Family Planning, Now and Later: Infertility Fears and Contraceptive Take-Up

Natalie Bau* David Henning† Corinne Low‡ Bryce Millett Steinberg§

June 10, 2025

Abstract

Early fertility is thought to be one of the key barriers to female human capital attainment in Sub-Saharan Africa, yet contraceptive take-up remains puzzlingly low among women in critical periods for human capital investment. We study a barrier to hormonal contraceptive uptake among young, nulliparious women that, while recognized in the qualitative literature, has not been causally tested: the persistent (incorrect) belief – grounded in medical mistrust and an adverse history of population control policies – that these contraceptives cause later infertility. This belief creates a perceived tradeoff between current and future reproductive control. We use a randomized controlled trial with female undergraduates at the flagship university in Zambia to test two interventions to increase contraceptive use. Despite high rates of sexual activity, low rates of condom-use, and near zero desire for current pregnancy, only 5% of this population uses hormonal contraceptives at baseline. Providing a voucher to visit a local clinic – greatly reducing access costs – only temporarily increases contraceptive use. However, pairing this transfer with information addressing fears that contraceptives cause infertility has a larger initial effect and persistently increases contraceptive take-up over 6 months, reducing pregnancy. This treatment, which was designed to persuasively change persistent incorrect beliefs, reduces the belief that contraceptives cause infertility. Compliers are more likely to cite fear of infertility as the reason for not using contraceptives at baseline. IV estimates indicate that eliminating the belief that contraceptives cause infertility would more than triple contraceptive use.

We gratefully acknowledge Professors Jacob Malungo and Simona Simona at the University of Zambia for facilitating the project and data access. We thank IPA Zambia for their collaboration. We are grateful to Anna Aizer, Nava Ashraf, Prashant Bharadwaj, Pascaline Dupas, Anne Karing, Adriana Lleras-Muney, Sara Lowes, Elisa Macchi, Pauline Rossi, Manisha Shah, and seminar and conference participants at UCSD, UCLA, UChicago, Rice, Princeton, Zurich, Stockholm School of Economics, SOCCAM, IIES, Dartmouth, the North American Winter Meetings of the Econometric Society, and the NBER Children's Meetings for helpful feedback. Alex Zhou, Anna Sander, Stephanie Dodd, and Yohan Wang provided excellent research assistance. The study is registered in the AEA RCT Registry (AEARCTR-0013733). Bau, Low, and Steinberg gratefully acknowledge funding from NSF grant SES2343873.

*UCLA, NBER, & BREAD. Contact: nbau@ucla.edu

†UCLA. Contact: djhenning@ucla.edu

‡Wharton (UPenn) & NBER. Contact: corlow@wharton.upenn.edu

§Brown & NBER. Contact: bryce_steinberg@brown.edu

1 Introduction

In the United States and Latin America, the introduction of hormonal contraceptives was instrumental in allowing women to delay their first birth, complete their education, and join the labor force (Goldin and Katz, 2002; Bailey, 2006; Miller, 2010). With dramatic growth in female education in Sub-Saharan Africa (SSA), where female primary enrollment is nearly universal, and tertiary and secondary enrollment have roughly doubled over the last two decades (World Bank, 2025), we might expect contraceptives to lead to the same freedom to complete education and plan careers. Yet, early fertility remains extremely common in SSA. Across the continent, 60% of women give birth before age 20 (Ahinkorah et al., 2021), and despite high rates of sexual activity, contraceptive use remains low even among young women in critical periods for human capital investment. Early pregnancies in Sub-Saharan Africa during these periods have been shown to reduce educational attainment and labor market outcomes, increase the likelihood of HIV, and even have negative intergenerational consequences (Branson and Byker, 2018; Ardington et al., 2015).

In this paper, we examine whether the incorrect belief that hormonal contraceptives make women permanently infertile may play a role in stalling a Sub-Saharan African "contraceptive revolution" among young women. Although modern hormonal contraceptives do not cause infertility (Mansour et al., 2011; Barnhart and Schreiber, 2009; Girum and Wasie, 2018), qualitative studies document widespread fear of infertility in SSA and suggest that it may hinder the take-up of hormonal contraceptives (Boivin et al., 2020; Engelbert Bain et al., 2021). Mistrust of Western medicine is common in many low-income countries and can derive from both day-to-day experiences and harmful historical experiences (Lowes and Montero, 2021; Sievert, 2024). Traditional beliefs often conflict with modern medical advice (Ashraf et al., 2017),[1] and fear of contraceptives may be particularly salient, as SSA was subject to forced sterilization campaigns and the distribution of harmful early contraceptive devices such as the Dalkon Shield, which *did* cause infertility, miscarriages, and even death (Connelly, 2010). Indeed, Western medicine and a perceived desire to sterilize or control population have become deeply intertwined (Kaler, 2009).[2] Myths about contraception's deleterious effects could also be fueled by the fact that infertility is common in SSA due to maternal morbidity and untreated sexually transmitted infections (Cates et al., 1985; Abebe et al., 2020). If unprotected sex is correlated with both contraceptive use and STI infections, there could be a misattribution of later fertility problems to contraceptives when, in reality, untreated STIs are to blame.

Believing that contraceptives cause infertility translates into meaningful perceived costs of take-up. A large literature outside of economics shows that infertility can be particularly costly in SSA. In addition to

[1] Ashraf et al. (2022) addresses another traditional belief around childbearing in Zambia–that maternal mortality is caused by promiscuity–and finds that targeted informational treatments impact fertility.

[2] The perception that contraception is one of the possible weapons of the West, led to, for example, the banning of Depo Provera in Zimbabwe in 1981 (Kaler, 1998).

well-documented psychological costs,[3] infertility can disrupt marriages through divorce, extramarital affairs, or polygyny (Dierickx et al., 2018; Araoye, 2003). If women rely on husbands for economic support, and men value having children, then limited future fertility could harm women not just personally, but also economically.[4] Thus, young Zambian women may believe that there is a tradeoff between controlling their fertility now (by reducing unwanted pregnancies) and the ability to control their fertility in the future (to achieve a desired pregnancy).

We implement a randomized controlled trial in Zambia with female college students – an increasingly important but understudied population – designed to directly address the fear that hormonal contraceptives cause later-life infertility. Our key treatment combines an informational intervention to dispel myths that contraceptives cause infertility with a voucher that pays women a small sum to visit a local, partner clinic that provides family planning services. The informational treatment, which includes both a presentation on how hormonal contraceptives work and personal stories by facilitators about fertility returning after using hormonal contraceptives, was specially designed to change beliefs in a setting where incorrect beliefs are persistent and statistical information alone is unlikely to be enough. Information was transmitted via personal narratives as well as statistics (Graeber et al., 2024) by a credible source with whom participants are likely to identify (Malmendier and Veldkamp, 2022).

We compare this treatment to an arm that received the voucher only, without the information, as well as an active control. The inclusion of the voucher arm allows us to compare the marginal effect of dispelling fears of infertility to the effects of reducing barriers to access alone. All three arms, including the control, attended a workshop led by the same two young Zambian women and received information about the local, off-campus partner clinic, the fact that it offered contraceptives, and a card that guaranteed free service and no wait-time at the partner clinic. We evaluate the effect of the two treatments on the take-up of hormonal contraceptives[5] and other family planning services, as well as beliefs, using both administrative data from our partner clinic and data on self-reported usage over six months from a high-frequency mobile survey. While both treatments successfully get women "in the door," indicating that the voucher successfully reduced barriers to access, only the treatment addressing fear of infertility caused lasting increases in the use of hormonal contraceptives and reductions in pregnancies relative to the other two arms.

[3] See, e.g, Alhassan et al. (2014), Olarinoye and Ajiboye (2019), Naab et al. (2019), and Donkor et al. (2017).

[4] This is in line with a body of literature in developed countries treating fertility as "reproductive capital" (Low, 2024b; Low, 2024a) and looking at the economic effects of infertility and IVF (Bögl et al., 2024; Gershoni and Low, 2021a; Buckles, 2007; Abramowitz, 2017; Gershoni and Low, 2021b).

[5] Throughout this paper, we will use the short-hand of "hormonal contraceptives" for modern, preventative contraceptive methods that include the pill, injection, and implant, as well as the copper IUD, which is not actually a hormonal contraceptive, but is also associated with fear of infertility. In practice, there is very little IUD use in our study population, so its inclusion as a hormonal contraceptive does not affect our results. We do not include emergency contraceptives in this group, as we are focused on the use of preventative methods and an increase in the take-up of preventative methods could reduce the use of emergency contraceptives.

We thus make a key contribution relative to a literature that has found contraception takeup is often hard to move in Sub-Saharan Africa (for example, in a large-scale, multi-year study, Dupas et al. (2024) find no effects of long-term subsidized access on usage in Burkina Faso). We demonstrate that a population who wishes to *delay* births, rather than change overall fertility, may be more elastic in their takeup, and that fear of infertility, rather than only financial or logistical barriers, may be a key determinant of currently low usage for this group. To do this, we study undergraduates at the University of Zambia (UNZA), Zambia's largest and oldest university, based in the capital city of Lusaka. While there is comparatively little work on college students in Sub-Saharan Africa, 9% of college-aged women enrolled in college in SSA in 2022, and this number is rising rapidly (World Bank, 2022). This population presents an excellent opportunity to examine why young women forgo contraception to delay first births during critical periods, since even a brief postponement could help them complete college. At baseline, only 5% of women in our sample are using a hormonal contraceptive, even though 60% are sexually active, 18% have had sex in the previous two weeks, and *no* women report not using contraceptives because they would be ok with becoming pregnant right now. For comparison, between 40-50% of college-aged women in the US report using hormonal contraceptives (Health Statistics (CDC), 2019; Kavanaugh and Pliskin, 2020).

Our findings also shed light on the contraceptive behavior of a much larger (and growing) population– young, unmarried women in SSA. Historically, both studies of contraceptive use in SSA and interventions by policymakers have focused on married women with children. This is because women married young, and most childbearing happened within marriage. However, in the most recent Zambian DHS, we estimate that 53% of first births were conceived before marriage. Given the importance of the timing of first birth for women's outcomes (e.g., because it conflicts with human capital investment), and the large share of first births that are conceived before marriage, the contraceptive behavior of childless young women should be of particular interest to policymakers. The usage rate of modern contraception in our sample is similar to the rate among sexually-active nulliparious secondary school (3.4%) and college-aged (3.1%) women in the 2018 Zambian DHS, suggesting that our sample's behavior may be more broadly representative of young, childless women. Fear that contraceptives cause infertility is also widespread in our sample. In our control group, 64% of respondents believe at least one form of hormonal birth control causes infertility. Because of these women's high levels of education and the availability of quality medical care in the capital, these beliefs are likely even more prevalent in the general population.

We collect two complementary data sets. The first was collected in collaboration with our partner clinic and provides us with administrative information on visits to the clinic and take-up of services, including hormonal contraceptives. The second is a mobile phone survey that was sent to participants every two weeks for up to 6 months following the intervention. These data allow us to measure contraceptive usage over

time, as well as sexual activity, condom usage, partner characteristics, and pregnancies. While the clinic data ensure that we can measure take-up at the clinic with no attrition, the survey data allow us to check whether effects in the clinic data are driven by substitution, identify persistent effects, and evaluate effects on a wider range of outcomes, including sexual behavior.

We find that the combined voucher and infertility information intervention had a statistically significant, persistent effect on students' hormonal contraceptive usage. Both the voucher and combined treatment were highly successful at encouraging participants to visit the clinic. However, students' behavior at the clinic and afterwards differs dramatically between the two groups. In the administrative data collected during students' initial clinic visits, the combined group is statistically significantly more likely to take-up at least one hormonal contraceptive method relative to the voucher group, unconditional on visiting the clinic (5.4 p.p. vs. 2.6 p.p. relative to the control mean of 2.1%). In the survey data, in the voucher treatment, use of hormonal contraceptives increases in the initial survey, but the effect begins to fade out one month after treatment, and usage levels converge with the control group's by the end of the study. In the combined group, the effects are remarkably persistent, with a 3.5 p.p. (40%) increase in hormonal contraceptive usage on average during the 6 months after the intervention. Over time, the effect of the combined voucher and infertility information treatment is largely driven by students taking up hormonal injections and implants, and the effect does not significantly decline over six months. Thus, the treatment effect cannot be explained by participants simply taking up a hormonal injection initially; for effects to persist, participants would have to receive another shot.[6] While the study was not designed to be powered to detect pregnancy effects – given our focus on barriers to take-up, our key outcome of interest is take-up – when we maximize power by comparing our treatment of interest to the pooled control and voucher arms, we detect meaningful and statistically significant reductions in pregnancy. In addition, there is no evidence of unintended negative consequences for condom usage in encounters, number of sexual partners, or likelihood of having sex. "Risky" sexual behavior appears to be unaffected.

Several additional pieces of evidence support our interpretation that the effect of the combined voucher and infertility information treatment is driven by changing the perceived threat of infertility. First, in our survey data, women in this group are substantially less likely to believe contraceptives cause infertility after the intervention, both initially and after six months, indicating that the intervention did successfully change beliefs. Second, compliers with this treatment are significantly more likely to cite fear of infertility or side effects as a reason for not using contraceptives at baseline. Heterogeneity in treatment effects detected by causal forests is also consistent with this analysis. Third, a complementary follow-up randomized experiment provides suggestive evidence that informing participants that STIs cause infertility also increases the take-up

[6]The contraceptive injection administered at our partner clinic lasts for 3 months.

of STI testing, further underscoring the importance of fear of infertility for healthcare decision-making.

Finally, we quantify the importance of fear of infertility as a barrier to contraceptive use among nulliparious women in SSA. We use the combination of the voucher with infertility information treatment as an instrument for the belief that contraceptives cause infertility and estimate the effect of this belief on take-up, controlling for being in a treatment arm that offered the voucher. Even in the population of women at UNZA, who are likely more informed about contraceptives and have better access to information than the general population, fear of infertility is a very significant barrier to take-up. Eliminating the belief that hormonal contraceptives cause infertility entirely would increase contraceptive use by 20 percentage points, more than tripling the usage in the control group. This effect is all the more striking given that nearly half our study population is not sexually active at baseline. We conclude that – in line with the qualitative literature – fear of infertility is an important barrier to delaying first births in SSA.

This paper makes contributions in three areas. First, while a wide body of qualitative and descriptive work has pointed to the importance of fear of infertility as a barrier to contraception take-up (Boivin et al., 2020; Engelbert Bain et al., 2021; Nalwadda et al., 2010; Otoide et al., 2001; Ochako et al., 2015; Munakampe et al., 2018; S. Castle, 2004; Sedlander et al., 2018; Adongo et al., 2014; Hindin et al., 2014), and some work has sought to quantify its prevalence (Bell et al., 2023), to our knowledge, we provide the first causal evidence for this link.[7] Moreover, we show that the effects of fear of infertility on take-up are quantitatively meaningful. Most studies on contraceptive take-up in low-income countries focus on married women who already have children (Phiri et al., 2015; Scott et al., 2015; Barham et al., 2021; Ashraf et al., 2014; Glennerster et al., 2022; Ashraf et al., 2022; Dupas et al., 2024; Athey et al., 2023), often contrasting the role of preferences for large families (Pritchett, 1994) versus barriers like physical access and intrahousehold disagreements. Hence, in contrast to this paper, most of the literature has focused on total fertility rather than the barriers to delaying first births during critical periods for human capital investment. A smaller literature focuses on young women but has mainly examined barriers related to access and cost and often finds little effect of removing these barriers (Rivera et al., 2001; Bankole and Malarcher, 2010; Shah et al., 2024; Dupas et al., 2024). This paper aims to isolate the effect of fear of infertility on take-up.

Second, this paper adds to the literature on the importance of medical distrust for healthcare take-up. In SSA specifically, quantitative research has identified deep repercussions of colonialism for trust, both in general (Nunn and Wantchekon, 2011) and specifically in medicine (Lowes and Montero, 2021), which can lead to healthcare under-utilization (Blair et al., 2017). Kaler (2009) links medical mistrust with forced sterility in Africa, relating colonial medical campaigns and political and ethnic tensions in more recent

[7] Glennerster et al. (2022) test the effect of radio messaging about contraceptives on usage in Burkina Faso. Their messaging includes information about the effects of contraceptives on fertility, but this messaging is bundled with many other pieces of information, and thus it is impossible to discern the separate effect of the fear of infertility in their setting.

history to the fear that medication carries contraceptive agents designed to inhibit local population growth. Sociological case studies of medical distrust related to fears of forced sterilization in SSA include tetanus vaccines in Cameroon (Feldman-Savelsberg et al., 2000), polio vaccines in Niger (Masquelier et al., 2012) and Nigeria (Yahya, 2007), condoms and AIDS in Malawi (Kaler, 2004), and family planning in Zimbabwe (Kaler, 1998; West, 1994) and South Africa (Brown, 1987). Given the qualitative literature documenting a strong focus on sterility in SSA as a driver of medical mistrust, even for unrelated medical care such as vaccination, quantifying and causally testing the role of fear of infertility in SSA is of particular importance.

Finally, this paper contributes by developing an intervention that persistently changes incorrect beliefs in a setting where these beliefs are likely to be especially sticky. In this setting, merely providing information is unlikely to be enough; incorrect beliefs are grounded in historical experiences and likely further reinforced by and transmitted across peers. Indeed, from an extensive review of the literature, Dupas and Miguel (2017) conclude that information alone often does not change health behaviors or the take-up of health products. Our intervention is carefully designed to induce changes in beliefs. The use of personal stories in our treatment operationalizes the research of Graeber et al. (2024), which shows that stories have much more persistent effects relative to statistical information. This approach also draws on lessons from work on soap operas/television, where stories again have strong and persistent effects on attitudes and beliefs (Jensen and Oster, 2009; Ferrara et al., 2012; Banerjee et al., 2019a; Banerjee et al., 2019b). Similarly, our use of individuals from a similar background to deliver information capitalizes on "information resonance" (Malmendier and Veldkamp, 2022), where information delivered by a source the participant identifies with is more likely to imprint itself on memory and affect decisionmaking. Indeed, despite the difficulty of changing beliefs, the effects of our treatment on beliefs (and contraceptive usage) persist 6 months afterwards with virtually no fadeout.

The paper proceeds as follows. Section 2 describes the context of the study, including the socioeconomic status, degree of under-utilization, and infertility fears in our study population. Section 3 discusses the experimental design, while Section 4 describes the data used to evaluate our interventions. Section 5 presents our empirical strategy, Section 6 describes the results, Section 7 discusses threats to the validity or interpretation of the results, Section 8 provides evidence on mechanisms, Section 9 quantifies the importance of fear of infertility as a barrier to contraceptive take-up, and Section 10 concludes.

2 Context

2.1 Sample Characteristics

University students are an increasingly important population throughout SSA, where the share of women enrolled in college has almost doubled from 5% in 2007 to 9% in 2020 (World Bank, 2023). Our survey data[8] provide key details about the lives of this population (see Table 1 for summary statistics). While college students in SSA are often viewed as elites, our survey suggests that financial disadvantage is common, consistent with growing rates of attendance. Sixty-seven percent of students receive financial aid from the government, and 60% come from outside the relatively wealthy capital of Lusaka. Thirty-two percent are first generation college students, and 14% would have to resort to taking out a loan if they needed 1,000 Zambian kwacha (52 USD) rather than relying on family help, employment income, or savings. Altogether, while some college students are from an urban, highly-educated background, many are socioeconomically vulnerable.

Rates of sexual activity are high but few women use hormonal contraceptives. Table 1 shows that, at baseline, 60% of women have had sex, and 18% had sex in the past two weeks. For a subsample that we follow for 6 months, 58% report having sex at least once during the survey period.[9] Figure 1, in which we report the percent using each type of hormonal contraceptive for both all and sexually active women, shows that overall 2.7% of women are using the pill and 1.5% are using the injection. Very few participants report using the implant or IUD (0.6% and 0.4%, respectively). The percent of sexually active women using hormonal contraceptives is higher (9%) but still relatively low given their risk of pregnancy.

[8] See Section 4 for more details on the data collection.

