DECISION-MAKING POWER FOR WOMEN AND GIRLS: EVALUATING INTERVENTIONS IN SEXUAL AND REPRODUCTIVE HEALTH IN SUB-SAHARAN AFRICA

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A dissertation submitted to the faculty at the University of North Carolina at Chapel Hill in partial fulfilment of the requirements for the degree of Doctor of Philosophy in the Department of Health Policy and Management in the Gillings School of Global Public Health.

Chapel Hill
2020

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ABSTRACT

Wei Chang: Decision-making power for women and girls: Evaluating interventions in sexual and reproductive health in sub-Saharan Africa
(Under the direction of Sean Sylvia)

The capacity to exercise choice for women and girls is an important development objective, but evidence on how health policies and programs affect decision-making is lacking in low-resource settings. This study aims to assess three different health interventions that may improve women's and girls' decision-making power in key life choices in sub-Saharan Africa. The first intervention consists of legal reforms that reduce restrictions on abortion, which may allow adolescent girls and young women to stay in school longer by delaying marriage and childbearing. I use a difference-in-differences approach to analyze the impact of expanding the legal grounds for abortion on marriage, birth, and schooling rates among adolescent girls and young women in 18 countries. The second intervention addresses financial barriers that might limit women's ability to choose their preferred contraceptive methods. I use a propensity score approach combined with machine learning techniques to evaluate how free access to a broad contraceptive method mix affects women's contraceptive choice in eight countries with high unmet needs for family planning. The third intervention distributes HIV self-tests through women with multiple sexual partners in Kenya. I use an instrumental variable approach to assess whether disclosing HIV-negative status affects women's decision-making in intimate partner and transactional sex relationships. Each of these three analyses is presented as a different chapter with an overview that summarizes the results. Taken together, this study leverages rigorous econometric methods, fills important evidence gaps in the literature on gender and health, and informs policies to improve women's and girls' well-being in low-resource settings.

To mom, grandmas, and many other women before and next to me, for bringing me to where I am today
To Mark, for helping me find my voice and always understanding what it takes

ACKNOWLEDGEMENTS

I am grateful to my committee, Drs. Ashu Handa, Sally Stearns, Sean Sylvia, Harsha Thirumurthy, and Kat Tumlinson, for setting the example of compassionate researchers. I feel especially fortunate to have two advisors, Harsha and Sean, who trusted me as a colleague and supported me to pursue my own path in research. I would like to thank Drs. Jim Kahn and Elliot Marseille at the University of California San Francisco for bringing me to Africa for the first time and for inspiring me to stay curious.

I could not have completed this work without the unconditional love and support from my family who always have my back, especially during the Covid-19 pandemic. I would also like to thank my friends and chosen family at the Department of Health Policy and Management and the Carolina Population Center; getting to know this group of caring and talented fellow graduate students remains the best part of my PhD journey.

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LIST OF ABBREVIATIONS

2SLS	Two-stage least squares
2SRI	Two-stage residual-inclusion
ATE	Average treatment effects
CI	Confidence interval
DHS	Demographic and Health Surveys
DID	Difference-in-differences
GBM	Generalized boosted modeling
IPV	Intimate partner violence
IUD	Intrauterine devices
IV	Instrumental variables
KES	Kenyan shillings
LARC	Long-acting reversible contraceptives
LATE	Local average treatment effect
LMIC	Low-and middle-income countries
PMA	Performance Monitoring and Accountability
RCT	Randomized controlled trial
SARC	Short-acting reversible contraceptives
SSA	Sub-Saharan Africa
VCT	Voluntary counseling and testing

CHAPTER 1: INTRODUCTION

1.1 Specific Aims

Gender equality and empowerment of women and girls are fundamental human rights, a necessary foundation for a sustainable world, and one of the United Nations' Sustainable Development Goals.[1] Agency, or the capacity to define, pursue, and achieve goals free of violence or retribution, is central to the concept of empowerment and starts with decision-making.[2,3] Despite years of efforts, women and girls today continue to play a limited role in household decision-making, lack educational and economic opportunities, and have poor access to essential health services.[4] In particular, only half of women of reproductive age from developing countries can freely make their own decisions about sexual relations, contraceptive use, and health care.[5]

Interventions that aim to improve gender equality usually consider empowerment as a pathway to other health and development outcomes and encourage women and girls to become agents of change.[6–9] However, empowerment is a development objective in its own right in addition to its instrumental value.[4,7,10,11] Giving women and girls more choices and agency can be an effective approach to achieving gender equality, but evidence on whether and how health policies and programs affect decision-making in sub-Saharan Africa (SSA) is lacking.[3,7,12–14]

My long-term goal is to contribute to the achievement of gender equality in agency, health, and development through evidence-based policies and programs. Given the broad scope of this goal, this study aims to assess the impact of three interventions, namely abortion legal reforms, free access to expanded family planning services, and HIV-status disclosure, on specific applications of decision-making related to sexual and reproductive health. First, early pregnancy and childbearing impede women's and girls' ability to participate in, complete, and benefit from education.[15] Legal reforms that

expand the grounds under which abortion is permitted may allow adolescent girls and young women to stay in school longer by delaying marriage and childbearing responsibilities. Second, financial barriers may restrict women's ability to use their preferred contraceptive methods, especially long-acting reversible contraceptives (LARC) that - although more effective - have relatively high upfront costs.[16–19] Providing free access to a broad contraceptive method mix, including both LARC and short-acting reversible contraceptives (SARC), may increase contraceptive use, meet women's various fertility needs, and increase their autonomy in contraceptive decision-making in resource-limited settings. Third, women with multiple sexual partners who often engage in transactional sex are at increased risk of HIV infection, economic hardship, and relationship abuse, especially in a generalized HIV epidemic.[20,21] Disclosing HIV-negative status to sexual partners may inform women's decision-making in sexual relationships, allow them to negotiate higher prices for transactional sex, and facilitate safer sexual behaviors.

I tested the hypotheses above through three aims:

- Aim 1: Assess whether the impact of abortion legal reforms on marriage, birth, and schooling rates among adolescent girls and young women differed between ten countries that expanded the legal grounds for abortion and eight countries that did not over the period 1996 to 2015;
- Aim 2: Assess whether access to a broad contraceptive method mix was associated with an increase in contraceptive use, LARC use, and autonomy in contraceptive decision-making among women in eight countries with high unmet need for family planning services; and
- Aim 3: Assess whether disclosing one's own HIV-negative status to sexual partners affected intimate partner and transactional sex relationships among women with multiple sexual partners in Kenya.

I conducted heterogeneity analyses in each of the three aims to understand whether and how intervention effects differed among population subgroups based on demographic and socioeconomic characteristics, with a focus on the most vulnerable women and girls in each setting. Despite different contexts and mechanisms, the three research aims all focus on ways to improve women's and girls' agency for better health and well-being in SSA.

1.2 Significance

Gender gaps in access, opportunity, and voice persist in every part of the world in spite of the progress in maternal and child health over recent decades.[4,7] Policymakers have pursued women's and girls' empowerment as a means to unlock greater development potential in low- and middle-income countries (LMIC), but the ability to exercise choice, which defines empowerment, is also an important development objective in its own right.[3,10,14,22] Based on the "resources, agency, and achievements" framework of empowerment, agency is the process dimension of empowerment and focuses on decision-making.[2,23] Previous research has tended to concentrate on the instrumental impact of women's agency as a determinant of health and household welfare,[6,8,9,11–13,24,25] but the ability to make decisions and exercise choices is also an essential measure of empowerment independent of the achievement dimension.[7,22,23,26] Thus, research that examines the impact of policies and programs on women's and girls' decision-making power in its intrinsic value is needed for a more holistic approach to empowerment and policy evaluation.

To fill this gap, my dissertation aimed to assess how three specific health interventions affected women's and girls' decision-making power related to sexual and reproductive health. The ability to make decisions manifests in key life milestones such as marriage, childbearing, and schooling, which is the focus of Aim 1. In SSA, gender disparity in education persists as only 78 females completed upper secondary education for every 100 males, and no country has achieved gender parity in tertiary education.[27] Early childbearing impedes girls' educational attainment,[15,28–36] which contributes to adulthood gender gaps in labor force participation, earnings, and agency.[37] Legal reforms that expand the grounds under which abortion is permitted may improve women's and girls' educational attainment by offering the option to end an unintended pregnancy, providing access to safe reproductive health services, and delaying childbearing and parental responsibilities.[38,39] In Aim 1, I evaluated the impact of expanding abortion legal grounds on marriage, birth, and schooling outcomes among adolescent girls and young women in a region with slow but steady shift towards more liberal abortion laws.[40] The results provided evidence on the potential of legal reforms as a policy tool to protect girls' power to determine their own life paths.

Aim 2 also studied expanded access to health services but focused on the effects of costs on women's decisions in health services utilization. User fees for family planning were introduced in the 1980s in many LMIC to ensure the quality of health services and sustainability of health systems, but charging a non-zero price for family planning services may lower demand and prevent women from choosing the contraceptive methods that best suit their needs and preferences, especially for those on the lower end of wealth spectrum.[41–46] Free access to a wide range of contraceptive methods gives women more decision-making power to use the method of their choice when faced with other competing household demands. By differentiating contraceptive method types, Aim 2 took a closer look at contraceptive decision-making, assessed the impact of financial barriers on women's agency, and provided guidance to policies and programs on scaling up family planning services in SSA.

While Aim 1 and 2 used data from multiple countries in SSA, Aim 3 focused on one country and evaluated how disclosing HIV-negative status to sexual partners affected intimate partner and transactional sex relationships among women with multiple sexual partners in Kenya. Women who engage in transactional sex are at increased risk of HIV infection due to their concurrent sexual partnerships, inconsistent condom use, and risks of violence, criminalization, and marginalization.[20,21] Disclosing HIV-negative status to sexual partners may influence relationship stability, allow women to negotiate higher prices for transactional sex, and facilitate safer sexual behaviors. However, such a strategy might not increase women's agency in sexual relationships due to gender-based power dynamics. Existing literature has focused on HIV-positive women and has not explored the potentially empowering effects of disclosing HIV-negative status on women's agency in a generalized HIV epidemic. To illustrate these broader social and economic impacts, Aim 3 assessed the effects of disclosing HIV-negative status on women's intimate partner relationships, transactional sex prices, and sexual behaviors.

This study is significant because it demonstrated the potential and limitation of different public health interventions, from country-level policies to individual behaviors, in enhancing women's ability to exercise agency in resource-limited settings. Understanding the social and economic implications of health interventions, especially their differential impacts on the most vulnerable segments of the

population, is also important as countries in SSA continue to face hard choices in resource allocation in their efforts to scale up health services.

1.3 Innovation

This study is innovative because the three aims leverage rigorous econometric methods and study designs to fill important gaps in the literature. For Aim 1, previous studies have demonstrated that restrictive abortion laws negatively affect women's well-being and abortion reforms improve schooling and employment among minority groups in high-income settings.[47–51] However, no study has examined the causal effects of abortion laws on marriage, birth, and schooling outcomes among adolescent girls and young women in SSA. By using a pseudo-panel dataset constructed from multiple data sources, Aim 1 evaluated the effects of a large-scale policy intervention on decisions critical to the health and development of adolescent girls and young women. For Aim 2, although evidence from developed countries demonstrates that removing financial barriers shifts contraceptive method choice,[52,53] research on vouchers for contraceptives from SSA has produced mixed results.[54–58] Linking health facility and individual data from eight countries, Aim 2 combined traditional econometric methods and machine learning techniques to examine how community-level access to family planning services affected women's contraceptive use and decision-making. The results from this aim provided a nuanced understanding of the reach of financial subsidies and contribute to the literature on the effect of price for public health commodifies in SSA. For Aim 3, existing literature on HIV-status disclosure has focused on identifying interventions to safely disclose HIV-positive status as HIV-positive women are at increased risks of violence, discrimination, and relationship dissolution.[59–61] However, for women who have multiple sexual partners and often engage in transactional sex, disclosing HIV-negative status has important implications for their intimate partner and transactional sex relationships in a generalized HIV epidemic. Aim 3 leveraged the exogenous variation in HIV-status disclosure from a randomized design to assess whether women could use HIV-negative status to make informed decisions about their sexual relationships. The results from this study contribute to the small but growing literature on the broader benefits of expanded HIV testing services beyond identifying HIV-positive cases.[62–64]

1.4 Guide to the Dissertation

The remainder of this dissertation proceeds as follows. Chapter 2 examines the effects of abortion legal reforms on marriage, birth, and schooling outcomes among adolescent girls and young women. Chapter 3 looks at the effects of free access to a broad contraceptive method mix on women's contraceptive use and decision-making in eight countries with high unmet need for family planning services. Chapter 4 explores the effects of disclosing HIV-negative status on intimate partner and transactional sex relationships among women with multiple sexual partners in Kenya. Chapter 5 concludes.

CHAPTER 2: ABORTION LAWS AND LIFE CHOICES OF ADOLESCENT GIRLS AND YOUNG WOMEN IN SUB-SAHARAN AFRICA: A CROSS-COUNTRY ANALYSIS

2.1 Overview

Background: Early marriage and pregnancy impede the educational attainment of adolescent girls and young women in sub-Saharan Africa. Expanding the legal grounds for abortion makes it easier and safer to end unintended pregnancies, but it is not clear whether such high-level policy changes could delay marriage, reduce childbirth, and promote education among girls and young women.

Methods: Using data from 18 countries in sub-Saharan Africa, we estimate the effects of abortion liberalization on marriage, birth, and schooling rates among girls and young women aged between 13 and 22 years. We use a difference-in-differences approach, comparing outcomes in ten countries that expanded the legal grounds for abortion and eight countries where abortion laws remained restrictive over the period 1996 to 2015.

Results: Expanding legal grounds for abortion led to a reduction of 2.5 percentage points (95% confidence intervals [CI]: -0.050 – -0.001), or 8.6%, in the annual likelihood of marriage and 0.7 percentage point (95% CI: -0.013 – -.002), or 7.3%, in the annual likelihood of birth. The legal reforms were not associated with a statistically significant effect on schooling. Effects on marriage and birth were largest among those from younger age groups, rural areas, and lower wealth quintiles. The results were robust to several sensitivity analyses.

Conclusions: Expanding the legal grounds for abortion enhanced the ability of adolescent girls and young women to delay marriage and childbearing. Despite the lack of effects on schooling, this study highlights the broader implications of reproductive health policies for women's agency in low-resource settings.

2.2 Introduction

Early marriage and pregnancy impede the educational attainment of adolescent girls and young women in sub-Saharan Africa (SSA).[15,29,31,32,35,65] In 2018, only 78 girls completed upper secondary education for every 100 boys, and no country had achieved gender equality in tertiary education in this region.[27] Despite having the highest adolescent pregnancy rates in the world,[66] adolescent girls and young women in SSA still face pregnancy-based discrimination in schools that do not apply to their male counterparts, including punitive measures that expel pregnant girls from schools, lack of support for young mothers to return after childbearing, and restrictive gender norms that prioritize women's role in motherhood above all else.[67] The disparities in educational attainment early in life limit women's labor force participation and earnings, affect the health and development trajectories of both women and their children, and contribute to large gender gaps in decision-making in the household, community, and political sphere.[37]

Expanding the legal grounds for abortion may allow adolescent girls and young women to stay in school longer by offering the option to end unintended pregnancies, providing better access to safe reproductive health services, and delaying marriage and parenting responsibilities.[38,39,68] Studies of legal reforms in the U.S. in the 1970s and Mexico in 2007 showed that more liberal abortion laws increased induced abortions, delayed marriage, and lowered fertility by providing better access to abortion services.[69,70] The liberalization of abortion laws in the 1970s and 1980s was associated with women's higher educational attainment through delaying childbearing in Norway, Spain, and the U.S.[50,71,72]

Compared with high-income countries, the abortion rate is lower in SSA but abortion is still common: in 2010-2014, an estimated 15% of all pregnancies ended in abortion in SSA compared with 27% in developed countries.[73,74] Restrictive abortion laws force women to undergo unsafe procedures, put women at greater risk of physical harm, and disproportionally affect women from disadvantaged socioeconomic backgrounds.[40,48,68,74] For example, it is usually more difficult for women with limited economic means to access abortion services, especially safe procedures.[40,48] It is within the context of safe motherhood and reducing maternal mortality that the heads of government of the African Union approved the Protocol to

the African Charter on Human and People's Rights on the Rights of Women in Africa in 2003, which recommended state parties authorize medical abortion when continued pregnancy endangers the life, physical health, and mental health of the mother.[75]

However, it is not clear whether such high-level policy changes would translate to greater access to abortion services and influence marriage, childbearing, and education outcomes of adolescent girls and young women. Legal changes are only the first step to make abortion safer and more accessible; clear implementation guidelines, trained healthcare providers, and resources to scale up reproductive health services are all important prerequisites.[40,76,77] Even with good access, more liberal abortion laws might not increase induced abortion because countries that support women's reproductive rights often provide better family planning services, reducing unintended pregnancies and the need for abortion.[73,78] Additionally, many women do not have accurate knowledge of current abortion laws, making it difficult for them to operationalize increased legal access. Moreover, since the majority of abortions are obtained by married women,[73,79] changes in abortion policies might not affect adolescent girls and young women in SSA as their marriage and childbearing decisions are often driven by poverty, inequitable gender norms, and socio-cultural values.[80,81] Lastly, when given the option to delay marriage or childbearing, adolescent girls and young women might still choose to drop out of school due to economic, social, and structural barriers that play a larger and more direct role in girls' schooling choices than early marriage and pregnancy.[27]

Despite the poor reproductive health and education outcomes among adolescent girls and young women in SSA and the need to assess the impacts of abortion laws in the region, existing evidence on the link between abortion laws and outcomes among adolescent girls and young women is concentrated in high-income settings and the only study from SSA used a cross-sectional approach.[82] The objective of this study, therefore, is to estimate the effects of liberalized abortion laws on women's and girls' key life choices in marriage, childbearing, and schooling in SSA. We used data from 18 countries spanning 20 years to examine whether expanding the legal grounds for abortion leads to greater development opportunities for adolescent girls and young women in SSA.

2.3 Methods

Using data from the Demographic and Health Surveys (DHS) supplemented by other policy datasets, we estimated the effects of expanding legal grounds for abortion on key life choices of adolescent girls and young women in 18 countries in SSA. Specifically, we used a difference-in-differences (DID) approach to compare trends in marriage, births, and schooling in countries that expanded abortion legal grounds to those in countries that did not in the 20-year period from 1996 to 2015. We examined the policy impacts among subgroups and performed sensitivity analyses to check the robustness of the study findings.

2.3.1 Data and sample

We combined data from three sources for the study. First, country-year data on abortion legality from 1996 to 2015 came from the World Population Policies Database maintained by the Department of Economic and Social Affairs at the United Nations.[83] This dataset provides information on government views and policies from 197 countries on reproductive health. The surveys were conducted decennially from 1976 and biennially from 2001 to 2015, but data on abortion legality were only collected since 1996. The policy data were based on a detailed review of national plans and strategies, program reports, legislative documents, and official responses to the United Nations Inquiry among Governments on Population and Development.[84] Second, individual-level survey data were extracted from the Women's Questionnaire in the DHS conducted between 1997 to 2018 in 18 countries in SSA (Benin, Burkina Faso, Burundi, Cameroon, Democratic Republic of Congo, Cote d'Ivoire, Ethiopia, Keya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Niger, Nigeria, Rwanda, Senegal, and Zimbabwe) harmonized by the IPUMS-DHS project.[85] We excluded five countries (Ghana, Guinea, Tanzania, Uganda, and Zambia) that already had relatively liberal abortion laws in 1996 (i.e., where abortion was allowed by law to preserve a woman's mental health). The dataset includes each woman's year of birth, total years of education, birth history, and household characteristics. Third, country-year data on primary school starting age were obtained from the World Development Indicator database from the World Bank.[86]

Using the DHS data, we constructed a longitudinal cohort with repeated observations for all female respondents that were at most 22 years of age in 1996 and at least 12 years of age in 2015. The analytical dataset consisted of binary variables to indicate marital status, birth, and school enrollment for each person-year together with other individual characteristics.

2.3.2 Measures

Abortion legal reform. We measured abortion legal reform in individual countries first as a binary indicator of whether abortion was allowed on all three grounds in a specific year: to save a woman's life, to preserve a woman's physical health, and to preserve a woman's mental health. Although truly liberal laws in the abortion legality continuum should permit abortion for socioeconomic reasons or without restriction as to reason, our definition of "liberal" abortion laws accommodates the fact that Zambia was the only country in SSA with DHS data where abortion was broadly legal as of December 2017.[40] Among the 18 countries that had restrictive abortion laws in 1996, ten countries had expanded the legal grounds for abortion by 2015 while the abortion laws in eight countries remained highly restrictive. Given the structure of the data collection schedule, we assumed abortion laws did not change in the years without policy surveys (e.g., we assumed the abortion laws in 1997, a year without abortion policy data, remained the same as the laws in 1996).

Since countries that eventually expanded abortion legal grounds to include "mental health" started from different places in the continuum of abortion legality, we recoded abortion legal reform to differentiate between countries with "large" versus "small" expansion of abortion policy. A country that initially only allowed abortion to save a woman's life and liberalized their laws to allow abortion to save a woman's life, to preserve a woman's physical health, and to preserve a woman's mental health was coded as a "large-expansion" country. In contrast, countries that initially allowed abortion to preserve a woman's physical health in addition to saving life were coded as "small"-expansion countries. Figure 1 shows the changes in abortion legality over 20 years in the 18 countries included in the study.

Outcome variables. We used similar methods to create three binary variables to measure marital status, childbirth, and school enrollment in each year before an individual turned 22 years of age. For

marital status, we created a variable to indicate whether an individual was married in each calendar year based on year of interview, an individual's age in the survey year, and her age of first marriage or cohabitation. For childbirth, we created a variable to indicate whether a woman or girl gave birth in a specific year based on the year of birth for all children she had. For school enrollment, we created a variable to indicate whether a woman or girl was in school based on her total years of education and country-year-specific primary school starting age. Specifically, we first calculated the individual's year of birth by subtracting age from the year of interview. Then, we calculated the year when an individual ended schooling by summing up the year of birth, the number of years before primary school for each birth cohort by country, and the total years of education for each individual. This calculation assumed that all children in a birth-cohort in a specific country started primary school at the same age and there was no interruption in individuals' school attendance.

