How Subsidies Affect Contraceptive Use among Low-Income Women in the U.S.: A Randomized Control Trial

By Martha J. Bailey[1], Vanessa Wanner Lang[2], Iris Vrioni[3], Lea Bart[2,3], Daniel Eisenberg[4], Paula Fomby[2], Jennifer Barber[4], and Vanessa K. Dalton[6]

[1]Department of Economics and California Center for Population Research, University of California, Los Angeles; [2] University of Michigan, Institute for Social Research, Population Studies Center; [3] University of Michigan, Department of Economics; [4] Indiana University, Department of Sociology and Kinsey Institute; [5] University of California Los Angeles, Fielding School of Public Health and California Center for Population Research; [6] University of Michigan, Department of Obstetrics and Gynecology, Program on Women's Healthcare Effectiveness Research

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Abstract

This paper examines how subsidies affect the use of contraceptives among low-income women seeking reproductive health care in the U.S. Study participants were randomized to receive vouchers for contraception, covering up to 50% or 100% of the lowest-cost, available long-acting, reversible contraceptive method (LARC). Women's choice of method is highly sensitive to price, with the elasticity of LARC take-up ranging from -2.3 to -3.4. The findings imply that a U.S. policy eliminating out-of-pocket costs for Title X women would reduce pregnancies by 5.4%, birth rates by 3.5%, and abortions by 8.1% and save $2.48 billion annually in public expenditures.

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Contact Information

Bailey: Department of Economics, University of California, Los Angeles, 315 Pertola Plaza, Los Angeles, California 90095; marthabailey@ucla.edu; https://sites.google.com/g.ucla.edu/marthajbailey.

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For over half a century, two schools of thought have debated the determinants of childbearing. One emphasizes *demand* factors, arguing that pregnancies and contraceptive use are mainly determined by preferences, wages, and income (Blake 1969, Easterlin 1980, Becker 1981, Pritchett 1994a, b). The other stresses the primacy of factors impeding *access* to contraception, suggesting that barriers such as cost play a critical role in preventing women from achieving their desired childbearing (Harkavy, Jaffe, and Wishik 1969, Ryder and Westoff 1971, Knowles, Akin, and Guilkey 1994).

The implications of this intellectual debate are highly relevant for public policy. Whereas demand factors are difficult for policy to influence in the short term, reproductive health policy could directly and immediately increase access to contraception. They are also relevant for human welfare. In the U.S. today, unintended pregnancies account for 45% of pregnancies overall and occur disproportionately among lower-income women (Finer and Zolna, 2016). Over 40% of unintended pregnancies end in abortion, and births resulting from unintended pregnancies are more likely to have low birth weight and other complications (Mohllajee et al. 2007, Kost and Lindberg 2015). Two thirds of unintended births are funded by public insurance programs, costing over $21 billion (Sonfield and Kost 2015). Quasi-experimental studies also suggest that unintended births contribute to the cycle of poverty by decreasing women's educational attainment, employment, and family resources (Bailey 2006, Bailey, Hershbein, and Miller 2012, Bailey, Malkova, and McLaren 2018, Miller, Wherry, and Foster 2020) and limiting the life opportunities of children (Ananat and Hungerman 2012, Bailey 2013).

Recent quasi-experimental studies suggest that both *demand* and *access* have mattered in the past, but the role of reproductive health policy in reducing unintended pregnancies today is less clear.[1] Quasi-experimental research has improved observational studies in terms of internal validity and (often) larger sample sizes, but unobserved confounders remain a lingering concern. The external validity of these studies

[1] For research on demand factors in the U.S., see Black et al. (2013), Currie and Schwandt (2014), Kearney and Levine (2015), Rotz et al. (2016), Kearney and Wilson (2018), Buckles, Guldi, and Schmidt (2019), Buckles, Hungerman, and Lugauer (2020). For research on the role of access to contraception, see Kearney and Levine (2009), Secura et al. (2010), Bailey (2012), Peipert et al. (2012), Buckles and Hungerman (2016), Lindo and Packham (2018), Sanders et al. (2018), Boudreaux et al. (2020).

for current policy may also be limited, because the Affordable Care Act (ACA) significantly expanded insurance coverage and reduced the costs of contraception for millions of women by requiring health insurance to cover them. In addition, data limitations have precluded quasi-experimental studies from detailed characterizations of subgroup heterogeneity in price sensitivity and from identifying factors that mediate or moderate the effects of price.

The Michigan Contraceptive Access, Research, and Evaluation Study (M-CARES) uses a randomized control trial to isolate the causal effects of price subsidies on the use of contraception among low-income women in today's policy environment. Beginning in August 2018, M-CARES recruited 1,591 low-income women between the ages of 18 and 35 at 12 Planned Parenthood of Michigan (PPMI) clinics. Half of the participants were randomized to receive vouchers that could be used toward their out-of-pocket costs for contraception, and the other half (the control group) received the usual clinical care at usual prices. Vouchers could be used for *any* contraceptive method at PPMI for up to 100 days after enrollment and varied in amount. In the first study phase, vouchers covered costs up to 50% of the lowest-cost available long-acting, reversible contraceptive method (LARC, defined as an intrauterine device, IUD, or subdermal implant). In the second phase, vouchers covered costs up to 100% of the lowest-cost LARC. The study then followed women's use of contraception in PPMI billing records and a follow-up survey to examine how vouchers affected their decisions.

The results show that the contraceptive choices of low-income, uninsured women were highly sensitive to their out-of-pocket costs. Recipients of vouchers were not only significantly more likely to purchase contraception, but they also purchased more effective methods. Whereas the 50% voucher increased LARC insertion by 7 percentage points (pp) over the control mean, making LARCs free increased take-up by 15 pp, or 340% over the control mean. The take-up of LARCs is highly price elastic, ranging from −2.3 for the 50% voucher to −3.4 for the 100% voucher. Receiving a voucher also affected participants' choices of other contraceptives, which resulted in a reduction in the expected one-year likelihood of pregnancy by 17 to 21 pp and increased days of contraceptive coverage by 194 to 339 in the 50% and 100% phases, respectively. These effects persist for two years after study enrollment.