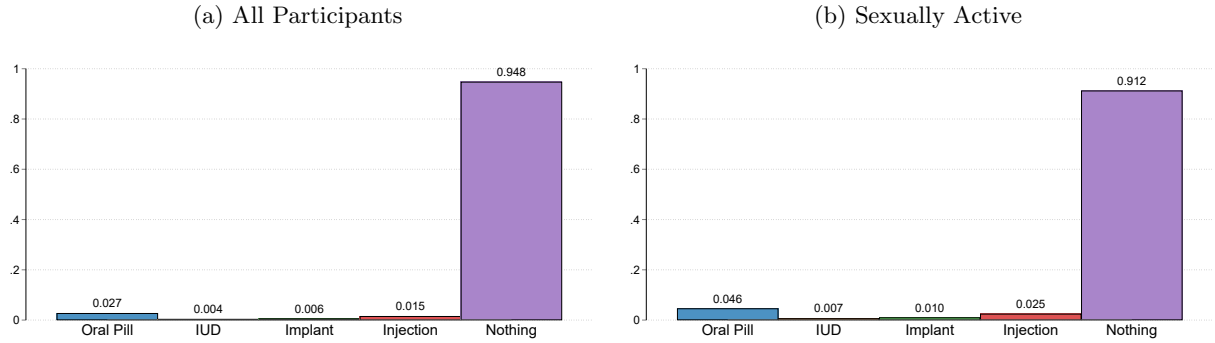
[9] This number differs from Panel B of Table 1 because Panel B includes the full sample, some of whom we observe for a shorter period. We discuss the study design in more detail in Section 3.

Table 1
Summary Statistics From the Survey Data

	Mean (1)	Std. dev. (2)	N (3)
Panel A- At Baseline			
First generation college student	0.319	0.466	1493
Desired level of education: Undergraduate degree	0.167	0.373	1507
Desired level of education: Postgraduate degree	0.831	0.375	1507
From Lusaka	0.396	0.489	1469
Has family in Lusaka	0.940	0.238	1491
On government bursary	0.666	0.472	1499
Would take out a loan if they needed 1,000 kwacha	0.141	0.348	1491
Ever had sex	0.596	0.491	1495
Does not use any contraception (if ever had sex)	0.562	0.496	891
Had sex in the last two weeks	0.177	0.382	1505
Number of partners in the last two weeks (if had sex)	1.034	0.201	266
Ever been pregnant	0.061	0.240	1505
Does not use a hormonal contraceptive	0.948	0.223	1508
Does not use a hormonal contraceptive due to fear of infertility/side effects	0.246	0.431	1408
Does not use a hormonal contraceptive because using condoms	0.180	0.385	1408
Does not use a hormonal contraceptive because ok with pregnancy	0.001	0.038	1408
Knows about the oral pill	0.802	0.398	1503
Believes the oral pill causes infertility	0.548	0.498	1201
Knows about the injection	0.480	0.500	1503
Believes the injection causes infertility	0.463	0.499	713
Knows about the IUD	0.359	0.480	1503
Believes the IUD causes infertility	0.339	0.474	534
Knows about the implant	0.443	0.497	1503
Believes the implant causes infertility	0.421	0.494	655
Panel B- Over Study Period			
Had sex	0.538	0.499	1495
Age gap between partner and student	4.071	3.091	793
Had condomless sex at least once	0.575	0.495	803
Had sex without any contraception at least once	0.487	0.500	803
Number of unique partners	0.765	0.984	1494
Share of sexual encounters that were condomless	0.414	0.493	3204

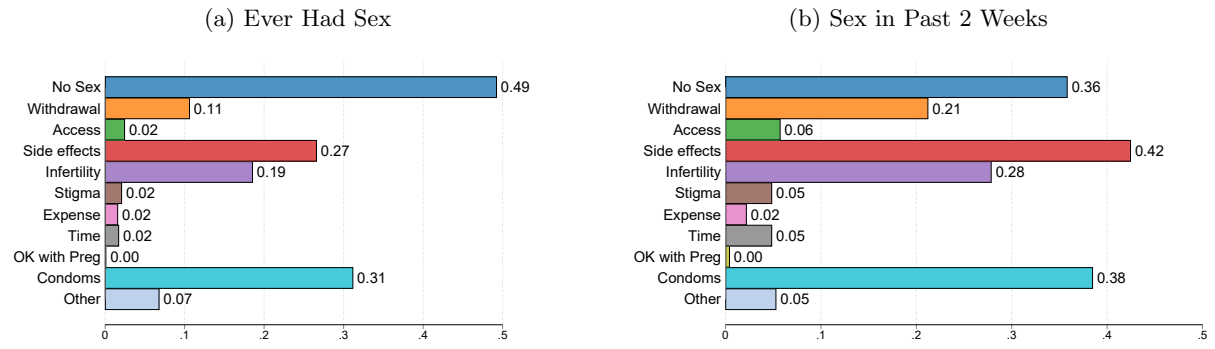
Notes: This table reports summary statistics from mobile surveys of participants. Panel A reports statistics from the baseline survey, and Panel B reports statistics from all the subsequent surveys, which occurred every two weeks. "Had sex," "Had condomless sex at least once", and "Had sex without any contraception at least once" are individual-level indicator variables that equal 1 if a participant reported engaging in these behaviors at least once across the surveys. "Age gap between partner and student", "Had condomless sex at least once", and "Had sex without any contraception at least once" are all conditional on reporting having had sex at least once over the study period. "Age gap between partner and student" are the individual-level averages over data collected on all sexual encounters during the study period. "Number of unique partners" counts the total number of unique partners a woman reports across the surveys. "Share of sexual encounters that were condomless" is the share of all sexual encounters in our survey period where the student reported not using a condom.

Figure 1
Baseline Hormonal Contraceptive Use



Notes: This figure shows the percent of students using a hormonal contraceptive (IUD, shot, oral pill, or implant) in the baseline survey data. Panel A reports results for all participants (N = 1,508), and Panel B reports results for all participants who have ever had sex at baseline (N = 891).

Figure 2
Self-Reported Reasons for the Non-Use of Hormonal Contraceptives at Baseline



Notes: This figure shows reported reasons given by participants for not using hormonal contraceptives in the baseline survey data. This question was asked of all participants who did not report currently using a hormonal contraceptive (IUD, shot, oral pill, or implant) at baseline. Reasons were not mutually exclusive; participants could choose more than one option. Panel A reports results for participants who have ever had sex at baseline (N = 813), and Panel B reports results for all participants who have had sex in the past two weeks at baseline (N = 227). "No sex" indicates that participants self-report that they do not use hormonal contraceptives because they do not have sex frequently enough, "Withdrawal" indicates that a student uses withdrawal or timing to avoid pregnancy, "Time" indicates that the respondent did not have time to access hormonal contraceptives, and "Condoms" indicates participants report that they don't use hormonal contraceptives because they use condoms.

One possibility is that women do not use hormonal contraceptives because they are using barrier methods, which have the advantage of also protecting against HIV and other STIs.[10] However, in Figure 2, we display the reported reasons for the non-use of hormonal contraceptives among those who did not use hormonal contraceptives at baseline. Only 31% of sexually active women report using male or female condoms as a reason for non-use (38% of those who have had sex in the last two weeks). Of these, only 55% actually consistently use condoms in all of the sexual encounters reported in our survey. One reason for the relatively low rate of condom use in this population may be that women have limited bargaining power with partners

[10]This may be particularly relevant in Zambia, where HIV prevalence among women 15–49 is 13% (CDC, 2022).

who are often older and provide financial support.[11] In Appendix Figure A1, we show that condom use is indeed lower with older partners, consistent with the literature (Dupas, 2011).

Finally, while delaying first births does not preclude having a large family, it may be easier to have a large family if women start earlier, potentially contributing to the low take-up of contraceptives. We therefore report descriptive information on whether women desire children, both now and in the future. Consistent with a high desire for children in the future, when asked out of 100 how important it is to have children, more than a quarter of respondents inputted 100, and the 50th percentile was 78. Only 7% of women report not being sure if they want children at all. However, across a variety of questions, almost no women report a desire to get pregnant now. In Figure 2, 0% of women report that they are not currently using contraceptives because they are OK with becoming pregnant right now. In response to questions about the "ideal timing of motherhood," 91% of women that want children say that they want to wait until graduation (or later) to have children. Taken together, these questions demonstrate that women in our sample *do* desire children at some point, but they are not interested in becoming pregnant during their studies.

The descriptive statistics above suggest that women would like to delay pregnancy, even though take-up of hormonal contraceptives (and even condoms) is low. Perhaps the strongest evidence that women are not adequately preventing pregnancies is that many pregnancies occur. At baseline, 10% of sexually active women report ever having been pregnant. Among the women we follow for 6 months, 61 (5.3%) report they are pregnant or tested positive for pregnancy during at least one survey. This is a (very high) hazard rate of reported pregnancies of about 0.9% per month, unconditional on sexual activity. Approximately one-quarter of these pregnancies end in self-reported abortions, while another two-thirds are reported as "false positives," which could reflect either early biochemical miscarriages (a woman has a positive pregnancy test but gets a period soon after), true false positives (possibly due to lower quality tests), or abortions not described as such, for example those induced using traditional abortifacients.[12] Altogether, the data suggest many unplanned pregnancies either result in early births, which are likely to derail young women's educational trajectories, or abortions, which are psychologically costly and expose young women to medical complications, particularly in this low-resource environment (Qureshi et al., 2021).[13]

[11] On average, partners are 4 years older than respondents, and 65% of partners have provided cash or paid school or housing fees.

[12] There is considerable qualitative evidence of women in Zambia using self-induced methods for abortion, including overdosing on chloroquinine, traditional roots and herbs, and ingesting washing powder (Webb, 2000).

[13] While abortion is nominally legal in Zambia, the interpretation of the law is ambiguous (Haaland et al., 2019), some providers will not provide abortions, and historically, there have been hurdles and costs (such as buying one's own anesthesia) to acquiring a legal abortion in a hospital (M. A. Castle et al., 1990). The rate of unsafe abortions is estimated to be relatively high at 7% annually (Lusaka Times, 2021). Even among abortions performed at hospitals, the rate of complications as measured by abortion-related near-misses and mortality in Zambian hospitals is high, even relative to other restrictive contexts (Owolabi et al., 2017).

2.2 Fear of Infertility

Having established the costly underutilization of contraceptives at UNZA, we provide motivating evidence that fear of infertility plays an important role in explaining this puzzle. As seen in Figure 2, 28% of women who have had sex in the last two weeks at baseline and are not using modern contraceptives report fear of infertility as their reason for not doing so (19% of those who have ever had sex). Among these same women, 50% report "side effects" or fear of infertility as a reason for non-use (35% of those who have ever had sex). In our qualitative work, we found that when women say that they fear side effects, they are primarily referring to damage to their reproductive system.[14] Hence, we also interpret these responses as indicative of non-use due to fear of infertility. Table 1 reports the baseline probabilities that women believe that various contraceptive methods cause infertility (conditional on knowing about these methods). The percentages are similar across methods and range from 34% for the IUD to 55% for the oral pill.[15]

To understand drivers of fear of infertility, in Appendix Table A1, we attempt to predict which participants (in the control group) believe that at least one type of contraceptive causes infertility.[16] Basic demographic characteristics do not predict this belief (column 1). Being sexually active marginally significantly positively predicts it, while having heard about contraceptives at an older age and approving of unmarried women using contraceptives decrease it (column 2). However, the adjusted-R^2 is close to 0. In column 3, when we use a LASSO estimator to select predictors from the pool of variables in columns 1–2 and fixed effects for province of birth and program of study, the LASSO does not choose a single variable. Though fear of infertility is widespread, it varies little with demographic characteristics, suggesting that the behavior of the college-going sample may be representative of other groups.

The widespread belief that contraceptives cause infertility will only matter for take-up if infertility is costly. In addition to its personal costs, infertility has economic costs for women if it interferes with economic security via marriage (Baudin et al., 2020; F. E. Okonofua et al., 1997; F. Okonofua, 1999; Dyer et al., 2002; Rouchou, 2013; Dhont et al., 2011). To capture whether participants believe infertility will lead to other negative consequences, at onboarding, we randomly asked half the participants how many married couples out of 10 would still be married in 2 years and the other half of the participants how many couples who could not conceive would still be together. We asked these questions to distinct samples to avoid priming respondents to think fertility matters. On average, respondents believed that 61% of couples would be together when no information was provided on fertility outcomes but that a highly statistically significantly

[14] In Appendix A, we include quotes from focus groups that illustrate this point.

[15] The smaller share for the IUD may reflect the fact that a smaller percent of our sample knew about the IUD, so respondents to this question are particularly well-informed.

[16] We focus on the control group because we use a question that was asked post-treatment in week 2 ("Do you believe any of the following cause infertility? (check all that apply)." We prefer this question because the question asked at baseline conditioned on a respondent reporting that she had heard of a specific method.

different 48% would be together when the couple could not conceive. Appendix Table A2 shows that, among those that were asked about the number of couples staying together if no child was conceived, reporting 1 more couple stays together is associated with a statistically significant 1.5 percentage point (6%) reduction in the likelihood of reporting not using contraceptives due to fear of infertility/side effects (column 1). When there was no information on conceiving, there is no relationship between these two variables (column 2). This is consistent with students who expect infertility to have more negative consequences later in life choosing not to take up contraceptives, which they believe cause infertility, today.

Observational evidence suggests these students' fears that infertility causes marital discord, which would in turn have substantial economic consequences, are indeed reasonable. Based on focus groups showing that women who became infertile often received lower support from their spouses, we fielded a survey between 2014 and 2016 to measure the connection between infertility and marital outcomes with a sample of married women in Lusaka between the ages of 17 and 44.[17] The survey measured whether women had been diagnosed with infertility or experienced medical events tied to infertility, such as hysterectomy, and then measured the quality of their relationships with their husbands, as well as how they paid for daily living expenditures. Appendix Table A3 reports the results. All regressions control for age and age-squared, since infertility increases with age, as well as own overall health, to avoid confounding from negative factors associated with poor health generally, as opposed to reproductive health specifically. With the caveat that these regressions are descriptive, infertility is associated with worse marital outcomes and less economic support from spouses across the board.

3 Experimental Design

In this section, we discuss the design and timing of the workshops, as well as our subsequent data collection and key outcome variables.

3.1 Recruitment, Randomization, and Timeline

We conducted a randomized controlled trial with female, full-time undergraduate students between the ages of 18 and 25 at the University of Zambia (UNZA) in Lusaka, the capital city of Zambia. We contacted 2,701 potential participants at UNZA between August 2022 and April 2023 and invited them to participate in the study. Invitees were recruited using a variety of different strategies, ranging from e-mails to in-person recruitment.[18] During recruitment, students were asked whether they were interested in attending

[17] The survey questions were added to the baseline survey for Ashraf et al. (2022).

[18] From August to September, participants were recruited via e-mail, text message, and calls. Afterwards, we switched to an in-person recruitment strategy on UNZA campus. From late February onwards, we also recruited participants by visiting

a workshop (all study arms attended on-campus workshops) to learn about women's health. Students were informed that the workshop was part of an academic study on women's health and that they would receive airtime for their participation in the workshop and subsequent data collection.[19]

If students expressed interest and passed our inclusion criteria (they were enrolled at UNZA and between 18–25 years old), they were provided with a link to sign up for an on-campus workshop. Workshops took place for all treatment arms, but the content of the workshop depended on the treatment. The link randomly assigned them – using Qualtrics internal randomization – to one of three groups: control, voucher, or voucher + infertility information. Once students clicked the link and entered their information, they could not sign up again. They also could not see what workshop times were available before entering their information, making it impossible to coordinate workshop attendance with friends. Upon randomization, the workshop times for a student's treatment group were displayed, and students were free to pick any available workshop.[20] If none of the times worked for the participant, she received a text message when new workshops were available. Similarly, if a student signed up but missed her workshop, she was invited to sign up for other workshops via text message and follow-up phone calls. Students' identities were verified with student IDs when they arrived at workshops, so students could not attend a different treatment than they had been assigned to or attend multiple workshops.

To be included in the study, invitees had to attend their assigned workshop during which students provided informed consent and baseline data were collected. Out of the 2,701 invitees, 1,508 ultimately attended a workshop and consented to participate in the study: 508 control (56% of those recruited), 486 voucher (54%), and 514 voucher + infertility information (58%). While students were informed that different workshops might have different content and that workshop assignment was completely random, they did not know their assignment until they arrived at the workshop. Therefore, attendance decisions could not depend on treatment assignment, and indeed, differences in attendance rates are not statistically significant. The experimental design and timeline of the study are shown in Figure 3.

There were two recruitment drives whose dates align with two terms of the academic calendar. The first was from July to November 2022, and the second was from February to April 2023. Two drives were necessary to achieve the desired sample size of ∼1,500, which was based on initial power calculations. During the first term (July to November 2022), we recruited 1,170 participants. For this first wave sample, we have six months of follow-up survey data (details below). During the second term (February to April 2023), we recruited an additional 338 women for a total of 1,508. For budgetary reasons, data collection ended for this group at the same time as in the initial sample. Hence, while we have complete clinic data (described below)

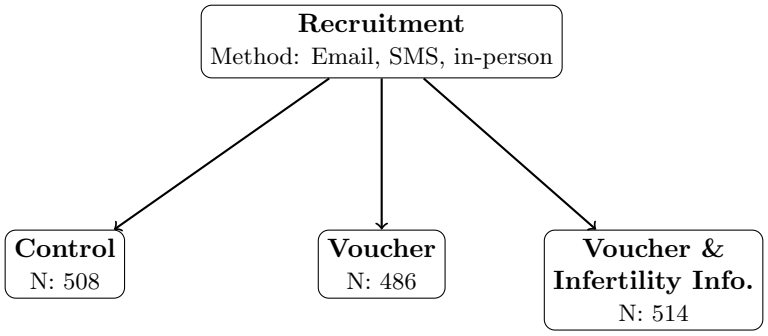
on-campus dormitories.

[19] Airtime can be used to conduct phone calls, send text messages, and buy mobile data.

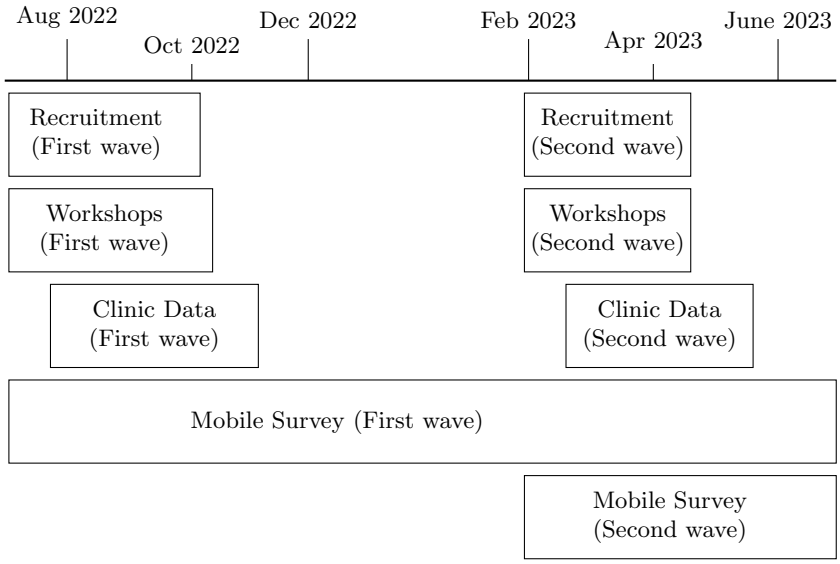
[20] We limited each workshop to 40 sign-ups, but this was not binding in practice.

Figure 3
Experimental Design and Timeline

Panel A: Experimental design



Panel B: Timeline



Notes: This figure provides details on the number of participants in each treatment group (Panel A) and the timeline of the study (Panel B).

for the the second wave sample, we only have survey data for these 338 students for 1.5 months after the workshop.

3.2 Balance

Appendix Table A4 reports average values of baseline observable characteristics for the control group, as well as the coefficients from regressions of those characteristics on the treatment variables (relative to the control). The majority of the baseline variables are balanced, but two characteristics are significantly different at the 5 percent level. First, students in the voucher group are 2 percentage points less likely to be married than students in the control group. This difference is small in magnitude and also reflects a small number of observations since marriage is extremely rare in this sample. Second, students in the voucher + infertility information treatment are 6.7 percentage points less likely to have had sex at baseline. Given that we are running 24 regressions – 12 for each treatment arm – these statistically significant differences are likely to occur by chance. Consistent with this, the p-value for a joint test of whether the covariates predict whether a student is in the voucher group relative to the control is 0.130, and the same F-test for the voucher + infertility information group has a p-value of 0.556. To the extent that any imbalance exists and students who are sexually active are more likely to be interested in contraceptives, this is likely to bias the voucher + infertility information effect downwards. Nonetheless, we will also ensure our main results are robust to different methods of accounting for potential imbalances.

3.3 Treatment Arms

We describe each of the treatment arms below. Every treatment group attended an on-campus workshop during which participants consented and baseline data were collected. All workshops were run by the same team of facilitators, who were employees of our program. *After* baseline data were collected, the facilitators delivered one of the three intervention workshops. The full protocol for all three workshops can be found in Appendix B.

