Gender, Intrahousehold Decisionmaking, and the Demand for Children *

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Abstract

We present evidence from a field experiment in Lusaka, Zambia that male involvement in the decision to seek out family planning services leads to substantial reductions in utilization. This phenomenon appears to be driven by average differences by gender in the demand for children rather than by a general distrust of or lack of information about family planning technologies among men. Study participants were offered a voucher that granted access to an appointment with a family planning nurse without a wait in line. Demand for family planning services is high, as evidenced by the 41 percent overall rate at which these vouchers were redeemed. Women were randomly assigned to receive the voucher either by themselves in private, or together with their husbands. Takeup among women assigned to receive the vouchers with their husbands was 9 percentage points (18 percent) lower than among women randomly assigned to receive the vouchers alone. We find evidence that this reduction in takeup was larger if husbands wanted more children than their wives, and stronger evidence that this reduction was larger among young couples than among older couples with completed fertility. There is no evidence that assignment to couples treatment reduces voucher use for women whose husbands want no more children, and evidence for a 12 percentage point reduction in use in the subsample of women whose husbands do want more children. Taken together, these results suggest that the unitary and collective bargaining models do not sufficiently richly describe the bargaining process over fertility within the household. Furthermore, policies or technologies that shift relative control of contraceptive methods from men to women may significantly increase contraceptive use and reduce average fertility in some contexts. (*JEL codes:* J12, J13, O12. *Keywords:* Fertility, intrahousehold bargaining, field experiments.)

1 Introduction

To the extent that rapid population growth leads to low levels of human capital investment and continued poverty for future generations, the ability to control fertility has broad social and economic consequences. Recent evidence from the United States and Colombia suggests that access to contraceptives may improve economic outcomes and reduce poverty by allowing women to optimally time births, increasing women's investments in education and participation in the labor market at childbearing ages (Goldin and Katz, 2002; Bailey, 2006; Miller, 2005). Finally, it also has direct consequences for individual well-being: significant reported unmet need for contraceptives and excess fertility suggest that fertility outcomes outstrip fertility desires in many parts of the developing world.

Unmet need and excess fertility, typically defined by outcomes or behaviors relative to women's reported desires, are often attributed to an insufficient supply of appropriate contraceptive methods, a lack of information or misinformation about those methods, or restrictive social norms governing fertility control. Others make the argument that, since the cost of preventing births using any method must be small even without complete access to modern contraception, these must reflect systematic mismeasurement of fertility desires in survey data (Becker, 1991).

An alternative hypothesis is that unmet need and excess fertility reflect the outcome of bargaining between partners with divergent fertility preferences or demands for fertility control. In particular, data from surveys such as the Demographic and Health Surveys (DHS) indicate that in many countries, men tend to report larger ideal family sizes and less demand for contraception than their wives (Becker, 1999). Excess fertility and unmet need, as conventionally defined, could thus reflect the outcome of a bargaining process between members of couples with different fertility preferences. If household bargaining is important, increasing access to contraceptive technologies that are relatively easy for a woman to hide from her husband may lead to reductions in excess fertility among couples with discordant fertility preferences by enabling women to attain fertility outcomes that more closely match their desires. Similarly, policies which allow women to access family planning services without the explicit consent of their husbands are likely to have important consequences for contraceptive adoption and fertility. This insight not only has the potential to explain observed regularities in existing data, but potentially should inform policy proposals which

change the outcome of the bargaining process in ways that systematically advantage the priorities of one gender over another[1].

Evidence from qualitative studies and survey data indicates that women in Zambia and elsewhere frequently hide contraceptive use from their partners (Biddlecom, 1998; Castle, 1999; Mccaraher, 2005), suggesting strategic behavior within the household in response to spousal disagreement over fertility. These studies suggest that spousal discordance is a potential factor influencing fertility outcomes. A remaining question is how many women are on the margin of adoption with respect to simple changes in institutional or technological features that would increase their ability to control fertility relative to their husbands. The evidence from existing surveys also cannot rule out systematic differences between the genders in the extent of information about modern contraceptive methods and beliefs about the safety and efficacy of the technologies or differentiate concealment of contraceptive use driven by discordant fertility goals from the more general phenomenon of hiding of income and consumption common in developing country households.

In this study, we experimentally assess the extent to which increasing female relative to male control of the contraceptive adoption decision could increase rates of adoption of modern birth control methods. Women in the study received vouchers that granted appointments with a family planning nurse without waiting more than one hour and guaranteed access to modern contraceptive methods at a government clinic[2]. Women were randomized into two treatment groups. In the "individual" arm of the study, women were given these vouchers alone. In the "couples" arm, women were given these vouchers in the presence of their husbands. In all other respects, the experimental protocol in the individual and couples arms was identical.

If couples bargain to an efficient outcome, then we would expect that experimental variation in information about or nominal control of the voucher would not matter for use. However, if they do not, we will find higher take-up (and possibly lower fertility) among women who are offered treatment alone than among those offered treatment with their husbands, particularly when husbands have measurably higher fertility preferences.

We find that women who received the voucher in the presence of their husbands were 9 per-

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[1]For example, by prioritizing availability of longer-term methods such as injectable contraceptives or contraceptive implants.

[2]In the absence of this program, the typical wait for services at this clinic and others lasted a few hours to a day or more, given nurse absenteeism and poor systems for disseminating information about staff schedules.

centage points (18 percent) less likely to use the voucher to obtain an appointment. We find some evidence that this reduction in takeup was larger if husbands wanted more children than their wives, and stronger evidence that this reduction was larger among young couples than among older couples with completed fertility. There is no evidence that assignment to couples treatment reduces voucher use for women whose husbands want no more children, and evidence for a 12 percentage point reduction in use in the subsample of women whose husbands do want more children.