The study's samples also allow an analysis of treatment-effect heterogeneity. Subsidizing contraceptives has large effects on contraceptive efficacy for a variety of pre-specified subgroups, including Hispanic and non-Hispanic women, mothers and childless women, more and less educated women, women who are and are not in serious relationships, women who are more and less religious, and women with and without a usual place for reproductive health care. Receiving a 100% voucher also raised the likelihood that women who did not plan to get LARCs changed their minds. Non-Hispanic Black women are the singular exception to this pattern, exhibiting smaller responses to vouchers in terms of LARC take-up and contraceptive efficacy.

These findings make several contributions to the literature. This study is the first to use a randomized control trial to show that contraceptive use in a developed country is highly sensitive to price. These findings inform a long-standing debate in the social science literature regarding the determinants of childbearing, showing that access can be critical for low-income populations. This study also has implications for recent policy initiatives to reduce funding for reproductive health.[2] Combining this study's results with cost estimates, our analysis implies that a national policy making contraception free for low-income women would reduce pregnancies by 5.4%, birth rates by 3.5%, and abortions by 8.1%, resulting in annual savings of $2.48 billion.

I. Theoretical Framework and Hypotheses

A simple theoretical framework by Michael and Willis (1976) combines elements of the neoclassical model of demand for children (Becker 1960, 1965, Becker and Lewis 1973, Willis 1973) with a model of contraceptive use (Sheps 1964, Sheps and Perrin 1966)—an innovation that relaxes the neoclassical assumption that fertility regulation is costless. Each contraceptive method j is associated with a fixed and marginal price per birth prevented, and pregnancy occurs probabilistically rather than deterministically as in the standard neoclassical model. The number of children is a random variable, and

[2] See discussion of these policy changes here https://www.plannedparenthood.org/planned-parenthood-michigan/healthcare/fees.

women choose a method j to reduce the monthly probability of conception, which is equivalent to choosing an expected distribution of the number of pregnancies, summarized by the first, μ_j, and second moment, σ_j. Women maximize their utility by weighing the marginal costs of preventing pregnancy using different contraceptive methods against the marginal benefit of attaining different expected distributions.

Closely related to the M-CARES intervention, the model distinguishes between the fixed and marginal costs of a contraceptive method. The total cost of using method j to attain an expected pregnancy distribution, μ, is given by $\pi_j = \alpha_j + \beta_j (\mu_N - \mu)$, where μ_N is the mean of the distribution of pregnancies in the absence of any contraception. The fixed cost of using a method j is α_j, which includes any out-of-pocket costs, the fixed cost of going to the doctor, and the cost of learning about a particular method (e.g., overcoming misinformation, personal circumstances, or other external factors). β_j is the marginal cost of preventing a pregnancy using method j. The marginal cost reflects behaviors (e.g., abstinence), inconvenience or discomfort at the time of intercourse (e.g., withdrawal or barrier methods like diaphragms or condoms), and the necessity of returning to fill a prescription (e.g., the pill or injections).

Figure 1A plots an example of total costs for different contraceptive methods and pregnancies prevented. Different methods are optimal for women wishing to avoid different numbers of pregnancies. For instance, if a woman wishes to prevent two pregnancies, then method 1, which entails a small fixed cost but a high marginal cost (like condoms or withdrawal, represented by line Π_1), would be her lowest cost option. One wishing to prevent three births would choose method 2, paying a higher fixed cost but gaining a lower marginal cost. The high fixed but near zero marginal cost of method 4 is similar to LARC methods, which require an upfront, fixed investment of time and out-of-pocket payments, but have the lowest total cost for women seeking to prevent eleven or more pregnancies. The lowest-cost function for achieving an expected number of births before the M-CARES intervention is given by the dashed, lower envelope, or $C(\mu)=\min_j[\alpha_j + \beta_j (\mu_N - \mu)]$.

This model does not include behavioral biases and optimization missteps in the behavioral hazard literature (Baicker, Mullainathan, and Schwartzstein 2015). Yet it clarifies the endogeneity of method

choice to prices and suggests several testable hypotheses for M-CARES. First, the use of contraception is endogenous to both the demand for children (preferences, wages, income) *and* the costs of different contraceptives. Method use itself does not indicate that women are constrained by costs or motivated by other factors. M-CARES uses random assignment to circumvent this complication and compares women expected to have identical demand for children who face different fixed, out-of-pocket costs for contraceptives. Second, reducing the fixed costs of contraception would lead many women to adopt more effective, and lower marginal cost, methods. M-CARES vouchers reduce the fixed costs of contraception and shift the lowest-cost function downward as shown in Figure 1B. For instance, the 50% voucher reduces the fixed costs of contraceptive method 2 from Π_2 to Π'_2; the fixed cost of method 3 from Π_3 to Π'_3; and so forth. The lowest-cost envelope would, therefore, shift such that women seeking to prevent three to nine pregnancies would choose method 3, and women seeking to prevent nine or more pregnancies would choose method 4. Third, receiving a higher valued voucher should have larger effects on take-up of higher fixed cost methods such as LARCs as shown in Figure 1C. For instance, the 100% voucher would reduce the lowest-cost envelope such that women seeking to prevent one to five pregnancies would choose method 3 and women wishing to prevent five or more pregnancies method 4.

II. M-CARES Methods

M-CARES recruited 1,591 women at 12 PPMI clinics to participate in a randomized control trial.[3] The analysis covers the period between August 20, 2018, and November 3, 2019, before Planned Parenthood withdrew from the Title X program, increasing its prices and altering other clinic operations and the trial.

A. Study Enrollment, Randomization, and Sample Inclusion

Study eligibility required that participants were (1) women ages 18 to 35, (2) at risk of unintended pregnancy, (3) facing out-of-pocket costs for contraceptives, and (4) at PPMI for a clinician visit. Criteria

[3] The trial protocol is approved by the University of Michigan's Health Sciences and Behavioral Sciences Institutional Review Board (HUM00132909) and registered at clinicaltrials.gov (NCT03673007). A pre-analysis plan for the first year is available at Open Science Framework and the American Economic Association RCT Registry.

(1) and (2) ensure that participants are legal adults and biologically female, fecund, are romantically interested in men (e.g., do not avoid sex with men due to sexual orientation), and are not pregnant at the time of enrollment or wishing to become pregnant in the next year. Out-of-pocket costs for criterion (3) are determined using PPMI's sliding scale assessed during check-in. PPMI did not charge patients with incomes below the poverty line for services, so this group had no out-of-pocket costs and was excluded from the study. Criterion (4) was logistically necessary, because few patients without clinician visits remained in the waiting room long enough to complete the screening and enrollment process. The study did *not* require that participants be visiting PPMI to obtain contraception.

