The Power to Protect: Household Bargaining and Female Condom Use*

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Abstract: Women may face systematically greater benefits than men from adopting certain technologies. Yet women commonly hold lower bargaining power, which may constrain adoption when decisions are joint. Introducing a version of the technology that is second-best in terms of cost or effectiveness, but more acceptable to men, may increase adoption and welfare. We conduct a field experiment introducing female condoms – which are less effective and more expensive than male condoms, but often preferred by men – in a setting with high HIV prevalence. We find strongest adoption among women with low bargaining power, who were previously having unprotected sex.

JEL classification: C78, O33, C93, J16, I12

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1 Introduction

The costs and benefits of adopting household technologies may differ systematically across genders. There is evidence that women have a stronger preference for risk reduction (Agnew et al., 2008), investment in children's education (Duflo, 2003), and investment in health via nutritious food (Duflo and Udry, 2004; Attanasio and Lechene, 2014). Women may also bear more of the costs of technology non-adoption, through responsibility for domestic chores, caring duties, and greater exposure to certain health and safety risks. If a technology can be adopted unilaterally then even women with low bargaining power may be able to adopt, for example the pill or concealable contraceptives (Goldin and Katz, 2002; Ashraf et al., 2014b). When adoption of a technology requires agreement between partners, intra-household bargaining matters, and men's preferences may constrain household adoption. Examples include improved cookstoves (Miller and Mobarak, 2013), private latrines, anti-malarial bednets, and condoms. One way to increase adoption may be to target men's preferences (Stopnitzky, 2017), although this can prove difficult (Creese et al., 2002). Another option is to increase women's bargaining power directly (Bandiera et al., 2015; Ashraf et al., 2017), or via giving women control of income from government safety nets (Field et al., 2019). A substantial proportion of women, however, will continue to have low bargaining power in the absence of broader changes in labour and marriage markets. In contexts where low female bargaining power and male preferences continue to constrain adoption, a second-best solution may be to introduce a variant of the technology that is more acceptable to men, even if less effective or more costly.

This paper puts forward and tests this idea, using a field experiment in Maputo Province, Mozambique. We study adoption of condoms: a technology which is observable to both parties and hence requires joint adoption;[1] but where women face higher costs from non-adoption, via higher risk of contracting HIV (in this context) and unwanted pregnancy.[2] We examine how intra-household bargaining affects adoption of

[1] Female condoms can be inserted by women prior to intercourse, but remain observable.

[2] In 2015, women accounted for 59% of all individuals aged 15 and over living with HIV in Sub-Saharan Africa, and the rate of new infections among young women aged 15-24 was double that among young men (UNAIDS, 2016a). Reasons for this gender disparity include that women tend to have older partners, lower access to sexual and reproductive health services, and a higher biological risk than men of becoming infected from heterosexual intercourse (UNAIDS, 2016b).

female condoms when they are introduced, in a setting where only male condoms are available. Female condoms are second-best insofar as they have lower effectiveness than male condoms and higher unit cost.[3] However, female condoms are viewed by men as less uncomfortable and less stigmatising than male condoms (Philpott et al., 2006; Wanyenze et al., 2011; Koster et al., 2015). We show that women with lower bargaining power — many of whom are unable to convince their partners to use male condoms at baseline — convince their partners to adopt female condoms when they are made freely available. An illustrative cost-benefit analysis shows that this could lead free provision of female condoms to be cost-effective, but that this result is sensitive to a behavioural response that we observe: namely, an increase in the number of sex acts.

Condoms are an important technology from a public health perspective, as they are the only well-established protection against HIV/AIDS and other sexually transmitted infections (STIs). They exemplify technologies where adoption is partially or fully observable within the household, agreement of both partners is needed to ensure sustained and proper use, and hence bargaining may constrain adoption. Indeed there is evidence that women may struggle to convince their partners to use male condoms (Anderson, 2018), helping to explain their persistent under-adoption.[4] Condoms are also a particularly good technology for studying the implications of introducing a second-best version of a technology. This is because existing epidemiological models of HIV transmission allow us to quantify the potential trade-offs between improving condom coverage and decreasing average effectiveness — as well as behavioural responses such as increases in the frequency of risky sex — while taking into account the negative externalities from HIV transmission.

We evaluate a condom programme in the slums of Maputo, Mozambique. The

[3]Female condoms have 95% efficacy at preventing pregnancy in the first year of use if perfectly used, or 79% effectiveness in ordinary use (Farr et al., 1994; Trussell, 2011; Beksinska et al., 2012). Male condoms are 98% effective if perfectly used, or 85% effective in ordinary use (`https://www.who.int/news-room/fact-sheets/detail/family-planning-contraception`). The unit production cost for female condoms at current volumes is $0.57 compared to $0.03 for male condoms (Mantell et al., 2015). There is currently a monopoly on the production of WHO-approved female condoms, and consequent low production volumes (Peters et al., 2010). Lower-cost female condoms have been developed in India and approved by the EU, but are still awaiting WHO approval (*ibid.*). Costs would likely substantially decrease at a larger scale of production (Dowdy et al., 2006).

[4]An estimated 3.3 billion risky sex acts took place without condoms in Sub-Saharan Africa in 2015, leading to 910,000 new HIV infections (UNAIDS, 2016a).

programme seeks to increase condom use by offering female condoms alongside male condoms. Women attend a series of group sessions that provide information about contraceptives including female condoms. Female condoms are also added to the set of products carried by local health workers — which already includes male condoms — that participants can access freely and discreetly at the end of each session. The intervention thus allows us to study which women, if any, adopt female condoms when informational, access, and price constraints are alleviated. Importantly, free provision allows us to study couples' willingness to adopt, unconfounded by their ability to pay, which may be correlated with female bargaining power. Free provision is also arguably the most relevant policy option in countries with high HIV/AIDS prevalence, where male condoms are typically already provided for free by the government.

We conduct a phased-in randomised control trial to assess the short-run impacts of the programme on women who were assigned to receive it at the end of 2014, compared to those who were assigned to receive it six months later. In addition to baseline and endline data, we collect weekly sexual diary data for a subsample of the women. This allows us to investigate impacts at the sex-act level, including effects on the frequency of sex acts. It also reduces concerns about recall bias and misreporting, which we further mitigate by recording the number of condoms that all participants take with them after each session. To measure bargaining power, we collect information about assets brought by the woman to the relationship, and also use two different survey modules covering decision-making and power dynamics in the relationship (Donald et al., 2017).

To formalise our predictions, we introduce a collective model of the household, where partners jointly choose STI protection methods. Both men and women value the levels of pleasure and of health protection associated with different contraceptive technologies. However, for the reasons outlined above, we argue that the marginal rate of substitution between health and pleasure is greater for women than for men. When the only STI protection options available are male condoms or unprotected sex,[5] the model predicts that women may prefer to use male condoms, but that those with low bargaining power may be unable to convince their partners to do so. When female condoms are introduced, the model predicts two effects. First, on the intensive margin, women with intermediate bargaining power may adopt female condoms. This is because some women with

[5]This includes sex protected by pure contraceptives such as the pill, but not by an STI protection method; see Section 3 for details.

low bargaining power who were previously having unprotected sex may now be able to convince their partners to adopt female condoms (but not male condoms), increasing condom coverage. At the same time, some women with intermediate bargaining power who were previously using male condoms may also substitute into using female condoms, decreasing average condom effectiveness. The relative magnitudes of each of these margins of response depend on how "close" the technologies are, as well as the distribution of bargaining power. These magnitudes are important to determine empirically, in order to establish total effects on transmission of HIV/AIDS and other STIs. Second, on the extensive margin, some couples who were previously not having sex now have sex with female condoms.

The results show a large impact of treatment on female condom use: an increase of 18.4 percentage points in the proportion of women who have ever used female condoms (equivalent to 209% of the endline mean in the control group) and an increase of 7.7 percentage points (385%) in the proportion of those currently using female condoms. We see no significant evidence of substitution away from male condoms, which is in line with our model's predictions given that male and female condoms are "close" on the technology frontier given their pleasure and health properties. As predicted by the model, adoption of female condoms is driven by women with lower baseline bargaining power, and those who are having unprotected sex at baseline. These results are robust across our measures of bargaining power. On the extensive margin, the diary data show that treatment leads to an increase of 9.1 percentage points (19%) in the probability that an individual has sex each week. We rule out various alternative explanations for the heterogeneous treatment effect by bargaining power: this result does not appear to be driven by experimenter demand or by women hiding male condoms, and is robust to controlling for proxies of traditional values and of access to condoms, as well as use of other contraceptives, HIV status, and beliefs about HIV risk.

Given that this is a second-best technology, a social planner should weigh the observed increase in condom coverage against the reduction in average condom effectiveness and increase in production and distribution costs.[6] To demonstrate the potential

[6]Given that the negative health effects and externalities of unprotected sex are large in the context of our study, it is reasonable to assume that these are the social planner's first-order concern, and to abstract from quantifying individuals' pleasure from using different types of condoms.

magnitude of this trade-off, we conduct an illustrative exercise in which we estimate the costs and benefits of scaling up access to female condoms to all of Southern Mozambique, focusing solely on the benefits in terms of reduced HIV transmissions. In our naïve scenario before accounting for the behavioural response (i.e. the observed increase in sex acts), both our full programme and adding female condoms to existing sex education programmes actually imply a cost saving. Intuitively, this is because low female bargaining power implies that the main margin of female condom adoption is from women previously having unprotected sex, rather than substitution away from male condoms. However, once we account for the increase in sex acts, only adding female condoms to existing sex education programmes has the potential to be cost-effective. These illustrative simulations thus show how accounting for behavioural responses that may offset direct impacts of a programme is crucial (Greenwood et al., 2017).

In terms of the literature on contraceptive technologies, to our knowledge this is the first experimental study explicitly to consider how intra-household bargaining may constrain adoption of male condoms. The existing literature on bargaining within couples focuses on fertility (Eswaran, 2002), and emphasises limited commitment or imperfect information (Rasul, 2008; Ashraf et al., 2014b). In contrast, we emphasise bargaining over STI protection, where use of the technology is fully observable and potentially negotiated each time. Gertler et al. (2005) model bargaining over male condom use between female sex workers and male clients in Mexico, as a finite-horizon, non-cooperative interaction mediated by price. Our contribution is to model bargaining over condoms within the collective model, capturing the efficiency arising from the repeated household bargaining process that takes place within couples.