Control Group. Workshop facilitators informed participants that they could access free contraceptives at our off-campus partner clinic, Kalingalinga clinic, which is a 25 minute walk from UNZA.[21] During the workshop, participants were given a small cardboard card that included the opening hours of the clinic, a map with directions, and their study ID. The card is shown in Figure C1 of Appendix C. Students were also told that if they brought the card to the clinic, they would be seen first by a dedicated nurse, skipping the

[21] While there is also a university-run clinic on campus where students can access free contraceptives, we partnered with an off-campus location to allow students to access contraceptives more privately.

usually long waiting times at public clinics in Zambia. Students were given a four week deadline to use their card at the clinic.[22]

Skipping the line is valuable in and of itself, and along with information about the clinic, may have led to contraceptive take-up on its own. Importantly for our study, these aspects (which are also held constant in the other two interventions) provided even control students with an incentive to visit and bring their study card to Kalingalinga clinic specifically, which aided in collecting administrative data on take-up, as we describe below. Therefore, the treatment effects we estimate are the differential effect of the interventions above and beyond any positive effects of providing information on the partner clinic and allowing students to skip the line.

Voucher. Students in the voucher treatment received the same workshop and information card, allowing them to skip the line just like the control students, but were also offered a 80 ZMW (about $4.30) voucher (labeled as being for "transportation") for visiting the partner clinic. Despite the labelling (which is common in our setting), most students walked to the clinic and did not have any transportation costs. To redeem the voucher, participants had to go to the clinic and have their voucher stamped by the nurse, who they would see one at a time. Requiring a one-on-one interaction allowed students to privately ask questions and request services. Once the student saw the nurse, she could decide whether to get family planning information, take-up contraceptives, or simply get her voucher stamped and leave immediately. A stamped voucher could be redeemed for payment from an employee of our program stationed at the clinic. Thus, the marginal costs from time, transportation, and even stigma are greatly reduced by this treatment, while the costs of the services themselves are zero in all groups. Figure C2 of Appendix C shows the voucher.

Voucher + Infertility Information. Students in the voucher + infertility information treatment also received the same information card and voucher as the other two groups, but in addition, we provided them with information intended to counter the incorrect belief that hormonal contraceptives cause permanent infertility. The informational portion of the workshop was based on extensive piloting. Given evidence that dry statical information is often insufficient to change health behavior (Dupas and Miguel, 2017), it was designed to make information salient and relatable and to provide direct evidence that women could bear children after using contraceptives. This portion of the workshop was divided into two parts. In the first part, the trained facilitators explained how hormonal contraceptives work, emphasizing that while hormonal contraceptives stop ovulation, it is temporary. To illustrate this, the facilitator would ask for a volunteer and blindfold her at the front of the room. The facilitator would then take out an orange and ask the volunteer

[22]This was not enforced, apart from for the students who attended the last workshops.

what she smelled. After the volunteer identified the orange, the facilitator held up mint oil between the volunteer's nose and the orange. Asked what she smelled, the volunteer would say "mint," though the audience could see that the orange was still there. Finally, the facilitator removed the mint oil and asked the woman what she could smell, which again was the orange. The facilitators then removed the blindfold and explained that similarly to how the mint oil blocked the orange's smell, hormonal contraceptives block fertility for a short time, but it is always there in the background and returns after removal.

During the second part, the two facilitators told their personal stories. This approach is in line with research showing that information conveyed via stories is more memorable than information conveyed via statistics (Graeber et al., 2024). Both facilitators were selected because they were young women who had used hormonal contraceptives and became pregnant afterwards. In addition, the facilitators were from a similar educational background to the UNZA students to increase information resonance (Malmendier and Veldkamp, 2022). The facilitators emphasized how the use of contraceptives did not prevent them from getting pregnant once they stopped using them. To make their stories salient, they also showed pictures of the children they had following cessation of contraceptive use, and one of the facilitators showed the small scar in her arm where she had the contraceptive implant inserted. Outside of this treatment, since contraceptive use is very uncommon among young women (and is stigmatized so that even women who use contraceptives are unlikely to publicly admit to using them), women rarely witness direct evidence that contraceptives do not hamper fertility.

4 Data

In this section, we describe our two data sources. The first data set was collected in partnership with Kalingalinga clinic by an enumerator based at the clinic. The second data set comes from smartphone surveys completed by participants every two weeks after the intervention.

4.1 Clinic Data

The clinic data were collected at the partner clinic and contain information on the services received at that clinic. As can be seen in Panel B of Appendix Figure C1 in Appendix C, the back of the clinic card included codes for the services that were provided to the students. Appendix Figure C3 reports the translations for the different codes used on the back of the clinic card. We collected information not only on whether students requested any contraceptive services but also the type, as well as information on whether they took up any other healthcare related services, such as sexually transmitted infection (STI) tests and pregnancy tests. When seeing the nurse, students brought their clinic card with them, gave it to the nurse, and the

nurse used it to indicate which services had been provided once the student left the room.[23] The codes used on the clinic card were not known to students. Throughout the day, a study employee collected all the clinic cards and recorded the information electronically.

The clinic data were used to measure two main outcomes: 1) whether students attended the clinic, and 2) take-up of different types of contraceptives during the clinic visit. This provides a short-run measure of take-up since students were incentivized to visit the clinic within 4 weeks of their workshop, and our enumerator was only present at the clinic to record data up to 4 weeks after the last workshop.[24] Importantly, the data are not subject to either social desirability bias or attrition issues that could impact self-reported data. An additional benefit of the clinic data is that they can be used to independently verify our self-reported survey data (described below).

However, the clinic data do have some shortcomings. One concern is that students in the control group are less incentivized to visit the partner clinic specifically, as opposed to another health provider, given that they did not receive a voucher. This concern does not affect the comparison between the voucher and voucher + infertility information treatments, as both these groups were equally incentivized by the cash payment to visit the partner clinic. Another concern is that the voucher simply led participants to substitute to taking up contraceptives from the partner clinic instead of their usual providers. We address both concerns by collecting data on overall contraceptive use in the mobile survey.

4.2 Mobile Survey

In addition to the clinic data, we collected survey data using mobile phone surveys for up to six months, starting at the intervention workshop. All students at UNZA have smartphones, as they are required for schoolwork. The first of these mobile surveys, which served as a baseline, was conducted at the workshop for all participants. Participants were sent the survey link to their mobile phones and asked to complete the survey upon receipt. This allowed the facilitators to ensure that all participants were receiving the links, solve any technical issues, and answer any questions about the survey. In addition, having all participants fill out the survey at the first workshop meant that all participants had to spend some time in a workshop in order to participate, helping to avoid any differential attrition from treatment groups.

Following the workshop, participants were sent a new survey link on their mobile phone every two weeks on Friday evenings.[25] Each survey had a maximum length of fifteen minutes. Participants who did not fill out the survey by Monday evening received a follow-up SMS from the field team, which reminded them to

[23] If a student forgot to bring their clinic card, we provided them with a spare one at the clinic and recorded their study ID on it before seeing the nurse. This helps address the concern that students in the control group may be less incentivized to bring the clinic cards.

[24] Potential second visits by students to the clinic were therefore not consistently recorded.

[25] Piloting had shown that many women respond quickly when they are not in class or engaged in other activities.

complete the survey. If the survey was still not filled out by Wednesday, students received a phone call from the field team reminding them again and helping to troubleshoot any technical issues. Upon completion of each survey, participants received 10 ZMW (about $0.5) of additional airtime as compensation for their time.

Every survey asked about contraceptive use, sexual encounters in the last two weeks, partner information, pregnancy, and clinic visits, forming a panel of data on these key outcomes. Importantly, unlike the clinic data, which measures contraception take up, the survey data measures actual *usage*. For some contraceptives, such as the oral pill, these measures may differ since a young woman can take-up the pill at the clinic but then fail to use it or delay using it until she starts having sex. In addition, different survey rounds included rotating questions on beliefs, attitudes and fears around contraceptives, child-bearing, and marriage, enabling us to explore additional outcomes and the mechanisms underlying the responses we observe.

The survey has several key advantages. First, it allows us to trace out the dynamic response to the workshop over 6 months for our main outcomes of interest. Second, it allows us to test for unintended consequences (such as reduced condom use or increased sexual activity). Third, it allows us to account for contraceptives taken up through other providers than the partner clinic. Fourth, it allows us to explore effects on pregnancy. Finally, the fact that the survey was administered online via Qualtrics and could be completed using a smartphone helped ensure that data collection was confidential and that young women could answer sensitive questions freely, in the privacy of their home, without interacting with an enumerator. In scoping work, we found that participants reported being the most honest in mobile surveys, as opposed to traditional surveys or interviews with in-person surveyors. This is consistent with work on sensitive topics in the US (Kranzler et al., 2004; Kiene et al., 2008; Gibbs et al., 2019). While mobile surveys often have high levels of non-response and attrition, the response rate to our survey was very high. We discuss non-response and attrition in Section 7.2.

5 Empirical Strategy

Using the clinic and survey data, we test whether the voucher and voucher + infertility information treatments affected young women's take-up/usage of contraceptives. We report standard regression equations for outcomes we observe once (e.g., take-up in the clinic data) and graph dynamic estimates for outcomes we observe over time (e.g., contraceptive use in the survey data). Our main estimating equation for outcomes that were only observed once per individual takes the following form:

$$y_i = \beta_0 + \beta^V V_i + \beta^{VI} VI_i + \Gamma \mathbf{X}_i + \varepsilon_i \tag{1}$$

where y_i is the outcome of interest, V_i is an indicator variable for whether student i is part of the voucher group, and VI_i is an indicator variable for the voucher + infertility information group. In our preferred specifications, the vector of controls $\mathbf{X}_i$ only includes indicator variables for use of each type of hormonal contraceptive at baseline (IUD, implant, oral pill, shot, or none) and an indicator variable for the students' recruitment wave. We include controls for baseline use of hormonal contraceptives in all regressions because in our primary specification, it is the outcome of interest, and thus including baseline usage can improve statistical power (Duflo et al., 2007). We keep this control in specifications with other outcomes so that our specifications are consistent across outcomes. To ensure that our results are not driven by our specific choices of controls, for our main outcomes of contraceptive take-up and usage, we also report specifications where we only control for the recruitment wave and where we select controls from the pool of baseline variables using double-LASSO.[26]

For cases where we observe participants' outcomes over multiple survey rounds, we investigate how treatment effects evolve over time. Our dynamic estimating equation takes the form

$$y_{it} = \lambda_0 + \sum_{k=1}^{12} \tau_k^V V_i \times \mathbb{1}\{k = t\} + \sum_{k=1}^{12} \tau_k^{VI} VI_i \times \mathbb{1}\{k = t\} + \delta_t + \Gamma \mathbf{X}_i + \varepsilon_{it}, \tag{2}$$

where t denotes a survey round, $\mathbb{1}\{k = t\}$ is an indicator variable for when $k = t$, and δ_t is a vector of survey round fixed effects. In addition to the dynamic equation, we also estimate an aggregate version of equation (2) in order to estimate average treatment effects throughout the data collection period:

$$y_{it} = \lambda_0 + \phi^V V_i + \phi^{VI} VI_i + \delta_t + \Gamma \mathbf{X}_i + \varepsilon_{it}. \tag{3}$$

Following standard practice, our standard errors are heteroskedasticity robust when we observe students once in a regression and clustered at the student-level when we observe students multiple times. As before, for the main outcomes, we report specifications with and without controls for baseline usage and that select controls using double-LASSO.

[26] Double-LASSO is a machine learning technique that allows for the selection of controls to improve power (best predictors of the outcome variable) and improve balance (best predictors of the treatment variables) in a principled way (Belloni et al., 2014).

6 Results

6.1 Clinic Data: Visits and Take-Up

Panel A of Table 2 reports the effects of the treatments on clinic attendance and the take-up of contraceptives in the clinic data. Both the voucher and combined voucher and infertility information treatments were extremely effective at encouraging students to visit; students in the voucher and voucher + infertility information groups are 53.3 p.p. (285%) and 50.1 p.p. (268%) more likely to visit the clinic than students in the control group (column 1). In Appendix Figure A2, we show treatment effects on participants' reported visits to Kalingalinga clinic in the survey data by survey round. The overall pattern is similar. In survey rounds 1 and 2 (the first month after onboarding), participants report visiting the clinic more than in the control group, but this difference disappears after survey 2.[27] Appendix Table A5 reports the results with parsimonious controls (only recruitment wave) and double-LASSO selected controls. In both cases, they are almost identical to the estimates in Table 2.

In column 2, we report the treatment effects on the take-up of preventative hormonal contraceptives, defined as the injection/shot, jadelle/implant, IUD, and oral contraceptive pills. These are the contraceptives that students typically fear cause infertility, and we include the copper IUD in this group, even though it is not hormonal, because it is long-acting and associated with fears of infertility.[28] We do not include emergency contraception (EC) in our preferred measure because we expect that the take-up of preventative methods may reduce the take-up of emergency contraceptives. Like all columns in this table, column 2 includes the full sample and does not condition on visiting the clinic. Students in the voucher group are 2.6 p.p. (163%) more likely to take up hormonal contraceptives at the partner clinic than students in the control group. These are already substantial treatment effects, more than doubling the share of girls taking up hormonal contraceptives at the clinic. However, the effects are twice as large in the voucher + infertility information group. Students in this group are 5.4 p.p. (338%) more likely to take-up hormonal contraceptives (a doubling relative to self-reported baseline rates of hormonal contraceptive usage). We reject that $\beta^V = \beta^{VI}$ at the 5% significance level. The last column of Panel A reports the effect on take-up of any contraceptive (condoms, EC, hormonal). This results in higher rates of take-up in both groups, though the size of the gap in take-up is stable. Panel B shows that the change in coefficients is mainly driven by the take-up of condoms, which

[27] The magnitudes of the treatment effects in this figure are smaller than the effect on visiting the clinic in the administrative clinic data shown in Table 2 (column 1). Adding up surveys 1 and 2, since most participants went to the clinic only once, yields 12 and 11 percentage point increases in the voucher + infertility information and voucher treatments. We attribute the difference in magnitudes to the wording of the question on the survey. It asked, "Have you gone to Kalingalinga clinic for family planning services in the past 2 weeks?" Participants who did not actually take up any services, but only went to collect their reimbursement, would likely have answered "no" to this question.

[28] In practice its inclusion in the measure of the take-up of hormonal contraceptives is irrelevant since no students take up IUDs at the clinic.

we do not expect to vary between the two treatments (and in fact, does not) since participants do not fear that condoms cause infertility.

Panel B of Table 2 reports the treatment effects on the take-up of different types of contraceptives. We omit a column for the IUD since there is zero take-up in the clinic data. We find that the difference in the take-up of hormonal contraceptives between the voucher and voucher + infertility information groups is driven by injections and oral pills (columns 1 and 2). This is consistent with the fact that take-up of the implant in the clinic data is very low. To ensure that hormonal contraceptive take-up did not crowd out condom demand, we show take-up of condoms in column 4 of Panel B, which is the same across the treatment groups. Finally, column 5 of Panel B reports the effect of the treatments on the take-up of emergency contraceptives. This is also similar across treatments, consistent with the idea that the effects of the voucher + infertility information treatment on EC take-up are ex-ante theoretically ambiguous. This treatment may make students perceive EC as less costly but also may lead to substitution away from EC to preventative hormonal contraceptives.

To summarize, both treatments encourage students to visit the clinic and take-up contraceptives while there. However, the voucher + infertility information treatment has twice as large an effect on students taking up hormonal contraceptives compared to the voucher.

6.2 Survey Data: Contraceptive Usage Over Time

We next investigate the dynamic effects of the interventions on contraceptive usage. Measures of contraceptive usage are coded based on a question asked in every survey about what method (if any) students were currently using. Using the group we follow for 6 months, Figure 4 shows the percent of participants in each treatment group who report using a hormonal birth control method, normalized to the baseline level of usage in each group.[29] In the first two surveys after the workshop (at two and four weeks), both the voucher and voucher + infertility information group increase usage by around 3 percentage points (in week 2) and 5 percentage points (in week 4).[30] After this point, the usage rates begin to diverge. In the voucher group, usage declines, and by the second half of the survey period, usage is only 2-3 percentage points higher than baseline and indistinguishable from the control. For the voucher + infertility information group, however, usage rates increase and remain about 6 percentage points higher than baseline for the remainder of the

[29] The non-normalized version of the figure, shown in Appendix Figure A3, is similar.

[30] While highly correlated, measures of initial take-up at the clinic and usage in the survey data need not be mechanically the same for several reasons apart from measurement error. First, as discussed above, take-up and usage measure different things and may differ for the oral pill, one of the main contraceptives that women take-up. Second, while women were told they had four weeks to visit the clinic, in practice, for most of the sample, vouchers were still redeemed and data were still collected if they visited later. Thus, while the clinic data were usually collected during the same period as the first two rounds of surveys, this is not always the case. Third, clinic data only captures take-up that occurs at the partner clinic, as opposed to usage, which includes contraceptives from other sources. Finally, the data in Figure 4 are restricted to the first recruitment round to show the evolution of take-up for the same sample over 6 months, while the clinic estimates include both rounds.

Table 2
Effect on Clinic Attendance and Contraceptive Take-Up (Clinic Data)

Panel A—Effect on Clinic Attendance and Contraceptive Take-Up

	Visits Clinic	Takes up Hormonal Contraceptives	Takes up Condoms, EC or Hormonal
	(1)	(2)	(3)
Voucher	0.533***	0.026**	0.074***
	(0.027)	(0.010)	(0.018)
Voucher & Infertility Info	0.501***	0.054***	0.100***
	(0.027)	(0.012)	(0.019)
N	1508	1508	1508
Adjusted R-squared	0.238	0.021	0.016
Control mean	0.187	0.016	0.053
P-value of $\beta^V = \beta^{VI}$	0.279	0.042	0.226

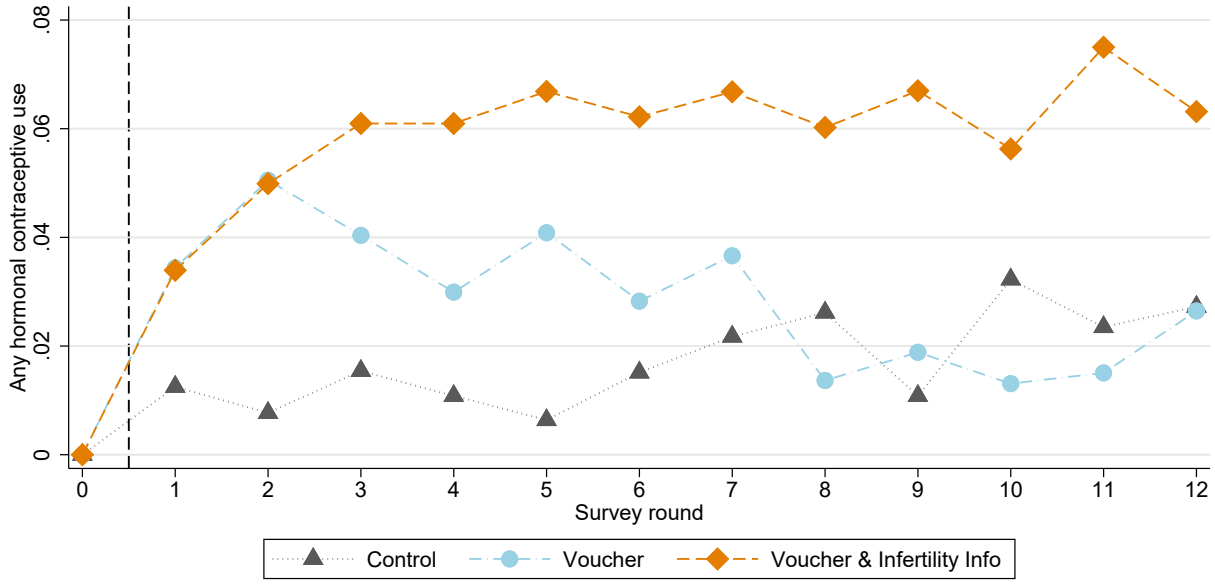
Panel B—Effect on Contraceptive Take-Up, by Type

	Hormonal Methods				
	Oral Pills	Injection	Implant	Condoms	Emergency Contraceptives
	(1)	(2)	(3)	(4)	(5)
Voucher	0.019**	0.003	0.003	0.044***	0.012
	(0.008)	(0.004)	(0.005)	(0.013)	(0.010)
Voucher & Infertility Info	0.038***	0.016**	0.003	0.044***	0.012
	(0.010)	(0.006)	(0.005)	(0.013)	(0.010)
N	1508	1508	1508	1508	1508
Adjusted R-squared	0.012	0.015	0.009	0.010	0.002
Control mean	0.006	0.004	0.006	0.024	0.018
P-value of $\beta^V = \beta^{VI}$	0.105	0.069	0.970	0.993	0.964

Notes: Panel A reports the effect of each of the treatments on visiting the partner clinic and contraceptive uptake. The estimates are from running equation (1) on the clinic data. All regressions include fixed effects for baseline hormonal contraceptive usage, as well as an indicator for whether a student was in the second recruitment wave. "Takes up Hormonal Contraceptives" is an indicator variable for whether a student requested any of the following: injection, implant, IUD, or oral contraceptive pills. While emergency contraception is hormonal, it is not a preventative method, and the effects of the treatment on it are theoretically ambiguous, so we do not count it as being part of this category. "Takes up Condoms, EC or Hormonal" is defined similarly to "Takes up hormonal contraceptives" but also includes condoms and emergency contraceptives. Panel B reports the effect of each treatment on contraceptive uptake, broken down by type. It is estimated with equation (1) in the clinic data and includes the same set of controls as Panel A. Standard errors are heteroskedasticity robust in all specifications.

survey period. Usage in the control group increases slightly over time as well, perhaps suggesting that our active control protocol, which informed participants of the services at Kalingalinga clinic and gave them a no-wait card, may have had an effect, or perhaps that there were some spillovers across groups over time. These results confirm that the increases in take-up we see in the clinic data do translate into increases in usage and do not just capture substitution across providers. Indeed, in Appendix Table A6, we show that while participants reported increased visits to our partner clinic, Kalingalinga, they did not reduce visits to other clinics.