Section 2 describes the context of the study. In Section 3, we lay out in detail the experimental design. In section 4, we discuss related literature and lay out a theoretical framework for the contraceptive adoption decision. Section 5 describes the data we use in this study, and Section 6 discusses the results. Section 7 discusses further directions and concludes.

2   Context

Our study took place in Lusaka, Zambia, a setting with high reported unmet need for contraception and high maternal mortality. According to the 2001/2002 Zambia DHS, 60% of currently pregnant women in the sample report that the pregnancy was unwanted. While 100% of women reporting unwanted pregnancies reported being familiar with at least one method of modern contraception, only 48% reported ever having used any modern method of contraception, and only 37% reported currently using modern contraceptives[3].

In Lusaka, contraceptives can be obtained through public clinics, private clinics, or pharmacies. Some methods, such as the pill and condoms, can be purchased in pharmacies while others, such as injectable contraceptives and contraceptive implants, are typically only available in clinics. In principle, the full range of methods is available for free through public clinics, although resource constraints manifest themselves in long waiting times for appointments and periodic stockouts of expensive or popular methods. Prior to the inception of this study, the clinic in our study had been out of stock of contraceptive implants for over a year. In addition, for certain methods such as contraceptive implants, women are required to supply some of the materials necessary for the procedure such as surgical gloves and disinfectant. Officially, women are not required to provide spousal consent in order to obtain contraceptive methods through public clinics anywhere

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[3]Author's tabulations.

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in Zambia, although anecdotally health care providers in rural Zambia, as in other parts of rural Africa, still commonly refuse to give contraceptives to women without the explicit consent of their husbands.

3   Experimental Design and Protocol

The experiment we analyze in this paper was nested within the treatment arm of a larger experiment designed to address a broader set of questions related to access to family planning services. Although this paper focuses exclusively on the results from the part of the experiment designed to examine issues related to intrahousehold bargaining, here we briefly describe the structure of the entire experiment. Women were first randomized into treatment and control arms of the study, and within the treatment arm, a subset of women was randomized into "individual" and "couples" arms. Women in both treatment arms received information about condom use and HIV prevention, information about family planning services, and a voucher that gave access to an appointment with a family planning nurse at the local government clinic without having to wait in line; the control arm only received information about condom use and HIV prevention. A graphical representation of the treatment assignments appears in Figure 1.

This paper focuses on the difference between the two treatment arms ("couples" and "individual") in takeup and use of family planning services and contraception in order to study intrahousehold decision making about fertility.[4]

Sample recruitment

We recruited 1994 women to participate in our study from the catchment area of Chipata Clinic, a large government clinic that serves low- and middle-income periurban neighborhoods of Lusaka, Zambia. Women were invited to participate in the study if they were between the ages of 18 and 45, married, had last given birth between January 2004 and December 2006, were not currently pregnant and were not known to have health conditions for which hormonal contraceptives are contraindicated at the time of the recruitment visit[5].

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[4]The inclusion of a control group allows us to answer in a one year follow up survey an additional set of questions about the impact of access to and use of family planning on female labor force participation and investments in the health and education of other children, and about peer effects in contraceptive adoption, in subsequent papers.

[5]These conditions include diabetes, heart disease and high blood pressure. Women were thoroughly screened for these conditions and others for which hormonal contraceptives are contraindicated by a nurse if they visited the

From July 2006 to March 2007, community health workers from Chipata Clinic recruited subjects for the study in two stages. In the first stage, which took place in July and August of 2006, subjects were recruited from the roster of women who had given birth at the clinic. For these women, the date of last birth, address and information on contraindications were obtained from clinic birth records. Only around 50 percent of women on the roster could be located, largely because of false or missing addresses and high rates of mobility within the city[6]. In the second stage, which took place from August 2006 to March 2007, women were recruited by sampling house numbers in the catchment area at random. Women residing at sampled house numbers were invited to participate if they met the criteria for inclusion. If more than one eligible woman resided at a sampled address, only one of the women – the woman whose first name came first in alphabetical order – was invited to participate.

At the time they were invited to participate in the study, eligible women who agreed to participate were told that the study would involve a background survey and information session, and that they would be compensated for their participation in the study. During the recruitment visit, community health workers also gathered a small amount of basic demographic information (parity, current use of contraceptive methods, months since last birth) that was used to stratify the first stage of the randomization into treatment and control arms, described in detail in the following subsection.

Experimental protocol

Using the data from the initial recruitment visits, women were first randomized into treatment and control arms of the study, stratifying on parity, current use of any contraceptive method, current use of the pill, current use of injectable contraceptives, and time since last birth. Of the 1994 women recruited for our study, 1150 were randomly selected to participate in the voucher experiment, after which point they were again randomized into two separate treatment arms[7].

Prior to the second stage of randomization, all 1150 women were visited a second time and administered a background survey in their homes that collected detailed demographic information,

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family planning clinic.

[6]The clinic staff reported that false addresses were often given by women who resided outside of the official catchment area of Chipata Clinic in order to obtain obstetric services at Chipata, which is larger and much better equipped than other clinics in Lusaka.

[7]Individuals assigned prior to March 12, 2007 were assigned to the treatment arm with a 2/3 probability, while those assigned following March 12, 2007 were assigned to the treatment arm with a 1/2 probability.

information about marriage and childbearing, school enrollment of children, fertility preferences, decision-making in the household, contraceptive use, and peer networks. This visit was conducted by a team consisting of one survey enumerator and one community health worker (CHW). Enumerators were responsible for administering the one hour survey and ensuring adherence to the experimental protocol. Community health workers were responsible for delivering health information[8].