Our study also highlights female condoms as a way to reduce HIV transmission in the presence of male resistance to male condoms and low female bargaining power. Numerous studies have examined the effects of information interventions which attempt to change preferences or beliefs, or incentive interventions which attempt to change risky sexual behaviour directly (see e.g. Thornton (2008); Dupas (2011); De Walque et al. (2012); Baird et al. (2012); Bjorkman Nyqvist et al. (2015); Duflo et al. (2015)). Many of these studies focus on young women. In contrast, we highlight the importance of considering male preferences in contexts where men typically hold high bargaining power within couples. Medical studies have shown that introducing female condoms

alongside male condoms improves protection rates (Fontanet et al., 1998; Vijayakumar et al., 2006; Coman et al., 2013; Mantell et al., 2015), but have largely overlooked the role of intra-household bargaining. Meanwhile Ashraf et al. (2014a) examine the effect of incentives on agents selling female condoms, but do not study impacts on end users.

More broadly, this study contributes to a literature examining the relationship between intra-household bargaining and technology adoption, for example in the form of cookstoves (Miller and Mobarak, 2013; Mohapatra and Simon, 2017), savings accounts (Schaner, 2015), saving through ROSCAs (Anderson and Baland, 2002) and microfinance (Van Tassel, 2004). To our knowledge, we are the first study explicitly to model and estimate the trade-offs inherent in introducing a second-best technology, when low female bargaining power constrains adoption of the first-best technology.

2 Context and intervention

2.1 HIV and condom use in Maputo

Our study took place in Matola, which is the capital of Maputo Province and lies approximately 10km west of Maputo City. HIV prevalence in Maputo Province is high and disproportionately affects women, at an estimated 29.6% for women and 15.8% for men (Ministério da Saúde, 2015). Concurrency among men has been identified as a contributing factor, even among men in stable relationships (Macia et al., 2011). Indeed, 85% of the women in our sample are in stable relationships, but of these 36% report believing their partner is "involved" with other people. In such a climate, technologies which protect against transmission of HIV and other STIs are not close substitutes for pure contraceptive technologies such as the pill, and may be used in addition to pure contraceptive technologies. In our baseline sample, 39% of respondents are currently using pure contraceptive methods (mainly the pill or injectables), and of these 40% are also currently using male condoms.

Both male and female condoms are available in Matola, but male condoms are far more accessible. Female condoms are typically only available at health facilities, which subjects report would take on average 60 minutes to reach, and even there are subject to frequent stock-outs (Pilz, 2014). In contrast, male condoms are readily available, both for free at health facilities and cheaply on the private market. Yet despite the

widespread availability of male condoms, there is evidence that men's preferences constrain adoption.[7] Of the women in our study who are currently sexually active but not using any form of protection at baseline, by far the most common reason given is that their partner does not like to or refuses to use them (45% of responses).

2.2 Female condom intervention

Evidence suggests that small-group information and education interventions may be particularly important for promoting female condom use (Terris-Prestholt and Windmeijer, 2016). The intervention we study is run by Pathfinder International, and is aimed at women in populations with high HIV transmission risk. The programme consists of six group sessions lasting ninety minutes each, held fortnightly over a three month period. Pathfinder trains female health workers from the local area to facilitate the programme, and thus facilitators are socially proximal to the participants. The sessions cover: information on female condoms and demonstration of their use on pelvic models; information about other contraceptive methods; information on HIV/AIDS and other STIs; and discussions around consent, negotiation of contraceptive use, intimate partner violence, and women's rights.[8] Group sizes range from a minimum of five to a maximum of twelve women per facilitator, which are thresholds set by the NGO for creating an environment conducive to discussion. Female condoms are also added to the set of products carried by local health workers — which already include male condoms — that participants can access freely and discreetly at the end of each session.

The intervention thus allows us to study which women, if any, adopt female condoms

[7] As well as being more vulnerable to HIV/AIDS, women in our sample also report lower desired current fertility than their male partners: 12% of women in stable relationships say that they want another child now, whereas 23% claim that their partner does. Men may also have higher desired total fertility: 68% of women claim their partner wants another child, whereas only 55% of the women say they themselves want another child. This is in line with evidence from the 2011 Mozambique Demographic and Health Survey (DHS) which shows that married women aged 15-49 years have a substantially lower ideal number of children (5.3) as compared to married men (6.1) (DHS, 2011). Indeed, evidence from twenty-five Sub-Saharan African countries shows that married women reporting lower desired fertility rates than married men is a widespread phenomenon (Westoff, 2010).

[8] Qualitative evidence from the medical literature suggests that information about use and about negotiation help introduction of female condoms (Schuyler et al., 2016). The discussions are also included for ethical reasons, to mitigate any risk of these women facing increased violence when introducing new contraceptives into the home.

when informational, access and price constraints are alleviated. The estimated treatment effect may also include the effect of simply coming together in a group with other women to discuss personal issues. We do not attempt to disentangle these mechanisms, since our primary objective is to study how bargaining power affects adoption of female condoms once all constraints to adoption apart from intra-household bargaining are alleviated. Moreover, any standard sex education programme would likely involve all of these components, their combined impact is arguably of most interest to policymakers.

3 Theoretical framework

In this section we introduce a simple model of intra-household bargaining over STI protection technologies. We abstract from pure contraceptive technologies such as the pill, in light of the evidence discussed above that these are not close substitutes for STI protection methods in contexts such as our study setting. We use the model to formalise two main predictions about the effects of making female condoms freely available: first, on the intensive margin, female condoms will be adopted by women with intermediate bargaining power; and second, on the extensive margin, this will lead to an increase in the probability that couples have sex. For ease of representation, we present the model here without the possibility of intra-household transfers – for example, if one partner offers to do more household chores in order to compensate the other partner for a given choice of contraceptive technology. Online Appendix B.1 shows that all of the predictions are robust to generalising the model to allow for transfers, as long as those transfers are not perfectly frictionless: a reasonable assumption if there are utility costs to negotiating transfers, or productivity losses from overriding the usual division of chores within the household.

Preferences: Consider a population of heterosexual couples each consisting of a male m and a female f. When considering the choice of STI protection technology, individual i has preferences over the levels of pleasure (P) and health (H) that the technology yields on average to the population, $u_i(P, H)$, which is quasi-concave and increasing in each argument. For example, P may include the average level of discomfort associated with the material used to produce the technology, and H may include the average level

of HIV transmission risk provided by the technology. We allow for idiosyncratic and gender-specific heterogeneity in preferences over P and H through the utility functions. For example, an individual may place a larger weight on health if she is particularly risk-averse, or believes that she has a particularly high risk of HIV infection due to her beliefs about her partner's sexual behaviour. However, we assume that on average, couples' preferences satisfy the following single-crossing property:

Assumption 1.

$$\frac{\partial u_m(P,H)/\partial P}{\partial u_m(P,H)/\partial H} > \frac{\partial u_f(P,H)/\partial P}{\partial u_f(P,H)/\partial H} \tag{1}$$

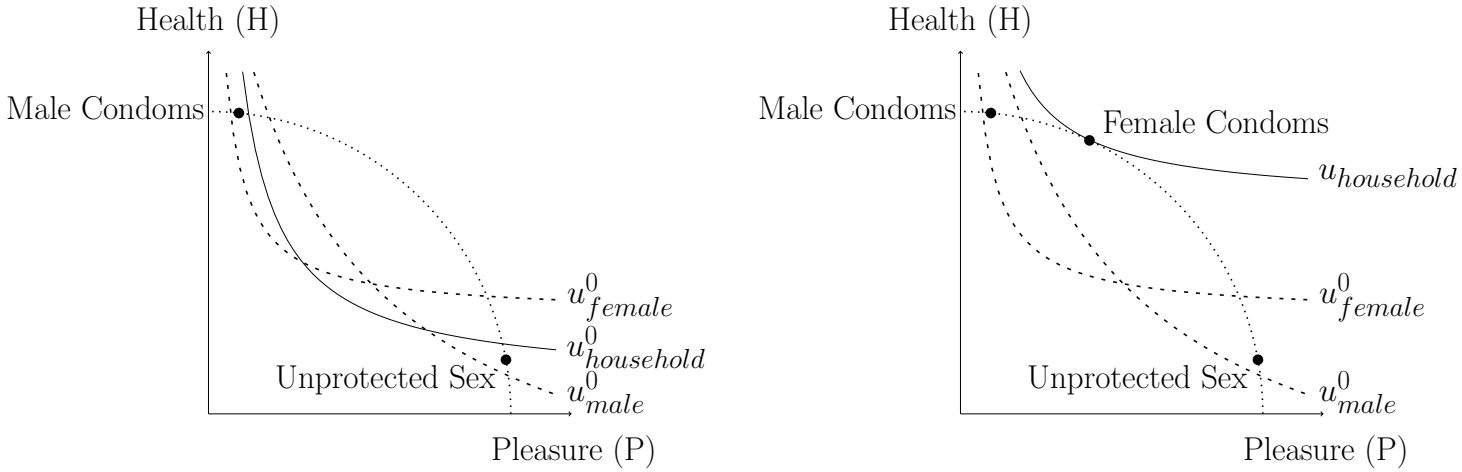
That is, we argue that the marginal rate of substitution between health and pleasure is greater for women than for men. This assumption is motivated by the facts discussed above, that women on average face greater risk of contracting HIV and greater costs from pregnancy than men do, and that men have stronger reported displeasure and stigma from condom use.

Technologies: In general, let the STI protection technology frontier be represented by a continuously-differentiable function $P(H)$ for $H \in \left[\underline{H}, \overline{H}\right]$. By definition of being on the frontier, $P'(H) < 0$, and let $P''(H) \leq 0$ such that the frontier is weakly concave. This is illustrated in Figure 1. In reality, only certain points on the frontier are easily accessible to couples, depending on the technologies that are readily available.[9] For simplicity, we assume that prior to our intervention, the set of readily available technologies, unprotected sex (US) and male condoms (MC), is just the binary set of points on the frontier $\{US, MC\}$. This is presented in Panel 1a in Figure 1. We model no sex (abstinence) as an outside option, rather than a technology on the frontier $P(H)$, see below. Male condoms offer greater health than unprotected sex (US) because of their protection against HIV/AIDS and other STIs, but offer lower pleasure.

Our treatment expands the set of readily available technologies, by introducing female condoms (FC), to the ternary set of points on the frontier $\{US, FC, MC\}$. As discussed in Section 1, female condoms provide lower effectiveness and thus lower health

[9]Couples could mix their use of two or more technologies such as to obtain a wider range of points on the frontier. By abstracting from such solutions, we are effectively assuming that due to transaction costs from mixing, the range of points available on the frontier is larger the greater the number of technologies available, and couples prefer to adopt a new technology that yields a given point rather than mixing two other technologies to obtain that point.