Figure 4
Hormonal Contraceptive Use by Survey Round and Treatment Group



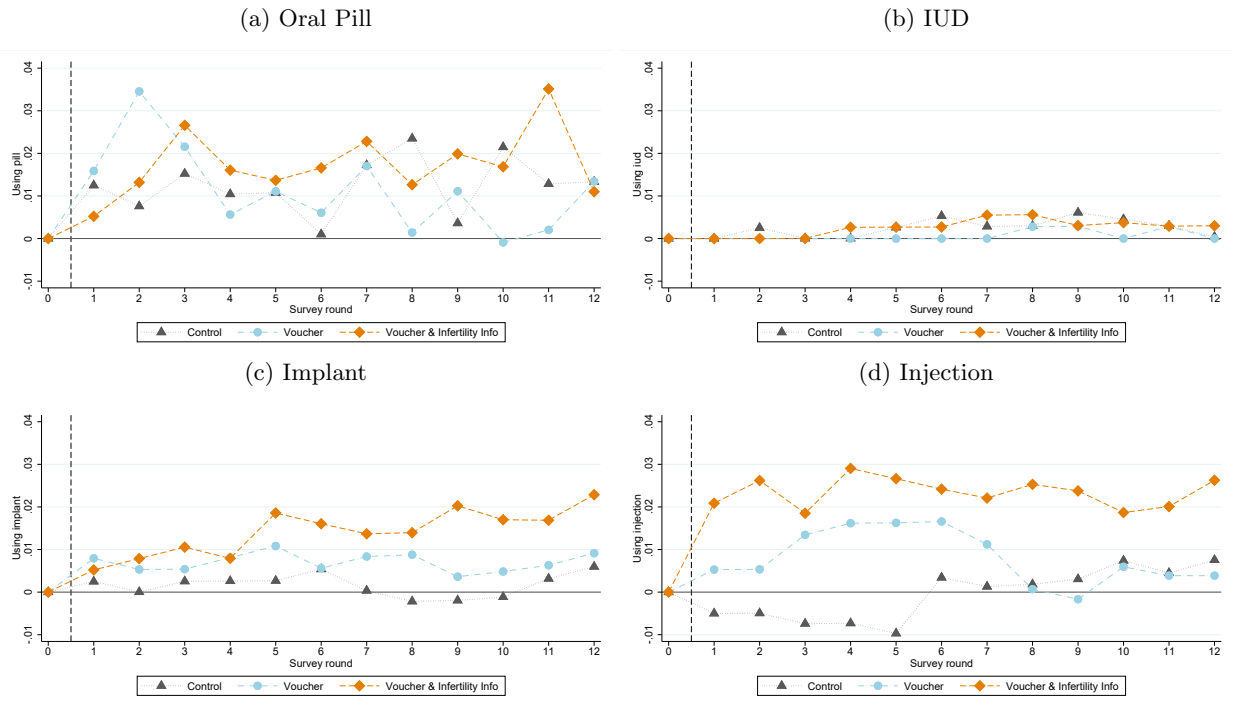
Notes: This figure plots the average use of hormonal contraceptives reported in the mobile survey by treatment group and survey round. Usage rates are normalized to baseline usage in each group. Hormonal contraceptives include the oral pill, shot, implant, and IUD. Survey rounds occur every two weeks. To explore the effect over time, we restrict to the first recruitment wave, which was followed for 6 months.

To complement Figure 4, in Appendix Figure A4, we report estimates of equation (2) with an indicator variable for using any hormonal contraceptive as the outcome variable and report confidence intervals around the estimates of the treatment effects. The voucher group is only statistically different from the control in the second survey (week 4), while the voucher + infertility information group is statistically distinguishable from the control in the majority of the twelve surveys.

In Figure 5, we show usage over time separately by type of contraceptive. While the study is not powered to distinguish between different types of contraceptives in each week, these figures suggest that the differences in long-run usage between the voucher + infertility information group and the other two arms are primarily driven by injections and implants. Appendix Figure A5 shows the coefficients of equation (2) separately by

type. Coefficients for the voucher + infertility information group are consistently positive, particularly for the implant and injection, though not consistently statistically significant.

Figure 5
Use of Hormonal Contraceptives by Type and Survey Round



Notes: This figure plots the average use of hormonal contraceptives reported in the mobile survey by treatment group and survey round, separately for each type of hormonal contraceptives. To explore the effect over time we restrict to the first recruitment wave. Usage rates are normalized to baseline usage in each group. Survey rounds occur every two weeks.

In Table 3, we use equation (3) to estimate the average difference in usage over the course of the survey. These estimates paint a very similar picture. Column 1 shows that students in the voucher group are not more likely to have used contraceptives than students in the control group on average over the entire data collection period (up to 6 months for the first wave). The coefficient is small (0.4 p.p.) and not statistically significant. However, students in the voucher + infertility information group are 3.5 p.p. (40%) more likely to be using hormonal contraceptives. This is statistically significant and statistically distinguishable from the voucher effect at the 5% level. The smaller effect in percent terms than the clinic data reflects the fact that average contraceptive usage in the control group is substantially higher than take-up at the clinic in the clinic data (8.8% vs 1.6%), consistent both with the fact that young women may access contraceptives in other ways than visiting the partner clinic and that women may have taken up contraceptives after the four week period for visiting the clinic.

Table 3
Effect on Average Contraceptive Use Over the Survey Data Collection Period

	Any Hormonal (1)	Pills (2)	IUD (3)	Implant (4)	Injection (5)
Voucher	0.004	-0.005	-0.002	0.008	0.004
	(0.012)	(0.009)	(0.003)	(0.005)	(0.006)
Voucher & Infertility Info	0.035**	0.005	0.000	0.011*	0.018**
	(0.014)	(0.010)	(0.003)	(0.006)	(0.007)
N	14240	14240	14240	14240	14240
Adjusted R-squared	0.179	0.053	0.348	0.279	0.206
Control mean	0.088	0.044	0.007	0.009	0.026
P-value of $\phi^V = \phi^{VI}$	0.022	0.235	0.179	0.591	0.068

Notes: This table reports the effect of each of the treatments on contraceptive usage throughout the survey period. For the first wave, this is up to 6 months after the workshop, and for the second wave, this is 1.5 months after the workshop. The outcomes are indicator variables for whether a student used any hormonal contraceptives or any of each type of contraceptive during the survey period. Estimates are produced by running equation (3) with the survey data. All regressions include fixed effects for baseline hormonal contraceptive usage and an indicator for whether a student was in the second recruitment wave, and survey round fixed effects as controls. Standard errors are robust to heteroskedasticity and clustered at the student level.

Columns 2 – 5 report treatment effects separately for each contraceptive type. An increase in injections (column 5) plays an important role in driving the increase in the take-up of hormonal contraceptives for the voucher + infertility information group. Students in this group are 1.8 p.p. (69%) more likely to use injections than students in the control group. These estimates highlight an interesting difference from the clinic data, where the take-up of the oral pill is relatively more important. The clinic data only tells us about initial take-up, while these estimates report average usage up to 6 months after the intervention and may reflect the fact that some pill users desisted using the pill while others switched to other, longer-acting forms of contraceptives. Appendix Table A7 reports the estimates for the alternative specifications. The point estimates with no controls are similar, albeit more imprecise, and the difference between the voucher and voucher + infertility information treatment remains statistically significant for using any hormonal contraceptive. The estimates selecting controls with the double-LASSO are almost identical to Table 3.

The results from the survey data highlight the importance of pairing the clinic data with a longer-term survey. The voucher treatment did not significantly change the longer-term behavior of students, though the shorter-term clinic data would have led us to conclude it increased take-up. While voucher participants visit our partner clinic and pick up contraceptives there more than the control group, these effects are short-lived, and their contraceptive usage throughout the post-workshop period is not significantly different from the control group. On the other hand, the voucher + infertility information treatment *did* have persistent effects, and these students are more likely to take up long-lasting contraceptives, such as injections and perhaps even implants.

Finally, a natural question is how the survey effect sizes compare to those of other programs. Choosing the appropriate benchmark is complicated by the fact that most interventions do not target the hard-to-

reach population of nulliparous women. One exception, Shah et al. (2024), studies an access intervention in Tanzania that focuses on young women and finds a null effect. Alternatively, we can compare our effects to interventions that focus on broader populations. Glennerster et al. (2022) study the effect of a large-scale media campaign in Burkina Faso. This campaign included messaging related to fear of infertility along with other components. Glennerster et al. (2022) find that the campaign increased usage by 5-6 percentage points, slightly larger than our point estimates, but their sample is older (average age of 30) and has a higher baseline rate of contraceptive use (23%). Athey et al. (2023) study the effects of personalized digital counseling in Cameroon, which increased the take-up of long-acting contraceptives by more than 20 percentage points. However, not only is their study sample older (average age 29), it is also drawn from women who either sought out family planning services or were at the hospital for maternity-related visits and are therefore likely already on the margin of taking up contraceptives. Hence, our effect size is similar to another successful intervention (Glennerster et al., 2022) – and may even point to one of the reasons that intervention was successful – even though we focus on a group where contraceptive use is thought to be particularly hard to move (Chandra-Mouli et al., 2014). Perhaps unsurprisingly, our estimates are smaller than the effects of interventions like Athey et al. (2023), which focus on populations that are already on the margin of taking up contraceptives.

6.3 Pregnancy

For our key research question – whether fear of infertility hinders contraceptive take-up among women who would benefit from delaying first births – the relevant outcome is contraceptive take-up. However, given that the voucher + infertility information treatment increased the take-up of preventative contraceptives, it is natural to examine whether it also affected pregnancy. Thus, in this subsection, we estimate effects on pregnancy with the caveat that our study was not designed to be powered to identify these effects. Recall, we followed 1,170 women for six months and another 338 women for 6 weeks after intervention, half of whom were not sexually active at baseline. Not only is pregnancy a relatively rare outcome; for the treatment to have observable effects, a woman must take-up a contraceptive method, which usually occurs with at least a couple of weeks lag after treatment, engage in sex, and then observe whether she is pregnant or not from a missed period or positive test (at least another two weeks after having sex). This limits the scope for observing changes in pregnancies over 1.5 or even 6 months.

We collected data about pregnancies in two ways during the study. First, on every biweekly survey, we asked participants whether they had been pregnant or had a positive pregnancy test over the previous two weeks. If they answered yes, we asked about the outcome of that pregnancy/pregnancy test: false

positive, miscarriage, abortion, still pregnant, or gave birth. Second, at endline, we asked participants whether they had ever been pregnant during the previous 6 months. Fewer pregnancies were reported at endline than contemporaneously, while almost every pregnancy at endline was also reported at some point previously on the survey. The discrepancy in number of pregnancies may reflect both underreporting at endline (e.g., choosing not to report a pregnancy that ended in abortion or miscarriage) and overreporting in the contemporaneous survey (false positive tests due to poor test quality or concerns about pregnancy due to missed periods). We therefore view the endline as giving us a lower bound measure of the rate of pregnancy prevalence, while the contemporaneous surveys provide something closer to an upperbound, and present estimates for both.

Given limited statistical power – only 1.7% of control women report a pregnancy in the last 6 months at endline who were not already pregnant at baseline – and the fact that the voucher program did not persistently affect contraceptive use, we compare the voucher + infertility information treatment to the pooled voucher and control treatments. To improve statistical power, in our preferred specifications, we also control for whether a woman was pregnant at baseline. To estimate the effects in the endline, we regress an indicator variable for reporting a new pregnancy at endline on an indicator variable for being in the voucher + infertility information treatment, controlling for initial contraceptive use (as in all our preferred specifications), baseline pregnancy status, and survey wave. Column 1 of Table 4 reports the results. Relative to the pooled control and voucher arms, the combined treatment reduces the likelihood of a new pregnancy by a statistically significant 1.2 percentage points (71%).

The remaining columns of Table 4 analyze the survey-level data. Analyzing these data is more complicated because we do not expect to see effects on pregnancy in the initial rounds (before women have taken up contraceptives or before enough time has passed since take-up for a pregnancy to be detected). We therefore report three different estimates for these data. In column 2, we pool all surveys and estimate the average effect on the likelihood of reporting a non-false positive pregnancy in any given survey. The point estimate (-0.003) is not significant at conventional levels ($p = 0.15$), but is consistent with a reduction of survey-round rate of pregnancy of 47%. Column 3 allows the treatment to have larger effects in surveys that took place after the first 2 months (allowing 4 weeks for take-up and 4 weeks for a late period to be detected). The treatment has close to 0 effect in the first 2 months but reduces pregnancy by a marginally significant 0.5 percentage points (71%) thereafter. Column 4 instead allows the treatment effect to depend linearly on the survey round. Reassuringly, it predicts no effect in the first round, but by the last survey round (round 12), it predicts a reduction of 0.7% in the likelihood of reporting a pregnancy, or close to 100%. Appendix Table A8 reports the results for the alternative specifications (only controlling for belonging to the second wave and using the double-LASSO selected controls). The point estimates are almost identical. Reassuringly, the

magnitude of the effects in terms of percent reductions is similar regardless of which pregnancy measure we use. Using the estimates in column 1 of Table 4, we can also calculate the cost of averting 1 pregnancy per year with the treatment.[31] Using the voucher + infertility information intervention's cost of 9.44 USD per participant, we calculate an annual cost per pregnancy averted of 393 USD.

While we caution that this analysis is underpowered, taking the estimates in Table 4 seriously, the voucher + infertility information treatment almost eliminated the occurrence of unplanned pregnancies among the treatment group by the end of 6 months. This may seem initially surprising given that take-up of contraceptives only increased by 3.5 percentage points over the study period. However, this could occur if the intervention specifically increased take-up among the group that was most at risk of pregnancy, which is what we would expect in a standard model of take-up. Reducing over-estimation of the cost of take-up should exactly increase take-up among women for whom take-up was efficient (and likely to have large returns). We further explore if this is consistent with the data in our compliers analysis in Section 8.

Table 4
Effects on Pregnancy

	New Pregnancy at Endline	Reported Pregnancy		
	(1)	(2)	(3)	(4)
Voucher & Infertility Info	-0.012**	-0.0033	-0.00036	0.00034
	(0.005)	(0.002)	(0.002)	(0.002)
Voucher & Infertility Info × After 2 Months			-0.0049*	
			(0.003)	
Voucher & Infertility Info × Survey				-0.00061*
				(0.000)
N	1367	14228	14228	14228
Adjusted R-squared	-0.001	0.043	0.043	0.043
Control mean	0.017	0.007	0.007	0.007

Notes: Column 1 estimates the effect of the voucher + infertility information treatment on an indicator variable for a new pregnancy in the last 6 months in the endline data. Columns 2–4 estimate the effects on an indicator variable for reporting a pregnancy that is not listed as a false positive in the survey data. All columns control for fixed effects for baseline hormonal contraceptive use, an indicator variable for survey wave, and an indicator variable for being pregnant at baseline. Columns 2–4 also include controls for survey round fixed effects. Standard errors are robust in Column 1 and robust and clustered at the individual-level in Columns 2–4.

6.4 Did the Treatments Have Unintended Consequences?

We next explore whether increased usage of hormonal contraceptives had negative unintended consequences. Hormonal contraceptive use could have crowded out condom usage, even though condoms also protect against STIs. Alternatively, by reducing the expected costs of sex, the treatments might encourage students to have more sex or to have sex with more partners. In Table 5, we test for the presence of unintended consequences

[31] To be conservative, this calculation assumes women do not get pregnant more than once.

using the survey data on sexual activity. An observation is again at the individual-by-survey level. In column 1 of Table 5, we show that neither treatment has a positive effect on the likelihood a participant had sex in a given survey round. Additionally, effects on survey-level condomless sex (column 2) are close to zero, and the point estimates for number of sexual partners are negative and close to zero (column 3). Another form of "risky" sex is sex with older partners. Older partners are statistically more likely to have HIV and might also be more likely to exert pressure for students to have sex without condoms (Dupas, 2011). In column 4 of Table 5, we show that, if anything, partner age is marginally lower in the voucher treatment and is not different in the voucher + infertility information treatment.

Table 5
Unintended Consequences: Sexual Behavior, Condoms, and Partners

	Any Sex (1)	Any Condom-less Sex (2)	Number of Partners (3)	Average Partner Age (4)
Voucher	-0.000	0.001	-0.013	-0.531*
	(0.017)	(0.011)	(0.024)	(0.316)
Voucher & Infertility Info	-0.005	0.011	-0.007	-0.000
	(0.018)	(0.012)	(0.024)	(0.354)
N	14190	14169	14190	2971
Adjusted R-squared	0.029	0.023	0.016	0.053
Control mean	0.220	0.089	0.249	25.855
P-value of $\phi^V = \phi^{VI}$	0.758	0.380	0.739	0.097

Notes: This table reports the effect of each of the treatments on sexual behavior throughout the survey period. For the first wave, this is up to 6 months after the workshop, for the second wave, this is 1.5 months after the workshop. The outcome used here is different from the statistics in Table 1. In Table 1 we show the probability of having had sex (and having condomless sex) at least once during the survey period, while here we use the probability of having had sex (and having condomless sex) in each bi-weekly period. The estimates are produced by running equation (3) on the survey data. All regressions include fixed effects for baseline hormonal contraceptive usage and an indicator for whether a student is part of the second wave as controls. Standard errors are robust to heteroskedasticity and clustered at the student level.

7 Threats to Identification

Before turning to mechanisms and quantifying the importance of fear of infertility, in this section, we consider threats to the validity of our study.

7.1 Accuracy of Self-Reported Data

The survey data are self-reported without a surveyor present. It is possible that at least some women either answer questions randomly to get through the survey quickly or answer falsely due to self-stigma or social desirability bias. Because we can link the clinic take-up data to the survey data, we can use this linkage to validate some of the survey answers. Some responses should not necesarily line up: if a participant took up condoms or the oral pill at the clinic, it is possible that she then decided not to use them, so that the clinic

take-up and usage data may not agree. However, for injections and implants, take-up at the clinic should imply usage. Once a woman has gotten a single shot (administered at the clinic), she will be covered by that contraceptive for 3 months. Of the women who were coded as receiving the injection at the clinic, 93% reported using the injection in the survey. Similarly, of the women who received the implant at the clinic, 91% reported using this method in the survey (the only one who did not report implant use instead reported using an IUD, which would make sense as a simple terminology error). Hence, the survey data appears to accurately measure contraceptive usage.

7.2 Attrition

While attrition is a natural concern for phone surveys over an extended period, attrition from the surveys was low. Appendix Figure A6 reports the attrition rate separately for students recruited in the first and second recruitment drive by treatment arm. Non-completion monotonically increases over time because individuals could not move on to the next survey until they had completed the previous survey. That is, students could not skip surveys but could complete surveys late. Nonetheless, 87% of surveys were completed during the correct two week period. For students in our first recruitment drive, 94% of the sample completed at least 75% of the surveys, and typically non-completion is associated with randomly missing a few surveys (and becoming behind) due to technical issues or being busy rather than permanently dropping out of the sample. In the first wave sample, only 1% of participants stopped filling out any surveys after the first month. Similarly, in our second recruitment drive, 96% of the sample completed two out of three surveys. Additionally, attrition rates were extremely similar across the three groups. In the first recruitment drive, on average, the voucher and voucher + infertility information groups completed 11.4 surveys, and the control group completed 11.2; there are no significant differences in the completion rates. For the second recruitment drive, the average completion rate is 2.8 for the voucher and control group, and 2.9 for the voucher + infertility information group. Again, there is no significant differences in the completion rates.