Immediately following the background survey, women were given information about the prevention of sexually transmitted diseases (STDs) such as HIV and condom use, and also given a three-pack of Maximum brand condoms. In addition, they were given information about the benefits of family planning, the range of family planning methods, the availability of these methods at Chipata Clinic, specific information about Depo Provera and Jadelle, including contraindications and side effects, and counseling about dual protection. Husbands were not present during either the survey or the information session.

After completing the baseline survey and information session, respondents were asked if they would be willing to have their husbands participate in a short second round of interviews during a third visit by enumerators. The experimental manipulation we discuss in this paper took place during this third visit. At the third visit, in addition to the second round of husband and wife interviews, all women were also given a voucher that granted them access to a family planning appointment at Chipata Clinic with a maximum wait time of one hour and guaranteed that they would have access to methods such as Depo Provera and Jadelle that have been periodically out of stock at Chipata Clinic. Women were told that if, at the time of using the voucher, they waited more than an hour to see a nurse for unforeseeable reasons such as exceptionally high demand for appointments at a certain time on a certain day, they would receive a small gift as compensation[9]. These methods were in stock at Chipata Clinic for the duration of the study, so in practice the primary benefit of the voucher was to significantly reduce the time cost of obtaining an appointment with a family planning nurse[10]. The voucher, which appears in Appendix A, was valid for one month from the day it was issued. Our primary outcome of interest is use of this voucher.

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[8]Community health workers have worked with the clinic previously to implement information campaigns and homecare programs.

[9]In practice, this happened extremely infrequently.

[10]In order to provide these wait-free appointments, we hired a dedicated nurse for the study.

The experimental variation we study concerns to whom the voucher was given: Women who agreed to third visits were randomly assigned to receive either "individual" or "couples" treatments, which determined whether the voucher was given to the woman alone or in the presence of her husband. These assignments were stratified on demographic information collected during the background survey, including age, education, the current number of living children, the reported desired number of children, the reported differential in fertility desires between the woman and her husband, whether or not the woman was currently using injectables, and whether or not the woman was currently using the pill.

The third visit treatments proceeded as follows: When the field team arrived, in all cases the couple was told that to ensure confidentiality of answers, the woman and man would be surveyed separately and in private. The husband survey took place first, followed by the wife survey. Experimental assignment was "announced" to the survey team by prior stapling of the voucher to either the husband or the wife survey sheet. If, upon opening the envelope that contained the surveys, the team found that the voucher was stapled to the male survey, the voucher was given and explained to the husband and wife together ("couples" treatment). The husband was then thanked and compensated for his time. The wife was then administered a short followup survey and given compensation for her participation in the survey[11]. If the voucher was attached to the female survey, the voucher was given and explained to the wife alone ("individual" treatment), after which the followup survey and compensation were given. This followup survey was extremely brief and contained only questions about whether the woman had visited a clinic since the time of the baseline survey and whether she had seen or heard about the voucher.

Given the complexity of the survey and intervention, we established a number of procedures to ensure adherence to the protocol. First, approximately one-third of household visits at the survey and intervention stages were accompanied by Zambian supervisors working on the study. In addition, supervisors re-visited households to confirm that survey teams had implemented the experimental protocol correctly and given the correct information. Finally, short debriefing surveys were conducted for approximately one-half of all study participants who used the voucher at the

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[11]Initially, women were given a choice between cash and compensation in the form of a piece of printed cloth known as a chitenge, of similar value. At later stages of the study, women were only given chitenges as compensation for their participation in the background survey and information session. Men were given the choice of compensation in cash or in cell phone minutes of equal value.

clinic, in order to verify that the correct treatment had been given and that the protocol had been implemented correctly.

To ensure the security of the voucher, the wife's name and national id numbers were written on it.

One final thing to note is that the intervention was very much in line with the existing policies and priorities of the Ministry of Health – for example, encouraging women who had recently given birth to come in for followup family planning appointments, and allowing women access to family planning without husband consent.

Attrition in Individual vs. Couples Treatment Groups

Any woman who completed the baseline survey was assigned to either the Individual or Couples treatment. However, not all women who were assigned a treatment received either the Individual or Couples treatment. In total, 290 of the 1150 treatment women were excluded from the intrahousehold bargaining experiment, 280 of which happened after treatment assignment. This occurred for three reasons. First, in a handful of cases, either the woman or her husband refused to participate in the third visit. Rates of refusal to participate in the third visit were less than 5 percent in both treatment arms. Second, due to resource constraints, women who had not received the third visit by May 4, 2007 were excluded. In this group, which accounts for the bulk of the attrition, no attempt had been made to conduct the third visit for 70 women, while one or two attempts had been made for the remainder. For the latter group, their nonparticipation potentially reflects some combination of the timing of the visit attempts and nonparticipation due to scheduling or tacit unwillingness to participate in the study. Finally, enumerators were unable to carry out the joint appointment after three attempts in the remaining 4 percent of cases, largely because husbands' work schedules made it extremely difficult for them to schedule and keep appointments.