Professionally trained NORC field interviewers recruited patients in the waiting room to complete a 5-minute screening survey, which was compensated with $10.[4] 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. Consenting participants were randomly assigned in a 1:1 ratio to receive a voucher. After the appointment, interviewers invited participants to complete a baseline survey, and the patient was compensated with $60 for taking the survey in the clinic that day. Participants unable to complete the survey in the clinic received a link by email/text to complete the survey later for $40.

2,556 participants met eligibility criteria (1), (3), and (4) and agreed to take the 5-minute screening survey on a tablet. 1,597 patients met all inclusion criteria, were able to enroll before their appointment began, and elected to participate. 811 received vouchers, and 786 were assigned to the control group. After randomization, two participants withdrew from the voucher group, and four withdrew from the control group. All participants but seven were linked to PPMI billing records. The baseline survey, which contains information for subgroup analyses, achieved a response rate of 79%, which did not differ between the treatment and control groups.[5] Appendix Figure A1 documents enrollment, randomization, and inclusion

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

[5] To evaluate systematic non-response, we regress a binary variable equal to 1 if a participant completed the baseline survey/0 otherwise on voucher receipt and correct standard errors for heteroskedasticity. The estimate of the effect of receiving a voucher on response is 0.0072 (se: 0.021).

in the analysis samples.

Table 1 compares M-CARES participants (column 1) to a nationally representative sample of women ages 18-35 from the 2017-2019 National Survey of Family Growth (NSFG, column 2) and to the 2018 Title X population from the Health and Human Services (HHS) Annual Report (column 3). Relative to the NSFG, M-CARES participants are slightly younger, less likely to be a racial or ethnic minority, and significantly more likely to have lower income ($27,000 per year in M-CARES versus $67,000 in the NSFG) and be uninsured. They are also less likely to use contraception than the national sample of women. The M-CARES sample also differs in expected ways from the national population of Title X patients. While similar in age and use of birth control, the M-CARES sample is less likely to have health insurance and income below than the federal poverty line. In addition, the M-CARES sample is less likely to be Hispanic, owing to this group's underrepresentation in Michigan, and less likely to be Black, owing to this group's underrepresentation in the areas served by participating Planned Parenthood clinics.

Table 1 also documents balance in the intervention (column 4) and control groups (column 5) in pre-specified patient characteristics, including contraceptive methods used before enrollment, age, race/ethnicity, marital/cohabitation status, income as percent of federal poverty line, and previous childbearing. Consistent with randomization, these characteristics do not jointly predict voucher receipt (F-statistic of 1.07, p-value=0.38). Our main specifications include indicator variables for race and education to account for the slight imbalance between these groups that occurred by chance.

B. The Intervention and Voucher Amounts

After randomization, voucher recipients were handed an M-CARES card with their voucher amount. They were also sent a text and email with the same information in case they lost the M-CARES card. Recipients were told that vouchers could be used to pay for *any* contraceptive and related services at PPMI *for 100 days after enrollment*.[6] The voucher could not be used for abortion. The 100-day time limit allowed recipients to return to PPMI to use their vouchers, which was enough time to get two shots of Depo-

[6] "Related services" are those medically required to use a contraceptive. For example, inserting an IUD requires a pregnancy test.

Provera (each lasts 90 days) or have an IUD inserted (which often requires a return visit). We used a deadline to help minimize procrastination, which could lead participants to forget about or lose the voucher (Ariely and Wertenbroch 2002, O'Donoghue and Rabin 1999). Surveyors informed voucher recipients that M-CARES would pay for removal of any device funded by the voucher within one year of enrollment. Participants assigned to the control group received usual clinical care with costs determined by the standard PPMI sliding scale. Participants in both the voucher and control groups were handed a standard information sheet about the effectiveness of different contraceptive methods. Following enrollment, participants proceeded with their pre-scheduled appointments with PPMI clinicians with no involvement from the M-CARES study team.

Voucher amounts were chosen to make the lowest-cost LARC free of charge after applying the PPMI sliding scale and were applied at check-out like a gift card. When the study started, PPMI indicated that the lowest cost LARC was a Liletta IUD, which cost half as much as name-brand devices (e.g., Skyla, Paragard, and Mirena). During the first study phase from August 20, 2018, to March 3, 2019, all contraceptives at PPMI up to the out-of-pocket costs of a Liletta insertion were free for voucher recipients. PPMI charges patients with incomes at 101-150% of the poverty line 25% of the total costs for services; those with 151-200% of the poverty line 50%; 201-250% of the poverty line 75%; and 251% or above the poverty line 100% for the services they receive. Voucher amounts were, therefore, $123, $246, $369, and $492 for the respective income categories (Appendix Table A2). The voucher could also be used for less expensive methods such as birth control pills, injections, rings, and hormonal patches or more expensive methods, such as name-brand IUDs or an implant. However, participants had to pay any costs above the voucher value out of pocket. PPMI's sliding fee scale means that out-of-pocket costs depend on a woman's income relative to the federal poverty line.

In early 2019, the M-CARES team learned that Liletta was only rarely stocked or inserted by PPMI. This meant that—although the voucher was intended to make the lowest cost, available LARC free—the voucher only covered 50% of the cost for *available* IUDs. The study team subsequently increased voucher amounts to cover the costs of the available, name-brand IUDs as of March 4, 2019. The cost of insertion

and related services did not change, so the amount of the voucher almost doubled in the second study phase. Voucher amounts were $223, $446, $669, and $892 for women with incomes at 101-150%, 151-200%, 201-250%, and 251% or above of the poverty line. Our analysis refers to the period before March 4, 2019, as the 50% phase, and the period on or after March 4, 2019, as the 100% phase.

On November 4, 2019, Planned Parenthood withdrew from Title X due to new Trump Administration requirements that organizations providing both family planning and abortion services must physically separate these services in order to receive federal funding, affecting both PPMI pricing and clinic operations. This paper, therefore, analyzes the period from August 20, 2018, to November 3, 2019, which informs the causal effect of providing a 50% and 100% voucher for contraceptives to low-income women with out-of-pocket costs.

