

An investigation of the impact of the Global Gag Rule on women's sexual and reproductive health outcomes in Uganda: a difference-in-differences analysis

Margaret Giorgio,^a Fredrick Makumbi,^b Simon P. S. Kibira,^c Suzanne O. Bell,^d Doris W. Chiu,^e Lauren Firestein,^f Elizabeth Sully^{lg}

a Senior Research Scientist, Guttmacher Institute, 125 Maiden Lane 7th Floor, New York, NY 10038, USA. *Correspondence:* mgiorgio@guttmacher.org

b Senior Lecturer and Departmental Chair, Epidemiology and Biostatistics, School of Public Health, College of Health Sciences, Makerere University, Kampala, Uganda

c Lecturer, School of Public Health, College of Health Sciences, Makerere University, New Mulago Hill Road, Kampala, Uganda

d Assistant Professor, Department of Population, Family, and Reproductive Health, Johns Hopkins Bloomberg School of Public Health, Baltimore, MD, USA

e Senior Research Assistant, Guttmacher Institute, New York, NY, USA

f Research Associate, Guttmacher Institute, New York, NY, USA

g Senior Research Scientist, Guttmacher Institute, New York, NY, USA

Abstract: *In 2017, the Trump administration reinstated the Global Gag Rule (GGR), making non-U.S. non-governmental organisations ineligible for US government global health assistance if they provide access to or information about abortion. Little is known about the impact of the Trump administration's GGR on women's outcomes. Data for this analysis come from a panel of women surveyed in 2018 and 2019 in Uganda (n = 2755). We also used data from meetings with key stakeholders to create a detailed measure of exposure to the GGR within Uganda, classifying districts as more or less exposed to the GGR. Multivariable regression models were used to assess changes in contraceptive use, all births, unplanned births, and abortion from before to during implementation of the GGR. Difference-in-differences (DID) estimates were determined by calculating predicted probabilities from interaction terms for exposure/survey round. Descriptive analyses showed long-acting reversible contraceptive use increased more rapidly among women in less exposed districts after GGR implementation. DID estimates for contraceptive use were small. We observed a DID estimate of 3.5 (95% CI -0.9, 7.9) for all births and 2.9 (95% CI -0.2, 6.0) for unplanned births for women in more exposed districts during the period the policy was in effect. Our results suggest that the GGR may have attenuated Uganda's recent progress in improving SRHR outcomes, with women in less exposed districts continuing to benefit from this progress, while previously increasing trends for women in more exposed districts levelled off. Although the GGR was rescinded in January 2021, the impact of these disruptions may be felt for years to come. DOI: 10.1080/26410397.2022.2122938*

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Introduction

The Global Gag Rule (GGR), as it is commonly referred to, is a United States (US) government policy that deems non-US non-governmental organisations (NGOs) ineligible for US government global health funding if they provide, refer, or promote

access to abortion. Different variations of this policy have been in place under every US Republican president since Ronald Reagan. The Trump administration's version (officially titled Protecting Life in Global Health Assistance (PLGHA)) was first instated in January 2017 and remained in

place for four years until the Biden administration rescinded it in January 2021.^{1,2} While prior iterations of the GGR applied only to family planning assistance (~\$600 million USD annually, fiscal year 2019), the Trump administration's version of the GGR applies to all global health funding (~\$7.4 billion USD annually, fiscal year 2019), which covers funding for family planning and reproductive health, maternal and child health, HIV/AIDS, tuberculosis, malaria, infectious diseases, nutrition, and water, sanitation, and hygiene programmes.³ In 2019, the policy's reach was extended even further, prohibiting NGOs that sign the policy from using any source of funds (US or otherwise) to support other non-US-based NGOs that engage in activities prohibited by the policy.^{4,5}

The stated purpose of the PLGHA policy and its earlier iterations is to cease US funding for organisations that support abortion, thereby eroding women's right to abortion access. However, policy makers, service providers, and advocates have argued that the GGR could actually impact a range of sexual and reproductive health and rights (SRHR) outcomes, including interfering with contraceptive service provision, which could potentially result in an increase in induced abortions. The policy infringes on a range of sexual and reproductive rights, impacting women's ability to decide when and whether to have a child.