Figure 1: Intra-household bargaining over STI protection technologies



(a) Before the introduction of female condoms

(b) After the introduction of female condoms

Notes: The dotted line is the STI protection technology frontier. The dashed line labelled u^0_{female} represents the reservation utility of the female. The dashed line labelled u^0_{male} represents the reservation utility of the male. The solid line labelled $u^0_{household}$ represents the reservation utility of the household. The solid line labelled $u_{household}$ is the indifference curve of the household that maximizes utility in case female condoms are available.

than male condoms, but are considered more pleasurable by both genders. For both men and women, female condoms hence represent an intermediate option between male condoms and unprotected sex, as shown in Panel 1b in Figure 1. Of course, couples may have initial uncertainty about the pleasure and health associated with female condoms. In what follows we abstract from such uncertainty and consider the permanent adoption decision, once learning has taken place.

Co-operative decision-making: Almost all women in our sample (91%) report in the survey that they can refuse sex with their partner, and thus we can assume that the woman's (as well as the man's) participation constraint is binding. It is reasonable to assume that decision-making over condom use occurs under full information – since use of both male and female condoms is observable by both parties. We can also assume commitment, since in stable couples (who comprise 85% of our sample) the decision to use condoms can be thought of as a repeated game with an infinite horizon. It is therefore natural to make the following modelling assumption:

Assumption 2. Decisions over STI protection technologies are taken co-operatively, resulting in choices that are Pareto efficient.

Chiappori (1992) shows that any bargaining process which satisfies these properties can be represented by the collective model, in which the household maximises the following utility function

$$V = \alpha u_f \left(P(H), H\right) + (1 - \alpha)\, u_m \left(P(H), H\right), \tag{2}$$

where $\alpha_i \in [0, 1]$ is the woman's Pareto weight in the couple's sharing rule (Browning and Chiappori, 1998). The weight α may depend on factors such as the woman's relative contribution to the couple's income and housework, and her options outside of the relationship. It is important to note that such a model can accommodate altruistic or "caring" preferences, where each individual's utility function also has the other partner's consumption as an argument. We abstract from this here by making P and H public goods. However, intuitively the predictions below about the relationship between female bargaining power and choice of STI protection technology would hold if pleasure and health were private, as long as both partners' degree of altruism was not perfect.

As a simplification, we also assume that the financial and opportunity costs of ac-

quiring any of the technologies is zero, and hence that there is no budget constraint (only the constraint imposed by the technology frontier). This is true in our experimental setting, and in most public health programmes, where male and female condoms are made available for free if they are provided.

Intensive margin: It is straightforward to show that as long as Assumption 1 holds, the optimal choice of health is increasing in α. Online Appendix B.1 contains the full proof, and shows that this is still the case if partners can make transfers to one another, as long as these transfers are not perfectly frictionless. The intuition is simple: if the female places relatively greater weight on health than the male does, then the more bargaining power she holds, the more the household's choice of STI protection technology will be tilted towards health, and consequently away from pleasure.

Given this result, our first prediction follows straightforwardly (again, full proof in Online Appendix B.1):

Proposition 1. Women with intermediate bargaining power will adopt female condoms.

Intuitively, women with the lowest bargaining power will remain having unprotected sex, and women with the highest bargaining power will continue to use male condoms since they offer the highest health protection. Women with intermediate bargaining power will convince their partners to use female (but not male) condoms.

Note that this does not mean that we will necessarily observe an "inverse-U" relationship between bargaining power and female condom adoption in our experimental sample. The relationship observed in the sample will depend on the extent to which women from the full distribution of bargaining power sign up for the condom programme. Women with the lowest bargaining power may not select into our sample if their partners disapprove of them participating, or indeed if they predict that they will not be able to convince their partners to use female condoms even after the training. If so, then we may not observe the upward-sloping portion of the inverse-U relationship between bargaining power and female condom adoption, but instead observe a simple negative relationship. On the other hand, women with the highest levels of bargaining power may not sign up, if they are already able to persuade their partners to use male condoms. If so, then we may observe a simple positive relationship between bargaining

power and female condom adoption.

In terms of the margins of adoption, both couples who were previously having unprotected sex and couples who were previously using male condoms may adopt female condoms, if this interior option allows them to get closer to their optimal point on the technology frontier. Among the women who are engaging in unprotected sex at baseline, women with relatively higher bargaining power — although still relatively low bargaining power compared to the whole distribution — may take up female condoms. Among women using male condoms at baseline, women with relatively lower bargaining power may switch from male to female condoms. The quantitative importance of these margins of adoption will depend on the distribution of preferences and bargaining power in the population, but also crucially on the position of the new and old technologies on the frontier. The difference between male condoms and female condoms in terms of health protection is relatively small, as compared to the difference between unprotected sex and female condoms. The qualitative studies cited earlier also suggest that while men in particular may find female condoms more pleasurable than male condoms, this difference is not as large as the difference in pleasure between unprotected sex and use of either type of condom. As a result, male and female condoms may lie close together on the technology frontier (as shown in Figure 1). The proportion of couples in the population who are induced to switch from male to female condoms may therefore naturally be small, as there may be few couples whose optimal point on the technology frontier is between male condoms and female condoms. We might expect a larger proportion of couples in the population to switch from unprotected sex to female condoms, because there are substantial differences in the health-pleasure trade-off between unprotected sex and female condoms. This implies that many couples may have an optimal point on the technology frontier which is between unprotected sex and female condoms. This argument does not rely on switching costs.[10]

Which effect dominates empirically is an important question. If take-up of female condoms mainly comes from women who were engaging in unprotected sex at baseline, then introducing female condoms unambiguously increases rates of protection against

[10]The argument is further reinforced if there are for example learning costs which are not outweighed by the marginal improvement in the pleasure-health trade-off from a couple's perspective when moving from male to female condoms, but are outweighed by the large change in trade-off when moving from unprotected sex to female condoms.

HIV/AIDS and other STIs. On the other hand, if female condoms are mainly used as substitutes for male condoms, then offering female condoms will not lead to an increase in condom coverage. Whilst couples who switch to female condoms must be better off in terms of their private utility, the marginal loss of effectiveness is likely to reduce welfare from the perspective of a social planner, given the negative externalities inherent in transmission of HIV and other STIs.

Extensive margin: Let $s \in \{0, 1\}$ indicate the choice of whether to have sex or not. The no-sex option $s = 0$ can be enforced by either partner, and gives reservation utility u_i^0 to each partner. This can be thought of as the utility from partners' best immediate alternative, for example in terms of time use. Along with $s = 1$ partners make a choice of contraception from the available sets as described above.

It is straightforward to see that the introduction of female condoms increases the probability that both couples' reservation utilities are satisifed, and hence that $s = 1$; see Online Appendix B.1 for formal proof. This leads to our second prediction:

Proposition 2. Making female condoms freely available increases the probability that couples have sex.

To illustrate, Figure 1 depicts a couple whose reservation utilities are only both satisfied following the introduction of female condoms.

Note that bargaining power α does not enter a couple's decision as to whether to have sex or not: this extensive-margin decision depends only on individual reservation utilities and preferences over pleasure and health, and the set of readily available points on the technology frontier. Thus the effect of the intervention on the extensive margin decision is not predicted to vary by female bargaining power, conditional on these factors.

4 Study design and data sources

4.1 Study design

Pathfinder International began its female condom programme in Matola in 2011. We expanded the progamme to four additional neighbourhoods in 2014, using a phased-in experimental design with participant-level randomisation across all four neighbourhoods.

Seventeen programme facilitators — healthcare workers from the local community — were recruited and trained by Pathfinder to deliver the programme. These facilitators then conducted door-to-door recruitment to identify women willing to participate. The eligibility criteria were that women needed to be between 18 and 49 years of age, sexually active, and not pregnant.

The baseline survey was conducted by enumerators from an independent survey firm in August 2014, after the sign-up period but before randomisation and the start of the programme. At the end of the baseline survey, each participant was told that two rounds of training sessions would be organized to accommodate the large number of interested participants, and that assignment to the first or second round would be determined randomly by a computer for fairness. Once the entire sample had responded to the baseline survey, the research team randomly allocated half of the respondents recruited by each facilitator to the treatment group and half to the control group.[11] The reason for stratifying on facilitator was to improve power, and to ensure that there would be enough space for treatment and control participants to attend sessions close to their home. To limit spillovers between participants in the treatment and the control arm, we organized a third and separate set of training sessions for women who registered together and who knew one another. This separate group received the intervention at a later stage, but was not included in the study. Spillovers between unconnected individuals were less of a concern in our context, given the small number of participants compared to the total population of these neighbourhoods (which each had 20,000 inhabitants on average). Indeed, Section 5.1 presents evidence that there do not appear to be spillovers from our treatment.

The treatment group then received the intervention from September-December 2014. The endline survey was conducted in February-March 2015, five to six months after the intervention had started for treated individuals, and two to three months after treated individuals had received their last group session. Overall 317 women were recruited into the baseline survey, of whom 248 participated in the endline survey. However, one facilitator fell severely ill shortly after the baseline, and there was nobody sufficiently

[11] The randomisation was done in private, given the sensitive nature of participating in our intervention. A member of the research team took the list of respondents for each facilitator, sorted them by a randomly-generated number, and assigned the first half to treatment and the second half to control.

trained to replace her. She had recruited a total of 19 participants at baseline, half of whom had been assigned to treatment and half to control. 16 of her recruitees completed the endline. For the analysis we drop all of these women, to leave a final baseline sample of 298 and endline sample of 232. The retention rate was thus 78%, which is similar to that in other studies tracking female populations in urban or peri-urban areas (see e.g. Banerjee et al. (2015); Cohen et al. (2017)). Online Appendix Table B.1 shows that the observable predictors of attrition are not differential across treatment and control groups. However, Table 1 shows that attrition is higher in the control group (27% in the control group versus 18% in the treatment group, significant at the 10% level). Section 5.4 details a number of robustness tests that we perform to account for this.

Following the endline survey, the control group then received the intervention from March-May 2015. Due to administrative complications in rolling out the endline survey, the control group for one facilitator was administered the endline survey after having been treated. These five observations are dropped from all estimations of treatment effects, leaving a final estimating sample of 227 respondents.[12] All effects estimated below are "intent-to-treat", given that respondents assigned to treatment did not attend all sessions.