III. Outcomes and Research Design

M-CARES combines survey and administrative data to create five pre-specified primary outcomes capturing different dimensions of contraceptive efficacy: (1) the dollar value of services purchased; (2) a binary measure for whether any contraceptives were purchased; (3) a binary measure of LARC insertion; (4) a continuous measure of method efficacy, defined by 1 − the CDC failure rate with typical use of the most effective method purchased (Trussell 2011); and (5) the expected days of coverage of the most effective method purchased.[7] Following Kling, Liebman, and Katz (2007), an index of contraceptive efficacy combines these five outcomes to summarize the *overall* effect of receiving a voucher and limit the number of statistical tests. The index is constructed as the arithmetic mean of its component z-scores,

$$ContraceptiveEfficacy_i = \frac{1}{5}\sum_{o=1}^{5}\frac{y_i^o - \bar{y}^{o,c}}{\sigma^{o,c}}.$$

y_i^o is the value of outcome o for individual i, $\bar{y}^{o,c}$ is the mean of outcome o and $\sigma^{o,c}$ is the standard

[7] 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 (3 years) for implants, 2190 days (6 years) for Liletta, 1825 days (5 years) for Mirena, 3650 days (10 years) for Paragard, 1095 days (3 years) for Skyla, 28 days for birth control pills, 90 days for Depo-Provera injections, 1 day for diaphragm, and 28 days for rings.

deviation of outcome o in the control group.

We estimate the reduced-form effects of receiving a voucher for contraceptives using the following linear specification,

$$Y_{ij} = \tau_1 Voucher_i + \tau_2 Voucher_i \times 100\%Phase + \boldsymbol{X}_i'\boldsymbol{\beta} + \varepsilon_i, \quad (1)$$

where Y_i is the index of contraceptive efficacy or a primary outcome; $Voucher_i$ is a binary variable equal to 1 if an individual i was randomly selected to receive a voucher and 0 otherwise; *100%Phase* is a binary variable equal to 1 between March 4 and November 4, 2019, when the voucher value was increased to cover 100% of the out-of-pocket costs of the lowest price LARC. $\boldsymbol{X}_i$ is a vector of exogenous covariates, including a binary variable for the *100%Phase* to account for changes in the control group between trial phases. Indicator variables for race and education account for slight imbalance in these characteristics in Table 1, and indicators for the patient's income relative to the poverty line, which determine the PPMI sliding scale and level of the voucher, are included to improve precision.[8] Standard errors are corrected for heteroskedasticity (Huber 1967, White 1980).

The "intention-to-treat" (ITT) estimates capture the net, causal effect of providing a voucher to women seeking reproductive health care, which could be used for any contraceptive up to the maximum value of 50% (τ_1) or 100% ($\tau_1 + \tau_2$) of the lowest cost of LARC. We also explore heterogeneity in the ITT effects in equation (1) by stratifying the analysis sample on pre-specified subgroups per our pre-analysis plan.

IV. The Effect of Subsidizing Contraception on Use

A central question of the study is whether out-of-pocket costs affect patients' use of contraception or their choice of method. If patients' choice of method is not driven by financial constraints, voucher dollars may simply crowd out money that patients would have spent in the absence of the intervention.

[8] Our pre-analysis plan explained that the inclusion of covariates "is intended to increase precision by accounting for differences in characteristics between the treatment and control groups that occur by chance" (p. 12). Slight imbalance in race and education characteristics in Table 1 led us to include indicators for race and education. The results without covariates are available upon request.

Table 2 decisively shows that financial constraints play a large role in patients' choice of contraception. In panel A, outcomes are measured after the patient had been in the study for 100 days—the day the voucher expired.

Receiving a voucher affected the likelihood that patients purchased contraceptives and which methods they purchased. For recipients of the 50% voucher, total spending at PPMI increased by 68% ($199 over a control mean of $293) and, in the 100% phase, by 99% ($290 over a control mean of $293). The "++" symbol indicates that the 100%-voucher effect was also significantly larger than the 50%-voucher effect at the 5% level. Recipients of the 50% and 100% vouchers were 35% and 44% more likely to purchase contraception relative to the control group, and they also purchased more effective methods. The purchase of LARCs was 7 pp higher among 50%-voucher recipients and 15 pp among 100%-voucher recipients—the latter effect reflecting more than a three-fold increase over the control mean of 4.5. The treatment effect of the 100% voucher is significantly larger than the 50% effect. Even at half price, high upfront, out-of-pocket costs dissuade many women from choosing LARCs.

Vouchers also induced switching to more effective methods that were not LARCs. The 50% voucher reduced the one-year expected pregnancies by 17 pp and increased the days of contraceptive coverage by 194; the 100% voucher reduced expected pregnancies by 21 pp and increased coverage by 339 days. Appendix Table A3 shows that 34% of 100%-voucher recipients switched to a more effective method versus 25% in the control group; 63% of women in the voucher group stayed on the same method or did not purchase any contraceptives at PPMI compared to 72% in the control group. Only 3% switched to less effective methods in both the control and treatment groups. Summarizing over all five primary outcomes, the index shows that the 50% voucher increased contraceptive efficacy by 0.43 std and the 100% voucher by a staggering 0.69 std.

Table 2B examines the long-term effect of the intervention using data obtained in February 2021. Although not pre-specified, this analysis aims to understand whether receiving a one-time voucher merely hastened contraceptive purchases by a few months or fundamentally altered method choices. The February 2021 data capture cumulative contraceptive use for the 50% phase an average of 27 months after enrollment;

the 100% phase an average of 20 months after enrollment. In both groups, treatment effects remain highly persistent. For the 50% voucher, the treatment effect on purchases and expected pregnancies fell by 1.3 pp, on LARC purchase by 1.5 pp, and on coverage by 43 days. In the 100% phase, the treatment effect on purchases and expected pregnancies fell by 2.1 pp; on LARC purchase by 2.2 pp; and on coverage by 53 days. These modest reductions indicate that vouchers hastened some contraceptive purchases, but the treatment effects are highly persistent. The "set-and-forget-it" nature of LARCs along with the near zero failure rate suggests that the voucher's effect on pregnancy could last from 3 years (e.g., implants) up to 10 years (e.g., Paragard IUD). Also noteworthy is that removals of LARCs are rare and not statistically different in the treatment and control groups.