8 Mechanisms

Having established that the voucher + infertility information treatment increased the take-up of contraceptives, in Subsection 8.1, we provide evidence that it did so specifically by reducing fear of infertility. We first test whether the treatment did in fact persistently change beliefs. We then show that complier characteristics are consistent with the treatment increasing take-up by reducing fear of infertility. Finally, we report results from a complementary experiment on fear of infertility and STI testing. In Subsection 8.2, we provide evidence on two alternative mechanisms, role models and stigma.

8.1 Fear of Infertility

8.1.1 Changes in Beliefs and Self-Reported Preferences

Table 6 uses post-treatment survey data on beliefs about the infertility effects of different contraceptives to test whether the voucher + infertility information intervention changed beliefs both immediately after the treatment (week 4) and over the long-term (week 22). So that we can see how beliefs evolve over time for a stable sample, we report results for the first recruitment wave in this table.[32] In week 4, the voucher + infertility information treatment reduced the likelihood that respondents believed any hormonal contraceptives cause infertility by 11.5 p.p. or 19% (Table 6, Panel A, column 1). Columns 2 – 5 break this result down by contraceptive type. We find the largest reduction in infertility fears for implants (column 4) and injections (column 5). Recall that injections and implants are the two types of contraceptives for which there appear to be more persistent effects on take-up, in line with the change in fertility fears driving take-up.

In contrast, the voucher has no effect on the belief that any contraceptive causes infertility, though it did significantly affect fear that the implant caused infertility. This may be because the voucher treatment caused students to visit our partner clinic and meet the nurse, who could have provided them with additional information about different contraceptives. This may in turn have been particularly relevant for the implant since women were less familiar with this method. However, we caution against over-interpreting a single significant result when there is no aggregate effect for the voucher group.

In the latter 5 columns (Panel A, Block 2) of Table 6, we test whether the voucher + infertility information treatment also changed the (correct) belief that contraceptives can cause weight gain. This placebo would capture cases where the students became more positive about contraceptives in general or reported fewer side effects due to social desirability bias. Reassuringly, we do not see any changes in beliefs about whether contraceptives cause weight gain. All the coefficients in Block 2 are small and statistically insignificant.

Panel B of Table 6 shows that the voucher + infertility information treatment's effect on beliefs was persistent. In week 22, those in the voucher + infertility information treatment were 12.7 p.p. (19%) less likely to report that at least one of the hormonal contraceptives caused infertility.[33] There is *no* fadeout of the treatment effect. As before, the voucher has no effect on the belief that at least one of the contraceptives causes infertility, and beliefs about weight gain remain unaffected.

In Appendix Table A10, we further explore whether the voucher + infertility information treatment had

[32] Appendix Table A9 reports the results for the full sample in week 4 (the second wave does not have a week 22 survey) and shows they are almost identical.

[33] Within the first recruitment wave, the belief that contraceptives cause infertility is about 5 percentage points more prevalent in the control group at 22 weeks than at 4 weeks. However, note that the (correct) belief that contraceptives cause weight gain also increases by 4 percentage points. In the data, this increase appears to be explained by the beliefs of new first year students catching up with other students. This may be because these students both become sexually active and exposed to new ideas.

Table 6
Effects on Beliefs About Infertility & Contraceptives (Survey Data —Weeks 4 & 22)

Panel A —Week 4

	Block 1: Cause Infertility					Block 2: Cause Weight Gain				
	(1) Any	(2) Pill	(3) IUD	(4) Implant	(5) Injection	(6) Any	(7) Pill	(8) IUD	(9) Implant	(10) Injection
Voucher	-0.023	0.001	0.007	-0.065**	-0.024	-0.024	-0.004	0.010	0.009	-0.002
	(0.035)	(0.034)	(0.026)	(0.031)	(0.033)	(0.023)	(0.035)	(0.022)	(0.033)	(0.036)
Voucher & Infertility Info	-0.115***	-0.033	-0.007	-0.121***	-0.088***	-0.016	-0.011	-0.002	0.023	0.001
	(0.035)	(0.033)	(0.025)	(0.030)	(0.031)	(0.023)	(0.035)	(0.021)	(0.034)	(0.036)
N	1158	1158	1158	1158	1158	1158	1158	1158	1158	1158
Adjusted R-squared	0.010	0.001	-0.002	0.011	0.004	0.000	0.002	-0.003	-0.002	-0.000
Control mean	0.614	0.328	0.145	0.286	0.296	0.892	0.607	0.098	0.311	0.454
P-value of $\beta^V = \beta^{VI}$	0.011	0.318	0.580	0.051	0.040	0.725	0.847	0.573	0.666	0.944

Panel B —Week 22

	Block 1: Cause Infertility					Block 2: Cause Weight Gain				
	(1) Any	(2) Pill	(3) IUD	(4) Implant	(5) Injection	(6) Any	(7) Pill	(8) IUD	(9) Implant	(10) Injection
Voucher	0.001	-0.008	-0.049	-0.075**	-0.007	-0.028	-0.042	0.026	-0.012	0.002
	(0.035)	(0.036)	(0.030)	(0.036)	(0.035)	(0.020)	(0.035)	(0.024)	(0.036)	(0.037)
Voucher & Infertility Info	-0.127***	-0.049	-0.050*	-0.116***	-0.085**	-0.031	-0.028	0.006	-0.022	-0.016
	(0.036)	(0.035)	(0.030)	(0.035)	(0.033)	(0.020)	(0.034)	(0.023)	(0.036)	(0.037)
N	1087	1087	1087	1087	1087	1087	1087	1087	1087	1087
Adjusted R-squared	0.025	0.002	0.001	0.015	0.004	0.012	0.015	-0.000	-0.004	0.002
Control mean	0.664	0.373	0.229	0.395	0.320	0.933	0.680	0.112	0.384	0.483
P-value of $\beta^V = \beta^{VI}$	0.000	0.247	0.978	0.223	0.019	0.872	0.698	0.420	0.771	0.641

Notes: This table reports the effect of each of the treatments on whether the student believes each type of contraceptive causes infertility – in Block 1 – or weight gain – in Block 2. Panel A shows the Week 4 survey results, and Panel B the Week 22 results. It is estimated with equation (1) in the survey data. Column 1 is an indicator for whether the student believed one or more of the hormonal contraceptives shown here cause infertility. In Columns 2 – 5, the outcomes are indicator variables for each contraceptive separately. Block 2 repeats the analysis for weight gain. All regressions include indicator variables for baseline hormonal contraceptive usage. To maintain a consistent sample across surveys, we restrict to the first recruitment wave. Standard errors are robust to heteroskedasticity.

persistent effects on stated preferences over contraceptives near the end of the survey period. As in Table 6, we limit our analysis to the first wave so that we can test whether stated preferences were different a substantial period (22 weeks) after the intervention. The voucher + infertility information treatment more than doubled interest in the implant, increasing it by 15.0 p.p., and marginally significantly reduced interest in the oral pill. Thus, not only did beliefs change, but this appears to have led stated preferences for longer-lasting methods, about which girls may have more concerns but which are ultimately more effective, to increase. This is also consistent with Appendix Figure A5, which provides suggestive evidence that, by the end of the sample period, the voucher + infertility information group had started switching to the implant.

8.1.2 Evidence on Compliers

To further understand which students were most affected by the treatments, we estimate the average characteristics of the compliers (where taking up a hormonal contraceptive at the clinic is the outcome of interest) separately for the voucher and voucher + infertility information groups using the methodology in Pinotti (2017). To estimate the characteristics of the compliers for the voucher + infertility information treatment, we run a two-stage least squares regression whose first and second stage are given by

$$h_i = \lambda^{VI} VI_i + \lambda^{V} V_i + \gamma X_i + \epsilon_i,$$

and

$$h_i \times k_i = \theta^{VI} h_i + \theta^{V} V_i + \zeta X_i + \nu_i,$$

where h_i is an indicator variable equal to 1 if student i took up any hormonal contraceptives at the clinic, X_i is a control for belonging to the second wave, and k_i is the characteristic of interest. The characteristics of the compliers with the voucher + infertility information treatment are given by θ^{VI}. To estimate the characteristics of the compliers with the voucher treatment, we simply switch V_i and VI_i in the estimating equations. We use the clinic data to measure h_i since the voucher has no effect on take-up in the survey data.

Table 7 reports the average characteristics of the compliers with each treatment, as well as the average characteristics of the sample and p-values for tests of the differences between the complier and sample averages. Consistent with the fact that the voucher + infertility information treatment addressed fears of infertility, compliers with that treatment are much more likely to report that they do not use contraceptives due to fear of infertility/side effects (43% vs. 25% for both voucher compliers and the sample average).

Interestingly, voucher + infertility information compliers are also much more likely to be sexually active at baseline (81% vs. 65% for voucher compliers and 60% overall). Consistent with the pregnancy results, the voucher + infertility information treatment seems to target individuals with particularly high returns to using contraceptives.

Table 7
Characteristics of Compliers

	Voucher & Infertility Info	P-value	Voucher	P-value	Sample
	(1)	(2)	(3)	(4)	(5)
Fear of Infertility/Side Effects	0.425	0.092	0.252	0.968	0.246
Sexually Active	0.812	0.007	0.650	0.763	0.596
Use Hormonal Contraception	0.002	0.669	-0.415	0.134	0.052
Use Condoms	0.230	0.593	0.312	0.326	0.180
SES Index	-0.013	0.959	-0.325	0.555	-0.000
Father Years Education	12.748	0.582	11.943	0.883	12.248
Mother Years Education	11.068	0.601	10.697	0.957	10.590
Age	21.435	0.503	20.976	0.847	21.149
Year	2.317	0.590	1.724	0.282	2.183

Notes: This table reports the average characteristics of compliers (for taking up any hormonal contraceptive at the clinic) for the voucher + infertility information (column 1) and voucher (column 3) treatments, as well as the average characteristics of the sample (column 5). The even columns report the p-values for tests of whether the characteristics of the voucher + infertility information compliers (column 2) or voucher compliers (column 4) are statistically significantly different from the sample average. All variables are measured at baseline. The SES Index is the first predicted component from a pca of indicator variables for being from Lusaka (the capital), sharing a bedroom growing up, being on government bursary, growing up in a place where the clinic was more than 30 minutes walking distance away, and having undergone an initiation ritual. "Fear of Infertility/Side Effects" indicates the respondent does not use hormonal contraceptives for these reasons, while "Use Condoms" indicates that she reports not using hormonal contraceptives at baseline because she uses condoms. Standard errors are robust to heteroskedasticity.

In contrast to the voucher + infertility information treatment, none of the voucher compliers' characteristics differ statistically significantly from the sample average. Unlike the voucher + infertility information treatment, the voucher treatment does not seem to specifically target the sexually active, a population with higher returns to take-up. This difference between the treatments may help to explain the persistent effects of the voucher + infertility information treatment – it permanently changes the perceived costs of take-up for a group that also had larger benefits from take-up.

To supplement these results, in Appendix D, we use a causal forest machine learning technique to identify heterogeneous treatment effects (Athey et al., 2019; Wager and Athey, 2018). The results are in line with our findings in Table 7. Students that were sexually active at baseline and report not using contraceptives due to fear of infertility are predicted to respond more to the voucher + infertility information treatment but not the voucher (see Figure D1) when the outcome is take-up at the clinic. Similarly, students with the largest predicted treatment effect for the voucher + infertility information treatment are much more likely to be sexually active and to report not using contraceptives due to fear of infertility (see Table D1).

8.1.3 Extension: STI Experiment

To complement our main findings, we further test whether fear of infertility motivates health-seeking behavior in a follow-up experiment. Our sample consists of the 1,015 ever sexually active participants in the original experiment. All these students were offered another 40 kwacha (∼2 USD) voucher to visit the partner clinic for STI testing. Half (the treatment group) also received a text message highlighting that untreated STIs are a leading cause of infertility. As before, the cash transfer conditions on visiting the clinic rather than taking up healthcare. The exact wording of each message can be found in Appendix E. We note that this is a very light touch informational treatment relative to the design and delivery of the voucher + infertility information treatment, so we may not expect as sizable effects.

Table 8 reports the estimated effect of this new treatment, controlling for the voucher + infertility information and voucher treatments (which are independent). We first observe that the text message treatment had no effect on visiting the clinic (column 1). Unconditional on visiting the clinic, the coefficient for treatment on taking up a STI test is positive (a 2.5 p.p. or 15% increase), but we do not have the precision to reject a zero effect (column 2). Conditional on visiting the clinic, as seen in column 3, the information had a marginally statistically significant and sizable effect on taking up a STI test (a 9.5 p.p. or 13% effect). Thus, the point estimates are consistent with this very light-touch text message informational treatment having meaningful effects. We take this as suggestive evidence that fear of infertility also affects other health-seeking behavior.

Table 8
Follow-up Experiment: Fear of Infertility & Testing for Sexually Transmitted Diseases

	Clinic Visit	Any STI Test	Any STI Test | Clinic Visit
	(1)	(2)	(3)
STI Information Treatment	0.003	0.025	0.095*
	(0.027)	(0.024)	(0.055)
N	1015	1015	242
Adjusted R-squared	0.020	0.006	0.004
Control mean	0.238	0.169	0.711

Notes: This table reports the effect of the STI information treatment, which gave information on the infertility effects of STIs, on visiting the clinic and STI testing, controlling for treatment status in the original contraceptives experiment. The outcomes are an indicator variable for visiting the clinic (Column 1) and an indicator variable for taking an STI test (Columns 2-3). Column 3 conditions the sample on visiting the clinic. All regressions include indicator variables for baseline hormonal contraceptive usage, an indicator for whether a student is part of the second wave, and indicator variables for the voucher and voucher + infertility information treatments as controls. Standard errors are robust to heteroskedasticity.

8.2 Alternative Explanations: Role Models & Stigma

One possible alternative explanation for the lasting impact of the voucher + infertility information treatment on hormonal contraceptive take-up and usage is that it provided participants with older role models who had used contraceptives in the past. While the same two facilitators ran all of the workshops (control, voucher, and voucher + infertility information), they only shared personal stories about using contraceptives themselves in the voucher + infertility information workshop. These stories were intended to help convey to the participants that it is common for women to become pregnant after stopping contraceptive use in a particularly salient way, but it is possible that sharing them also helped destigmatize the use of hormonal contraceptives.

We evaluate the scope for this mechanism in a few ways. First, it is worth noting at baseline that stigma was not an important driver of contraceptive non-use in self reports. In Figure 2, we report the reasons women give for not using hormonal contraceptives at baseline. One option was, "I am afraid of stigma from my partner or family." This option was only selected by 2% of the women who had ever had sex and 5% of those who had had sex in the past 2 weeks (as opposed to fear of infertility, which was reported by 19% and 28%, respectively, and fear of side effects, which was even higher).

Second, we directly test whether the voucher + infertility information treatment affected proxies for stigma related to contraceptive use in our data. Appendix Table A11 reports the results. In the second and eleventh surveys (weeks 4 and 22), we asked participants how frequently they had conversations with their friends about contraceptives. To examine the effect in a consistent sample over time, we restrict to the first recruitment wave. If the voucher + infertility information treatment destigmatized contraceptive use, we might expect open discussion of contraceptive use to increase. Column 1 shows that, if anything, conversations about contraceptive use declined right after the workshops in both treatment arms, perhaps because the participants substituted to accessing information via the clinic. Column 2 shows that there were no differences in frequency of conversations about contraceptives by the end of the study for the voucher + infertility information vs. control group. Interestingly, there is an increase in the voucher group – perhaps mechanically due to the group talking about the voucher treatment with friends or spuriously – but since this group did not persistently increase take-up, it does not suggest any link between reduced stigma and take-up. The remaining columns examine the effect of the treatments on whether respondents report that they approve of unmarried women using modern contraceptives to prevent pregnancy during premarital sex (column 3) or whether their mother would approve (column 4).[34] In either case, the voucher + infertility information

[34]These questions were asked in the week 2 survey of the original sample (first wave) but were only asked in the baseline (pre-treatment) survey during the second wave. Since we are interested in the effect of the treatment on stigma, we restrict the sample to the first wave for these regressions.

treatment has no significant effect on approval. Interestingly, the voucher statistically significantly increases the probability a young woman approves but reduces the probability she reports her mother would approve. But given that the voucher did not have any long-run effects, these changes in perceived stigma do not appear to persistently affect contraceptive take-up. Overall, the results are inconsistent with the voucher + infertility information treatment increasing take-up by differentially reducing stigma.

9 Quantifying the Importance of Fear of Infertility

Our results suggest that fear of infertility is a barrier to contraceptive take-up in SSA. In this section, we quantify the importance of this barrier. Table 6 shows that the voucher + infertility information treatment reduced participants' belief that hormonal contraceptives cause infertility, and Tables 2 and 3 show that the treatment also led to an increase in the take-up and usage of hormonal contraceptives. However, even in the voucher + infertility information treatment, nearly 50% of women still believed that hormonal contraceptives can cause infertility (consistent with the fact that these are widespread and potentially deeply-held beliefs). To capture the importance of fear of infertility as a barrier, we exploit our RCT variation to estimate the effect of fully eliminating the belief that contraceptives cause infertility.

We use a two-stage least squares approach where we instrument for the belief that at least one contraceptive causes infertility using assignment to the voucher + infertility information treatment. Because this group also got information about the clinic and a voucher, we control separately for these components using assignment to *either* of the two treatment groups (relative to control). This results in a Wald estimator, where the numerator is the change in contraceptive use from the voucher + infertility information treatment relative to voucher alone, and the denominator is the change in beliefs from this treatment relative to the voucher alone.

We estimate that going from 100% of the population having the belief that at least one contraceptive causes infertility to 0% would lead to a 31.3 ppt (se=0.188, $p = 0.096$) increase in the take-up of hormonal contraceptives in the clinic data. The average estimate across surveys is an almost identical 31.4 ppt (se=0.168, $p = 0.062$). Since 64% of control participants believe that at least one contraceptive causes infertility, these estimates imply that eliminating these beliefs entirely from the study population would increase take-up and usage by 20 percentage points. This would more than half the gap in usage between U.S. and Zambian female college students. These are very large effects, especially given that (a) the sample of college students is likely to be better-informed than most women in Sub-Saharan Africa, and (b) only 60% of the sample were sexually active at baseline. Fear of infertility appears to be a significant barrier to the take-up of contraceptives among young women in sub-Saharan Africa, and addressing it could substantially increase

usage.

We present these estimates with the caveat that interpreting the Wald estimator as the causal effect of fear of infertility on take-up requires a few assumptions. First, it requires the reasonable assumption that the effect of treatment on contraceptive take-up is monotonic. That is, there are no participants who would have used contraceptives in the control group but are pushed out of doing so by being assigned to either of the two treatment groups (and similarly, that no one who would not have used contraceptives in the fertility information treatment would do so in the voucher-only treatment). Second, it requires that the two treatments are additively separable. That is, it assumes there is no complementarity between the infertility information component of the treatment and the voucher component. If this is not true, our estimates measure the effect of changing beliefs when a voucher is in place (e.g., in contexts where access is not likely to be a barrier). Lastly, it assumes that the exclusion restriction holds: the only difference between the voucher + infertility information treatment and the voucher treatment is the former's effect on the belief that contraceptives cause infertility. While some of these assumptions are strong, the results in Section 8 suggest that the change in beliefs is the primary cause of the change in behavior that we see, and thus, the Wald estimate is a useful scaling exercise for quantifying fear of infertility as a barrier to contraceptive take-up in this population.

10 Conclusion

This study uses a randomized controlled trial to understand whether fear of infertility causes young women in Sub-Saharan Africa to avoid using hormonal contraception, risking unwanted pregnancies during a critical period. We focus on college students in Zambia. This is a population where baseline contraceptive take-up is low, and take-up is likely to have especially high returns: 60% are sexually active at baseline, 91% of those that want kids want to delay child-bearing until they complete their studies, and 58% of those who have sex during the study period have condomless sex at least once. We find that increasing access to available contraception is not enough; only our targeted fertility information treatment is able to increase use of hormonal contraception over the course of the study.

Our results suggest that the singular focus on increasing contraceptive use among older, married women to reduce total fertility rates in Sub-Saharan Africa may have missed one of the key populations who benefit from increased access: young women who want to delay fertility in order to finish their education. The demographic transitions in the U.S. and Europe happened before the advent of modern contraceptives (Bhattacharya and Chakraborty, 2017); reducing total fertility does not require methods with 99% accuracy. An important benefit of these highly effective preventative methods is that they allow women (without

the consent or even the knowledge of their male partners) to almost completely prevent pregnancy during a critical period in which it might be extremely costly. This was the essence of the "pill revolution" in developed countries: in most cases, women did not reduce total fertility, but rather used highly effective contraception to time their pregnancies (Goldin and Katz, 2002; Bailey, 2006), allowing them to finish their education and join the labor force. Unmarried women in unstable or unequal partnerships may particularly benefit from options outside barrier or traditional methods (timing, withdrawal) that require agreement and participation from the male partner. Young, nulliparous women are often difficult to target with access treatments alone (see, e.g. Shah et al., 2024), but they may also have particularly high benefits from even a short duration of contraceptive use to optimally time their first birth and avoid pregnancies while in school.