The third visit recruitment protocol was as follows: Surveyors asked to schedule an appointment directly with husbands by contacting them via cell phone or at their place of work, if possible. If the husbands could not be contacted directly, the survey teams left an appointment card containing a short description of the study and the purpose of the visit, and returned at a later date in order to finalize a time to interview the husband and transfer the voucher. Women were told that the teams would return to further interview them and their husbands, and at that time would also

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have a small gift for them in appreciation of their participation in the study. The field teams were instructed to make three separate attempts to schedule joint appointments. However, if all three were unsuccessful, for example due to husbands' unpredictable work schedules, respondents were excluded.

Women for whom the joint appointment could not be accomplished for any of the three above reasons were given the voucher alone and excluded from the intrahousehold bargaining experiment[12]. Hence, in this discussion we refer to attritors as "wife-only".

Table 1 presents the distribution of treatment assignments among the 1150 women who participated in the baseline survey. Throughout the paper, we exclude the 290 attritors and therefore compare outcomes for women who were assigned and received the individual treatment arm to those for women who were assigned and received the couples treatment arm[13]. The high rate of attrition out of our study post-treatment assignment raises concerns over differential selection into the study across comparison groups. In particular, our comparison of fully treated participants will produce a valid estimate of the treatment effect for women in this sample only if the factors determining whether a woman participated in the full experiment were uncorrelated with assignment to the Couples or Individual arm of the study.

A number of steps were taken to ensure that the three sources of attrition did not differ by treatment assignment and therefore compromise the validity of our estimates. Most importantly, the requirement that husbands of women in both treatment arms be present during the third visit prevents differential selection into the couples vs. individual treatments based on husband's availability or either spouse's willingness to participate as a couple. Furthermore, the collection of survey data from husbands at this visit ensured that men in both arms were indeed present at the third visit [14]. This particular - and crucial - aspect of the study design addresses potential differential selection due to both the second and third sources of attrition since households that were dropped from the study because they had not been found by the time of the funding cutoff are likely to be disproportionately those in which a joint appointment was difficult to arrange. Since joint appointments were required of both treatment groups, selection out of our experiment at this

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[12] Although they were necessarily dropped from the bargaining experiment, the voucher was given to these women in order to keep them in the treatment arm for the purposes of the larger experiment outside the scope of this paper.

[13] The estimates shown are ITT with respect to errors in protocol, which occurred in a handful of cases.

[14] The husband survey also provided a valuable tool for directly gathering information on sensitive variables such as fertility preferences and income.

stage should be balanced by treatment assignment. With respect to the first source of attrition (refusal), survey teams were instructed not to tell the study participants that they would be given a voucher for priority access to a family planning nurse at the time the appointment was scheduled [15] Furthermore, since assignment to couples/individual treatments occurred after the second visit, neither surveyors, CHWs nor study participants knew at the time of the survey whether a woman was assigned to the couples treatment or individual treatment, further ensuring that there would not be not differential selection into the two arms at this stage.

Based on these features of the study design, there is little scope for systematic differences in attrition according to treatment assignment. Without prior knowledge of treatment assignment nor differences in participation requirements across study arms, there is no reason for subjects' participation choices to depend on assignment to the Individual and Couples arms. The only concern that is impossible to fully rule out is differential recruitment behavior on the part of the enumerator teams, which could threaten the integrity of the estimates insofar as it leads to differential selection into our study by treatment arm. In the experimental protocol, we take two steps to minimize this concern. First, teams were instructed to wait to open the sealed envelope containing third visit surveys and vouchers until the time of the visit, in order to keep treatment assignment blind until visits had been arranged. Second, survey teams were monitored with frequent unannounced supervisor visits throughout the study in order to ensure adherence to the protocol, and differential recruitment behavior was not discovered during these visits. Furthermore, it is worth noting that the teams had little incentive to violate protocol given that their greatest difficulty was in locating husbands, which was required for both treatments.

While we cannot completely rule out this concern, we test for differences and find no evidence of unbalanced attrition across the Individual and Couples treatment arms. Appendix B shows that the rates of participation at each stage were comparable across the Individual and Couples treatment arms. Furthermore, Table III shows that within this sample, assignment to the couples arm was in fact not correlated with a wide set of observable characteristics (Column II), consistent with attrition being independent of treatment assignment. In contrast, selection out of the couples

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[15]This was done to minimize selection into participation based on the valuation of the voucher. However, given that we randomized the treatment at the individual level, over time information about the voucher may have spread and some subjects may have known that they would receive a voucher if they and their husbands participated in the followup visit. To the extent that this led to selection out of the our experiment among women for whom the treatment effect would be greatest, this has implications for the external but not internal validity of the estimates.

vs. individual experiment (achieved by delaying a followup appointment until after the couples vs. individual stage of the study was complete) was significantly correlated with some individual characteristics (Column I). This does not threaten the internal validity of the estimates. To the extent that the most conflictual couples opted out of this stage of our study, we may underestimate the extent to which increasing female relative to male control of the contraceptive adoption decision in the entire population may matter for outcomes.

As a final check on the selection concern, we run the basic results using an intent-to-treat approach in which all respondents that were randomized to receive treatment and had not been completely excluded from the third visit on account of the budget deadline are included in the regression estimate. Specifically, this estimate includes the 222 attritors who dropped out of the experiment midway through because they could not be recontacted. The anticipated effect in the ITT specification is necessarily smaller than that observed among the subsample that remained in the study. However, our estimates indicate that the basic results retain statistical significance - though are substantially weaker - in the ITT framework.

4 Fertility Decisions in the Household

Related Literature

This experiment sheds light on whether intra-household decision-making leads to efficient outcomes in a particularly important type of household production - reproduction. Standard unitary or collective models of the household imply that fertility should not respond to whether husbands or wives are given nominal control over access to contraceptives, or the availability of concealable contraceptives; finding that those factors do matter would inform the way we think about household decisions about fertility.