Control variables. Individual characteristics, including age, residence (rural or urban), and household wealth quintiles, were extrapolated as control variables from the year when the DHS surveys were conducted. This assumes there was no physical or social mobility among the women and girls in the dataset (e.g., an individual surveyed in rural areas was considered a rural resident across all survey years).

2.3.3. Statistical analyses

We estimated the effects of expanding the legal grounds for abortion using the following two-way fixed-effect linear probability model:

$$Y_{ijt} = \alpha + \beta Treat_{jt} + c_j + d_t + Z_{ijt}'\gamma + \varepsilon_{ijt}$$

where Y_{ijt} is the outcome variable set to 1 if individual i from country j in year t was married, gave birth, or was enrolled in school, $Treat_{jt}$ is set to one if individual i resided in country j that had expanded legal grounds for abortion laws in year t, c_j is a full set of country dummies to control for time-invariant country characteristics, d_t is a full set of year dummies to account for secular trends, and Z_{ijt} is a vector of descriptive characteristics including age, rurality, and household wealth index quintiles. The coefficient of interest is β, which represents the DID estimate of the abortion legal reform, or the average annual change

in the likelihood of schooling among women and girls in countries with more liberal abortion laws. We clustered standard errors at the country level.[87]

To test whether the effects vary by the extent of legal reform, we fitted a linear probability model specified below:

$$Y_{ijt} = \alpha + \beta_1 Large_{jt} + \beta_2 Small_{jt} + c_j + d_t + Z'_{ijt}\gamma + \varepsilon_{ijt}$$

where $Large_{jt}$ is set to one if individual *i* resided in country *j* that had expanded legal grounds for abortion from only allowing abortion to save a woman's life in year *t* and $Small_{jt}$ is set to one if individual *i* resided in country *j* that had expanded legal grounds for abortion from allowing abortion to save a woman's life and to preserve her physical health in year *t*. The coefficients β_1 and β_2 capture the impacts of different extent of legal reform (i.e., "large" expansion vs. "small" expansion). We used a Wald test to assess the equality hypothesis between β_1 and β_2.

We conducted additional subgroup and sensitivity analyses. First, to examine whether the treatment effects vary by key demographic variables, we repeated the main analyses for subgroups based on individuals' age group, residence (urban vs. rural), and household wealth quintile. Second, we checked whether the main assumption of the DID design was valid, i.e., the "parallel trend" assumption that countries with more liberal abortion laws would have identical trends in the outcomes as countries with more restrictive abortion laws in the absence of any legal reform. Although this could not be directly tested, we fit a linear probability model that excluded observations from treatment countries in years after the policy change to compare the pre-trends between treatment and comparison countries. Third, we excluded one country at a time from the dataset to check whether the results were driven by any single country. Fourth, because the policy survey was only conducted in selective years during the 20-year period, we also restricted the analysis to only years where abortion legality was surveyed. Lastly, we tested the effects of abortion liberalization on marriage and schooling in a sample that included also men and boys using a triple-difference model. Specifically, we hypothesized that abortion liberalization should

not affect the schooling of young men and boys and would affect their marriage, if at all, to a lesser degree than the marriage of women and girls.

Statistical tests were 2-sided and we set the statistical significance at $p<.05$. Analyses were performed using Stata, version 15.1 (StataCorp LLC).

2.4 Results

The full sample contained 1,786,310 woman-year observations, of which 994,168 (55.7%) were from the ten countries that expanded abortion legal grounds between 1996 and 2015 and the remaining 792,142 (44.3%) were from the eight comparison countries (see Table 1). Among the 270,422 individuals in the sample, at the age of 15 years, 3% were married, 0.8% gave birth, and 23% were enrolled in school; at age 22, 58.7%, 10.5%, and 1.8% were married, gave birth, and enrolled in school respectively. Detailed information showing country-specific marriage, birth, and schooling rates are presented in Table A1.

The main results are presented in Table 2. The average annual likelihood of marriage was 2.5 percentage points lower after the legal reform (95% confidence interval [CI]: -0.050 – -0.001), or a decrease of 8.6% relative to a control mean of 28.8%. The average annual likelihood of giving births was 0.7 percentage point lower after the legal reform (95% CI: -0.013 – -.002), or a decrease of 7.3% relative to a control mean of 9.5%. The legal reform was not associated with a statistically significant effect on schooling outcomes. While the effect of "small" expansion (i.e., from "saving life and preserving physical health" to "saving life and preserving physical and mental health") on marriage was significant, we were not able to detect a statistically different effect between "small" and "large" expansions on marriage (p-value = 0.092). The effect on birth was different by the extent of legal change and "small" expansion had a larger impact than "large" expansion on birth (p-value = 0.027).

Heterogeneity. The effects of abortion legal expansion varied considerably by age groups (Table 3). For those between 13 and 15 years of age, expanding abortion legal grounds reduced the likelihood of marriage by 0.8 percentage point (95% CI -0.016 – -0.000), a sizable 16% reduction from before the expansion, but did not affect birth or schooling. For those between 16 and 18 years of age, the legal change reduced the likelihood of giving birth by 0.5 percentage points (95% CI -0.010 – -0.001) and

"small" expansion had stronger effects than "large" expansion. In "small" expansion countries, there is also a significant reduction in marriage by 2.4 percentage points (95% CI -0.044 – -0.005) among those between 16 and 18 years of age. For those above 18 years of age, changes in abortion laws had no statistically significant effect on any of the three outcomes despite larger point estimates of marriage and birth than those of younger groups.

Similarly, the effects of abortion legal expansion varied by wealth quintiles, assuming individuals' household economic status remained the same as that of the DHS survey year (Table 4). For women and girls from the poorest wealth quintile, the legal change lowered the likelihood of marriage and birth by 6.0 percentage points (95% CI -0.094 – -0.026) and 1.7 percentage points (95% CI -0.028 – -0.005) respectively. In addition, "small" expansion increased school attendance among this poorest group by 3.5 percentage points (95% CI 0.001 – 0.069). For those from the richest wealth quintile, the legal change did not affect any of the three outcomes. For those in between, the legal change reduced the likelihood of birth but not marriage.

In terms of residence, abortion legal reform only affected those from rural but not urban areas (Table 5). For women and girls who lived in rural areas (at least in the DHS survey years), expanding abortion legal grounds was associated with a reduction of 3.6 percentage points (95% CI -0.067 – -0.005) in the annual likelihood of marriage, or 10.4% from before the expansion. The effect on birth was of similar magnitude at 1.2 percentage points (95% CI -0.019 – -0.005) or 10.3% from before the expansion. Moreover, "small" expansion was associated with a 5.1 percentage point increase (95% CI 0.011 – 0.091), or 19.8%, in schooling compared to no expansion and was statistically significantly different from "large" expansion (p-value = 0.017).

Sensitivity analyses. We tested the robustness of the main results with the following sensitivity analyses. First, the test for parallel pre-trends showed that while marriage and birth were declining in all countries during the 20-year period, the trends in marriage, birth, or schooling did not differ between the treatment and comparison countries in the years before legal reforms (Table 6).

The effects of legal reforms were not sensitive to excluding any single country's data, except for the effect on schooling when Senegal was dropped from the dataset (Figure 2). Without Senegal, the expansion of abortion legal grounds was associated with a 2.9 percentage point increase in schooling (Table A2; 95% CI: 0.003 – 0.051). This might be explained by other policy changes unique to Senegal that affected schooling independent of abortion laws. According to the UNESCO Institute for Statistics, only two countries among the 18 countries in the dataset changed the duration of compulsory education between 1998 and 2015: in Senegal, a comparison country, compulsory education duration was increased from six to 11 years in 2004 and in Kenya, a treatment country, compulsory education duration was increased from eight to 12 years in 2013.[88] Assuming compulsory education policies were implemented well, schooling in Senegal could have increased due to the effect of longer compulsory education. Thus, including Senegal as a comparison country would have violated the common trend assumption of DID. Similarly, the effect of abortion legal expansion in Kenya, if any, might have been partially attributed to the change in compulsory education duration, but the magnitude of bias might be smaller since the change in compulsory education occurred towards the very end of the 20-year period in 2013. To minimize the potential bias due to changes in compulsory education duration, we dropped Kenya and Senegal and repeated the main analyses (Table A3), which showed that abortion legal expansion was associated with a 3.0 percentage point, or 8.7%, increase in the likelihood of being in school (95% CI 0.001 – 0.059). Similar to the patterns in marriage and birth, the effect of "small" expansion was stronger than the effect of "large" expansion. However, we were not able to rule out other potential confounders.

When the dataset included only years when abortion policy was surveyed, the effect on marriage was no longer statistically significant (Table A4). However, the average annual likelihood of giving birth was still 1.2 percentage point lower (95% CI -0.023 – -0.001), or 12.8%, after the expansion of abortion legal grounds.

The triple-difference models showed that abortion legal changes did not affect men's marriage or schooling (Table A5). The coefficients of the treatment and female interaction term had the expected signs but were not statistically significant in the full sample. However, abortion legal expansion had

gender-differential effects for older age groups, reducing the likelihood of marriage by 5.1 percentage points (95% CI: -0.098 – -0.003) and 5.8 percentage points (95% CI: -0.116 – -0.000) for women aged 16 – 18 years and 19 – 22 years respectively compared to their male counterparts. In addition, in "small-expansion" countries, abortion legal change reduced the likelihood of marriage in the full sample by 6.5 percentage points (95% CI: -0.125 – -0.006) among women compared to men, assuming men were not affected by the legal change.

2.5 Discussion

Abortion laws play a critical role in adolescent girls' and young women's ability to access healthcare services, make reproductive decisions, and plan for their future. For policymakers, it is important to understand the broader implications of abortion laws as 15% of all pregnancies end in abortion in Africa.[73] To our knowledge, this is the first cross-country study in SSA that assessed the impacts of abortion laws on key life choices of adolescent girls and young women who usually have limited access to family planning services and face gender-based barriers to pursuing development opportunities.[27,89]

Using quasi-experimental methods and cross-country data sources, this study found that expanding abortion legal grounds was associated with a reduction of more than 7% in the annual likelihood of marriage and birth among adolescent girls and young women in SSA. Although legal reforms might coincide with changes in other socioeconomic factors and laws alone do not guarantee access to reproductive health services,[76,77,79,90,91] a country's decision to expand legal grounds for abortion nonetheless signals stronger political will to protect women's health and human rights and is likely to be correlated with greater access to reproductive health services. The results of this study demonstrated such state-level policy instruments enhanced young women's and girls' ability to make decisions about marriage and childbearing.

The subgroup analyses showed that the liberalization of abortion laws was especially beneficial to women who were younger, in rural areas, and from lower wealth quintiles. Since an estimated 3 million adolescent girls were married annually in SSA,[92] a 7% reduction would roughly translate into an effect that allows more than 200,000 girls to avoid child marriage each year. Allowing adolescents to postpone

births has major implications for maternal and child health in addition to girls' development opportunities.[93] Pregnancy and childbirth complications are the leading cause of death for girls aged 15 – 19 years in developing countries and babies of adolescent mothers face higher risks of low birth weight, preterm delivery, and severe neonatal conditions.[94,95] Our results also show that such legal reforms were progressive in that they helped those with the least wealth avoid childbirth and marriage. This is a promising finding since these women are most likely to experience complications from unsafe abortions whereas wealthier women are more likely to access abortion services even in the absence of legal reforms.[48]

This study also found that "small" expansion of abortion legal grounds had stronger effects than "large" expansion, which seems counterintuitive as it suggests abortion reform would be less effective if a country took a larger step towards more liberal laws on the abortion legality continuum. However, all countries that had a "large" expansion of abortion laws started with extremely restrictive abortion policies in 1996 (i.e., only allowing abortion to save a woman's life) while all countries that had a "small" expansion started with less restrictive abortion policies (i.e., allowing abortion to save a woman's life and preserve physical health). This difference in the starting point may reflect how ready a country's health sector was to scale up its reproductive health services after the legal change. For example, a "small"-expansion country might have had more trained healthcare workers, better healthcare infrastructure, and more accepting social norms about abortion before the reform. As a result, women and girls could obtain an abortion under the ground of "preserving mental health" after the reform. By comparison, in a "large"-expansion country where abortion was rarely performed before the legal change, the health sector might be less prepared to expand reproductive health services and its providers who were used to prior restrictive laws might be more likely to impose their personal bias, posing significant barriers for women and girls to access abortion even with the legal reform. Although we are unable to draw definitive conclusions, the larger effects of "small" expansion might indicate the limitation of high-level policy change alone without social norm shifts or adequate resources, instead of evidence for the lack of effects of more liberal laws.

Despite the sizable effects on marriage and birth, the study did not find any downstream effect of abortion legal reform on schooling, except for those from the lowest wealth quintile in "small"-expansion countries. In contrast, previous studies on abortion liberalization in the 1970s and 1980s from developed countries have shown a strong link between abortion laws and young women's education outcomes through delayed marriage and childbearing, although such effects might have only occurred to subgroups of population (e.g., black but not white women in the U.S.) and at specific educational levels (e.g., college but not high school in Norway and vice versa in Spain).[50,71,72] Since child marriage and early childbearing are known factors that limit girls' educational potential in SSA,[28,32–35,65,91,96,97] it is puzzling that the reforms' effects on marriage and childbearing did not translate into higher school enrollment in this study. This might be because early marriage and pregnancy only explained less than 20% of school dropouts in the case of francophone Africa.[15] Alternatively, women and girls who chose to obtain an abortion to delay marriage or childbearing under the more liberal laws might have dropped out of school even if they did not become pregnant, perhaps due to economic shocks. A closer examination of country-specific abortion laws, education systems, and socioeconomic contexts similar to the analyses conducted in developed countries will provide stronger evidence on whether and how abortion laws affect education in low-resource settings.

This study has several limitations. First, we used selective and distinct legal grounds to categorize abortion laws at the country level, which did not capture the complex policy nuances of abortion policies or reflect how these laws were interpreted in practice.[76,98] However, the oversimplified classification of abortion laws made the cross-country comparison possible and were used by the best longitudinal policy data we had access to.[83] Future studies could take advantage of other measures of abortion laws, such as the Global Abortion Policies Database,[99] to develop a more systematic framework for abortion legality and assess the effects on access to services and of other dimensions of abortion laws. Second, although the DID approach controlled for unobserved time-invariant confounders, other factors that were not fixed over time might still have biased the results. The fact that excluding Senegal from the original dataset produced different effect estimates on schooling led us to consider the changes in compulsory education

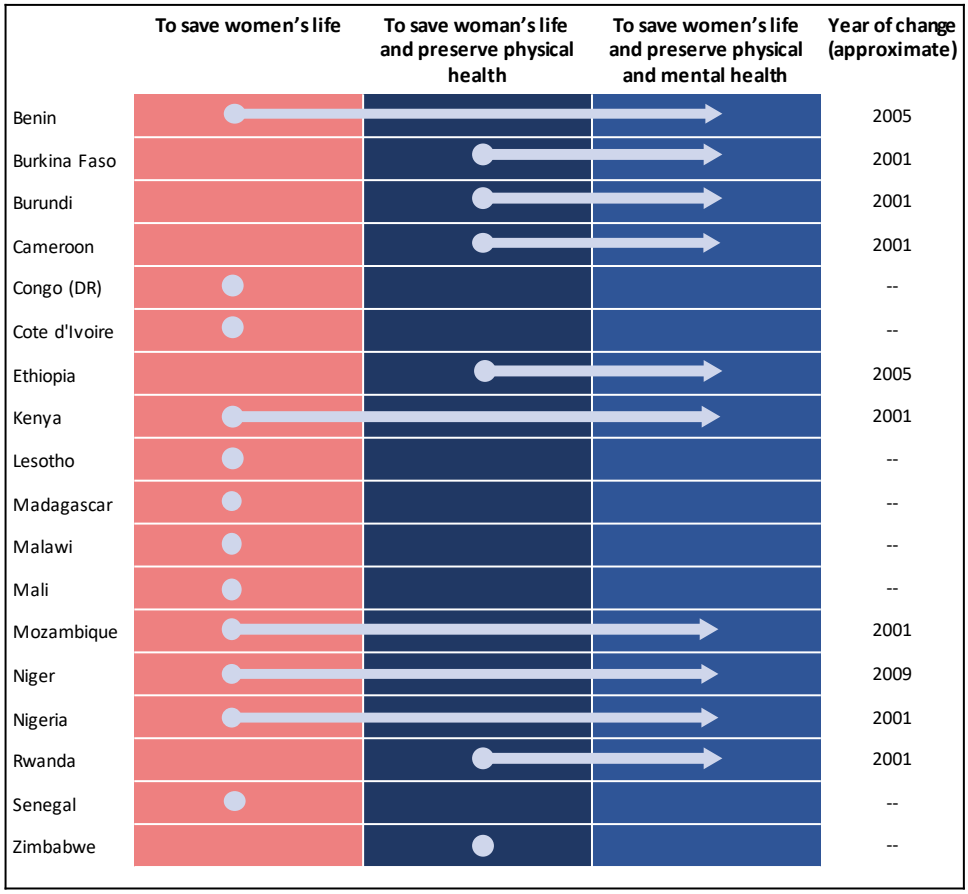
duration, which is by no means the only country-specific time-varying factor. Nonetheless, the main results were not affected by the omission of any country other than Senegal. Third, we extrapolated data from cross-sectional surveys to construct a pseudo-panel dataset, an approach that has been used in other studies,[100,101] which created considerable measurement errors in key demographic variables such as residence or wealth. However, these errors were unlikely to be related to abortion policy changes and the subgroup analyses showed stronger effects among those from rural areas and poorer backgrounds. Fourth, to estimate an individuals' schooling status, we assumed every cohort of girls in a country started primary school at the same age while in reality delayed entry to primary school is common.[102] Since the sample was restricted to women and girls who were still in school at the age of 12, it is possible that some observations may have been misclassified as "out of school", but it is unlikely that such measurement errors are correlated with the abortion policy change. Fifth, related to the school-attendance criterion in sample selection, individuals in our dataset are a relatively privileged group as only 67% of women and girls completed primary education in SSA in 2018.[27] This limits the generalizability of the study findings. Sixth, we chose to use linear models for easier interpretability of parameter estimates. However, such models could yield out-of-range predictions when the true probabilities are close to 0 or 1, as in the case of the birth outcome we examined. Non-linear models, such as logistic regressions, might fit the data better for rare outcomes. Last, we did not examine how abortion laws were applied in practice or assess their effects on abortion incidence. Previous studies found that abortion rates were similar regardless of the abortion legal grounds in a country.[73] The effects of laws on marriage or births might be spurious if more liberal laws did not reduce the number of abortions. However, a country's decision to adopt more liberal abortion laws is likely to prelude or reflect a shift in social norms that are more supportive of women's agency, which enhances the ability of adolescent girls and young women to decide the timing of marriage and childbearing independent of fertility outcomes.

2.6 Conclusions

Policy reforms that expanded the legal grounds for abortion were associated with reductions in marriage and birth but had no effect on schooling among adolescent girls and young women in SSA.

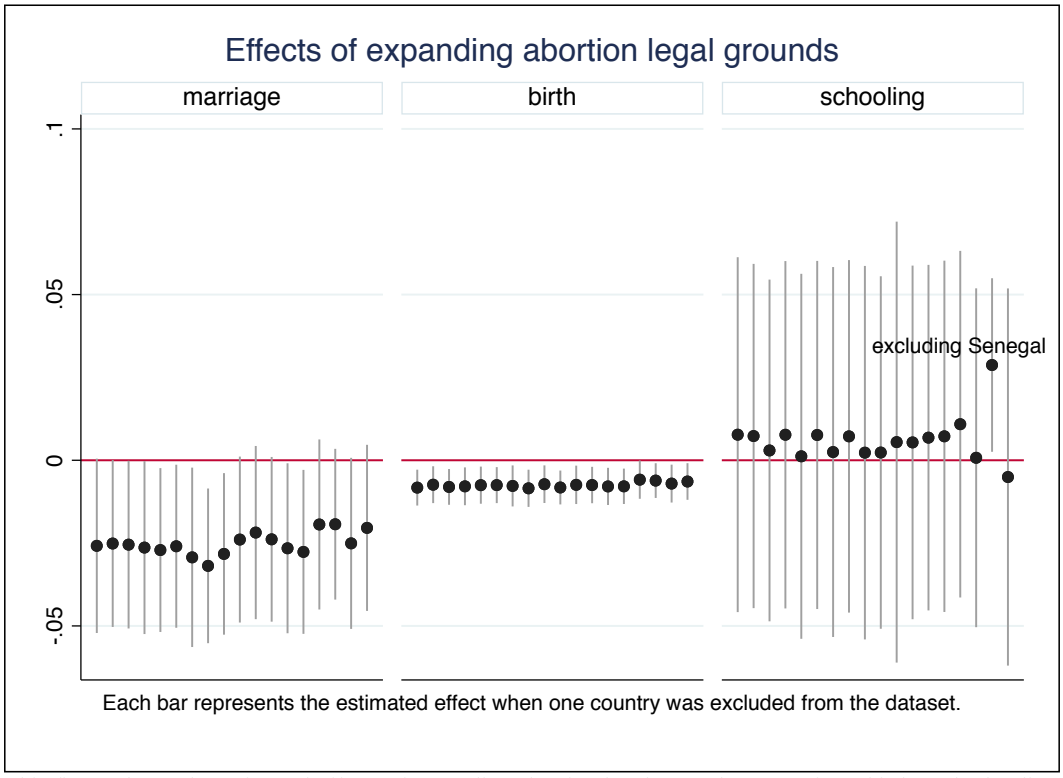
Whether such reforms improved access to reproductive health services or merely signaled greater political will to protect women's health and rights, the liberalization of abortion laws enhanced the ability of adolescent girls and young women to make key life choices, had important implications for maternal and child health, and were especially beneficial to those with the fewest resources. This study adds to our understanding of the broader implications of reproductive health policies to women's agency beyond saving lives from unsafe abortion practices.

