occurring after the survey). 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 at providers other than PPMI. We believe this source of mismeasurement will be fairly small, because all study participants had used PPMI as a source of care by the date of enrollment. Similarly, miscarriages are also 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.

Analysis Sample Characteristics

Our analysis sample includes individuals who could be followed in (1) the Y2FU survey, (2) PPMI medical records (99%), and (3) MDHHS birth records, resulting in a final sample of 3,172 individuals. Table 1 documents balance by voucher receipt (columns 3-4) in pre-specified characteristics, including contraceptive methods used before enrollment, age, race/ethnicity, marital/cohabitation status, income as a percent of federal poverty line, and previous childbearing. Tests reveal a handful of differences that are statistically different from zero, but no more than expected by chance. Consistent with randomization, these characteristics do not jointly predict voucher receipt (F-statistic=0.75, p=0.80). The remaining columns

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

	(1)	(2)	(3)	(4)	(5) Test of difference	
	Title X population	All M-CARES participants	Voucher group	Control group	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 pov	verty line (FPI	(_)				
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	-	0.74	0.73	0.75	0.28	
Birth control pills	-	0.26	0.26	0.26	0.96	
LARC (IUD, implant)	-	0.14	0.15	0.14	0.82	
Injection	-	0.06	0.06	0.06	0.73	
Withdrawal	-	0.02	0.01	0.02	0.04	
Other method	-	0.23	0.23	0.23	0.70	
No method	-	0.26	0.27	0.25	0.28	
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. ¹For M-CARES participants, birth control use refers to the month before enrollment and is asked on the screening survey. Age and fee scale are derived from the pre-enrollment survey.

compare our analytic sample of M-CARES participants to all Title X clients nationwide from the 2018 Health and Human Services (HHS) Annual Report. Relative to the national statistics, the M-CARES sample is less likely to have income below the federal poverty line—largely owing to the fact that M-CARES only recruited individuals in poverty in phase 3. In addition, the M-CARES sample is less likely to identify as Hispanic/Latina and Black, owing to this group's underrepresentation in Michigan and in the areas served by Planned Parenthood health centers participating in M-CARES.

Primary Outcomes

Our research question relates to how financial access to contraception affects pregnancies and pregnancy outcomes. A key step in understanding how financial access to contraception matters is understanding how receiving a voucher affects the choice of contraceptive method. If contraceptive choice is not affected by the voucher, one could reasonably conclude that financial access is not a relevant barrier (Dupas et al. 2025). If, however, receiving a voucher alters method choice, this suggests that individuals are priced out of their preferred method.

To test the effects of the voucher on different dimensions of *contraceptive choice*, we pre-specified six outcomes measured cumulatively by month up to 26 months after the intervention: (1) the dollar value of contraception purchased; ⁷ (2) a binary measure for whether any contraceptives were purchased; (3) a binary measure of LARC insertion (either an implant or IUD); (4) a continuous measure of contraceptive efficacy, defined by the CDC failure rate over one year for typical use of the most effective method purchased (Trussell 2011); and (5) the expected days of coverage of the most effective method purchased.⁸ A sixth measure, the index contraceptive choice, (6) combines these five outcomes to capture the same-signed and multi-dimensional effects of receiving a voucher (Kling, Liebman, and Katz 2007):

$$Index_{i} = \frac{1}{5} \sum_{c=1}^{5} \frac{y_{i}^{o} - \bar{y}^{o,c}}{\sigma^{o,c}}$$

⁷ Dollars have been transformed into real 2025 dollars using the historical consumer price index for all urban consumers (CPI-U).

⁸ Days of coverage is the number of days that a purchased unit covers multiplied by the number of units purchased. Unit coverage is 1095 days for implants, 2190 days for Liletta, 1825 days for Mirena, 3650 days for Paragard, 1095 days for Skyla, 28 days for birth control pills, 90 days for Depo-Provera injections, 1 day for diaphragm, and 28 days for rings.

where y_i^o is the value of outcome o for individual i, $\overline{y}^{o,c}$ is the arithmetic mean and $\sigma^{o,c}$ is the standard deviation of outcome o in the control group, with both defined within study phase. An increase in the index indicates higher contraceptive use, efficacy and coverage.