Our RCT is designed to measure the effect of one important barrier to contraception adoption that may be particularly relevant to young women: medical distrust and the fear that contraceptives cause infertility. These fears are known to be widespread in Sub-Saharan Africa (Boivin et al., 2020) and are built on generations of negative experiences with colonial medicine and population control. Given that informational interventions are often ineffective at changing health behavior (Dupas and Miguel, 2017), even in cases where beliefs are less likely to be sticky, we carefully design an intervention to change long-held beliefs using a combination of narratives, facts, and facilitators with whom students identify. Unusually, this allows us to achieve a strong and persistent effect on beliefs with zero fadeout over 6 months. Moreover, this change in beliefs translates into a 40% increase in the use of hormonal contraceptives, and more suggestively, a reduction in pregnancy rates of 71%.

Our results establish that fear of infertility causally reduces the take-up of contraceptives among young college-going women, and our quantification exercise suggests this effect is large. This population is important in its own right. First, 9% of women now enroll in college in Sub-Saharan Africa, and as lower levels of education become increasingly universal, this rate is rapidly growing. Second, these women likely play an out-sized role in productivity and the allocation of talent (Hsieh et al., 2019). Indeed, using Mincerian regressions, Montenegro and Patrinos (2014) estimate that globally, the returns to college education and to educating women are highest. Nonetheless, two pieces of evidence suggest that our results are likely to be externally valid for other nulliparous women. First, baseline rates of contraceptive usage in our sample are almost identical to the rates by nulliparous women of secondary and high school age in the Zambian DHS. Second, while the belief that contraceptives cause infertility is widespread in our sample, it is not predicted by any demographic characteristics (see Appendix Table A1), suggesting that it is not specific to a given socioeconomic class or educational group. This is consistent with the qualitative literature, which documents these beliefs across a wide array of women from different ages and socioeconomic groups across SSA (Boivin et al., 2020; Engelbert Bain et al., 2021).

In closing, the belief that contraceptives cause infertility is not unique to Sub-Saharan Africa. However, a colonial legacy of coercive medical campaigns and population control policies have likely made these beliefs especially widespread and persistent. Moreover, the importance of fear of infertility is likely compounded by the fact that considerable importance is placed on having children in SSA, where individuals may desire large families (Pritchett, 1994; Dupas et al., 2024). Several studies have suggested that infertility may result in divorce, husbands' infidelity, and poverty (see van Balen and Bos (2009) for a review). Indeed, our participants who believe that couples that cannot have children will be more likely to divorce are also more likely to not use contraceptives due to fear of infertility. Thus, interventions focused on reducing early births need to consider both the now (desire to prevent children) and the later (desire to have children in the future) of family planning.

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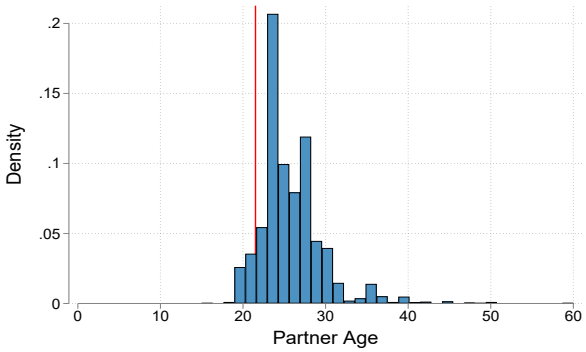
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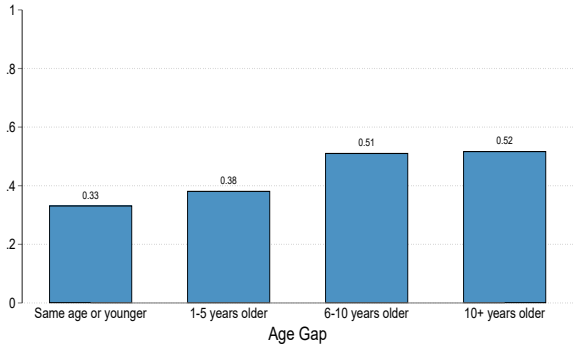
Appendix Figures

Figure A1
Partner Age and Condom Use

(a) Number of Encounters by Partner Age

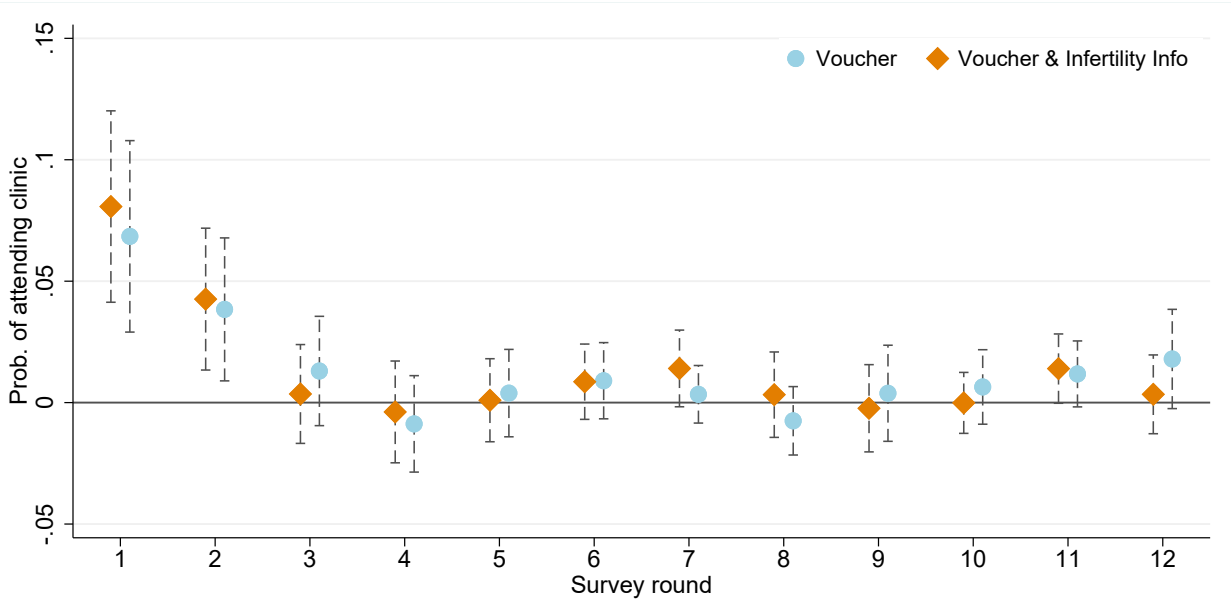


(b) Share Condomless Encounters by Partner Age Gap



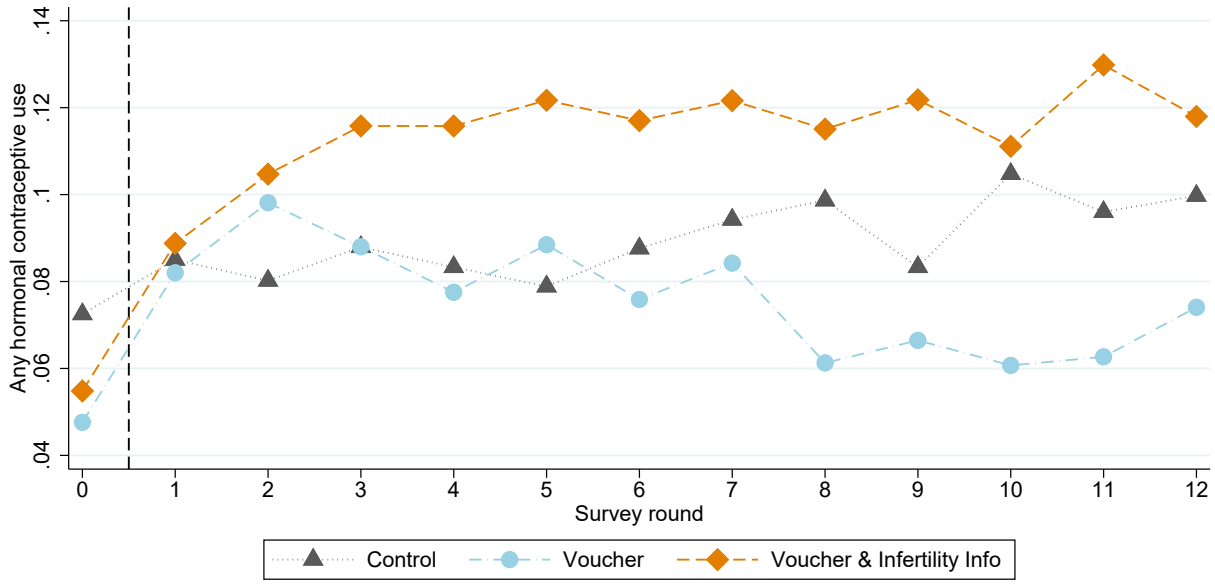
Notes: This figure reports summary statistics on women's sexual encounters. Panel (a) plots a histogram of number of encounters by partner age, and Panel (b) plots the share of encounters that are condomless by the age gap between participants and partners. Encounter-level data on condomless sex and partner age were collected every two weeks through the mobile phone survey. The red line in Panel (a) indicates the average age of a student that reported having at least one sexual partner in at least one survey: 22.

Figure A2
Self-Reported Visits to Partner Clinic for Family Planning Services by Survey Round



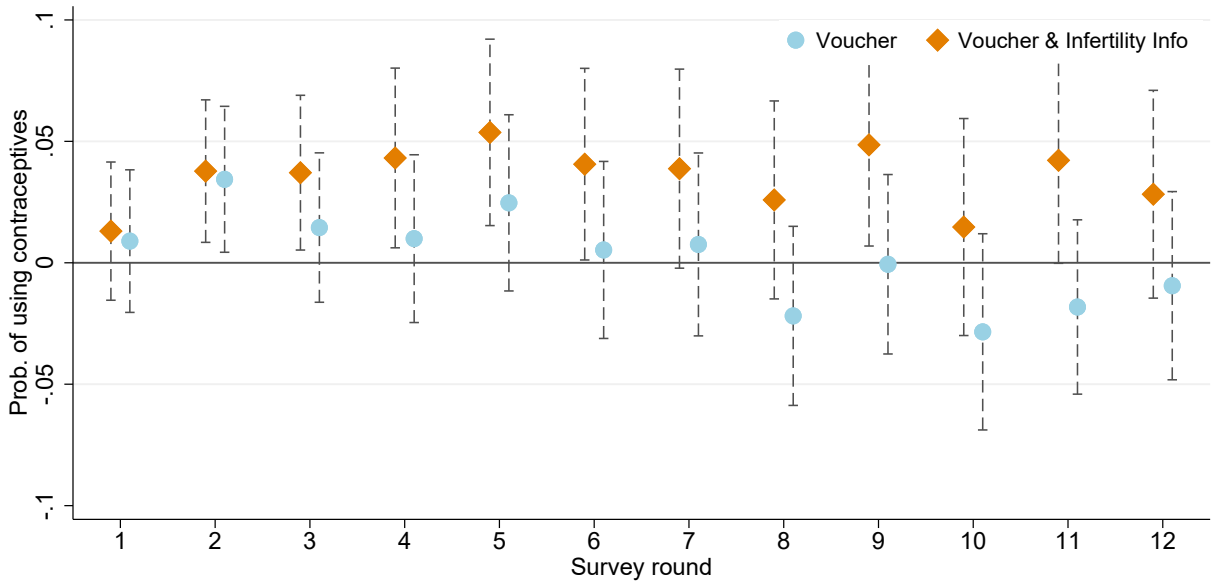
Notes: This figure plots the coefficients in equation (2), where the outcome is an indicator variable for whether a student reported attending our partner clinic for family planning or contraceptives in the two weeks preceding the survey in question. Survey rounds occur every two weeks. The regressions include controls for baseline hormonal contraceptive usage and whether a student was part of the second recruitment wave. Standard errors are robust and clustered at the student level.

Figure A3
Hormonal Contraceptive Use by Survey Round and Treatment Group Without Normalization



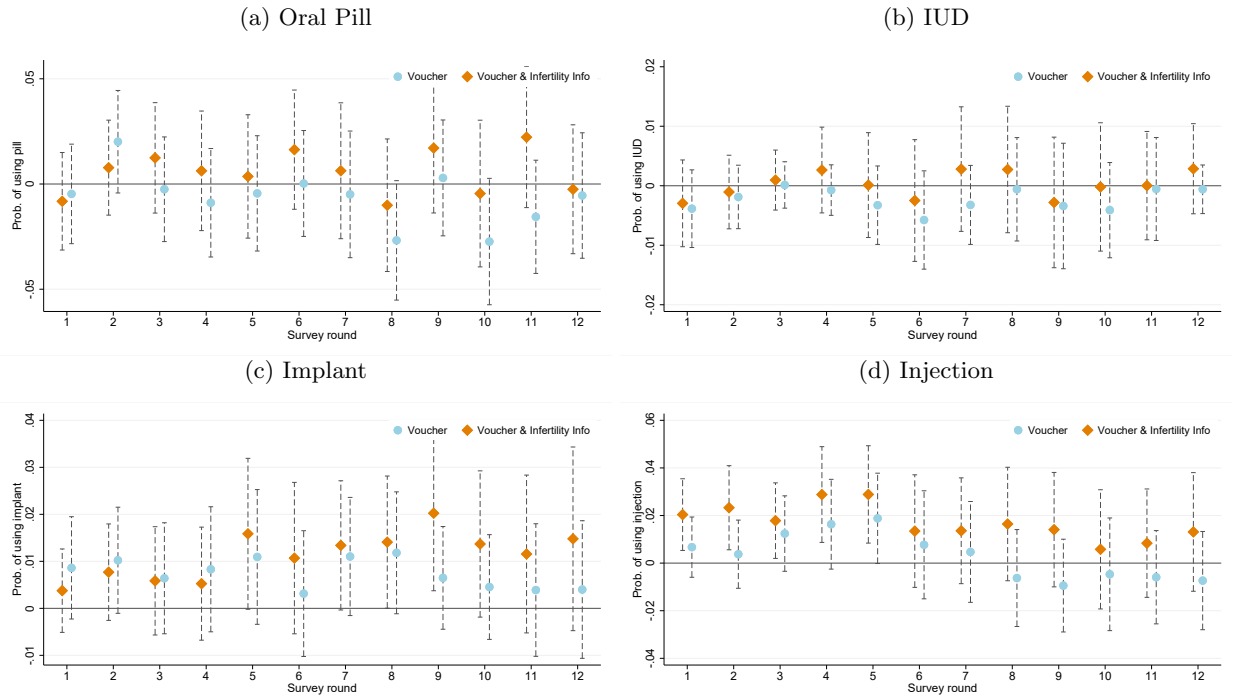
Notes: This figure plots the non-normalized average use of hormonal contraceptives reported in the mobile survey by treatment group and survey round. Hormonal contraceptives include the oral pill, shot, implant, and IUD. Survey rounds occur every two weeks. To explore the effect over time in a fixed sample, we restrict to the first recruitment wave, which was followed for 6 months.

Figure A4
Use of Hormonal Contraceptives by Survey Round



Notes: This figure plots the coefficients in equation (2), where the outcome is whether a student reported using hormonal contraceptives in the two weeks preceding each survey. Hormonal contraceptives include the pill, shot, implant, and IUD. Survey rounds occur every two weeks. The regressions include controls for baseline hormonal contraceptive usage and whether a student was part of the second recruitment wave. Standard errors are robust and clustered at the student level.

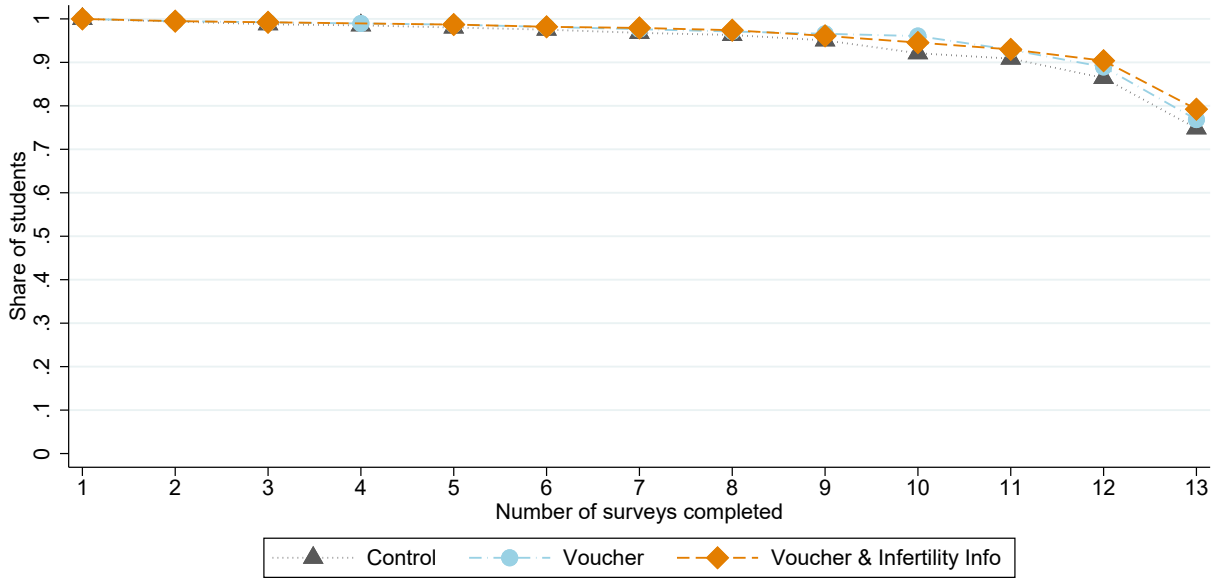
Figure A5
Use of Hormonal Contraceptives by Type and Survey Round



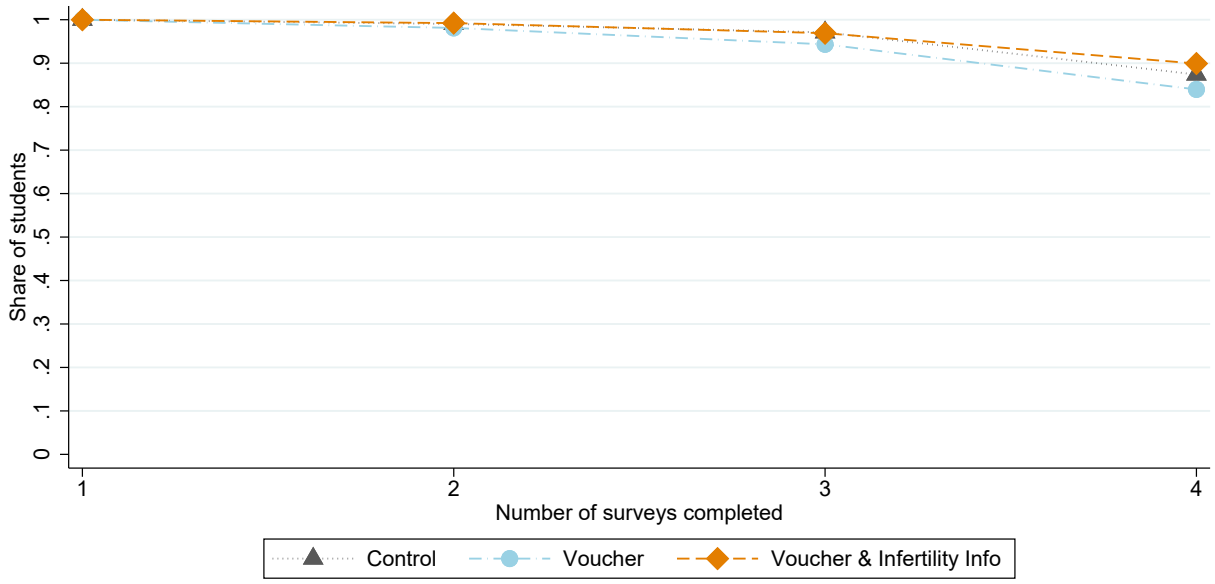
Notes: This figure plots the coefficients in equation (2), where the outcome is whether a student reported using a specific type of hormonal contraceptive in the two weeks preceding the survey. Survey rounds occur every two weeks. The regressions include controls for baseline hormonal contraceptive usage and whether a student was part of the second recruitment wave. Standard errors are robust and clustered at the student level.

