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The Negligible Effect of Free Contraception on Fertility: Experimental Evidence from Burkina Faso[†]

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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 modern contraception for three years did not have lower birth rates; we can reject even modest effects. We cross-randomized additional interventions to address inefficiencies that might depress demand for free contraception, specifically misperceptions about the child mortality rate and social norms. Free contraception did not significantly influence fertility even in combination with these interventions. (*JEL* D12, J13, J16, J18, O12)

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 only 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), arguably the most influential view is that limited access to affordable contraception is the key driver of high fertility in sub-Saharan Africa (Bongaarts, Mauldin, and Philipps 1990; Bongaarts 2017). Under this view, there is a large "unmet

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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 percent of governments in sub-Saharan Africa have laws or regulations that guarantee access to contraceptive services (United Nations 2021). A global partnership known as FP2030 allows governments and foreign aid agencies to allocate billions of dollars each year in pursuit of universal access to family planning by 2030 (FP2030 2021). The goal of these efforts goes beyond giving women control over their reproductive health as a human right. 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, Bongaarts, and Mberu 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 tested 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 offered women of reproductive age in rural communities vouchers that cover 100 percent ("full subsidy") of the cost of contraception at their local government health clinic. In the remaining communities ("control," or comparison group), households were provided vouchers for a 10 percent subsidy. Our estimates should therefore be interpreted as the effect of offering a full subsidy versus a small subsidy. In short, the full subsidy intervention ensures free, local access to modern contraception for three years.

In the spring of 2018, we recruited a baseline sample of more than 14,000 households from 499 villages that cover half of the Burkina Faso provinces. To assess impacts on fertility behaviors, we conducted an endline survey three years later, in the spring of 2021. Despite challenges (a rise in terrorism in many parts of the country and the COVID-19 pandemic), we were able to survey 88 percent of the baseline sample. The primary outcome we study is realized fertility, but we also analyze effects on contraceptive use.

We find that the full subsidy intervention had no significant effect on fertility during the 3-year period, or on the probability and duration of modern contraceptive use, relative to the 10 percent subsidy comparison group. Based on the 95 percent confidence interval, we can rule out declines larger than 4.1 percentage points in the likelihood of giving birth over the 3-year period, which represents 6.5 percent of the mean of 62 percent.[2] Our large sample size allows us to investigate heterogeneity along many dimensions and to show that the effect remains negligible in many subsamples, including those that ex ante would be expected to have larger unmet demand for modern contraception, such as women who were

[1] This view is supported by the fact that 20 to 40 percent 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).

[2] The study was 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.

not using contraception, were not pregnant, and did not want to get pregnant in the near future, and those who face low nonprice barriers, such as distance to the clinic.

The insignificant effects on fertility and contraceptive use contrast with the significant positive effect of the subsidy on voucher take-up: Women who received the 100 percent voucher were 20 percent more likely to redeem the voucher than those who received the 10 percent voucher. Using this measure of contraceptive take-up, free access to contraception would appear to have an effect. 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 modern contraception. Instead, the vouchers simply paid for contraception for women who would also have used it with only a 10 percent subsidy.

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, the 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 such as Senderowicz and Valley (forthcoming), 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.

Our finding of low unmet demand for contraception is consistent with a high desired fertility. Understanding whether high desired fertility is 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 addressed.

The previous literature in economics has recognized that there are many nonfinancial barriers to contraceptive use, including misperception of side effects such as fear of infertility (Bau et al. 2024; Glennerster, Murray, and Pouliquen 2021), misperception of female mortality risks among men (Ashraf et al. 2022), and disagreement within couples (Ashraf, Field, and Lee 2014). We conducted extensive pilot work to investigate demand-side frictions in our context. Although the frictions studied by the literature are also present in rural areas of Burkina Faso, we focused on two dimensions that appeared relevant and have been mostly explored outside of economics: the role of social norms and the role of perceptions of child mortality. We designed two interventions to assess if these frictions could be addressed and whether they impact the price elasticity of the demand for contraception.

Our first 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, González, and Yanagizawa-Drott 2020). We created common knowledge about the community views by having attendees vote anonymously, after each debate, on which side represented their view, with the aggregate votes tallied and shared publicly at the end of the meeting. Vogt et al. (2016) show that it is possible to change the acceptability of female genital mutilation by screening filmed debates where actors discuss various positions commonly held in the community. Previous field interventions also suggest that community dialogues can be successful in changing views about contraception in various contexts including Bangladesh (Kincaid 2000), India (Daniel, Masilamani, and Rahman 2008a), Kenya (Wegs et al. 2016a), and Niger (Erhardt-Ohren et al. 2023).

Our 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 1986a; Preston 1978; Carey and Lopreato 1995; Kalemli-Ozcan 2003). Child mortality has halved in Burkina Faso in recent years, going from 179 deaths per 1,000 live births in 2000 to 91 in 2018 (United Nations Inter-agency Group for Child Mortality Estimation 2025). 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 ideal. 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 had no significant direct effect on fertility or contraceptive use, and they did not modify the effect of the full subsidy. One explanation is that misperceptions are hard to change in a sustained way, and we indeed fail to detect an effect of the interventions on beliefs (whether first order or second order) three years later. Complementarities may exist between subsidies and more intensive demand-side interventions, though. An

[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's (2019) 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.

alternative hypothesis consistent with our findings is that, in this economic and social environment, families may well be making a *rational* choice to have large families.[5]

This paper contributes to the literature on the impact of access to contraception on fertility, and in particular on the role of financial constraints. There are a large number of studies including RCTs that have investigated the effects of price on contraceptive use. 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, Macouillard, and Meessen 2016; Bellows et al. 2016). Surprisingly, virtually all previous experimental studies mentioned in these review articles 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 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.[7] In contrast, a study in Ethiopia that only provided pills and condoms on credit for three years found no significant effect on fertility (Desai and Tarozzi 2011). Similarly, an RCT in Lusaka, Zambia also found null effects of free provision, though the program only covered injectables and implants and was only one month long (Ashraf, Field, and Leight 2013).[8]

We build on this literature to show that making all contraceptive products completely free for a *sustained period* for all women of childbearing age in rural areas (where most of the population of West Africa lives) is insufficient to affect fertility—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.