Few studies have attempted to quantitatively measure the impact of the policy on women's outcomes, and those that have are limited to examining impacts of the George W. Bush administration's iteration of the GGR.^{6–8} Some of this work, which used Demographic and Health Survey (DHS) data, found that women living in sub-Saharan African countries with higher exposure to the GGR had an increased odds of having an induced abortion while the policy was in effect,⁶ as well as a reduction in induced abortions after the policy was subsequently lifted by the Obama administration.⁷ However, it is likely that social, political, and structural factors may lead to variation in the GGR's influence across as well as within countries.⁹ For example, Jones found that women in rural Ghana experienced reduced contraceptive use and increased rates of unintended pregnancy while the GGR was in effect compared to urban women.⁸

Most of the available evidence on the effects of the Trump-era GGR is qualitative in nature. In Kenya, interviews with NGOs and health providers

revealed service delivery programme closures, staff shortages, and increased stock-outs of family planning methods and safe abortion supplies.¹⁰ A similar study in Nepal found that the GGR lessened the effectiveness of the SRHR delivery environment as a whole in the country.¹¹ A study in Madagascar was able to qualitatively interview contraceptive clients who were directly affected by the policy, finding that women experienced increased difficulties obtaining their preferred method after the policy's implementation.¹²

In the case of Uganda specifically, little is known about the policy's impact on women's outcomes. Qualitative reports warn that the policy could complicate partnerships between signing and non-signing organisations.¹³ These reports also suggest the presence of a "chilling effect", where some GGR-compliant organisations avoid activities that they feel unable to adequately distinguish from abortion-related care, such as work around reducing maternal mortality caused by complications of unsafe abortions.¹³ Further, one quantitative study found a decrease in the average number of Community Health Workers engaged to work on family planning in health facilities that were more exposed to the GGR.¹⁴ While this reduction has the potential to reduce contraceptive use in the affected communities,^{15–17} to date no studies have attempted to measure and assess changes in women's SRHR outcomes as a result of the policy.

This study aims to address the need for further quantitative evidence of the impact of the policy that is both country-specific and focused on the Trump administration's iteration of the GGR. The current state of SRHR funding policies in Uganda makes women particularly vulnerable to the impacts of the GGR. The US Government has appropriated approximately US\$ 470 million in global health assistance annually for Uganda, an amount that has remained relatively stable from 2016 (US\$ 471 million) through 2020 (US\$ 476 million).¹⁸ This accounts for just under one-third of all health expenditures in Uganda – across donors, domestic investments and out-of-pocket payments. For family planning specifically, the US Government appropriations for Uganda remained at US\$ 27.5 million from 2015 through 2017, increased to \$29 million USD in 2018, and then dropped to US\$ 27 million in 2019 and 2020. This represents by far the largest donor investment in family planning in Uganda over this period.¹⁹

The goal of this study is to assess the impact of the expanded GGR on women's SRHR in Uganda. Capitalising on the ongoing data collection efforts from the Performance Monitoring for Action (PMA) data platform in Uganda, we use a quasi-experimental study design to examine changes in modern contraceptive use, all births, unplanned births, and induced abortions from pre-GGR to after the policy was in effect by comparing women who lived in areas of Uganda that were "more" and "less" exposed to the policy. We hypothesise that the GGR has led to a decrease in contraceptive use, which will ultimately lead to an increase in unintended pregnancies, unplanned births, and abortions. Documenting the policy's impact on these outcomes will provide additional evidence for policy makers on the global impact of the GGR, as well as on how to best improve SRHR for women in Uganda.

Methods

Data sources and sampling design

In this analysis, we use data from the 2018/2019 rounds of the PMA female surveys in Uganda.²⁰ PMA Uganda is a nationally representative survey of women of reproductive age (15–49); women are selected using a two-stage cluster sampling design, with urban-rural and administrative regions as the strata, resulting in a nationally representative collection of 110 enumeration areas (EAs). In the 2018 round, interviewers mapped and listed all households within an EA or EA cluster, and 44 households were randomly selected. All women aged 15–49 who were usual members of the sampled households or slept in the household the night before the survey were invited to participate in the female survey. This process resulted in 4288 women interviewed in April/May 2018. In the next round (2019), PMA created a panel by re-contacting all women who participated in the 2018 survey and consented to be followed-up, even if women had aged out of the initial age selection criteria.* A total of 1533 (35.7%) women were lost to follow-up; 83% ($n = 1269$) of these women had moved out of the EA in the intervening year and were not eligible for follow-up, 13% ($n = 203$) had not provided consent