Section II's model emphasizes how the price of contraception can influence method choices. A second exploratory analysis tests this model prediction by examining whether receiving a voucher altered patients' choices of contraception relative to their plans on the day they enrolled (Appendix Table A4C). Prior to randomization, the screening survey asked respondents what they planned to do during their PPMI appointment that day. If they answered, "get family planning services," the survey asked what methods they "*planned* to get today [emphasis added]." Only 10% of women planned to get a LARC at enrollment. In the control group, 32% of this group followed through with their plan to get a LARC within 100 days. In the 100% voucher group, 76% purchased a LARC within 100 days, a statistically significant 44 pp increase. This finding suggests that around two thirds of the gap between women's plans and their follow-through is explained by the high price of LARCs, with the remaining gap reflecting factors not considered in this study (e.g., medical contraindications). For the 90% of participants who did not plan to get a LARC, only 2% in the control group got one within 100 days whereas 12% of the 100%-voucher group did—a statistically significant effect *five times* the size of the control mean. Making LARCs free allowed more women to follow through on their plans and caused others to change plans.

Table 3 summarizes treatment-effect heterogeneity by pre-specified demographic subgroups (panel A) and baseline characteristics (panel B). We use the index of contraceptive efficacy to increase statistical power to detect same-signed changes in efficacy across the five outcomes for these smaller subgroups.

Appendix Table A4 presents estimates for each of the primary outcomes separately. For the 100% phase, the treatment effect exceeds 0.50 std for all demographic subgroups in panel A with one exception. For Non-Hispanic Black women, the 100% voucher increased contraceptive efficacy by 0.25 std—an effect that is significantly smaller than the 0.69 std increase in efficacy for Non-Hispanic White women (test of difference p-value=0.029). These treatment effect differences appear for the 50% phase as well, which increased efficacy by a statistically insignificant 0.10 std among Non-Hispanic Black women versus 0.50 std among Non-Hispanic White women (test of difference p-value=0.021). Much of this difference in treatment effect reflects the fact that receiving a 100% voucher had no detectable effect on LARC take-up among non-Hispanic Black women (-0.001, se: 0.044, see Appendix Table A4C). This finding is consistent with qualitative and quantitative evidence that Black women are less likely to use contraceptives[9] and use them less consistently than White women (Kusunoki et al. 2016). They also have more negative attitudes about contraception and concerns about the potential side effects of LARCs, including concerns that LARCs could cause harm or increase infertility (Guzzo and Hayford 2012, Barber, Yarger, and Gatny 2015, Alfred and Holmes 2019, Littlejohn in press).

Despite comparably small sample sizes, the data show that financial constraints are important among Hispanic women. For this group, the 50% voucher increased contraceptive efficacy by 0.20 std, whereas the 100% voucher increased efficacy by 0.83 std. Doubling the voucher amount quadruples contraceptive efficacy among Hispanic women, underscoring that financial constraints remain binding even when highly effective methods are half price. Table 3 also shows that the treatment effects on contraceptive efficacy are significantly smaller among women under 26 relative to older women (p-value=0.027 for the 100% voucher) and significantly larger for women who are married or cohabitating compared to single women (p-value=0.006 for the 50% voucher). Treatment effects are smaller (but not significantly so) for less educated women, women without children, and women with lower incomes.

Panel B of Table 3 stratifies on other pre-specified baseline characteristics. The treatment effects

[9] https://www.cdc.gov/nchs/data/databriefs/db388-tables-508.pdf#1 (Accessed 3/15/2021).

are large and significantly different from zero in most of the pre-specified sub-groups but *not* significantly different among these groups. Financial constraints appear similarly binding for women with and without a usual place to obtain birth control, those using highly effective and less effective methods at enrollment, and women with different beliefs that they will achieve their career aspirations. The effects are statistically larger for women in the 100% phase who delayed getting birth control in the previous year relative to those who did not (p-value 0.009), which is consistent with financial barriers playing a role in their choices to delay getting contraception before the trial. In addition, the effects of the voucher were significantly larger for women indicating the strongest desire to avoid childbearing relative to women with less strong feelings (p-value 0.034).

Appendix Table A5 additionally examines heterogeneity by pre-specified measures of beliefs about contraception, religiosity, relationship seriousness, intimate partner violence, life-satisfaction, and general health. Overall, only one subcomponent of 14 tests is significantly correlated with the magnitude of the treatment effect—no more than would be expected by chance. These results suggest that financial constraints affect women with different beliefs and in many different circumstances similarly.

V. Conclusion: Subsidizing Contraception Facilitates Take-Up of More Effective Methods

This study shows that the use of effective contraception among low-income women is highly price elastic. Subsidizing contraceptives allowed women to purchase more effective methods, especially expensive methods like LARCs. These effects attenuate slightly but largely persist for two years after women enroll. A one-time voucher does not just induce intertemporal substitution. Rather, subsidies eliminate a binding credit constraint on women's choices. The study also sheds light on how the generosity of a subsidy—50% versus 100%—matters. Making LARCs half price increased take-up by 113%, whereas doubling the value of the voucher to 100% increased take-up by 342%. Even at half price, the out-of-pocket costs deter many low-income, uninsured women from taking up IUDs and implants. A key finding is that making contraception more affordable could have large effects on the take-up of effective methods and, ultimately, unintended pregnancies.

As with any RCT, it is important to carefully consider external validity, which will be limited to populations seeking reproductive health care. To this end, we reweight the M-CARES sample to reflect the age, race/ethnicity, and income of the national Title X population—all of whom were seeking care at Title X providers (Hainmueller 2012, Fowler et al. 2019).[10] If every Title X patient in the U.S. received free contraception up to the price of the lowest-cost LARC, the reweighted results expect pregnancies to fall by 0.256 pp (versus 0.213 unweighted) and remain 0.230 pp lower 20 months later. An important caveat is that reweighting does not account for treatment effect heterogeneity due to unobserved factors. For instance, treatment effects for Title X patients nationally may differ due to different state reproductive health care programs or policies or states' decisions to expand Medicaid coverage under the ACA (as Michigan did). In addition, these results may misstate the intervention's true effects on pregnancies if (1) low-income, uninsured women obtain contraception from other providers not observed in our data; (2) women do not use the most effective method purchased for one year (we use the one-year method failure rate as a summary metric); or (3) women make other adjustments in their sexual behavior to accommodate their contraceptive method. The first issue is not likely important in practice, because PPMI served 70% of all Michigan Title X patients in 2018, and Title X patients have few other options for affordable care. The quantitative importance of the second and third issues is harder to gauge, so they remain important caveats to the interpretation of the results.

