The Negligible Effect of Free Contraception on Fertility: Experimental Evidence from Burkina Faso*

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Abstract

We conducted a randomized trial among 14,545 households in rural Burkina Faso to test the oft-cited hypothesis that limited access to contraception is an important driver of high fertility rates in West Africa. We do not find support for this hypothesis. Women who were given free access to medical contraception for three years did not have lower birth rates; we can reject even modest effects. We cross-randomized additional interventions to address possible inefficiencies leading to low demand for free contraception, specifically misperceptions about the child mortality rate, limited exposure to opposing views about family size and contraception, and social pressure. Free contraception did not influence fertility even in combination with these other interventions.

Keywords: Family planning; Demographic transition; Social norms; Randomized trial
JEL codes: J13; J18; O12

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1 Introduction

Over the past 50 years, the number of children born per woman has declined significantly, from a global average of 5 in 1970 to 2.3 today. However, many countries in Sub-Saharan Africa have not experienced this fertility transition to levels near the replacement rate. For example in Burkina Faso, the total fertility rate declined from 6.7 children in 1970 to 4.8 children today (World Bank, 2022). While the reasons for this exceptionalism are debated (Casterline, 2017), the arguably most influential view is that limited access to affordable contraception is the key driver of high fertility in Sub-Saharan Africa (Bongaarts et al., 1990; Bongaarts, 2017). Under this view, there is a large "unmet need" for contraception: many pregnancies are unintended and could be prevented if women had access to reliable and diverse birth control methods.[1]

This view is widely held in policy circles and has shaped government and donor priorities. Over 90% of governments in Sub-Saharan Africa have laws or regulations that guarantee access to contraceptive services (United Nations, 2021). In pursuit of universal access to family planning by 2030, a global partnership known as FP2030 was established through which governments and foreign aid agencies are allocating billions of dollars each year toward the goal (FP2030, 2021). The goals of these efforts go beyond the human rights one of giving women control over their reproductive health: Affordable contraception is often seen as a strategy for economic development, with high fertility viewed as a key barrier to poverty reduction (Cleland et al., 2006; Ezeh et al., 2012; Canning and Schultz, 2012). However, whether unmet need for contraception is actually driving fertility levels in Sub-Saharan Africa is unsettled in the academic literature, which has long debated the relative importance of access to contraception in determining fertility (Pritchett, 1994).

We test the hypothesis that the cost of contraception is an important determinant of fertility by conducting a large-scale randomized controlled trial (RCT) in Burkina Faso between 2018 and 2021. In collaboration with the Ministry of Health, we randomly offer women of reproductive age in rural communities vouchers that cover 100% ("full subsidy") of the cost of contraception at their local government health clinic. In the remaining communities ("control group"), households are provided vouchers for a 10% subsidy. The full subsidy intervention spans a three-year period and ensures free, local access to modern contraception.

[1] This view is supported by the fact that 20 to 40% of women in low-income countries report not wanting to get pregnant in the near future, yet they are not using any modern contraception method—this is the definition of "unmet need" (UNFPA, 2016).

Our sample consists of over 14,000 households recruited from 499 villages covering half of the provinces of Burkina Faso in spring 2018. To assess impacts on fertility behaviors, we conducted an endline survey three years later, in the spring of 2021. Despite numerous challenges (a rise in terrorism in many parts of the country over the study period and the COVID pandemic), we were able to survey 88% of the baseline sample. The primary outcome we study is realized fertility, but we also analyze effects on contraceptive use and desired fertility.

We find that the full subsidy intervention had no significant effect on fertility during the three-year period, nor on the probability and duration of modern contraceptive use, relative to the 10% subsidy comparison group. Based on the 95% confidence interval, we can rule out declines larger than 4.1 percentage points in the likelihood of giving birth over the three-year period, which represents 6.5% of the mean of 62%.[2] This null result contrasts with the significant positive effect of the subsidy on voucher take-up: women receiving the 100% vouchers were 20% more likely to redeem the voucher. Using this measure of contraceptive take-up (the one typically used in other studies), free access to contraception changed contraceptive use. However, when we measure contraceptive use in general, accessed through our vouchers and also elsewhere, we find that the subsidy did not induce new users to take-up contraception. Instead the vouchers simply paid for modern contraception for women who were already using it.

These precise null effects on contraceptive use and fertility imply that financial constraints are not a first-order barrier to contraception and not an important driver of high fertility in this context. Consistent with this, women in our sample state that they would like to have six children, on average, *higher* than their realized fertility at baseline. Nevertheless, significant attention and resources are currently devoted to relaxing supply-side constraints. The Burkina Faso government has recently started a national program providing free contraception. The policy, announced in July 2020 and rolled out in the following months, could not affect births during our study period. Our results suggest that this program will have little impact on birth rates, at least in the short to medium term. Thus, as argued by other scholars e.g. Senderowicz and Valley (2023), the rationale for providing free access should be based on arguments regarding reproductive freedom and universal access to basic care, rather than on its impact on birth rates or even birth spacing.

[2]The study is powered to detect a decrease in fertility of 3.4 percentage points or larger, based on an ex post power calculation that uses the estimated standard error but not the estimated coefficient.

Our finding of low demand for contraception is consistent with a high desired fertility. Whether high desired fertility is optimal or an error from the couple's perspective is crucial for understanding the ethical and distributional implications of policies that aim to reduce desired fertility. We shed light on this question through two cross-randomized 'demand-side' interventions designed to address potential errors stemming from imperfect information. This allows us to test whether access to free contraception influences fertility once other potential frictions are also addressed.

One demand-side intervention aims to correct potential misperceptions regarding social norms by organizing or showing a film of public debates around fertility and contraception in a random subset of villages. Prior work suggests that social norms influence people's fertility choices (Munshi and Myaux, 2006; De Silva and Tenreyro, 2020). Our theory of change was that the debates might change individual beliefs or community norms. First, because the debates present both sides of an issue, they expose attendees to arguments that challenge their current beliefs.[3] Second, the debates might spark ongoing dialogue in the community and shift the social norm. Public discussion might be particularly valuable in contexts like rural Burkina Faso where norms prevent dialogue across generations and genders.[4] Third, the debates could help correct "pluralistic ignorance," whereby members of a group privately disagree with a norm but incorrectly assume that all others agree, and the norm persists because of conformism (Allport, 1924; Bursztyn et al., 2020). We created common knowledge about the community views by having attendees vote, after each debate, on which side represented their view, with the votes tallied and shared publicly at the end of the meeting.[5]

The second demand-side intervention corrects potential misperceptions about the child mortality rate. It is based on the prominent theory in the demography literature that when mortality rates are high, couples adopt a strategy of "hoarding": they have many children to increase the chances of having a few survive (Notestein, 1953; Davis, 1963; Coale, 1986; Preston, 1978; Carey and Lopreato, 1995). Child mortality has halved in Burkina Faso in recent years, going

[3] A number of studies document that individuals tend to expose themselves to individuals who already share similar views, resulting in poor diffusion of information and persistence of shared values – see Levy and Razin (2019)'s review.

[4] A conjecture that motivated this intervention is that social change may be especially slow in societies in which the elderly hold significant power, as is the case in most of West Africa, because it is difficult for younger individuals to express and act on their preferences in such a context.

[5] Several field interventions document that community dialogues can be successful in changing views about contraception in many contexts including Bangladesh (Kincaid, 2000), India (Daniel et al., 2008), Kenya (Wegs et al., 2016) and Niger (Erhardt-Ohren et al., 2023).

from 179 deaths per 1,000 live births in 2000 to 91 in 2018 (United Nations Inter-agency Group for Child Mortality Estimation, 2024). If high child mortality were a significant driver of high fertility, fertility rates would fall. However, it takes time for individuals to become aware of the new mortality rate. If individuals overestimate the mortality rate (because their beliefs are based on past rates), they might have more children than they would deem optimal. We inform a random subset of households about the level and trend in child mortality in their region of Burkina Faso, using an infographic. The true rate in the infographic is based on data we collected in the same communities, before our baseline survey, in which we administered a birth history module to about 30,000 women covering roughly 200,000 births.

We find a null effect for both types of demand-side interventions: they did not modify the effect of the full subsidy and, more generally, they did not impact beliefs (whether first-order or second-order), fertility or contraceptive use. These findings suggest that in this economic and social environment, families may well be making an *optimal* choice to have large families. Previous research in West Africa has shown that parents view having many children as offering economic security—to mitigate their lack of savings, insurance, social safety nets, and property rights, especially for women (Rossi and Godard, 2022; Lambert and Rossi, 2016). The common African saying "*The rich have money, the poor have children,*" expresses this idea. Thus, the policy focus on high fertility as a cause of poverty might be misguided. Instead, redirecting attention toward reducing poverty and expanding the social safety net might be more effective in improving people's economic well-being, and more likely to trigger fertility declines in West Africa.

This paper contributes to the literature on the impact of access to contraception on fertility, and in particular on the role of financial constraints. Two recent reviews conclude that the estimated effect of user fees on contraceptive use is inconsistent across studies and call for more research, particularly to better understand effects on different outcomes (Korachais et al., 2016; Bellows et al., 2016). Surprisingly, virtually all previous experimental studies focus on the effect of contraception access on contraceptive use, but not on fertility. Some notable exceptions are the Matlab experiment in Bangladesh (e.g. Joshi and Schultz (2013)), the Navrongo experiment in Ghana (Phillips and Binka, 2006), and a recent RCT in Malawi (Karra et al., 2022), all of which found that family planning programs significantly reduced fertility.[6] Both the Matlab and

[6]The Matlab intervention lowered lifetime fertility by 14% to 23%, and the Navrongo intervention reduced fertility by 15% (see the review by Miller and Babiarz (2016)). The intervention in Malawi lowered the birth rate from 9% to 5% over a two-year period.

the Navrongo experiments have been criticized for methodological flaws (Miller and Babiarz, 2016). The Malawi experiment only studies women who recently gave birth, so its findings might reflect increases in birth spacing rather than decreases in lifetime fertility. Most importantly, all three experiments included education and counseling, bundling better access to contraception with efforts to increase the demand for contraception. In contrast, a study in Ethiopia that only provided contraceptives on credit for three years found no significant effect on fertility (Desai and Tarozzi, 2011). Our contribution is to show that making contraception completely free for a sustained period for all women of childbearing age is insufficient to affect reproductive behaviors—even in the presence of interventions targeting potential misperceptions, which have not been tested before. Our experiment has sufficient statistical power to detect even modest changes in fertility over a meaningful period of three years.

2 Study Design

2.1 Experimental Design

Figure 1 presents the experimental design. Our sampling frame starts with 100 public health centers across 20 provinces, sampled from the total of 1,500 health centers in the 45 provinces of Burkina Faso (see map in Figure A.1). We sample 5 villages (from among the roughly 10 villages) in the catchment area of each of the 100 health centers, for a total of 500 villages. Health centers are sampled so as to ensure a minimum of 12.5 kilometers between any two health centers in the sample; villages in the catchment area of health centers are selected to maximize the minimum distance between any two villages sampled.

The first randomization is done across health centers: 50 health centers (encompassing 250 sample villages) are selected for the vouchers for free contraceptives intervention. The other 50 health centers receive a 10% discount voucher intervention. This allows us to compare administrative voucher redemption rates across the two arms. We do not expect the 10% discount to have a large effect on contraception use, so this is meant to approximate the status quo.

The second randomization is done across villages within each health center catchment area. In each catchment area, we allocate 3 villages to receive a group intervention and 2 villages to individual interventions. The group (i.e., village-level) intervention entailed a village meeting

with debates or a viewing of an edutainment film on the debate topics. The third randomization is across households within each of the 200 villages allocated to individual interventions. In each village, households receive either information about child mortality, viewing of the edutainment film at home, or nothing.[7]

The goals of the design were to (i) estimate the effect of the subsidy, (ii) estimate the effect of the subsidy when paired with the demand-side interventions of mortality information or the debates/film, and (iii) estimate the effect of the demand-side intervention on their own. In the next sections, we provide details on our interventions and then describe our household sampling and data collection strategy.

2.2 Interventions

2.2.1 3-year-long voucher for free contraception

This intervention, approved by the Ministry of Health in Burkina Faso, was implemented by Innovations for Poverty Action (IPA) in partnership with government health centers. The implementation was identical across the two subsidy levels (100% and 10%).[8]

Vouchers were delivered to all eligible women in sampled households during the baseline survey in spring 2018. The vouchers were accepted by 70% of women; a quite large share (30%) rejected them out of hand. The value of the voucher is small by high-income context standards. For example, an implant (the most widely used contraceptive product) cost about 5 US dollars (USD) to insert and 4 USD to remove. However, this monetary cost is large when compared to local incomes: in rural areas, half of the population lives on 250 USD a year or less (Institut National de la Statistique et de la Demographie, 2019).

All family planning (FP) services, including consultation for side effects or removal of implants and intrauterine devices, were eligible for the discount. In the 100% arm, all costs were

[7] We vary the share of households treated within the village to assess the extent of spillovers. The medium saturation arm assigns 30% of households to mortality info, 30% of households to edutainment, and 40% to be pure controls. In the low saturation arm, these rates are 15%, 15% and 70%, respectively. We exclude the analysis of spillovers from this paper. We find no evidence of spillovers.

[8] A memorandum of understanding was signed between IPA and each health center. IPA committed to cover 100%/10% of the value of family planning (FP) services rendered to women from the 5 villages in the health center's catchment area sampled for the study. In addition, IPA provided a financial compensation to the health center's agents trained to implement the program (identifying eligible women and recording their visits in a dedicated register). IPA made an initial payment at the onset of the program to ensure that health centers did not need to pay in advance for the cost of the subsidized contraceptives and did not run out of stock.

covered, not only contraceptive products but also ancillary products like cotton, alcohol, etc. Records kept by health centers during the intervention show that the most common contraceptives sought were implants, then injections, and to a much smaller extent, oral pills. Stock-out rates for these items were below 5% during monitoring visits, and health centers reported that they could typically restock within a few days.

There are three important points regarding the implementation of the intervention. First, the study was initially planned to last for 2 years, until the spring of 2020. However, we had to postpone the collection of the endline data by one year due to the COVID-19 pandemic. We therefore extended the coverage of the vouchers from 2 to 3 years. In July of 2020, the government of Burkina Faso announced it would launch a nationwide free FP policy. We decided to keep our program running because it was not clear that the policy would be immediately implemented in all centers and we wanted to ensure continuous free access in the treatment group.[9] Second, Burkina Faso experienced a strike among health workers from June to December 2019. Nonetheless, 92% of our health centers reported that all services were fully available to patients during the strike. Third, four health centers in our sample were permanently closed in June 2019 due to insecurity issues.[10] Our coefficients should therefore be interpreted as intent-to-treat estimates.

2.2.2 Community meetings

The goal of the community meetings was to expose participants to both sides of arguments regarding contraception and fertility, generate community dialogue, and create common knowledge about community views. We implemented two types of meetings, where people had either an active or a passive role.

Village debates: Villagers were invited to a public meeting and asked to volunteer for a debate. Six teams (with 5-7 members, diverse in age and gender composition) were formed and assigned a specific position, creating three debates on (1) the quantity-quality trade-off; (2) the relevance of pro-natalist norms in a changing world, and (3) the pros and cons of modern

[9] Figure A.2 report the dates when the free FP policy was implemented in our health centers according to the endline survey. The policy was piloted in the second semester of 2019 in two regions. In a robustness test, we check that our results remain the same if we exclude these regions. Most centers implemented free access during the second semester of 2020, meaning that the policy could only affect births at the very end of our study period (endline data was collected between February and June 2021). Finally, some health centers did not start implementing the free FP policy until 2021, justifying our choice to extend the duration of our program.

[10] Burkina Faso experienced a major rise in jihadist terrorism theats over our study period, as shown in Figure A.3.

contraception.[11] Teams prepared their arguments for about 15 minutes and then debated in public for about 15 minutes. After both teams had presented their case for each of the three debate prompts, all attendees were asked to cast an anonymous vote for the position they personally agreed with for each debate topic. Men and women of older and younger generations cast their votes in separate urns, and facilitators tallied votes and announced the results (vote shares by gender and age group). A concern about the unscripted, participatory nature of debates is that participants might make factually incorrect statements. Thus, in half of the villages with debates, at the end, the project staff member shared factually correct information about child mortality levels and trends, which is a topic where there was scope for participants to make untrue claims.

Village edutainment: Villagers were invited to a public meeting where a 30-minute film was shown. The film depicted villagers engaging in the same three debates on quantity-quality trade-off, pro-natalist norms, and modern contraception, in a more naturalistic, conversational way. Again, at the end of the film, meeting attendees cast anonymous votes and tallies were publicly reported. To write the script of the movie, we piloted similar debates in other villages in 2017 and used the arguments often mentioned by participants. We then hired a professional filmmaker and actors to create the film, and to implement the showings, we partnered with Cinema Numérique Ambulant, an NGO dedicated to bringing cinema to remote areas throughout West Africa.[12] Creating the film allowed us to compare a community-level versus household-level intervention. Another advantage of the film was that it ensured there were no false but purportedly factual statements made. Just like in the debate, at the end of the film individuals voted and the results were publicly shared.