*Note: By attempting to follow-up with all 2018 participants, the age range for the 2019 survey extended to women aged 50.

to be reinterviewed in 2018, and study staff were unable to reach the remaining 4% ($n = 61$) of women. As such, this study's analytic sample includes the 2755 women who were located within the EA and re-interviewed in May/June 2019 (response rate = 64%). Larger proportions of women lost to follow-up were younger, unmarried, more educated, urban, and lived in wealthier households (see Supplemental Table S1 for details). We address potential biases associated with this loss to follow-up by creating inverse probability propensity score weights (see below).

In order to monitor national trends in key contraceptive outcomes, PMA also created a nationally representative cross-sectional sample in 2019 by interviewing an additional 1829 women.²¹ While our main impact analysis is conducted among the panel women, we present supplemental, descriptive analyses that utilise data from the nationally representative cross-sectional samples in 2018 ($n = 4288$) and 2019 ($n = 4502$). Respondents provided informed consent at the time of each survey.

To gather data related to GGR exposure, in April 2018 we conducted extensive interviews with key informants at non-US-based NGOs that provide family planning services to women, the Ugandan Ministry of Health, representatives at the US Agency for International Development office in Uganda, and other relevant policy and advocacy organisations. In order to ensure that our exposure variable did not change over time, we also collected additional information from key organisations periodically over the course of the study.

The Institutional Review Boards of the Guttmacher Institute (ref IRB00002197), Johns Hopkins Bloomberg School of Public Health (ref IRB 8436), and Makerere University School of Public Health (ref 565) provided ethical approval, and Uganda National Council for Science and Technology (ref SS 4546) provided final approval. All approvals were received between February and March 2018.

Measures: exposure

There are two possible pathways through which the GGR could impact sexual and reproductive health outcomes among women. In pathway 1, non-US-based NGOs that refuse to sign the GGR lose US government funding, resulting in reductions in the services they provide directly to women and/or the technical support provided

to public health facilities. In pathway 2, non-US-based NGOs that choose to sign the policy are forced to stop providing referrals, services, or advocacy efforts that are newly prohibited. NGOs impacted through pathway 2 could also be forced to terminate partnerships with non-signing organisations.

In this study, our meetings with key stakeholders did not reveal any examples of changes in service delivery as a result of pathway 2. This is largely due to the fact that abortion is highly legally restricted and stigmatised in Uganda;^{22,23} meetings with key stakeholders revealed that NGOs that signed the policy and complied with its requirement to not provide or promote abortion were largely not engaged in those activities prior to the GGR's reinstatement in 2017. In some cases, non-US-based NGOs were forced to reorganise certain services or activities to remain compliant with the policy and to continue to receive US global health assistance. However, these impacts were largely absorbed internally by these organisations, and women continued to receive the same levels of services. Given this context, our exposure variable focuses on the exposure to the GGR through pathway 1.

Two large non-US NGOs did not sign the policy and reported losing US funding. Using information gathered through meetings with key stakeholders, along with detailed monitoring and evaluation data provided by some organisations, we identified SRHR service changes as a result of this funding loss. These changes included programme scale-backs, where resources for providing SRHR services to specific populations (adolescents, women living in rural areas) were redirected within organisations, as well as advocacy and contraceptive service delivery programme closures. A major mobile outreach programme was also impacted; some outreach teams were eliminated, while others experienced reduced capacity as they were forced to cover more areas less frequently to make up for the loss of teams.