Measures of pregnancy and pregnancy outcomes include (1) cumulative incidence of pregnancy defined by the date the pregnancy ended, and includes all births, abortions, and miscarriages; (2) cumulative incidence of abortion by date of occurrence; and (3) cumulative incidence of childbirth by date of occurrence, which are examined monthly up to 26 months after enrollment. These outcomes are a subset of our prespecified outcomes for unintended pregnancy and childbearing for reasons explained in Appendix C. (See Appendix Tables A2-A3 for pre-specified analyses.)

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 choice and pregnancy outcomes.

We use standard intention-to-treat (ITT) and instrumental variables (IV) estimators. The ITT specification is

(1)
$$Y_i = \pi_1 Voucher_i + \mathbf{W}_i' \mathbf{\pi}_2 + \varepsilon_{1i},$$

where Y_i is a primary outcome measured in a month after the intervention, and $Voucher_i$ is a binary variable equal to 1 if i is randomly assigned to receive a voucher and 0 otherwise. The vector, W, includes categorical controls for sliding-scale income category, an indicator for prior childbearing, indicators for age, indicators for the most effective contraceptive method used in the month before recruitment, indicators for trial phase, indicators for visit type on the day of enrollment, and health center of recruitment to increase precision. 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 (GMM2S) estimator, which is more efficient than two-stage least squares under heteroskedasticity:

(2a)
$$VoucherUse_i = \delta_1 Voucher_i + \mathbf{W}_i' \mathbf{\delta}_2 + \varepsilon_{2ai}$$

(2b)
$$Y_i = \delta_3 VoucherUse_i + \mathbf{W}_i' \mathbf{\delta}_4 + \varepsilon_{2bi}$$

where $VoucherUse_i$ is a binary variable equal to 1 for individuals who used their voucher and 0 otherwise, and other variables are as defined. Under standard assumptions, δ_3 can be interpreted as the local average treatment effect (LATE) of receiving a voucher among compliers (Imbens and Angrist 1994).

Important differences in the intensity of the intervention across groups and changes in the trial over time recommend modeling first-stage heterogeneity to improve efficiency. We pursue several approaches. Our first, and preferred, specification is theoretically motivated and accounts for changes in national and PPMI policies, recruitment population, naturally occurring differences 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. (14% of women using a LARC before enrollment are less likely to need contraception, whereas the effect of the voucher on individuals who were not using contraception before enrollment could be much larger.) Consistent with this hypothesized heterogeneity, Appendix Table A4 shows that the data strongly reject homogenous first-stage effects across these groups with an F-statistic of 143. We incorporate this heterogeneity by using an interacted first stage as follows,

(3)
$$VoucherUse_{i} = Voucher_{i}\mathbf{Z}_{i}'\boldsymbol{\theta}_{1} + \boldsymbol{W}_{i}'\boldsymbol{\theta}_{2} + \varepsilon_{3i},$$

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⁹ The causal interpretation turns on four main assumptions. First, financial barriers are relevant to women's decisions about which contraceptive method to use. This study is premised on the assumption that financial barriers matter, which is born out of a variety of studies regarding the determinants of healthcare utilization (Finkelstein et al. 2012) as well as prior analyses of the M-CARES data (Bailey et al. 2023). Second, voucher assignment is exogenous, which is consistent with randomization and balance tests in Table 1. Third, voucher assignment is excludable, which requires that receiving a voucher affects outcomes *only* 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. In short, all four assumptions seem very likely to hold in this context. In order to increase comparability with the analysis of contraception, we use a first-stage dependent variable for pregnancy outcomes in equations 2a and 2b that deviates from our pre-analysis plan. This is described in more detail in Appendix C, and the full set of results for the pre-specified analysis are in Appendix Table A3.