FIGURE 1: Changes in legal grounds for abortion between 1996 and 2015



This figure shows the changes in the legal grounds under which abortion was allowed in each country from 1996 to 2015 based on the World Population Policies Database – 2015 Revision (https://esa.un.org/poppolicy/wpp_datasets.aspx). The surveys on abortion policies were conducted first in 1996 and then biennially from 2001 to 2015. Given these gaps in survey years, the "Year of change" data are approximate. For example, in Benin, abortion was only allowed under the ground to save women's life in 1996. By 2015, Benin had expanded the legal grounds to allow abortion to preserve women's physical and mental health. The legal change, represented by an arrow in the figure, occurred on or before 2005 (a year when abortion policy was surveyed). By comparison, in Congo (DR), abortion was only allowed to save women's life in 1996 and this did not change in 2015, represented by a dot in the figure.

FIGURE 2. Effects of expanding abortion legal grounds, excluding one country at a time



This figure shows the estimated effects of expanding abortion legal grounds on marriage, birth, and schooling using a dataset that originally included 18 countries from sub-Saharan Africa. Each bar represents the estimated effect when excluding one country at a time from the original dataset.

TABLE 1. Sample description

Indicator	Treated countries	Comparison countries	Full sample
Number of individual-year observations	994,168	792,142	1,786,310
Number of individual-year observations after legal change	655,161	--	655,161
Number of individuals in sample	151,466	118,956	270,422
Number of countries in sample	10	8	18
Marriage rate per 1,000 person-years	240.7	301.1	267.5
Birth rate per 1,000 person-years	84.6	96.7	90.0
Schooling rate per 1,000 person-years	375.4	338.3	358.9

Note: Treated countries (year of change in abortion legality based on the World Population Policies Database): Benin (2005), Burkina Faso (2001), Burundi (2001), Cameroon (2001), Ethiopia (2005), Kenya (2001), Mozambique (2001), Niger (2009), Nigeria (2001), Rwanda (2001). Comparison countries: Democratic Republic of Congo, Cote d'Ivoire, Lesotho, Madagascar, Malawi, Mali, Senegal, and Zimbabwe. Marriage, birth, and schooling rates are calculated as simple means across all person-years.

TABLE 2. Effects of expanding abortion legal grounds on marriage, birth, and schooling

	Marriage	Birth	Schooling
Post-reform	-0.025*	-0.007**	0.006
	[-0.050,-0.001]	[-0.013,-0.002]	[-0.045,0.057]
Small	-0.034*	-0.009*	0.024
	[-0.067,-0.002]	[-0.017,-0.002]	[-0.031,0.078]
Large	-0.018	-0.006	-0.008
	[-0.050,0.013]	[-0.013,0.001]	[-0.058,0.043]
***P*-value (Large = Small)**	0.092	0.027	0.139
Age	0.075***	0.024***	-0.088***
	[0.065,0.085]	[0.021,0.028]	[-0.095,-0.082]
Rural	0.032**	0.010**	-0.049***
	[0.010,0.053]	[0.004,0.016]	[-0.071,-0.026]
Poorer	-0.029**	-0.009***	0.038***
	[-0.045,-0.013]	[-0.013,-0.004]	[0.021,0.055]
Middle	-0.058***	-0.021***	0.091***
	[-0.084,-0.032]	[-0.031,-0.011]	[0.061,0.120]
Richer	-0.089***	-0.034***	0.149***
	[-0.117,-0.060]	[-0.048,-0.021]	[0.108,0.191]
Richest	-0.169***	-0.063***	0.292***
	[-0.202,-0.136]	[-0.080,-0.046]	[0.249,0.334]
Observations	1786310	1786310	1786310
Adjusted R^2	0.2669	0.0693	0.3393
Control mean	0.288	0.095	0.350

95% confidence intervals in brackets

Included country and year fixed effects. Standard errors clustered at country level. Small: abortion legal grounds expanded from "to save life & preserve physical health" to including "to preserve mental health." Large: abortion legal grounds expanded from "to save life" only to including "to preserve mental health". Included only individuals that were still in school at age 12.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 3. Effects of expanding abortion legal grounds by age group

	13-15 years of age	16 – 18 years of age	19-22 years of age
Panel A: Marriage			
Treat	-0.008*	-0.017	-0.033
	[-0.016,-0.000]	[-0.037,0.002]	[-0.083,0.017]
Small	-0.005	-0.024*	-0.065
	[-0.011,0.001]	[-0.044,-0.005]	[-0.147,0.016]
Large	-0.011	-0.011	-0.008
	[-0.022,0.001]	[-0.038,0.015]	[-0.058,0.042]
P-value (Large = Small)	0.074	0.057	0.263
Observations	591096	569924	625290
Control mean	0.049	0.246	0.561
Panel B: Birth			
Treat	-0.001	-0.005*	-0.010
	[-0.004,0.002]	[-0.010,-0.001]	[-0.025,0.005]
Small	-0.001	-0.007**	-0.015
	[-0.005,0.003]	[-0.012,-0.003]	[-0.040,0.010]
Large	-0.001	-0.003	-0.007
	[-0.005,0.003]	[-0.010,0.003]	[-0.021,0.007]
P-value (Large = Small)	0.747	0.010	0.374
Observations	591096	569924	625290
Control mean	0.015	0.087	0.181
Panel C: Schooling			
Treat	0.006	0.017	0.003
	[-0.080,0.092]	[-0.044,0.077]	[-0.022,0.029]
Small	0.030	0.027	0.027
	[-0.065,0.125]	[-0.041,0.095]	[-0.007,0.060]
Large	-0.013	0.008	-0.015
	[-0.095,0.069]	[-0.054,0.071]	[-0.033,0.004]
P-value (Large = Small)	0.167	0.636	0.086
Observations	591096	569924	625290
Control mean	0.653	0.318	0.079

95% confidence intervals in brackets

Included rural/urban, wealth quintile, country, and year fixed effects. Standard errors clustered at country level. Small: abortion legal grounds expanded from "to save life & preserve physical health" to including "to preserve mental health". Large: abortion legal grounds expanded from "to save life" only to including "to preserve mental health".

Included only individuals that were still in school at age 12.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4. Effects of expanding abortion legal grounds by wealth quintiles

	Poorest	Poorer	Middle	Richer	Richest
Panel A: Marriage					
Treat	-0.060**	-0.044	-0.033	-0.027	-0.006
	[-0.094,-0.026]	[-0.095,0.007]	[-0.071,0.004]	[-0.056,0.001]	[-0.029,0.017]
Small	-0.060**	-0.057*	-0.042	-0.041*	-0.015
	[-0.094,-0.026]	[-0.105,-0.009]	[-0.085,0.001]	[-0.077,-0.006]	[-0.047,0.017]
Large	-0.060*	-0.036	-0.028	-0.020	0.003
	[-0.107,-0.014]	[-0.100,0.028]	[-0.074,0.017]	[-0.055,0.015]	[-0.022,0.028]
P-value (Large = Small)	0.003	0.065	0.134	0.074	0.535
Control mean	0.392	0.356	0.320	0.295	0.220
Panel B: Birth					
Treat	-0.017**	-0.015*	-0.014*	-0.008*	0.000
	[-0.028,-0.005]	[-0.028,-0.003]	[-0.024,-0.003]	[-0.016,-0.000]	[-0.006,0.006]
Small	-0.007	-0.016*	-0.014**	-0.010	-0.004
	[-0.019,0.004]	[-0.029,-0.002]	[-0.024,-0.004]	[-0.024,0.003]	[-0.012,0.004]
Large	-0.024***	-0.015	-0.013	-0.007	0.004
	[-0.037,-0.012]	[-0.031,0.001]	[-0.026,0.000]	[-0.015,0.001]	[-0.002,0.010]
P-value (Large = Small)	0.003	0.038	0.016	0.131	0.074
Control mean	0.131	0.121	0.108	0.097	0.069
Panel C: Schooling					
Treat	0.014	0.023	0.008	0.015	0.007
	[-0.019,0.048]	[-0.022,0.068]	[-0.067,0.082]	[-0.049,0.080]	[-0.037,0.051]
Small	0.035*	0.040	0.027	0.045	0.026
	[0.001,0.069]	[-0.007,0.087]	[-0.055,0.109]	[-0.032,0.122]	[-0.023,0.074]
Large	-0.003	0.013	-0.003	-0.000	-0.012
	[-0.030,0.025]	[-0.032,0.058]	[-0.077,0.071]	[-0.063,0.063]	[-0.050,0.025]
P-value (Large = Small)	0.014	0.058	0.366	0.200	0.134
Control mean	0.184	0.227	0.281	0.336	0.470
Observations	158254	230557	305921	407809	683769

95% confidence intervals in brackets
Included age, urban/rural, country, and year fixed effects.
Standard errors clustered at country level.
Small: abortion legal grounds expanded from "to save life & preserve physical health" to including "to preserve mental health".
Large: abortion legal grounds expanded from "to save life" only to including "to preserve mental health".
Included only individuals that were still in school at age 12.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 5. Effects of expanding abortion legal grounds by rurality

	Marriage		Birth		Schooling	
	Urban	**Rural**	**Urban**	**Rural**	**Urban**	**Rural**
Treat	-0.013	-0.036*	-0.003	-0.012**	-0.009	0.025
	[-0.033,0.006]	[-0.067,-0.005]	[-0.007,0.002]	[-0.019,-0.005]	[-0.075,0.058]	[-0.013,0.064]
Small	-0.019	-0.047*	-0.006	-0.012*	0.005	0.051*
	[-0.043,0.005]	[-0.085,-0.009]	[-0.012,0.000]	[-0.023,-0.001]	[-0.065,0.074]	[0.011,0.091]
Large	-0.008	-0.028	-0.000	-0.012**	-0.019	0.007
	[-0.036,0.019]	[-0.065,0.009]	[-0.007,0.007]	[-0.020,-0.004]	[-0.085,0.046]	[-0.030,0.044]
P-value (Large = Small)	0.253	0.041	0.160	0.009	0.264	0.017
Observations	875784	910526	875784	910526	875784	910526
Control mean	0.229	0.345	0.073	0.116	0.439	0.263

95% confidence intervals in brackets
Included age, wealth quintile, country, and year fixed effects.
Standard errors clustered at country level.
Small: abortion legal grounds expanded from "to save life & preserve physical health" to including "to preserve mental health".
Large: abortion legal grounds expanded from "to save life" only to including "to preserve mental health".
Included only individuals that were still in school at age 12.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 6. Test for parallel pre-trends

	Marriage	Birth	Schooling
Treated country	-0.485	-1.175	-9.917
	[-9.472,8.503]	[-4.837,2.487]	[-22.731,2.897]
Linear year	-0.003***	-0.001***	0.001
	[-0.005,-0.002]	[-0.002,-0.001]	[-0.004,0.006]
Treated country # Linear year	0.000	0.001	0.005
	[-0.004,0.005]	[-0.001,0.002]	[-0.002,0.011]
Age	0.080***	0.025***	-0.089***
	[0.067,0.092]	[0.021,0.030]	[-0.096,-0.082]
Rural	0.039***	0.013***	-0.053***
	[0.020,0.057]	[0.008,0.017]	[-0.078,-0.027]
Poorer	-0.032**	-0.009**	0.038**
	[-0.053,-0.011]	[-0.015,-0.003]	[0.016,0.060]
Middle	-0.057**	-0.019**	0.082***
	[-0.091,-0.024]	[-0.031,-0.008]	[0.052,0.113]
Richer	-0.087***	-0.031***	0.132***
	[-0.118,-0.056]	[-0.044,-0.018]	[0.100,0.164]
Richest	-0.171***	-0.060***	0.276***
	[-0.198,-0.144]	[-0.072,-0.047]	[0.242,0.311]
Constant	5.470**	2.449***	-0.081
	[2.476,8.464]	[1.209,3.688]	[-10.430,10.269]
Observations	1131149	1131149	1131149
Adjusted R^2	0.2836	0.0720	0.3424

95% confidence intervals in brackets
Treated country variable indicates whether a country eventually changed abortion policy.
The coefficient of interest is the interaction of treated country indicator and linear time trend.
Included country fixed effects.
Standard errors clustered at country level.
Excluded observations from treated countries after the abortion policy was changed.
Included only individuals that were still in school at age 12.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

CHAPTER 3: FREE ACCESS TO A BROAD CONTRACEPTIVE METHOD MIX AND WOMEN'S CONTRACEPTIVE CHOICE IN SUB-SAHARAN AFRICA

3.1 Overview

Background: Financial barriers may restrict women's ability to use their preferred contraceptive methods, especially long-acting reversible contraceptives (LARC) that - although more effective - have relatively high upfront costs. Providing free access to a broad contraceptive method mix, including both LARC and short-acting reversible contraceptives (SARC), may increase contraceptive use, meet women's various fertility needs, and increase their agency in contraceptive decision-making in resource-limited settings.

Methods: Linking health facility and individual data from eight countries in sub-Saharan Africa, we examine how free access to a broad contraceptive method mix affects women's contraceptive choice. We use a propensity score approach combined with machine learning techniques to compare current contraceptive use, LARC use, and contraceptive decision-making in communities that provide free access to both method types and those that do not.

Results: Free access to both LARC and SARC was associated with an increase of 3.2 percentage points (95% confidence interval: 0.006 – 0.058) in the likelihood of modern contraceptive use. This increase was driven by greater use of SARC. Among current contraceptive users, free access did not prompt women to switch to LARC and had no effect on women's autonomy in decisions about contraceptive methods. The price effects on contraceptive use were larger among women who were older, more educated, and more knowledgeable about contraceptive methods, while free access was associated with lower contraceptive use among adolescents.

Conclusions: Providing full or partial subsidies to contraceptives is associated with a modest increase in current contraceptive use. While price remains an important policy tool to increase contraceptive

uptake, removing user fees alone does not address all barriers women face, especially for the most vulnerable groups of women that need family planning services the most.

3.2 Introduction

Access to a wide variety of contraceptive methods is a fundamental element of quality of care and a critical dimension in multiple frameworks of family planning services.[103–105] LARC, including implants and intrauterine devices (IUDs), are 20 times as effective as shorter-term methods, have much higher continuation rates, and suit women's growing needs for limiting births.[16–19] However, a number of demand- and supply-side factors continue to limit women's uptake of LARC, such as upfront costs that often include informal fees, lack of trained providers that could perform both insertion and removal of LARC, and concerns about side effects.[106–109] Free access to both LARC and SARC may increase contraceptive use by meeting women's various fertility needs in low- and middle-income countries (LMIC).

Free access to a broad contraceptive method mix also gives women greater autonomy in contraceptive decision-making, especially for those from lower socioeconomic backgrounds. Previous research has shown that women from the poorest wealth quintiles in sub-Saharan Africa (SSA) are less likely to use LARC and such wealth-related inequalities have increased in several countries.[109,110] Although the costs per couple-year served are similar or even lower for LARC compared to SARC, higher upfront out-of-pocket costs, including both the commodity prices and additional consultation charges or insertion/removal fees, may be a barrier to LARC use.[111–114] Since men usually control household spending and often have different contraceptive or fertility preferences compared to women, providing both LARC and SARC free of charge removes the concern of costs from the decision-making process and may increase a woman's ability to use any contraceptive method without her partner's knowledge or approval.[115–117]

Previous literature is mixed on whether free access to contraceptives is effective in increasing contraceptive use or optimal for the sustainability of health systems. User fees for family planning were introduced in the 1980s in many LMIC to ensure the quality of health services and sustainability of health

systems, but charging a price for basic health services may prevent women from choosing the method that best suits their needs and preferences, especially for those on the lower end of the wealth spectrum.[41,43–46] For public health products, field experiments have shown that any increase in price can considerably dampen demand in SSA.[118–123] Specifically for LARC, recent studies showed that removing the cost barriers substantially increased LARC use among low-income women and adolescents in the United States.[52,53,124]

Contrary to these two strands of literature, previous reviews suggest that the effects of price on contraceptive use in LMIC have been largely inconclusive due to lack of recent studies, weak study designs, and challenges in measuring supply and demand simultaneously.[41,54,55,125–127] For example, providing free vouchers for contraceptives did not increase contraceptive use or shift method choice among postpartum women in Kenya in a randomized controlled trial,[56] although quasi-experimental studies have shown that vouchers were effective in increasing contraceptive use in Cambodia, Pakistan, and Madagascar.[58,128,129] In addition, program evaluations of service expansion have demonstrated uptake and acceptability of LARC, such as the multi-component LARC expansion initiative (including free or subsidized family planning services) implemented by Marie Stopes International across 14 countries in SSA.[57]

Further evidence is needed to understand whether free access to a broad contraceptive method mix is an effective and empowering strategy to meet women's family planning needs in SSA. To inform pricing policies for governments and international donors, this study estimates the effects of providing both LARC and SARC free of charge on women's contraceptive use and agency.

3.3 Methods

Linking data from health facility and individual surveys, we estimated whether free access to both LARC and SARC was associated with an increase in women's contraceptive use in eight countries in SSA. Among current contraceptive users, we estimated whether free access was associated with increased use of LARC and greater autonomy in decisions about contraceptive methods. We combined propensity

score weighting with machine learning techniques to account for differences in selection into the treatment and estimated differential treatment effects among key subgroups.

3.3.1 Data and sample

The data source is Performance Monitoring and Accountability 2020 (PMA2020) harmonized by the IPUMS-PMA project.[130] PMA2020 collected data annually from 2014 to 2017 from households, women, and service delivery points (i.e., health facilities) in 11 countries that have pledged to the Family Planning 2020 effort. These data are collected by trained enumerators using mobile devices and include key indicators of family planning use by individuals as well as provision of reproductive health services by healthcare facilities. In particular, PMA2020 surveyed all private facilities located within each enumeration area and public facilities that serve the enumeration area, representing facilities accessible to women in the community.[131] To identify public facilities, the survey team consulted district or local authorities to locate public sector facilities designated to cover the residents of each enumeration area, which may be located outside of the community.[131]

The sample came from the most recent PMA2020 surveys that were conducted in 2016/2017 and are representative at the national level in eight countries in SSA, including Burkina Faso, Cote d'Ivoire, Ethiopia, Ghana, Kenya, Niger, Nigeria, and Uganda (we excluded countries that do not have nationally-representative data or are not in SSA). Individual data excluded pregnant women or women who expressed the desire to have a child soon. The analysis dataset linked individual data with service delivery points data by enumeration area. For the main outcome, use of modern contraceptives, the sample included all women of reproductive age. For the other outcomes, use of LARC and autonomy in contraceptive decision-making, the sample consisted of women of reproductive age who reported using a modern contraceptive method at the time of the survey.

3.3.2 Measures

Exposure. The main policy variable of interest, *free access* to a broad contraceptive method mix, is a binary variable coded to 1 if both LARC (implants or intrauterine devices) and SARC (injectables that include Depo-Provera and Sayana Press, pills, male/female condoms, diaphragms, spermicide, and n

tablets) are routinely provided and offered free of charge in at least one facility that serves an enumeration area. Diaphragms, spermicide, and n tablets collectively make up less than 1% of the total method mix within our sample. We did not include permanent methods because we expect the decision-making process to be different from those for reversible methods. Similarly, we did not include emergency contraception because we expect a different user profile for emergency contraception compared to other SARC.

Free access was constructed based on two categories of questions in the Service Delivery Points Questionnaire to measure routine provision and charge of family planning services. First, routine provision is defined by whether a facility provides a contraceptive method, has trained personnel and supplies, and has the devices in stock. Service provision is measured by the question "*which of the following methods are provided to clients at this facility?*". A facility is considered to routinely provide LARC only if it has trained personnel that are able to insert implants/IUDs as measured by the question "*on days when you offer family planning services, does this facility have trained personnel able to insert implants/IUDs?*". A facility also needs to have the supplies necessary to perform the procedures (clean gloves, antiseptic, sterile gauze pad, anesthetic, implant pack for implants and speculums, forceps, and tenaculum for IUDs), as measured by the question "*Does this facility have the following supplies needed to insert and/or remove implants/IUD?*". Moreover, a facility needs to have the LARC or SARC method in stock on the day of the survey, regardless of whether this is based on interviewers' observation or providers' response. Second, a LARC or SARC method was provided free of charge if a facility did not charge any consultation fee or method-specific fee. This is assessed by two questions – "*Do family planning clients need to pay any fees in order to be seen by a provider even if they do not obtain a method of contraception*" and "*are clients charged for obtaining any of the following methods at this facility?*".

Outcome variables. The three outcome variables are based on questions included in the Female Questionnaire for women of reproductive age. Use of modern contraceptives is measured by the pre-coded binary "modern contraceptive user" variable that indicates current use of a modern family planning method. Use of LARC is a binary variable coded to 1 if the response to current use of either implant or

IUD is yes. Contraceptive method choice is measured based on the question "*during that visit, who made the final decision about what method you got?*" and is coded to 1 if the answer is respondent alone, respondent and provider, or respondent and partner.

Covariates. The Female Questionnaire has a wide range of individual-level covariates. These include women's socio-demographic characteristics (e.g., age, education, marital status, rurality, and wealth), sexual and reproductive history (e.g., age of first sex, age of first birth), fertility preference (e.g., when to have another child), knowledge about different family planning methods (e.g., ever heard of a specific method), and sources for family planning information (e.g., read about family planning in newspaper). Using a principle component analysis approach, we constructed two indices based on all knowledge or information source variables, with higher scores indicating women's greater knowledge of different contraceptive methods or exposure to various information sources about family planning.