I. Study Design

A. *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

[5] 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; Donald et al. 2024).

[6] The Matlab intervention lowered lifetime fertility by 14 percent to 23 percent, and the Navrongo intervention reduced fertility by 15 percent (see the review by Miller and Babiarz 2016). The intervention in Malawi lowered the birth rate from 9 percent to 5 percent over a 2-year period.

[7] For example, the Matlab program offered family planning, reproductive health, and child health services; see Joshi and Schultz (2013).

[8] Using the same experimental data and focusing on the treatment arm with free access, Ashraf, Field, and Lee (2014) show that the way in which vouchers are delivered matters: Involving husbands reduces take-up, with subsequent effects on fertility, compared to giving vouchers to women in private.

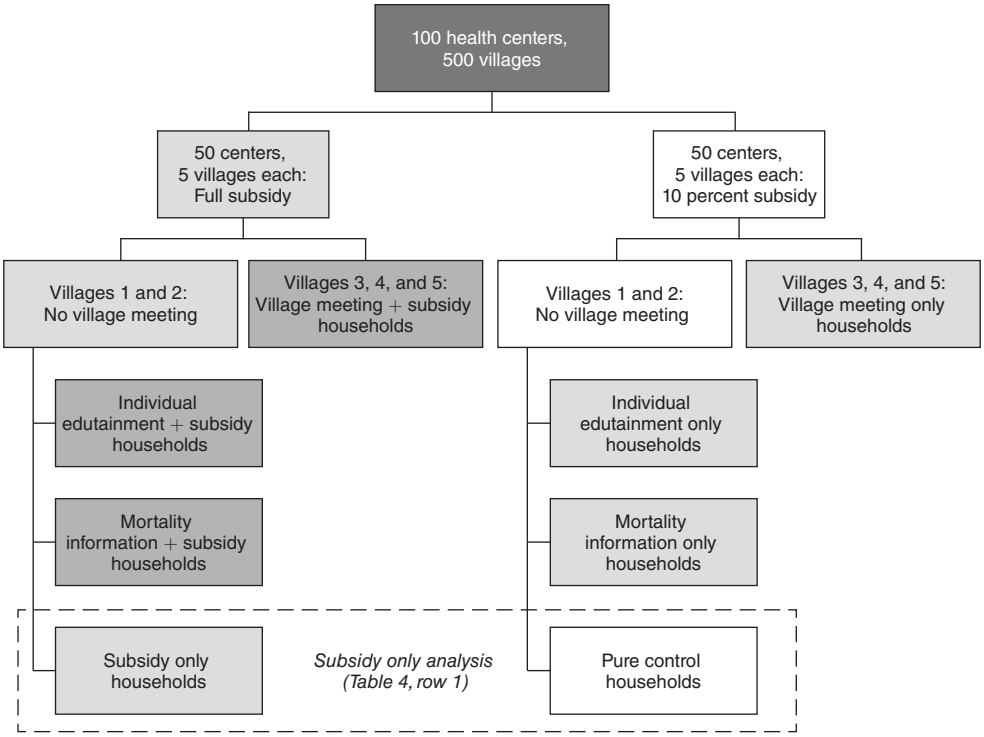

Figure 1. Experimental Design

Notes: The figure presents the experimental design. There are three levels of randomization. (i) Across health centers: 50 centers (encompassing 250 villages) were assigned to the full subsidy arm, and 50 centers were assigned to the 10 percent subsidy arm. (ii) Across villages, within each health center catchment area: Three villages were assigned to village meetings, and two villages were assigned to individual interventions. (iii) Across households, within each village allocated to individual interventions: Some were assigned to the individual edutainment arm, some households were assigned to the mortality information arm, and the rest received no demand intervention. Cells colored in dark gray received both the full subsidy and a demand intervention. Cells colored in light gray received either the full subsidy or a demand intervention. The white cells received neither the subsidy nor any demand intervention: These are the pure control households. Supplemental Appendix Figure A2 provides more details.

centers in the 45 provinces of Burkina Faso (a map is shown in Supplemental Appendix Figure A1). We sampled 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 were 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 were selected to maximize the minimum distance between any two villages sampled.

The first randomization was done across health centers: 50 health centers (serving 250 sample villages) were selected for the vouchers for free contraceptives intervention. Women in the other 50 health centers received a 10 percent discount voucher. This allows us to compare administrative voucher redemption rates across the two arms. We did not expect the 10 percent discount to have a large effect on contraception use, so this was meant to approximate the status quo.

The second randomization was done across villages within each health center catchment area. In each catchment area, we allocated three villages to receive a

group intervention and two 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 was across households within each village allocated to individual interventions. In each village, households either (i) received information about child mortality, (ii) watched the edutainment film at home, or (iii) did nothing.[9]

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, and (iii) estimate the effect of the demand-side interventions on their own. In the next sections, we provide details on the supply-side intervention (the full subsidy) and then describe our household sampling and data collection strategy. The demand-side interventions are described in detail in Section IVB.

B. *Intervention: Three-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 percent and 10 percent).[10]

Vouchers were delivered to all eligible women in sampled households, in private, during the baseline survey in spring 2018. The vouchers were accepted by 70 percent of women; a quite large share (30 percent) rejected them out of hand.[11] The value of the voucher is small by high-income context standards. In our baseline survey, health centers reported charging on average $2.5 to insert an implant and $2.7 to remove it (implants are the widely used contraceptive product); these prices range between $1 and $6.5. However, this monetary cost is large when compared to local incomes: In rural areas, half of the population lives on $250 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.[12] In the 100 percent arm, all costs were 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 percent during monitoring visits, and health centers reported that they could typically restock within a few days.