The literature on intra-household decision-making puts structure on the aggregation of preferences within families by imposing axiomatic restrictions on the bargaining process or the outcome of the bargaining process. In common preference or unitary models, husbands and wives maximize a consensus welfare function that is a function of their individual utilities subject to the budget constraint pooling total family income. These models are theoretically appealing because they give

rise to demand functions with standard properties. However, they imply strong restrictions on the behavior of demand that are not borne out in the data (Duflo, 2003).

Nash bargaining provides a tractable alternative solution concept, and has the attractive feature that outside options, such as divorce or a non-cooperative equilibrium (Lundberg and Pollak, 1996), influence the allocation of resources within a marriage. Holding fixed the threat point of one spouse, raising the threat point of the other results in a shift of resources to that partner. This change in threat point could correspond to changes in policies regarding divorce, alimony or child maintenance payments, changes in the market wage for women, or changes in the availability or quality of remarriage opportunities (Manser and Brown, 1980). Renegotiation guarantees ex-post efficient allocations in these models, as well as ex-ante efficient investment if investments can be contracted upon. However, such simplified models do not stand up well to empirical tests. For instance, Udry (1996) finds empirical evidence against cooperative bargaining models with full commitment in agricultural production data from Ghana, which reveal that inputs are misallocated across plots of land held by men and women in the same family. Similarly, Rangel (2005) finds that a change in alimony rights for unmarried couples changed the allocation of women's time between market work and housekeeping activities, as well as the level of investment in the schooling of children, particularly for oldest girls, but there were likely wealth and price effects associated with the change in alimony rights, as well as changes in bargaining power.

These findings suggest that incomplete contracts with sunk investments is a more appropriate analytical framework for studying household decision-making. Furthermore, fertility possesses features which make an incomplete contracts approach, such as Rasul (2004), a particularly attractive way to model bargaining over this outcome: fertility investments are sunk in the sense that children are not liquid, investments in fertility are relationship-specific, and it is difficult for couples to write contracts that condition division of marital surplus on number of children.

Empirical evidence in support of this framework comes from Field (2003), who finds that the inclusion of women on formal land titles in Peru lead to significantly fewer pregnancies in the year following the titling program, consistent with a bargaining model in which threat points influence fertility outcomes. As in the Rangel (2005) paper, the potential problem with this interpretation is that land-titling may have had a direct effect on fertility preferences or an indirect effect through wealth or price effects. More generally, any experiment or quasi experiment which attempts to

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induce shifts in the bargaining position or threat point of spouses by providing one partner with additional income or wealth in some form will confound changes in preferences induced by wealth and price effects (such as changes in the time cost of children) with the effects of shifts in bargaining power.

Thus, our experiment provides a more rigorous test of whether couples follow a unitary model or bargain with commitment over fertility decisions by testing whether nominal control over contraceptive access affects real outcomes. Nominal control over access to contraceptives should only impact real outcomes in a model in which couples bargain without commitment over fertility outcomes. We can reject the class of simpler models–which predict that couples can enforce agreements ex post, thereby decreasing incentives for one member of the couple to hide information or actions–if we find higher take-up (and/or lower fertility) among women who are offered treatment alone than among those offered treatment with their husbands. Importantly, this finding would contrast the dominant policy perspective on couples and reproductive health, in which many have advocated the introduction of family planning involvement for husbands as a way of raising use and compliance.

Results from previous quantitative studies on this topic have been mixed: Over the past 40 years, only three randomized studies–Fisek and Sumbuloglu (1978), Terefe and Larson (1993), and Wang, Vittinghoff, Hwa, Yun, and Rong (1998)–have found any evidence that providing education about family planning to husbands raised adoption of contraception, and one very large study found no effect (Freedman and Takeshira, 1969). Our study may help to explain these results by showing that a negative effect of male involvement among couples with conflicting fertility preferences may offset a positive effect of providing family planning education to men. In addition, it may show that a small positive or zero average effect of efforts to include men in family planning decisions may mask important heterogeneity based on differentials in fertility preferences or the distribution of bargaining power in the household. This finding would motivate caution in implementing programs to increase "male involvement" in the policy context, as such programs may make a significant fraction of women worse off if they are not implemented carefully.

Finally, more recent work by Arunachalam and Naidu (2006) suggests that some of the welfare benefits to women from the introduction of family planning may be offset by price changes in the marriage market. Note that as bride prices are typically paid in installments rather than as an up front lump sum in Zambia, their model and predictions do not apply to this context

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in a straightforward way. In future work, we plan to examine the long-run general equilibrium implications of changes in the prices and concealability of contraceptives in a stylized institutional context that more closely matches marriage institutions in Zambia and sub-Saharan Africa more generally.

Theoretical Framework

In future work, we plan to tie our experimental results more closely to the theoretical literature on intrahousehold decisionmaking and the allocation of resources in the household. For now, we simply note that our results appear to be inconsistent with any model that suggests or assumes that couples bargain to a Pareto efficient outcome. Given that the value of the voucher was small, and extremely small relative to expected future lifetime income, there should be no effects of the treatment assignment on the bargaining weights in the household. Absent changes in the bargaining weights, these models would predict that changes in nominal control over or information about the voucher should not affect the decision to use or not use the voucher.

5   Data

The data used in this analysis come from three types of surveys, as well as administrative records of voucher use kept by the study nurse at Chipata Clinic.