The village meetings could influence fertility through several mechanisms. First, seeing both sides of a controversial topic may prompt participants to change their views and behavior, independent of social interactions. Second, the discussion made these topics salient and could prompt further discussion among people to persuade one another. Third, the votes generated

[11] Debates were framed as follows. **1:** Having plenty of children makes you feel rich and proud. The more children you have, the more likely you are that one of them will succeed *versus* It is not quantity of children that matters, but whether they surpass you. If you have many children, it is difficult to give everyone a good chance in life. **2:** Having a lot of children gives prestige. Even couples who would prefer a small family should have as many kids as everyone else; otherwise they will get ostracized *versus* Each couple is in the best position to know what is best for their individual case. The community appreciates everyone. **3:** If a couple wants to wait several years before the next birth, they should use traditional means of birth control. Modern contraception is a danger to the family *versus* They should use modern contraception; this is the most reliable way to control and space births.

[12] The film can be seen at this link.

common knowledge about the distribution of views in the community, solving potential information failures regarding others' views. In this context, women rarely publicly speak up in the presence of men, as is also the case for younger people in the presence of older people. Thus, revealing votes by gender and age might surface heterogeneity across groups who usually have limited conversation on these topics. Fourth, the common knowledge and discussion "from the bottom up" with the participation of village leaders could enable people to coordinate, for example with younger people deciding to disregard views mainly held by elders.

Individual edutainment: Some but not all of the hypothesized mechanisms through which the village meetings could change behavior are collective in nature. To help separate individual-level belief change from social/collective mechanisms, we followed Bidwell et al. (2020) and implemented an individual treatment in which couples (including co-wives in polygamous households) saw the edutainment film at home.

Public meetings were successfully implemented in 297 out of 300 villages in spring 2018.[13] Sampled households were informed at the end of the survey about the village meeting, if applicable, and local leaders were asked to diffuse information to the rest of the village. Attendance at the start of the meeting averaged 95 people [median: 75, range: 23 to 299], representing roughly 25% of the village population, and was almost identical between debates and edutainment villages. People typically stayed until the end, and additional people often came as the event progressed. Afterwards, many villagers expressed their satisfaction to the implementing team.

2.2.3 Mortality information

This intervention provided information about the province-level child mortality rate and its recent decline. We chose to provide province-level information rather than national-level information because there is a lot of geographic heterogeneity in mortality rates within Burkina Faso, and we thought the risk of providing locally-irrelevant information was high with national statistics alone.

The statistics were computed using birth history modules (similar to those used in the Demographic and Health Surveys) collected in sampled villages during the listing phase. We

[13] Three villages refused to host a meeting before knowing which type of activity would be proposed. They were all in the "Debates" arm. These villages are, of course, kept in the analysis (the results sections below present intent-to-treat estimates).

collected the data ourselves because province-level data on child mortality was unavailable with enough precision in any existing dataset. One attractive feature of the process we used to generate the information is that it was done in partnership with the very population with whom we were working. People may be more likely to comprehend and trust information when they are involved in the information generation process.

In the individual-level treatment, the mortality information was delivered by a trained enumerator at the end of the baseline survey. All adults in the household who were present were invited to listen to the information and were given the chance to ask questions. The script also described the source of the data: *"Recall we came here a few months ago to ask some women in your village about their birth history. We have compiled this information and can now report on the results"*. To maximize comprehension, the information was presented verbally and visually. Figure A.4 displays an example of the charts shown by enumerators and explained as follows: *"The picture on the left shows the situation in the past, and the picture on the right shows the situation today. All the children in purple died before age 5, while all the children in yellow survived until age 5. As you can see, for the previous generation of mothers, such as your mother, 21 children out of 100 died. For the current generation of mothers like yourself, only 11 died. This means that there are 10 children who would have died in the past but, because of the recent progress, they were able to survive."* Enumerators asked a series of follow-up questions to gauge the reaction to the information. Over 95% reported understanding the information. About 50% described the information as "new".

In the village debates with mortality information provided, the information was delivered after the debates. Facilitators used the same script and charts (printed on large posters) as in the individual treatment.

2.3 Sampling of households and randomization

To enroll households in the study, we used the same protocol across all 500 villages. We started by doing a "census" listing to identify the eligible households. The criterion to be eligible for the study was that the household had at least one woman who was (i) age 17 and 35 years old, (ii) married and living with her husband, and (iii) neither pregnant nor the mother of a child under age 6 months at the time of the survey.[14] In households with multiple eligible women, we

[14]We excluded women who had recently given birth since the locally recommended FP method for them at the time was exclusive breastfeeding.

randomly chose one as the "focal" woman. If she was in a polygamous marriage, information on co-wives was collected through the husband.

Our sample is representative of a large share of the population in Burkina Faso. According to the 2019 census, 74% of the national population lives in rural areas. The average woman in our sample is comparable to the average woman in rural areas nationwide, having similar education, religion, polygamy status and household assets (Institut National de la Statistique et de la Demographie, 2022).[15] Low urbanization rates are not specific to Burkina Faso. Overall, 60% of the population in Sub-Saharan Africa live in rural areas; the fraction ranges between 66% and 80% in several large countries, such as Ethiopia, Kenya, Sudan, Tanzania and Uganda (United Nations, 2018).

We randomized health centers to free FP services, stratifying by province, and randomized villages to group interventions, stratifying by health center. In each village allocated to a group intervention, we randomly sampled 25 eligible households. In each village allocated to individual interventions, we randomly sampled 35 households, and we randomized them to one of the individual treatments (mortality information or private screening of the edutainment video), stratifying by village. We sample more households in villages with individual interventions to increase the statistical power of the tests comparing treated and control households within a village.

To ensure balance, our randomization procedure followed Imbens (2011). We generated thousands of potential random assignments. For each assignment, we tested for balance across the different arms we intended to compare, for a set of key variables collected during the baseline listing. We set criteria for a potential assignment to be considered balanced, and then we randomly chose one of those balanced randomizations as the final assignment.[16] Table A.1 shows that the procedure was successful in creating balanced groups in terms of fertility preferences and contraceptive use at baseline.

[15] In 2019, 75% of women in rural areas had no formal education, 64% were Muslims, 38% had a polygamous husband, 4% of rural dwellings relied on the national electricity grid to illuminate their homes, 50% had a radio, 54% had a toilet, 55% had a cemented or tiled floor.

[16] More precisely, we generated 300,000 randomizations. We had 10 key variables and 10 pairwise comparisons (between the 5 treatment arms at the village level), so 100 comparisons in total. We defined a randomization as balanced if all 100 comparisons showed a standardized difference between groups below 0.2. This was the case for 2,232 randomizations.

3 Data

3.1 Data collection

Listing. The census listing took place in Fall 2017. In addition to socio-demographic information on 68,241 households in our 500 study villages, it contains survival data for 190,706 births across the period 1973-2012. Given its size and the paucity of mortality data, our listing data is itself a contribution. It reveals important sub-national heterogeneity in both levels and trends, awareness of which enables a government to appropriately target resources (Figure A.5).

Baseline. We conducted a baseline survey with the focal wife in the spring of 2018 in 499 villages; one village could not be surveyed due to security issues. We completed baseline surveys with 14,545 focal wives, who are our primary respondents, as well as shorter surveys with 10,683 husbands.[17] Each spouse was surveyed in private. The outcome measures collected in the survey were fertility history (pregnancy and births) and proximate determinants of fertility (desired family size, contraceptive use). We collected two types of secondary outcomes. One type represents intermediate steps in the theory of change: affordability of contraceptives, perceptions about child mortality, and community norms about fertility and contraception. The other type are measures of physical and overall well-being (e.g. self-reported health status, happiness and overall life satisfaction, spousal communication), since well-being could be affected by the interventions even without changes in fertility (e.g., knowing that FP services are available for free if one ever needs them may improve a woman's well-being). To measure these variables, we used questions from the Demographic and Health Surveys or newly developed questions that we validated through piloting.

Endline. Due to COVID-19, the endline survey did not take place in 2020 as planned, but in the spring of 2021, three years after the baseline. The security situation in Burkina Faso had deteriorated since the summer of 2018, preventing free movement in the national territory. Therefore, the endline survey could not be conducted in person in all villages. We had to conduct the survey by phone in 169 villages (34%) (see Figure A.6). The phone version of the survey was shorter than the in-person survey and focused on primary and secondary outcomes. Fortunately, the stratification by province, health center and village ensures that all treatment

[17] Roughly one quarter of the husbands were away from the village during the home visits and could not be surveyed. This fraction is the same in all treatment arms (see Table A.2, column 4).

arms are equally affected by these issues, which are highly spatially correlated.

We were able to survey 87% of women and 88% of men initially sampled, which is very high given the disruptions that occurred in this time period. Attrition is higher in places surveyed by phone: 18% compared to 10% in places surveyed in person. Attrition is particularly concerning if it is non-random. Table A.3 shows that the attrition rate is balanced across places that did or did not receive the free FP intervention.

Monitoring and administrative records. Once a year, we visited the health centers to collect (i) voucher redemption data and (ii) monitoring data on stocks of contraceptives, prices charged, and health-worker strikes. In 2019, we sampled a few households from each village and surveyed them about their health center visits in order to audit the quality of the administrative records. Health centers were found to be following the study protocol.

3.2 Baseline summary statistics

Table 1 reports summary statistics for the focal wives at baseline (see Table A.4 for husband data). The average woman is around 28 years of age and is married to a 38 year-old man. 83% of women have no formal education and 45% of women live in polygamous households. Roughly two thirds are Muslim. Households are poor: for example, only 1% have access to an electricity network, 48% own a radio, 47% of the dwellings have cemented or tiled floor and 43% have a toilet.[18]

The average women had 3.5 pregnancies before baseline. Desired fertility is high in the study population. Almost all women (92%) want another child, and 35% want a child in the next 2 years. Women report wanting a total of 6 children. There is little variation in the ideal number of children: half of women want 5 or 6.

Almost all women know about modern contraception. 46% of women have ever used modern contraceptives and 31% are currently using them at baseline.[19] This low use does not appear to be driven by a rejection of these methods: only 20% of individuals think modern contraception is dangerous to health. Finally, almost 40% have an unmet need for contraception and 41% report they would not be able to afford contraception if they wanted to use it. Thus, there

[18]The sample is not constant due to attrition or non-response for outcomes. Some baseline characteristics are also missing.

[19]For reference, 65% of U.S. women aged 15–49 were using a contraceptive method in 2018: https://www.cdc.gov/nchs/data/databriefs/db388-H.pdf

is evidence based on the survey that financial constraints may be a barrier in this context, justifying our intervention.

3.3 Individuals' attitudes and perceived social norms

Although people know about contraception and many use it, social acceptability of contraception is perceived as low: a substantial fraction of people report that the community views using it to delay first births negatively (65%) or stigmatizes family planning users (39%). These negative social perceptions stand in contrast with private views about contraception within our sample: most individuals think modern contraception is acceptable. This mismatch could reflect either pluralistic ignorance, but another possibility is that elders (who are not in our sampling frame) hold the more traditional views and set the community norms. Regarding family size, the vast majority of respondents agree there is a quantity-quality trade-off and do not perceive there is a social norm to have a large family.

Our post-intervention data – specifically, the votes held after village meetings — provide some descriptive evidence suggesting a generational divide. Support for non-traditional views is quite high overall, at over 50% in most villages for all three debates (see Figure A.7 and Figure A.8). However, older adults are more likely to support the traditional views, as are men. In a quarter of the villages, the votes reveal disagreement between genders and generations regarding FP and acceptable family size. This suggests scope for the village meetings to spark cross-group dialogue about their different views, with perhaps resulting changes in the norms. Moreover, group-specific views differ considerably across villages, which suggests scope for misperceptions. Thus, the public meetings during which the vote tallies were revealed could lead to more accurate knowledge about norms in the community.

3.4 Perceived and actual child mortality

Almost 30% of women in our sample have had a child who died, so child mortality is a salient phenomenon. Current levels of mortality remain high by international standards: around 10% of children do not survive to age 5. The rate ranges across provinces from 7% to 14%. The situation was much worse and more diverse in the past: the average was around 16%, ranging from 12% to more than 20% in several provinces, including 32% in one province.[20] The

[20] Figure A.9 plots the under-5 mortality rates estimated from our listing data in different provinces for the most recent cohorts (born between 2007 and 2012) and older cohorts (born between 1973 and 1998). This is

beginning of the twenty-first century was thus a period of progress and convergence. These levels and downward trends are consistent with that observed for the country as a whole in the Demographic and Health Surveys.

How do people form expectations in this rapidly evolving context? We piloted different ways of eliciting subjective expectations and opted for a straightforward question: *According to you, out of 100 children born today in your village, how many will survive to the age of 5?* Respondents overestimate current mortality levels in all provinces but one: the average perceived risk is 20% (see Figure A.9). This is consistent with demographers' hypothesis that there is a lag between perceived risk and actual risk because it takes time to adjust to mortality changes (Montgomery, 2000). An alternative explanation is that people overestimate small probabilities, and since mortality is declining, the perceived risk is mechanically closer to mortality rates in the past than today. Our evidence that on average there is over estimation is consistent with studies by Delavande and Kohler (2009) in rural Malawi, and LeGrand et al. (2003) in Zimbabwe and Senegal who also find that people tend to overestimate child mortality on average.

Given heaping in responses, we classify responses that are within 5 percentage points (pp) of the true rate as accurate. About a third of beliefs are accurate (see Figure A.10, which plots the distribution of perceived mortality rates, by gender). About half of respondents overestimate the mortality rate by more than 5 pp. Many of them make large mistakes: The mean perceived rate is 19% for women and 16% for men. The remainder of people underestimate the actual rate by more than 5 percentage points.

To recap, while some people greatly overestimate child mortality, overestimation is far from universal. This has implications for the expected sign of the information treatment's effects. The information can help everyone re-calibrate their beliefs, with heterogeneous effects on subsequent behavior, depending on whether people underestimate or overestimate at baseline.

precisely the information provided in the information treatment arms.

4 Experimental Results

4.1 Regression specification

We estimate the impact of the supply intervention (vouchers for free contraception) with the following model:

$$Y_{ivcp} = \beta_1 FullSubsidy_c + \gamma_p + X_{iv}\rho + \varepsilon_{ivcp} \quad (1)$$

where Y_{ivcp} is the outcome of interest (typically fertility, or contraceptive use) for a given individual i in a given village v covered by health center c located in province p. $FullSubsidy_c$ is an indicator variable for whether the health center was assigned to the 100% subsidy. γ_p is a set of province fixed effects. We add them to account for the stratified design. In our baseline specification, the controls in X_{iv} are an indicator for whether the village had to be surveyed by phone due to security concerns and the date of the endline survey (as a continuous variable measured in days). We report heteroskedasticity-robust standard errors clustered at the level of randomization, namely the health center.

As a robustness check, we include a set of individual controls measured at baseline in X_{iv}. Most importantly, we include the baseline outcome (when available) and additional controls that are highly predictive of fertility behaviors in previous work (wife's age, spousal age gap, polygamous union, whether the wife has ever gone to school, number of births at baseline, whether she has had a child who died, whether the husband has been surveyed, and DMI radio exposure).[21] Adding controls should not affect the point estimates (if the randomization worked) but can reduce the variance of the error term and thus improve the precision of the estimates. The results are similar if we include controls.

We also estimate the effect of the subsidy in sub-populations that would be expected to have the largest effect of free contraception – those for whom the cost of contraception seems most likely to be a binding constraint. We identify these sub-populations using baseline information, for example on financial constraints or stated unmet need for contraception.

In addition, we test how the effects vary with the extent of demand-side information frictions

[21] The baseline survey collected information on all baseline outcomes. When the values of any controls are missing we impute a value and include an indicator dummy equal to one if the variable was imputed. DMI radio exposure is included to account for the fact that DMI, an international NGO, broadcast a radio show about FP in parts of Burkina Faso during the study period. See Glennerster et al. (2021) for more information.

to test the idea that relaxing supply constraints is most effective when there are no additional demand constraints (i.e., misperceptions or conformity to norms), which drive up desired fertility. We use two approaches. First, we examine the effects of the subsidy in sub-populations that appear less constrained by demand-side frictions. Second, to address the fact that baseline proxies of demand-side frictions are not exogenous, we exploit our randomized demand-side interventions and estimate a version of equation 1 in which we interact the subsidy with dummies for different demand-side treatment arms.