[9] Supplemental Appendix Figure A2 provides more details.

[10] A memorandum of understanding was signed between IPA and each health center. IPA committed to cover 100 percent/10 percent of the value of family planning 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.

[11] Among those who refused, the majority said that they did not need contraception; another 20 percent said they did not want the voucher because they feared the husband or relatives of the husband could see her with the voucher, and 5 percent mention fear of side effects.

[12] If women opted for long-lasting methods, they could visit the health center to interrupt them whenever they wanted (potentially, just before the expiration date of the voucher); these costs were covered.

There are three important points regarding the implementation of the intervention. First, the study was initially planned to last for two 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 two to three years.[13] 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.[14] Second, Burkina Faso experienced a strike among health workers from June to December 2019. Nonetheless, the strike only affected reporting activities to the Ministry of Health but not the provision of services. In our monitoring surveys, 92 percent 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.[15] Our coefficients should therefore be interpreted as intent-to-treat estimates. We discuss the implications of these macro shocks for our findings in Supplemental Appendix C.

C. *Sampling of Households and Randomization*

To enroll households in the study (Dupas et al. 2025), 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) between the ages of 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.[16] In households with multiple eligible women, we 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 percent of the national population live 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).[17] Low urbanization rates are not specific to Burkina Faso. Overall, 54 percent of the population in West Africa live in rural areas; the fraction ranges between 60 percent

[13] We informed health agents of this change through in-person visits to health facilities, and health agents relayed the information to women in the village.

[14] Supplemental Appendix Figure A3 reports 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 were 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.

[15] Burkina Faso experienced a major rise in jihadist terrorism threats over our study period (Supplemental Appendix Figure A4).

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

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

and 85 percent in several countries, such as Guinea, Niger, Sierra Leone, and Togo (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 assigned to a group intervention, we randomly sampled 25 eligible households. In each village assigned to individual interventions, we randomly sampled 35 households and randomized them to one of the individual treatments (mortality information or private screening of the edutainment video, described in Section IV), stratifying by village. We sampled 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.[18] Supplemental Appendix Table A1 shows that the procedure was successful in creating balanced groups in terms of fertility preferences, contraceptive use, important demographics, and potential sources of heterogeneity at baseline.

II. Data

A. *Data Collection*

Listing.—The census listing took place in fall 2017. In addition to sociodemographic 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 subnational heterogeneity in both levels and trends, awareness of which enables a government to appropriately target resources (Supplemental Appendix Figure A5).

Baseline.—We conducted a baseline survey with the focal wife in the spring of 2018 in 499 villages; 1 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.[19] 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 is

[18] 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.

[19] 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 (Supplemental Appendix Table A2).

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 34 percent of the villages (Supplemental Appendix Figure A6). 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 arms are equally affected by these issues, which are highly spatially correlated.

We were able to survey 87 percent of women and 88 percent 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 percent compared to 10 percent in places surveyed in person. Attrition is particularly concerning if it is differential. The attrition rate is balanced across the full and 10 percent subsidy groups (Supplemental Appendix Table A3).

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.

B. *Baseline Summary Statistics*

Table 1 reports summary statistics for the focal wives at baseline.

The average woman is around 28 years of age and is married to a 38-year-old man. In this agricultural setting, most women work in the fields of the family farm. Eighty-three percent of women have no formal education, and 45 percent of women live in polygamous households. Roughly two-thirds are Muslim. Households are poor: For example, only 1 percent have access to an electricity network, 48 percent own a radio, 47 percent of the dwellings have cemented or tiled floor, and 43 percent have a toilet.[20]

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

[20] The sample is not constant due to attrition or nonresponse for outcomes. Some baseline characteristics are also missing.

TABLE 1—SUMMARY STATISTICS FROM BASELINE SURVEY OF WIVES

	Mean	SD	Observations
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			
Number 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 number 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
Share of implant users (lasting for 3–5 years) among current users	0.59	0.49	4,589
Share of injectable users (lasting for several months) among current users	0.32	0.47	4,589
Distance in km between visited CSPS (baseline coord.) and village (final coord.)	6.20	4.58	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

Notes: Data from baseline survey with wives. Supplemental Appendix Table A4 shows summary statistics for the husbands' data. 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.

Almost all women know about modern contraception: 46 percent of women have ever used modern contraceptives, and 31 percent are currently using them at baseline.[21] Among current users, 59 percent use implants (lasting for 3 to 5 years), and 32 percent use injectables (lasting for several months). This relatively low use does not appear to be driven by a rejection of these methods: Only 20 percent of individuals think modern contraception is dangerous to health. Finally, almost 40 percent satisfy the conditions for having an "unmet need for contraception" (they do not want a child and are not using modern contraception), and 41 percent report they would not be able to afford contraception if they wanted to use it.

III. The Effects of a Full Subsidy for Contraception

A. *Regression Specification*

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

$$Y_{ivcp} = \beta_1 FullSubsidy_c + \gamma_p + \mathbf{X}'_{iv}\boldsymbol{\rho} + \varepsilon_{ivcp}, \tag{1}$$

[21] For reference, 65 percent of US women aged 15–49 were using a contraceptive method in 2018: https://www.cdc.gov/nchs/data/databriefs/db388-H.pdf (last accessed March 19, 2025).

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 percent 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 $\mathbf{X}_{iv}$ indicate 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 $\mathbf{X}_{iv}$. 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).[22] 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 subpopulations 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 subpopulations using baseline information, for example, on financial constraints or stated unmet need for contraception.