3.3.2 Statistical analyses

The study's main hypothesis is that free access to a broad contraceptive method mix is associated with women's greater overall contraceptive use, LARC use, and autonomy in decision-making. We used the propensity score approach because differences between individuals who live in communities with good access to family planning services and those that do not might affect contraceptive use and women's agency. For example, women who prioritize health might live closer to well-funded facilities and are more likely to use effective modern contraceptives. Conventional propensity score methods reduce confounding by accounting for observed characteristics that predict treatment and often apply propensity weights in treatment effect models.[132,133] However, such propensity score models assume comparability of unobserved pre-treatment characteristics between groups, are likely to be misspecified, and might omit covariates that are important to treatment selection.[134–136]

To strengthen the propensity score approach, we used generalized boosted modeling (GBM) to estimate propensity scores. GBM is a non-parametric machine learning technique that adds together a collection of simple regression tree models to fit a nonlinear surface and is effective in producing probability estimates with a large number of covariates.[136,137] Similar machine learning algorithms have

increasingly been applied in health and medical studies.[138,139] GBM is particularly tuned to produce well-calibrated probability estimates and has been shown to outperform standard logistic regression and covariate balancing propensity methods in complex models for non-linear relationships.[136,140]

The propensity score approach consists of two steps. First, we used GBM to estimate probability of having free access using all available covariates at the individual level (Table A6). The algorithm was stopped at the number of iterations that minimized the average standardized absolute mean difference in the covariates. A dummy missing variable approach was used to create a missing category for all factor variables so that the algorithms would use the missingness in the predictions.

Then, weights generated from the GBM propensity scores were applied to the logistic regression model specified below:

$$Y_{ijc} = \alpha + \beta Access_{jct} + z_c + X_{ijc}{}'\gamma + \varepsilon_{ijc}$$

where Y_{ijc} is the outcome of interest for individual i of community j in country c, $Access_i$ is a dummy equal to one if the community provides access to both LARC and SARC free of charge, z_c is a full set of country fixed effects to control for time-invariant country characteristics, and X_{ijc} is a vector of individual characteristics including age (continuous variable), education (categorical variable for highest level of school attended), marital status (categorical variable), rurality (binary variable), household wealth index quintile (categorical variable), family planning knowledge score (continuous variable), family planning information exposure score (continuous variable), fertility preference (binary variable), and provider type (categorical variable for LARC use and decision-model only). The coefficient of interest is β, which represents the estimated effects of free access to LARC and SARC. The results are presented as average marginal effects.

In addition to the primary models, we conducted additional sensitivity and subgroup analyses. First, we used standard logistic regression models to generate propensity scores and compared the results with the GBM-based models. Second, to examine whether the price effects vary by contraceptive type, we assessed the effects of free access to both LARC and SARC as well as free access to only SARC on method-specific use. Third, we used an alternative definition for the treatment variable and examined the

effects of access to contraceptives without removing user fees. Specifically, we used multinomial logit models to generate propensity scores for a categorical treatment variable with three levels: no access, access with some fee, and free access. Lastly, to examine heterogeneity in the effects of free access among subgroups of women, we draw on the recursive partitioning approach used in previous empirical work.[141–143] We first split the data into two randomly chosen sub-samples, one sample to identify the sources of heterogeneity (training subsample) and the other to estimate the treatment effects and confidence intervals (estimation subsample). The machine learning algorithm created a decision tree that aimed to correctly classify individuals' outcome status by splitting the training subsample into high-dimensional and mutually exclusive groups. Based on the structure of the decision tree and the variable important measure, we identified the subgroups and estimated heterogeneous treatment effects using the estimation subsample.

Statistical tests were 2-sided and the statistical significance was set at $p<.05$. Analyses were performed using R v3.5.1 (the R Foundation, packages "rpart" and "twang")[144,145] and Stata, version 15.1 (StataCorp LLC).

3.4 Results

The full sample contained 29,833 individuals, of which 15,998 (53.6%) had free access to a broad contraceptive method mix and the remaining 13,835 (46.4%) did not. Overall, 28.8% of individuals were using a modern contraceptive method. Among these contraceptive users, 29.4% were using LARC and 91.2% reported having participated in the decisions about which method to use. Unweighted, 32.5% of women with free access to both LARC and SARC were using a modern contraceptive method compared to 24.4% among those without access. The proportion of LARC users did not differ by access status while more women with free access participated in contraceptive decision-making (93.3% vs. 87.8%). Most other covariates differed by treatment status (see Table 7 for the unweighted descriptive statistics).

We used the machine learning algorithm GBM and all variables listed in Table A6 to generate propensity weights, including missing values as dummy variables. Weighted standardized differences in covariates between treatment and comparison groups were reduced to below .03 on average and

below .06 in all variables except for one country. For comparison purposes, we also used the standard logistic regression approach to generate propensity weights, including a limited set of covariates. A comparison on covariate balance between the GBM approach and the logistic regression approach were presented in Figure A1. The distribution of propensity scores generated from GBM was presented in Figure A2.

The main results are presented in Table 8. We applied the propensity weights built from GBM, our preferred approach, and the weights from logistic regression models to the main specification and presented average marginal effects. Free access to a broad contraceptive mix was associated with an increase of 3.2 percentage points (95% confidence interval [CI]: 0.006 – 0.058) in the likelihood of contraceptive use among all women, representing an increase of 13.1% from the 24.4% contraceptive use in the control group. Among current contraceptive users, free access had no effect on LARC use or women's role in contraceptive method decision-making. The specifications using propensity weights from logistic regression models produced similar effects except for the LARC use outcome: free access was associated with a decrease of 3.8 percentage points (95% CI: -0.075 – -0.000) in the likelihood of LARC use.

To further investigate the price effects on contraceptive use, we estimated the effects of free access to different types of contraceptives on women's contraceptive use by method-type (Table 9). Table A7 shows that 77.7% of all women and 84.0% of current contraceptive users who had free access to SARC also had free access to LARC.

Panel A of Table 9 shows that free access to both LARC and SARC was associated with an increase in overall contraceptive use as well as an increase of 2.5 percentage points in SARC use (95% CI: 0.003 – 0.047) but had no effect on LARC use. Free access to SARC only was associated with an increase of 4.1 percentage points (95% CI: 0.007 – 0.074) in overall contraceptive use and had no effect on LARC use. Although free access to SARC was not associated with any statistically significant effect on SARC use, the effect size is similar to that of free access to both LARC and SARC with a *p*-value of 0.079. These

results suggest that LARC use was not responsive to the removal of user fees, or at least not as sensitive as SARC use.

Turning to women who were contraceptive users, Panel B of Table 9 shows that neither free access to both LARC and SARC nor free access to SARC alone was associated with any statistically significant effect on method-specific use. This indicates that among women who were already using a modern contraceptive method, removing user fees did not shift their preference for contraceptives defined by the two general contraceptive types.

Using an alternative definition of the treatment variable, Table 10 assesses the effects of access to contraceptives with and without user fees. Among all women, access alone without removing user fees did not affect contraceptive use. By comparison, free access was associated with an increase of 4.7 percentage points in contraceptive use (95% CI: 0.019 – 0.074), which underscores the importance of removing financial barriers in addition to making family planning services available. Among contraceptive users, neither access alone nor free access affected LARC use or decision-making.

Heterogeneity. We draw on the machine learning recursive partitioning approach to identify subgroups that best predict modern contraceptive use.[141–143] The machine learning algorithm tried to predict which distinct groups of women would use modern contraceptives based on a list of observed characteristics. The training sub-sample used in the exercise was a randomly generated half sample of the full dataset. The other half sub-sample was used in estimating treatment effects. This "honest" approach helps to avoid identifying spurious relationships by overfitting the model.[141–143] The decision tree generated by the training sub-sample correctly classified contraceptive use for 76.5% of the observations in the estimation sub-sample.

In the final tree structure (see Figure A3), the candidates for the primary splits for the first node include marital status, family planning knowledge, and age. These variables are also the three highest ranked variables according to the variable importance measure (see Table A8), which indicates how much a model uses a given variable to make accurate predictions. We also included education and wealth as potential source of heterogeneity based on the literature and the fact that they had higher or similar

variable importance values compared to the treatment variable. We used the splitting rules as cutoff values to create subgroups in the estimation sub-sample.

Free access to a broad contraceptive method mix was associated with greater contraceptive use among older women who were at least 20 years of age (see Table 11 Panel B). By comparison, adolescents used contraceptives less when LARC and SARC were offered for free. Free access also had greater effects on overall contraceptive use among women who were relatively knowledgeable about family planning, had at least some schooling, and came from an above-the-lowest wealth quintile, although the result on wealth was not statistically significant ($p = 0.061$). For current contraceptive users, free access was associated with a 9.9 percentage point decrease (95% CI: -0.194 – -0.003) in LARC use among women who came from the lowest wealth quintile, although we could not reject this is different from the effect among women from a wealthier background. The effects on LARC use and decision-making did not differ in any of the other subgroups we examined.

3.5 Discussion

Free access to a broad contraceptive method mix may enhance women's agency in family planning, but existing literature is lacking on whether removing user fees is an effective approach to increase women's contraceptive use and autonomy in low-resource settings. Linking facility and individual-level data from eight countries in SSA, this study suggests that free access was associated with a modest increase in overall contraceptive use, did not prompt current users to switch contraceptive types, and did not seem to reach the most vulnerable women that need access to services the most.

The results showed that free access to both LARC and SARC was associated with 13% increase in modern contraceptive use, indicating that demand for contraceptives is sensitive to price in contexts with limited resources, in line with recent field studies.[58,146,147] In addition, the increase in contraceptive use was only observed when contraceptives were provided free of charge, indicating that only making family planning services available to women without removing cost barriers would not increase women's contraceptive use. However, this is a modest effect as it only represents an increase of contraceptive use from 24.4% to 27.3% among women who had no immediate plan to have a child. The lack of stronger

effect might be explained by frequent stockout of contraceptive commodities in this setting,[148,149] which can prompt providers to prioritize women who are older, married, and with children.[150] The finding that the price effect was driven by an increase in SARC use might reflect women's preference for short-term methods because of their convenience, privacy, and fewer side effects compared to LARC as well as the ability to discontinue SARC without assistance from a willing and skilled provider.[112,151,152] Moreover, by definition, free access to SARC indicated that a facility does not charge any consultation fee for family planning visits. Consultation fees can be an important access barrier in addition to method-specific charges and can add up as women need more frequent visits for short-term methods.[114] These findings suggest that removing user fees alone might not have a transformative impact on contraceptive uptake. On the other hand, when resources are limited, partial subsidies that target the methods women prefer could increase overall contraceptive use.

The finding that free access was not associated with an increase in LARC use suggests that other factors might have played a larger role in LARC use than financial barriers. A recent review suggests that different promotion strategies, including vouchers, could raise uptake of IUDs, but such effects did not translate into any impact at the national level due to providers' preference for methods that take less time and women's discomfort with having an IUD insertion in facilities that lack the space for privacy or by a male doctor.[153] In addition, some countries have policies that restrict the use of IUDs by unmarried women or adolescents, further limiting access to LARC for certain subgroups.[154] Even when LARC are provided for free, women might not want to use them due to the concern for removal, reliance on health providers, and less familiarity through social network.[112,155] Addressing these supply- and demand-side factors may have a larger effect on LARC use than relying on pricing strategies alone.

Additionally, among women from the poorest households, free access was associated with decreased LARC use, indicating that women who were using LARC switched to non-LARC methods when LARC were offered for free. While we need more detailed information on women's preferences and access to understand this switching pattern, we offer one potential explanation: the treatment variable *free access* is defined as having free access to at least one method under two general categories of contraceptive

method-types (i.e., LARC and SARC), which does not measure accurately whether a woman has free access to her preferred methods. For example, injectables, a short-term method, are the most popular contraceptive in SSA.[156] In Kenya, all public sector users were supposed to receive contraceptives for free, but users of injectables were more likely to pay compared to users of implants, IUDs, pills, or condoms, indicating women's strong preference for injectables.[157] If *free access* to LARC and SARC reflected more resources to cover LARC and some previously not-covered SARC methods, such as injectables, LARC users might switch to injectables when LARC were provided free of charge as defined by the treatment variable. Given the wide differences in women's preferences for contraceptive methods,[156] more detailed analysis is needed to examine whether women's preferences and access align and how price affects demand for different contraceptive methods in specific contexts.

Older women, women who were more educated or more informed about contraceptive methods, and women from wealthier economic background were more likely to use contraceptives when LARC were offered for free. By comparison, women from more disadvantaged backgrounds might face more non-financial barriers that limit their contraceptive use, such as low health literacy, provider bias, and time costs.[112] In particular, free access was associated with lower contraceptive use among adolescents. One explanation is that adolescents might be less likely to receive services from public facilities where contraceptives are provided free of charge but providers are reluctant to offer them to young unmarried women due to personal biases.[112,150] In the analysis dataset, non-public facilities rarely offer both LARC and SARC for free (see Table A9) and there is a moderate negative correlation between age (under 20) and visiting public-sector provider for the most recent family planning method ($r = -0.213$, $p < 0.001$), suggesting that adolescents were less likely to obtain contraceptives from public facilities where youth-friendly services are often lacking. Thus, the association between free access and lower contraceptive use among teenagers might indicate that other factors, such as provider bias, are more prominent barriers for young women's access to contraceptive services. This is alarming as young women are less able to afford user fees and more susceptible to the negative consequences of unintended pregnancies, such as worse maternal and infant health outcomes, compromised educational prospects, and fewer economic

opportunities.[93,94] To provide better access to contraceptives for all women, especially those who are most vulnerable and in need of family planning services, more research is needed to examine the barriers these women face and test the effectiveness of other interventions combined with financial subsidies, which include but are not limited to targeted information campaigns, community-based distribution strategies, and trainings that address provider biases.

Removing cost barriers did not enhance women's role in contraceptive decision-making based on women's response to a single standardized survey question. This might not be a surprise as 91% of women who were current contraceptive users in the dataset participated in contraceptive method decisions, leaving relatively little space to improve. In addition, this specific survey question might not capture the complex decision-making process or reflect women's agency in whether or not to use any contraceptive. Nevertheless, this finding suggests that removing costs alone is unlikely to have any large effect on women's autonomy in contraceptive method decisions for those already using family planning services.

This study has several limitations. First, the main treatment variable *free access* is defined based on the charges reported in the Service Delivery Points Questionnaire, which might not be an accurate measure due to informal fees.[114] We used data from the Female Questionnaire to validate facility-reported contraceptive charges. Specifically, women were asked whether they paid any fees for family planning services in the past 12 months. There was a negative correlation between user-reported fees for contraceptives and free access defined by facility surveys, $r = -.19$ ($p < .001$), providing some assurance for the validity of the treatment variable. Second, women who live in a community where contraceptives are available free of charge might still have limited access due to travel and time costs, especially since public facilities can be located outside of the communities they serve. Women might also be denied free services due to provider biases based on age, parity, or marital status.[150] On the other hand, while some women might travel to obtain free family planning services outside of their communities, others might prefer to visit a facility that is farther away for privacy or better quality of care. Future studies could use detailed service utilization data to examine the importance of costs relative to other barriers for women's

contraceptive uptake. Third, contraceptive stockout is usually high in the study settings but was not common based on the data from the Service Delivery Points surveys.[148,149] For example, among facilities that routinely provided implants and IUDs, about 90% had them in stock and this was verified by enumerators on the day of the survey. Although the Family Planning 2020 Initiative might have brought more resources and strengthened supply chain systems in these countries, it is still likely that data from these facility surveys might not capture whether women had routine access to family planning services. Thus, women who did not have free access might have been misclassified in our analyses, which would underestimate the treatment effects. Fourth, this study pooled data from eight countries in SSA that have pledged to the Family Planning 2020 Initiative to estimate the association between access and contraceptive use in this region. However, these eight countries vary greatly among themselves, as indicated by the contraceptive prevalence rates in our data, and our analyses did not provide country-specific estimates. Meanwhile, these eight countries might not be representative of all countries in SSA, limiting the generalizability of the study findings. For example, these countries might have higher government commitment to financing family planning services, lower contraceptive prevalence before the Initiative, or different social norms about the use of LARC versus short-term methods. Lastly, unobserved factors could have confounded the estimated associations between free access and contraceptive use. Future studies that randomly assign treatment status would provide stronger evidence on the effects of price on contraceptive use.

3.6 Conclusions

Free access to a broad contraceptive method mix was associated with a modest increase in contraceptive use, driven by higher use of short-term methods, based on data from eight countries in SSA. Among current contraceptive users, free access to LARC did not prompt more women to switch to LARC and had no effect on women's autonomy in contraceptive decision-making. Governments and international donors should continue using financial subsidies to accelerate contraceptive uptake, but removing user fees alone does not necessarily increase access or choice, especially for women from more disadvantaged segments of the population who need family planning services the most.

TABLE 7. Descriptive statistics

Variable	All	Treat	Comparison	*p*-value
N	29833	15998	13835	
Modern contraceptive user	8579 (28.8%)	5207 (32.5%)	3372 (24.4%)	<0.001
Use of LARC	2520 (29.4%)	1522 (29.2%)	998 (29.6%)	0.72
Decided contraceptive method	7820 (91.2%)	4860 (93.3%)	2960 (87.8%)	<0.001
Age, mean (SD)	28.0 (9.3)	27.9 (9.3)	28.0 (9.3)	0.42
Urban	13744 (46.1%)	6920 (43.3%)	6824 (49.3%)	<0.001
Highest level of education				<0.001
never attended	7224 (24.2%)	3043 (19.0%)	4181 (30.2%)	
primary/middle school	10232 (34.3%)	6122 (38.3%)	4110 (29.7%)	
secondary/post-primary	9486 (31.8%)	5097 (31.9%)	4389 (31.7%)	
tertiary/post-secondary	2891 (9.7%)	1736 (10.9%)	1155 (8.3%)	
Marital status				<0.001
never married	9971 (33.4%)	5352 (33.5%)	4619 (33.4%)	
currently married	15200 (51.0%)	8096 (50.6%)	7104 (51.3%)	
currently living with partner	2178 (7.3%)	1000 (6.3%)	1178 (8.5%)	
divorced or separated	1666 (5.6%)	1085 (6.8%)	581 (4.2%)	
widow or widower	818 (2.7%)	465 (2.9%)	353 (2.6%)	
Married once or more than once				0.22
never	9971 (33.5%)	5352 (33.5%)	4619 (33.5%)	
once	17484 (58.7%)	9348 (58.5%)	8136 (59.0%)	
more than once	2311 (7.8%)	1280 (8.0%)	1031 (7.5%)	
Partner has other wives	4655 (27.1%)	2088 (23.3%)	2567 (31.3%)	<0.001
Wealth score quintile				<0.001
lowest quintile	6048 (20.3%)	3062 (19.1%)	2986 (21.6%)	
lower quintile	5851 (19.6%)	3020 (18.9%)	2831 (20.5%)	
middle quintile	5530 (18.5%)	2902 (18.1%)	2628 (19.0%)	
higher quintile	5503 (18.4%)	2929 (18.3%)	2574 (18.6%)	
highest quintile	6901 (23.1%)	4085 (25.5%)	2816 (20.4%)	
Ever given birth	20059 (67.3%)	10678 (66.8%)	9381 (67.8%)	0.050
Age at first sex, median (IQR)	17.0 (15.0, 19.0)	17.0 (15.0, 19.0)	17.0 (15.0, 19.0)	0.054
Age at first birth, mean (SD)	19.9 (4.2)	19.9 (4.1)	20.0 (4.3)	0.28
Prefer no more children	7768 (26.0%)	4484 (28.0%)	3284 (23.7%)	<0.001
Months to wait before another child, mean (SD)	48.3 (38.9)	50.8 (39.7)	45.6 (37.9)	<0.001
Family planning knowledge score, mean (SD)	0.0 (2.1)	0.2 (2.1)	-0.2 (2.2)	<0.001
Family planning information exposure, mean (SD)	0.0 (1.4)	0.0 (1.4)	-0.0 (1.3)	<0.001
Country				<0.001
Burkina Faso	2650 (8.9%)	67 (0.4%)	2583 (18.7%)	
Ethiopia	5617 (18.8%)	4711 (29.4%)	906 (6.5%)	
Ghana	2427 (8.1%)	35 (0.2%)	2392 (17.3%)	
Kenya	4801 (16.1%)	4281 (26.8%)	520 (3.8%)	
Niger	1830 (6.1%)	1339 (8.4%)	491 (3.5%)	
Nigeria	7425 (24.9%)	2794 (17.5%)	4631 (33.5%)	
Uganda	3234 (10.8%)	2674 (16.7%)	560 (4.0%)	
Cote d'Ivoire	1849 (6.2%)	97 (0.6%)	1752 (12.7%)	

* The analysis dataset excludes pregnant women and women who would like to have another child soon.