The two large non-US-based NGOs provided detailed information on which districts lost programmes and/or services as a result of their refusal to comply with the GGR. If a district experienced at least one change in NGO service provision, we coded it as “more exposed” to the programmatic and service changes that occurred as a result of the reinstatement of the GGR. “Less exposed” districts experienced no reported

changes in service provision as a result of organisations not complying with the GGR. We use the terms “more exposed” and “less exposed” to account for the potential that there are additional changes that occurred as a result of the GGR that were not documented during our meetings with key stakeholders, including those that affect the country as a whole.[†]

At the time the 2018 PMA survey of women was fielded, there were 112 districts in Uganda, and 75 were included in the PMA sample.²⁰ Table 1 displays the distribution of more and less exposed districts across the major regions of Uganda. Overall, exposure to the GGR was evenly distributed across the PMA districts in the Northern and Western regions of the country. While this distribution was less even in the Central and Eastern major regions, no regions were entirely comprised of districts that were more exposed to the policy. (The one exception to this is the Kampala region, which only includes one district.) Exposure was highest in the Karamoja region (80%) and lowest in the Western region (14%). After linking the exposure data to the PMA sample, 52% of panel women ($n = 1423$) resided in more exposed districts, 48% ($n = 1332$) in less exposed districts.[‡]

While the GGR officially went into effect in July 2017, the pre-period for this analysis is the 12 months prior to the 2018 data collection (April 2017–March 2018). Despite this temporal overlap, there are several reasons why data captured during this period can be considered to have occurred prior to exposure to the GGR. First, the implementation of the policy was not instantaneous, which meant that service changes for the two affected organisations occurred gradually over the course of 2017. As such, the potential for exposure misclassification due to our study's definition of the pre-period would likely have only affected outcomes that occurred in the first few

[†]As an example, it is possible that some individual providers could be less likely to provide certain types of SRHR care, including contraceptive services, either due to the “chilling effects” of the policy or as a result of the GGR reinforcing existing opposition to providing these services. While we did not document any evidence of these phenomena, we acknowledge that it is possible these changes occurred, potentially impacting service provision in ways that we were not able to detect or document in this study.

[‡]PMA2020 provided unmasked district codes so that we could link our exposure data to the PMA2020 women's survey data.

Table 1. Distribution of more and less exposed districts across regions in Uganda

	Number of districts included in PMA sample	More exposed districts		Less exposed districts	
		N	%	N	%
Central	16	11	69%	5	31%
<i>Central 1</i>	6	4	67%	2	33%
<i>Central 2</i>	9	6	67%	3	33%
<i>Kampala</i>	1	1	100%	0	0%
Eastern	21	6	29%	15	71%
<i>East Central (Busoga)</i>	10	2	20%	8	80%
<i>Eastern (Bukedi, Bugisu, Teso)</i>	11	4	36%	7	65%
Northern	21	10	48%	11	52%
<i>Karamoja</i>	5	4	80%	1	20%
<i>North (Acholi, Lango)</i>	11	4	36%	7	64%
<i>West Nile</i>	5	2	40%	3	60%
Western	17	7	41%	10	59%
<i>South West (Kigezi, Ankole)</i>	10	6	60%	4	40%
<i>Western (Bunyoro, Tooro)</i>	7	1	14%	6	85%

months of 2018. This concern is further minimised by the fact that impacts on women's SRHR outcomes, which we focus on in this analysis, are the downstream impacts of service changes and therefore would not have occurred immediately at the start of 2018. Data from 2019 are classified as the time-period during which the GGR was in effect. Data collection ended before the policy was rescinded in 2021, so we do not have data for the post-GGR period.

Measures: outcomes

To investigate changes in contraceptive use, we categorised current use into three types: long acting reversible contraceptives (LARCs) (implants and intrauterine devices), short-acting (pill, injectables, male/female condom, emergency contraception, standard days/beads, diaphragm, foam/jelly, lactational amenorrhea), and no use/traditional method use. We combined non-users and traditional method users, as we hypothesise the GGR will impact contraceptive use through connections to the healthcare system, which are not required for traditional method use. Women reporting male/female sterilisation at the time of the 2018 survey were excluded from the contraceptive use variable ($n = 66$) as the permanent

nature of the methods makes them insusceptible to GGR-related changes in use. Pregnant women were also excluded (2018 $n = 332$; 2019 $n = 253$) so as not to conflate changes in pregnancy status with contraceptive use. In 2019, women also completed a retrospective contraceptive calendar, which is a month-by-month history of contraceptive method use in the life of the respondent for the 24-month period preceding the date of interview.²⁴ Using these data, we created person-month reports of contraceptive use using the same three categories and exclusion criteria described above for the 24 months prior to the date of interview.