where Z captures variation in the intervention's intensity, including indicators for trial phase (see Appendix Table A1); indicators for visit type on the day of enrollment to capture changes in participant population (non-clinician, clinician, and abortion visits); indicators for health center of recruitment; and indicators for most effective pre-recruitment contraceptive method. Note that the interpretation of θ_1 is more complicated than a simple weighted average of treatment effects across groups in Z, due to the inclusion of covariates and because the specification differentially weights groups by their first-stage relevance (Abadie et al. 2024). We supplement our theoretical selection of instruments with a data-driven selection procedure following Belloni, Chernozhukov, and Hansen (2014) and Chernozhukov, Hansen, and Spindler (2015) and implemented using *ivlasso* in Stata (Ahrens et al. 2018). This procedure uses lasso to select instruments among 40 possible voucher interactions, while ensuring that all covariate interactions are included in the second stage. Our tables present the results using post-double selection instrument selection. All specifications correct standard errors for heteroskedasticity (Huber 1967, White 1980).

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 the choices of low-income women about their birth control method or affect their risk of unintended pregnancy. If their contraceptive choices are not driven by financial constraints, receiving a voucher may simply crowd out their own spending rather than altering their choice sets. 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 relationship: 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 state heterogeneity (column 2,6), theoretically motivated first-stage heterogeneity (column 3,7), and LASSO selected first-stage heterogeneity (column 4,8). In all cases, the randomized offer of the voucher easily passes the weak instrument test. Moreover, LASSO selects a similar set of instrument groups as the theoretically motivated specification, but it omits some of the

Table 2. LATEs of the Voucher on Contraceptive Choice

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	At 100 days				At 26 months			
	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 heterogeneit
A. First stage								
Effect		0.512 (0.0120)	0.512 (0.0112)	0.510 (0.0109)		0.512 (0.0120)	0.512 (0.0112)	0.510 (0.0109)
Effective F ¹		1,810	120 Voucher ×	318 Voucher ×		1,810	120 Voucher ×	318 Voucher ×
Instruments		Voucher	P, V, S, C	P, V, S, C, E		Voucher	P, V, S, C	P, V, S, C, E
B. Index of Contraceptive Choice				0.660	0.257	0.502	0.401	0.414
Effect	0.434	0.848	0.656	0.669	0.257	0.502	0.401	0.414
(0.0342) C. PPMI Charges on Birth Control in Dollars		(0.0584) (0.0483) (0.0515) Control mean: 116			(0.0305) (0.0546) (0.0466) (0.0487) <i>Control mean: 266</i>			
Effect	192	374	331	336	141	276	248	254
	(12.2)	(20.6)	(17.6)	(18.7)	(16.1)	(29.0)	(26.8)	(27.6)
% change	165%	323%	286%	290%	53.0%	104%	93.1%	95.5%
D. Any Birth Control Purchase		Control mean: 0.369			Control mean: 0.446			
Effect	0.158	0.309	0.253	0.262	0.124	0.241	0.191	0.199
	(0.0155)	(0.0263)	(0.0233)	(0.0239)	(0.0159)	(0.0281)	(0.0230)	(0.0240)
% change	42.9%	83.9%	68.7%	71.0%	27.7%	54.1%	42.9%	44.6%
E. LARC Insertion		Control me	an: 0.0530				an: 0.0928	
Effect	0.0855	0.167	0.115	0.129	0.0664	0.130	0.0959	0.107
	(0.0104)	(0.0193)	(0.0164)	(0.0173)	(0.0117)	(0.0222)	(0.0195)	(0.0206)
% change	161%	315%	217%	243%	71.5%	140%	103%	116%
F. Contraceptive Efficacy		Control me	ean: 0.343		Control mean: 0.417			
Effect	0.152	0.296	0.242	0.249	0.118	0.231	0.185	0.189
	(0.0145)	(0.0246)	(0.0217)	(0.0223)	(0.0150)	(0.0265)	(0.0214)	(0.0225)
% change	44.2%	86.3%	70.5%	72.7%	28.4%	55.4%	44.4%	45.4%
G. Temporal Coverage in Days		Control mean: 152			Control mean: 304			
Effect	200	390	256	275	155	302	221	252
	(22.0)	(40.6)	(28.3)	(31.8)	(26.3)	(49.5)	(40.0)	(45.5)
% change	132%	257%	168%	181%	50.9%	99.4%	72.9%	82.9%

Notes: ¹Effective 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. ²Control mean for index of contraceptive choice is zero by construction. Each point estimate is from a separate regression. Columns 2-4 and 6-8 model first-stage heterogeneity as indicated. 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).