B. *Average Effects*

We start by investigating the effect of the full subsidy 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, measured in months (Figure 2). By construction, the fraction is 100 percent at $t = 0$. The interventions could only influence births taking place at least nine months (shown as a vertical line) after the intervention start. At the 9-month mark, 12.5 percent 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, and this confirms balance across the two arms, there are no differences up to that time between the full subsidy group and the comparison (10 percent subsidy) group.

If the intervention had an effect, we would expect to see fertility diverging after nine months. Instead, we observe that the curves follow each other closely, suggesting no impact of the full subsidy voucher. 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. Under the hypothesis that free contraception only affected the

[22] 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, Murray, and Pouliquen (2021) for more information.

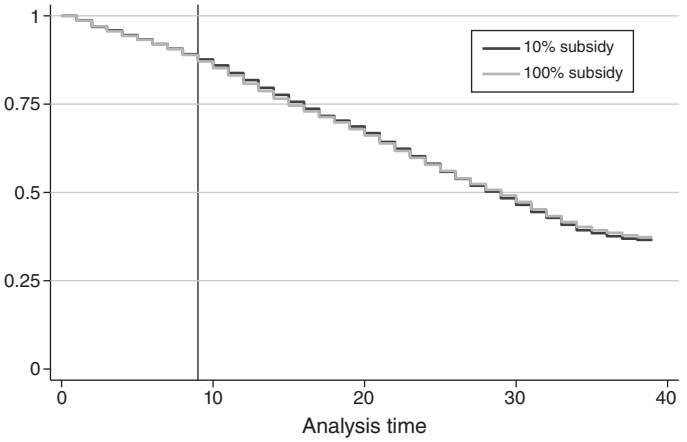

FIGURE 2. DURATION UNTIL NEXT BIRTH, BY SUBSIDY STATUS

Notes: The figure shows a Kaplan-Meier survival curve for the 10 percent subsidy group and the 100 percent 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 the endline survey ($t = 36$ months). The intervention could not have affected fertility in the first nine months (area left of the vertical line). The fraction of women who have given birth during the first 9 months, and hence were presumably pregnant at baseline, is equal to 12.5 percent. The fraction is the same in both groups. A test that the survival curves are identical cannot reject the null (p-value $= 0.55$); thus, the fertility behavior postintervention is the same regardless of the contraception subsidy received.

spacing of birth, but not the probability of birth after 3 years, the slope of the 10 percent curve would be steeper than the slope of the 100 percent curve right after the 9-month mark (women without free access would have another birth sooner), but eventually both curves would converge toward the end of the intervention period. This is not what we observe: Women in both groups give birth at the exact same rate during the whole period. This result implies that any aggregate measure of fertility will not be affected by the voucher because we do not detect differences in fertility rates at any time during the three-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 3 years since the baseline, which is a 2.7 percent decrease relative to the control mean of 62 percent. 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: In column 2 of Table 2, we find a 1.9 percentage point lower probability of having a pregnancy, a 2.7 percent decrease relative to the control mean of 70.5 percent. The coefficient is insignificant. Therefore, the magnitude and precision of the effect are 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 0.03 percentage points,

Table 2—Treatment Effects on Primary Outcomes

	Had a live birth since baseline	Had a pregnancy since baseline	Used medical contraception in last 3 years	Month(s) used modern contraception (last spell of each type) in last 3 years	Used IPA subsidy voucher
	(1)	(2)	(3)	(4)	(5)
Panel A. With only endline controls					
Full subsidy	−0.017	−0.019	0.000	−0.318	0.032
	(0.012)	(0.012)	(0.015)	(0.399)	(0.010)
Baseline controls	No	No	No	No	No
Panel B. With endline and baseline controls					
Full subsidy	−0.017	−0.018	0.008	−0.194	0.035
	(0.011)	(0.011)	(0.013)	(0.360)	(0.009)
Baseline controls	Yes	Yes	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	12,542	12,543	12,131	12,107	12,519
Control (10% subsidy) mean	0.623	0.705	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-five mortality rate reported by wife at baseline; wife and husband's first-order beliefs at baseline, that is, 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 fifth 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.

less than a 0.1 percent increase (column 3 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 comparison group; see column 4 of Table 2).[23] The point estimates and significance levels are very similar in panel B, when we add baseline controls.

This null result is important for policy. The 95 percent confidence interval on having a birth is $[-0.041, 0.007]$. We can rule out a fertility decline larger than 6.5 percent ($= 0.0406/0.623$). Extrapolating from the 3-year effect to the effect of lifetime access, 30 years of free contraception (from age 15 and 45) would lower fertility by at most $0.041 \times 10 = 0.41$ children per woman from a starting point of 6.2 children. While not insignificant, this is a small effect relative to what one would expect if financial barriers were the main factor behind high fertility.

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; that is, clinics did honor vouchers. In the endline survey, only 13 percent of women report not having heard of the IPA vouchers. We also know that voucher use was not 0: 14 percent of women in the comparison group and 17 percent of women in the treatment group reported redeeming the voucher at least once (column 5 of Table 2). This 3 percentage points difference

[23] When we look at the method mix, we find no detectable effect: Women do not switch from one contraceptive method to another.

is significant at the 1 percent level. In the health centers' registers, the difference is 4 percentage points and is also significant at the 1 percent level. The fact that voucher redemption is higher in the treatment group confirms that women understood the discount associated with their voucher, being more likely to use it when it was more generous. However, the fact that we see no increase in contraception use in column 3 suggests that all or 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 would also have used it with only a 10 percent subsidy.