For pregnancy/birth outcomes, we assessed if there were changes in all (live) births, unplanned births, or induced abortions. An unplanned birth was one where the woman reported wanting to wait to have a(nother) child or not wanting a(nother) child at the time she became pregnant. Induced abortion was defined as doing something to intentionally and successfully end a pregnancy. All three outcomes were limited to "the last 12 months", which corresponded to April 2017–March 2018 for the pre-period, and May 2018–April 2019 for the period during the implementation of the GGR.

Measures: covariates

We measured and controlled for socio-demographic characteristics, including respondent's age, educational attainment, marital status, household wealth quintile, region, and urban/rural residence.

One concern in a quasi-experimental design is the potential for selection bias, where districts served by NGOs impacted by the policy are different from “less” exposed districts. For example, mobile outreach programmes that were impacted by the policy may have been more likely to target districts in greater need of SRHR services, resulting in baseline differences in the outcomes. We address this concern in two ways. First, we control for the modern contraceptive prevalence rate (mCPR) for each district during the pre-period, as communities with lower mCPRs are typically higher priority for family planning programmes. Second, we conduct a parallel trends analysis to ensure the validity of our analytic approach (see below).

Statistical analysis

We calculated inverse probability propensity score weights using key socio-demographic characteristics (age, urban/rural residence, education, marital status, household wealth) in order to balance differences between exposure arms that resulted from loss-to-follow-up. We assessed covariate balance using standardised bias approach; we calculated the difference in weighted proportions between exposure groups divided by the weighted standard deviation of the proportion of the more exposed group.²⁵

In order to provide context for our main outcome models, we conducted a descriptive analysis that investigated national trends in contraceptive use. We graphed the prevalence of both long-acting and short-acting contraceptive use by exposure status over time using the person-month contraceptive calendar data from the 2019 survey. We did not conduct a similar descriptive analysis using pregnancy or birth outcomes from the calendar data due to data quality concerns (see Supplemental Table S2 for details).

The impact of the GGR is estimated using a difference-in-differences (DID) approach. In order to draw causal inference from the DID results, the parallel trends assumption must be met. However, our panel sample only includes

two observations points (one pre- and one during the implementation period), from which a trend cannot be determined. To address this issue, we used data from the 2014–2017 PMA2020 cross-sectional surveys of women in Uganda; we identified whether women in the previous rounds of the surveys were located in districts that were either “more” or “less” exposed to the GGR and calculated trends in our key outcomes prior to our study period.[§]

We calculated doubly robust DID estimates; all multivariable models were weighted using the inverse probability propensity score weights and also included controls for each of the indicators used to create the propensity scores.²⁶ All models are estimated using the following formula:

$$Y_{ij} = E_i + T_j + (E_i * T_j) + I_i + C_{ij} + \varepsilon_{ij}$$

where Y_{ij} represents the SRHR outcome for respondent i in pre/implementation period j , E_i represents exposure to GGR for respondent i , T_j represents the pre/implementation period, $E_i * T_j$ represents the interaction of respondent i 's exposure to the GGR in pre/implementation period j , I_i represents a vector of individual-level controls for respondent i , and C_{ij} represents the vector of the community-level control for respondent i in pre/implementation period j . The DID estimator was determined by calculating predicted probabilities for each exposure group and pre/implementation period using the interaction term. Dichotomous outcomes were fitted using logit models, and the three-category contraceptive outcome was fitted using ordered logistic models. Models were estimated using individual-level random effects and robust standard errors. All analyses were performed using Stata version 16.0 (StataCorp LP, College Station, TX).

Results

Table 2 displays propensity score weighted proportions of pre-period socio-demographic characteristics and indicates that our propensity score

[§]PMA2020 refreshed their sample of enumeration areas (EAs) after the 2016 round of data collection. Thus, not all EAs are the same during the period of observation. However, we expect that averages for more and less exposed areas will be similar given that PMA randomly samples EAs, and the refreshed EAs were contiguous to original ones and in the same parish.