The baseline survey was comprised of eight modules which gathered information about basic demographic characteristics of the wife and husband; the wife's marital history; childbirth; ages and characteristics of children in the household; fertility preferences of the wife and husband, as reported by the wife; household decisionmaking and transfers; contraceptive use; and peer networks. The husband survey gathered information about demographic variables that were typically poorly measured in the pilots of the wife baseline survey, as well as information about fertility preferences. The wife follow-up survey was very short and functioned mainly as a device to ensure the field teams had an opportunity to bestow the voucher in private in the individual arm of the study, but gathered information about use of clinic services since the baseline survey and any prior knowledge about the voucher.

Our main outcome variables come from data collected at the clinic on voucher utilization and

the methods chosen during voucher appointments. All patients with vouchers were seen by the study nurse, who collected the vouchers and recorded details of the visit in a log sheet that was checked frequently by study staff. This log sheet included information about the date of the visit, the name and ID number of the woman seen, the method requested, the method given, any followup visits, and other comments.

In a one-year followup survey, we plan to collect information about a broader set of outcomes, including use of and adherence to contraceptive methods, use of services at clinics and pharmacies other than Chipata Clinic, and pregnancy and childbirth.

One key set of variables we construct from the baseline survey data are measures of the extent to which husbands' and wives' fertility desires differ. Using the background survey and husband survey data, we construct three separate measures. First, we consider the difference between the wife's self-reported ideal number of children and the husband's self-reported ideal number of children. Second, we consider the difference between the wife's self-reported ideal number of children and the wife's report of the husband's ideal number of children. Third, we consider the difference between the wife's desired number of additional children and the husband's desired number of additional children[16]. In making comparisons across these indicators, it is importnt to keep in mind that the subsample of women with non-missing discordance data changes somewhat across measures, and is particularly low for the second measure.

Summary statistics for the variables used in the analysis are shown in Table 2. Note the individuals we study are relatively well off and well educated on average, although there is a considerable amount of variation in baseline characteristics in our sample. On average, husbands in our sample report wanting 0.4 more children than their wives.

6 Results

The primary outcome we analyze here is use of the voucher to obtain an appointment with a family planning nurse at Chipata Clinic. Takeup of the voucher was high at 47 percent, indicating that women valued the substantial reduction in the time cost of an appointment associated with the

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[16]Given that approximately 5 percent of women in the sample report being in polygamous marriages, and both men and women in the sample have previous marriages, in all questions regarding fertility preferences we ask about the ideals within the current partnership.

voucher. Furthermore, true demand for voucher use is underestimated to some extent given the "exploding" nature of the voucher offer. In Table 5, we regress voucher use on the indicator for assignment to the couples arm of the study and a set of controls:

$$UsedVoucher_i = \alpha_0 + \alpha_1 * Icouples_i + \beta * X_i + \epsilon_i$$

The observed difference in utilization rates between the two treatment arms indicates that assignment to couples treatment resulted in a 9 percentage point (18 percent) reduction in use of the voucher. The magnitude of the effect is unchanged when demographic controls are added to the regression, including contraceptive use and age, education, income and ideal number of children of husband and wife. Note that complete information on all of these controls is unavailable for 7.6% of the sample. Furthermore, the estimated effect retains statistical significance when the 222 attritors who dropped out of the experiment midway through because they could not be recontacted are added back into the sample. The ITT estimate (column I) provides a lower bound on the estimated treatment effect equal to a 6 percentage point reduction in use of the voucher. The fact that the effect remains in the most conservative specification suggests that it is not driven by differential (in nature, not rate) selection out of the sample across treatment arms.

The basic finding of a treatment effect of the intervention indicates that sharing information about family planning services with husbands reduces the couple's propensity to utilize these services. This could reflect negative male attitudes towards family planning in general, or suspicion of the survey teams among men, in addition to preferences over contraceptive use driven by fertility preferences or cost of contracepting. However, treatment effects for subgroups suggest that the overall effect reflects differential demand for children rather than these other factors. For example, there is no evidence that assignment to couples treatment reduces voucher use for women whose husbands want no more children, and evidence for a 12 percentage point reduction in use in the subsample of women whose husbands do want more children (Table 5, columns III and IV).

To explore further the source of this differential in takeup in treatment arms, we estimate interactions between treatment status and demographic variables from the baseline survey using the following specification:

$$UsedVoucher_i = \alpha_0 + \alpha_1 * Icouples_i + \alpha_2 * Icouples_i * X_i + \beta * X_i + \epsilon_i$$

We find suggestive evidence that the effect of the couples treatment was greater for couples in which the husband's ideal exceeded the wife's ideal, although these interactions are imprecisely estimated (Table 6, columns I through III).

In addition, we see strong evidence that the treatment effect was significantly larger for younger couples (as measured by either the husbands' and wives' ages), also consistent with a story in which differentials in future preferences over fertility drive differences in demand for use of the voucher (Table 6, columns IV through V). In contrast, we find no evidence that takeup was differentially lower when husbands were less educated – if anything, assignment to couples treatment led to larger reductions in takeup in couples where the husband had more education, although this interaction is not statistically significant at conventional levels. This suggests that we can reject a story in which the differential is driven by a pure lack of information among men about the safety and efficacy of family planning methods available in government clinics or a general suspicion of modern medicine or new technologies.

We also explore the relationship between the observed treatment effect and the general phenomenon of hiding of income and consumption using questions in the baseline survey about the degree to which household resources are shared . In particular, respondents were asked, "If you were given 5000 kwacha [$1.25 US], how much of it would you keep hidden from your husband?" and "If your husband were given 5000 kwacha, how much of it would he keep hidden from you?" On average, respondents reported that they would hide 24 percent of this amount from their spouses and that their spouses would hide 27 percent of this amount from them. We estimate the interaction between couples treatment and the reported shares and find that the propensity to hide income is not significantly negatively associated with the impact of couples treatment and cannot explain the observed treatment effect on average (not shown).