4.2 Survey data

Table 1 shows measures of key covariates and contraceptive use for the full baseline sample, and demonstrates that all are balanced across treatment and control. These variables are also balanced when attritters are excluded, see Online Appendix Tables B.2 and B.3. 85% of respondents report being in a stable relationship with an average duration of 8.7 years, comprising 63% who are married and 22% who are unmarried but still in relationships of on average 4.8 years.[13] The rest of the sample (15%) are sexually active but not in a stable relationship. The vast majority of respondents report having had just one sexual partner in the last twelve months, with 10% reporting zero partners and 3% reporting two partners. A third of respondents report being HIV-

[12]Table B.3 in the Online Appendix shows that the sample remains balanced across treatment and control when excluding these five individuals.

[13]The former includes traditional marriages and respondents who describe themselves as "living as married" but not legally married. The latter is common in this region due to the high bride price and costs of obtaining a marriage certificate.

positive, which is close to the official statistics reported above. Slightly more than 10% of respondents report having had an STI in the last three months; although this may be under-reported.[14] Fewer than half, 41%, mention the female condom when asked to list contraceptive methods that they know about.

Our primary outcome variables are the use of contraceptive methods, disaggregated by female condoms, male condoms and other modern contraceptive methods (mainly the pill and injectables). For each method, we ask respondents whether they have ever used that method, and whether they are currently using it (i.e. consider it to be part of their current portfolio). For male and female condoms, we also ask whether they have used that method in the last thirty days. Table 1 describes the baseline values of each of these measures. Baseline use of female condoms is low: 9% of the respondents have ever used a female condom, 3% have used one in the last 30 days, and 2% are currently using female condoms. Male condom use is substantially higher: around three quarters of women have ever used a male condom, 32% have used one in the last 30 days, and 39% percent say they are currently using male condoms. Altogether, 39% of our sample are currently using pure contraception methods at baseline, comprising 20% using the pill and 14% using injectables, and a small number using intrauterine devices (IUDs), the diaphragm, and sterilisation. These women may have signed up to the female condom programme as a way to switch out of these methods, or because they are seeking additional protection against HIV/AIDS and other STIs.

Finally, Table 2 compares our sample to a representative urban sample of women from Maputo Province, from the 2011 Demographic Health Survey (DHS, 2011). It is important to stress that we did not seek to recruit a representative sample of women into our intervention; not least because it may have been unethical (as well as very difficult) to convince the least empowered women to attend, given our prediction that such women would never be able to convince their partners to use male or female condoms. Table 2 is, however, important for two reasons. First, our sample happens to be close to the overall adult female population of Maputo Province in terms of demographic characteristics such as age, years of education, marital status, pregnancy,

[14] We do not test for HIV, since the accuracy of testing is sensitive to the timing of infection, especially shortly after infection, and our endline survey is only a few months after the end of the intervention. We also opted not to test for STIs such as chlamydia, given the already sensitive nature of participation in the study and the budgetary implications of providing treatment to those who test positive (as required by medical research ethics guidelines).

Table 1: Baseline balance of covariates and use – full sample

	(1) Mean	(2) Control Mean	(3) Treatment Mean	(4) t-test	(5) Total N	(6) Control N	(7) Treatment N
Demographics							
Age in years	30.32	30.12	30.52	-0.42	298	146	152
Years of education	6.21	6.26	6.17	0.27	295	146	149
Literate	0.84	0.84	0.85	-0.17	295	144	151
Respondent is household head	0.22	0.21	0.24	-0.51	298	146	152
Income							
Has job	0.38	0.42	0.33	1.64	295	144	151
Personal income last 30 days (MZN)	745.85	854.52	641.46	1.41	298	146	152
Relationships							
In a stable relationship (incl. married)	0.85	0.85	0.84	0.17	298	146	152
Married (officially or unofficially)	0.63	0.64	0.62	0.37	297	146	151
Years relation	8.66	8.62	8.70	-0.08	235	114	121
# Partners last 12 months	0.92	0.92	0.93	-0.23	298	146	152
Sexual knowledge & behaviour							
Pregnant	0.05	0.05	0.06	-0.42	297	145	152
HIV positive (self-report)	0.33	0.35	0.31	0.75	260	129	131
STI last 3 months (self-report)	0.13	0.13	0.13	-0.10	259	124	135
Mentions female condoms as contraceptive	0.41	0.44	0.39	0.90	296	146	150
HIV knowledge score	5.56	5.61	5.53	0.97	290	140	150
HIV risk perception general	4.54	4.59	4.50	0.92	297	145	152
HIV risk perception for self	4.61	4.62	4.60	0.22	298	146	152
Baseline use							
Ever use female condoms	0.09	0.09	0.09	0.11	298	146	152
Ever use male condoms	0.74	0.76	0.73	0.59	298	146	152
Ever use other	0.72	0.72	0.72	0.04	298	146	152
Use female condoms last 30 days	0.03	0.01	0.04	-1.39	298	146	152
Use male condoms last 30 days	0.32	0.28	0.35	-1.26	298	146	152
Current use female condoms	0.02	0.02	0.03	-0.33	298	146	152
Current use male condoms	0.39	0.37	0.41	-0.79	298	146	152
Current use other	0.39	0.41	0.37	0.75	298	146	152
Attrition							
Interviewed at endline	0.78	0.73	0.82	-1.86*	298	146	152

Notes: N=298 in the baseline sample. Lower sample sizes reflect observations that are missing or not applicable. "Treatment" contains all individuals assigned to the treatment group (i.e. to the first round of the family planning training sessions), whether or not they attended the sessions. "Control" contains all individuals assigned to the control group (i.e. to the second round of training sessions). Column 4 presents the test statistic for the null hypothesis that the mean in the treatment group is equal to the mean in the control group. Unless otherwise indicated, all are binary variables. MZN stands for Mozambican meticais. HIV stands for Human Immune-deficiency Virus. STI stands for Sexually Transmitted Infections. The HIV knowledge score is calculated as the sum of six knowledge statements on HIV. "HIV risk perception general' and "... for self" are responses to the questions "What is the risk of being infected with HIV when having unprotected sex for a women in general? And for you specifically?" measured on a 1-5 scale ranging from No risk to Very risky. "Ever used other" and "Current use other" refer to use of any other modern contraceptive method apart from condoms, e.g. the pill, injectables, or an IUD. *: p-value <0.10.

and desired fertility.[15] Second, in contrast, the women in our study appear to have greater bargaining power than the representative sample: they began to have sex at a later age, are more likely to have used a condom the last time they had sex, and report greater decision-making power. We may therefore not expect to see the upward-sloping portion of the inverse-U relationship between bargaining power and adoption, if our sample is more concentrated towards the right-hand end of the bargaining power distribution.

4.3 Diary data

At the end of the baseline survey, all respondents were also invited to participate in a weekly sexual diary exercise. Altogether 56 respondents volunteered to participate, comprising 27 who were subsequently randomised into the treatment group and 29 who were subsequently randomised into the control group.[16] The diaries recorded detailed information on all of the respondents' sexual encounters in the seven days prior to each interview. Diary interviews took place with carefully trained enumerators (again from an independent survey firm not associated with the programme) in a private place chosen by the respondent. The same enumerator interviewed a given respondent each week, to maximise trust and confidentiality. Diary interviews took place over a period of 17 weeks, beginning four weeks prior to the first group receiving its first session and ending one week after the last group received its last session. The baseline period for each respondent is taken to run from the start of the diary data collection until one week after the facilitator that the respondent was assigned to began her first meeting for her treatment-group participants (5.6 weeks on average). The endline period is taken to run from the end of the baseline period until the end of the diary data collection,

[15] One exception is that the women in our sample are much less likely to have a job, which makes sense if women with a lower opportunity cost of time are more willing to participate in a time-intensive programme.

[16] We did not stratify the randomisation on diary participation, but there is balance on treatment status, covariates and baseline contraceptive use within this diary subsample; see Online Appendix Tables B.4 and B.5. Online Appendix Tables B.6 and B.7 also show that the diary participants are representative of the balanced panel of all survey participants, except that the diary participants have been in a relationship for longer than the average study participant, no diary respondents are pregnant, and diary respondents are more likely to have ever used other contraceptives. The results from the diary subsample presented below are robust to re-weighting to make the diary subsample representative of the full sample (available on request).

Table 2: Comparison of Study Sample to DHS Representative Sample

	(1) **Study Mean**	(2) **DHS Mean**	(3) **t-test**	(4) **Study N**	(5) **DHS N**
Demographics					
Age in years	30.18	29.47	1.28	276	1007
Years of education	6.35	6.72	-1.82	273	1007
Literate	0.85	0.76	3.82	273	1007
Income					
Has job	0.37	0.58	-6.33	273	1007
Relationships					
Married (officially or unofficially)	0.63	0.61	0.58	275	871
Pregnant	0.05	0.07	-0.76	275	1007
Wants another child in future	0.57	0.57	0.14	260	961
Sexual Behaviour					
Age of sexual debut in years	16.62	16.16	3.26	273	955
Used condom during last time sex	0.54	0.31	6.32	243	871
Decision-making power					
Decision-making visiting family	0.62	0.39	6.43	272	580
Decision-making spending earnings	0.59	0.21	10.85	275	569
Decision-making her health	0.53	0.39	4.04	275	580

Notes: Column 1 displays the mean from our study sample at baseline (N=298). Column 2 shows the 2011 Demographic and Health Survey (DHS) mean for women in urban areas of Maputo Province (N=1007). Lower sample sizes in Columns 4 and 5 reflect observations that are missing or not applicable. Unless otherwise indicated, all are binary variables. Column 3 presents the test statistic for the null hypothesis that the mean in the study sample is equal to the mean in the DHS sample. Variables selected for comparison are those that appear in both our study and the DHS, with similar or identical wording. The three "Decision-making" variables are indicators for whether the respondent is involved in making decisions on the respective activities.

comprising 8.9 weeks on average. On average 75% of the diary sample participated each week.[17]

Although the diary data come from a small and potentially selected sample, they allow us to provide illustrative evidence on a number of important concerns. First, the fact that the diaries are administered very shortly after the sex acts of interest reduces recall bias (Das et al., 2012). The diaries are also a more complex and granular instrument than the baseline and endline surveys. Although the time period covered by the surveys was slightly different to that covered by the diary interviews, we are able to cross-check with the surveys to reduce concerns about misreporting in either instrument. There is limited evidence of under-reporting of contraceptive use in the surveys: 5 out of 56 diary participants report never having used a female condom during the endline survey but report using them in the diaries; whilst for male condoms the figure is 4 out of 56 respondents.[18] We therefore run the main impact analyses using both the survey data and the diary data; see Section 5.4 for details.