4.2 The effects of contraception subsidies

4.2.1 Average effects

We start by investigating the effect of free-contraception vouchers on fertility. A preliminary way to summarize the results is to plot the fraction of women who have not given birth to another child as a function of time since the intervention began (Figure 2). By construction, the fraction is 100% at t=0. The interventions could only influence births taking place at least 9 months (shown as a vertical line) after the intervention start. At the 9-month mark, roughly 20% have already given birth suggesting that many women were pregnant but did not know it at the time of the baseline survey and thus were included in the study. Importantly, there are no differences at this time between the full subsidy group and the control (10% subsidy) group.

If the intervention had an effect, we would expect to see fertility diverging after 9 months. Instead we observe that the curves follow each other closely, suggesting no impact of the free vouchers. Indeed, a test of equality of the Kaplan-Meier survival curves cannot reject the null that the curves are the same (p-value = 0.55). Moreover, there does not appear to be any timing differences that emerge at any point. This result suggests that any aggregate measure of fertility will not be affected by the voucher because we do not see differences in fertility rates at any time during the 3-year duration of the study.

This null result is also seen in column 1 of Table 2, which reports the results from estimating equation 1 without baseline controls in Panel A and with baseline controls in Panel B. In Panel A, those receiving the full subsidy have a 1.7 percentage point lower probability of having a live birth in the three years since the baseline, which is a 2.7% decrease relative to the control mean of 62%. This is a small and statistically insignificant effect. One explanation for the

absence of effect on live births could be that access to contraception leads to fewer and healthier pregnancies. Pregnancies would decrease, but miscarriages would also decrease conditional on pregnancies, which would leave live births unchanged. Since we collected data on pregnancies, we can directly test and rule out this hypothesis: using the same specification as in Table 2, we find a 1.9 percentage point lower probability of having a pregnancy, a 2.7% decrease relative to the control mean of 70.5%. The coefficient is insignificant (the standard error is equal to 0.012). The magnitude of the effect is therefore similar for live births and pregnancies. Consistent with the lack of effects on fertility, the fraction of women using contraception in the last 3 years did not increase in response to the subsidy: receiving a free voucher increases self-reported contraceptive use by 0.0003 percentage points, less than 0.1 percent increase (column 2 of Table 2). There is also no effect on the intensive margin of use: the subsidy led to a statistically insignificant and very modest decline in the reported number of months that women used contraception in the last 3 years (a decrease of 0.3 months relative to a mean of 9.6 in the control group, Column 3 of Table 2). The points estimates and significance levels are very similar in Panel B, when we add baseline controls.

This null result is important for policy. The 95% confidence interval on having a birth is [-0.041; 0.007]. An ex post power calculation, based on only the standard error of the estimate, indicates that the study is powered to detect a decrease in fertility of 3.4 pp or more, implying that, in the most conservative case, we can rule out a fertility decline larger than 5.5%.[22] Using these estimates we can expect (under some assumptions) that 30 years of access to free contraception, between ages 15 and 45, would lower lifetime fertility by at most 0.034*10 = 0.34 children per woman. While not insignificant, this is a small effect relative to the actual total fertility rate of around 6 children.

Why are households not sensitive to the price of contraceptive services? We know from audits and surveys with female participants that implementation problems can be ruled out, i.e., clinics did honor vouchers. We also know that voucher use was not zero: 14% of women in the control group and 17% of women in the treatment group redeemed the voucher at least once (Column 4 of Table 2). This 3 pp difference is significant at the 1% level. In the health centers' registers, the difference is 4 pp and is also significant at the 1% level. The fact that voucher redemption is barely higher in the treatment group confirms the low responsiveness to prices, and suggests that

[22]The minimum detectable effect size with rejection rate $\alpha = 5\%$ and power $\kappa = 80\%$ is obtained by multiplying the standard error of the coefficient by $t_{1-\kappa} + t_{\alpha/2} = 2.8$.

most of those who redeemed the free contraception voucher were *inframarginal*: they would have used contraception in any case. Thus, the intervention subsidized the use of modern contraception among individuals who were using it already.

Note that voucher take-up in the treatment group (17%) may seem low compared to the fraction of women currently using contraception at baseline (31%). There are two reasons explaining the gap. First, 59% of baseline users had a long-lasting implant and either needed no replacement during the period (the implants last 3 to 5 years) or planned to get pregnant after the expiration. Among baseline users, 16% rejected the voucher right away during the home visit, most of them explaining that they did not need it because they already had an implant. Second, women may prefer to get contraception from a different provider than the health clinic, potentially a more convenient one, even though they had to pay for it.[23]

4.2.2 Effects in sub-populations with the likely largest effects

Although our overall impacts on fertility are small and insignificant, these results could mask significant heterogeneity. We investigate this by estimating the treatment effects within sub-groups that would *a priori* be expected to have higher responses to the subsidy.

We first examine women who have unmet demand for contraception based on their baseline survey responses. We start with the UNFPA definition of unmet need: the woman did not want another child in the next 2 years *and* was not using modern contraception. Then, we look at both dimensions separately. We also look at women whose husband did not want a child in the next 2 years. Finally, we consider women who stated that they could not afford contraception if they wanted to use it.

The results are shown in Table 3, which reports the same outcomes as Table 2 (see Table A.5 for the specification with baseline controls). Since we are testing a large number of hypothesis among sub-samples, we report Anderson's q-values correcting for multiple hypothesis testing (MHT). The effect sizes are still surprisingly modest. The largest effects on fertility (i.e. "had a live birth since baseline") that we estimate are for women who could not afford contraception at baseline (2.5 pp, a 4% decline). However, the effects of the 100% subsidy on fertility and modern contraception use are statistically insignificant, even without MHT corrections.

[23] While it is possible women are getting free contraception from other sources (including towards the end of the period from government sources), in the endline survey, 78% of women currently using contraception in the control group said they had to pay for it.

These findings suggest that financial constraints are not the main or only barrier to contraceptive use for most women. Either women do not want to use contraception, they are afraid to do so, or they face other barriers, and its free provision does not ameliorate these issues.[24] To investigate this, we test whether the subsidy had larger effects on contraception use and fertility among subgroups for whom other frictions are small.

Social pressure could lower the demand for modern contraception. In Burkina Faso, use of modern contraception is stigmatized. We assess whether the demand for contraception is higher and the fertility effects of the full subsidy are larger among women who at baseline either: (a) do not believe that their community disapproves of contraception, or (b) do not report being punished or feeling stigmatized for using contraception. The results in Table 3 show that there are no statistically significant effects of the free contraception among these groups either (all of Anderson's q-values in columns 1 to 3 exceed 0.10), and the magnitude of the fertility effects is similarly modest (a decrease by 2.7 pp (=4.3%)).

Next, we assess the role of perceived child mortality rates. Perhaps women do not want contraception because they perceive child mortality as higher than it is and, thus, desire a large number of children. In this case, free vouchers would be ineffective. However, Table 3 shows that the full subsidy had no impact (by 1.6 pp , or 2.5%) on fertility or contraceptive use in the group of women who do not over-estimate child mortality. This suggests that factors other than child mortality keep the demand for children high and/or the demand for modern contraception low.

We consider two other factors besides cost that might deter people from using contraception even when they want to control their fertility. The first is the distance to the health center. While the full subsidy makes modern contraception free, it is well known that many other barriers prevent individuals from using health care, including the time cost to access such services (Karra et al., 2022). The second is the perception (quite commonly cited in Sub-Saharan Africa) that modern contraception has significant negative side effects (Glennerster et al., 2021; Bau et al., 2024). Indeed in our focus groups some cited fears of infertility and other health concerns when asked about why they opted out of modern contraception.[25] Again Table 3 shows there are no statistically significant declines in fertility among groups for whom

[24] Since we do not find that the voucher affected contraceptive use, we cannot estimate 2SLS models of the effects of contraceptive use on any outcome.

[25] Among the wives surveyed at baseline, 30% agreed either with the statement that modern contraception is dangerous for health or with the statement that it causes sterility.

these additional frictions are low. However, the effect of the full subsidy is 3.2 pp (=5.4%) among those who are close to the health center—the largest magnitude we estimate among any subsample in our data, though the effect is again statistically insignificant with or without MHT corrections. Among this group, the vouchers appear to also increase contraceptive use and the duration of contraceptive use. However, the statistical significance of the modern contraception results does not survive MHT correction. The results are nevertheless suggestive that, in our context, distance may reduce the price elasticity of demand for contraception, whereas fear of side effects does not.

Note that for all of these subgroups, we estimate statistically significant impacts of the full subsidy on voucher use even when accounting for MHT, further confirming our hypothesis that the vouchers primarily subsidized those who were already using modern contraception.

In sum, we do not find any statistically significant effects of free contraception on fertility and modern contraceptive use even among sub-populations for which we might have anticipated larger impacts, though the results do suggest there are subgroups for whom there might be modestly sized effects: those who report not being able to afford contraception, those who do not perceive modern contraception to be socially unacceptable and those who live more than 2 kilometers from a health center.[26]

4.3 The effects of contraception subsidies when potential information frictions are addressed

In addition to the free contraception intervention, we implemented two other types of cross-randomized interventions aimed at addressing potential information frictions. We next test whether these additional interventions modified the impact of the full subsidy, with the results presented in Table 4. We estimate a version of equation 1 where we interact the subsidy with dummies for different demand-side treatment arms. Results for the specification including

[26]We also investigated a number of other subgroups, see Table A.6. The largest impact of the full subsidy was found among those what were already using modern contraception at baseline (2.8 pp decline in fertility, not significant). We also hypothesized that older women who are closer to having completed their desired fertility would be more responsive to the subsidy, but if we split the sample by median age, we find no effect on contraception in either sub-group. We considered the possibility that the vouchers would be less effective in polygamous marriages in which wives' status is influenced by their fertility. We find similar null effects for both polygamous and non-polygamous couples, and for junior and senior wives. We also tested for the possibility that the demand would vary with the number of prior pregnancies but found this is not the case. Finally, we find that the results are robust to excluding the two regions, Cascade and Centre-West, where the government made contraception free ahead of the roll-out of its national program (see Figure A.2).

baseline controls are similar and reported in Table A.7.

4.3.1 Providing information about opposing views and social norms

One set of our interventions aimed to expose people to a diversity of views and disseminate information on the community's collective views about modern contraception and family size. This occurred through a village meeting with debates or the viewing of a film. We pool the debates and edutainment in the analysis because they have a mostly common theory of change, and we see no difference between them in their effects (see online appendix Table B.1).

The theory of change is that if individuals are hearing new arguments in favor of less traditional views or had overestimated how conservatives others are, then the intervention might systematically shift them toward lower fertility. Similarly, post-village-meeting dialogue might shift the collective norm away from traditional views. In these cases, free contraception might lead to a larger decrease in fertility when combined with this village-level intervention. Conversely, better information could have the opposite effect or no effect on views and behavior, depending on the nature of information gaps that existed pre-intervention. Thus, by testing if the combined interventions (village meetings and free contraception) reduce fertility, we are testing the joint hypothesis that (a) misinformation about attitudes and norms systematically increases desired fertility and reduces acceptance of modern contraception under the status quo and (b) cost is a barrier to contraceptive use.

We do not find that the village-level interventions modified the effects of the full subsidy: none of the interaction terms are significant at the 5% level (Table 4) (although in this interacted specification there is a decline in live births of 3.6 pp (5.7%) associated with the full subsidy that is statistically significant at the 10% level, suggesting larger impacts of the vouchers for populations without other interventions). The main effects of the interventions themselves are also insignificant, suggesting that the interventions had no average effect. We confirm this when we estimate the effects of the interventions without interaction terms (online appendix Table B.2). We find similar null results for the individual edutainment treatment, which entailed showing the woman and her household members the edutainment film on a tablet computer in her home. Importantly, the interventions did not change either individual beliefs or perceptions of social norms, either on average or based on initial beliefs (Appendix Table A.8).[27]

[27]One of the twenty reported treatment and treatment interaction coefficients is significant at the 5% level.

The absence of an average effect could mask some people updating and changing their behavior in either direction. However, when we examine heterogeneity by baseline personal views or by beliefs about social norms, we also see no effect (Appendix Table A.9). The lack of effect cannot be attributed to poor implementation: Attendance at the meetings was high, so people did hear others' views. The results indicate that hearing these views did not change their own views.

4.3.2 Addressing mortality misperceptions

A second demand-side intervention aimed to correct beliefs regarding child mortality. On average, people overestimated child mortality at baseline. The "hoarding" hypothesis is that if these beliefs are corrected, then the demand for children will be lower, leading to a higher take-up of modern contraception and lower fertility.

This is not what we find. The full subsidy did not lead to a larger decline in fertility for those provided accurate child mortality information. The coefficient on the interaction is small and statistically insignificant (Table 4). The interaction term is also small and insignificant for the contraceptive use measures (columns 2 and 3). Table 4 further shows that intervention itself did not affect fertility or the demand for contraception (the main effects are not significant).

This null effect is not due to some people revising their mortality beliefs downward while others revised upward. Figure A.11 shows the distribution of perceived minus actual child mortality at endline among women, for the treatment and control groups. If people updated toward the statistics provided, we would expect the treatment group to have a narrower distribution, bunched closer to a zero gap between actual and perceived mortality. We see no such narrowing and furthermore we cannot reject that the distributions are identical.[28] Thus, this intervention did not work in the sense that the first step in the theory of change – updating beliefs – did not occur.

Why did individuals not update their beliefs in response to the information? Study participants seem to have understood the information, so the explanation seems to lie elsewhere. One

[28]The average under-5 mortality is perceived to be 16.4 percent at endline. The individual information treatment lowered this by 0.29, or less than 2%. The null effect holds for men as well. When we split the sample by whether individuals initially over- or under-estimated child mortality, we find no effect on perceptions for either group. The effects of the mortality information treatments are also small and insignificant among subgroups with the least reason to disregard the information we provided, such as those who had never lost a child (online appendix Table B.3). Finally, when we compare the effect of the village debate and the debate with mortality information, there is no difference in beliefs.

possibility is that they did not believe the information, perhaps because it conflicted with their experience. Recall that 30% of women have lost a child. The first-born mortality rate is higher than the mortality rate for later-births, which mechanically makes the rate experienced so far by a cohort higher than what they should expect going forward. Another possibility is that the political instability and COVID-19 pandemic made people fearful that the recent progress would be erased, rendering the information on the recent past less pertinent. While we can only speculate why people did not update, our results highlight that it is very difficult to change beliefs about child mortality.

4.4 Effects of free contraception on other outcomes

In the follow-up survey, we included a module on intimate partner violence (IPV) to test if any of the interventions had the unintended consequence of increasing IPV. We used well-validated questions to measure various domains of violence, and extensively piloted them in the field during summer 2019. The sample size for the IPV module is smaller since it could only be administered in-person, not by phone. We also included a set of well-being measures that were asked of all participants to test if the interventions increased well-being or at least to verify that the interventions did not lower it.

We find that the full subsidy treatment seemingly *decreased* the incidence of IPV (columns 1-4 of Table 5). Thus, although the full subsidy did not impact fertility substantially, it improved relations between spouses. This is not due to an improvement in the communication between spouses regarding fertility and contraception: if anything the subsidy decreased the number of times spouses talked about desired fertility and contraceptive use (online appendix Table B.4). Instead, we hypothesize that the subsidy lowered tensions between spouses related to financial issues—we know that most of those taking up the vouchers were already using contraception but they were likely paying a higher price for it. However, these results are to be taken with a grain of salt: the sample is smaller and we do not have baseline IPV data to verify balance pre-intervention. Moreover, Columns 5 and 6 of Table 5 do not reveal a positive effect of the subsidy on self-reported health or life satisfaction. Finally, as reported in column 7, the subsidy did not change the likelihood that initially monogamous husbands took another wife.[29]

[29] 99 percent of respondents stayed married throughout the study so there is no scope for an effect on separation.

4.5 Discussion: The residual explanation for high fertility is high desired fertility

Our finding that free modern contraception did not influence fertility, even among subgroups with ex-ante high demand for contraception (based on the conventional measure of unmet need) or when other potential frictions influencing desired fertility were addressed, suggest that it may be time for policy-makers concerned about the "stalled" fertility transition to start suspecting the obvious: households *want* many children. This is, after all, what they say when asked about their ideal family size: the mean is 9 for men and 6 for women in our data, and similar levels are found in DHS surveys.

The high demand for children, on average, does not seem to be due to misinformation. Instead our survey data lends support to the view that there are still high returns to quantity of children in the context we study. First, individuals cite their need for old age support. Table 6 shows that 74% of men and 81% of women report they would not be able to cope in old age without the support of children. Moreover, not all children are able to support their parent, and a common view is that each child is a "lottery ticket": the more children one has, the higher the chances that at least one of them succeeds and can provide old age support. Men expect only 20% of their children to send enough money back to sustain parents, for example. Second, children are also an important source of labor even before their parents reach old age. 39% of household heads report that family labor is the main constraint to expanding their farm size. When asked what would happen if they had one less child than their desired family size, 17% of women say that they would not have enough children to perform tasks around the house and 13% not enough children to work in their agricultural fields. Finally, children are a source of happiness. 14% of women say that they would be less happy if they had one less child than their ideal family size. Altogether this qualitative evidence suggests that households want many children and that the value of additional children may exceed the value of investing more in each individual child in rural Burkina Faso.