Table 2. Propensity score weighted socio-demographic characteristics and sexual and reproductive health outcomes among women age 15–49 by exposure status, 2018 ($n = 2755$)^{a-c}

	Women in More Exposed Districts ($n = 1423$)		Women in Less Exposed Districts ($n = 1332$)	
Socio-demographic characteristics				
Region, n (%)				
Central 1	91	(6%)	43	(3%)
Central 2	160	(12%)	60	(6%)
East Central	78	(6%)	300	(22%)
Eastern	143	(12%)	197	(14%)
Kampala	283	(13%)	0	(0%)
Karamoja	156	(11%)	31	(2%)
North	135	(12%)	235	(17%)
South West	239	(18%)	144	(12%)
West Nile	93	(8%)	116	(8%)
Western	45	(3%)	206	(15%)
Residence, n (%)				
Rural	1006	(80%)	1174	(83%)
Urban	417	(20%)	158	(17%)
Age, n (%)				
15–19	232	(16%)	200	(15%)
20–24	273	(18%)	239	(18%)
25–29	254	(18%)	238	(18%)
30–34	209	(15%)	220	(15%)
35+	455	(33%)	435	(33%)
Marital status, n (%)				
Currently married/living with man	971	(73%)	1028	(75%)
Divorced/widow	187	(12%)	138	(11%)
Never married	265	(15%)	166	(14%)
Wealth quintile, n (%)				
Lowest	336	(27%)	403	(28%)
Lower	234	(21%)	334	(21%)
Middle	259	(20%)	300	(21%)
Higher	219	(15%)	207	(16%)
Highest	375	(17%)	88	(14%)
Education, n (%)				
Never attended	223	(15%)	182	(15%)
Primary	736	(57%)	833	(58%)
“O” level and above	464	(28%)	317	(27%)
District-level modern contraceptive prevalence rate (mCPR), median (IQR)	0.31	(0.22–0.37)	0.33	(0.21–0.37)

(Continued)

Table 2. Continued

	Women in More Exposed Districts (<i>n</i> = 1423)		Women in Less Exposed Districts (<i>n</i> = 1332)	
Contraceptive outcomes				
Type of contraceptive method currently used, <i>n</i> (%)				
Long-acting reversible contraceptives ^d	107	(8%)	128	(10%)
Short-acting modern ^e	285	(20%)	228	(18%)
Traditional or no method	994	(72%)	936	(72%)
Pregnancy, birth and abortion outcomes				
Gave birth in the last 12 months, <i>n</i> (%)	251	(19%)	312	(23%)
Unplanned birth (all women), <i>n</i> (%)	90	(7%)	162	(11%)
Abortion in the last 12 months, <i>n</i> (%)	19	(1%)	11	(1%)

^aWeighted proportions and unweighted N's displayed; n's may not sum to total N due to missing values.
^bWeighted using inverse probability propensity score weights.
^cBalance between exposure groups for age, urban/rural status, education, marital status, and household wealth confirmed with standardised bias test (<0.25).
^dIUD or implant.
^eInjectables, pill, emergency contraception, male or female condom, diaphragm, foam/jelly, LAM, or standard days/cycle beads.