As well as constructing variables for contraceptive use at the respondent level, the diary data allow us to analyse the impact of the intervention at the level of the sex act. Altogether respondents report a total of 349 sex acts during the endline period: an average of 6.1 sex acts per respondent, with a minimum of zero and a maximum of 30. We see that a large proportion of sex acts involve discussions or disagreements over the use of condoms: 31% of sex acts in the last fourteen days in the control group B.33. This lends support to our bargaining model, and also to the idea that even if sorting on contraceptive preferences occurs in the dating or marriage market, a substantial gap in preferences still persists.

4.4 Bargaining power

To test the model's predictions, we require proxies of women's bargaining power within a relationship. By virtue of their definition, we elicit these measures only for women

[17]Individual respondents took part in the diaries an average of 13 times, with a minimum of three weeks and a maximum of 17 weeks. There are no significant differences in participation between the treatment and control group.

[18]We cannot make the opposite comparison, given that the endline survey took place two months after the end of the diaries: if a respondent reports using condoms in the survey but not the diaries, it may be that she adopted them during those two months.

with stable partners, although as mentioned earlier this comprises 85% of the sample. We first include a survey module about assets brought by the woman to the relationship. This has the advantage of proxying bargaining power in a way that is unconfounded by current contraceptive use. We next include a standard survey module on decision-making, and a novel survey module on power dynamics in the relationship, which we developed through extensive piloting in the local context. These modules have the advantage of proxying bargaining power more directly and more currently, although such elements of bargaining may be simultaneously determined along with contraceptive use. Table 3 provides summary statistics for each of the questions in each module.

Since each of these modules contains multiple questions that are highly correlated, we first perform a principal component analysis to construct indices. The full tables of component loadings can be found in Online Appendix Tables B.8 and B.9. The first three principal components of the assets module jointly explain 68.5% of the variance in the assets questions. Similarly, we run a principal component analysis including the questions from the decision-making and power dynamics modules. These two survey modules each load a different principal component, which together explain approximately 40% of the variance in the survey questions. Thus altogether we take five principal components as our preferred measures of bargaining power: three from the assets module, and one each from the decision-making and power dynamics modules. For ease of comparability, we scale the components so that the woman with least bargaining power on that measure has a score of zero, and normalise them such that a one point increase in each measure represents an increase of one standard deviation. Online Appendix Table B.10 shows that these principal components are balanced across treatment and control.

Reassuringly, these measures of bargaining power are correlated with baseline demographic characteristics in the way that one would expect: both the second and the third principal components of assets brought by the respondent to the relationship are strongly correlated with her education; the respondent's decision-making power is strongly positively correlated with her personal income in the last thirty days, age, whether she is the household head, and whether she has a job; whilst her decision-making power is strongly negatively correlated with whether the couple is married. One anomaly is that her decision-making power is negatively correlated with her education. To avoid the measures of bargaining power spuriously proxying the effects of any

Table 3: Bargaining power – summary statistics

	Mean	sd	Min	Max	Total
Did you bring the following assets to your relationship...					
...jewellery?	0.08	0.27	0.00	1.00	264
...animals?	0.00	0.06	0.00	1.00	264
...land?	0.01	0.11	0.00	1.00	264
...electronics?	0.02	0.14	0.00	1.00	264
...money?	0.07	0.25	0.00	1.00	264
...mobile phone?	0.07	0.25	0.00	1.00	264
...kitchen utensils?	0.10	0.30	0.00	1.00	263
Who decides about...					
...buying clothes for you?	0.80	0.40	0.00	1.00	297
...buying phone credit?	0.76	0.43	0.00	1.00	297
...education for the children?	0.49	0.50	0.00	1.00	288
...health expenses for you?	0.55	0.50	0.00	1.00	297
...health expenses for the children?	0.41	0.49	0.00	1.00	291
...if you are allowed to work?	0.59	0.49	0.00	1.00	296
...how earnings are used?	0.60	0.49	0.00	1.00	297
...visits to friends?	0.64	0.48	0.00	1.00	296
...visits to family?	0.64	0.48	0.00	1.00	294
Who usually has more say when you talk about serious things	0.47	0.50	0.00	1.00	250
In general, who do you think has more power in your relationship	0.39	0.49	0.00	1.00	249
Power dynamics					
Most of the time, we do what my partner wants to do	2.33	1.08	1.00	4.00	250
My partner won't let me wear certain things	2.61	1.11	1.00	4.00	250
When my partner and I are together, I'm pretty quiet	3.07	0.96	1.00	4.00	250
My partner has more say about important decisions that affect us	2.39	1.09	1.00	4.00	250
My partner tells me who I can spend time with	2.79	1.09	1.00	4.00	249
I feel trapped or stuck in our relationship	3.20	0.86	1.00	4.00	250
My partner does what he wants, even if I do not want him to	2.86	1.00	1.00	4.00	249
I am more committed to our relationship than my partner is	2.74	1.08	1.00	4.00	250
My partner is involved with other people apart from me	2.77	1.02	1.00	4.00	249
My partner always wants to know where I am	2.16	1.10	1.00	4.00	250
When my partner and I disagree, he gets his way most of the time	2.73	1.06	1.00	4.00	248

Notes: All values taken from the baseline survey (N=298). The assets module contains indicators for whether respondent took specific assets to her relationship. It was enumerated to all women who lived with their partner at baseline, including a few who did not claim to be in a *stable* relationship (N=264). The decision-making module was enumerated to all respondents (N=298), except the questions "who has more say" and "who has more power" which were asked only of women in a stable relationship at baseline (N=250). Decision-making variables are indicators for whether respondent was involved in making decisions on each of the activities or if respondent had more say/more power than her partner."Power dynamics" questions were only asked from women who were in a stable relationship at baseline (N=250), based on a Likert-scale coded from 1 (completely disagree) to 4 (completely agree), and recoded such that a greater value represents higher bargaining power for the respondent. Lower observation numbers in the final column reflect missing values or unwillingness to answer.

of these demographic characteristics, we therefore add these characteristics as controls when estimating the effects of bargaining power on condom adoption; see Section 5.3.

As predicted by the model, the measures of bargaining power are strongly negatively correlated with the respondent reporting that her partner refuses to use male condoms. Specifically, the correlation between "partner refuses male condoms" and a one standard deviation increase in the principal component of bargaining power in the following domains are: power dynamics -0.11 (p-value 0.09); assets score 1 -0.13 (p-value 0.03); assets score 2 -0.10 (p-value 0.002); assets score 3 -0.18 (p-value <0.01). We also observe a positive correlation between assets brought by the woman to the relationship and baseline use of male condoms in the last thirty days: a 7.3 percentage point increase in the likelihood of use per one standard deviation in assets score 1 (p-value 0.035).

5 Results

5.1 Impacts on condom use

Our preferred estimations are derived from an analysis of covariance (ANCOVA) linear probability models of the following form:[19]

$$Pr\left[Y_{if1} = 1 | Y_{if0}, treat_{if}, \eta_f\right] = \alpha + \delta Y_{if0} + \beta treat_{if} + \eta_f, \tag{3}$$

where Y_{if1} is the outcome variable of interest at endline, and Y_{if0} is its value at baseline. $treat_{if}$ is a dummy for being assigned to the treatment group, i.e. to receiving the programme in the first rather than the second phase. β represents the intent-to-treat effect, since not all individuals assigned to treatment attended the programme: the participation rate was around 65% for each individual session, with 20 women (17.7% of the treatment group) not attending any of the six sessions. η_f is a facilitator fixed effect, which is included for inference since randomisation was blocked on the seventeen facilitators (Bruhn and McKenzie, 2009). Standard errors are robust to individual-level heteroskedasticity, as this was the level of randomisation (Abadie et al., 2017).

[19] Results are robust to using OLS specifications without the lagged dependent variable (see Online Appendix Tables B.11 to B.16) and to using logit, firthlogit or relogit specifications (see Online Appendix Tables B.41 to B.45).

Given concerns about the use of asymptotic results about standard errors in smaller samples, we also report additional p-values for the treatment coefficients as calculated from randomisation inference tests (Young, 2016).

Figure 2 displays the treatment effects on condom use as estimated from Equation 3, while the full estimations are reported in Table A.1 in the Appendix. The programme has a substantial and highly significant effect on the use of female condoms: we observe an 18.4 percentage point increase in the proportion of women who have ever used female condoms (equivalent to 209% of the endline mean in the control group); a 4.7 percentage point (470%) increase in the proportion who have used a female condom in the last thirty days; and a 7.7 percentage point (385%) increase in the proportion who are currently using female condoms. The fact that the treatment effect on ever use is higher than the treatment effect on use in the last thirty days and current use suggests that many women in the treatment group try female condoms at the start of the intervention, then a smaller although sizeable fraction continue to use them. This is a natural adoption pattern if couples experiment with female condoms and thereby learn more about their costs and benefits, then some return to their original contraceptive method while others adopt female condoms more permanently.

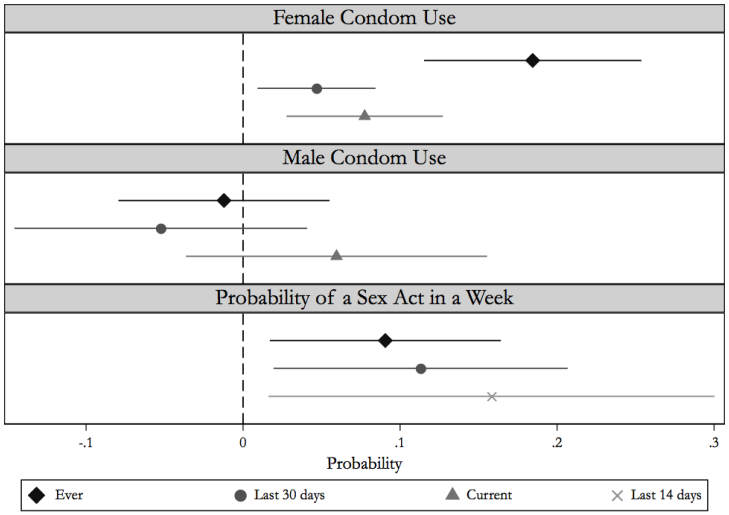
We see no evidence of anticipation effects or spillovers — through the control group obtaining female condoms from the treatment group — as there are no significant differences between baseline use in the control group and endline use in the control group for any of our outcome indicators.[20] This is unsurprising, since female condoms are difficult to obtain in the study area through channels other than our intervention. Indeed, the number of free female condoms that a respondent in the treatment group took from the sessions is highly correlated with her report of ever use (correlation 0.38, p-value < 0.01), use last 30 days (0.21, p-value 0.02), and current use (0.29, p-value <0.01). This also weighs against concerns that reported use of female condoms might represent response bias.