5 Conclusion

This study investigated the effectiveness of providing free contraception for three years in rural Burkina Faso, a context where both desired fertility and realized fertility are among the highest in the world. We found a precise null effect of offering free contraception on birth

rates. This is an important finding from a policy perspective given that subsidized access to modern contraceptives is a major focus of women's empowerment programs around the world, among governments and international organizations alike. Our results imply that, in contexts similar to rural Burkina Faso, the resources allocated to family planning might be ineffective in achieving women's empowerment or economic development.

Our study provides other lessons. First, even when people overestimate child mortality, providing accurate, relevant and reliable information on levels and trends did not influence mortality beliefs in the medium run, and did not increase the take-up of modern contraception even when it was offered for free. Research remains to be done to better understand how survival expectations are formed and most importantly how they may be modified – our results suggest that this is not an easy or cheap task since our efforts to collect and disseminate information locally failed. Second, while social norms and perceptions of these norms are important, there do not appear to be large misperceptions regarding these norms in our context. As a result, the provision of free modern contraception was not more effective in communities with debates or movies addressing social norms. One-time community-level interventions may not be a useful policy lever.

Do these results, especially the lack of responsiveness of fertility to contraception access, suggest a "West Africa" exceptionalism? Our reading of the evidence from other contexts does not suggest so. In higher-income countries the introduction of the pill is thought to have had modest effects on aggregate declines in fertility (Bailey, 2012; Knowles Myers, 2017). In lower-income countries, many family planning programs also had a modest impact on fertility decline, with some exceptions (Miller and Babiarz, 2016). Conversely, in high-income countries, policies that provide financial incentives to households to have more children have small effects (Kim 2020, Aizer et al. 2020). One interpretation of this evidence is that financial levers can only affect fertility in populations that desire to change their fertility but are prevented from doing so by financial constraints. This group appears to be small in our context, which is similar to the context in most West African rural areas. We conclude, contrary to the conventional wisdom in policy circles but in line with early contributions by Becker (1991), Easterlin (1975) and Pritchett (1994) and recent empirical evidence by Dupas et al. (2023), that fertility levels are primarily determined by deep economic factors.[30]

[30]This conclusion is also in line with the historical experience in the 1960s of international efforts (such as those by USAID) to expand access to contraception in low-income countries to lower fertility and curb population growth. Many perceived these efforts as having modest impacts at best (Hartman, 1997).

Our results do not imply that efforts to improve access to modern contraception have no value. Reproductive freedom is an important right. For some women, even if not many, access to contraception enables them to reduce their fertility to their desired level. For others, it allows them to manage the timing of fertility more easily, if not better. It is also possible that free family planning will change attitudes among people who grow up in this regime, leading to larger long-run effects. Nonetheless, we conclude that, relative to family planning interventions, policies to promote economic development will likely be the larger driver of fertility decline in Sub-Saharan Africa.

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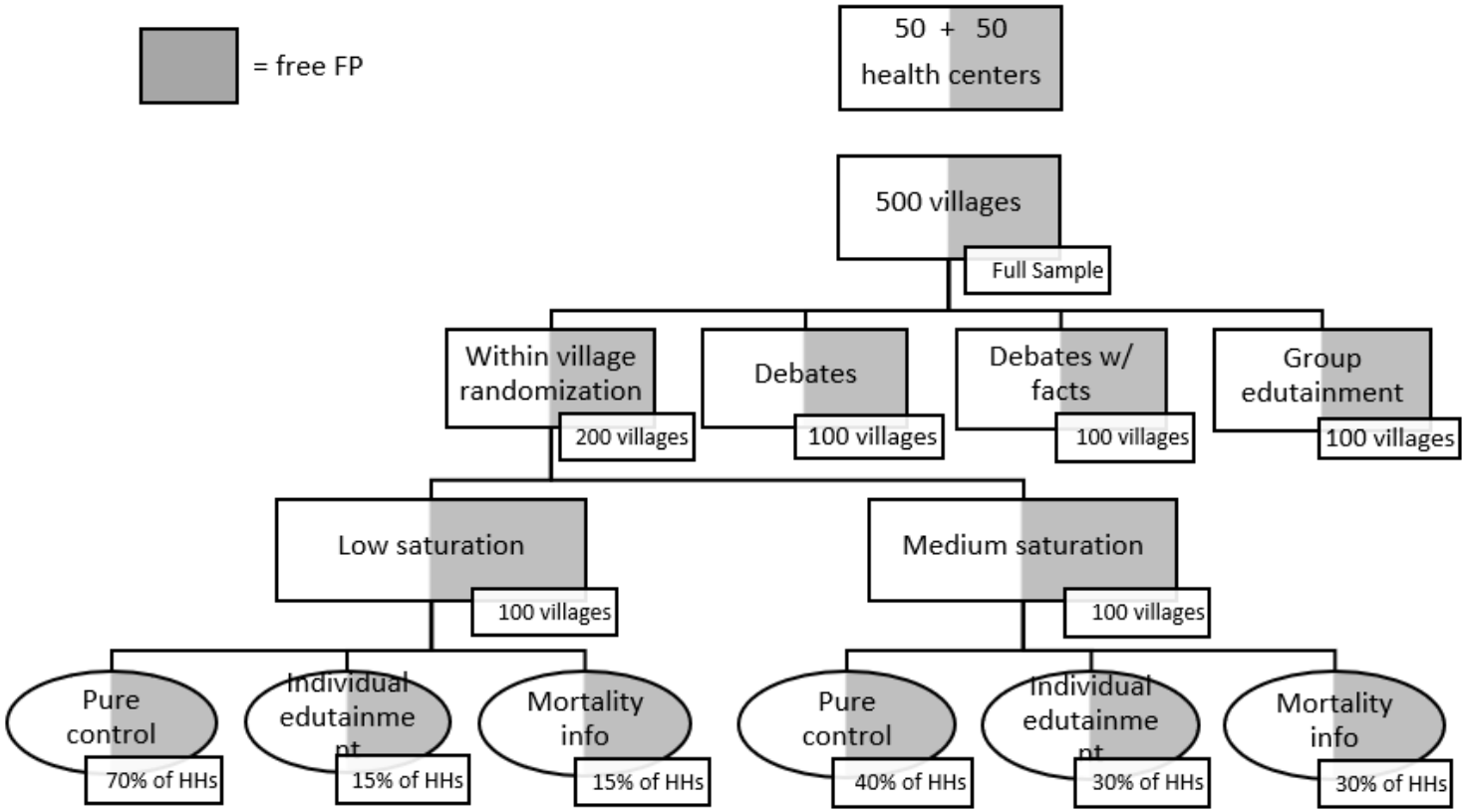
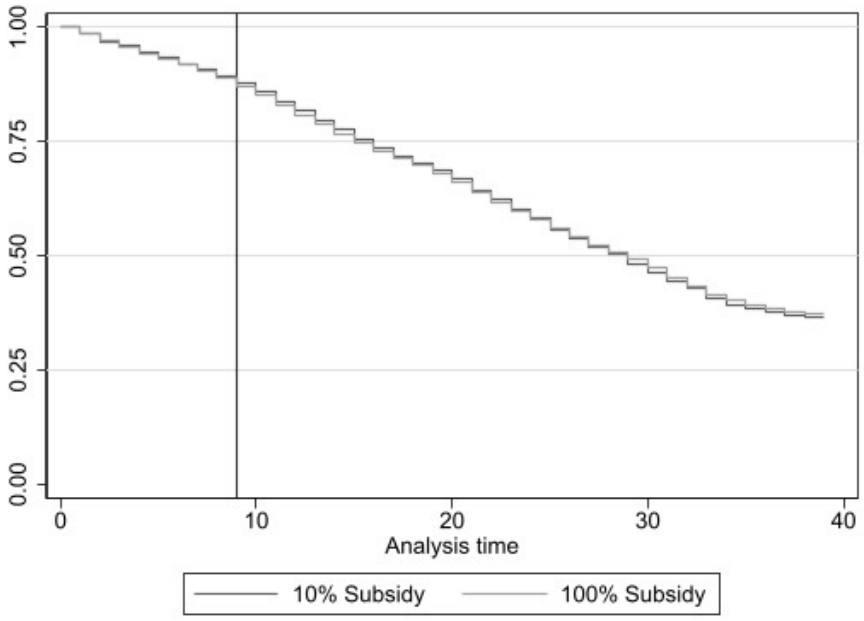
Figure 1: Experimental design



Notes: The figure presents the experimental design. There are three levels of randomization. (1) Across health centers: 50 centers (encompassing 250 villages, shown in gray) are assigned to the free contraception arm. (2) Across villages, within each health center catchment area: 300 villages are assigned to group interventions (100 per arm: debates, debates followed by mortality information (debates with facts) and group edutainment film) and 200 villages are assigned to individual interventions (100 per arm: low saturation and medium saturation). (3) Across households, within each village allocated to individual interventions: 15% of households are assigned to the mortality information arm, 15% are assigned to the individual edutainment arm and 70% are pure control in the low saturation arm. In the medium saturation arm, these rates are 30%, 30% and 40%, respectively.

Figure 2: Duration until next birth, by subsidy status



The figure shows a Kaplan-Meier Survival curve for the 10% subsidy group and the 100% subsidy group. The y-axis shows the fraction of people who have not given birth, starting from the date of the baseline survey (t=0) and up to 36 months later (endline survey). The intervention could not have affected fertility in the first 9 months (area left of the vertical line). A test that the survival curves are identical cannot reject the null (p-value = 0.55), thus the fertility behavior post intervention is the same regardless of the contraception subsidy received.

Table 1: Summary statistics from baseline survey: wives

	Mean	SD	N
Wife's age	28.24	5.45	14,607
Husband's age	38.26	11.54	14,051
Wife reports husband is polygamous	0.45	0.50	14,609
Muslim	0.64	0.48	14,597
Wife has no formal education	0.83	0.37	14,605
HH has access to electricity network	0.01	0.12	14,607
HH has a radio	0.48	0.50	14,603
HH has a toilet	0.43	0.49	10,022
HH has a cement/tiled floor	0.47	0.50	9,920
Fertility:			
# of pregnancies before baseline	3.51	1.84	12,543
Wants another child	0.92	0.27	14,609
Wants another child in next 2 years	0.35	0.48	13,931
Total # of children desired	6.00	1.87	13,212
Exposure to contraception:			
Ever heard of contraception/ methods to delay births	0.91	0.28	14,602
Ever used modern contraception	0.46	0.50	14,595
Currently using modern contraception	0.31	0.46	14,590
Distance to closest local health center (kilometres)	5.39	3.98	14,609
Has unmet need for contraception	0.38	0.49	14,596
Could not afford contraception if ever wanted to use it	0.41	0.49	13,240
Personal views (first-order beliefs):			
Agrees: modern contraception is not dangerous to health	0.79	0.41	13,011
Agrees: modern contraception is not against tradition	0.80	0.40	13,051
Agrees: modern contraception is a reliable way to control births	0.93	0.25	12,301
Agrees: there is a quantity-quality tradeoff	0.83	0.37	12,207
Perceived social norms (second-order beliefs):			
Agrees: times are changing and there is no social norm on family size	0.87	0.33	12,358
Agrees: community disapproves couple using contraception to delay 1st birth	0.65	0.48	14,609
Reports women sometimes punished/stigmatized for using contraception	0.39	0.49	13,353
Child mortality:			
Has had at least one child who died	0.28	0.45	14,353
True under 5 mortality rate (%)	10.52	2.09	14,609
Wife's perceived under 5 mortality rate (%)	19.81	19.47	11,271
Wife overestimates under 5 child mortality	0.55	0.50	11,271

Notes: Data from Baseline survey with wives. As standard in the literature, a woman is considered as having unmet needs for contraception if (i) she is not currently using contraception and (ii) she does not want another child in the next two years.

Table 2: Treatment effects on primary outcomes

	(1) Had a live birth since baseline	(2) Used medical contraception in last 3 yrs	(3) Month(s) used modern contraception (last spell of each type) in last 3 years	(4) Used IPA subsidy voucher
Panel A: with only endline controls				
Full Subsidy	-0.017	0.000	-0.318	0.032***
	(0.012)	(0.015)	(0.399)	(0.010)
Baseline Controls	No	No	No	No
Panel B: with endline and baseline controls				
Full Subsidy	-0.017	0.008	-0.194	0.035***
	(0.011)	(0.013)	(0.360)	(0.009)
Baseline Controls	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Observations	12,542	12,131	12,107	12,519
Control (10% Subsidy) Mean	0.623	0.531	9.609	0.142

Notes: Endline controls are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. Panel B additionally has the following baseline controls: number of births at baseline; whether wife was using modern contraception at baseline; number of children desired by wife at baseline ; number of children desired by husband at baseline; under 5 mortality rate reported by wife at baseline; wife and husband's first-order beliefs at baseline i.e. whether or not they each agree that "there is a quantity-quality trade-off", "times are changing and there is no social norm on family size" and (in response to a vignette) "Z. should use long-lasting contraception to delay 5th birth"; wife's age at baseline, spousal age gap at baseline, polygamous union at baseline, whether husband was surveyed at baseline, whether the wife has ever gone to school, whether she has had a child who died at baseline, exposure to DMI radio programs at baseline. Province fixed effects are used in all specifications. Robust standard errors in parentheses. Clustering at health center level. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

Table 3: Treatment effects on primary outcomes by subsamples

Sub-sample		(1) Had a live birth since baseline	(2) Used medical contracep-tion in last 3 yrs	(3) Month(s) used modern contraception (last spell of each type) in last 3 yrs	(4) Used IPA subsidy voucher
Need for Contraception:					
Had unmet need for contraception at baseline (N=4,649)	Full Subsidy	-0.011	0.018	-0.104	0.042***
		(0.015)	(0.019)	(0.459)	(0.014)
	Anderson's q-value	0.602	0.576	0.864	0.031
	Control Mean	0.662	0.437	7.229	0.128
Wife did not want another child over next 2 yrs at baseline (N=7,583)	Full Subsidy	-0.016	0.009	-0.392	0.037***
		(0.013)	(0.017)	(0.462)	(0.013)
	Anderson's q-value	0.444	0.720	0.576	0.031
	Control Mean	0.611	0.559	10.649	0.157
Husband did not want another child over next 2 yrs at baseline (N=4,724)	Full Subsidy	-0.013	0.022	0.159	0.033**
		(0.016)	(0.019)	(0.514)	(0.013)
	Anderson's q-value	0.576	0.503	0.842	0.054
	Control Mean	0.612	0.560	10.594	0.155
Was not using modern contraception at baseline (N=8,191)	Full Subsidy	-0.011	-0.002	-0.303	0.029***
		(0.012)	(0.014)	(0.352)	(0.010)
	Anderson's q-value	0.576	0.869	0.576	0.031
	Control Mean	0.641	0.421	6.854	0.113
Could not afford contraception at baseline (N=4,519)	Full Subsidy	-0.025	0.008	-0.099	0.037***
		(0.017)	(0.017)	(0.497)	(0.013)
	Anderson's q-value	0.326	0.759	0.864	0.031
	Control Mean	0.616	0.505	9.055	0.148
Social Norms:					
Does not believe community disapproves use of contraception (N=4,277)	Full Subsidy	-0.027*	-0.005	-0.534	0.043***
		(0.014)	(0.019)	(0.509)	(0.013)
	Anderson's q-value	0.205	0.853	0.540	0.021
	Control Mean	0.627	0.531	9.600	0.150
Does not report women being punished or stigmatized for using contraception (N=6,855)	Full Subsidy	-0.027*	-0.013	-0.609	0.030**
		(0.015)	(0.016)	(0.464)	(0.012)
	Anderson's q-value	0.207	0.586	0.427	0.05
	Control Mean	0.634	0.559	10.239	0.155
Mortality Perceptions:					
Does not overestimate under-5 child mortality (N=4,227)	Full Subsidy	-0.016	0.031	0.411	0.034***
		(0.016)	(0.019)	(0.529)	(0.012)
	Anderson's q-value	0.576	0.273	0.586	0.034
	Control Mean	0.628	0.527	9.473	0.148
Other Frictions:					
Health center <= 2km away (N=2,241)	Full Subsidy	-0.032	0.064**	1.493*	0.068***
		(0.025)	(0.029)	(0.831)	(0.019)
	Anderson's q-value	0.428	0.118	0.219	0.021
	Control Mean	0.592	0.523	9.315	0.151
Disagrees with modern contraception being harmful for health (N=8,563)	Full Subsidy	-0.007	-0.007	-0.641	0.036***
		(0.013)	(0.016)	(0.427)	(0.011)
	Anderson's q-value	0.701	0.767	0.326	0.021
	Control Mean	0.617	0.573	10.633	0.159