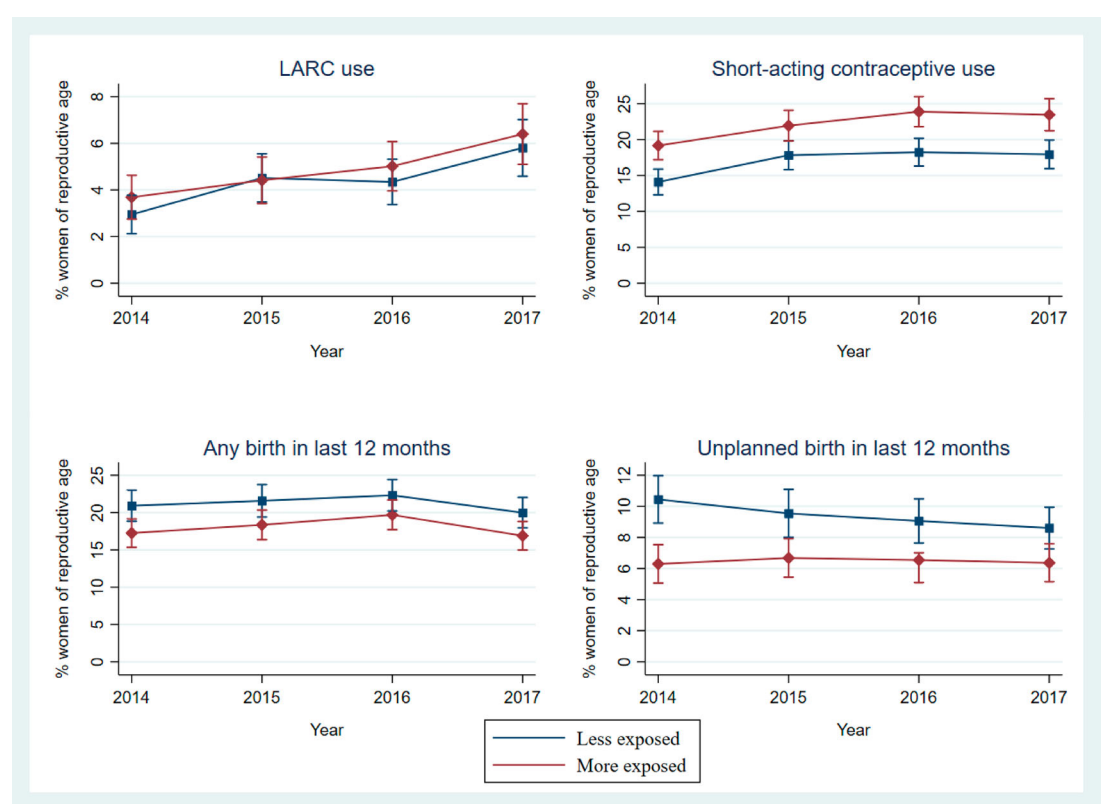
weights successfully achieved balance by exposure status. Differences by contraceptive method type were small in magnitude (LARC: 10% vs. 8%, short-acting: 18% vs 20%). Larger proportions of women in less exposed districts reported any birth (23% vs. 19%) or an unplanned birth (11% vs. 7%) compared to women in more exposed districts. There were no differences by exposure status in self-reported abortion.

Figure 1 displays trends in our key study outcomes by exposure status using the PMA nationally representative cross-sectional surveys of women from 2014–2017. This descriptive analysis suggests that the parallel trends assumption is met for our analysis; the differences between exposure groups for LARC use, short acting contraceptive use, all live births, and unplanned births remained constant in the years prior to the reinstatement of the GGR.

To investigate the impact of the GGR on contraceptive use, we first calculated the proportion of women using LARCs and shorter-acting methods over time by exposure status using the nationally representative contraceptive calendar data from the 2019 PMA survey (Figure 2). Trends in LARC use for women in more and less exposed districts cross over after the policy's implementation, with

the proportion of women using LARCs increasing more rapidly in the less exposed group. Differences in trends in short-acting methods are less apparent. Next, we calculate DID estimates for the impact of the GGR on contraceptive use (Figure 3, Supplemental Table S3). The model estimates a non-statistically significant decrease of 0.5 percentage points for both LARC use (95% CI –2.5 to 1.6) and shorter-acting method use (95% CI –2.2 to 1.2, $P = 0.650$), and a non-statistically significant one percentage point increase for no use/traditional method use (95% CI –2.8 to 4.7, $P = 0.580$) among women in more exposed districts compared to their less exposed counterparts in the implementation period.

Focusing on birth outcomes, the adjusted proportion of women who gave birth in less exposed districts decreased from pre-GGR to after the implementation of the policy (22.4–19.2%) (Supplemental Table S3). This proportion increased by 0.3 percentage points in the more exposed group (19.1–19.4%), corresponding to a DID estimate of 3.5 (95% CI –0.9 to 7.9, $P = 0.121$) (Figure 4). This pattern held for unplanned births; while estimated unplanned births decreased among women in less exposed districts (10.4–7.6%), this proportion increased slightly for

Figure 1. National trends in key study outcomes by exposure status prior to the implementation of the GGR*

*Data come from the 2014–2017 PMA2020 surveys of women in Uganda. Proportions weighted using PMA2020 generated weights for national representativeness.