We do not observe any significant evidence that respondents substitute away from or increase their use of male condoms.[21] Table A.2 in the Appendix shows that when

[20] A t-test comparing baseline use and endline use in the control group for "ever use", "last 30 day use" and "current use" of female condoms, male condoms, and the use of other contraceptives does not show any significant differences.

[21] We have 80% power to detect the following minimum detectable effect sizes at the 5% level in a two-tailed test: ever use – female condoms 7.6 pp, male condoms 9.6 pp, other 10.3 pp; use

Figure 2: Treatment Effects on Condom Use and Sex Acts



Notes: Predicted average marginal effect of treatment on outcome variables. Each marker (diamond, circle, triangle, x) represents the average marginal effect. Each bar represents the 90% confidence interval. Treatment is an indicator for being assigned to the treatment group (i.e. to the first round of the family planning training sessions) as opposed to the control group (i.e. the second round of training sessions). Not all respondents assigned to treatment attended the sessions, thus the treatment coefficients represent the intent-to-treat effect. The panels titled "Female Condom Use" and "Male Condom Use" present regressions on the balanced survey sample, N=227. Dependent variables are binary indicators. The bars with a "diamond" marker refer to whether the respondent has ever used the method, the bars with a "circle" marker refer to whether she has used it in the last 30 days, and the bars with the "triangle" marker whether she is currently using it. Regressions in these panels are linear probability model ANCOVA specifications, including the baseline value of the dependent variable as a regressor. All regressions include facilitator indicators (N=17) since randomisation was stratified on facilitator. The panel titled "Probability of a Sex Act in a Week" presents regressions on the balanced diary sample, N=56. Dependent variables are binary indicators for whether a respondent had at least one sex act in a particular week. In this panel, the bars with a "diamond" marker refer to whether the respondent had at least one sex act in a week, the bars with a "circle" marker refer to whether she had at least one sex act in the last 30 days, and the bars with the "x" marker whether she had at least one sex act in the last 14 days. All regressions in this panel are linear probability individual fixed effects models with the respondent-week as the unit of observation (N=863 for the full endline period, N=536 for the last 30 days, and N=367 for the last 14 days). All regressions include facilitator × endline fixed effects (N=17) since randomisation was stratified on facilitator. Standard errors in all regressions are robust to individual-level heteroskedasticity, since this was the level of randomisation.

we split the sample by women who are using or not using male condoms at baseline, both groups experiment with male condoms (Columns 1 and 2), but it is those women not using male condoms at baseline who appear to drive the more sustained adoption (Columns 3 and 5); although again we note concerns about power.[22] This is in line with the idea that female condoms are "close" to male condoms in terms of pleasure and health, and so few couples may find it worthwhile to switch. Importantly, the lack of substitution suggests that the intervention decreases the number of women having sex unprotected from HIV/AIDS and other STIs. We do not observe an impact on self-reported HIV or STI status (see Online Appendix Table B.15); although we are reluctant to place too much emphasis on these measures. Firstly, because our endline is only a few months after the end of the intervention and the accuracy of testing is relatively low shortly after infection. Secondly, only 28% of our respondents indicate they were tested after our intervention. Finally, since testing is voluntary this subset of respondents is most likely to be highly selective. Section 6 describes use of epidemiological modelling to estimate how our observed impacts on condom coverage translate into impacts on longer-term rates of HIV transmission.

Table A.1 shows that we also see no increase in or substitution away from other contraceptive methods such as the pill and injectables. This suggests that women who adopt female condoms were either previously using no contraceptives, or use female condoms in addition to other methods in order to protect against HIV/AIDS and other STIs. Indeed, of the women who are currently using female condoms at endline, 42% are also using other contraceptive methods (mainly the pill or injectables).

We would expect women who are not in a stable relationship to place a larger weight on the health offered by STI protection technologies, and so to have a higher demand for condoms. Indeed, Online Appendix Table B.16 shows that the treatment effect on ever use of female condoms is stronger for women who are not in a stable relationship at baseline. Nonetheless, when we restrict the sample to just those women in a stable relationship, we still observe positive treatment effects on female condom use: a 16.4 percentage point increase in ever use of female condoms (p-value < 0.01), a 5.6 per-

last 30 days – female condoms 3.5 pp, male condoms 14.0 pp; current use – female condoms 4.9 pp, male condoms 13.9 pp, other 13.6 pp.

[22] We do not observe any significant impacts on male condom use when we split the sample by those using and not using male condoms at baseline (see Online Appendix Table B.14).

centage point increase in use in the last 30 days (p-value 0.042), and a 7.9 percentage point increase in current use (p-value 0.019). This may be rational if one partner is HIV-positive while the other is HIV-negative, or if one or both partners have relations with others or suspect that their partner does. Even individuals who are already HIV-positive have an incentive to avoid further infection, as getting infected with a different strain of HIV may increase the viral load, and getting infected with other STIs may lead to further complications and increase the risk of transmitting HIV/AIDS.

5.2 Extensive-margin impacts

We use the diaries to examine the effects on the extensive margin, i.e. the probability of having sex. Our preferred measure of this is the likelihood of at least one sex act per respondent per week, so that results are not unduly influenced by a very small number of respondents who report a large number of sex acts. Taking advantage of the weekly nature of the diaries, we estimate the following fixed effects panel specification:

$$\begin{aligned} &Pr\left[Y_{ift}=1|treat_{if},\eta_f,\phi_{if}\right] \\ &=\alpha+\delta\times endline_t+\beta treat_{if}\times endline_t+\eta_f\times endline_t+\phi_{if}, \quad t=1,2,..T \end{aligned} \tag{4}$$

where Y_{ift} is the outcome variable of interest for individual i assigned to facilitator f in week t. The unit of observation is thus the respondent-week. Standard errors are again clustered at the individual level.

Figure 2 shows that, in line with Proposition 2 of the model, the introduction of female condoms leads to a significant increase in the likelihood of sex acts. The full estimations of Equation 4 are presented in Table A.3 in the Appendix. In the full endline period, respondents in the treatment group were on average 9.1 percentage points (pp) more likely to report a sex act in a given week, compared to a control group mean of 46.9%. In the last 30 and 14 days, the treatment effect on the likelihood of sex acts per week was 11.3 pp and 15.8 pp respectively, compared to 47.1% and 49.1% in the control group. The fact that we observe this increase in the treatment group indicates that there are couples in which one or both partners' participation constraints are sometimes or always binding when the only options are male condoms or unprotected sex, but where

both find sex with female condoms preferable to not having sex. The introduction of female condoms therefore increases utility for such couples. Moreover, Online Appendix Table B.32 shows that the increase in sex acts is driven by those respondents who are not using male condoms when they do have sex at baseline. Again, we do not see evidence of spillovers or anticipation effects in the control group, for example that control-group respondents withheld from regular sex in anticipation of treatment: the mean of sex acts per week in the control group is 0.91 (standard deviation 0.51) during the baseline phase and 0.86 (s.d. 0.54) during the endline phase, and a t-test that these are different is rejected (t=0.71).

Table B.33 in the Online Appendix also shows that we observe a large and highly significant reduction in the proportion of sex acts in which a discussion or disagreement about condoms takes place. This supports the idea that the expansion from a binary to a ternary choice allows the couple to choose an STI protection technology that is closer to their preferred choice on the technological frontier. Reassuringly, in the survey data we see no negative impact of treatment on measures of women's self-reported well-being, nor do we see any impacts on emotional or physical violence (see Online Appendix Table B.15). This contrasts Ashraf et al. (2014b)'s finding of a negative effect of concealable contraceptives on women's self-reported well-being. The difference is likely attributable to the fact that our treatment expands the set of observable contraceptives available in a cooperative setting; rather than introducing a concealable contraceptive in a non-cooperative setting, which women may choose to use but with some psychic cost of hiding.

5.3 Heterogeneity by bargaining power

We now test our main predictions about which women, among those in stable relationships, adopt female condoms in terms of their bargaining power. Based on the predictions of our model (see Proposition 1 in Section 3) and the fact that the women who have selected into our sample are relatively empowered (see Table 2) we would expect the relationship between bargaining power and female condom adoption in our sample to be negative.[23]

[23]We have power to detect reasonably modest increases in female condom use among the least empowered women in our sample — corresponding to women with intermediate levels of

Figure 3 displays the predicted marginal treatment effects on endline current use of female condoms (our preferred proxy of sustained adoption) across the distribution of each of our baseline principal component measures of bargaining power. Table A.4 in the Appendix reports the full estimations of the heterogeneity of the treatment effect by each measure of bargaining power.[24] We control for baseline female condom use and for the demographic factors which are significantly correlated with baseline bargaining power: age, education, income in the last thirty days, and whether the respondent is the household head. The results are striking: for almost every measure of bargaining power, we observe a negative interaction between baseline bargaining power and treatment, showing that women with the lowest bargaining power in our sample are the most likely to adopt female condoms as a result of the intervention. As a robustness check, we also show that a post-double LASSO specification in which we include all survey variables and their interaction with treatment selects four out of the five bargaining power measures and their interaction with treatment, as well as age, education and their interactions with treatment (see Online Appendix Table B.17).[25] [26]

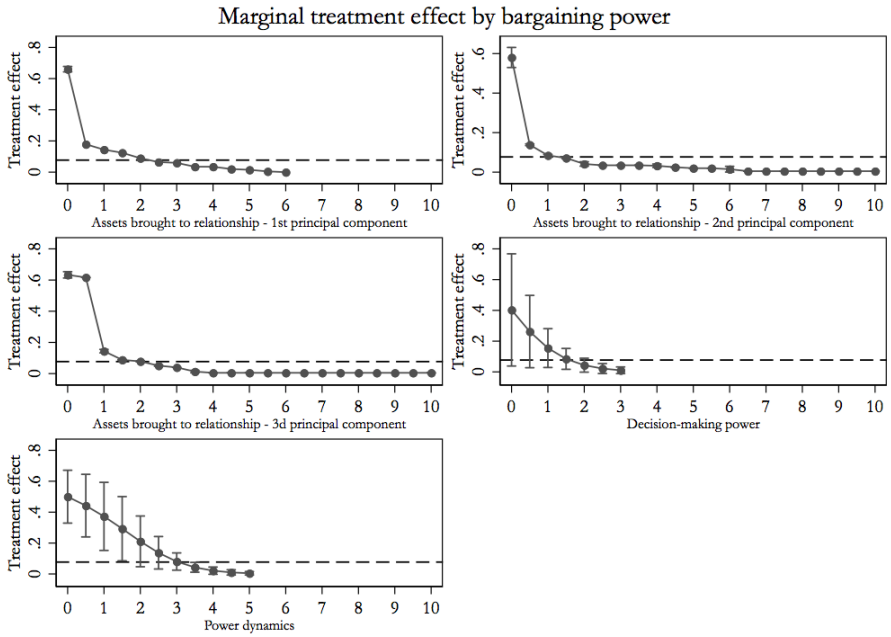
power in the population as per Proposition 1, as explained above — as well as among the most empowered women. If we split the sample along each measure of bargaining power, we have 80% power to detect the following minimum detectable effect sizes at the 5% level in a two-tailed test when the dependent variable is current use of female condoms and the same controls are included as in Table A.4: assets 1 – below or equal to median 7.2pp, above median 0.0pp; assets 2 – below or equal to median 7.0pp, above median 0.0pp; assets 3 – below or equal to median 6.8pp, above median 0.0pp; decision-making – bottom quartile 13.3pp, top three quartiles 4.7pp; power dynamics – bottom quartile 0pp, top three quartiles 6.7pp. Since each of the assets indices is heavily right-skewed, a median split corresponds to a split by those who have a value of zero or greater than zero. Decision-making and power dynamics are much less skewed; hence we report power calculations for the bottom quartile in order to isolate the women with the lowest bargaining power in our sample.