Notes: Endline controls are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. Province fixed effects are used in all specifications. Robust standard errors in parentheses. Clustering at the health center level. Control Mean refers to that of 10% subsidy sub-sample. N refers to the number of observations in each subsample. The number of observations may vary slightly between outcomes; in this case, we report the minimum number across all 4 regressions. Anderson's q-value refers to the adjustment for Multiple Hypothesis Testing in Anderson (2008). $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$

Table 4: Treatment effects on primary outcomes: fully interacted

	(1) Had a live birth since baseline	(2) Used medical contraception in last 3 yrs	(3) Month(s) used modern contraception (last spell of each type) in last 3 years	(4) Used IPA subsidy voucher
Full Subsidy	-0.036*	0.006	-0.280	0.047***
	(0.019)	(0.023)	(0.542)	(0.017)
Village Debate or Edutainment	-0.010	0.012	0.291	0.026**
	(0.017)	(0.019)	(0.484)	(0.012)
Individual Edutainment	0.006	-0.006	0.393	0.013
	(0.022)	(0.021)	(0.627)	(0.015)
Individual Mortality Info	0.010	0.025	0.886	-0.015
	(0.025)	(0.022)	(0.665)	(0.014)
Village Interventions X Full Subsidy	0.023	-0.011	0.117	-0.027
	(0.023)	(0.029)	(0.691)	(0.021)
Individual Edutainment X Full Subsidy	0.031	0.030	-0.285	-0.024
	(0.031)	(0.031)	(0.831)	(0.025)
Individual Mortality Info X Full Subsidy	0.027	-0.025	-0.728	0.017
	(0.034)	(0.033)	(0.842)	(0.022)
Province FE	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No
Observations	12,542	12,131	12,107	12,519
Control Mean	0.628	0.526	9.373	0.129

Notes: Endline controls are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. Province fixed effects are used in all specifications. Robust standard errors in parentheses. Clustering at the village level. Control mean refers to that of the pure control. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table 5: Treatment effects: other potential benefits to the wife

	(1) Emotional Violence Index (12m)	(2) Physical Violence Index (12m)	(3) Sexual Violence Index (12m)	(4) IPV Index (past 12m)	(5) Self reported health today is very good	(6) Agrees to statement that life is good	(7) Husband polygamous at endline
Full Subsidy	-0.043	-0.016	-0.057**	-0.040**	0.037	-0.026	-0.001
	(0.036)	(0.028)	(0.027)	(0.020)	(0.027)	(0.023)	(0.009)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No	No	No	Yes
Observations	8,399	8,327	8,400	8,402	12,527	12,499	11,774
Control (10% Subsidy) Mean	0.001	0.000	0.000	0.009	0.395	0.759	0.430

Notes: The IPV module was only administered to women surveyed in person, since we couldn't guarantee confidentiality for women surveyed by phone. Endline controls are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. Province fixed effects are used in all specifications. Robust standard errors in parentheses. Clustering at the health center level. Column 7 additionally controls for husband's polygamous status at baseline. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$

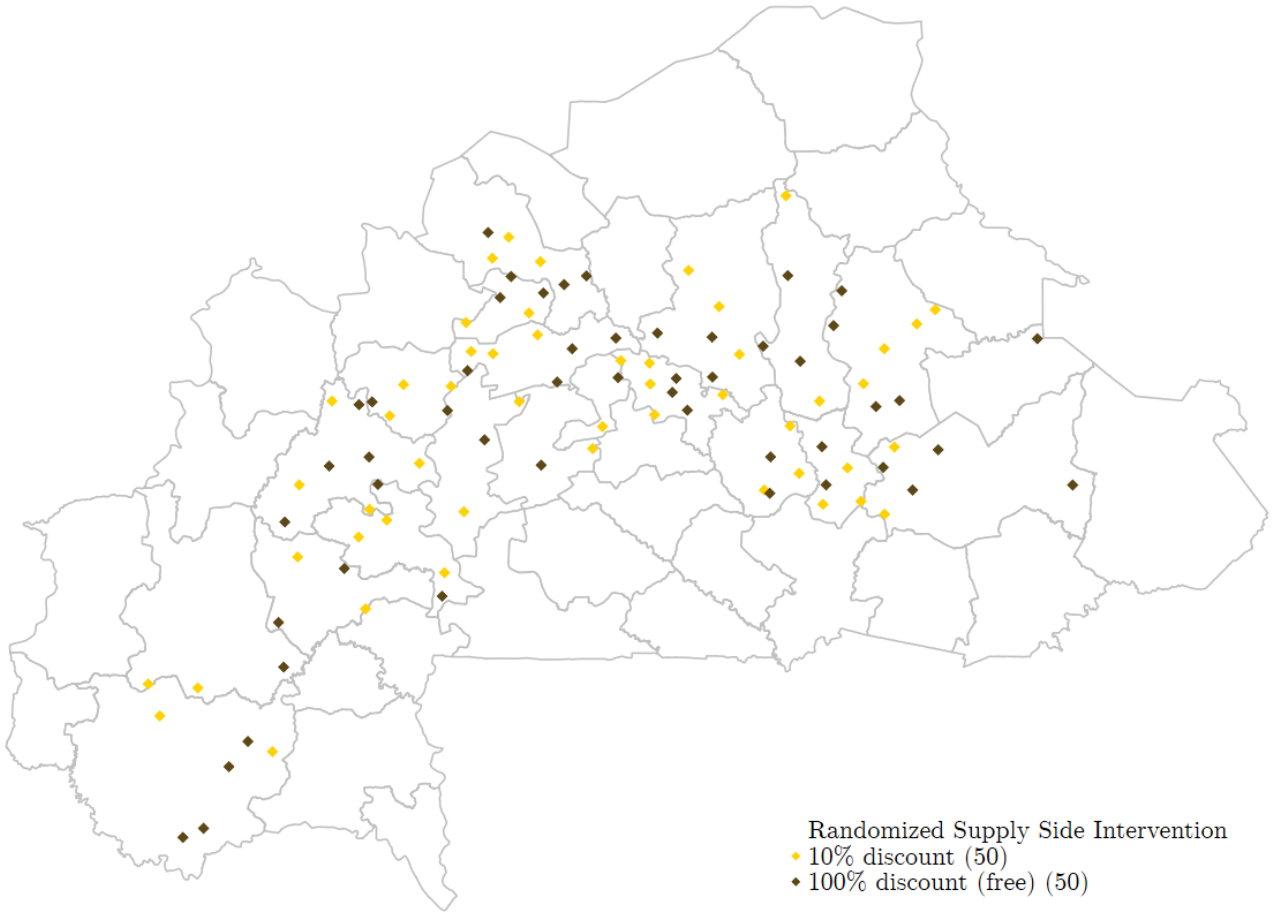
Table 6: High returns to quantity

Statistic	Mean	N	Mean	N	Source
	Husband		Wife		
Will not be able to cope in old age without support of child	0.74	206	0.81	226	Scoping
Share of children expected to send enough money back to sustain parents	0.20	252	0.26	277	Scoping
Labor constrained to expand farm activity	0.39	67523			Listing
Associate lack of children with: No labour for land	0.10	8567	0.13	12424	Baseline
Associate lack of children with: No labour for chores	0.07	8567	0.17	12424	Baseline
Associate lack of children with: Unhappiness	0.07	8567	0.14	12424	Baseline

Notes: Scoping visits were conducted between September 2016 and March 2017. We conducted semi-qualitative interviews with married men and women of reproductive age across 8 regions. The listing exercise took place in Fall 2017 in 500 villages. The baseline took place in Spring 2018 in 499 villages.

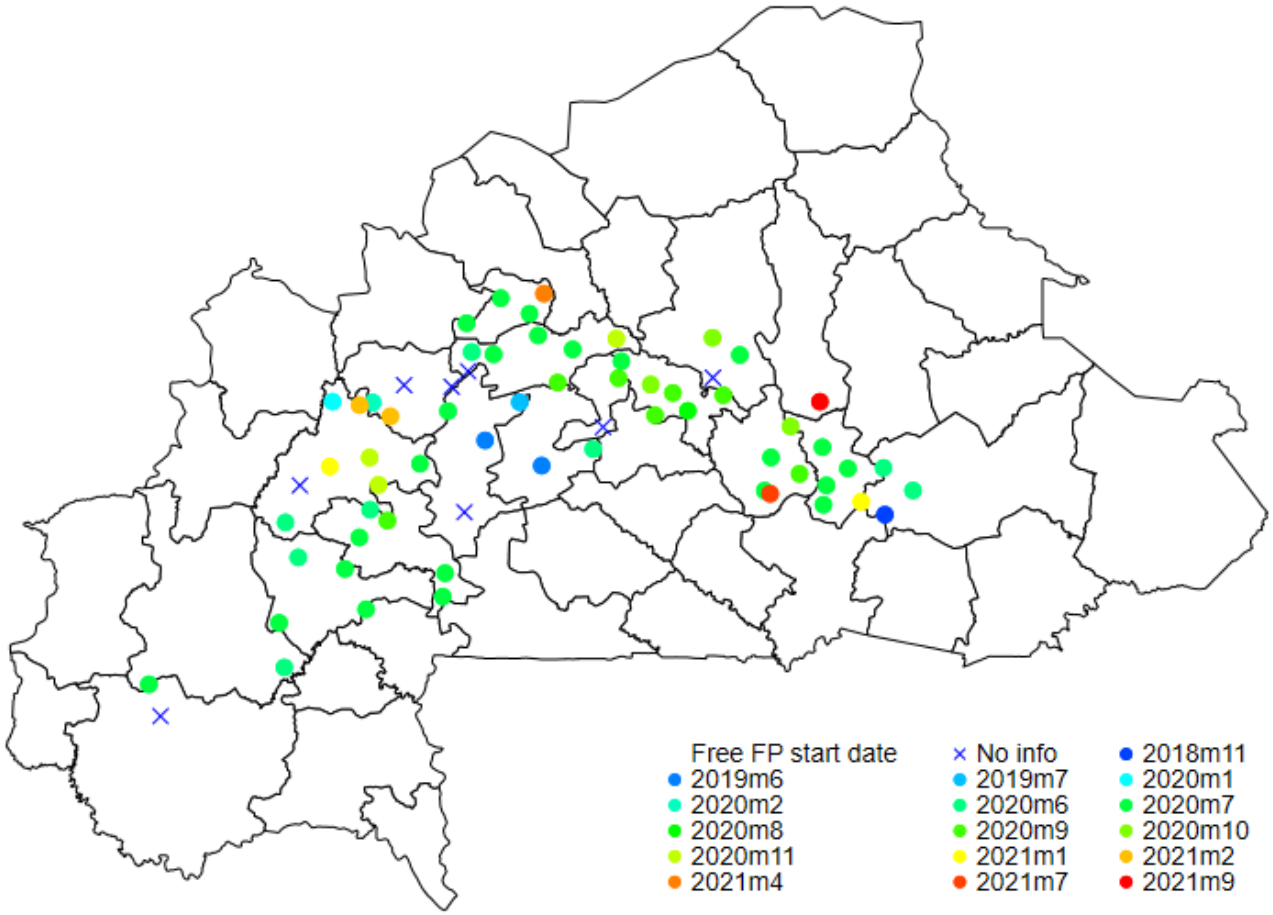
Appendix

Figure A.1: Geographical distribution of sample and randomized subsidy treatment



Note: Each dot represents a health center. N=100. The inner borders drawn correspond to the 45 administrative provinces.

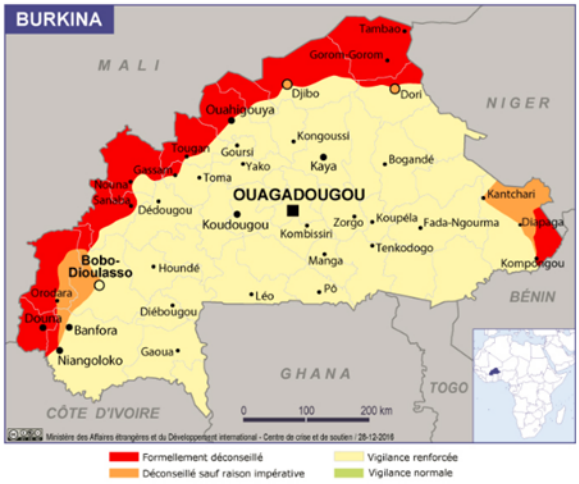
Figure A.2: Start dates of Free Family Planning Program as reported by CSPS



Note: The inner borders drawn correspond to the 45 administrative provinces. Officially, the program was piloted in July 2019 in two regions (Cascades and Centre Ouest) and scaled up in July 2020. The map reports the actual date when the program started being implemented in health care centers included in the sample. The data was collected through in-person visits conducted at endline. The information is missing for facilities located in areas surveyed by phone; they are not shown on the map. The baseline and interventions took place between February and June 2018 and the endline took place between February and June 2021. See Table A.6 (bottom row) for the results excluding the regions where the national free family planning program was piloted in 2019.

Figure A.3: Gradual worsening of security situation in Burkina Faso over the study period

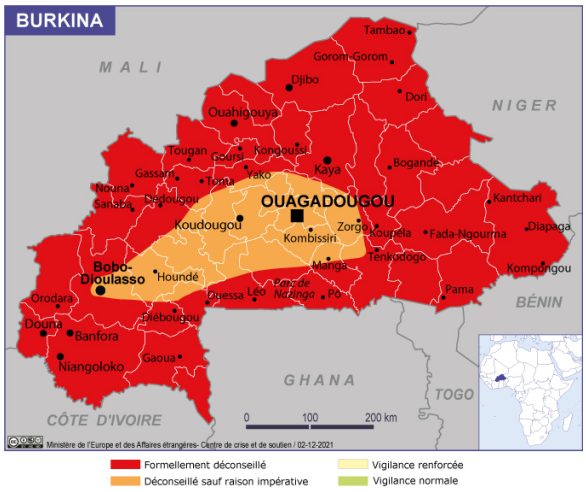
(a) June 2017



(b) July 2019

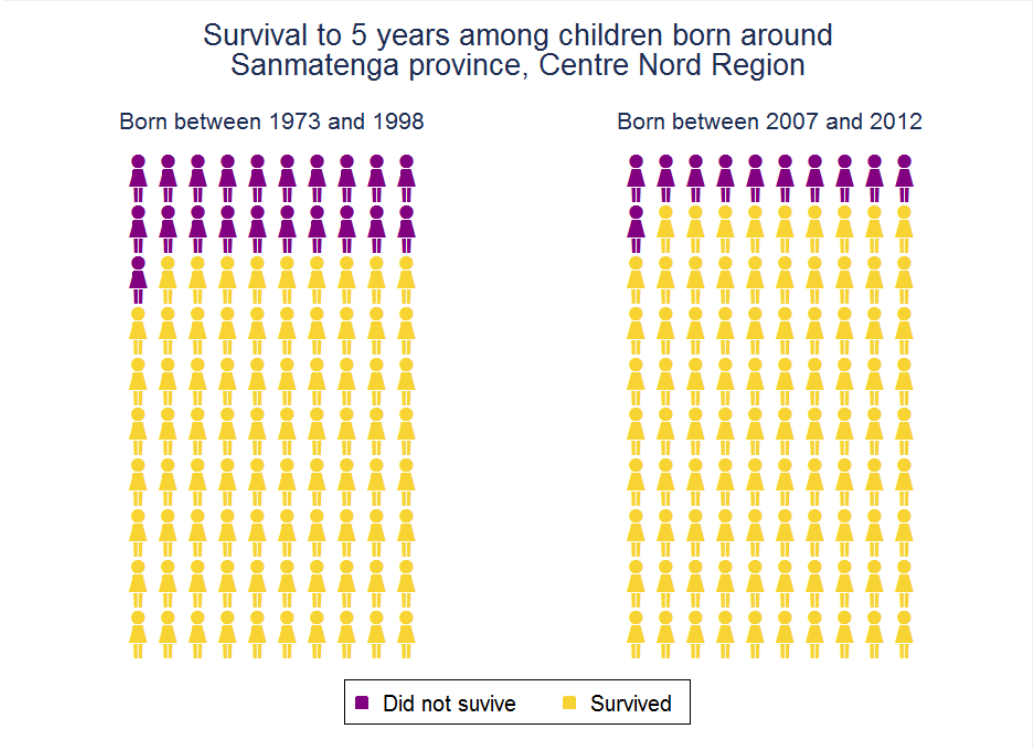
(c) December 2020

(d) December 2021



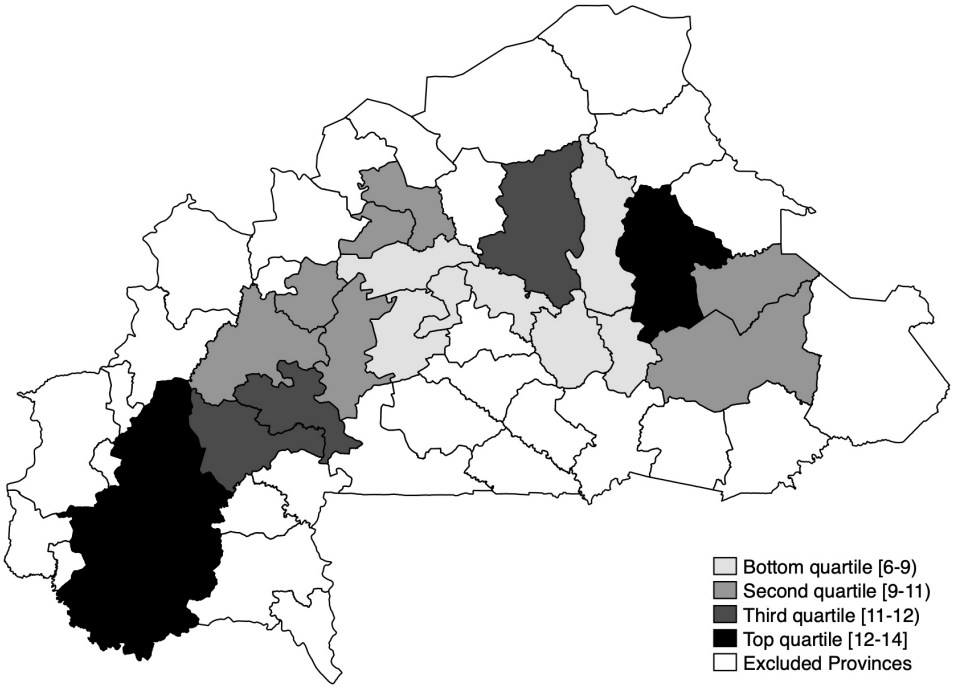
Notes: Maps published by the French embassy in Burkina Faso. Red corresponds to areas where the recommendation is "No travel". Orange corresponds to areas where travel can be considered only under special circumstances. Source: https://www.diplomatie.gouv.fr/fr/conseils-aux-voyageurs/conseils-par-pays-destination/burkina-faso/#securite