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). Our discussion focuses on the results for theoretically motivated heterogeneity, but the LASSO instrument selection yields similar results.

We find that relaxing financial constraints led to large changes in contraceptive method purchases in the short run (at 100 days when the voucher expired) and at 26 months. Within the first 100 days, the voucher raised the purchase of any contraception by at least 25 percentage points, or 69% (Table 2D); increased dollars spent on contraceptives by \$331, or 286% (Table 2C); and increased the efficacy of the most effective method purchased by 24 percentage points, or 70.5% (Table 2F), among compliers relative to the control group. Importantly, out-of-pocket expenditure fell by only \$51 (s.e.=4.0, not in Table 1) versus the \$331 dollar increase, implying that receiving the voucher had modest crowd-out effects.

The increase in method efficacy partially reflects that the voucher boosted the use of LARCs by 12 percentage points, or 217% (Table 2E), which are some of the most expensive and effective methods. But this is only part of the story. Appendix Table A5 shows that 27% 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 more than 80% in the control group. Only 2-3% switched to less effective methods, which does not differ between the voucher and control groups (-0.0034, s.e=0.0027).

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, larger 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 at one time. If receiving the

¹¹ Although qualitatively similar to those presented in Bailey et al. (2023), this paper's results differ because it uses all recruited individuals, not just those from phases 1 and 2.

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¹⁰ The increase in the effective F-statistic reflects the fact that LASSO selected the instruments with the strongest first stage. This statistic is not a valid test of the instrument selection in this case, so its inclusion is only for comparison.

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, the voucher increased the number of days covered by purchased methods by 256 (Table 2G). As a result of changes in each of these dimensions, the index of contraceptive efficacy increased by 0.66 standard deviations among compliers (Table 2B).

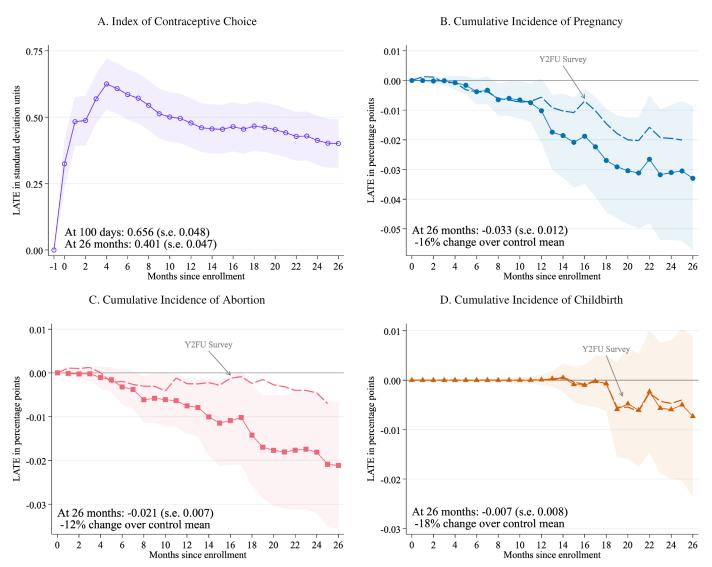
One explanation for these findings is that the voucher hastened purchases, allowing women to switch methods a few months earlier while not fundamentally altering their use of method at endline. But an examination of the effects of the voucher on contraceptive purchases by month over two years rejects this hypothesis. Both Figure 2A and Appendix Figure A1 show large and persistent cumulative effects of the voucher on method choice lasting 26 months. Slight reductions in LATEs indicate that vouchers hastened contraceptive purchases in some cases, but the high persistence of the treatment effects reveals that financial constraints were binding in the longer term, limiting women's ability to purchase their desired method of contraception. 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 due to inconsistent use, method nonadherence, or other pregnancy risk mitigation strategies in the control group.