The voucher take-up in the treatment group (17 percent) may seem low compared to the fraction of women currently using contraception at baseline (31 percent), raising the question of whether inframarginal women (those who would have used contraception in any case) left money on the table by not using their voucher. However, recall that 59 percent of baseline *users* had a long-lasting implant and thus either needed no replacement during the study period (the implants last 3 to 5 years) or planned to get pregnant after the expiration. The voucher usage rate is higher than the 13 percent share of women who were using a form of contraception other than implants (mostly injectables) at baseline. In addition, some women may prefer to get contraception from a different provider than the health clinic where their voucher was valid, potentially because of a more convenient location, even though they had to pay for it.[24]

More generally, we investigate whether free contraception affected women's well-being more generally in Supplemental Appendix Table A5. We find that the full subsidy treatment seemingly *decreased* the incidence of intimate partner violence (IPV) but had no effect on self-reported health, life satisfaction, various measures of reproductive control, and monogamy.[25]

C. *Effects in Subpopulations with the Likely Largest Effects*

Although our overall impacts on fertility are small and insignificant, these results could mask heterogeneity. We investigate this by estimating the treatment effects within subgroups 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 dos not want another child in the next two years *and* is not using modern contraception. Then, we look at both dimensions separately. We also look at women

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

[25] 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 (Supplemental Appendix Table B1). 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. We do not study divorce; 99 percent of respondents stayed married throughout the study.

Table 3—Treatment Effects on Primary Outcomes by Subsamples

Subsample		Had a live birth since baseline (1)	Had a pregnancy since baseline (2)	Used medical contraception in last 3 years (3)	Month(s) used modern contraception (last spell of each type) in last 3 years (4)	Used IPA subsidy voucher (5)
Need for contraception						
Had unmet need for contraception at baseline ($N = 4{,}649$)	Full subsidy	−0.011	−0.015	0.018	−0.104	0.042
		(0.015)	(0.014)	(0.019)	(0.459)	(0.014)
	q-value	0.571	0.327	0.779	0.948	0.007
	Control mean	0.662	0.736	0.437	7.229	0.128
Wife did not want another child over next 2 years at baseline ($N = 7{,}583$)	Full subsidy	−0.016	−0.021	0.009	−0.392	0.037
		(0.013)	(0.013)	(0.017)	(0.462)	(0.013)
	q-value	0.571	0.250	0.862	0.896	0.007
	Control mean	0.611	0.703	0.559	10.649	0.157
Husband did not want another child over next 2 years at baseline ($N = 4{,}724$)	Full subsidy	−0.013	−0.036	0.022	0.159	0.033
		(0.016)	(0.016)	(0.019)	(0.514)	(0.013)
	q-value	0.571	0.118	0.779	0.948	0.015
	Control mean	0.612	0.712	0.560	10.594	0.155
Was not using modern contraception at baseline ($N = 8{,}191$)	Full subsidy	−0.011	−0.014	−0.002	−0.303	0.029
		(0.012)	(0.012)	(0.014)	(0.352)	(0.010)
	q-value	0.571	0.327	0.947	0.896	0.007
	Control mean	0.641	0.709	0.421	6.854	0.113
Was not using modern contraception and was not pregnant at baseline ($N = 7{,}052$)	Full subsidy	−0.017	−0.017	0.001	−0.040	0.034
		(0.014)	(0.014)	(0.015)	(0.372)	(0.010)
	q-value	0.571	0.311	0.947	0.948	0.005
	Control mean	0.585	0.677	0.408	6.551	0.109
Could not afford contraception at baseline ($N = 4{,}519$)	Full subsidy	−0.025	−0.032	0.008	−0.099	0.037
		(0.017)	(0.015)	(0.017)	(0.497)	(0.013)
	q-value	0.571	0.118	0.862	0.948	0.007
	Control mean	0.616	0.702	0.505	9.055	0.148
Other frictions						
Health center $<$ 2 km away ($N = 2{,}060$)	Full subsidy	−0.025	−0.040	0.043	0.827	0.095
		(0.028)	(0.029)	(0.036)	(0.965)	(0.023)
	q-value	0.571	0.304	0.779	0.896	0.001
	Control mean	0.578	0.680	0.558	10.022	0.150
Disagrees with modern contraception being harmful for health ($N = 8{,}563$)	Full subsidy	−0.007	−0.013	−0.007	−0.641	0.036
		(0.013)	(0.014)	(0.016)	(0.427)	(0.011)
	q-value	0.571	0.334	0.862	0.896	0.005
	Control mean	0.617	0.705	0.573	10.633	0.159
Neither wanted another child over next 2 years ($N = 3{,}771$)	Full subsidy	−0.010	−0.037	0.025	0.036	0.035
		(0.018)	(0.018)	(0.021)	(0.536)	(0.015)
	q-value	0.571	0.118	0.779	0.948	0.023
	Control mean	0.599	0.709	0.567	11.044	0.163

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 percent subsidy subsample. *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 four regressions. *q*-value refers to the adjustment for Multiple Hypothesis Testing in Anderson (2008).

whose husband did not want a child in the next two years. In addition, we note that, despite it being an exclusion criterion, 12.5 percent of women were indeed pregnant at baseline (but did not know it) and hence presumably did not need contraception at the beginning of the intervention (although they may have needed it after the birth). We therefore also look at the subset of women who were neither pregnant at baseline nor using modern contraception. 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 (Supplemental Appendix Table A6 shows the specification with baseline controls).

Since we are testing a large number of hypotheses among subsamples, we report q-values correcting for multiple hypothesis testing (MHT) (Anderson 2008). The effect sizes are still surprisingly modest. For instance, the point estimates and the standard errors for women who were neither pregnant nor using contraception are very close to the whole sample. The largest effects on fertility that we estimate are for women who could not afford contraception at baseline (2.5 percentage points, a 4 percent decline in live births, and 3.2 percentage points, a 4.5 percent decline in pregnancies). However, the effects of the 100 percent subsidy on live births and modern contraception use are statistically insignificant for all subgroups, even without MHT corrections. The effects on pregnancies are insignificant once we adjust for MHT (all of Anderson's q-values in columns 1 to 4 exceed 0.10).