A final exercise uses the reweighted estimates to evaluate the implications of scaling the M-CARES intervention to the U.S.—implementing a federal policy making all contraception free for Title X patients up to the cost of the lowest price LARC (see details in Online Appendix C). Based on the income distribution of Title X patients historically and costs based on voucher use from M-CARES, a national policy would cost $233 million annually—an increase of 81% over current funding levels for the program.[11] Assuming the demand for children remained constant, we apply the reweighted long-run estimate of the

[10] See details in Online Appendix B.
[11] Estimates assume no increases in the use of Title X services due to an increase in funding generosity.

reduction in pregnancies with the 100% voucher (0.230) to the 1.4 million Title X patients nationally with out-of-pocket costs. As with the M-CARES sample, all of the Title X patients sought reproductive health care. The results in Table 2B imply that the policy should reduce pregnancies by 321,361, or 5.4% relative to the estimated 2018 level. We expect 41% of pregnancies to Title X women to result in childbirth, so the policy should reduce births by 130,794, or 3.5%, from the 2018 level.[12] Given that 22% of births to Title X patients result in abortions, the policy would reduce the number of U.S. abortions by 69,735, or 8.1% relative to the 2017 level. Reductions in demand using the methodology in Kearney and Levine (2020) do not alter this paper's conclusions.[13]

In addition to individual welfare gains, reductions in births would have immediate budgetary implications. The costs of unplanned births prevented by a policy of free contraception would likely accrue to Medicaid at $20,717 each, according to the Healthcare Cost and Utilization Project (HCUP). Consequently, a national policy making contraception free up to the cost of the lowest price LARC for Title X patients would reduce public expenditures on Medicaid by $2.7 billion annually, ignoring cost savings in administration and care associated with pregnancy and childbirth not captured in the HCUP estimate. Subtracting $233 million to fund free contraception for Title X patients leaves $2.48 billion in savings *annually*. While the actual reductions in childbirth could vary, this study's estimates of births averted would have to be wrong by an order of magnitude to change the conclusion that a policy making contraception free to Title X patients would pay for itself.

VI. References

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[12] Many of these births may still occur but later, when they are more likely to be planned and women are more likely to have higher incomes and private insurance.

[13] See Online Appendix D.

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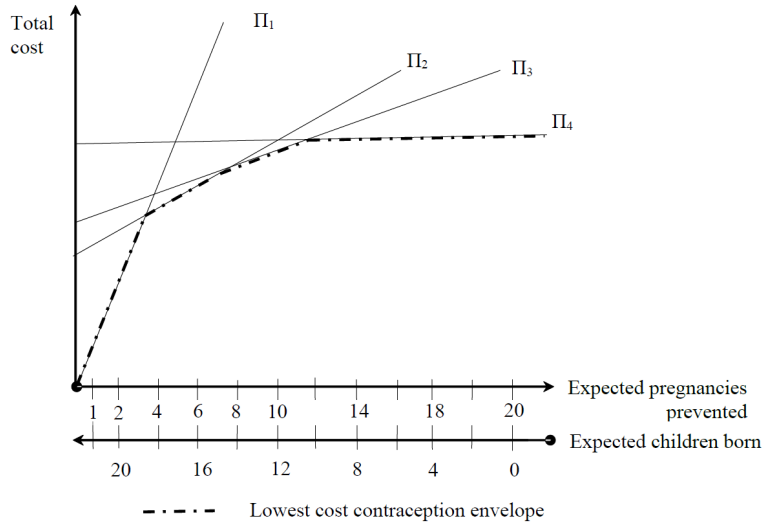
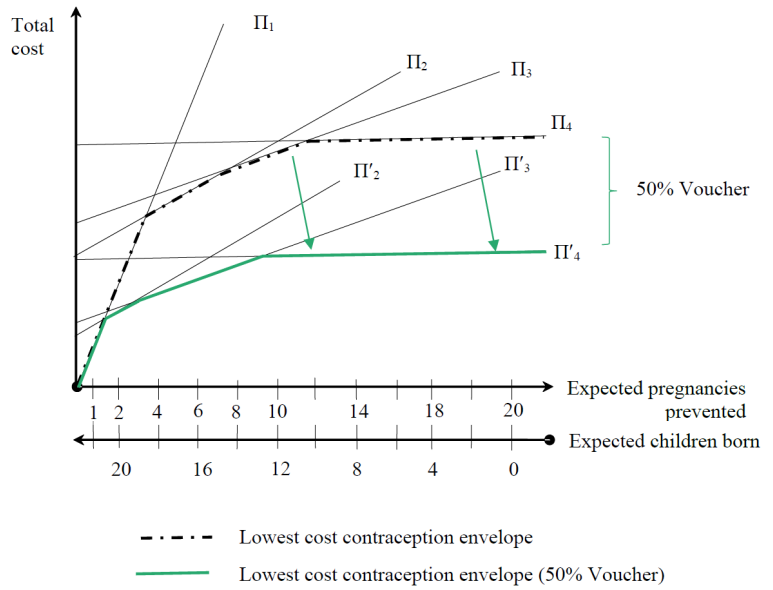
Figure 1. How Out-of-Pocket Costs and Vouchers Affect the Choice of Contraceptives