Figure A6
Attrition in the Survey Data by Workshop Type

(a) First Wave



(b) Second Wave



Notes: This figure shows the attrition rate from the survey data for students in the first (a) and second (b) waves, separately by workshop type. Each graph reports the share of students in the sample who completed at least the indicated number of surveys.

Appendix Tables

Table A1
Predictors of the Belief that Contraceptives Cause Infertility (Control Only)

	Believes Contraceptives Cause Infertility		
	OLS (1)	OLS (2)	LASSO (3)
Age	-0.024	-0.016	
	(0.017)	(0.018)	
Year	0.032	0.027	
	(0.027)	(0.027)	
SES Index	0.033	0.025	
	(0.025)	(0.024)	
Father Years Education	0.006	0.006	
	(0.007)	(0.007)	
Mother Years Education	-0.007	-0.008	
	(0.006)	(0.006)	
On Government Scholarship	-0.057	-0.061	
	(0.058)	(0.058)	
Sexually Active		0.090*	
		(0.051)	
Sex in Past 2 Weeks		-0.014	
		(0.060)	
Medical Student		0.089	
		(0.071)	
Age First Heard About Contraceptives		-0.054**	
		(0.022)	
Approve of Unmarried Woman Taking Contraceptives		-0.037**	
		(0.018)	
N	495	492	492
Adjusted R-squared	-0.002	0.017	
Control mean	0.645	0.645	0.645

Notes: This table reports the effect of baseline characteristics on reporting that any of the hormonal contraceptives cause infertility as measured in week 2. The analysis is restricted to the control group. Column 1 includes demographic characteristics and measures of socioeconomic status. Column 2 further includes measures of sexual activity, attitudes, and knowledge of contraceptives. Column 3 uses LASSO to select the set of predictive controls from column 2, as well as program and province of origin fixed effects. No controls are selected.

Table A2
Beliefs About Costs of Infertility & Non-use due to Fear of Infertility

	Does not Take Up due to Fear of Infertility/Side Effects	
	(1)	(2)
Together, Cannot Conceive	-0.015**	
	(0.007)	
Together		0.004
		(0.007)
N	695	695
Adjusted R-squared	0.004	-0.001
Mean Dep. Var	0.255	0.240

Notes: This table reports the association between the number of couples a respondent expects to stay together out of 10, with and without being informed that the couples cannot conceive, and reporting not taking up hormonal contraceptives due to fear of infertility/side effects. "Together" is the number of couples out of 10 the respondent expects to be together in 2 years. "Together, Cannot Conceive" is the number of couples who cannot conceive that a respondent expects to be together in 10 years. These two questions were asked to distinct, randomly chosen subsamples on the baseline survey. Standard errors are robust.

Table A3
Marital Outcomes & Infertility

	Worries Husband will Divorce	Husband Threatens Violence	Husband uses Violence	Marital Unhappiness	Husband does not Pay Expenses (Index, SDs)
	(1)	(2)	(3)	(4)	(5)
Infertility	0.095**	0.096*	0.079	0.070**	0.156
	(0.044)	(0.050)	(0.049)	(0.035)	(0.101)
N	875	892	891	888	892
Adjusted R-squared	0.010	0.001	-0.001	0.007	0.026
Mean non-infertile	0.147	0.312	0.292	0.071	0.000

Notes: This table reports the association between a woman being infertile and different measures of the quality of their relationship with their husbands. All columns include controls for age; the square of age; and a four-valued self-health variable ranging from 1 ("Poor") to 4 ("Excellent"). The sample consists of married women between the ages of 17 and 44 from the surrounding areas of Lusaka, Zambia. Data were collected between 2014 and 2016. For more information on data collection, see Ashraf et al. (2022). The infertility indicator is constructed from medical variables correlated with infertility which include early menopause, hysterectomy, sterilization, obstetric fistula, or being told by a healthcare worker that you are infertile. Women who report any of the medical signs of infertility are given "treatment value" of 1, which applies to 12% of the sample. "Control" women report no medical signs of infertility and receive value 0. Marital unhappiness is measured from the question, "How happy are you with your marriage?" Responses are on a five-point scale ranging from being very happy and content to very unhappy and discontent in one's marriage, which we use to create a binary measure valued 1 if the respondent answers "somewhat unhappy" or "very unhappy or discontent." "Husband does not pay expenses" is an index for whether a women does not receive spousal support across seven different categories of spending. In these questions, women are asked where they get money for various expenditures. Options include their own money, asking their family for money, borrowing, or using their husband's, husband's family's, or housekeeping money, with the latter three categories indicating financial support from spouses. The individual spending categories include healthcare, child healthcare, child education, child food, child clothes, shelter, and parental support. This index is the average of the z-scores (normalized to the non-infertile distribution) for the seven money provider variables. For observations with at least one response to the set of money provider variables, missing responses for other variables in the set are replaced with the group (infertile or non-infertile) mean. Standard errors are robust to heteroskedasticity.

Table A4
Balance

	Control		Voucher vs Control		Voucher & Infertility Info vs Control	
	Mean (1)	Std. dev. (2)	Coeff (3)	Std. err. (4)	Coeff (5)	Std. err. (6)
General						
Age	21.238	(1.732)	-0.127	(0.109)	-0.141	(0.106)
Year at UNZA	2.219	(1.051)	-0.077	(0.065)	-0.032	(0.065)
Are you married?	0.024	(0.152)	-0.020***	(0.007)	-0.000	(0.009)
Sex and Pregnancy						
Number of pregnancies	0.101	(0.408)	-0.037*	(0.022)	-0.031	(0.024)
Number of children	0.045	(0.235)	-0.016	(0.013)	-0.008	(0.015)
Ever had sex	0.620	(0.486)	-0.003	(0.031)	-0.067**	(0.031)
Sex in past two weeks	0.183	(0.387)	0.003	(0.025)	-0.020	(0.024)
Contraceptive usage						
Using any modern contraceptive	0.065	(0.247)	-0.024*	(0.014)	-0.014	(0.015)
Using the pill	0.030	(0.169)	-0.007	(0.010)	-0.000	(0.011)
Using IUD	0.004	(0.063)	-0.002	(0.003)	0.002	(0.004)
Using an implant	0.008	(0.088)	-0.002	(0.005)	-0.004	(0.005)
Using an injection	0.024	(0.152)	-0.013	(0.008)	-0.012	(0.008)
Not using because of fear of infertility or side effects	0.222	(0.416)	0.024	(0.027)	-0.001	(0.026)
P-value (joint F-test)				0.130		0.556

Notes: This table reports summary statistics for students in the control group, in addition to the estimated difference between each of the treatment arms and the control group. To arrive at the values in columns 3–6, the row name characteristic (collected in the baseline survey) is regressed on an indicator for whether the student is in the relevant treatment group using a sample that only includes the relevant treatment group and the control. The final row regresses an indicator variable for whether the student is part of the indicated treatment arm on all the baseline characteristics displayed here and reports the p-value of a joint test of significance on all the covariates. Standard errors are robust to heteroskedasticity.

Table A5
Effect on Clinic Attendance and Contraceptive Take-Up in Clinic Data, Alternative Specifications

	Visits Clinic		Takes up Hormonal Contraceptives		Takes up Condoms, EC or Hormonal	
	OLS	LASSO	OLS	LASSO	OLS	LASSO
	(1)	(2)	(3)	(4)	(5)	(6)
Voucher	0.532***	0.538***	0.024**	0.025**	0.073***	0.072***
	(0.027)	(0.027)	(0.010)	(0.010)	(0.018)	(0.018)
Voucher & Infertility Info	0.500***	0.503***	0.053***	0.057***	0.099***	0.105***
	(0.027)	(0.027)	(0.012)	(0.013)	(0.019)	(0.019)
N	1508	1439	1508	1439	1508	1439
Adjusted R-squared	0.238		0.011		0.016	
Control mean	0.187	0.189	0.016	0.016	0.053	0.053
P-value of $\beta^V = \beta^{VI}$	0.274	0.244	0.038	0.029	0.222	0.134

Notes: This table reports the effect of each of the treatments on visiting the partner clinic and contraceptive uptake. The OLS regressions are from running equation (1) on the clinic data and only include an indicator for whether the student was in the second recruitment wave as control. All LASSO specifications include fixed effects for baseline hormonal contraceptive usage and an indicator for whether a student was in the second recruitment wave as controls. In addition, they include any controls selected by the LASSO algorithm from the set of baseline variables that were asked to all students. This includes the following set of variables: Whether the student had ever had sex, whether the student had sex in the two weeks prior to the workshop, number of sexual partners the student had in the two weeks prior to the workshop, number of times student has been pregnant, whether student is pregnant at the time of the workshop, number of children of the student, fathers' and mothers' years of education, age of the student, indicators for year at UNZA, whether the student is married, indicators for the students' study programme at UNZA, indicators for the province the student is from, whether the student shared a bedroom growing up, whether the student is on a government bursary, distance to a clinic from where the student grew up, whether student underwent an initiation ritual, indicators for whether student has heard of each of the hormonal contraceptives, and indicators for whether the student is interested in each of the hormonal contraceptives. "Takes up hormonal contraceptives" is an indicator variable for whether a student requested any of the following: injection, implant, IUD, or oral contraceptive pills. While emergency contraceptive is hormonal, it is not a preventative method, so we do not count it as being part of this category. "Takes up Condoms, EC or Hormonal" is defined similarly to "Takes up hormonal contraceptives" but also includes condoms and emergency contraceptives. Standard errors are robust to heteroskedasticity.

Table A6
Effect on Visiting Clinics for Family Planning Services in the Survey Data

	Any Clinic (1)	Kalingalinga (2)	UNZA (3)	Other (4)
Voucher	0.072***	0.089***	0.020	-0.023
	(0.028)	(0.024)	(0.018)	(0.015)
Voucher & Infertility Info	0.075***	0.095***	0.003	0.000
	(0.027)	(0.024)	(0.016)	(0.016)
N	1495	1495	1495	1495
Adjusted R-squared	0.051	0.026	0.022	0.048
Control mean	0.233	0.137	0.076	0.078
P-value of $\beta^V = \beta^{VI}$	0.918	0.808	0.335	0.104

Notes: This table reports the effect of each of the treatments on indicator variables for ever visiting a clinic (any or different types) in the survey data by the end of the data collection period. It is estimated with equation (1) using the survey data. All regressions include indicator variables for baseline hormonal contraceptive usage and an indicator for whether a student is part of the second recruitment wave as controls. "Any Clinic" is an indicator variable for whether a student attended any clinic. "Kalingalinga" is an indicator variable for whether a student attended our partner clinic, "UNZA" is an indicator variable for whether the student attended the clinic that is on UNZA campus, and "Other" is an indicator variable for whether the student attended any other clinic. Standard errors are robust to heteroskedasticity.

Table A7
Effect on Average Contraceptive Use Over the Survey Data Collection Period With Alternative Specifications

	Any Hormonal		Pills		IUD		Implant		Injection	
	OLS	LASSO	OLS	LASSO	OLS	LASSO	OLS	LASSO	OLS	LASSO
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Voucher	-0.011	0.003	-0.007	-0.004	-0.006	-0.002	0.006	0.005	-0.005	0.005
	(0.015)	(0.012)	(0.009)	(0.009)	(0.004)	(0.003)	(0.007)	(0.005)	(0.008)	(0.007)
Voucher & Infertility Info	0.025	0.039***	0.006	0.008	-0.001	0.000	0.009	0.011*	0.010	0.018**
	(0.016)	(0.014)	(0.011)	(0.010)	(0.005)	(0.003)	(0.007)	(0.006)	(0.009)	(0.007)
N	14240	13712	14240	13712	14240	13712	14240	13712	14240	13712
Adjusted R-squared	0.002		0.000		0.001		0.001		0.001	
Control mean	0.088	0.087	0.044	0.044	0.007	0.007	0.009	0.009	0.026	0.026
P-value of $\phi^V = \phi^{VI}$	0.022	0.007	0.187	0.176	0.135	0.176	0.680	0.281	0.093	0.080

Notes: This table reports the effect of each of the treatments on contraceptive usage throughout the survey period. For the first wave, this is up to 6 months after the workshop, and for the second wave, this is 1.5 months after the workshop. The outcomes are indicator variables for whether a student used any hormonal contraceptives or any of each type of contraceptive during the survey period. OLS estimates are produced by running equation (3) with the survey data, and only include indicator variables for whether a student was in the second wave as controls. All LASSO specifications include indicator variables for baseline hormonal contraceptive usage and an indicator for whether a student was in the second wave as controls. In addition, they include any controls selected by the LASSO algorithm from the set of baseline variables that were asked to all students. This includes the following set of variables: whether the student had ever had sex, whether the student had sex in the two weeks prior to the workshop, number of sexual partners the student had in the two weeks prior to the workshop, number of times student has been pregnant, whether the student is pregnant at the time of the workshop, number of children of the student, fathers' and mothers' years of education, age of the student, indicators for year at UNZA, whether the student is married, indicators for the students' study programme at UNZA, indicators for the province the student is from, whether the student shared a bedroom growing up, whether the student is on a government bursary, distance to a clinic from where the student grew up, whether student underwent an initiation ritual, indicators for whether student has heard of each of the hormonal contraceptives, and indicators for whether student is interested in each of the hormonal contraceptives. Standard errors are robust to heteroskedasticity and clustered at the student level.

Table A8
Effects on Pregnancy With Alternative Specifications

	New Pregnancy at Endline		Reported Pregnancy					
	OLS (1)	LASSO (2)	OLS (3)	LASSO (4)	OLS (5)	LASSO (6)	OLS (7)	LASSO (8)
Voucher & Infertility Info	-0.012**	-0.013**	-0.003	-0.003	0.000	0.001	0.001	0.001
	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Voucher & Infertility Info × After 2 Months					-0.005*	-0.005*		
					(0.003)	(0.003)		
Voucher & Infertility Info × Survey							-0.001*	-0.001*
							(0.000)	(0.000)
N	1367	1317	14240	13712	14240	13712	14240	13712
Adjusted R-squared	0.002		0.000		0.001		0.001	
Control mean	0.017	0.017	0.007	0.007	0.007	0.007	0.007	0.007

Notes: Columns 1–2 estimate the effect of the voucher + infertility information treatment on an indicator variable for a new pregnancy in the last 6 months in the endline data. Columns 3–8 estimate the effects on an indicator variable for reporting a pregnancy that is not listed as a false positive in the survey data. Column 1 only includes a control for the second recruitment wave. The remaining odd columns also include survey round fixed effects. Column 2 includes fixed effects for baseline hormonal contraceptive use, an indicator variable for being pregnant at baseline, and double-LASSO selected controls. The remaining even columns also include survey round fixed effects. The pool of controls for the LASSO includes the following set of variables: whether the student had ever had sex, whether the student had sex in the two weeks prior to the workshop, number of sexual partners the student had in the two weeks prior to the workshop, number of times student has been pregnant, whether the student is pregnant at the time of the workshop, number of children of the student, fathers' and mothers' years of education, age of the student, indicators for year at UNZA, whether the student is married, indicators for the students' study programme at UNZA, indicators for the province the student is from, whether the student shared a bedroom growing up, whether the student is on a government bursary, distance to a clinic from where the student grew up, whether the student underwent an initiation ritual, indicators for whether the student has heard of each of the hormonal contraceptives, and indicators for whether the student is interested in each of the hormonal contraceptives. Standard errors are robust in columns 1–2 and robust and clustered at the individual-level in columns 3–8.

Table A9
Effects on Beliefs About Infertility & Contraceptives in Week 4 for Full Sample

	Block 1: Cause Infertility					Block 2: Cause Weight Gain				
	(1) Any	(2) Pill	(3) IUD	(4) Implant	(5) Injection	(6) Any	(7) Pill	(8) IUD	(9) Implant	(10) Injection
Voucher	-0.045	-0.021	-0.000	-0.068**	-0.041	-0.018	-0.002	0.001	0.011	-0.026
	(0.031)	(0.030)	(0.024)	(0.028)	(0.029)	(0.020)	(0.031)	(0.020)	(0.030)	(0.032)
Voucher & Infertility Info	-0.138***	-0.052*	-0.009	-0.129***	-0.095***	-0.016	-0.008	-0.005	0.032	-0.014
	(0.031)	(0.030)	(0.023)	(0.027)	(0.028)	(0.019)	(0.031)	(0.019)	(0.030)	(0.032)
N	1481	1481	1481	1481	1481	1481	1481	1481	1481	1481
Adjusted R-squared	0.015	0.004	0.003	0.020	0.014	0.005	0.005	-0.003	-0.000	0.003
Control mean	0.645	0.351	0.161	0.307	0.325	0.900	0.616	0.106	0.317	0.474
P-value of $\beta^V = \beta^{VI}$	0.003	0.294	0.710	0.018	0.054	0.919	0.832	0.778	0.478	0.712

Notes: This table reports the effect of each of the treatments on whether the student believes each type of contraceptive causes infertility – in Block 1 – or weight gain – in Block 2. It reports effects at 4 weeks with the full sample, including the second recruitment wave. It is estimated with equation (1) in the survey data. Column 1 is an indicator for whether the student believed one or more of the hormonal contraceptives shown here cause infertility. In Columns 2 – 5, the outcomes are indicator variables for each contraceptive separately. Block 2 repeats the analysis for weight gain. All regressions include fixed effects for baseline hormonal contraceptive usage. Standard errors are robust to heteroskedasticity.

Table A10
Preferences Over Contraceptives at End of the Study Period

	Interested in Taking Up After 6 Months (First Wave Only)			
	Pill (1)	IUD (2)	Implant (3)	Injection (4)
Voucher	-0.007	0.022	0.028	-0.006
	(0.037)	(0.025)	(0.027)	(0.033)
Voucher & Infertility Info	-0.067*	-0.003	0.150***	-0.001
	(0.036)	(0.024)	(0.030)	(0.033)
N	1087	1087	1087	1087
Adjusted R-squared	0.017	0.017	0.028	0.012
Control mean	0.419	0.125	0.139	0.285
P-value of $\beta^V = \beta^{VI}$	0.101	0.326	0.000	0.884

Notes: This table uses the first wave sample to analyze whether the treatments persistently affected stated preferences over contraceptives at the end of the data collection period. The outcome variables are indicator variables for whether participants expressed an interest in taking up each type of contraceptive on their 11th round survey (approximately 6 months after baseline). All regressions include baseline hormonal contraceptive usage as controls. Standard errors are robust to heteroskedasticity.

Table A11
Evidence on Alternative Explanations: Role Models & Stigma

	Conversations ≥ 1 Week 4 (Survey 2) (1)	Conversations ≥ 1 Week 22 (Survey 11) (2)	Approve (3)	Mother Approve (4)
Voucher	-0.042	0.069**	0.074**	-0.067**
	(0.035)	(0.033)	(0.034)	(0.034)
Voucher & Infertility Info	-0.069**	0.015	0.044	-0.003
	(0.035)	(0.032)	(0.034)	(0.034)
N	1157	1087	1158	1157
Adjusted R-squared	-0.000	0.003	0.011	0.004
Control mean	0.412	0.243	0.614	0.368
P-value of $\beta^V = \beta^{VI}$	0.441	0.106	0.376	0.061

Notes: This table uses the first wave to estimate the effect of the treatments on proxies for stigma. The dependent variables are an indicator variable for talking to friends about contraceptives at least once a week in the last month in survey 2 (column 1) and survey 11 (column 2) and an indicator variable for whether the girl reports she approves of an unmarried woman using modern contraceptives to prevent pregnancy (column 3) or believes her mother would approve (column 4). The dependent variable in columns 3 and 4 is measured in survey 2. In columns 1 and 2, we restrict to the first recruitment wave to observe the effect in a consistent sample over time. Columns 3 and 4 restrict to the first wave since the approval questions were asked in the baseline survey for the second wave. All regression include baseline hormonal contraceptive usage as controls. Standard errors are robust.

A Quotes on Side Effects and Infertility from Focus Groups

In this section, we report several illustrative quotes from focus groups with UNZA students conducted prior to the experiment. These focus groups motivated our focus on fear of infertility as a barrier to contraceptive use. The quotations below are from students' answers to a question about why sexually active students do not use contraceptives.

"So even the same contraceptives, some of them, most of them in fact have side effects, they are not safe and they can affect your fertility, yah. Like maybe you might not be able to have children when you want to."

"Because maybe the reason they may not be having those children is as a result of the effects she has gotten from the contraceptives. I think the more you use these contraceptives, they definitely have side effects, so the more you use them, the higher the effects. So might end up maybe getting barren or something like that just because of the constant use of contraceptives."

"Afraid of not becoming pregnant because of a contraceptives. And also, they mess up with your menstrual cycle."