Figure A.4: Area-specific infographic used for mortality information treatment

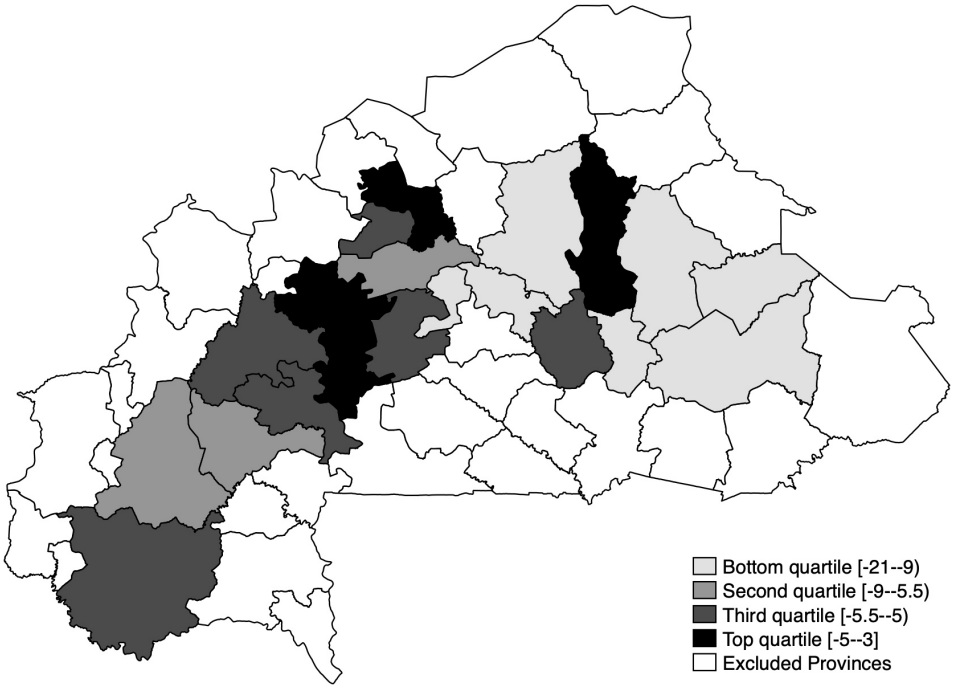


Notes: We created one such graphic for each of 20 provinces based on data from over 190,000 births collected during the listing survey. See section 2.2.3 for details.

Figure A.5: Under 5 mortality rates by province: levels and trends



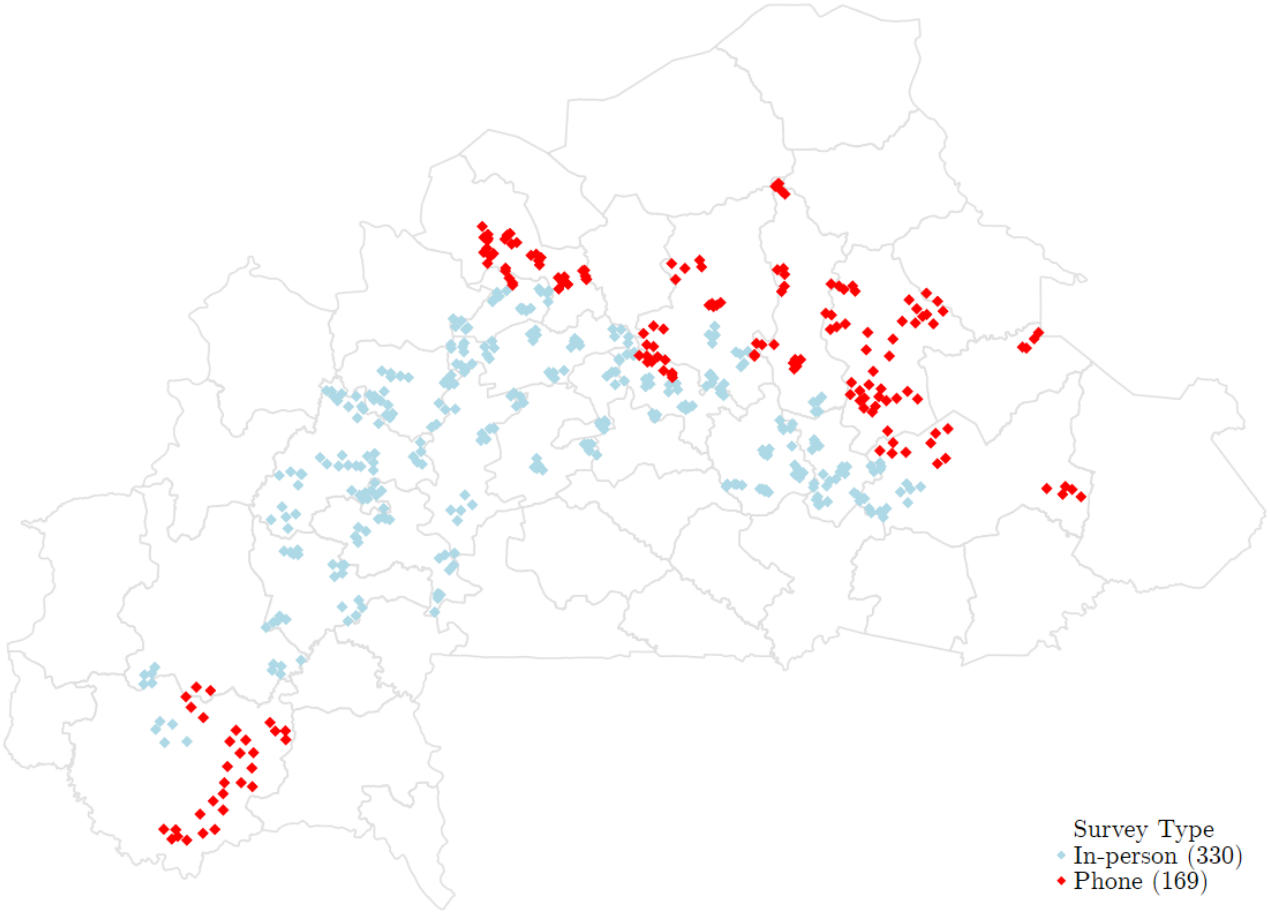
(a) Recent levels



(b) Recent trends

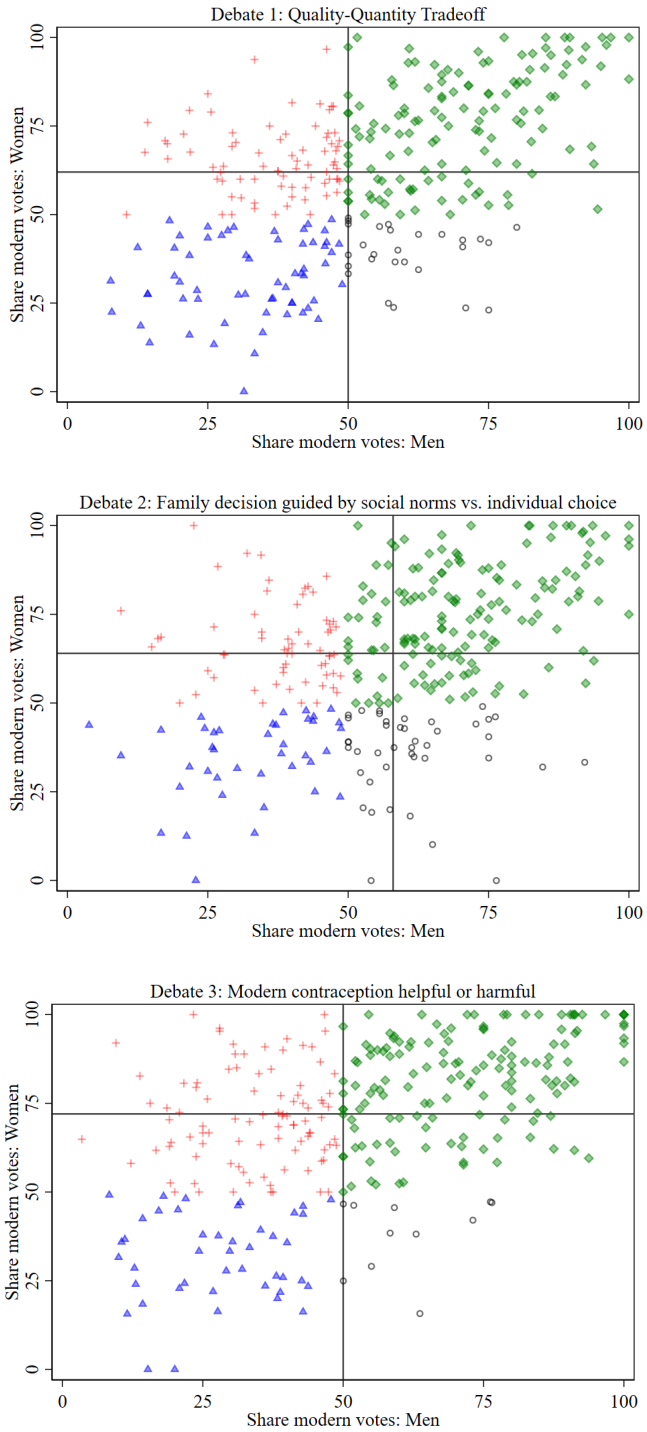
Notes: Source: Listing data. Graph (a) shows the estimated under 5 mortality rates for cohorts born between 2007 and 2012, by province. Darker colors indicate higher rates. Graph (b) shows the estimated change in under 5 mortality rates between cohorts born between 2007 and 2012 and cohorts born between 1973 and 1998, by province. Darker colors indicate smaller declines.

Figure A.6: Location of in-person vs. phone survey at endline



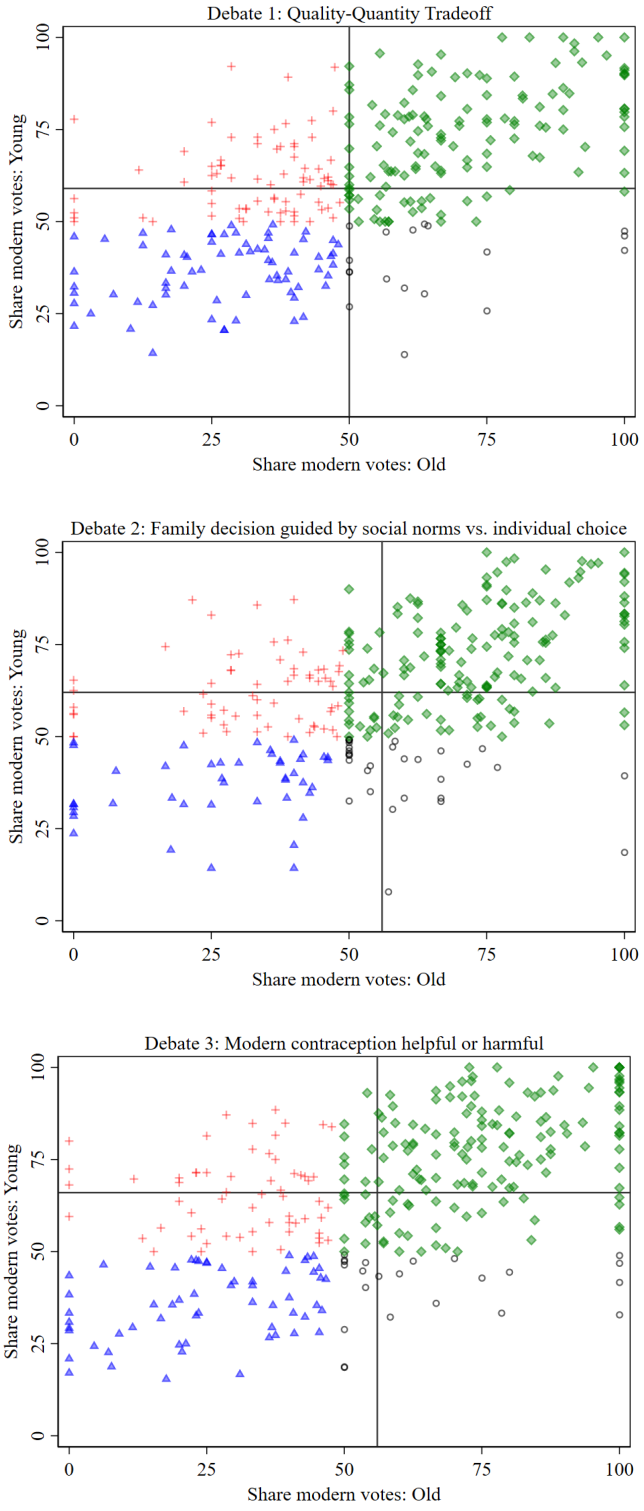
Note: Each dot represents a village. N=499. The inner borders drawn correspond to the 45 administrative provinces. All respondents in all villages assigned to a given health center were surveyed using the same mode. The decision of which village could not be surveyed in person due to security concerns was made by the Burkina Faso office of Innovations for Poverty Action.

Figure A.7: Votes after village debates/edutainment: gender gaps



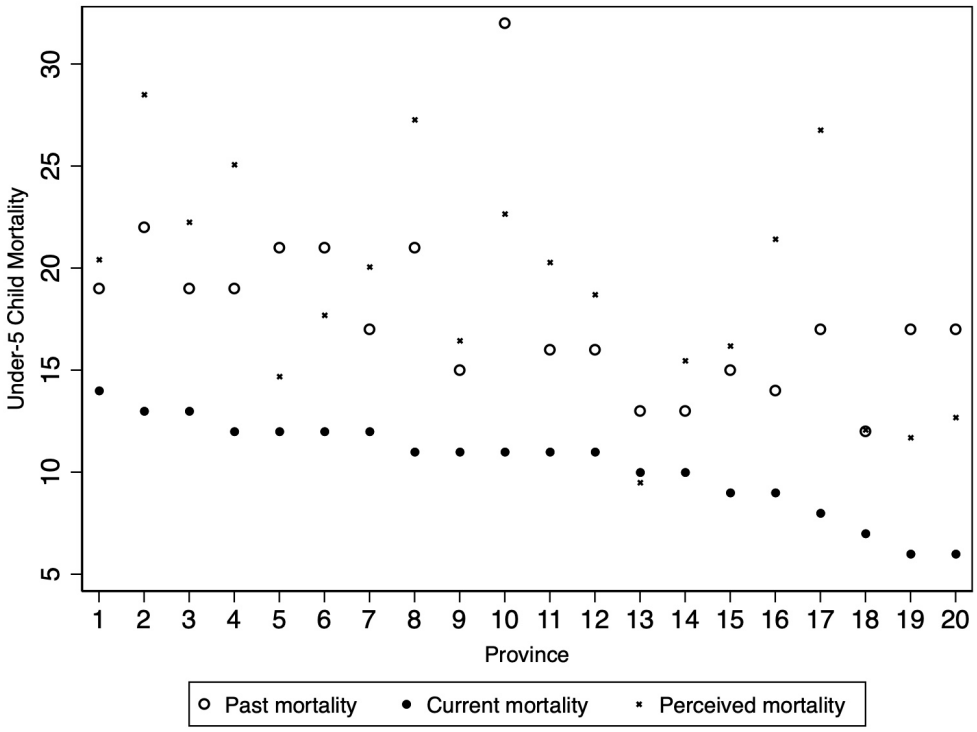
Notes: Data Source: Records kept by field team implementing the village intervention visits. An observation is a village. There are 197 villages with Debates and 100 villages with Edutainment. Black lines show median across all villages for each subgroup. In villages represented by red plus signs, the majority of women vote in favor whereas the majority of men vote against (N = 76, 62 and 94 in debates 1, 2 and 3, respectively).