[24]The seventeen facilitator indicators are no longer included in Table A.4, due to low number of observations per facilitator to examine heterogeneity along the distribution of bargaining power.

[25]We also check robustness of the results to an alternative measure of bargaining power: whether the woman's or her partner's family was wealthier at the time of their marriage. This variable is strongly and significantly correlated with our three asset measures of bargaining power. However, we do not see significant heterogeneity of the treatment effect on this measure; perhaps because it is a less direct proxy of bargaining power, or perhaps because it is categorical, whereas our bargaining power measures are continuous.

[26]Online Appendix Table B.18 shows no systematic treatment impacts on the time-variant measures of bargaining power themselves, namely decision-making and power dynamics.

Figure 3: Impacts on Female Condom Use by Female Bargaining Power



Notes: The solid lines represent the predicted marginal treatment effect on current use of female condoms by bargaining power. Each marker (circle) represents the marginal treatment effect, each bar represents the 95% confidence interval. The horizontal dashed lines represent the predicted average treatment effect. Estimations are maximum likelihood "rare events logit (relogit)" estimations on the balanced sample of respondents (N=227) with stable relationship at baseline, N=206; N=201 of them have non-missing values on all assets and control variables; N=182 of them have non-missing values all decision-making, power dynamics, and control variables. Relogit estimates are presented rather than linear probability (LPM) estimates, since while LPM is appropriate for estimating the average marginal effect, it is not appropriate for estimating the full range of marginal effects (see Online Appendix B.6 for full discussion). Dependent variables are binary indicators for current use of female condoms. "Treatment" is a dummy for being assigned to the treatment group (i.e. to the first round of the family planning training sessions) as opposed to the control group (i.e. the second round of training sessions). Not all respondents assigned to treatment attended the sessions, thus the coefficient on "Treatment" is the intent-to-treat effect. The first, second, and third principle component of "Assets brought to relationship" are the first three principal components from the assets module, as described in Table 3 and identified in Table B.8. "Decision-making" and "Power dynamics" are the first two principal components from all the survey questions referring to these two modules, as identified in Table B.9. For comparability the components are scaled so that the woman with least bargaining power on that measure has a score of zero. The components are normalized such that a one point increase in each measure represents an increase of one standard deviation. All regressions are (relogit) ANCOVA specifications, including the baseline value of the dependent variable as a regressor. Regressions do not include facilitator fixed effects due to loss of sample size where baseline use perfectly predicts endline use conditional on a given facilitator. Standard errors are robust to individual-level heteroskedasticity, since this was the level of randomisation. Controls are: respondent's age, education, and income in the last 30 days; whether the respondent has a job, is married or in a stable relationship, and whether the respondent is the household head.

In terms of margins of adoption, as predicted by our model we observe a strong negative correlation between baseline bargaining power and female condom adoption *conditional on using male condoms at baseline* (see Online Appendix Table B.19). On the other hand, as discussed earlier, we do not see evidence of a large degree of substitution away from male condoms. A possible explanation is that women with higher bargaining power who take up female condoms also intersperse their use with the use of male condoms. Indeed, 81% of women who are currently using female condoms at endline also report currently using male condoms. This "double protection" is a typical pattern of adoption observed in the medical literature, and is found to be associated with a large increase in the number of protected sex acts (Vijayakumar et al., 2006).

5.4 Alternative explanations and robustness

Experimenter demand: A possible alternative explanation for the negative interaction terms could be if women with lower bargaining power are more susceptible to experimenter demand, and so over-report use of female condoms whilst more empowered women do not. As described above, our potential concerns over misreporting are allayed by observing high consistency in reported use and results across the survey and diary data, and a strong correlation between reported condom use and the number of condoms an individual took from the sessions. We also use the diary data to estimate the treatment effects on the proportion of sex acts protected by condoms, and observe a pattern of results that is similar to the treatment effects on the proportion of individuals using condoms in the survey data (see Online Appendix Table B.28). Thus whilst we acknowledge the limitations of our self-reported measures, we doubt that misreporting of female condom use and a systematic correlation of this misreporting with bargaining power is driving the observed heterogeneity.

Hiding: Another possibility is that women who are already able to convince their partners to use male condoms might "hide" their participation in the intervention, or the fact that the intervention makes female condoms available. We do not see evidence of this in the endline data. Conditional on attending at least one session, only 3 respondents (2.4%) report that they did not discuss what they learned in the intervention with their partner, and only 2 women (1.6%) report that their partner did not support

their participation. Most importantly, neither whether a woman discussed the intervention with her partner nor whether he supported her participation are correlated with whether the couple was using male condoms at baseline.

Traditional values: Another alternative story is one in which our measures of bargaining power in fact proxy a woman or a couple having traditional or conservative values, which may directly influence their willingness to adopt condoms. This story would struggle to explain our results: if low bargaining power in fact proxies traditional values, then we would not expect that the women with traditional values are the ones willing to adopt female condoms (which are likely perceived as even less traditional or conservative than male condoms, and certainly less traditional than unprotected sex). Nonetheless, we test for heterogeneous treatment effects by a question in which respondents were asked "Ideally, how many hours a day would you like to spend working?". This is our best available proxy of traditional values, since in this setting conservative norms dissuade partnered women from doing paid work outside the home, and from working long hours away from their home or children. This measure of traditionalism is positively correlated with our measures of bargaining power; but we do not see heterogeneity of the treatment effect on this measure, nor does including it and its interaction with treatment change the sign and significance of the interactions between treatment and bargaining power (see Online Appendix Table B.24).[27]

Access: Another possible alternative explanation could be if women with lower bargaining power are less able than women with higher bargaining power to access male condoms (or other contraceptives) through the market or at health clinics. However, if this was the case then we would expect to see a similar pattern of heterogeneity in current use of male condoms, which the health workers also carry. Yet this is not the case: women with lower bargaining power are not consistently more likely than women with higher bargaining power to take up male condoms (see Online Appendix Table B.20). Furthermore, we also re-run estimations of the bargaining power heterogeneity controlling for walking distance to the nearest health centre — a proxy of pre-treatment access

[27] In a similar vein, adding age of marriage (as an alternative proxy for traditional values) and its interaction with treatment again does not change the sign and significance of the interactions between treatment and bargaining power.

to male condoms — and its interaction with treatment. These terms are not significant for female or male condom adoption, and do not significantly change the coefficient on the interaction of bargaining power and treatment.

Use of other contraceptive methods: The interaction between bargaining power and treatment is also not proxying a differential effect of treatment depending on whether the respondent is using other methods of contraception (i.e. the pill or injectables) at baseline. Our measures of bargaining power are positively correlated with current use of the pill at baseline, and negatively correlated with use of injectables at baseline, consistent with the idea that less empowered women use injectables instead of the pill, as injectables are more concealable Ashraf et al. (2014b). However, when baseline use of other forms of contraception and its interaction with treatment is included into the regressions, the interactions between treatment and bargaining power remain negative and highly significant (see Online Appendix Table B.21).

HIV status: Heterogeneity by bargaining power is also not proxying the observed heterogeneity by HIV status. This could have been the case since we observe that women with lower bargaining power are more likely to be HIV-positive. However, the interaction of the bargaining power measures with treatment remain negative and significant when controlling for HIV status and its interaction with treatment (see Online Appendix Table B.22).

Risk beliefs: Finally, heterogeneity by bargaining power is not proxying heterogeneity by women's beliefs about the risk of contracting HIV. This could have been the case if women with lower bargaining power revised their risk perceptions upwards more strongly as a result of the intervention. However, again the interaction of the bargaining power measures with treatment remain negative and significant when controlling for baseline risk beliefs and their interaction with treatment (see Online Appendix Table B.23). We also consider whether the respondent believes her partner is involved with other women. This variable is negatively and significantly correlated with three of our five bargaining power measures at baseline; but again, including it and its interaction with treatment does not remove the negative interaction between treatment and bargaining power (see Online Appendix Table B.25.)

Attrition: As mentioned earlier, attrition is higher in the control group. To account for this, Online Appendix Table B.26 shows the main estimation results with observations re-weighted by the inverse of the predicted probability of an individual appearing in the endline sample, based on her treatment status and her baseline covariates. The results are robust to this re-weighting. Online Appendix Table B.27 also presents Lee Bounds for the main impact estimations (Lee, 2009). Note that we do not condition on facilitator fixed effects, since whether attrition is higher in the treatment or control group varies by facilitator, and thus the monotonicity assumption required for conditioning fails. This means that the bounds presented are particularly conservative. Whilst the lower bounds cannot rule out a treatment effect of zero for the various measures of female condom use, we are able to rule out any sizeable negative effects, and the upper bounds are large and highly significant. We also note that the lower bounds are likely to be overly pessimistic in the context of our study, since it was almost impossible for those who attritted from the control group to obtain access to female condoms.

6 Cost-Benefit and Cost-Effectiveness Analysis

To understand how our results might combine to impact welfare and policy, it is important to weigh the increase in condom coverage — and associated reduction in negative externalities from HIV transmission — against the decrease in average condom effectiveness compared to pure use of male condoms, and the observed increase in the number of sex acts. As an illustrative exercise, we conduct a cost-benefit analysis of two possible scale-ups to the entire female population of South Mozambique: a scale-up of our full intervention; and a scale-up of just the free distribution of female condoms, with the assumption that information about female condoms can be provided with zero marginal cost via existing sex education programmes. The purpose of this exercise is to highlight the potential magnitudes of the trade-offs involved in introducing a second-best technology, and the quantitative importance of the behavioural response. The purpose is not to provide an accurate cost-benefit estimation, given the inherent uncertainty in extrapolating from our observed treatment effects to what treatment effects would be in the whole population, over a longer time horizon, and from a different version of the intervention in the case of provision via existing sex education programmes.