Taken together, these results suggest that the unitary and collective bargaining models do not sufficiently describe the bargaining process over fertility within the household. To make these models sufficiently rich to explain phenomena such as fertility, it is necessary to take account of

asymmetric information across spouses in contraceptive availability and utilization.

7   Conclusion

We find that male knowledge of a voucher granting priority family planning appointments led to a substantial reduction in use of family planning services. This suggests that policies or technologies that shift relative control of contraceptive methods from men to women may significantly increase contraceptive use and reduce average fertility in some contexts. This is important to note given that an increasing number of policymakers have started to promote "male involvement" in family planning. It also suggests that take-up of particular modern contraceptive methods may be sensitive to the concealability of those methods.

In future work, we plan to track whether this leads to longer-run differences in contraceptive use and fertility. In addition, we plan to explore the implications of our work for the impact of shifts in relative control in a general equilibrium marriage market context.

Appendix A: Voucher

Figure 1: Voucher for family planning visit and guaranteed supply of Depo Provera and Jadelle

Appendix B: Attrition

APPENDIX TABLE I

Attrition by Treatment Assignment

Outcome	Share of Women Initially Assigned to Couples	Share of Women Initially Assigned to Individual
Third visit completed	0.76	0.74
Wife refused third visit immediately following baseline survey and information session	0.01	0.01
Husband or wife refused third visit at a later point	0.04	0.04
Total wife-only's	0.19	0.22
Wife-only: Attempted 3 times before cutoff date	0.04	0.03
Wife-only: Attempted once or twice before cutoff date	0.09	0.13
Wife-only: No third visit attempt made before cutoff date	0.06	0.05

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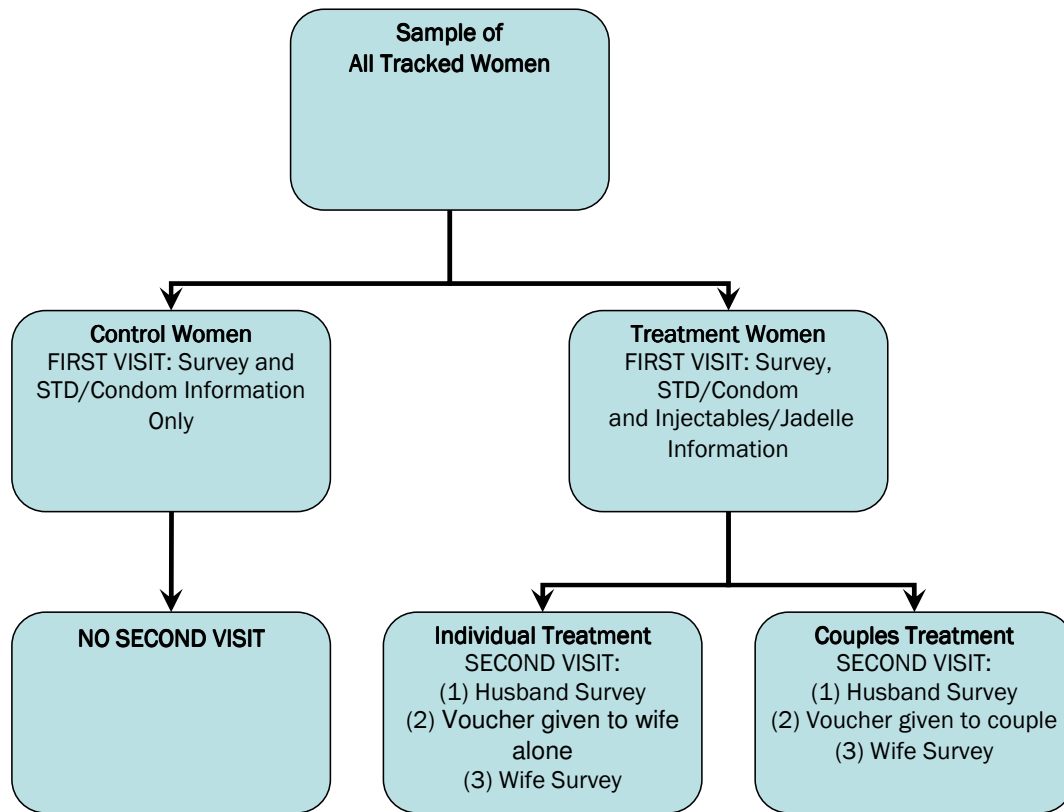
Figure 2: Treatment Assignments

TABLE I
Number of Women Assigned to Each Treatment Arm

	Overall	Individual	Couples
Control	844		
Treatment	1150	590	560

Notes go here.

TABLE II
Summary Statistics

Variable	Mean	Standard Deviation	N
Age	27.50	6.15	821
Husband's age	33.89	7.63	819
Highest schooling attained	6.45	3.02	827
Husband's highest schooling attained	8.68	2.93	826
Average monthly household income (USD)	28.69	73.43	829
Husband's average monthly income (USD)	129.09	151.72	829
Number of living children	3.04	1.77	829
Ideal number of children	4.00	1.57	828
Husband's ideal number of children	4.37	2.86	819
Currently using any modern contraceptive method	0.59	0.49	829
Currently using pill, IUD, injectable or implant	0.47	0.50	829
Have used contraceptive method without husband's knowledge	0.15	0.36	822

Waiting on Stan to send me cleaned section BC.