Figure A.8: Votes after village debates/edutainment: inter-generational gaps



Notes: Data Source: Records kept by field team implementing the village intervention visits. An observation is a village. There are 197 villages with Debates and 100 villages with Edutainment. Black lines show median across all villages for each subgroup. In villages represented by red plus signs, the majority of younger people vote in favor whereas the majority of older people vote against (N = 75, 63 and 61 in debates 1, 2 and 3, respectively).

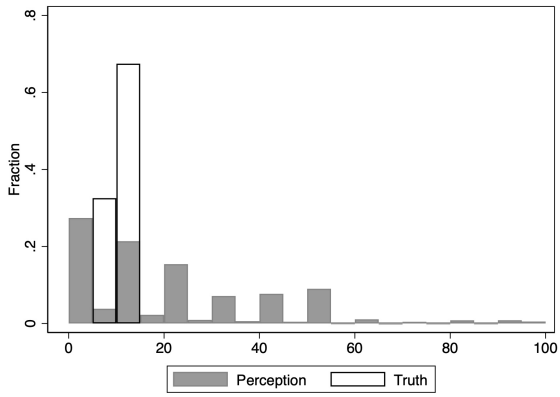
Figure A.9: Baseline mis-perceptions of the child mortality rate



Notes: We compare the average perceived rates reported by women surveyed at baseline (shown with crosses, N=11,298) with the observed rates measured during the listing, by province. The solid dots represent the most recent cohorts, born between 2007 and 2012. The hollow dots represent older cohorts, born between 1973 and 1998. Provinces are ordered by recent mortality rates.

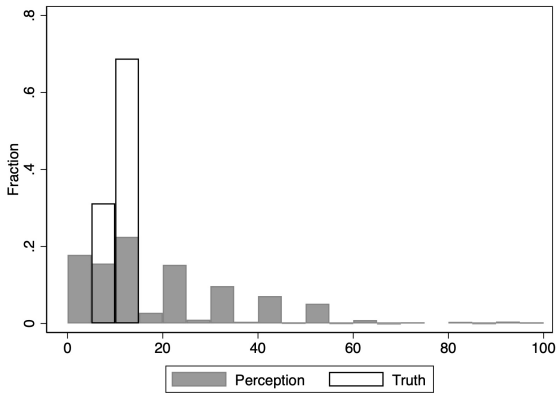
Figure A.10: Distribution of perceived vs actual mortality at baseline

(a) Women



Baseline survey with focal wives. N: 11,298.
Perceived rate: average= 19.8; median= 10

(b) Men

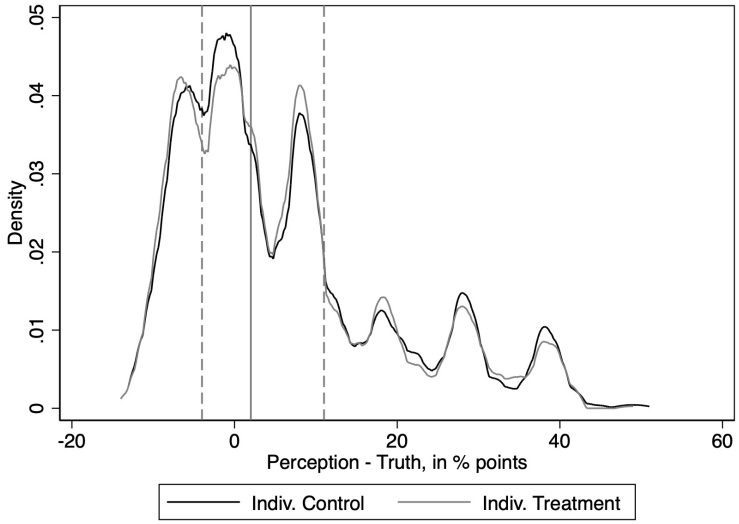


Baseline survey with husbands. N: 9,797.
Perceived rate: average= 17.6; median= 10

Notes: The white bars show the distribution of the actual mortality rate measured during the listing for children born in 2007-2012 (average= 10.5; median= 10). The grey bars show the baseline distribution of the perceived rate reported by women in graph (a) and by men in graph (b). The bins are as follows: [0,4], [5,9], [10,14] ... [90,94] and [95,100]. Rates are expressed in percentage points.

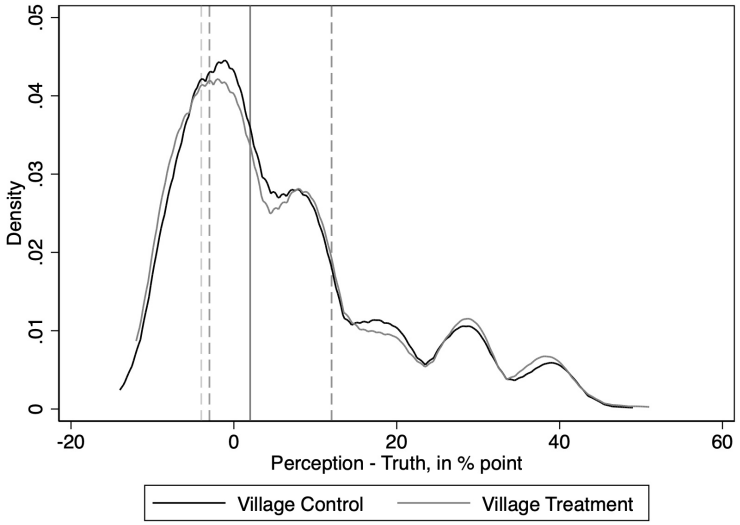
Figure A.11: Distribution of the gap between perceived and actual mortality at endline

(a) Individual Mortality Info Treatment versus Control



Note: Treatment (N: 1290, average=5.9), Control: (N : 3137, average=6.16), Pvalue = 0.93

(b) Village Mortality Info Treatment versus Control



Note: Treatment (N:2264, average=6.19), Control: (N : 2229, average=6.28), Pvalue = 0.48

Notes: The figure shows a Kernel estimate of the distribution of the gap between the perceived rate reported by women at endline and the actual, local mortality rate measured during the listing for children born in 2007-2012. Graph (a) restricts the sample to villages assigned to individual interventions and compares the distribution in the mortality information treatment arm (in gray) and the distribution in the pure control arm (in black). Graph (b) restricts the sample to villages assigned to debates and compares the distribution in the debate + mortality information treatment arm (in gray) and the distribution in the pure debate arm (in black). The solid vertical lines indicate the average in each group; the dashed vertical lines indicate the first and third quartiles in each group. Rates are expressed in percentage points.

Table A.1: Balancing tests in baseline survey (for the non-attrition sample)

	(1) Wants another child	(2) Total # of children desired	(3) Wants next child within 2 years	(4) Ever used modern contraception	(5) Currently using modern contraception
Full Subsidy	-0.002	0.059	0.007	-0.006	-0.014
	(0.005)	(0.066)	(0.012)	(0.015)	(0.014)
Province FE	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No	No
Observations	12,546	11,350	11,968	12,539	12,537
Control (10% Subsidy) Mean	0.924	5.940	0.349	0.469	0.324

Notes: All specifications include province fixed effects. Controls are: whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. Robust standard errors in parentheses. Clustering at health center level. The sample is restricted to individuals surveyed at endline (the analytic sample). $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$

Table A.2: Baseline visit outcomes (for the non-attrition sample)

	(1) Accepted voucher booklet	(2) Refused voucher because no need	(3) Refused voucher because scared husband/ someone finds out	(4) Husband could be surveyed
Full Subsidy	0.043**	-0.020	-0.008	-0.009
	(0.021)	(0.015)	(0.006)	(0.018)
Province FE	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No
Observations	12,546	12,529	12,529	12,546
Control (10% Subsidy) Mean	0.679	0.145	0.056	0.746

Notes: All specifications include province fixed effects. Controls are: whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. Robust standard errors in parentheses. Clustering at health center level. The sample is restricted to individuals surveyed at endline (the analytic sample). $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$

Table A.3: Attrition at endline

	(1) Focal wife surveyed	(2) Husband surveyed
Full Subsidy	-0.016	-0.012
	(0.016)	(0.016)
Baseline Controls	No	No
Observations	14,609	14,609
Control (10% Subsidy) Mean	0.874	0.884

Notes: All specifications include Province fixed effects. Robust standard errors in parentheses. Clustering at health center level. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$

Table A.4: Summary statistics from baseline survey: husbands

	Mean	SD	N
Husband's age	40.24	11.37	10,781
Focal wife's age	28.58	5.42	10,782
Wife reports husband is polygamous	0.48	0.50	10,784
Husband has no formal education	0.81	0.40	10,784
Muslim	0.64	0.48	10,778
Fertility:			
# of children (from all wives)	6.06	4.91	8,567
Wants another child	0.92	0.27	10,437
Wants another child in next 2 years	0.44	0.50	9,734
Total # of children desired	9.48	6.98	8,527
Exposure to contraception:			
Ever heard of contraception/methods to delay births	0.89	0.31	10,778
Ever used modern contraception	0.40	0.49	10,504
Currently using modern contraception	0.31	0.46	10,178
Distance to closest local health center (kilometres)	5.42	4.06	10,784
Personal views (first-order beliefs):			
Agrees: modern contraception is not dangerous to health	0.74	0.44	9,303
Agrees: modern contraception is not against tradition	0.78	0.41	9,346
Agrees: modern contraception is a reliable way to control births	0.90	0.31	8,056
Agrees: there is a quantity-quality tradeoff	0.80	0.40	8,097
Perceived social norms (second-order beliefs):			
Agrees: times are changing and there is no social norm on family size	0.86	0.35	8,149
Agrees: community disapproves use of contraception to delay 1st birth	0.61	0.49	10,784
Reports women sometimes punished/stigmatized for using contraception	0.37	0.48	9,636
Child mortality:			
True under 5 mortality rate (%)	10.56	2.12	10,784
Husband's percieved under 5 mortality rate (%)	17.63	16.73	9,651
Husband overestimates under-5 mortality	0.52	0.50	9,651

Notes: Data from Baseline survey with husbands.

Table A.5: Treatment effects on primary outcomes by subsamples (with baseline controls)

Sub-sample		(1) Had a live birth since baseline	(2) Used medical contracep-tion in last 3 yrs	(3) Month(s) used modern contraception (last spell of each type) in last 3 yrs	(4) Used IPA subsidy voucher
Need for Contraception:					
Had unmet need for contraception at baseline (N=4,649)	Full Subsidy	-0.011	0.022	-0.062	0.043***
		(0.014)	(0.018)	(0.475)	(0.014)
	Control Mean	0.662	0.437	7.229	0.128
Wife did not want another child over next 2 yrs at baseline (N=7,583)	Full Subsidy	-0.019	0.020	-0.166	0.040***
		(0.013)	(0.015)	(0.428)	(0.012)
	Control Mean	0.611	0.559	10.649	0.157
Husband did not want another child over next 2 yrs at baseline (N=4,724)	Full Subsidy	-0.014	0.031*	0.311	0.036***
		(0.016)	(0.017)	(0.448)	(0.012)
	Control Mean	0.612	0.560	10.594	0.155
Was not using modern contraception at baseline (N=8,191)	Full Subsidy	-0.010	0.000	-0.283	0.030***
		(0.012)	(0.014)	(0.362)	(0.009)
	Control Mean	0.641	0.421	6.854	0.113
Could not afford contraception at baseline (N=4,519)	Full Subsidy	-0.027*	0.014	-0.009	0.039***
		(0.016)	(0.016)	(0.498)	(0.013)
	Control Mean	0.616	0.505	9.055	0.148
Social Norms:					
Does not believe community disapproves use of contraception (N=4,277)	Full Subsidy	-0.020	0.003	-0.568	0.044***
		(0.014)	(0.016)	(0.477)	(0.013)
	Control Mean	0.627	0.531	9.600	0.150
Does not report women being punished or stigmatized for using contraception (N=6,855)	Full Subsidy	-0.023	-0.005	-0.531	0.035***
		(0.014)	(0.014)	(0.426)	(0.011)
	Control Mean	0.634	0.559	10.239	0.155
Mortality Perceptions:					
Does not overestimate under-5 child mortality (N=4,227)	Full Subsidy	-0.012	0.034**	0.433	0.035***
		(0.016)	(0.015)	(0.451)	(0.012)
	Control Mean	0.628	0.527	9.473	0.148
Other Frictions:					
Health center <= 2km away (N=2,241)	Full Subsidy	-0.038	0.069**	1.424**	0.066***
		(0.025)	(0.026)	(0.670)	(0.018)
	Control Mean	0.592	0.523	9.315	0.151
Disagrees with modern contraception being harmful for health (N=8,563)	Full Subsidy	-0.008	0.002	-0.481	0.038***
		(0.012)	(0.014)	(0.391)	(0.010)
	Control Mean	0.617	0.573	10.633	0.159

Notes: Endline controls are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. Baseline controls are: number of births at baseline; whether wife was using modern contraception at baseline; number of children desired by wife at baseline ; number of children desired by husband at baseline; under 5 mortality rate reported by wife at baseline; wife and husband's first order beliefs at baseline i.e. whether or not they each agree that "there is a quantity-quality trade-off", "times are changing and there is no social norm on family size" and (in response to a vignette) "Z. should use long-lasting contraception to delay 5th birth"; wife's age at baseline, spousal age gap at baseline, polygamous union at baseline, whether husband was surveyed at baseline, whether the wife has ever gone to school, whether she has had a child who died at baseline, exposure to DMI radio programs at baseline. Province fixed effects are used in all specifications. Robust standard errors in parentheses. Clustering at the health center level. Control Mean refers to that of 10% subsidy sub-sample. N refers to the number of observations in each subsample. The number of observations may vary slightly between outcomes; in this case, we report the minimum number across all 4 regressions. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$

Table A.6: Treatment effects on primary outcomes: other subsamples

Sub-sample		(1) Had a live birth since baseline	(2) Used medical contraception in last 3 yrs	(3) Month(s) used modern contraception (last spell of each type) in last 3 yrs	(4) Used IPA subsidy voucher
Young wives (<=median age)	Full Subsidy	-0.020	0.016	-0.266	0.021*
		(0.015)	(0.016)	(0.474)	(0.012)
	Observations	6,290	6,066	6,056	6,277
	Control Mean	0.722	0.549	9.255	0.148
Older wives (>median age)	Full Subsidy	-0.011	-0.016	-0.419	0.044***
		(0.014)	(0.018)	(0.443)	(0.011)
	Observations	6,252	6,065	6,051	6,242
	Control Mean	0.523	0.512	9.962	0.136
Monogamous husband	Full Subsidy	-0.018	0.010	-0.077	0.031***
		(0.013)	(0.016)	(0.420)	(0.011)
	Observations	6,974	6,756	6,743	6,969
	Control Mean	0.660	0.556	9.870	0.149
Polygamous husband	Full Subsidy	-0.011	-0.010	-0.635	0.034***
		(0.014)	(0.018)	(0.481)	(0.012)
	Observations	5,568	5,375	5,364	5,550
	Control Mean	0.573	0.496	9.256	0.132
Senior wife in a polygamous marriage	Full Subsidy	-0.014	-0.012	-0.741	0.050***
		(0.025)	(0.029)	(0.675)	(0.019)
	Observations	1,728	1,669	1,666	1,721
	Control Mean	0.556	0.515	9.971	0.126
Junior wife in a polygamous marriage	Full Subsidy	-0.009	-0.011	-0.576	0.026**
		(0.016)	(0.019)	(0.532)	(0.013)
	Observations	3,840	3,706	3,698	3,829
	Control Mean	0.581	0.487	8.937	0.134
Low nb of pregnancies at baseline	Full Subsidy	-0.017	0.005	-0.244	0.022*
		(0.014)	(0.017)	(0.458)	(0.012)
	Observations	6,479	6,236	6,225	6,467
	Control Mean	0.709	0.518	8.616	0.138
High nb of pregnancies at baseline	Full Subsidy	-0.019	-0.005	-0.399	0.043***
		(0.015)	(0.017)	(0.430)	(0.012)
	Observations	6,063	5,895	5,882	6,052
	Control Mean	0.532	0.544	10.653	0.146
Neither wanted another child over next 2 years	Full Subsidy	-0.010	0.025	0.036	0.035**
		(0.018)	(0.021)	(0.536)	(0.015)
	Observations	3,908	3,778	3,771	3,903
	Control Mean	0.599	0.567	11.044	0.163
Was using modern contraception at baseline	Full Subsidy	-0.028	0.014	-0.267	0.041***
		(0.018)	(0.019)	(0.609)	(0.014)
	Observations	4,043	3,911	3,907	4,037
	Control Mean	0.587	0.752	15.159	0.200
Excluding regions with Family Planning pilot (2019)	Full Subsidy	-0.020	-0.003	-0.372	0.032***
		(0.012)	(0.016)	(0.424)	(0.010)
	Observations	11,378	11,014	10,991	11,357
	Control Mean	0.623	0.548	9.936	0.147