TABLE 8. Effects of free access to a broad contraceptive method mix

	(1)	(2)	(3)	(4)	(5)	(6)
	All Women		*Current Contraceptive Users*			
	Modern Contraceptive Use		LARC Use		Decision-Making	
	GBM	Logit	GBM	Logit	GBM	Logit
Treat (free access)	**0.032***	**0.029***	**-0.029**	**-0.038***	**0.013**	**0.012**
	[0.006,0.058]	**[0.002,0.055]**	**[-0.066,0.007]**	**[-0.075,-0.000]**	**[-0.021,0.047]**	**[-0.021,0.045]**
Age	0.003	0.010	-0.008	-0.005	0.018	0.022
	[-0.022,0.028]	[-0.016,0.036]	[-0.048,0.033]	[-0.047,0.037]	[-0.013,0.049]	[-0.010,0.053]
Urban	-0.002***	-0.002***	0.000	0.000	0.002**	0.002**
	[-0.003,-0.001]	[-0.003,-0.001]	[-0.003,0.003]	[-0.003,0.004]	[0.001,0.004]	[0.001,0.004]
Education (base = no schooling)						
Primary school	0.047***	0.032*	0.011	0.014	-0.010	-0.012
	[0.026,0.069]	[0.003,0.061]	[-0.025,0.047]	[-0.025,0.053]	[-0.041,0.021]	[-0.043,0.018]
Secondary school	0.048**	0.030	-0.033	-0.022	0.012	0.012
	[0.018,0.078]	[-0.006,0.067]	[-0.076,0.011]	[-0.063,0.020]	[-0.016,0.040]	[-0.016,0.039]
Tertiary school	0.045**	0.025	0.019	0.025	0.000	0.001
	[0.011,0.079]	[-0.017,0.066]	[-0.039,0.077]	[-0.039,0.089]	[-0.036,0.036]	[-0.035,0.036]
Marital status (base = never married)						
Married	0.270***	0.257***	0.084**	0.087**	0.029	0.029
	[0.240,0.299]	[0.224,0.290]	[0.023,0.146]	[0.029,0.144]	[-0.015,0.072]	[-0.015,0.072]
Cohabiting	0.243***	0.239***	0.060	0.074	0.060*	0.063*
	[0.205,0.280]	[0.200,0.279]	[-0.010,0.130]	[-0.003,0.151]	[0.004,0.117]	[0.004,0.122]
Divorced or separated	0.108***	0.095***	0.159***	0.158***	0.094***	0.094***
	[0.073,0.143]	[0.055,0.134]	[0.076,0.241]	[0.077,0.240]	[0.050,0.138]	[0.049,0.138]
Widow	-0.006	-0.019	0.053	0.089	0.055	0.056
	[-0.042,0.030]	[-0.057,0.018]	[-0.065,0.171]	[-0.049,0.226]	[-0.012,0.121]	[-0.010,0.121]
Wealth index quintile (base = lowest)						
Lower quintile	0.059***	0.064***	-0.046*	-0.033	-0.033	-0.035
	[0.033,0.084]	[0.037,0.091]	[-0.089,-0.003]	[-0.075,0.010]	[-0.076,0.011]	[-0.074,0.004]
Middle quintile	0.052***	0.050**	-0.052*	-0.042	-0.023	-0.030*
	[0.021,0.083]	[0.018,0.083]	[-0.102,-0.002]	[-0.094,0.010]	[-0.055,0.009]	[-0.060,-0.000]
Higher quintile	0.093***	0.097***	-0.036	-0.033	-0.053*	-0.060**
	[0.057,0.129]	[0.059,0.134]	[-0.087,0.014]	[-0.086,0.020]	[-0.096,-0.010]	[-0.104,-0.017]
Highest quintile	0.078***	0.081***	0.020	0.025	-0.019	-0.028
	[0.045,0.112]	[0.046,0.116]	[-0.039,0.079]	[-0.036,0.087]	[-0.054,0.016]	[-0.062,0.006]
Family planning knowledge score	0.051***	0.054***	0.019**	0.016*	0.006	0.006
	[0.045,0.058]	[0.047,0.061]	[0.007,0.030]	[0.004,0.028]	[-0.002,0.015]	[-0.002,0.015]
Family planning information	0.006	0.006	-0.001	0.000	-0.001	-0.001
	[-0.000,0.013]	[-0.000,0.013]	[-0.012,0.010]	[-0.011,0.011]	[-0.010,0.007]	[-0.010,0.007]
Fertility preference (base = prefer more children)						
Prefer no more child	0.017	0.014	0.032*	0.034*	-0.015	-0.011
	[-0.004,0.038]	[-0.007,0.034]	[0.003,0.061]	[0.005,0.064]	[-0.036,0.006]	[-0.033,0.010]
Family planning provider (base = public sector)						
private sector	--	--	-0.290***	-0.281***	-0.033*	-0.031*
			[-0.317,-0.263]	[-0.309,-0.252]	[-0.062,-0.004]	[-0.059,-0.002]
NGO	--	--	0.083	0.085	-0.057	-0.059
			[-0.094,0.260]	[-0.089,0.260]	[-0.177,0.064]	[-0.178,0.061]
other	--	--	-0.347***	-0.348***	-0.072*	-0.087*
			[-0.385,-0.308]	[-0.385,-0.311]	[-0.137,-0.008]	[-0.162,-0.012]
Number of observations	29833	29833	8464	8464	8464	8464
Control mean	0.244	0.244	0.303	0.303	0.890	0.890

95% confidence intervals in brackets

LARC: long-acting reversible contraceptives, including implants and intrauterine devices. Decision-making: whether respondent participated in the decision on contraceptive method. Logistic regression models report average marginal effects and include country fixed effects. Propensity weights generated from generalized boosted models (GBM) or logistic models were applied to adjust for observed differences by treatment status.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 9: Effects of free access on contraceptive use by method type

	(1) Modern Contraceptive Use	(2) LARC Use	(3) SARC Use
	Panel A: among all women		
Free access to both LARC & SARC	0.032*	0.003	0.025*
	[0.006,0.058]	[-0.010,0.016]	[0.003,0.047]
Control mean	0.244	0.072	0.170
Free access to SARC only	0.041*	0.016	0.023
	[0.007,0.074]	[-0.000,0.031]	[-0.003,0.049]
Control mean	0.258	0.081	0.175
Number of observations	29833	29833	29833
	Panel B: among current contraceptive users		
Free access to both LARC & SARC	--	-0.026	0.015
		[-0.066,0.013]	[-0.027,0.056]
Control mean	--	0.296	0.697
Free access to SARC only	--	0.014	-0.020
		[-0.030,0.058]	[-0.065,0.025]
Control mean	--	0.314	0.679
Number of observations	--	8579	8579

95% confidence intervals in brackets

Logistic regression models report average marginal effects and include country fixed effects. Propensity weights generated from generalized boosted models were applied to adjust for observed differences by treatment status. LARC: long-acting reversible contraceptives, including implants and intrauterine devices. SARC: short-acting reversible contraceptives.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 10. Effects of access and free access to a broad contraceptive method mix

	(1)	(2)	(3)
	All women	*Current contraceptive users*	
	Modern Contraceptive Use	LARC Use	Decision-Making
Access to both LARC and SARC	0.021	0.055	0.014
	[-0.007,0.049]	[-0.007,0.116]	[-0.020,0.049]
Free access to both LARC and SARC	0.047**	0.004	0.014
	[0.019,0.074]	[-0.055,0.062]	[-0.020,0.047]
p-value	0.004	0.027	0.663
Control mean	0.252	0.282	0.906
Number of observations	29802	8462	8462

95% confidence intervals in brackets

LARC: long-acting reversible contraceptives, including implants and intrauterine devices. SARC: short-acting reversible contraceptives. Decision-making: whether respondent participated in the decision on contraceptive method. Propensity weights generated from multinomial logistic models were applied to adjust for observed differences by treatment status. The *p*-value row reports the two-sided *p*-value from an *F*-test of equality of the treatment effects of access and free access.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 11. Effects of free access to a broad contraceptive method mix among key sub-groups

	(1)	(2)	(3)
	All women	*Current contraceptive users*	
	Modern Contraceptive Use	LARC Use	Decision-Making
Panel A: Heterogeneity by marital status			
Treat × Marstat1 (married, cohabiting, & divorced)	0.028	-0.016	0.034
	[-0.002,0.059]	[-0.059,0.028]	[-0.011,0.080]
Treat × Marstat0 (never married & widow)	0.055*	-0.112	-0.016
	[0.001,0.108]	[-0.235,0.011]	[-0.091,0.059]
p-value	0.428	0.120	0.247
Panel B: Heterogeneity by age			
Treat × Older (age > 19.5)	0.066***	-0.025	0.025
	[0.038,0.094]	[-0.068,0.019]	[-0.017,0.067]
Treat × Younger (age < 19.5)	-0.183***	-0.079	-0.004
	[-0.238,-0.128]	[-0.169,0.011]	[-0.069,0.062]
p-value	0.000	0.195	0.275
Panel C: Heterogeneity by knowledge			
Treat × High (knowledge score > -1.14)	0.042***	-0.025	0.019
	[0.020,0.065]	[-0.070,0.020]	[-0.028,0.066]
Treat × Low (knowledge score < -1.14)	-0.043	-0.022	0.019
	[-0.118,0.033]	[-0.133,0.088]	[-0.045,0.082]
p-value	0.029	0.967	0.997
Panel D: Heterogeneity by education			
Treat × Edu1 (at least some schooling)	0.043***	-0.026	0.022
	[0.021,0.065]	[-0.073,0.021]	[-0.027,0.072]
Treat × Edu0 (no schooling)	-0.047	-0.048	0.009
	[-0.109,0.015]	[-0.140,0.044]	[-0.057,0.075]
p-value	0.000	0.371	0.670
Panel E: Heterogeneity by wealth			
Treat × Wealth1 (above lowest quintile)	0.029*	-0.022	0.027
	[0.005,0.054]	[-0.067,0.023]	[-0.021,0.075]
Treat × Wealth0 (lowest quintile)	0.003	-0.099*	-0.038
	[-0.064,0.071]	[-0.194,-0.003]	[-0.099,0.023]
p-value	0.061	0.115	0.171
Number of observations	14917	4229	4229

95% confidence intervals in brackets

LARC: long-acting reversible contraceptives, including implants and intrauterine devices. Decision-making: whether respondent participated in the decision on contraceptive method. This table reports the average marginal effects of the treatment (free access to a broad contraceptive method mix) on subgroups of women indicated in each panel. The subgroups were identified by a recursive partitioning machine learning analysis. A random half of the full data were used to identify the source of heterogeneity and the other half were used to estimate treatment effects reported in this table. Logistic regression models included individual covariates and country fixed effects. Propensity weights generated from generalized boosted models were used to adjust for observed differences by treatment status. The *p*-value rows report the two-sided *p*-value from an *F*-test of equality of the treatment effects for the two sub-groups indicated in each panel.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

CHAPTER 4: EFFECTS OF DISCLOSING HIV-NEGATIVE STATUS ON SEXUAL RELATIONSHIPS AMONG WOMEN WITH MULTIPLE SEXUAL PARTNERS IN KENYA

4.1 Overview

Background: Women with multiple sexual partners who often engage in transactional sex are at increased risk of HIV infection, economic hardship, and relationship abuse in a generalized HIV epidemic. Disclosing HIV-negative status to sexual partners may influence relationship stability, allow women to negotiate higher prices in transactional sex, and facilitate safer sexual behaviors. However, such a strategy might not increase women's agency in sexual relationships due to gender-based power dynamics.

Methods: This paper uses an instrumental variable approach to assess how disclosing HIV-negative status affects intimate partner and transactional sex relationships for women with multiple partners in Kenya. We conduct a secondary analysis of data collected at baseline, 6-month, and 12-month surveys from a randomized controlled trial and use intervention assignment to instrument HIV-status disclosure. We also assess whether the effects differed between sex workers and non-sex workers.

Results: Disclosing HIV-negative status reduced the likelihood of ending any sexual relationship by 27.5 percentage points (95% confidence interval [CI]: -0.494 – -0.056), almost eliminating the possibility of any relationship dissolution. Disclosure increased the price for unprotected transactional sex by 80% at 12 months and increased women's likelihood of refusing sex after a partner declined to test for HIV or tested positive by 18.5 percentage points (95% CI: 0.091 – 0.279) at 6 months. There was no effect on number of sexual partners, condom use, or intimate partner violence. HIV-status disclosure had differential effects by sex worker status on relationship dissolution, prices for and income from transactional sex, and women's ability to refuse sex.

Conclusions: Disclosing HIV-negative status reduced relationship dissolution, increased prices for transactional sex, and allowed women to exert greater control over sexual behaviors. While these effects might be moderated by women's bargaining power, our findings suggest that women with multiple sexual partners in Kenya used disclosure of HIV-negative status to inform their decisions in sexual relationships.

4.2 Introduction

Women who engage in transactional sex are at increased risk of HIV infection due to having multiple concurrent partners, inconsistent condom use, and risks of violence, criminalization, and marginalization.[20,21] Providing women with HIV self-tests, which may encourage women to test regularly and to offer self-tests to sexual partners, has been shown to be an effective approach to facilitating safer sexual behaviors in sub-Saharan Africa,[158–163] assuming HIV test results are disclosed between women and their sexual partners. Existing literature on HIV status disclosure has focused on identifying interventions for HIV-positive men and women to safely disclose HIV status to their primary sexual partners. HIV-positive women in particular face higher risks of violence, relationship dissolution, and stigma following disclosure of their HIV status.[59–61] However, for women who have multiple sexual partners and often engage in transactional sex, disclosing HIV-negative status also has important implications for their intimate partner and transactional sex relationships in a generalized HIV epidemic.

HIV information may influence sexual relationships by allowing women to know their own HIV status, empowering women to negotiate condom use, and changing their attitudes towards women's rights in sexual relationships.[164–166] A woman might infer her partner's HIV status, sexual safety, or faithfulness from her own HIV-negative status, making her more likely to stay with the same partner.[167] As long-term health and togetherness are important motivators for women to use HIV self-tests for couples testing,[168] a woman might disclose her HIV-negative status to a sexual partner to show her care for the partner's health and interest in a committed relationship, which could increase relationship stability. A woman might also disclose her HIV-negative status as a strategy to demand her partners' HIV information; if a partner refused to test for HIV or tested HIV-positive, a woman could end the relationship as a strategy to reduce her risks of infection and maintain her HIV-negative status.[169–171]

Knowledge and disclosure of one's HIV-negative status may also help women decide when and with whom to have higher-paying but riskier transactional sex. Recent randomized controlled trials (RCTs) showed that distributing two HIV self-tests to female sex workers reduced their numbers of clients and non-client partners in Zambia but had no impact in Uganda.[172,173] By allowing women to test for HIV in a time and place of their choice, access to HIV self-tests might have increased women's sense of control, which in turn enhanced their agency in sexual relationships.[172,174] Since the price premium for sex without a condom can be high,[175] e.g., $3 with a condom vs. $10 without in urban Uganda,[173] a sex worker could disclose her own HIV-negative status to signal safety, charge higher prices for transactional sex, and insist on safer sexual acts to maintain HIV-negative status. Based on data from these two RCTs in Uganda and Zambia, researchers showed that sex workers' knowledge of HIV-negative status was associated with increased condom use, suggesting that these women forwent higher economic gains to reduce their risks of HIV infection.[62,63]

Although women might disclose HIV-negative status as a way to select safer sexual partners, such a strategy might not enhance women's agency in sexual relationships due to unequal power dynamics based on gender. In Malawi, the relative risk of divorce was three times higher for HIV-positive women compared with HIV-negative women, but the adverse effects of HIV infection on marriage did not apply to married men.[176] Similarly, HIV-positive women in serodiscordant couples were at higher risk of divorce or separation than HIV-negative women in serodiscordant couples in Uganda.[177] These gendered differences indicate that women's decision-making power in sexual relationships is lower than men, potentially due to less stigma towards HIV-positive men, women's role as caregivers, and women's economic dependence on men.[178,179] Thus, women who want to have safer sex or choose safer sexual partners might not be able to do so because of the unequal gender dynamics. Moreover, if a woman insists on using a condom or refuses to have sex, such conflicts might strain relationships and, in some cases, lead to intimate partner violence (IPV).[158–160]

HIV status disclosure has potentially important implications for sexual relationships for women who engage in transactional sex. Existing literature has focused on HIV-positive women and has not explored

the potentially empowering effects of disclosing HIV-negative status on women's agency in places with a generalized HIV epidemic. To illustrate these broader social and economic impacts, this study aims to evaluate how disclosing HIV-negative status to sexual partners affects intimate partner and transactional sex relationships among women with multiple sexual partners in Kenya.

4.3 Methods

Using data from an RCT and an instrumental variable (IV) approach, we assessed the effects of disclosing HIV-negative status on intimate partner relationships, transactional sex prices, condom use, and agency among women with multiple sexual partners in Kenya.

4.3.1 Data and sample

The study data came from a cluster RCT, the *Jikinge* trial, that examines whether providing multiple self-tests to high-risk, HIV-negative women can promote HIV testing in their sexual networks, facilitate safer sexual decision-making, and reduce women's risk of acquiring HIV in the Nyanza region of Kenya (ClinicalTrials.gov identifier: NCT03135067). Between June 2017 and August 2018, about 2,090 adult women from 66 study clusters who reported having at least two sexual partners within the past four weeks were enrolled and randomized into one of the two study arms. In 33 randomly selected intervention clusters, participants were offered multiple oral fluid-based HIV self-tests, which provide results in 20 minutes, and encouraged to offer self-tests to current and potential partners with whom unprotected sex is likely. In the remaining 33 comparison clusters, participants were offered multiple referral cards for HIV voluntary counseling and testing (VCT) at local testing venues and were encouraged to hand out these cards to sexual partners. The intervention period lasted 18 – 24 months. Participants can obtain additional HIV self-tests or referral cards on 3-monthly basis. Four follow-up surveys are conducted every six months. The main outcomes of the *Jikinge* trial include HIV incidence, testing uptake, unprotected sex, and IPV. The analysis sample for this study consists of data from the baseline, 6-month, and 12-month follow-up surveys of the *Jikinge* trial. Nine individuals (less than 0.5% of the sample) with positive or indeterminate HIV testing results at 6 months were dropped from the dataset.

4.3.2 Measures

Exposure. The key variables of interest are two binary variables. First, the treatment assignment variable, *HIVST*, indicates whether a participant was assigned to the HIV self-tests or VCT referral card arm at baseline. This variable was used as an instrument in the main analyses. Second, *disclosing HIV-negative status* indicates whether a study participant shared her HIV-negative status with her primary partner in the past 6 months assessed at either the 6-month or 12-month follow-up surveys.

Outcome variables. Relationship outcomes include four variables assessed at the 12-month survey. *Same primary partner* indicates whether a participant was with the same primary partner she had at 6 months. *Ended sexual relationship* indicates whether a participant had a sexual relationship ended in the past 6 months, between 6 and 12 months, either by the participant or a sexual partner. *Number of sexual partners* and *number of transactional partners* indicate the total number of partners and transactional sex partners, respectively, that a participant had in the past month.

Prices for transactional sex, condom use, and agency indicators were measured at both 6-month and 12-month follow-up surveys since we expected that HIV-status disclosure might have immediate and delayed effects on these outcomes. First, transactional sex prices include *charge per sexual encounter with* or *without condom* in Kenyan shillings. *Total value of sexual transactions* in the past month is the approximate total value of money, goods, or services a participant received in exchange for sex. *Typical monthly income* was also included as a measure of women's economic status. Second, condom use includes whether a participant used a condom the last time she had sex and whether a participant had difficulty negotiating condom use with primary sexual partner in the past month. Third, agency was measured by two variables that reflect women's ability to make choices for themselves. *Free from intimate partner violence* measures whether a participant reported no experiencing any type of physical, sexual, and emotional violence in the past 6 months. *Declined sex due to a partner's refusal to test or positive result* indicates whether a woman declined to have sex with any sexual partner after the partner refused to accept an HIV self-test/VCT voucher or after the partner tested HIV-positive in the past 6 months.

4.3.3. Statistical analyses

The unit of analysis is the study participant. To evaluate the effects of disclosing one's own HIV status, we used a two-stage least squares (2SLS) model to account for potential bias due to self-selected disclosure behaviors. We instrumented *disclosing HIV-negative status* using an ordinary least squares regression model in the first stage:

$$Disclose_{ij} = \alpha + \gamma HIVST_j + c_j + \varepsilon_{ij}$$

where $Disclose_{ij}$ is a binary variable indicating if the participant disclosed her HIV-negative status following an HIV self-test or facility-based VCT to her primary partner in the past 6 months, $HIVST_j$ is an indicator for if the participant belonged to the HIVST study arm, and c_j are study cluster fixed effects. Then in the second stage we included the fitted values for $Disclose_{ij}$ from the first stage in the regression of outcomes on disclosing HIV-negative status:

$$Y_i = \alpha + \delta \widehat{Disclose}_i + \varepsilon_i$$

where Y_i is the outcome of interest for participant *i* at either the 6-month or 12-month follow-up survey and $\widehat{Disclose}_\iota$ is predicted value of disclosing HIV-negative status at the follow-up surveys from the first stage regression. The effect, δ, is the estimated local average treatment effect (LATE) using IV, which shows the effect among subgroups of women who would disclose their HIV status if assigned to the HIV self-test arm but not the VCT arm. Standard errors were clustered at the level of geographic area clusters using the cluster-corrected Huber-White estimator. Since we included multiple variables that measure conceptually similar outcomes, we used the Romano-Wolf method to account for multiple hypothesis testing.[180] Specifically, we grouped all variables that measure relationship outcomes and all variables that measure transactional prices and income together and used the adjusted *p*-values in hypothesis testing.

We assessed the effects of disclosing HIV-negative status at 6 months on relationship outcomes measured at 12 months because we expected changes in relationship status would take some time to occur. By comparison, since the effects on transactional prices, condom use, and agency were more

immediate, we assessed the effects of disclosing HIV-negative status on these outcomes measured at the same survey round in addition to the delayed effects.

The IV approach aims to compare women in the HIV self-test group who disclosed their HIV-negative status as a result of the treatment and women in the VCT group who would have disclosed their HIV status if they had been randomized into the treatment group (i.e., LATE). Three key assumptions need to be met for the IV approach to yield unbiased estimator of the LATE.[181] First, the relevance assumption requires that the instrument, which is the experimental arm assignment, is correlated with *disclosing HIV-negative status*. The first stage regression from the two stage least squares of *disclosing* on intervention assignment yielded *F*-statistic of 52.5 and 36.5 at 6 months and 12 months respectively. These values are greater than the conventional rule of thumb that the *F* -statistic should be higher than 10,[182] indicating sufficient predictive power of the instrument. Second, the independence assumption suggests that the instrument does not share common causes with the relationship outcomes. We expect randomization to distribute confounders equally across the two study groups. Third, the exclusion restriction suggests that treatment assignment affects outcomes only through participant's HIV-status disclosure. While we could not test this directly, it is possible that the intervention assignment would affect women's relationship dynamics through other mechanisms related to access to HIV self-tests, such as women's knowledge of her own and her partner's HIV status, subsequent beliefs about partners' sexual behaviors or HIV risks, and partner's reaction to women's demand for testing. However, these other mechanisms are all closely related to women's HIV-status disclosure and we discussed these possibilities in the discussion section.

Two other statistical methods were used to estimate average treatment effects (ATE) for binary outcomes since 2SLS could produce inconsistent estimates in non-linear models.[183] First, the bivariate probit (BiProbit) models ran both stages simultaneously and assumed the error terms to be jointly normal. The Murphy's score test was used to check the goodness of fit.[184] Second, the two-stage residual-inclusion (2SRI) models ran the same first stage as 2SLS but used the residuals, instead of the predicted values of the treatment, together with the endogenous treatment variable in the second stage.[185] Standard

errors were bootstrapped with 500 repetitions. Since simulation studies suggested that BiProbit models produced less biased estimates than 2SRI,[186] results from BiProbit models were reported by default unless the assumption for joint normality was violated based on the score test. In principle, both BiProbit and 2SRI models estimate the effects over the entire population (i.e., ATE) and would produce different results from the LATE estimated by 2SLS.