Online Appendix Section B.5 details the methodology of our cost-benefit analysis in full. We adjust the epidemiological model used by UNAIDS in order to estimate the number of HIV infections and disability-adjusted life years (DALYs) that free access to female condoms would help avert, based on our observed treatment effects. We also factor in productivity gains from a reduction in the burden of HIV, as is standard in the literature. On the cost side, we consider programme costs of introducing female condoms, but also cost savings from reduced provision of anti-retroviral therapies and prevention of mother-to-child transmission treatments.

The results show that accounting for the behavioural response, i.e. the observed increase in the number of sex acts, is crucial. Before accounting for this, both our full programme and adding female condoms to existing sex education programmes actually imply a cost saving. Intuitively, this is because low female bargaining power implies that the main margin of female condom adoption is from women previously having unprotected sex, rather than substitution away from male condoms. However, once we incorporate the behavioural response, only adding female condoms to existing sex education programmes has the potential to be cost-effective in our illustrative simulations.

7 Conclusion

Our results suggest that low female bargaining power indeed acts as a key constraint to adoption of male condoms in contexts such as Mozambique. If female condoms are introduced with adequate information and support, they are taken up by women with lower bargaining power who are otherwise having unprotected sex. In terms of policy, this means that the correct cost comparison for free provision of female condoms is not to the free provision of male condoms, but rather to the costs of anti retroviral therapies and other costs associated with unprotected sex. However, evidence from a similar intervention with a representative sample of the population and a longer time horizon after adoption are crucial to aid more comprehensive cost-benefit calculations and funding decisions.

More broadly, we have highlighted how low female bargaining power may constrain adoption of potentially welfare-improving household technologies, in cases where women have a stronger preference for adoption or face higher costs of non-adoption compared to

men. There are many other examples of technologies where women may have a stronger willingness than men to adopt. For instance, women may have a higher demand for insurance, given evidence that they are more risk-averse. In such cases, information and social norm campaigns targeted specifically at men may be the first-best approach to increasing investments and adoption. Otherwise, providing inferior versions of the technology that are more acceptable to men, or bundling technologies with goods for which men have strong demand, may offer a second-best solution. These remain important topics for future research.

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A Appendix

Table A.1: Treatment effects – primary outcome variables

	(1) **Ever use female condoms**	(2) **Ever use male condoms**	(3) **Ever use other**	(4) **Use last 30 days female condoms**	(5) **Use last 30 days male condoms**	(6) **Current use female condoms**	(7) **Current use male condoms**	(8) **Current use other**
Treatment	0.184***	-0.012	0.020	0.047**	-0.052	0.077**	0.060	0.030
(s.e.)	(0.042)	(0.041)	(0.042)	(0.023)	(0.057)	(0.030)	(0.058)	(0.053)
[r.i. p-value]	[0.000]	[0.777]	[0.649]	[0.080]	[0.359]	[0.025]	[0.348]	[0.583]
Observations	227	227	227	227	227	227	227	227
Control mean endline	0.088	0.824	0.735	0.010	0.363	0.020	0.353	0.412

Notes: Regressions on the balanced sample, N=227. Dependent variables are binary indicators for the use of female condoms (FC), male condoms (MC) and other modern contraceptive methods (other), such as the pill, injectables or IUD. Columns 1-3 refer to whether the respondent has ever used the method, columns 4 and 5 to whether she has used it in the last 30 days (this was only asked for condoms, not for other contraceptive methods), and columns 6-8 whether she is currently using it. "Treatment" is an indicator for being assigned to the treatment group (i.e. to the first round of the family planning training sessions) as opposed to the control group (i.e. the second round of training sessions). Not all respondents assigned to treatment attended the sessions, thus the coefficient on "Treatment" is the intent-to-treat effect. All regressions are linear probability model ANCOVA specifications, including the baseline value of the dependent variable as a regressor. All regressions include facilitator indicators (N=17) since randomisation was stratified on facilitator. Standard errors (s.e.) are robust to individual-level heteroskedasticity, since this was the level of randomisation. Randomisation inference p-values [r.i. p-value] are estimated from Monte Carlo simulations re-assigning treatment within facilitator strata, with 1000 repetitions. Significance levels $p < 0.10^{*}$, $p < 0.05^{**}$, $p < 0.01^{***}$.

Table A.2: Treatment effects on female condom use, by baseline male condom use

	(1) **Ever use female condom** **No use male condom at baseline**	(2) **Ever use female condom** **Current use male condom at baseline**	(3) **Last 30 days female condom** **No use male condom at baseline**	(4) **Last 30 days female condom** **Current use male condom at baseline**	(5) **Current use female condom** **No use male condom at baseline**	(6) **Current use female condom** **Current use male condom at baseline**
Treatment	0.169***	0.232***	0.073**	0.030	0.085***	0.049
(s.e.)	(0.047)	(0.074)	(0.030)	(0.034)	(0.031)	(0.057)
[r.i. p-value]	[0.004]	[0.006]	[0.023]	[0.532]	[0.035]	[0.490]
Observations	141	86	141	86	141	86
Control mean endline	0.092	0.081	0.000	0.027	0.000	0.054

Notes: Regressions on the balanced sample, N=227. Dependent variables are binary indicators for the use of female condoms: ever used in columns 1-2, used in last 30 days in columns 3-4, and currently using in columns 5-6. Odd-numbered columns present results for the subsample of individuals who were not currently using male condoms (No use) at baseline; even-numbered columns present results for the subsample of individuals who were currently using male condoms (Current use) at baseline. "Treatment" is an indicator for being assigned to the treatment group (i.e. to the first round of the family planning training sessions) as opposed to the control group (i.e. the second round of training sessions). Not all respondents assigned to treatment attended the sessions, thus the coefficient on "Treatment" is the intent-to-treat effect. All regressions are linear probability model ANCOVA specifications, including the baseline value of the dependent variable as a regressor. All regressions include facilitator indicators (N=17) since randomisation was stratified on facilitator. Standard errors are robust to individual-level heteroskedasticity, since this was the level of randomisation. Randomisation inference p-values [r.i. p-value] are estimated from Monte Carlo simulations re-assigning treatment within facilitator strata, with 1000 repetitions. Significance levels $p < 0.10^{*}$, $p < 0.05^{**}$, $p < 0.01^{***}$.

Table A.3: Impacts on likelihood of sex acts per respondent week – diary subsample

	(1) **Sex act per week full endline period**	(2) **Sex act per week last 30 days**	(3) **Sex act per week last 14 days**
Treat × endline	0.091**	0.113**	0.158*
	(0.045)	(0.057)	(0.086)
Treat × Facilitator f.e.'s	✓	✓	✓
Observations	863	536	367
Control mean	0.469	0.471	0.491

Notes: Regressions on the balanced diary sample, N=56. Dependent variables are binary indicators for whether a respondent had at least one sex act in a particular week. Column 1 refers to whether the respondent had at least one sex act per week in the full endline period, Column 2 whether she had at least one sex act in the last 30 days, and Column 3 whether she had at least one sex act in the last 14 days. All regressions are linear probability individual fixed effects models with the respondent-week as the unit of observation (N=863 for the full endline period, N=536 for the last 30 days, and N=367 for the last 14 days). "Treat × endline" is an indicator for observations in the treatment group (i.e. to the first round of the family planning training sessions) during the relevant endline period ("full endline", "last 30 days", or "last 14 days") as opposed to the control group (i.e. the second round of training sessions). Not all respondents assigned to treatment attended the sessions, thus the coefficient on "Treat × endline" is the intent-to-treat effect. All regressions include facilitator × endline fixed effects (N=17) since randomisation was stratified on facilitator. Standard errors (in parentheses) are robust to individual-level heteroskedasticity, since this was the level of randomisation. Significance levels $p < 0.10^{*}$, $p < 0.05^{**}$, $p < 0.01^{***}$

Table A.4: Impacts on current use of female condoms – heterogeneity by bargaining power

	(1) Current use female condoms	(2) Current use female condoms	(3) Current use female condoms	(4) Current use female condoms	(5) Current use female condoms
Treatment	0.129***	0.112***	0.160***	0.224**	0.200*
	(0.045)	(0.039)	(0.051)	(0.093)	(0.114)
Assets score 1	-0.001				
	(0.010)				
Treatment*Assets score 1	-0.045**				
	(0.017)				
Assets score 2		0.005			
		(0.010)			
Treatment*Assets score 2		-0.032**			
		(0.015)			
Assets score 3			-0.000		
			(0.005)		
Treatment*Assets score 3			-0.055***		
			(0.018)		
Decision-making				-0.016	
				(0.022)	
Treatment *Decision-making				-0.079**	
				(0.038)	
Power dynamics					0.015
					(0.018)
Treatment*Power dynamics					-0.039
					(0.039)
Controls	✓	✓	✓	✓	✓
Observations	201	201	201	182	182
Control mean endline	0.020	0.020	0.020	0.020	0.020

Notes: Based on balanced sample of respondents (N=227) with stable relationship at baseline, N=206. N=201 of them have non-missing values on all assets and control variables. N=182 of them have non-missing values on all decision-making, power dynamics, and control variables. Dependent variables are binary indicators for current use of female condoms (FC). "Treatment" is a dummy for being assigned to the treatment group (i.e. to the first round of the family planning training sessions) as opposed to the control group (i.e. the second round of training sessions). Not all respondents assigned to treatment attended the sessions, thus the coefficient on "Treatment" is the intent-to-treat effect. "Assets 1", "Assets 2" and "Assets 3" are the first three principal components from the assets module, as described in Table 3 and identified in Table B.8. "Decision-making" and "Power dynamics" are the first two principal components from all the survey questions referring to these two modules, as described in Table 3 and identified in Table B.9. For comparability the components are scaled so that the woman with least bargaining power on that measure has a score of zero. The components are normalized such that a one point increase in each measure represents an increase of one standard deviation. All regressions are linear probability model ANCOVA specifications, including the baseline value of the dependent variable as a regressor. Regressions do not include facilitator indicators due to loss of sample size where baseline use perfectly predicts endline use conditional on a given facilitator. Standard errors are robust to individual-level heteroskedasticity, since this was the level of randomisation. Controls are: respondent's age, education, and income in the last 30 days; whether the respondent has a job, is married or in a stable relationship, and whether the respondent is the household head. Significance levels $p < 0.10^{*}$, $p < 0.05^{**}$, $p < 0.01^{***}$.