TABLE III

Relationship between Individual Characteristics and Assignment Within Treatment Arm

Variable	Wife Only (Nonrandom) (I)	Received Couples Treatment (Randomized) (II)
Age	0.006	-0.002
	(0.004)	(0.006)
Highest schooling completed	-0.010**	0.003
	(0.005)	(0.007)
Number of living children	-0.040***	-0.006
	(0.014)	(0.020)
Ideal number of children	0.008	-0.005
	(0.010)	(0.014)
Currently using pill, IUD, injectable or implant	-0.039	-0.051
	(0.027)	(0.037)
Average monthly income	-0.000	0.000
	(0.000)	(0.000)
Husband's age		0.004
		(0.004)
Husband's highest schooling		-0.005
		(0.007)
Husband's ideal number of children		-0.001
		(0.007)
Husband's monthly income		0.000
		(0.000)
Constant	0.282***	0.477***
	(0.088)	(0.129)
Observations	1137	797
R-squared	0.013	0.008
p-values of all listed variables	0.02	0.74

* significant at 10%; ** significant at 5%; *** significant at 1%.

TABLE IV

Relationship between Individual Characteristics and

Use of Pill, IUD, Injectable, or Implant at Baseline

Variable	
Age	-0.021***
	(0.006)
Husband's age	-0.001
	(0.004)
Highest schooling attained	0.010
	(0.006)
Husband's highest schooling attained	0.016**
	(0.007)
Average monthly income (USD)	0.001**
	(0.000)
Husband's average monthly income (USD)	-0.000
	(0.000)
Number of living children	0.058***
	(0.020)
Ideal number of children	-0.005
	(0.014)
Husband's ideal number of children	-0.011*
	(0.007))
Constant	0.756***
	(0.122)
Observations	801
R-squared	0.05

* significant at 10%; ** significant at 5%; *** significant at 1%.

TABLE V

Effect of Assignment to Couples Treatment on Voucher Use

	ITT	Received third visit		Husband wants no more children	Husband wants more children
Variable	(I)	(II)	(III)	(IV)	(V)
Assigned to Couples Treatment	-0.060**	-0.093***	-0.092***	-0.019	-0.121***
	(0.030)	(0.034)	(0.035)	(0.065)	(0.043)
Age			0.003	0.001	0.007
			(0.006)	(0.010)	(0.008)
Husband's age			-0.001	0.007	-0.006
			(0.004)	(0.006)	(0.005)
Highest schooling completed			-0.001	-0.008	0.001
			(0.007)	(0.011)	(0.001)
Husband's highest schooling completed			-0.008	0.001	-0.013
			(0.007)	(0.019)	(0.008)
Average monthly income (USD)			-0.000*	-0.000	-0.000
			(0.000)	(0.000)	(0.000)
Husband's average monthly income (USD)			-0.000	-0.000	0.000
			(0.000)	(0.000)	(0.000)
Number of living children			0.011	-0.023	0.022
			(0.020)	(0.041)	(0.027)
Ideal number of children			-0.007	-0.014	0.001
			(0.014)	(0.026)	(0.018)
Husband's ideal number of children			0.001	0.009	0.003
			(0.007)	(0.031)	(0.007)
Currently using pill, IUD, injectable or implant			0.028	0.052	0.029
			(0.036)	(0.066)	(0.044)
Constant	0.435***	0.502***	0.498***	0.390***	0.533***
	(0.021)	(0.024)	(0.129)	(0.233)	(0.159)
Observations	1082	860	796	257	537
R-squared	0.004	0.009	0.018	0.026	0.032

* significant at 10%; ** significant at 5%; *** significant at 1%.

TABLE VI

Heterogeneous Treatment Effects of Assignment to Couples Treatment on Voucher Use

Variable	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)
Assigned to Couples treatment	-0.084**	-0.082**	-0.076**	-0.416**	-0.438***	-0.052	-0.115**	0.007	-0.013	-0.314
	(0.036)	(0.037)	(0.036)	(0.163)	(0.160)	(0.049)	(0.049)	(0.085)	(0.113)	(0.214)
Couples*Desired Fertility Differential 1	-0.010									-0.013
	(0.015)									(0.015)
Couples*Desired Fertility Differential 2		-0.034								
		(0.028)								
Couples*Desired Fertility Differential 3			-0.026							
			(0.022)							
Couples*Age				0.012**						0.004
				(0.006)						(0.010)
Couples*Husband age					0.010**					0.007
					(0.005)					(0.008)
Couples*Using pill, injectables, IUD or implant						-0.078				-0.056
						(0.071)				(0.072)
Couples*Average monthly income							0.000			0.000
							(0.001)			(0.001)
Couples*Husband's average monthly income							0.000			0.000
							(0.000)			(0.000)
Couples*Highest schooling completed								-0.015		-0.010
								(0.012)		(0.013)
Couples*Husband's highest schooling completed									-0.009	-0.004
									(0.012)	(0.013)
Constant	0.517***	0.616***	0.527***	0.690***	0.703***	0.495***	0.525***	0.480***	0.477***	0.654***
	(0.129)	(0.136)	(0.130)	(0.154)	(0.154)	(0.130)	(0.130)	(0.132)	(0.140)	(0.169)
Observations	797	748	791	797	797	797	797	797	797	797
R-squared	0.019	0.025	0.021	0.024	0.025	0.020	0.020	0.020	0.019	0.029

* significant at 10%; ** significant at 5%; *** significant at 1%. Controls include XX.