In addition to the main analyses, we assessed whether the effects of disclosing HIV-negative status differed between women who were sex workers and those who were not. Sex workers were defined by whether participants reported sex work as their primary or secondary source of income. 2SLS models were used to estimate the differential effects among these two subgroups.

This study is not considered human subject research because there was no interaction with research subjects and only de-identified data were used in the analyses. Statistical tests were 2-sided and the statistical significance was set at $p<.05$. Analyses were performed using Stata, version 15.1 (StataCorp LLC).

4.4 Results

A total of 2087 women were enrolled at the baseline survey (Table 12). Of these 2087 women, 1831 (87.7%) remained in the sample at the 6-month follow-up survey and 1783 (85.4%) remained at the 12-month survey. Attrition rates did not vary between study groups (Table 13). At the 6-month survey, 1772 (96.8%) women had a primary partner and 1822 (99.5%) tested HIV-negative (Table 12). These 1822 women constituted the full analysis sample.

Table 14 shows the baseline characteristics of the analysis sample by study arm. Among the 1822 women in the sample, 66.2% were married, 95.0% had ever exchanged sex for money, and 49.2% experienced IPV in the past 12 months. On average these women supported three other people financially, had two transactional sex partners in the past month, and earned about US$33 from transactional sex in the past month. Demographic and socioeconomic characteristics did not vary by experimental arm at baseline.

Effects on sexual relationship. At 6 months, 91.3% of participants in the HIV self-test group and 71.9% of those in the VCT group disclosed their HIV status to sexual partners. Table 15 shows the effects of disclosing HIV-negative status at 6 months on relationship outcomes at 12 months. Disclosing HIV-negative status reduced the likelihood of having ended any sexual relationship in the past 6 months by 27.5 percentage points (95% confidence interval [CI]: -0.494 – -0.056) based on the 2SLS model, almost eliminating the possibility of any relationship dissolution. The BiProbit and 2SRI models produced similar effect sizes. Disclosure of HIV status did not have any effect on the likelihood of staying with the same primary partner or number of sexual partners.

Panel A of Table 16 shows that for more than 90% of women who had relationship dissolution in the past 6 months, the relationship was ended by women themselves instead of by their sexual partners. In addition, about a quarter of these women cited reasons related to HIV testing or positive HIV results for relationship dissolution at the 12-month survey. These suggest that women were the decision-makers in relationship dissolution and that HIV status information provided to the partners or obtained from the partners influenced their decisions. By disclosing her own HIV-negative status, a woman might be more assertive in demanding to know her partner's HIV status, as indicated by a strong correlation between women's own disclosure and her partner's testing behavior at 6 months ($r = 0.545$, $p < 0.001$) and 12 months ($r = 0.613$, $p < 0.001$). Since less than 5% of women had any HIV-positive sexual partners at 12 months (Panel B of Table 16), sexual relationships might be more stable as women chose to stay longer with sexual partners after knowing their HIV-negative status.

Effects on transactional sex prices. Table 17 shows the immediate and delayed effects of disclosing HIV status on transactional sex prices and income. Overall, disclosing HIV-negative status increased the price for unprotected sex at 12 months but had no effect on the price for sex with a condom or income. Panel B shows that disclosing HIV-negative status at 12 months had an immediate effect and increased the price for transactional sex without condom by KES$764.805 (95% CI: 187.165 – 1342.445), which was about US$7 and a substantial 80% increase from the control group mean. HIV-negative status might have signaled lower risk and higher "safety" to transactional partners and allowed women to charge

higher prices for sex without a condom. This effect was only significant at 12 months, perhaps because it took some time for women to learn how to use HIV status information in price negotiations. However, disclosing HIV-negative status did not raise women's total income from transactional sex.

Effects on condom use and agency. Table 18 shows the immediate and delayed effects of disclosing HIV status on condom use and agency. Disclosing HIV-negative status did not have any effect on actual condom use or women's ability to negotiate condom use. This suggests that individuals did not stop using condoms even with the information that women had relatively low risk of transmitting HIV to their sexual partners. In addition, there was no effect on women's experience of IPV, suggesting that disclosing HIV-negative status did not put women at greater harm. Meanwhile, disclosing HIV-negative status increased women's likelihood of declining sex after a partner refused to get tested or was tested positive by at least 18.5 percentage points (95% CI: 0.091 – 0.279), indicating women's greater agency in sexual behaviors. However, this effect was only observed at the 6-month survey.

Effects by sex worker status. Table 19 shows that disclosing HIV-negative status had differential effects among sex workers and non-sex workers on relationship dissolution, prices for and income from transactional sex, and agency. Panel A and Panel D suggest that disclosing HIV-negative status reduced relationship dissolution and enhanced women's ability to decline sex only among sex workers. By comparison, Panel B shows that the effects on transactional sex prices and income were observed only among non-sex workers. In particular, disclosure led to substantial increases in transactional sex prices and more than doubled transactional sex income for women who did not consider sex work as their first or secondary source of income, although the sample sizes were relatively small.

4.5 Discussion

For women with multiple sexual partners who often engage in transactional sex, disclosing their HIV-negative status has important implications for sexual relationships in a generalized HIV epidemic. Our study suggests that disclosing HIV-negative status to sexual partners decreased relationship dissolution, increased the price women could charge for transactional sex, and allowed them to decline sex if a partner

refused to test for HIV or tested HIV-positive. These findings indicate that HIV status information informed women's decision-making and enhanced their agency in sexual relationships in this setting.

Our finding shows that disclosing HIV-negative status made sexual relationships more stable for women with multiple sexual partners, for which we offer three potential explanations. First, in an HIV epidemic, sexual partners' HIV status and their propensity to engage in risky sexual behaviors is valuable in partner selection but often hidden. Thus, women's disclosure of their HIV-negative status, a desirable trait, might make male sexual partners want to stay in a relationship longer. However, women in the study had a high level of decision-making power as the majority of them ended sexual relationships themselves, making this explanation less plausible. A second explanation is that a woman could infer her sexual partners' HIV status, sexual behaviors, and faithfulness through her own HIV-negative results. This would imply that positive perceptions about sexual partners, instead of the disclosure behavior, was the reason for decreased relationship dissolution. A third mechanism is that after a woman discloses her own HIV-negative status, she might be more assertive in demanding to know a sexual partner's HIV status and could use this information to make informed relationship decisions. HIV self-testing implementation studies from Malawi have shown that women felt empowered to offer HIV self-testing kits to their partners and that concordant HIV-negative couples reported increased trust and stronger relationships.[187] In addition, individuals could signal their sexual safety to potential partners through repeated HIV testing, which accelerated marriage among young unmarried women in Malawi.[188] Thus, based on sexual partner's HIV-negative result, women might have decided to stay with a "safe" partner with whom she would have broken up without the HIV information. Similar to the second explanation, this suggests that women used HIV information to make informed decisions in sexual relationships. The fact that the effect was only significant among sex workers provides further support to an agency-based explanation as sex workers had greater incentives to maintain relationships with multiple clients.

The positive effect of HIV-status disclosure on transactional sex prices is important to the economic wellbeing of women and their households as women in the dataset provided food, housing, or money to three family members on average. Disclosing HIV-negative status allowed women to charge significantly

more for unprotected sex, potentially because HIV-negative status signaled greater "quality" of transactional sex for clients. However, this effect was only observed at 12 months and disclosure did not increase income from transactional sex. In addition, the effect on transactional sex prices was not significant among sex workers. By comparison, for women who did not rely on sex work for income and mostly worked in sales/services, fishing/trade, and unskilled manual labor, disclosing HIV-negative status raised prices for sex with and without condom and more than doubled their income from transactional sex. Qualitative research on sex workers in Uganda and South Africa has revealed that while women had some agency, the reliance on sex work for livelihood, especially the need to earn money for child care, often limits how much women could bargain in transactional sex relationships.[189,190] Since the effects of HIV-status disclosure on economic agency in transactional sex might be moderated by women's own bargaining power, it might be easier for women who were less dependent on sex work to negotiate higher pay for transactional sex.

It is reassuring that disclosing HIV-negative status did not decrease condom use despite the fact that women could charge more for unprotected transactional sex.[175] Based on qualitative findings from the same setting, sex workers who had access to HIV self-tests told clients that they would agree to have unprotected sex only if the clients used self-tests and tested HIV-negative, but these women deliberately avoided having unprotected sex with HIV-negative clients by telling them they still had to go for confirmatory tests.[166] This might be motivated by women's desire to further reduce risks of HIV infection after discovering their own HIV-negative status.[167,191] Multiple studies have shown a correlation between knowledge of own HIV-negative status and increased condom use among young unmarried women,[192] married women,[167] and sex workers.[62,63] Our finding contributes to this literature by showing that disclosing HIV-negative status to sexual partners did not reduce women's ability to insist on using condoms despite potentially higher economic gains. Meanwhile, we did not find any increase in condom use, perhaps because while some women used condoms more often to maintain their HIV-negative status, other women chose to have unprotected sex after learning their sexual partners' HIV-negative status.

The positive effect of HIV-status disclosure on women's ability to decline sex and the null effect on IPV highlight that women in our study could use HIV information to safely make informed choices in sexual behaviors. A systematic review suggests that sex workers have limited ability to refuse a client who is unwilling to use a condom.[175] In our study, sex workers who disclosed her own HIV-negative status were able to refuse a client if he did not accept HIV testing or tested HIV-positive. However, this strategy to screen out high-risk clients only worked in the first six months of the study, potentially because women were able to convince sexual partners to test for HIV and the majority of them were HIV-negative. Importantly, the evidence did not point to greater risks of IPV as a result of disclosing HIV-negative status and potentially demanding partners' HIV information. Although a systematic review concluded that HIV self-testing did not increase risk-taking behaviors or harm to women,[193] recent qualitative research has shown that some women who persuaded their partner to test for HIV reported verbal or physical abuse and economic hardship.[187] The lack of effect on IPV in this study suggests that disclosing HIV-negative status to a sexual partner did not lead to relationship strain that would put women at greater harm. This might be because when introducing self-tests to clients, women used various strategies to avoid conflict with or physical harm from sexual partners, such as not responding to angry partners or forgoing pay, based on qualitative research with sex workers in similar settings.[166]

This study has several limitations. First, the results based on the IV approach should be interpreted carefully and have limited generalizability. The IV approach precisely estimated the effects of HIV-status disclosure among subgroups of women who would disclose their HIV-negative status if assigned to the HIV self-tests arm but not to the VCT arm. Thus, if these women had unobserved characteristics that are correlated with the outcomes of interest, the findings might not be generalizable to other women. However, the estimates based on BiProbit or 2SRI models produced largely similar results, suggesting that this subgroup might not be that different from an average woman in the sample. Second, several key measures used in the analyses are subject to reporting bias as they rely on self-reported behaviors for sensitive topics about sexual relationships, such as HIV-status disclosure, condom use, and IPV. Meanwhile, findings on outcomes such as relationship dissolution or transactional prices might suffer less

from misreporting. Third, there are considerable missing values in certain outcomes, especially for those related to transactional sex prices. For example, if women who did not experience any change in transactional sex prices were more likely to skip these questions, the finding we produced would overestimate the true impacts of HIV-status disclosure. Fourth, the main exposure variable, disclosure of HIV status, was defined based on whether a participant shared her HIV test result with her primary partner. However, many outcomes we examined measured sexual relationships with both primary and non-primary partners. Although we do not have data on whether participants shared HIV status with non-primary partners, we expect that women who disclosed HIV status to one partner were more likely to share the same information with other sexual partners. Lastly, although the study quantifies the effects of HIV-status disclosure, it does not answer how women used HIV information to inform their relationship decisions, to what extent women could control sexual behaviors when their preferences did not align with their partners' preferences, or why there were differential effects among sex workers vs. non-sex workers. Although we provided potential explanations, qualitative research is essential to provide an in-depth understanding of the mechanisms through which HIV information affects women's sexual relationships.

4.6 Conclusions

In a generalized HIV epidemic, potential HIV transmission is a constant risk that affects individuals' decision-making in sexual relationships and HIV-negative status is considered a desirable but often hidden trait in sexual partners. This paper uses the exogenous variation in HIV-status disclosure from a randomized design to show that women with multiple sexual partners in Kenya used their HIV-negative status to make informed decisions in their sexual relationships. Specifically, disclosing HIV-negative status reduced relationship dissolution, increased prices for transactional sex, and allowed women to exert greater control over sexual behaviors. Our study contributes to the small but growing literature on the broader beneficial effects of expanded HIV testing services beyond identifying HIV-positive cases, including safer sexual behaviors and better psychological wellbeing among HIV-negative women.[62–64] Importantly, our findings echo previous research that argues that women, even those who are dependent

on sex work, have agency in sexual relationships and HIV information could help them further exercise this agency.

TABLE 12. Sample description

	Full Sample	Intervention	Control
Number of clusters	66	33	33
Total number of women at the baseline survey	2087	1054	1033
Ever exchanged sex for money	1981 (94.9%)	998 (94.7%)	983 (95.2%)
Women who completed the 6-month survey, % of baseline	1831(87.7%)	919 (87.2%)	912 (88.3%)
With a *primary partner*	1772 (96.8%)	888 (96.6%)	884 (96.9%)
Tested HIV-negative	1822 (99.5%)	915 (99.6%)	907 (99.5%)
Women who completed the 12-month survey, % of baseline	1783 (85.4%)	896 (85.0%)	887 (85.9%)
With a *primary partner*	1708 (95.8%)	862 (96.2%)	846 (95.4%)
Tested HIV-negative	1766 (99.1%)	886 (98.9%)	880 (99.2%)

Primary partner includes husband, boyfriend, and someone a woman regularly has sex with.
HIV test results include positive and indeterminate in addition to negative.

TABLE 13. Comparison of attrition across intervention arms

	(1) Lost to follow-up at 6-month survey	(2) Lost to follow-up at 12-month survey
Intervention	0.011	0.008
	[-0.024,0.046]	[-0.026,0.043]
Observations	2087	2087

95% confidence intervals in brackets
Regression estimates from linear probability models that included strata fixed effects.
Standard errors are clustered at the level of geographic area clusters.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 14. Baseline characteristics by experimental arm

	All	Intervention	Control	*p*-value
N	1822	915	907	
Age, mean (SD)	27.5 (6.9)	27.6 (6.9)	27.4 (6.9)	0.62
Highest level of school completed				0.58
less than primary	593 (32.5%)	302 (33.0%)	291 (32.1%)	
primary	566 (31.1%)	274 (29.9%)	292 (32.2%)	
more than primary	663 (36.4%)	339 (37.0%)	324 (35.7%)	
Number of people to support, mean (SD)	3.0 (2.2)	2.9 (2.1)	3.0 (2.2)	0.56
Overall health				0.17
Very Good	45 (2.5%)	22 (2.4%)	23 (2.5%)	
Good	602 (33.0%)	294 (32.1%)	308 (34.0%)	
Fair	1073 (58.9%)	537 (58.7%)	536 (59.1%)	
Poor	102 (5.6%)	62 (6.8%)	40 (4.4%)	
Typical monthly income (KES), mean (SD)	4731.4 (5221.4)	4832.3 (5246.8)	4629.5 (5196.6)	0.41
Marital status				0.16
Married	1207 (66.2%)	590 (64.5%)	617 (68.0%)	
In a relationship but not married	167 (9.2%)	94 (10.3%)	73 (8.0%)	
Not in a relationship	448 (24.6%)	231 (25.2%)	217 (23.9%)	
With a primary partner	1747 (95.9%)	878 (96.0%)	869 (95.8%)	0.88
Number of sexual partners last month, mean (SD)	2.8 (1.7)	2.8 (2.0)	2.7 (1.2)	0.40
Age at 1st sex, mean (SD)	15.9 (2.2)	15.8 (2.3)	15.9 (2.2)	0.20
Ever exchanged sex for money	1730 (95.0%)	865 (94.5%)	865 (95.4%)	0.42
Sex work as primary income source	272 (14.9%)	128 (14.0%)	144 (15.9%)	0.26
Age difference (partner - woman), mean (SD)	5.5 (5.2)	5.4 (5.3)	5.7 (5.1)	0.22
Experienced intimate partner violence past 12 months	896 (49.2%)	442 (48.3%)	454 (50.1%)	0.46
Had no difficulty negotiating condom use last month	596 (40.9%)	312 (42.3%)	284 (39.6%)	0.29
Number of men exchanged sex with last month, mean (SD)	1.9 (1.7)	2.0 (2.1)	1.9 (1.2)	0.40
Charge per sexual encounter with condom (KES), mean (SD)	921.1 (1204.6)	950.1 (1482.7)	891.4 (827.2)	0.40
Charge per sexual encounter without condom (KES), mean (SD)	1205.9 (1401.0)	1202.5 (1432.2)	1209.3 (1369.8)	0.93
Total value of sex transactions last month (KES), mean (SD)	3405.0 (5090.1)	3429.9 (5603.8)	3380.6 (4536.3)	0.89

Abbreviations: SD, standard deviation; KES: Kenyan Shillings
Sample included only women who completed the 6-month survey and excluded those with HIV testing results that were positive or indeterminate (less than .5% of the full sample).

TABLE 15. Effects of disclosing HIV-negative status on relationship outcomes

	(1) Same primary partner	(2) Ended any sexual relationship	(3) Number of sexual partners	(4) Number of transactional partners
2SLS	0.033	-0.275*	0.298	0.565
	[-0.133,0.200]	[-0.494,-0.056]	[-0.714,1.311]	[-0.395,1.526]
BiProbit	0.045	-0.260**	--	--
	[-0.125,0.215]	[-0.435,-0.085]		
p-value for Murphy's score test	0.858	0.978	--	--
2SRI	0.042	-0.286*	--	--
	[-0.134,0.218]	[-0.517,-0.054]		
N	1611	1611	1611	1351
Control mean	0.889	0.291	1.962	1.517

95% confidence intervals in brackets

Disclosing HIV-negative status indicates a participant disclosed her HIV-negative status to her primary partner in the past 6 months. Study arm assignment was used to instrument *disclosing own HIV status* at 6-month follow-up survey with a first-stage *F*-statistic of 52.5. Outcomes were measured at 12-month follow-up survey. *Same primary partner* indicates whether a participant was with the same primary partner she had 6 months ago. *Ended any sexual relationship* indicates whether a participant had a sexual relationship ended in the past 6 months by the participant or any of her partners. The time frame for *Number of sexual partners* and *Number of transactional partners* is past month. 2SLS: two-stage least squares model with standard errors clustered at the level of geographic area clusters. BiProbit: bivariate probit model with errors modeled as jointly normal (Murphy's score test was used to test departure from normality). 2SRI: two-stage residual inclusion model that used a probit model in both stages with bootstrapped standard errors (500 repetitions). In the 2SLS models, all outcomes were grouped together to adjust for multiple hypothesis testing using the Romano-Wolf method.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 16. Relationship dissolution and partner testing details

	6-month survey		12-month survey	
	All women	Sex workers	All women	Sex workers
Panel A. Relationship dissolution details				
One or more sexual relationship ended, % of all women	589 (32.3%)	422 (34.6%)	466 (26.4%)	328 (27.9%)
Number of women who decided themselves (instead of their partners) to end a relationship, % of all women who had relationship ended	536 (90.9%)	380 (89.8%)	439 (94.2%)	310 (94.5%)
Number of women whose relationship ended due to partner's refusing to get tested or positive HIV test results, % of all women who had relationship ended	222 (37.6%)	160 (37.8%)	112 (24.0%)	79 (24.1%)
Panel B. Primary partner testing details				
Number of women who gave their sexual partners HIV self-tests or vouchers for facility-based tests, % of all women	1694 (93.0%)	1125 (92.2%)	1607 (91.2%)	1066 (90.7%)
Number of women who had at least one HIV-positive sexual partner, % of all women who gave sexual partners HIV self-tests or vouchers	176 (10.4%)	123 (10.9%)	78 (4.9%)	51 (4.8%)

***** *Who decided to end relationship* and *relationship ended due to partner's refusing to HIV test or positive results* are based on the first episode of relationship dissolution participants reported. For women who had relationship ended, fewer than 5% reported decision-maker and 20% reported reasons for more than one episode of relationship dissolution. *Sex worker* status was defined by whether sex work was a participant's primary or secondary source of income measured at the baseline survey. The time frame for all outcomes is past 6 months.

TABLE 17. Effects of disclosing HIV-negative status on transactional sex prices and income

Outcome:	(1) Charge per sexual encounter with condom (KES)	(2) Charge per sexual encounter without condom (KES)	(3) Total value of sexual transactions last month (KES)	(4) Typical monthly income (KES)
Panel A: Disclosing HIV status at 6-month				
Effects at 6-month	359.417	509.210	1,070.244	-1459.222
	[-578.436,1,297.271]	[-534.633,1,553.052]	[-1128.597,3,269.086]	[-6927.554,4,009.109]
N	1073	843	1316	1752
Control mean	760.387	968.456	2,352.196	3,832.120
Effects at 12-month	495.690	649.079	-51.328	2,674.767
	[-193.416,1,184.796]	[41.698,1,256.461]	[-2312.334,2,209.677]	[-1275.224,6,624.757]
N	910	712	1086	1608
Control mean	783.333	936.131	1,946.750	3,950.350
Panel B: Disclosing HIV status at 12-month				
Effects at 12-month	535.107	764.805*	967.588	2,051.073
	[-375.491,1,445.705]	[187.165,1,342.445]	[-1449.504,3,384.679]	[-1966.236,6,068.383]
N	953	737	1131	1687
Control mean	731.737	913.139	1,937.563	3,805.158

95% confidence intervals in brackets

Disclosing HIV-negative status indicates a participant shared her HIV-negative status with her partner in the past 6 months. Study arm assignment was used to instrument *disclosing own HIV status* at 6-month and 12-month follow-up surveys with first-stage *F*-statistic of 52.5 and 36.5 respectively. Two-stage least squares models with standard errors clustered at the level of geographic area clusters. Outcomes were grouped together to adjust for multiple hypothesis testing using the Romano-Wolf method.