Why do women who do not want to get pregnant not take up modern contraception that is provided for free? One possibility is that pregnancy intentions may be ambivalent; that is, women may have unresolved or contradictory feelings about having a child. Ambivalence is systematically correlated with lower use of contraception in various contexts (LaCross, Smaldone, and Angelson 2019). In West Africa, qualitative studies document that pregnancy intentions are poorly specified and vary over time for many women (Agadjanian 2005). In Burkina Faso, Speizer (2006) assesses the strength of fertility intentions among women. She finds that a quarter of those who expressed wanting to delay their next birth or to stop having children also reported that a pregnancy in the next few weeks would not be a problem. Our small effect sizes suggest that estimated rates of unmet need for contraception may overestimate the share of women who unambiguously want to avoid getting pregnant. Alternatively, it may be that women can control their fertility using traditional contraception methods. Indeed, fertility levels fell in many now-rich countries prior to the availability of modern contraception (Coale 1986b). Recent research shows that traditional birth control methods are also used effectively today in other parts of Africa (e.g., Alam and Portner 2018).

An alternative explanation could be that women do not want to have children and cannot control their fertility but face other barriers that prevent them from using modern contraception. Our 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 or they face other barriers, and its free provision does not ameliorate these issues.[26] To investigate this, we test whether the subsidy had larger effects on contraception use and fertility among subgroups for whom other frictions are small.

We consider three 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, Murray, and Pouliquen 2021; Bau et al. 2024). Indeed, in our focus groups some cited fears of infertility

[26] 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.

and other health concerns when asked about why they opted out of modern contraception.[27] Again, Table 3 shows there are no statistically significant declines in fertility among groups for whom these additional frictions are low. In particular, although the effect of the full subsidy on voucher take-up is around 10 percentage points among those who are close to the health center, the size of the treatment effect on contraceptive use (4 percentage points) and births (2.5 percentage points) remains very modest.[28] Finally, since Ashraf, Field, and Lee (2014) find that spousal disagreements matter for contraceptive take-up, we also examine whether the subsidies affected the fertility of couples in which neither spouse wants another child. There is a significant effect on pregnancies but no effect on live births or on contraceptive use, and the magnitude of the effect remains modest. Note that for all of these subgroups, we estimate statistically significant impacts of the full subsidy on voucher use even when accounting for MHT (column 5), further confirming 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 births and modern contraceptive use even among subpopulations for which we might have anticipated larger impacts. The results do suggest there are subgroups for whom there might be modestly sized effects: those who report not being able to afford contraception and those who live less than two kilometers from a health center.[29]

In the next section, we take a closer look at the idea that relaxing supply constraints is not effective when there are additional demand constraints. We investigate two specific information frictions that potentially drive up desired fertility: misperceived acceptability of birth control and misperceived child mortality.

IV. The Role of Information Frictions in Explaining the Lack of Impact of the Full Subsidy

To test how the effects of the full subsidy vary with the extent of demand-side frictions, we use two approaches. First, we examine the effects of the subsidy in subpopulations that appear less constrained by the demand-side frictions we study. Second, to address the fact that baseline proxies of demand-side frictions are not

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

[28] If we explore the heterogeneity by distance in a more systematic way, estimating treatment effects within different distance bins, we find a monotonic, negative relationship for voucher take-up but no relationship for contraception or fertility.

[29] We also investigated a number of other subgroups (Supplemental Appendix Table A7). The largest impact of the full subsidy was found among those who were already using modern contraception at baseline (2.8 percentage points decline in births, not significant). 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 nonpolygamous couples, and for junior and senior wives. Based on the literature, we also hypothesized that older women who are closer to having completed their desired fertility would be more responsive to the subsidy (in the Matlab experiment, for example, the point estimates suggest larger declines in fertility among older women (Joshi and Schultz 2013)). If we split the sample by median age, we find no effect on contraception and fertility in either subgroup. We also tested whether demand varies with the number of prior pregnancies but found this is not the case. The point estimates of the effect of the subsidy on pregnancy and births are small in magnitude and insignificant, even among women who already had five pregnancies or more. Finally, we see no increase in modern contraceptive use for this group.

Table 4—Survey Data on Potential Misperceptions

	Mean	SD	Observations
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 trade-off	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 first 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

Note: Data from baseline survey with wives.

exogenous, we cross-randomize information interventions. In this section, we start by providing summary statistics on the extent of misperceptions, then we describe the information interventions and finally, present the results.

A. *Descriptive Evidence on Potential Information Frictions*

Individuals' Attitudes and Perceived Social Norms.—Table 4 shows that, 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 its use to delay first births negatively (65 percent) or stigmatizes family planning users (39 percent). These negative social perceptions stand in contrast with private views about contraception: Most individuals think modern contraception is acceptable. This mismatch could reflect either pluralistic ignorance or the fact 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 postintervention data provide some descriptive evidence suggesting a generational divide. Although this varies across villages, acceptability of contraception and small families is quite high overall, with more than 50 percent in most villages supporting them, particularly among younger individuals and women. This suggests scope for village meetings to spark cross-group dialogues, perhaps resulting in changing the norms.

Perceived and Actual Child Mortality.—As reported in Table 4, current levels of mortality in this context remain high by international standards: Around 10 percent of children do not survive to age 5. The rate ranges across provinces from 7 percent to 14 percent. The situation was much worse and more diverse in the past: The average was around 16 percent, ranging from 12 percent to more than 20 percent

in several provinces, including 32 percent in one province.[30] Almost 30 percent of women in our sample have had a child who died, so child mortality is a salient phenomenon.[31] The beginning of the twenty-first century was thus a period of progress and convergence. Current levels and trends are consistent with those 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 percent (Supplemental Appendix Figure A7). 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 overestimation 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 of the true rate as accurate. About a third of beliefs are accurate (Supplemental Appendix Figure A8 plots the distribution of perceived mortality rates by gender). About half of respondents overestimate the mortality rate by more than 5 percentage points. Many of them make large mistakes: The mean perceived rate is 19 percent for women and 16 percent 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 recalibrate their beliefs, with heterogeneous effects on subsequent behavior, depending on whether people underestimate or overestimate at baseline.