Notes: Endline controls are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. Province fixed effects are used in all specifications. Robust standard errors in parentheses. Clustering at health center level. Control Mean refers to that of 10% subsidy sub-sample. Province fixed effects used across all specifications. The median age of wives is 28. The median number of births before baseline and sons desired by wife at baseline is 3. The regions with family planning pilot in 2019 are Cascades and Centre Ouest. $^{***}p < 0.01$,** $p < 0.05$,* $p < 0.1$

Table A.7: Treatment effects on primary outcomes: fully interacted (with baseline controls)

	(1) Had a live birth since baseline	(2) Used medical contraception in last 3 yrs	(3) Month(s) used modern contraception (last spell of each type) in last 3 years	(4) Used IPA subsidy voucher
Full Subsidy	-0.034*	0.013	-0.196	0.050***
	(0.018)	(0.021)	(0.475)	(0.016)
Village Debate or Edutainment	-0.015	0.014	0.293	0.034**
	(0.016)	(0.018)	(0.455)	(0.013)
Individual Edutainment	0.009	0.000	0.480	0.016
	(0.021)	(0.021)	(0.623)	(0.016)
Individual Mortality Info	-0.000	0.021	0.800	-0.014
	(0.023)	(0.022)	(0.641)	(0.014)
Village Interventions X Full Subsidy	0.022	-0.007	0.236	-0.024
	(0.022)	(0.025)	(0.599)	(0.020)
Individual Edutainment X Full Subsidy	0.027	0.021	-0.406	-0.026
	(0.030)	(0.030)	(0.804)	(0.026)
Individual Mortality Info X Full Subsidy	0.025	-0.030	-0.800	0.015
	(0.033)	(0.032)	(0.829)	(0.022)
Province FE	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Observations	12,542	12,131	12,107	12,519
Control Mean	0.628	0.526	9.373	0.129

Notes: Province fixed effects are used in all specifications. Robust standard errors in parentheses. Clustering at the village level. Control mean refers to that of the pure control. Baseline controls are: number of births at baseline; whether wife was using modern contraception at baseline; number of children desired by wife at baseline ; number of children desired by husband at baseline ;under 5 mortality rate reported by wife at baseline; wife and husband's first order beliefs at baseline i.e. whether or not they each agree that "there is a quantity-quality trade-off", "times are changing and there is no social norm on family size" and (in response to a vignette) "Z. should use long-lasting contraception to delay 5th birth"; wife's age at baseline, spousal age gap at baseline, polygamous union at baseline, whether husband was surveyed at baseline, whether the wife has ever gone to school, whether she has had a child who died at baseline, exposure to DMI radio programs at baseline. Endline controls are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

Table A.8: Treatment effects of village interventions on wife's beliefs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First-order beliefs				Second-order beliefs			
	Modern contraception is not dangerous to health	Modern contraception is not against tradition	Modern contraception is a reliable way to control births	There is a quantity-quality tradeoff	Community disapproves couple using contraception to delay 1st birth	Women sometimes punished/ stigmatized for using contraception	Modern contraception is a reliable way to control births	There is a quantity-quality tradeoff
Panel A: Effect of village interventions								
Village Debate or Edutainment	0.001	-0.029***	-0.008	-0.007	0.010	0.011	0.000	0.002
	(0.010)	(0.011)	(0.006)	(0.010)	(0.015)	(0.013)	(0.004)	(0.004)
CSPS FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No	No	No	No	No
Observations	11,459	11,461	12,546	12,546	12,546	11,314	12,511	12,501
Control Mean	0.818	0.814	0.908	0.841	0.649	0.483	0.627	0.524
Panel B: Effect of village interventions by baseline beliefs								
Village Debate or Edutainment	-0.002	-0.008	-0.022	-0.014	0.001	0.008		
	(0.018)	(0.019)	(0.028)	(0.022)	(0.019)	(0.017)		
Agrees at baseline	0.051***	0.014	0.068***	0.073***	0.018	0.030**		
	(0.014)	(0.015)	(0.018)	(0.015)	(0.012)	(0.014)		
Village Int. X Agrees at baseline	0.004	-0.023	0.012	0.010	0.014	-0.001		
	(0.019)	(0.020)	(0.028)	(0.022)	(0.017)	(0.020)		
CSPS FE	Yes	Yes	Yes	Yes	Yes	Yes		
Baseline Controls	No	No	No	No	No	No		
Observations	10,331	10,358	10,588	10,498	12,546	10,464		
Control Mean	0.775	0.822	0.826	0.811	0.627	0.478		

Notes: Robust standard errors in parentheses. Clustering at village level. CSPS fixed effects used across all specifications. Endline controls used are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. In all regressions, we include other demand treatment arms (Individual Mortality Info and Individual Edutainmment) but we don't report the coefficients in the table. The control mean in Panel A is the average outcome in the group that did not receive any demand intervention. The control mean in Panel B is the average outcome in the group that did not receive any demand intervention and disagrees at baseline. Second order beliefs in column 7 and 8 refer to the proportion of people in the community that the respondents think agree with the outcome statement as opposed to columns 3 and 4 where the outcome measures whether the respondent herself agrees with the outcome statement. Panel B controls for whether the wife agreed with the outcome statement at baseline and interacts this with the village interventions. We cannot estimate the regression in columns 7 and 8 in panel B because we did not collect the relevant variable at baseline. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Table A.9: Treatment effects of village interventions on primary outcomes by baseline beliefs

Belief of wife		(1) Had a live birth since baseline	(2) Used medical contracep-tion in last 3 yrs	(3) Month(s) used modern contraception (last spell of each type) in last 3 yrs	(4) Used IPA subsidy voucher
Personal views (first-order beliefs):					
Modern contraception is not dangerous to health (N=10867)	Village Debate or Edutainment	-0.037*	0.009	0.295	-0.007
		(0.020)	(0.022)	(0.541)	(0.016)
	Agrees at baseline	-0.058***	0.077***	1.790***	0.025**
		(0.016)	(0.018)	(0.417)	(0.013)
	Village Interventions X Agrees at baseline	0.042*	-0.011	0.114	0.028*
		(0.022)	(0.023)	(0.584)	(0.016)
Modern contraception is not against tradition (N=10900)	Village Debate or Edutainment	-0.017	0.022	0.451	-0.003
		(0.021)	(0.022)	(0.558)	(0.017)
	Agrees at baseline	-0.018	0.030	0.376	-0.012
		(0.018)	(0.019)	(0.463)	(0.012)
	Village Interventions X Agrees at baseline	0.018	-0.027	-0.125	0.021
		(0.024)	(0.024)	(0.595)	(0.017)
Modern contraception is a reliable way to control births (N=10227)	Village Debate or Edutainment	0.022	-0.020	-0.916	0.033
		(0.038)	(0.038)	(0.911)	(0.022)
	Agrees at baseline	0.032	0.122***	1.213**	0.076***
		(0.025)	(0.025)	(0.609)	(0.014)
	Village Interventions X Agrees at baseline	-0.036	0.034	1.711*	-0.006
		(0.039)	(0.038)	(0.894)	(0.021)
There is a quantity-quality tradeoff (N=10138)	Village Debate or Edutainment	-0.033	0.013	0.751	0.013
		(0.025)	(0.027)	(0.639)	(0.020)
	Agrees at baseline	-0.016	0.084***	1.892***	0.017
		(0.018)	(0.019)	(0.442)	(0.014)
	Village Interventions X Agrees at baseline	0.028	0.003	-0.028	0.017
		(0.026)	(0.027)	(0.638)	(0.020)
Perceived social norms (second-order beliefs):					
There is no social norm on family size (N=10275)	Village Debate or Edutainment	0.005	-0.036	-0.600	0.013
		(0.028)	(0.030)	(0.708)	(0.021)
	Agrees at baseline	0.014	0.053**	0.899*	0.031**
		(0.021)	(0.021)	(0.539)	(0.015)
	Village Interventions X Agrees at baseline	-0.017	0.059**	1.527**	0.017
		(0.029)	(0.030)	(0.730)	(0.021)
Community disapproves coupleusing contraception to delay 1st birth (N=12107)	Village Debate or Edutainment	-0.008	0.007	0.730*	0.008
		(0.016)	(0.017)	(0.396)	(0.014)
	Agrees at baseline	0.000	0.014	0.607*	-0.011
		(0.013)	(0.014)	(0.330)	(0.010)
	Village Interventions X Agrees at baseline	0.010	-0.005	-0.554	0.008
		(0.018)	(0.019)	(0.444)	(0.014)
Reports women sometimes punished/stigmatized for using contraception (N=11152)	Village Debate or Edutainment	-0.002	0.005	0.204	0.007
		(0.013)	(0.015)	(0.370)	(0.012)
	Agrees at baseline	0.008	0.004	-0.355	-0.018*
		(0.014)	(0.014)	(0.352)	(0.011)
	Village Interventions X Agrees at baseline	-0.002	-0.009	0.475	0.021
		(0.019)	(0.020)	(0.488)	(0.015)

Notes: Robust standard errors in parentheses. Clustering at village level. CSPS fixed effects used across all specifications. Endline controls used are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. In all regressions, we include other demand treatment arms (Individual Mortality Info and Individual Edutainmment) but we don't report the coefficients in the table. N refers to the number of observations that had a non-missing response to the belief question at baseline. The number of observations may vary slightly between outcomes; in this case, we report the minimum number across all 4 regressions. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$

Online Appendix

Table B.1: Treatment effects on primary outcomes: fully interacted (version with village interventions not pooled together)

	(1) Had a live birth since baseline	(2) Used medical contraception in last 3 yrs	(3) Month(s) used modern contraception (last spell of each type) in last 3 years	(4) Used IPA subsidy voucher
Full Subsidy	-0.036*	0.006	-0.281	0.047***
	(0.019)	(0.023)	(0.541)	(0.017)
Village Debate	-0.008	0.008	0.031	0.018
	(0.021)	(0.025)	(0.661)	(0.017)
Village Debate + Mortality Info	-0.017	0.009	0.282	0.031
	(0.021)	(0.029)	(0.703)	(0.019)
Village Edutainment	-0.007	0.018	0.552	0.029**
	(0.021)	(0.025)	(0.630)	(0.015)
Individual Edutainment	0.006	-0.006	0.393	0.013
	(0.022)	(0.021)	(0.627)	(0.015)
Individual Mortality Info	0.010	0.025	0.886	-0.015
	(0.025)	(0.022)	(0.665)	(0.014)
Village Debate X Full Subsidy	0.033	0.008	1.068	-0.023
	(0.030)	(0.037)	(0.924)	(0.026)
Village Debate + Mortality Info X Full Subsidy	0.013	-0.031	-0.520	-0.021
	(0.030)	(0.041)	(0.954)	(0.028)
Village Edutainment X Full Subsidy	0.021	-0.012	-0.232	-0.036
	(0.028)	(0.037)	(0.895)	(0.026)
Individual Edutainment X Full Subsidy	0.031	0.030	-0.285	-0.024
	(0.031)	(0.031)	(0.832)	(0.025)
Individual Mortality Info X Full Subsidy	0.027	-0.025	-0.728	0.017
	(0.034)	(0.033)	(0.842)	(0.022)
Province FE	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No
Observations	12,542	12,131	12,107	12,519
Control (10% Subsidy) Mean	0.628	0.526	9.373	0.129

Notes: See Table 4.

Table B.2: Treatment effects on primary outcomes

	(1) Had a live birth since baseline	(2) Used medical contraception in last 3 yrs	(3) Month(s) used modern contraception (last spell of each type) in last 3 years	(4) Used IPA subsidy voucher
Panel A: Supply Intervention (Price Subsidy)				
Full Subsidy	-0.017	0.000	-0.318	0.032***
	(0.012)	(0.015)	(0.399)	(0.010)
Province FE	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No
Observations	12,542	12,131	12,107	12,519
Control (10% Subsidy) Mean	0.623	0.531	9.609	0.142
Panel B: Demand Interventions: Pooled				
Village Debate or Edutainment	-0.002	0.003	0.371	0.013
	(0.010)	(0.013)	(0.307)	(0.010)
Individual Edutainment	0.021	0.009	0.228	0.003
	(0.015)	(0.015)	(0.416)	(0.012)
Individual Mortality Info	0.022	0.015	0.562	-0.004
	(0.017)	(0.016)	(0.420)	(0.011)
CSPS FE	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No
Edutain. Indiv=Vill.	0.108	0.727	0.732	0.412
Panel C: Demand Interventions: Breakdown				
Village Debate	0.007	0.011	0.661	0.007
	(0.014)	(0.016)	(0.411)	(0.012)
Village Debate + Mortality Info	-0.012	-0.014	-0.110	0.021
	(0.014)	(0.017)	(0.405)	(0.013)
Village Edutainment	0.000	0.013	0.541	0.012
	(0.013)	(0.017)	(0.395)	(0.012)
Individual Edutainment	0.021	0.009	0.227	0.003
	(0.015)	(0.015)	(0.416)	(0.012)
CSPS FE	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No
Edutain. Indiv=Vill.	0.198	0.810	0.516	0.529
Deb=Deb + Mortality	0.207	0.180	0.096	0.324
Deb = Edutain. Vil	0.662	0.885	0.793	0.705

Notes: All specifications include province fixed effects (Panel A) or health centers (CSPS) fixed effects (Panels B and C). Controls are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. Robust standard errors in parentheses. Panel A: clustering at health center level. Panels B and C: clustering at village level. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$

Table B.3: Treatment effects of demand interventions on perceived Under 5 mortality rates, by sub-samples

	(1) Women who Over-estimate	(2) Women who Underesti-mate	(3) Women who had lost child	(4) Women who had not lost child	(5) Province Where Mortality Increased	(6) Province Where Mortality Decreased
Village Debate	0.340	-0.624	0.606	-0.146	-0.699	0.171
	(0.685)	(0.601)	(0.909)	(0.503)	(0.671)	(0.630)
Village Debate + Mortality Info	0.649	-0.809	1.596*	-0.285	1.108	-0.691
	(0.647)	(0.594)	(0.860)	(0.476)	(0.741)	(0.531)
Village Edutainment	0.595	-1.344**	-1.170	-0.161	-1.048	-0.238
	(0.639)	(0.633)	(0.883)	(0.468)	(0.673)	(0.559)
Individual Edutainment	0.065	0.129	2.256**	-0.167	1.231*	-0.415
	(0.645)	(0.731)	(1.106)	(0.466)	(0.741)	(0.467)
Individual Mortality Info	-0.130	-0.822	0.413	-0.475	0.286	-0.439
	(0.695)	(0.708)	(0.961)	(0.475)	(0.768)	(0.493)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No	No	No
Observations	5,299	4,314	2,430	9,039	3,276	9,117
Control (10% Subsidy) Mean	16.912	16.199	16.687	16.318	16.780	16.314

Notes: The table presents the effects of demand interventions on percieved under-5 mortality rates for different sub-samples: women who overestimated (col 1) or underestimated (col 2) child mortality at baseline; women who had lost (col 3) or not (col 4) a child at baseline; provinces where the mortality increased (col 5) or decreased (col 6) between baseline and endline. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$

Table B.4: Treatment effects: spousal communication

	(1)	(2)	(3)	(4)
	Wife		Husband	
	Talked to husband about modern contraception in past 3 years	Ever discussed number of children with husband	# wives with whom husband ever talked about contraception	# wives with whom husband ever talked about number of children
Full Subsidy	0.010	-0.036**	-0.063**	-0.052***
	(0.017)	(0.016)	(0.026)	(0.019)
Province FE	Yes	Yes	Yes	Yes
Baseline Controls	No	No	No	No
Observations	11,440	12,535	11,774	11,774
Control (10% Subsidy) Mean	0.588	0.320	0.726	0.335

Notes: Endline controls are: Whether the village had to be surveyed by phone due to security concerns at endline and date of the endline survey. Robust standard errors in parentheses. Clustering at health center level. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.