KES: Kenyan shillings

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 18. Effects of disclosing HIV-negative status on condom use and agency

Outcome:	(1) No difficulty negotiating condom use	(2) Condom use last sex	(3) Free from intimate partner violence	(4) Declined sex due to refusal to test or positive result
Panel A: Disclosing HIV status at 6-month				
Effects at 6-month	-0.265	-0.159	0.160	0.315**
	[-0.695,0.165]	[-0.512,0.193]	[-0.160,0.480]	[0.119,0.511]
Model	2SLS	2SLS	2SLS	2SLS
Effects at 6-month	-0.187	-0.115	0.186	0.185***
	[-0.444,0.070]	[-0.356,0.125]	[-0.122.,0.494]	[0.091,0.279]
Model	BiProbit	BiProbit	2SRI	BiProbit
N	1588	1758	1758	1754
Control mean	0.442	0.409	0.805	0.073
Effects at 12-month	-0.179	-0.063	-0.057	0.000
	[-0.643,0.285]	[-0.372,0.247]	[-0.274,0.160]	[-0.126,0.126]
Model	2SLS	2SLS	2SLS	2SLS
Effects at 12-month	-0.166	-0.079	-0.038	0.004
	[-0.474,0.141]	[-0.324,0.166]	[-0.170, 0.094]	[-0.075,0.084]
Model	BiProbit	BiProbit	2SRI	BiProbit
N	1369	1610	1611	1609
Control mean	0.452	0.405	0.917	0.066
Panel B: Disclosing HIV status at 12-month				
Effects at 12-month	-0.105	0.001	-0.077	-0.050
	[-0.517,0.308]	[-0.364,0.365]	[-0.307,0.152]	[-0.194,0.094]
Model	2SLS	2SLS	2SLS	2SLS
Effects at 12-month	-0.016	0.027	-0.065	-0.058
	[-0.297,0.265]	[-0.244,0.297]	[-0.228,0.098]	[-0.252,0.136]
Model	BiProbit	BiProbit	2SRI	BiProbit
N	1477	1688	1689	1687
Control mean	0.424	0.413	0.940	0.054

95% confidence intervals in brackets

Disclosing HIV-negative status indicates a participant shared her HIV-negative status with her partner in the past 6 months. Study arm assignment was used to instrument *disclosing own HIV status* at 6-month and 12-month follow-up surveys with first-stage *F*-statistic of 52.5 and 36.5 respectively. *No difficulty negotiating condom use* indicates that a participant had difficulty negotiating condom use with primary sexual partner in the past month. *Condom use last sex* indicates a participant used a condom the last time she had sex with any sexual partner. *Free from intimate partner violence* indicates that a participant was free from any type of physical, sexual, and emotional violence in the past 6 months. *Declined sex due to refusal to test or positive result* indicates that a woman declined to have sex with any sexual partner after the partner refused to accept a HIV self-test/voucher or after the partner tested HIV-positive in the past 6 months. BiProbit: bivariate probit model with errors modeled as jointly normal (Murphy's score test is used to test departure from normality). 2SLS: two-stage least squares model with standard errors clustered at the level of geographic area clusters. 2SRI: two-stage residual inclusion model that used a probit model in both stages with bootstrapped standard errors (500 repetitions). For binary outcomes, results from BiProbit models were reported by default. If the assumption of joint normality was not met, results from 2SRI models were reported.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 19. Effects of disclosing HIV-negative status by sex worker status

	(1)	(2)	(3)	(4)
	Panel A: disclosure at 6-month on relationship outcomes at 12-month			
	Same primary partner	Ended any sexual relationship	Number of sexual partners	Number of transactional partners
Sex workers	0.104	-0.454**	0.426	0.472
	[-0.122,0.330]	[-0.789,-0.120]	[-0.803,1.655]	[-0.743,1.687]
N	1073	1073	1073	928
Control mean	0.868	0.308	1.978	1.577
Non-sex workers	-0.072	-0.014	0.02	0.683
	[-0.347,0.203]	[-0.292,0.265]	[-1.118,1.157]	[-0.317,1.683]
N	538	538	538	423
Control mean	0.925	0.262	1.935	1.402
p-value	0.578	0.024	0.769	0.376
	Panel B: disclosure at 6-month on transactional sex prices and income at 6-month			
	Charge per sexual encounter with condom (KES)	Charge per sexual encounter without condom (KES)	Total value of sexual transactions last month (KES)	Typical monthly income (KES)
Sex workers	-7.052	62.254	-213.584	-768.429
	[-1341.059,1,326.955]	[-1330.982,1,455.490]	[-3144.288,2,717.121]	[-4561.378,3,024.520]
N	735	600	889	1168
Control mean	846.522	1,015.79	2,707.14	3,853.55
Non-sex workers	706.371	957.83	1,202.70	-3818.587
	[-76.089,1,488.831]	[-230.596,2,146.256]	[-716.315,3,121.722]	[-1.53e+04,7,698.254]
N	338	243	427	584
Control mean	610.303	885.185	1,770.60	3,796.64
p-value	0.188	0.282	0.466	0.772
	Panel C: disclosure at 12-month on transactional sex prices and income at 12-month			
	Charge per sexual encounter with condom (KES)	Charge per sexual encounter without condom (KES)	Total value of sexual transactions last month (KES)	Typical monthly income (KES)
Sex workers	93.662	517.916	-541.106	179.829
	[-912.888,1,100.213]	[-284.736,1,320.568]	[-3588.826,2,506.613]	[-4746.916,5,106.574]
N	645	523	772	1125
Control mean	843.689	995.556	1,950.39	4,158.93
Non-sex workers	1,373.127*	1,038.566*	3,212.856*	3,743.66
	[167.133,2,579.121]	[97.093,1,980.038]	[673.615,5,752.098]	[-821.161,8,308.473]
N	311	219	364	572
Control mean	690.566	879.487	2,073.39	3,805.94
p-value	0.068	0.035	0.035	0.275
	Panel D: disclosure at 6-month on condom use and agency at 6-month			
	No difficulty negotiating condom use	Condom use last sex	Free from intimate partner violence	Declined sex due to refusal to test or positive result
Sex workers	-0.263	-0.162	-0.016	0.415**
	[-0.744,0.219]	[-0.605,0.282]	[-0.348,0.315]	[0.152,0.678]
N	1053	1172	1172	1170
Control mean	0.482	0.399	0.838	0.081
Non-sex workers	-0.284	-0.131	0.424	0.141
	[-0.800,0.232]	[-0.507,0.244]	[-0.053,0.901]	[-0.047,0.329]
N	535	586	586	584
Control mean	0.377	0.425	0.75	0.059
p-value	0.452	0.688	0.194	0.007
	Panel E: disclosure at 12-month on condom use and agency at 12-month			
	No difficulty negotiating condom use	Condom use last sex	Free from intimate partner violence	Declined sex due to refusal to test or positive result
Sex workers	0.011	-0.008	-0.177	-0.114
	[-0.500,0.521]	[-0.444,0.428]	[-0.422,0.068]	[-0.312,0.084]
N	978	1125	1126	1124
Control mean	0.412	0.414	0.935	0.089
Non-sex workers	-0.289	0.098	0.152	0.022
	[-0.776,0.199]	[-0.400,0.595]	[-0.212,0.515]	[-0.166,0.210]
N	505	573	573	573
Control mean	0.516	0.382	0.892	0.039
p-value	0.453	0.924	0.198	0.488

95% confidence intervals in brackets

Sex worker status was defined by whether sex work was a participant's primary or secondary source of income measured at the baseline survey. *Disclosing HIV-negative status* indicates a participant disclosed the result of her HIV test to her primary partner

in the past 6 months. *Same primary partner* indicates whether a participant was with the same primary partner she had 6 months ago. *Ended any sexual relationship* indicates whether a participant had a sexual relationship ended in the past 6 months by the participant or any of her partners. The time frame for *Number of sexual partners* and *Number of transactional partners* is past month. *No difficulty negotiating condom use* indicates that a participant had difficulty negotiating condom use with primary sexual partner in the past month. *Condom use last sex* indicates a participant used a condom the last time she had sex with any sexual partner. *Free from intimate partner violence* indicates that a participant was free from any type of physical, sexual, and emotional violence in the past 6 months. *Declined sex due to refusal to test or positive result* indicates that a woman declined to have sex with any sexual partner after the partner refused to accept a HIV self-test/voucher or after the partner tested HIV-positive in the past 6 months. Study arm assignment was used to instrument *disclosing own HIV status* at 6-month and 12-month follow-up survey with first-stage *F*-statistic of 52.5 and 36.5 respectively. All models are two-stage least squares models with standard errors clustered at the level of geographic area clusters. The *p*-value rows report the two-sided *p*-values from a *Wald*-test of equality of the treatment effects for sex workers vs. non-sex workers. KES: Kenyan shillings
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

CHAPTER 5: CONCLUSIONS

Policies and programs that expand access to sexual and reproductive health services in resource-limited settings can increase women's and girls' ability to exercise agency in decisions important to their health, development, and wellbeing. This dissertation has demonstrated their potential and limitations through evaluating three different interventions in sub-Saharan Africa (SSA), namely abortion legal reforms, free access to expanded family planning services, and HIV-status disclosure as a result of access to HIV self-tests. While all three interventions are associated with some positive impacts on various indicators of agency, they do not remove all constraints women and girls face to take control of their life, nor do they increase choice for all women and girls. The results from this dissertation provide a nuanced understanding of intervention impacts, call for careful consideration in policy decisions, and point to important topics for future research.

In Chapter 2, I studied policy reforms that expanded the legal grounds for abortion on marriage, birth, and schooling outcomes among adolescent girls and young women in 18 countries in SSA. I found that abortion liberalization was associated with a reduction of more than 7% of the annual likelihood of marriage and birth, with largest effects among those from younger age groups, rural areas, and lower wealth quintiles. Whether such reforms improved access to abortion services or merely signaled greater political will to protect women's health and rights, the liberalization of abortion laws enhanced the ability of adolescent girls and young women to make key life choices, had important implications for maternal and child health, and was especially beneficial to those with the fewest resources. However, the legal reforms did not have any effect on schooling rate, likely because other factors play a more direct role in girls' education decisions, such as economic pressures or lack of job opportunities. This chapter

contributes to the literature on broader implications of reproductive health policies on women's agency, which has been largely concentrated in high-income settings.

In Chapter 3, I assessed whether free access to a broad contraceptive method mix affected women's contraceptive use and decision-making in eight countries with high unmet needs for family planning services in SSA. I found that free access to both long- and short-term reversible contraceptives was associated with a modest increase in women's contraceptive use, which was driven by greater use of short-term methods. Among current contraceptive users, removing the cost barrier did not prompt women to switch to the more effective long-term methods or increase women's autonomy in contraceptive decision-making. While women that were older, more educated, and more knowledgeable about family planning used contraceptives more when services were offered for free, it is alarming that free access was associated with lower contraceptive use among teenagers. This might be because factors other than user fees are more prominent barriers to young women's uptake of family planning services, such as stockout of contraceptive commodities and provider biases against unmarried young women. The results from this chapter are consistent with existing literature in that removing user fees alone had only limited effects on women's contraceptive use, contrary to the evidence from high-income settings. The findings also highlight the importance of combining financial subsidies with other approaches to address multiple barriers women face in accessing sexual and reproductive health services.

In Chapter 4, I explored whether disclosing HIV-negative status affected intimate partner and transactional sex relationships for women with multiple sexual partners in Kenya. I found that disclosing HIV-negative status to sexual partners reduced relationship dissolution, substantially increased price of unprotected transactional sex, and increased women's ability to decline sex with a partner who might be HIV-positive. Meanwhile, disclosing HIV-negative status did not affect condom use or put women at greater risk of violence. These results indicate that women could safely use HIV information to inform their decisions in sexual relationships, contributing to the small but growing literature on the beneficial effects of expanded testing services in a generalized HIV epidemic beyond identifying HIV-positive cases.

The ways women and girls exercise agency, or the ability to make strategic life choices,[23] vary widely across settings. These three studies focused on different interventions, indicators of agency, and mechanisms through which women and girls can gain and express agency, which reflects the fact that decision-making power manifests in multiple and often intersecting domains in a woman's life. Despite these differences, in all three studies I explored the intersectionality between gender and other forms of inequality through analyzing the differential effects of interventions among key segments of the study populations. While abortion liberalization was more beneficial to adolescent girls and young women from rural areas and lower economic background, free access to contraceptives did not seem to reach women who were most in need, including those younger, less educated, or less knowledgeable about family planning. Disclosing HIV-negative status led to higher income for women who did not consider sex work as their primary or secondary source of income. By comparison, sex workers whose livelihood was dependent on transactional sex were not able to bargain for higher pay, although HIV information allowed them to choose safer partners and have more control in sexual behaviors. The understanding of heterogeneous intervention effects is important to policy decisions as it provides insights on optimal ways to allocate limited resources while advancing equity. For example, to increase access to family planning services for all women, it might be more effective to combine partial subsidies, such as free provision to the most popular contraceptive methods, with other approaches that address information, supply chain, or provider barriers than full subsidies. Future studies should also prioritize evaluating interventions that target women and girls who face the combined discrimination from gender and other aspects of social, economic, and political identities.

This dissertation alludes that gender norms, or social norms reflected in individuals' attitudes about how women and men should behave based on their gender, moderate the effects of policies and programs, which echoes the findings from a recent review on interventions across different domains of women's agency.[194] For changes in abortion legality to lead to greater access to abortion services, there needs to be a social norm shift that gives women and girls more control over their body and in their childbearing decisions. Even if family planning services are provided free of charge, many women would still not gain

real access due to the expectations that a woman should not have sex before marriage, could only use a contraceptive method agreed upon by her husband, or would not need contraception before having several children. While HIV information could inform women's decision-making in transactional sex relationships, the stigma against sex workers, the social norm that condones sex work but refuses to formalize it, and the gender prejudices that limit women's economic opportunities will all restrict women's ability to benefit from expanded HIV testing services. High-level policy changes, such as the abortion legal reforms I studied, might accelerate changes in social norms, but perhaps a lower-hanging fruit is to start with changing women's own attitudes about gender norms. Future studies could include measures of individual and collective gender norms to assess interventions' effects on these important direct indicators of women's agency.

This dissertation used innovative approaches to make a significant contribution to the literature. First, I focused on important topics in sexual and reproductive health and went beyond health impacts to examine broader social and economic implications of these interventions on women's decision-making power. Second, I used advanced econometric methods to estimate intervention impacts, including difference-in-differences, propensity scores, and instrumental variables. These quasi-experimental approaches account for selection and produce findings that are more generalizable than randomized designs. Third, this dissertation demonstrated creative use of secondary data to answer new research questions. These approaches include combing data from policy database and existing surveys to generate a pseudo-panel dataset, linking individual and community-level survey data, and conducting secondary analysis of data from a randomized controlled trial.

The studies in this dissertation have several common limitations. First, all studies used quasi-experimental designs to estimate intervention effects and each approach makes different assumptions that could not be all tested. Future studies that use experimental designs might have higher internal validity than the quasi-experimental studies in this dissertation. Second, although these studies provided quantitative estimates of intervention effects, they did not address questions such as what it is like for women and girls to receive interventions, through what mechanisms these interventions affected women's

and girls' decision-making, or why the intervention impacts were only observed in certain subgroups of the study populations. Future research that uses qualitative or mixed methods will provide the details and depth that are essential to our understanding of complex and sensitive topics such as sexual behaviors and decision-making. Third, Chapel 2 and Chapel 3 pooled data from multiple countries and did not allow analysis of how country-specific implementation, socioeconomic contexts, and social norms affected policy or program impacts. Future analyses that focus on a single setting would provide more direct evidence for policy decisions. Fourth, the use of secondary data restricted the availability of variables to be used in the analyses. Some variables were not the ideal measures of the outcomes I studied, such as a single question to measure contraceptive decision-making, while other outcomes were not measured in the original surveys or were only available for a subset of study participants, such as abortion incidence or knowledge of sexual partner's HIV status. Nevertheless, the studies in this dissertation point towards important areas for primary data collection in future research and serve as a starting point for me to explore promising topics in the field of gender, health, and development.

Taken together, this dissertation shows that policies and programs in sexual and reproductive health could strengthen women's and girls' decision-making power in various domains of their life in resource-limited settings. Abortion legalization, free access to contraceptives, and HIV testing services all enable women and girls to make more informed decisions by expanding their choices. However, these interventions do not remove all barriers to exercising agency or equally benefit all women and girls, highlighting the importance of addressing multiple intersecting constraints. While each study contributes to a different strain of literature under the overarching subject of women's and girls' empowerment, a promising direction for future research across all studies is to use alternative and complementary analytical approaches to first understand and then shift gender norms that limit women's and girls' agency.