A. No Intervention



B. 50% Voucher



C. 100% Voucher

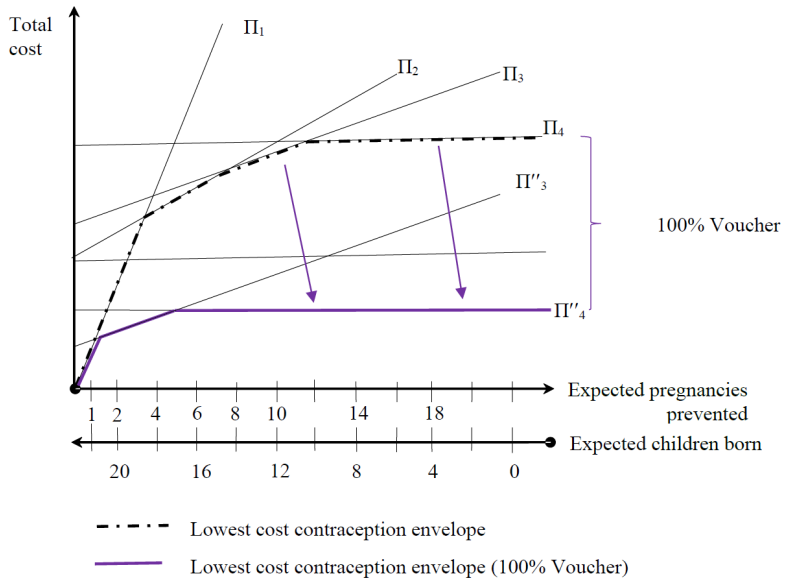


Table 1. Representation of M-CARES Participants and Balance in Characteristics

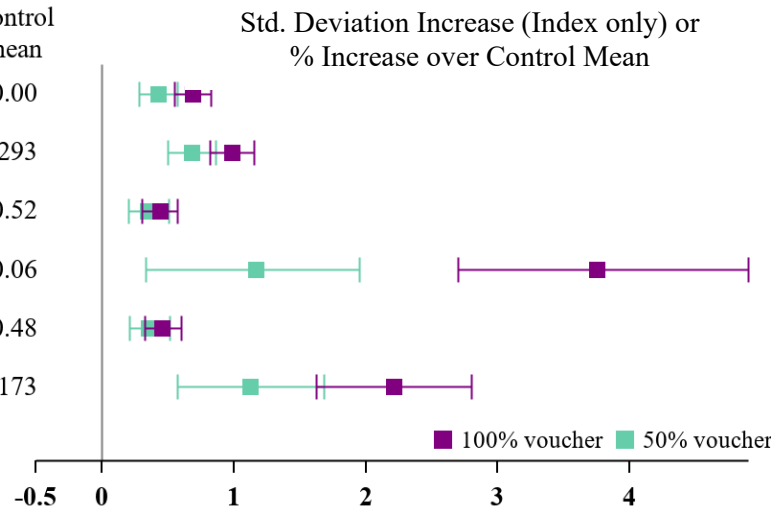
	(1) M-CARES participants	(2) NSFG	(3) 2018 Title X Participants	(4) Voucher	(5) Control	(6) Significance of diff (p-values)
Observations	1,591	2,768		809	782	
Birth control use						
Any birth control	0.79	0.90	0.79	0.79	0.79	0.98
Birth control pills	0.33	0.24	0.25	0.33	0.33	0.87
LARC (IUD, implant)	0.14	0.17	0.17	0.14	0.14	0.83
Withdrawal	0.02	0.06	0.03	0.01	0.02	0.26
Condoms	0.22	0.13	0.16	0.22	0.21	0.75
Other method	0.09	0.32	0.18	0.08	0.10	0.99
No method	0.21	0.10	0.17	0.21	0.21	0.98
Age						
Age 18-19	0.10	0.12	-	0.10	0.10	0.69
Age 20-21	0.16	0.11	-	0.16	0.16	0.57
Age 22-25	0.28	0.23	-	0.26	0.30	0.23
Age 26-29	0.25	0.23	-	0.25	0.25	0.73
Age 30-35	0.21	0.31	-	0.23	0.20	0.08
Race						
Non-Hispanic White	0.69	0.55	0.33	0.70	0.69	0.99
Non-Hispanic Black	0.10	0.16	0.20	0.09	0.12	0.14
Hispanic any race	0.12	0.21	0.34	0.12	0.12	0.98
Other	0.08	0.08	0.13	0.10	0.07	0.09
Marital status						
Single	0.60	0.57	-	0.58	0.61	0.13
Cohabitation	0.31	0.17	-	0.32	0.30	0.36
Married	0.09	0.26	-	0.10	0.08	0.28
Education						
Less than high school	0.02	0.07	-	0.02	0.02	0.82
High school degree	0.18	0.24	-	0.21	0.15	0.02
Some college	0.54	0.37	-	0.50	0.57	0.01
College degree or more	0.26	0.31	-	0.27	0.26	0.39
Previous childbearing						
0 births	0.86	0.63	-	0.86	0.87	0.38
1 birth	0.09	0.17	-	0.09	0.08	0.73
2 births	0.04	0.13	-	0.04	0.04	0.63
3+ births	0.01	0.08	-	0.02	0.01	0.31
Income as % of federal poverty line (FPL)						
Up to 100%	-	0.23	0.67	-	-	-
101-150%	0.46	0.13	0.15	0.46	0.46	0.59
151-200%	0.27	0.11	0.07	0.27	0.27	0.90
201-250%	0.13	0.08	0.04	0.12	0.14	0.53
251+%	0.14	0.46	0.08	0.15	0.13	0.99

Notes: Column 1 presents the M-CARES sample, column 2 the population-weighted means from the 2015-17 NSFG, and column 3 the characteristics of the Title X population. For M-CARES participants, birth control use refers to use the month before enrollment from the screening survey. Age and fee scale are also derived from the screening survey. Race, marital status, education, and previous childbearing come from both survey and, when missing, PPMI data.

Table 2. Treatment Effects of Receiving a 50% or 100% Voucher on Contraceptive Efficacy

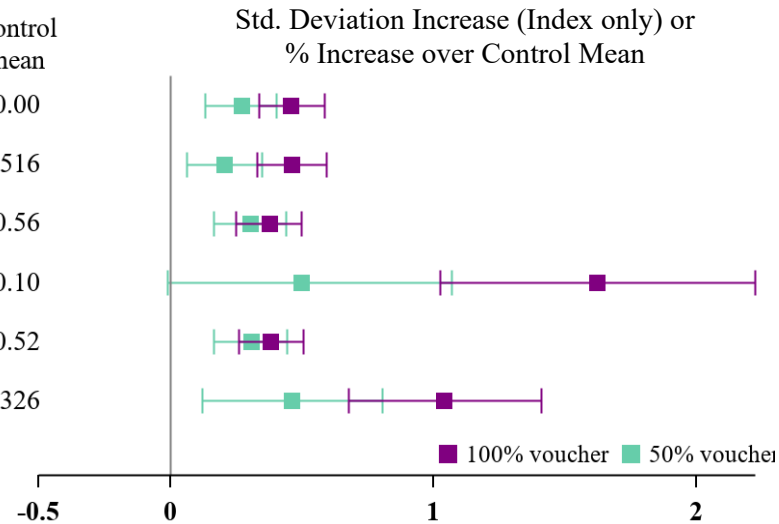
A. Treatment Effects at 100 Days from Enrollment in Std. or as a Percent over Control Mean