Heterogeneity Analyses.—We assess whether the demand for contraception and fertility effects of the full subsidy are larger among women who at baseline either (i) do not believe that their community disapproves of contraception or (ii) do not report being punished or feeling stigmatized for using contraception. The results in Table 5 show that there are no statistically significant effects of the free contraception among these groups (all of Anderson's (2008) q-values in columns 1 to 4 exceed 0.10), and the magnitude of the fertility effects is modest (a decrease by 2.7 percentage points ($=$ 4.3 percent)).[32]

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,

[30] Supplemental Appendix Figure A7 plots the under-five 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 precisely the information provided in the information treatment arms.

[31] The 30 percent rate is consistent with most women having had 3 children at baseline. With a 10 percent child mortality rate, the odds that at least one of them has died is 27 percent (assuming independence).

[32] The specification with baseline controls is shown in Supplemental Appendix Table A8.

TABLE 5—TREATMENT EFFECTS WHEN POTENTIAL MISPERCEPTIONS ARE ABSENT

Subsample		Had a live birth since baseline (1)	Had a pregnancy 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 (5)
Social norms						
Does not believe community disapproves use of contraception ($N = 4{,}277$)	Full subsidy	−0.027	−0.029	−0.005	−0.534	0.043
		(0.014)	(0.014)	(0.019)	(0.509)	(0.013)
	q-value	0.101	0.101	0.789	0.440	0.005
	Control mean	0.627	0.703	0.531	9.600	0.150
Does not report women being punished or stigmatized for using contraception ($N = 6{,}855$)	Full subsidy	−0.027	−0.027	−0.013	−0.609	0.030
		(0.015)	(0.015)	(0.016)	(0.464)	(0.012)
	q-value	0.101	0.101	0.643	0.440	0.012
	Control mean	0.634	0.714	0.559	10.239	0.155
Mortality perceptions						
Does not overestimate under-5 child mortality ($N = 4{,}227$)	Full subsidy	−0.016	−0.005	0.031	0.411	0.034
		(0.016)	(0.017)	(0.019)	(0.529)	(0.012)
	q-value	0.331	0.780	0.307	0.440	0.011
	Control mean	0.628	0.707	0.527	9.473	0.148

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 percent subsidy subsample. *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 four regressions. *q*-value refers to the adjustment for Multiple Hypothesis Testing in Anderson (2008).

desire a large number of children. In this case, free vouchers would be ineffective. However, Table 5 shows that the full subsidy had no impact on births (a magnitude of 1.6 percentage points or 2.5 percent) in the group of women who do not overestimate child mortality. This would suggest that factors other than child mortality keep the demand for children high and/or the demand for modern contraception low.

However, these results are difficult to interpret because they are based on baseline characteristics that are not randomly assigned. To overcome this limitation, we now make use of the other randomized interventions we implemented, which aimed to lower demand-side barriers.

B. *Interventions to Address Potential Frictions*

Village Meetings.—The goal of the village meetings was to expose participants to both sides of the debates 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 five to seven members, diverse in age and gender composition) were formed and assigned a specific position, creating three debates on (i) the quantity-quality trade-off, (ii) the relevance of pronatalist norms in a changing world, and (iii) the pros and cons of modern contraception.[33] Teams

[33] 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

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. Men and women of older and younger generations cast their votes in separate urns, and facilitators tallied votes and announced final vote shares by gender and age group. Note that unless all individuals within a group voted identically, no one's private vote could be inferred.

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. For the screenings, we partnered with Cinema Numérique Ambulant, an NGO dedicated to bringing cinema to remote areas throughout West Africa.[34] An advantage of the film over the debates was that it ensured there were no false but purportedly factual statements made.

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 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 reveal 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, Casey, and Glennerster (2020) and implemented an individual treatment in which couples (including co-wives in polygamous households) saw the edutainment film at home.

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.

[34] The film can be seen at this link: https://www.dropbox.com/s/ymnp5regqchwx3h/FINAL%20SUB%20WEB.mp4?dl=0.

Public meetings were successfully implemented in 297 out of 300 villages in the spring of 2018.[35] Sampled households were informed at the end of the survey about the village meeting, and local leaders were asked to diffuse the information to the rest of the village. Attendance at the start of the meetings averaged 95 people [median: 75, range: 23 to 299], representing roughly 25 percent 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. Afterward, many villagers expressed their satisfaction to the implementing team.

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 collected the data ourselves because province-level data on child mortality were 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.

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. Supplemental Appendix Figure A9 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 percent reported understanding the information. About 50 percent described the information as "new."

[35] 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).

Table 6—Treatment Effects on Primary Outcomes: Fully Interacted

	Had a live birth since baseline (1)	Had a pregnancy since baseline (2)	Used medical contraception in last three years (3)	Month(s) used modern contraception (last spell of each type) in last three years (4)	Used IPA subsidy voucher (5)
Full subsidy	−0.036	−0.025	0.006	−0.280	0.047
	(0.019)	(0.018)	(0.023)	(0.542)	(0.017)
Village debate or edutainment	−0.010	0.008	0.012	0.291	0.026
	(0.017)	(0.015)	(0.019)	(0.484)	(0.012)
Individual edutainment	0.006	0.003	−0.006	0.393	0.013
	(0.022)	(0.020)	(0.021)	(0.627)	(0.015)
Individual mortality info	0.010	0.008	0.025	0.886	−0.015
	(0.025)	(0.021)	(0.022)	(0.665)	(0.014)
Village interventions	0.023	0.001	−0.011	0.117	−0.027
× Full subsidy	(0.023)	(0.022)	(0.029)	(0.691)	(0.021)
Individual edutainment	0.031	0.044	0.030	−0.285	−0.024
× Full subsidy	(0.031)	(0.030)	(0.031)	(0.831)	(0.025)
Individual mortality info	0.027	0.009	−0.025	−0.728	0.017
× Full subsidy	(0.034)	(0.031)	(0.033)	(0.842)	(0.022)
Province fixed effects	Yes	Yes	Yes	Yes	Yes
Baseline controls	No	No	No	No	No
Observations	12,542	12,543	12,131	12,107	12,519
Control mean	0.628	0.701	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.