B Intervention Protocol

[Baseline Message]

Thank you for joining us today. Welcome to Empower Women's Health. This is a pilot program where we test different ways to empower women. We will start by reading some information about the program, and ask for your consent to join.

[Read the consent form and ask everyone to sign.]

Now, you will take your first survey, so we can see how much you already know about women's health. Please scan the QR code or type in the link to access the survey. Please fill in the information and begin the survey, we will come around to help you if you face any difficulties.

[Let everyone finish]

[instructor self-introductions, both Facilitator 1 and Facilitator 2]

Facilitator 1 Intro: Hi my name is [Facilitator 1], and I'm with the Empower Women's Health project, based in Lusaka. I am here to share some information on women's health with you, but I am not a nurse or a health worker, so if you have any questions I cannot answer, at the end of the session I will tell you how you can get more information from the Clinic. My partner will also introduce herself.

Facilitator 2 Intro: My name is [Facilitator 2] and, like [Facilitator 1], I work with Empower Women's Health.

Great! Now we are going to talk a bit as a group about how to access family planning.

Can someone define family planning for me?

Can you tell me what family planning options are there?

The commonly used contraceptive methods include condoms, morning after pills, oral pills, injectables, implants and IUD.

I would also like to talk a little about HIV. HIV is spread through exchange of particular body fluids with a person who has HIV. These fluids are blood, semen, pre-seminal fluids, rectal fluids, vaginal fluids, and breast milk. Having unprotected sex is one main way that HIV gets spread. I would like to emphasise that condoms are highly effective in preventing HIV if used correctly. They are also effective at preventing sexually transmitted diseases (STDs) that are transmitted through bodily fluids, such as gonorrhea and chlamydia. Non-condom contraceptives protect against pregnancy, but do not protect against the transmission of HIV or STDs.

[Fertility group only]

We will now share some information about how different modern family planning methods work. Different methods lasts for a different lengths of time. However, whenever you stop using the family planning, after a period of time, you will be able to get pregnant again. To understand why, we need to understand some biology. Your body releases an egg each month, which is called ovulation. If you have sex and the sperm meets the egg, that is when you can

become pregnant. Methods such as the pill, implant, and injectable are called "hormonal contraception." The hormones in these methods stop your body from ovulating, so there is no egg released, and thus you cannot become pregnant. When you stop the method, after some time, the hormones will leave your body, you will begin ovulating, and as a result,can become pregnant again. That is why if someone misses their dose of the pill, they can become pregnant—the hormones leave their body. Similarly, if you stop the shot or remove the implant, the hormones will leave your body, and you will begin to ovulate and be able to become pregnant again.

To make this more clear, let's play a game together. Anyone want to volunteer? [pick an audience member]

In this game, we are going to use an example to show how contraceptives can stop you from becoming pregnant.

[hold a strong mint underneath their nose, and hold an orange nearby, ask them if they could smell the orange]

[then have them take a few deep cleansing breaths without the mint smell and smell the orange]

The hormones in the pill, shot, or implant work like this. They block your body from releasing eggs for a little while, but after you stop using them, your body will naturally return to normal.

The longer the contraceptive works for, it's like holding the mint smell under your nose for longer. But, no matter how long, once it wears off or is removed, you will be able to smell the orange again.

Different forms of family planning last different amounts of time. A condom or female condom only works if you use it every time. The pill needs to be taken every day, but then wears off quickly when you stop taking it. Injections can last for months, and then may take some time to leave your system, but, and this is important, then you will be fertile again. Implants and IUDs last until you take them out, but you can always have them removed early if you decide you want to become pregnant, and your body will return to normal.

And finally, [name] will share her experience on using [method of long term contraceptive].

[Facilitator who has used either injectables or implants and subsequently become pregnant shares her experience, including what method she used, how long she used it, and how long it took for her to become pregnant after]

Facilitator 1 personal story:

I would like to share my personal story. I have two handsome sons, one is 8 years and my second is 5 years. Before I had my first kid, I first used condoms and later tried birth control pills. I could not keep up with taking them daily. Then I had my first born. I guess as a result of not being consistent. So, after my first kid, I started using depo-provera (an injection). It was easier for me to be consistent with this because I did not need to take it as often. Then, when I decided to have my second child, I stopped. In a few months, I conceived my second child. After my second child I decided that I should concentrate on my career and my business, so I went on a long term method, which is the implant. I have had this for 4 years

now, with no complications. The insertion was very small and left me with a very small scar. [show the arm] When I just had it, I only had some spotting, small bleeding for only two weeks, and everything went back to normal. And it hasn't moved. It's just there. And whenever I want to have the next baby, I will just remove it, any time. Thank you for listening to my story.

Facilitator 2 personal story:

I have three beautiful kids, two girls and a boy. They are aged 13, 9 and 4 years. So, between my first and my second, I used Depo Provera, the shot. I didn't think it was the right method for me because I had to make a fresh arrangement to see the doctor every three months for a shot. So, after my second child, I asked myself what am I going to use that will be long-lasting and not require me to visit a health care provider every three months. So, I settled with Jadelle, the implant. After I put in the Jadelle, I had minor headaches. I only needed paracetamol, and they went away over time. My periods were also not as heavy as before. My periods were actually reduced to only 3 or 4 days. I had it implanted when my daughter was 4 months old, and I had it taken out when she was five years. Within 6 months, I conceived my son. And that's my story.

There are many ways to access family planning. For example, there is a clinic here on UNZA's campus that has family planning. Today, I want to tell you about another option to access family planning that might be especially convenient for students. At Kalingalinga clinic, you can get all types of family planning methods for free.

[Voucher/Fertility group]

In your packet you have a voucher that you can redeem for an 80 Kwacha transport refund if you decide to go to Kalingalinga clinic. To receive the refund, you only need to go to the clinic, bring your id, and have your voucher stamped by a nurse.

Does everyone know where Kalingalinga clinic is? In your packet you have a card that has a map and walking instructions. You can easily walk to the clinic following these directions. If you visit the clinic, make sure to bring this card with you—you will be guaranteed fast and completely free service with this card within the next 4 weeks. [Instructions: show the card to participant]. This card is only for you, so make sure to bring it with you, and do not give it to someone else.

The Kalingalinga clinic operates from Monday to Friday, 9 – 15 hours and on Saturday, 9 – 12 hours. The family planning consultation and family planning options including condoms, oral pills, the morning after pill and injectables are offered every day. You can also access implants (also known as Jadelle), but these are only offered on Tuesdays, Thursdays, and Saturdays. We want to emphasize that if you bring this card, you will not be charged for *any* contraception method.

[Voucher/Fertility group]

To claim your refund, make sure to hand your voucher to the nurse at the beginning of your appointment, and give it to the "Empower Women's Health" representative afterwards. The representative will be sitting in the waiting area. The voucher can only be redeemed by you. To verify that it is you who is coming to the clinic, you must bring your student id, or another

form of id. Without it, you will not be allowed to claim the voucher. Because this is a pilot program, women at some workshops may not get a voucher.

Over the next 6 months, as part of this program, we will ask you to complete a short mobile survey every two weeks. For every survey you complete we will send you 10K of airtime. In addition, to ensure you do not incur any costs for completing the surveys, we will send you 50K of airtime every month until the end of the program. If you stop filling out the surveys, we will stop sending the monthly bundle. The first 50K bundle of airtime will be in your accounts within the next 1-2 days. If you change your phone numbers, please let us know! You can either email us, through the email address we sent you the invitation from, or text us, using the number quoted in the reminder message.

Thank you for coming through, we hope we'll see you at Kalingalinga clinic within the next 4 weeks.

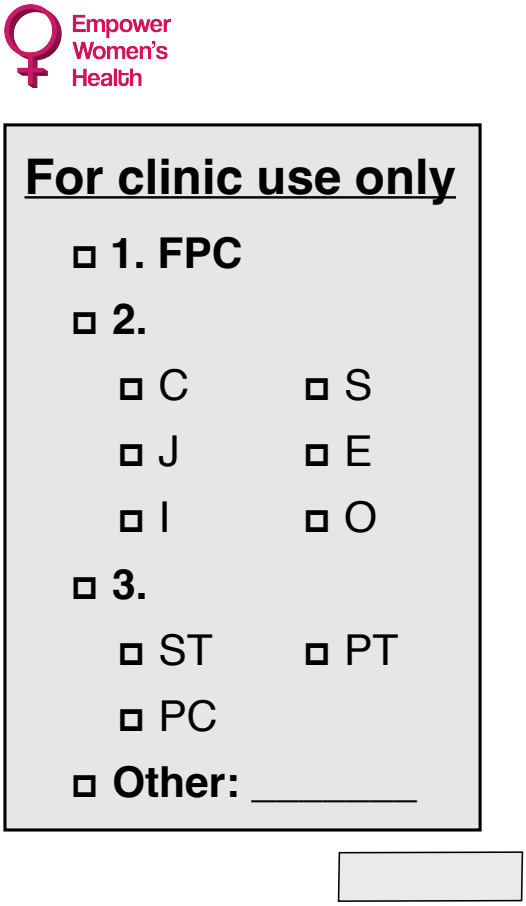
C Intervention Documents

Figure C1
Clinic Information Card

(a) Front

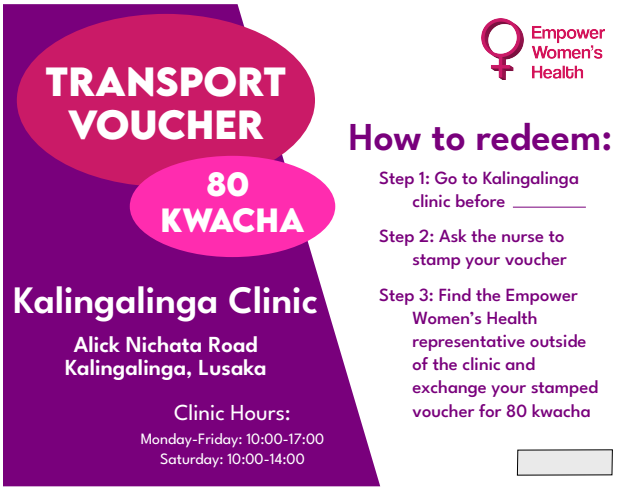


(b) Back



Notes: This figure shows the front and back of the clinic information card given to participants in all experimental arms during the workshop.

Figure C2
Voucher & Voucher + Infertility Information Treatments: Travel Voucher



Notes: This figure shows the travel voucher given to participants in the CCT and fertility arms of the experiment. Partipants could have the voucher stamped by a nurse at the clinic and then redeem it for 80 Kwacha from a study employee stationed at the clinic.

Figure C3
Explanation of Codes on the Back of the Information Card



Notes: This figure explains the codes used by the nurses to record what treatments participants received on the back of the clinic card.

D Machine Learning for Heterogeneous Treatment Effects

In this section, we use the double machine learning causal forest estimator to investigate what baseline variables lead to heterogeneity in the treatments' effects on hormonal contraceptive take-up. The goal of this exercise is to assess whether machine learning delivers similar lessons to the compliers analysis about which individuals benefited the most from the treatments.

Athey et al. (2019) and Wager and Athey (2018) develop a non-parametric causal forest methodology for identifying heterogeneous treatment effects using machine learning. A causal forest is composed of causal trees. The causal tree methodology randomly splits the sample into a training and validation sample and chooses partitions of the training sample (e.g., young women with and without a fear of infertility) for whom the treatment of interest (voucher + infertility information or voucher) is allowed to have different effects. These partitions are chosen to maximize the out-of-sample predictive power in the validation sample. Since the sub-samples are randomly selected for each causal tree, causal trees may choose different (though potentially highly correlated) covariates as sources of heterogeneity. Hence, to arrive at individual-level estimates of treatment effects that are not driven by a specific random draw of the data, a causal forest "grows" a large number of causal trees and estimates the individual-level treatment effects by averaging across the predicted effect for an individual in each causal tree. In this method, for each causal tree, a new training and validation sample are drawn. To calculate valid point estimates, we create each causal tree using the same randomly chosen, distinct subsample b of our main sample N, where $b = 0.5N$ (this is the sample from which the validation and testing samples are randomly drawn). The remainder of the observations are never used for estimating causal trees and are instead used to estimate the treatment effects based on the heterogeneity identified in the random forest. To ensure that we have enough participants in each arm to estimate valid treatment effects, we stratify by treatment status when drawing b.

When we implement this methodology, we apply a correction for regularization bias and overfitting developed by Chernozhukov et al. (2018a). We implement the algorithm using the econML package developed for python by the ALICE project at Microsoft research. We conduct the analysis for each intervention – voucher or voucher + infertility information – separately against the control. We take most of the parameters from the default settings in the econML package but impose a 5-fold cross-validation strategy and set the minimum number of observations per leaf (subsample size resulting from splits along covariate values) to 10. To compute our final estimates, we average over 100,000 trees and predict the treatment effects in the distinct sample b. For our outcome, we use take-up of hormonal contraceptives in the clinic data, consistent with the compliers analysis.

Since any specific causal tree may arrive at different partitions for the treatment effects, to characterize patterns in the heterogeneity of the effects, instead of reporting specific partitions, we (a) report the estimated average treatment effects for different subgroups, where our choice of subgroups is motivated by the compliers analysis, and (b) report the average values of our baseline covariates from the compliers analysis for the individuals estimated to have the 20% largest and smallest treatment effects for the voucher and voucher + infertility information treatments.[35]

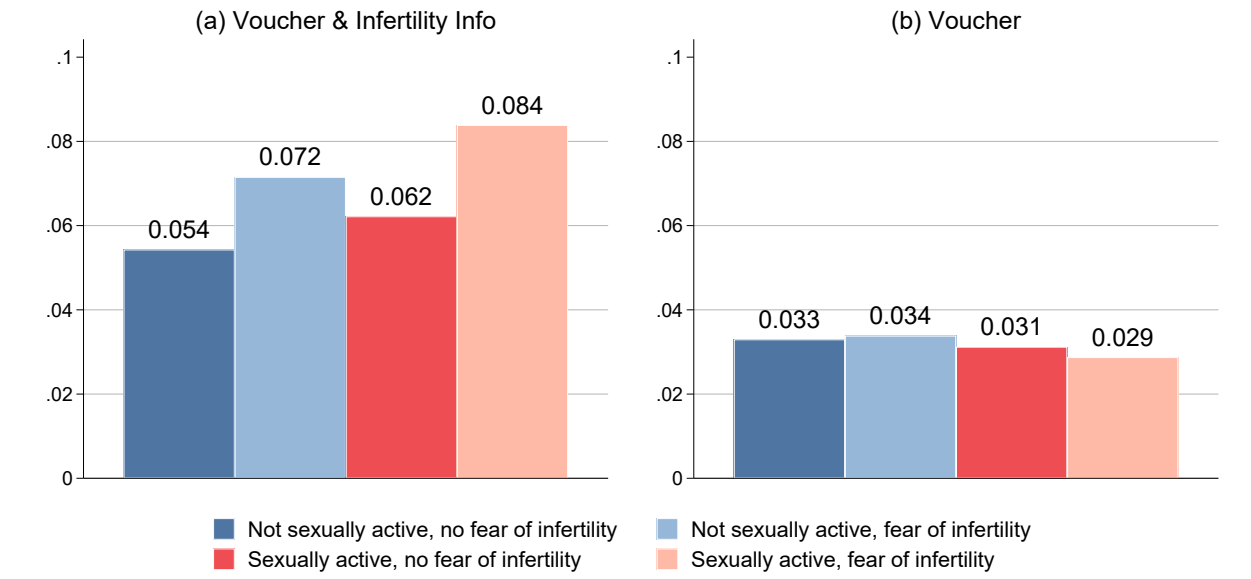
Figure D1 reports the predicted treatment effects for the voucher + infertility information (Panel (a)) and voucher (Panel (b)) treatments by whether participants had had sex at baseline and whether they reported not using contraceptives due to fear of infertility at baseline. Consistent with the compliers analysis, we see the largest effects (+8 percentage points) among women who were sexually active at baseline and did not use contraceptives due to fear of infertility. This is more than 50% larger than the effect for those who were not sexually active and did not fear infertility. In contrast, the magnitudes for the voucher intervention are similar across subgroups.

Table D1 follows Chernozhukov et al. (2018b) and reports the average values of participants' characteristics for those in the top and bottom 20% of treatment effects for both the voucher + infertility information and voucher interventions. The results again echo the compliers analysis. Among those with the largest treatment effects for voucher + infertility information, 66% reported fear of infertility/side effects at baseline (0% among those with the lowest predicted effects) and 87% were sexually active at baseline (28% among those with the lowest predicted effects). We note that characteristics that show up as significantly different between those with the largest and smallest treatment effects need not be the actual partitions selected by

[35] We exclude "Use hormonal contraception" because "Fear of Infertility/Side Effects" is only defined for students who do not use a hormonal contraception at baseline, and we cannot have missing values when running the causal forest algorithm.

the machine learning procedure; they could also be highly correlated with the characteristics which the treatment effects are partitioned on. For example, using condoms is likely to be strongly related to being sexually active, and parental education is related to SES. For the voucher treatment, the patterns are completely different. Consistent with the fact that the voucher did not affect beliefs about infertility, the directionality is flipped. Among those with the smallest treatment effects for the voucher group, 33% reported fear of infertility/side effects at baseline (16% among those with the highest effects). Interestingly, it was also most effective for the financially constrained. Altogether, we conclude that the machine learning approach delivers qualitatively similar findings to the compliers analysis and supports our interpretation that the voucher + infertility information treatment's larger effect relative to the voucher is due to reducing the perception that contraceptives cause permanent infertility.

Figure D1
Predicted Treatment Effect by Subgroup



Notes: This figure displays the average predicted treatment effects in the test sample for four different subgroups separately for the voucher + infertility information (Panel (a)) and voucher (Panel (b)) groups. The subgroups chosen are motivated by what we found in the compliers analysis (Table 7). The predicted treatment effects are calculated by training a causal forest on half of the sample and then predicting the treatment effects in the remaining half. We restrict to students that did not report missing observations for any of the baseline covariates reported in table D1.

Table D1
Characteristics of Participants with High and Low Predicted Treatment Effects

	Voucher & Infertility Info			Voucher		
	(1)	(2)	(3)	(4)	(5)	(6)
	20% Highest β	20% Lowest β	P-value	20% Highest β	20% Lowest β	P-value
Fear of Infertility/Side Effects	0.656	0.000	0.000	0.156	0.330	0.006
Sexually Active	0.871	0.277	0.000	0.556	0.780	0.001
Use Condoms	0.237	0.032	0.000	0.356	0.000	0.000
SES Index	-0.435	0.970	0.000	-0.351	0.024	0.012
Father Years Education	12.581	12.255	0.502	9.878	12.923	0.000
Mother Years Education	9.570	11.234	0.009	8.911	10.341	0.031
Age	22.419	19.851	0.000	21.444	21.681	0.313
Year	2.828	1.745	0.000	2.422	2.077	0.021

Notes: This table reports the baseline characteristics of students who are in the top and bottom quintile of the treatment effect distribution for the voucher + infertility information intervention (Columns 1 and 2) and voucher intervention (Columns 4 and 5). Column 3 (for voucher + infertility information) and 6 (for voucher) report the p-value for tests of whether the characteristics of students in the top quintile of the treatment effect distribution are significantly different from students in the bottom quintile. All variables are measured at baseline. The SES Index is the first predicted component from a pca of indicator variables for being from Lusaka (the capital), sharing a bedroom growing up, being on government bursary, growing up in a place where the clinic was more than 30 minutes walking distance away, and having undergone an initiation ritual. "Fear of Infertility/Side Effects" indicates the respondent does not use hormonal contraceptives for these reasons, while "Use Condoms" indicates that she reports not using hormonal contraceptives at baseline because she uses condoms.

E Extension Experiment on STI-Testing: Text Messages for Treatment & Control

This section presents the text messages to the control and treatment groups for the extension experiment on the take-up of STI testing. Additional text in the treatment messages that did not appear in the control messages is in bold.

Control:

1. *Empower Women's Health is sponsoring Free STI testing at Kalingalinga Clinic from 23 May to 6 June, Monday to Friday, 9am to 3pm.*
2. *Many women in your age group have STIs but show no symptoms.*
3. *Show your student ID and this text message at the clinic to receive a 40 K transport refund. You do not need to take up any health care to receive this refund.*

Treatment:

1. *Empower Women's Health is sponsoring Free STI testing at Kalingalinga Clinic from 23 May to 6 June, Monday to Friday, 9am to 3pm. A simple test can protect your ability to have children in the future!*
2. *Many women in your age group have STIs but show no symptoms.* ***Untreated STIs can lead to scarring that prevents pregnancy, which causes 85% of infertility in Sub-Saharan Africa.***
3. *Show your student ID and this text message at the clinic to receive a 40 K transport refund. You do not need to take up any health care to receive this refund.*