APPENDIX: ADDITIONAL FIGURES AND TABLES

FIGURE A1. Standardized differences for selected covariates

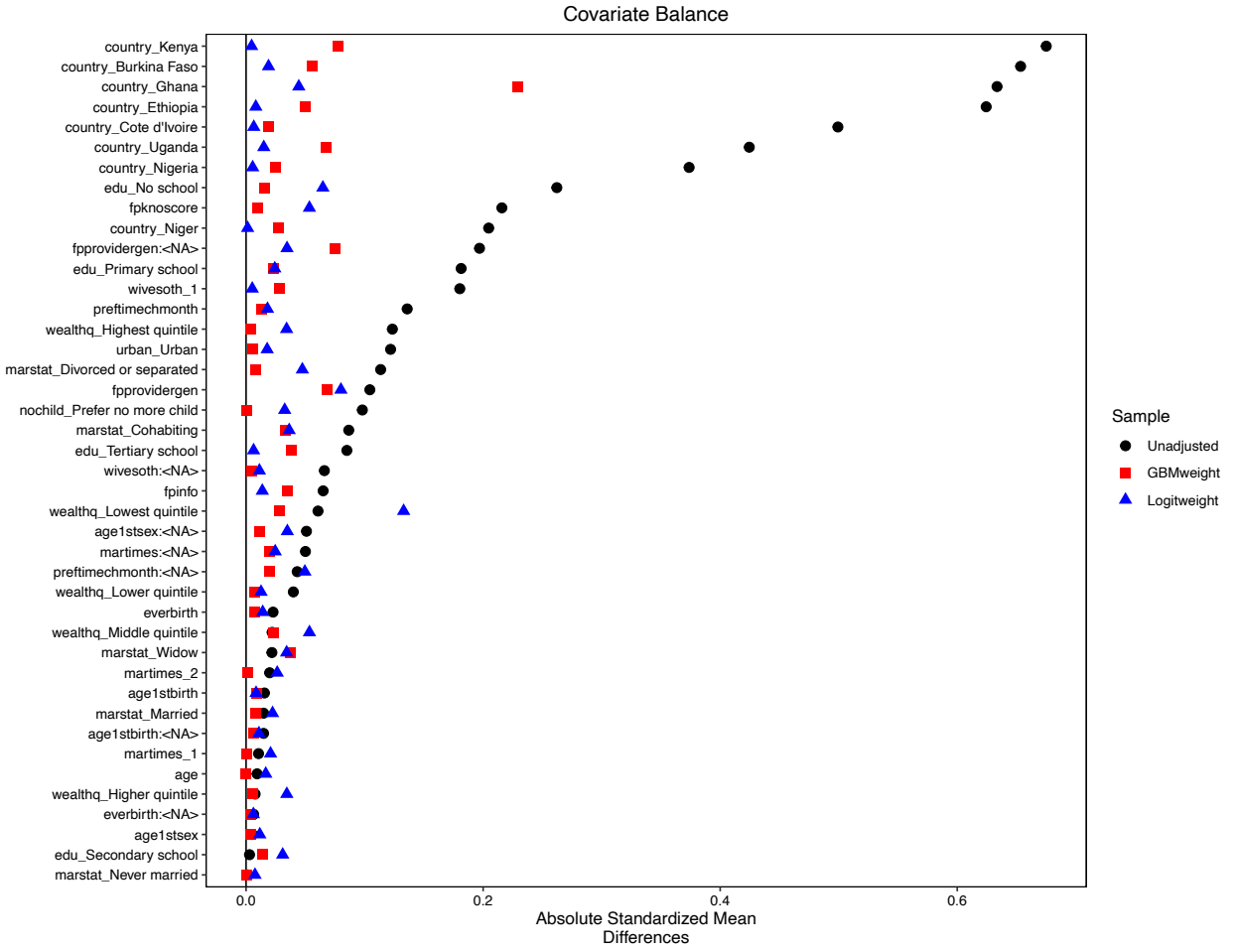


FIGURE A2. Distribution of propensity scores based on generalized boosted models

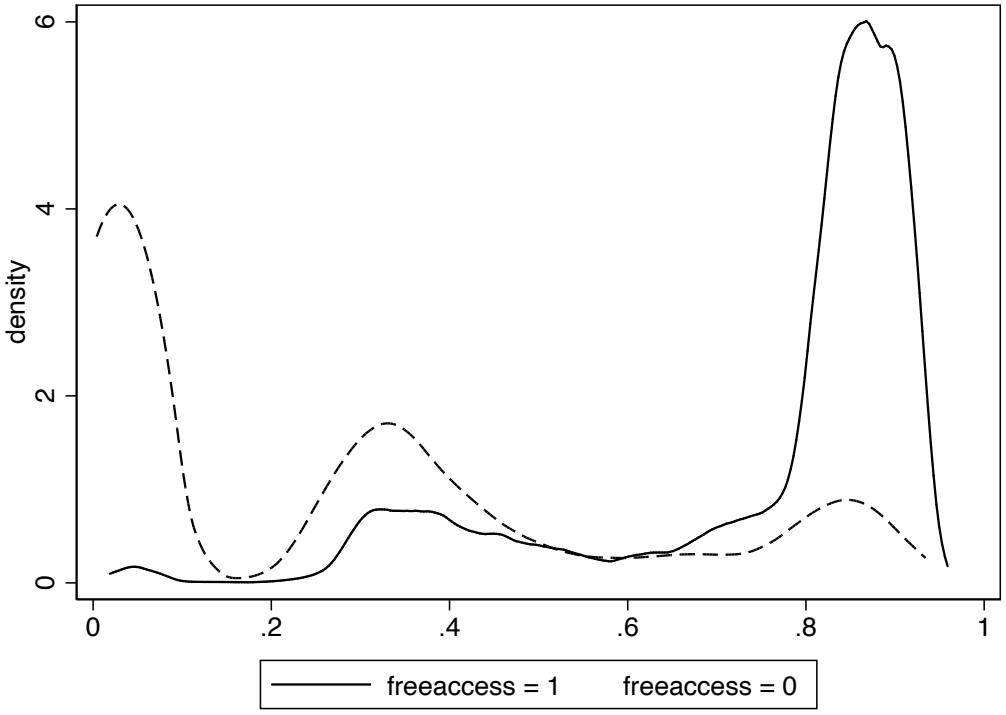
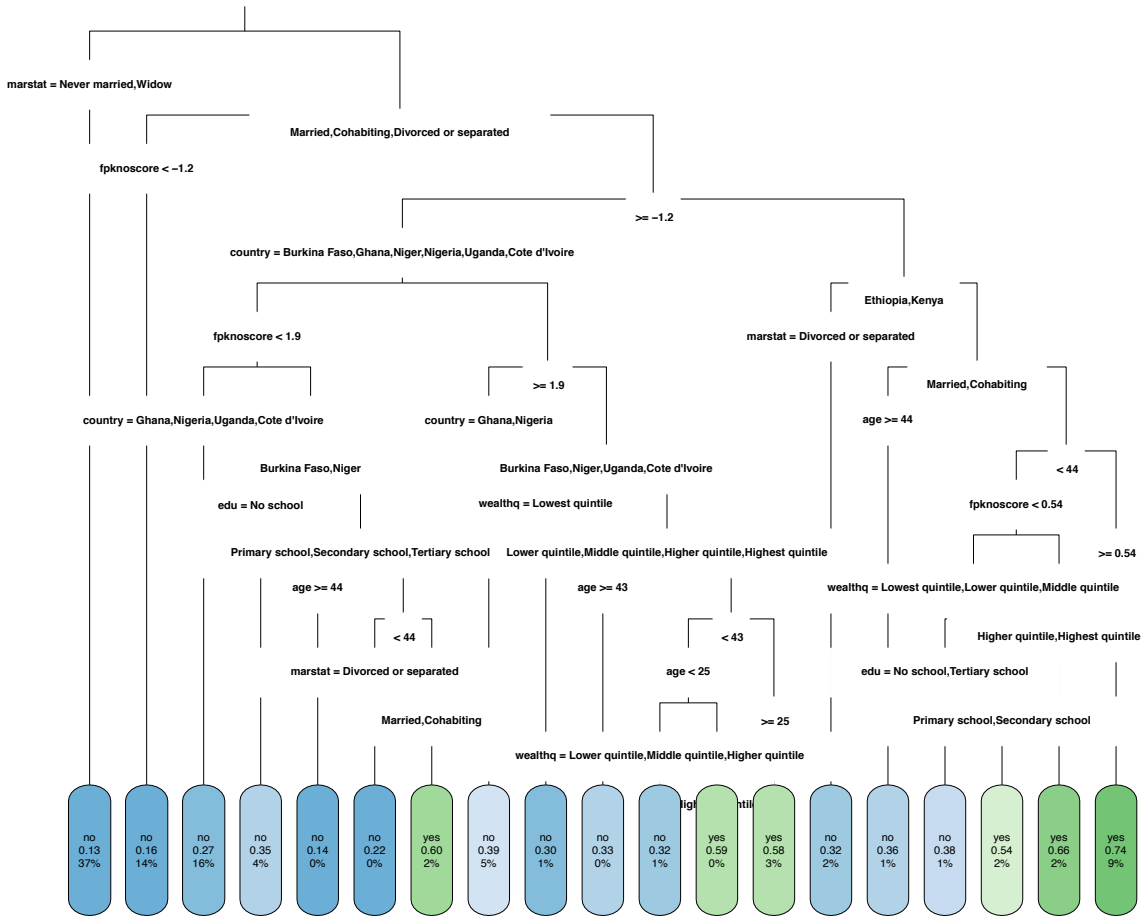


FIGURE A3. Classification tree based on the training sub-sample



This figure shows the simplified version of the pruned decision tree created by recursive partitioning (did not show all nodes). The tree model aims to predict contraceptive use (yes or no in the leaves) by classifying individuals into distinct groups based on a list of observed characteristics. The training sub-sample used in the exercise is a randomly generated half sample of the full dataset. The other half sub-sample is used in estimating treatment effects. Based on the pruned tree, the primary splits for the first node include marital status (shown in this figure), family planning knowledge (<-1.14), and age (< 19.5). These variables are also the three highest ranked variables based on the variable importance measure, which indicates how much a model uses a given variable to make accurate predictions.

TABLE A1. Sample Description by Country

	Number of individual-year observations	Number of individuals	Marriage rate*	Birth rate*	Schooling rate*
Benin	41845	6505	139.7	45.8	397.9
Burkina Faso	32073	5386	237.2	69.5	337.7
Burundi	77726	11767	164.8	61.5	366.3
Cameroon	85263	13815	298.5	100.9	318.1
Congo Democratic Republic	85614	12220	328.8	111.4	331.6
Cote d'Ivoire	18663	2959	168.4	71.6	384.9
Ethiopia	100226	15773	235.2	57.0	460.2
Kenya	213498	32051	284.0	114.2	352.3
Lesotho	92416	13707	260.8	83.9	279.0
Madagascar	35780	6483	291.7	78.7	317.9
Malawi	209247	30430	382.9	129.4	283.6
Mali	29507	4817	345.8	92.6	424.0
Mozambique	52974	8025	327.3	116.4	270.8
Niger	23406	3969	313.1	96.6	315.6
Nigeria	248911	35764	243.7	83.6	475.6
Rwanda	118246	18411	152.2	61.3	242.3
Senegal	165242	24461	189.0	56.6	382.5
Zimbabwe	155673	23879	328.4	103.0	386.4

* Marriage, birth, and schooling rates per 1,000 person-years, calculated as simple means across all person-years.

TABLE A2. Effects of expanding abortion legal grounds, excluding one country at a time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Benin	Burkina Faso	Burundi	Cameroon	Congo DR	Cote_dIvoire	Ethiopia	Kenya	Lesotho
Marriage	-0.026	-0.025	-0.025*	-0.026*	-0.027*	-0.026*	-0.029*	-0.032*	-0.028*
	(0.054)	(0.051)	(0.049)	(0.048)	(0.034)	(0.040)	(0.036)	(0.011)	(0.026)
Birth	-0.008**	-0.007*	-0.008**	-0.008*	-0.008*	-0.008**	-0.008*	-0.008**	-0.007*
	(0.005)	(0.012)	(0.006)	(0.010)	(0.012)	(0.010)	(0.017)	(0.006)	(0.016)
Schooling	0.008	0.007	0.003	0.008	0.001	0.008	0.002	0.007	0.002
	(0.764)	(0.769)	(0.905)	(0.760)	(0.964)	(0.762)	(0.926)	(0.777)	(0.932)
Observations	1744465	1754237	1708584	1701047	1700696	1767647	1686084	1572812	1693894
	Madagascar	Malawi	Mali	Mozambique	Niger	Nigeria	Rwanda	Senegal	Zimbabwe
Marriage	-0.024	-0.022	-0.024	-0.027*	-0.028*	-0.019	-0.019	-0.025	-0.020
	(0.060)	(0.096)	(0.059)	(0.043)	(0.031)	(0.129)	(0.091)	(0.056)	(0.104)
Birth	-0.008**	-0.007*	-0.007*	-0.008**	-0.008**	-0.006*	-0.006*	-0.007*	-0.006*
	(0.004)	(0.015)	(0.011)	(0.009)	(0.007)	(0.047)	(0.025)	(0.019)	(0.026)
Schooling	0.002	0.005	0.005	0.007	0.007	0.011	0.001	0.029*	-0.005
	(0.927)	(0.864)	(0.833)	(0.785)	(0.776)	(0.665)	(0.975)	(0.033)	(0.853)
Observations	1750530	1577063	1756803	1733336	1762904	1537399	1668064	1621068	1630637

p-values in parentheses
The country in the model title was excluded.
Included individual characteristics and country and year fixed effects.
Standard errors clustered at country level.
Included only individuals that were still in school at age 12.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A3. Effects of expanding abortion legal grounds, excluding Kenya and Senegal

	Marriage	Birth	Schooling
Treat	-0.032*	-0.008*	0.030*
	[-0.056,-0.007]	[-0.014,-0.002]	[0.001,0.059]
Small	-0.034*	-0.009*	0.048**
	[-0.067,-0.001]	[-0.017,-0.001]	[0.014,0.081]
Large	-0.029	-0.007	0.010
	[-0.060,0.002]	[-0.016,0.002]	[-0.012,0.031]
P-value (Large = Small)	0.053	0.037	0.026
Age	0.076***	0.024***	-0.088***
	[0.064,0.089]	[0.020,0.028]	[-0.096,-0.080]
Rural	0.036***	0.012***	-0.047***
	[0.025,0.047]	[0.008,0.017]	[-0.069,-0.026]
Poorer	-0.022**	-0.007*	0.028**
	[-0.036,-0.007]	[-0.013,-0.002]	[0.013,0.043]
Middle	-0.045**	-0.015**	0.079***
	[-0.072,-0.017]	[-0.024,-0.006]	[0.043,0.114]
Richer	-0.073***	-0.027***	0.141***
	[-0.102,-0.045]	[-0.038,-0.015]	[0.091,0.192]
Richest	-0.156***	-0.054***	0.292***
	[-0.191,-0.121]	[-0.068,-0.041]	[0.243,0.342]
Observations	1407570	1407570	1407570
Adjusted R^2	0.2734	0.0685	0.3493
mean	0.306	0.100	0.344

95% confidence intervals in brackets
Included country and year fixed effects.
Standard errors clustered at country level.
Small: abortion legal grounds expanded from "to save life & preserve physical health" to including "to preserve mental health".
Large: abortion legal grounds expanded from "to save life" only to including "to preserve mental health".
Excluded Senegal and Kenya – two countries that increased the duration of compulsory education.
Included only individuals that were still in school at age 12.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A4: Effects of expanding abortion legal grounds, restricted to years with abortion policy surveys

	Marriage	Birth	Schooling
Treat	-0.025	-0.012*	0.015
	[-0.056,0.005]	[-0.024,-0.000]	[-0.037,0.067]
Small	-0.040	-0.018*	0.021
	[-0.080,0.000]	[-0.032,-0.005]	[-0.036,0.079]
Large	-0.012	-0.006	0.009
	[-0.057,0.032]	[-0.021,0.009]	[-0.043,0.062]
P-value (Large = Small)	0.137	0.038	0.670
Age	0.075***	0.024***	-0.088***
	[0.064,0.085]	[0.020,0.027]	[-0.094,-0.082]
Rural	0.033**	0.011**	-0.050***
	[0.011,0.054]	[0.004,0.018]	[-0.073,-0.026]
Poorer	-0.031**	-0.007*	0.040***
	[-0.048,-0.013]	[-0.013,-0.001]	[0.023,0.057]
Middle	-0.061***	-0.020**	0.093***
	[-0.086,-0.036]	[-0.031,-0.009]	[0.063,0.123]
Richer	-0.092***	-0.033***	0.151***
	[-0.117,-0.067]	[-0.045,-0.021]	[0.109,0.193]
Richest	-0.171***	-0.062***	0.290***
	[-0.202,-0.141]	[-0.077,-0.047]	[0.248,0.332]
Observations	765460	765460	765460
Adjusted R^2	0.2662	0.0683	0.3359
mean	0.290	0.094	0.346

95% confidence intervals in brackets
Included country and year fixed effects.
Standard errors clustered at country level.
Small: abortion legal grounds expanded from "to save life & preserve physical health" to including "to preserve mental health".
Large: abortion legal grounds expanded from "to save life" only to including "to preserve mental health".
Included only years when abortion policy surveys were available: 1996, 2001, 2003, 2005, 2007, 2009, 2011, 2013, 2015.
Included only individuals that were still in school at age 12.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A5. Effects of expanding abortion legal grounds, including men and women

	All		13 – 15 years of age		16 – 18 years of age		19 – 22years of age	
	Marriage	**Schooling**	**Marriage**	**Schooling**	**Marriage**	**Schooling**	**Marriage**	**Schooling**
Treat	0.000	0.002	0.001	-0.004	0.020	0.009	0.008	0.002
	[-0.022,0.022]	[-0.056,0.059]	[-0.010,0.013]	[-0.092,0.084]	[-0.010,0.050]	[-0.063,0.081]	[-0.019,0.036]	[-0.035,0.040]
Treat x Female	-0.034	0.013	-0.011	0.020	-0.051*	0.017	-0.058*	0.005
	[-0.074,0.007]	[-0.012,0.038]	[-0.024,0.002]	[-0.015,0.056]	[-0.098,-0.003]	[-0.021,0.055]	[-0.116,-0.000]	[-0.012,0.022]
Small	0.012	0.015	0.009	0.006	0.036	0.008	0.014	0.032
	[-0.012,0.036]	[-0.044,0.074]	[-0.008,0.026]	[-0.094,0.106]	[-0.006,0.078]	[-0.069,0.085]	[-0.006,0.035]	[-0.011,0.074]
Small x Female	-0.065*	0.020	-0.019	0.036	-0.084*	0.036	-0.103*	-0.002
	[-0.125,-0.006]	[-0.010,0.050]	[-0.041,0.003]	[-0.004,0.075]	[-0.157,-0.010]	[-0.007,0.079]	[-0.186,-0.021]	[-0.026,0.021]
Large	-0.011	-0.011	-0.005	-0.014	0.006	0.009	0.005	-0.025
	[-0.032,0.011]	[-0.069,0.048]	[-0.018,0.007]	[-0.101,0.073]	[-0.021,0.033]	[-0.065,0.083]	[-0.031,0.040]	[-0.058,0.007]
Large x Female	-0.009	0.010	-0.004	0.009	-0.025	0.004	-0.027	0.014
	[-0.036,0.018]	[-0.017,0.036]	[-0.017,0.010]	[-0.027,0.046]	[-0.064,0.013]	[-0.034,0.041]	[-0.065,0.012]	[-0.001,0.028]
P-value (Large = Small)	0.314	0.169	0.412	0.712	0.231	0.967	0.356	0.125
Female	0.215***	-0.052***	0.047***	-0.044**	0.213***	-0.071***	0.379***	-0.041***
	[0.188,0.241]	[-0.078,-0.027]	[0.036,0.057]	[-0.076,-0.012]	[0.178,0.249]	[-0.107,-0.036]	[0.344,0.414]	[-0.055,-0.028]
Age	0.060***	-0.088***	--	--	--	--	--	--
	[0.050,0.069]	[-0.095,-0.081]						
Rural	0.025**	-0.051***	0.003	-0.061***	0.019*	-0.067***	0.049***	-0.027**
	[0.009,0.040]	[-0.071,-0.030]	[-0.004,0.010]	[-0.083,-0.038]	[0.001,0.037]	[-0.094,-0.039]	[0.023,0.075]	[-0.043,-0.010]
Poorer	-0.022***	0.039***	-0.007*	0.065***	-0.029***	0.044***	-0.032***	0.005*
	[-0.033,-0.011]	[0.022,0.055]	[-0.012,-0.002]	[0.038,0.092]	[-0.044,-0.015]	[0.022,0.065]	[-0.047,-0.016]	[0.000,0.010]
Middle	-0.043***	0.089***	-0.014**	0.134***	-0.051***	0.112***	-0.068***	0.018***
	[-0.062,-0.025]	[0.060,0.118]	[-0.022,-0.006]	[0.093,0.175]	[-0.073,-0.028]	[0.068,0.156]	[-0.096,-0.039]	[0.009,0.027]
Richer	-0.069***	0.145***	-0.021***	0.197***	-0.077***	0.193***	-0.108***	0.047***
	[-0.088,-0.049]	[0.104,0.187]	[-0.029,-0.012]	[0.147,0.246]	[-0.100,-0.053]	[0.124,0.262]	[-0.140,-0.077]	[0.026,0.067]
Richest	-0.133***	0.285***	-0.033***	0.312***	-0.134***	0.374***	-0.219***	0.169***
	[-0.157,-0.110]	[0.245,0.326]	[-0.043,-0.024]	[0.266,0.357]	[-0.163,-0.105]	[0.309,0.439]	[-0.253,-0.185]	[0.125,0.212]
Observations	2659340	2659340	877744	877744	847630	847630	933966	933966
Control mean	0.219	0.367	0.035	0.669	0.178	0.341	0.439	0.093

95% confidence intervals in brackets
Included country and year fixed effects.
Standard errors clustered at country level.
Small: abortion legal grounds expanded from "to save life & preserve physical health" to including "to preserve mental health".
Large: abortion legal grounds expanded from "to save life" only to including "to preserve mental health".
Included only individuals that were still in school at age 12.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.00$

TABLE A6. Balance of the treatment and comparison groups with and without propensity score weights

	Propensity Score Weighted (GBM)			Raw		
	Treatment Mean	Comparison Mean	Std. Eff. Size	Treatment Mean	Comparison Mean	Std. Eff. Size
Age	27.75	27.75	0.00	27.91	28.00	-0.01
Urban	0.47	0.47	-0.01	0.43	0.49	-0.12
Highest level of education						
never attended	0.24	0.25	-0.02	0.19	0.30	-0.29
primary/middle school	0.33	0.34	-0.02	0.38	0.30	0.18
secondary/post-primary	0.32	0.31	0.01	0.32	0.32	0.00
tertiary/post-secondary	0.11	0.09	0.04	0.11	0.08	0.08
Marital status						
never married	0.34	0.34	0.00	0.34	0.33	0.00
currently married	0.51	0.51	0.01	0.51	0.51	-0.02
currently living with partner	0.07	0.07	-0.04	0.06	0.09	-0.09
divorced or separated	0.05	0.06	-0.01	0.07	0.04	0.10
widow or widower	0.03	0.02	0.04	0.03	0.03	0.02
Married once or more than once						
never	0.34	0.34	0.00	0.34	0.33	0.00
once	0.59	0.59	0.00	0.58	0.59	-0.01
more than once	0.07	0.07	0.00	0.08	0.08	0.02
<missing>	0.00	0.00	-0.03	0.00	0.00	-0.07
Partner has other wives	0.15	0.16	-0.02	0.13	0.19	-0.16
<missing>	0.43	0.43	0.01	0.44	0.41	0.07
Wealth score quintile						
lowest quintile	0.21	0.20	0.03	0.19	0.22	-0.06
lower quintile	0.19	0.20	-0.01	0.19	0.21	-0.04
middle quintile	0.18	0.19	-0.02	0.18	0.19	-0.02
higher quintile	0.19	0.19	0.01	0.18	0.19	-0.01
highest quintile	0.23	0.23	0.00	0.26	0.20	0.12
Ever given birth	0.67	0.67	-0.01	0.67	0.68	-0.02
<missing>	0.00	0.00	0.00	0.00	0.00	-0.01
Age at first sex	17.05	17.04	0.00	17.00	16.98	0.01
<missing>	0.23	0.24	-0.01	0.24	0.22	0.05
Age at first birth	19.86	19.89	-0.01	19.90	19.97	-0.02
<missing>	0.37	0.37	0.01	0.36	0.37	-0.02
Prefers no more children	0.25	0.25	0.00	0.28	0.24	0.10
Months to wait before another child (months)	47.62	48.15	-0.01	50.83	45.56	0.14
<missing>	0.41	0.42	-0.02	0.44	0.42	0.04
Country						
Burkina Faso	0.08	0.09	-0.06	0.00	0.19	-2.83
Ethiopia	0.21	0.19	0.05	0.29	0.07	0.50
Ghana	0.02	0.09	-0.41	0.00	0.17	-3.65
Kenya	0.18	0.15	0.07	0.27	0.04	0.52
Niger	0.07	0.06	0.03	0.08	0.04	0.17
Nigeria	0.27	0.26	0.02	0.18	0.34	-0.42
Uganda	0.12	0.10	0.06	0.17	0.04	0.34
Cote d'Ivoire	0.06	0.06	-0.02	0.01	0.13	-1.55

Std. eff. size: standardized effect size; GBM: generalized boosted model

TABLE A7. Free access to different types of contraceptives

		Free access to both LARC and SARC		
		Yes	No	All
		All women		
Free access to SARC only	Yes	15,998 (77.69%)	4,593 (22.31%)	20,591 (100%)
	No	0 (0%)	9,242 (100%)	9,242 (100%)
		Current contraceptive users		
Free access to SARC only	Yes	5,207 (84.04%)	989 (15.96%)	6,196 (100%)
	No	0 (0%)	2,383 (100%)	2,383 (100%)

LARC: long-acting reversible contraceptives, including implants and intrauterine devices.
SARC: short-acting reversible contraceptives.

TABLE A8. Variable importance from recursive partitioning

Variable	Variable Importance
marital status	507.169
knowledge score	352.421
age	265.760
country	251.780
educational level	83.025
free access	29.225
wealth quintile	27.425
information score	15.219
urban	11.727

The variable importance score indicates how impactful each variable is in the final decision tree model generated from recursive partitioning. The higher the score is, the more the model uses a variable to predict contraceptive use. The table only shows the variables with the highest variable importance scores.

TABLE A9. Free access to both long- and short-acting reversible contraceptives by facility type

Facility type	Number of facilities	Number of facilities with free access (%)
Hospital	502	207 (41.2%)
Health center	1030	490 (47.6%)
Health clinic	127	3 (2.4%)
Other health facility	121	53 (43.8%)
Private practice	8	0 (0.0%)
Dispensary	196	153 (78.1%)
Pharmacy/chemist/drug	463	0 (0.0%)
Boutique/shop	5	0 (0.0%)
Other	12	1 (8.3%)

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