The results also support this hypothesis. Because pregnancies take time to reach resolution, Figure 2B shows little effect on their incidence in the short term. However, these effects grow to 3.3 percentage points at 26 months among compliers, a 16% reduction relative to the control group (Table 3A, column 7). A large share of this reduction in pregnancies is attributable to a reduction in abortions, which fell by 2.1 percentage points among compliers, or 12% (Table 3B, column 7). The data suggest that missing miscarriages and abortions at other providers would be disproportionate in the control group, leading these LATEs to be understated. Although the effects on the incidence of births are not statistically significant at 26 months (Table 3C, column 7), the pattern of estimates suggests that these effects may emerge later. 12

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¹² Appendix Table A6 shows suggestive evidence of treatment-effect heterogeneity, but the results are imprecise and do not survive corrections for multiple hypothesis testing.

Figure 2. LATE of the Voucher on Contraceptive Choice and Cumulative Incidence of Pregnancy, Abortion, and Childbirth, by Months since Enrollment



Notes: All individuals are observed through 25 months and 3,159 of 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 3. LATEs of the Voucher on the Cumulative Incidence of Pregnancy, Abortion, and Childbirth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
	Entire post-period: 0-26 months				At 26 months					
	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. Pregnancy		Control m	ean: 0.166		Control mean: 0.208					
Effect	-0.00533	-0.0104	-0.0172	-0.0160	-0.00945	-0.0184	-0.0330	-0.0313		
	(0.00480)	(0.00936)	(0.00536)	(0.00749)	(0.00938)	(0.0181)	(0.0123)	(0.0160)		
% change	-3.21%	-6.28%	-10.4%	-9.65%	-4.54%	-8.86%	-15.8%	-15.0%		
B. Abortion		Control mean: 0.152				Control mean: 0.173				
Effect	-0.00632	-0.0123	-0.0102	-0.0117	-0.0107	-0.0209	-0.0212	-0.0248		
	(0.00435)	(0.00850)	(0.00371)	(0.00548)	(0.00695)	(0.0135)	(0.00735)	(0.0108)		
% change	-4.16%	-8.13%	-6.72%	-7.68%	-6.17%	-12.1%	-12.2%	-14.3%		
C. Birth	Control mean: 0.0159				Control mean: 0.0414					
Effect	0.00465	0.00908	-0.00279	0.00111	0.00798	0.0156	-0.00731	0.000507		
	(0.00332)	(0.00650)	(0.00323)	(0.00404)	(0.00736)	(0.0143)	(0.00830)	(0.0104)		
% change	29.3%	57.3%	-17.6%	7.02%	19.3%	37.6%	-17.7%	1.23%		

Notes: Each point estimate is from a separate regression. Columns 2-4 and 6-8 model first-stage heterogeneity as indicated. Results are either for the entire post-intervention period (columns 1-4) or at 26 months (columns 5-8). Standard errors are corrected for heteroskedasticity (Huber 1967, White 1980).

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 critical for revealing substantively large effects on unintended pregnancy and abortion (Abadie et al. 2024). 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 LATE for pregnancies is around ¾ 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 the intervention had little effect at 2 years (Kearney and Levine 2009; Bailey 2012; Lindo and Packham 2017, 2020; Gartner et al. 2022; Kiser et al. 2024; Hurtado-Acuna and Rendell 2025).

V. Increasing Access to Contraception Promotes Contraceptive Choice and Reduces Unintended Pregnancies and Abortions

Our experimental design and rich data provide the most rigorous evidence to date that reducing the costs of contraception for low-income women will reduce both unintended pregnancies and abortions in the United States. Lowering out-of-pocket costs has clear, consequential effects on contraceptive choice: the intervention raises the likelihood that low-income women purchase contraception, choose more effective methods, and purchase methods covering more days. Lowering out-of-pocket costs for contraception also results in sizable reductions in unintended pregnancies and abortions among low-income women after two years. Generalizing these findings implies that the current Title X sliding scale results in significantly higher rates of unintended pregnancy among low-income women and causes roughly 2% more abortions nationally than if contraception were free. ¹³ The bottom line is that many low-income women are priced out of their preferred contraception. Making contraception more affordable would allow more women to use their preferred method rather than abortion and affect the cycle disadvantage in the U.S.

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¹³ See Appendix B for details on this calculation.

VI. References

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