C. *The Effects of Contraception Subsidies When Potential Information Frictions Are Addressed*

We estimate a version of equation (1) where we interact the subsidy with dummies for different demand-side treatment arms. The results are presented in Table 6. Results for the specification including baseline controls are similar and reported in Supplemental Appendix Table A9. 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 Supplemental Appendix Table B2).

Providing Information about Opposing Views and Social Norms.—By testing if the combined interventions (village meetings and free contraception) reduce fertility, we are testing the joint hypothesis that (i) misinformation about attitudes and norms systematically increases desired fertility and reduces acceptance of modern contraception under the status quo and (ii) 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 (Table 6). 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 demand interventions without interaction terms (Supplemental Appendix Table B3). 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 (Supplemental Appendix Table A10).[36]

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 (Supplemental Appendix Table A11). The lack of effect cannot be attributed to poor implementation: Attendance at the meetings was high, so people did hear others' views but did not change their own views as a result.

Addressing Mortality Misperceptions.—In the same way, 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 for births and pregnancies (columns 1 and 2 of Table 6). The interaction term is also small and insignificant for the contraceptive use measures (columns 3 and 4). Table 6 further shows that the 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. Supplemental Appendix Figure A10 shows the distribution of perceived minus actual child mortality at endline among women, for the treatment and comparison 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. Furthermore, we cannot reject that the distributions are identical.[37] 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 possibility is that they did not believe the information, perhaps because it conflicted with their experience. Recall that 30 percent 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.

The Effect of Subsidies in the Absence of Demand Interventions.—The coefficients on "Full subsidy" in Table 6 indicate the effect of the subsidies in the absence of any demand intervention, comparing the pure control households and the households who received only the subsidy, as shown in Figure 1. In column 1 (live birth),

[36] One of the 20 reported treatment and treatment interaction coefficients is significant at the 5 percent level.

[37] 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 percent. The null effect holds for men as well. When we split the sample by whether individuals initially over- or underestimated 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 (Supplemental Appendix Table B4). Finally, when we compare the effect of the village debate and the debate with mortality information, there is no difference in beliefs.

the coefficient is significant at 10 percent and larger than in the noninteracted specification reported in Table 2 (3.6 percentage points against 1.7 percentage points). This result could be interpreted as suggesting that, in the absence of demand-side interventions, the subsidy would have resulted in a decrease in births, and that the demand-side interventions played against the subsidies. However, the estimated impact is still modest: The point estimate translates into a 5.7 percent decrease in births. In addition, the results in columns 2, 3 and 4 show that there is no statistically significant effect on pregnancy and modern contraceptive use. In magnitude, the coefficients are very similar to what we find in Table 2. Moreover, we do not find evidence that there were effects of the demand-side interventions on beliefs or outcomes, so it is not clear why we would want to concentrate attention on the subgroup that did not receive them. That is why our preferred estimate remains the coefficient in Table 2, which has the greatest statistical power.

D. *Discussion: The Residual Explanation for High Fertility May Be 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 after addressing potential frictions influencing desired fertility, suggests that it may be time for policymakers 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.

Our survey data lend support to the view that there are still high returns to having many children in the context we study. First, individuals cite their need for old-age support. Supplemental Appendix Table A12 shows that 74 percent of men and 81 percent 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 percent 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 percent 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 fewer child than they desire, 17 percent of women say that they would not have enough children to perform tasks around the house, and 13 percent would not have enough children to work in their agricultural fields. Finally, children are a source of happiness: 14 percent of women say that they would be less happy if they had one fewer child than they want. 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.

V. 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 suggest that, in contexts similar to rural Burkina Faso, the relationship between free family planning, women's empowerment, and economic development is less straightforward than usually assumed.

Our results do *not* imply that efforts to improve access to modern contraception have no value. First, reproductive freedom is an important right. Second, some women, even if not many, may be financially constrained. Free access to contraception may enable them to reduce their fertility to their desired level or to better manage the timing of fertility. Third, we studied only married women. Access to contraception might affect age at marriage and delay first births, even without affecting the total fertility rate. 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. Fourth, we studied the complementarity between subsidies and two specific information treatments. Complementarities may exist with interventions targeting other nonfinancial barriers identified in the literature, such as distance/convenience, misperceptions of side effects and female mortality risks, or intrahousehold bargaining.

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. Future research should further investigate the conditions under which price reductions can be effective.

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), and these effects, moreover, were found in contexts where legal and social change was occurring in tandem (Goldin and Katz 2002). In lower-income countries, family planning programs also had a modest impact on fertility decline, with some exceptions (Miller and Babiarz 2016). Reflecting on the historical experience in the 1960s, Hartman (1997) concludes that international efforts (such as those by USAID) to expand access to contraception in low-income countries played a limited role in lowering fertility and curbing population growth. Conversely, in high-income countries, policies that provide financial incentives to households to have more children have small effects (e.g., Kim 2020; Aizer, Eli, and Lleras-Muney 2024). 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. (2024), that fertility levels are primarily determined by deep economic factors.

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