	100% voucher effect	Std. error			Control mean	50% voucher effect	Std. error		Control mean
Index of contraceptive efficacy	0.69	0.07	***	++	0.00	0.43	0.07	***	0.00
PPMI charges in dollars	290	25	***	++	293	199	27	***	293
Any birth control purchase	0.22	0.03	***		0.50	0.18	0.04	***	0.52
LARC insertion	0.15	0.02	***	++	0.04	0.07	0.02	***	0.06
1-method failure rate	0.21	0.03	***		0.46	0.17	0.04	***	0.48
Temporal coverage	339	45	***	++	153	194	48	***	173



B. Long-term Treatment Effects at 100 Days from Enrollment in Std. or as a Percent over Control Mean

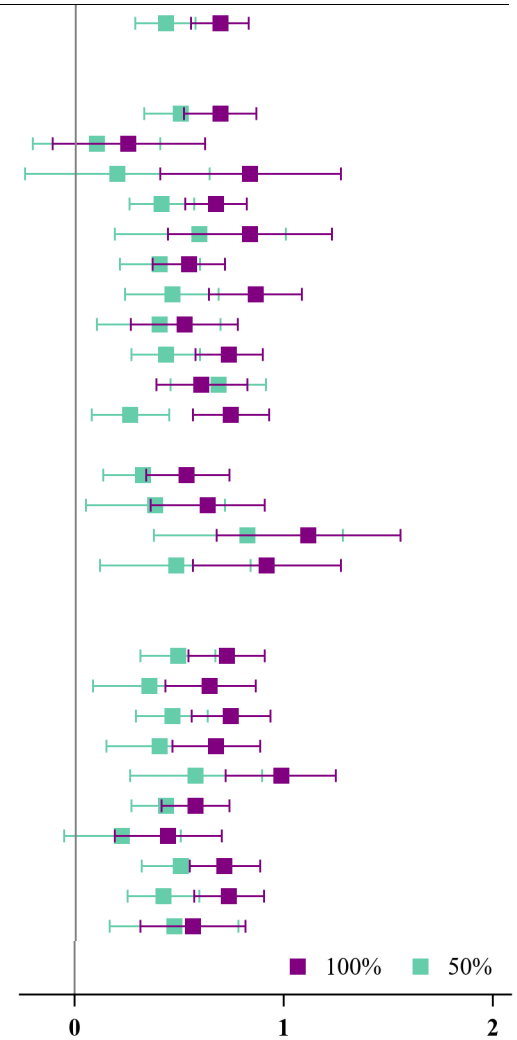
	100% voucher effect	Std. error			Control mean	50% voucher effect	Std. error		Control mean
Index of contraceptive efficacy	0.46	0.06	***	++	0.00	0.27	0.07	***	0.00
PPMI charges in dollars	238	34	***	++	516	123	43	***	516
Any birth control purchase	0.20	0.03	***		0.53	0.17	0.04	***	0.56
LARC insertion	0.13	0.02	***	++	0.08	0.05	0.03	*	0.10
1-method failure rate	0.19	0.03	***		0.50	0.16	0.04	***	0.52
Temporal coverage	286	51	***	+	274	151	57	***	326



Notes: Panel A presents the estimated treatment effects using equation 1 for participants up to 100 days after enrollment when the voucher expired; panel B presents the estimated treatment effects for participants at an average of 20 (100% phase) to 27 months (50% phase) after enrollment. ***, **, * indicate that the treatment effect is significantly different from zero at the 1, 5, and 10% levels, respectively. ++ and + indicate that the 100% effect is statistically different from the 50% effect at the 5 or 10% levels, respectively.

Table 3. Subgroup Heterogeneity in Treatment Effects of Receiving a 50% or % Voucher on Contraceptive Efficacy

	100% Voucher Group					50% Voucher Group				
	N	T	Ste.			N	T	Ste.		
Overall effect on index of contraceptive efficacy	758	0.69	0.07	***	++	559	0.43	0.07	***	
A. Pre-specified demographic groups										
Non-Hispanic White	519	0.69	0.09	***		391	0.50	0.09	***	
Non-Hispanic Black	83	0.25	0.19			58	0.10	0.16		
Hispanic any race	88	0.83	0.22	***	++	69	0.20	0.22		
Women without children	654	0.67	0.08	***	++	482	0.41	0.08	***	
Mothers	104	0.83	0.20	***		77	0.59	0.21	***	
Age<26	419	0.54	0.09	***		297	0.40	0.10	***	
Age≥ 26	339	0.86	0.11	***	++	262	0.46	0.12	***	
Below associate's degree	158	0.52	0.13	***		110	0.40	0.15	***	
Associate's degree or higher	600	0.73	0.08	***	++	449	0.43	0.08	***	
Married or cohabiting	296	0.60	0.11	***		228	0.68	0.12	***	
Single	462	0.74	0.09	***	+++	331	0.26	0.10	***	
Pay scale										
101-150% FPL	335	0.53	0.10	***		267	0.32	0.10	***	
151-200% FPL	212	0.63	0.14	***		145	0.38	0.17	**	
201-250% FPL	100	1.11	0.22	***		70	0.82	0.23	***	
251+% FPL	109	0.91	0.18	***	+	77	0.48	0.18	**	
B. Pre-specified pre-randomization characteristics										
Have a usual place of care for BC	412	0.72	0.09	***	+	343	0.49	0.09	***	
Do not have a usual place of care for BC	343	0.64	0.11	***	+	188	0.35	0.14	**	
Using Tier 1 or 2 method pre-randomization	403	0.74	0.10	***	++	310	0.46	0.09	***	
Not using tier 1 or 2 method pre-randomization	339	0.67	0.11	***		234	0.40	0.13	***	
Delayed getting BC	221	0.98	0.13	***	+	150	0.57	0.16	***	
Did not delay BC	536	0.57	0.08	***		383	0.43	0.08	***	
Positive desire to have a baby	176	0.44	0.13	***		125	0.22	0.14		
Negative desire to have a baby	543	0.71	0.09	***	+	396	0.50	0.09	***	
More likely than not to meet career aspirations	530	0.73	0.09	***	++	399	0.42	0.09	***	
Less likely to meet career aspirations	226	0.56	0.13	***		134	0.47	0.16	***	



Notes: N denotes observations in the indicated subgroup, T the treatment effect, and Ste. the standard error of the treatment effect. The figure on the right plots the treatment effects with the 95% confidence intervals. ***, **, * indicate that the treatment effect is significantly different from zero at the 1, 5, and 10% levels, respectively. ++ and + indicate that the 100% effect is statistically different from the 50% effect at the 5 or 10% levels, respectively.

[CLICK HERE FOR ONLINE APPENDIX]