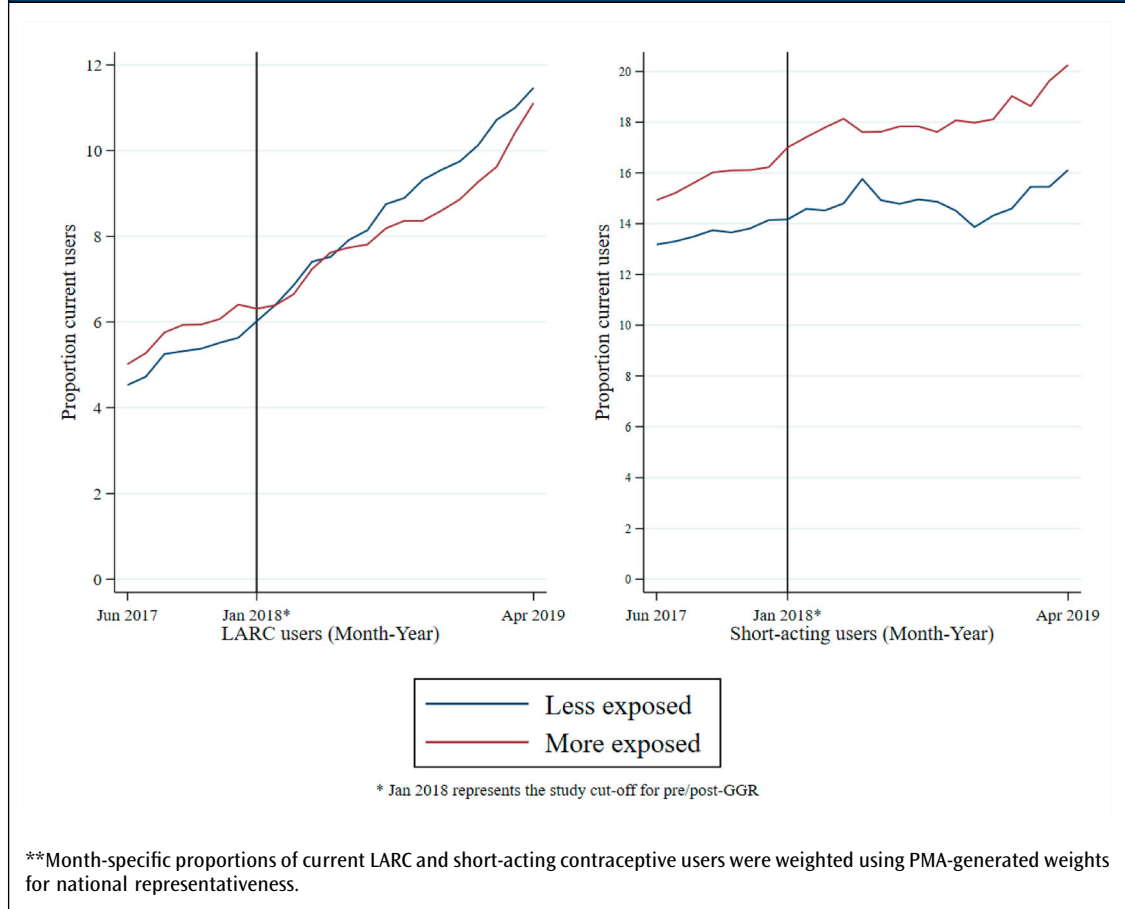
women in more exposed districts (7.9–8.1%), reflecting a DID estimate of 2.9 percentage points for women in more exposed districts in the implementation period (95% CI -0.2 to 6.0 , $P = 0.064$). Our DID estimate for the impact on self-reported abortion in the last 12 months showed little change (-0.7 , 95% CI -1.6 to 0.3 , $P = 0.155$).

Discussion

The results from this study suggest that the GGR may have begun to attenuate the recent progress Uganda has achieved in improving key SRHR outcomes. While the magnitude of the study effect sizes makes it difficult to detect statistical significance with conventional interpretations of p -values, we do observe marginally significant

results for changes in all births and unplanned births. Further, the consistency of our results, both within our study analyses and compared to previous research, leads us to conclude that the policy negatively impacted women's health in Uganda. We hypothesised that the GGR would lead to a decrease in contraceptive use, which would ultimately lead to an increase in unintended pregnancies, unplanned births, and abortions. Our analyses revealed a slightly more nuanced impact; instead, the policy is likely disrupting gains in SRHR that Uganda has achieved in recent years, resulting in a stagnation of this progress. Historically, there has been a need for increased contraceptive services in Uganda; modern contraceptive use has been low, unmet need and total fertility high, and unintended

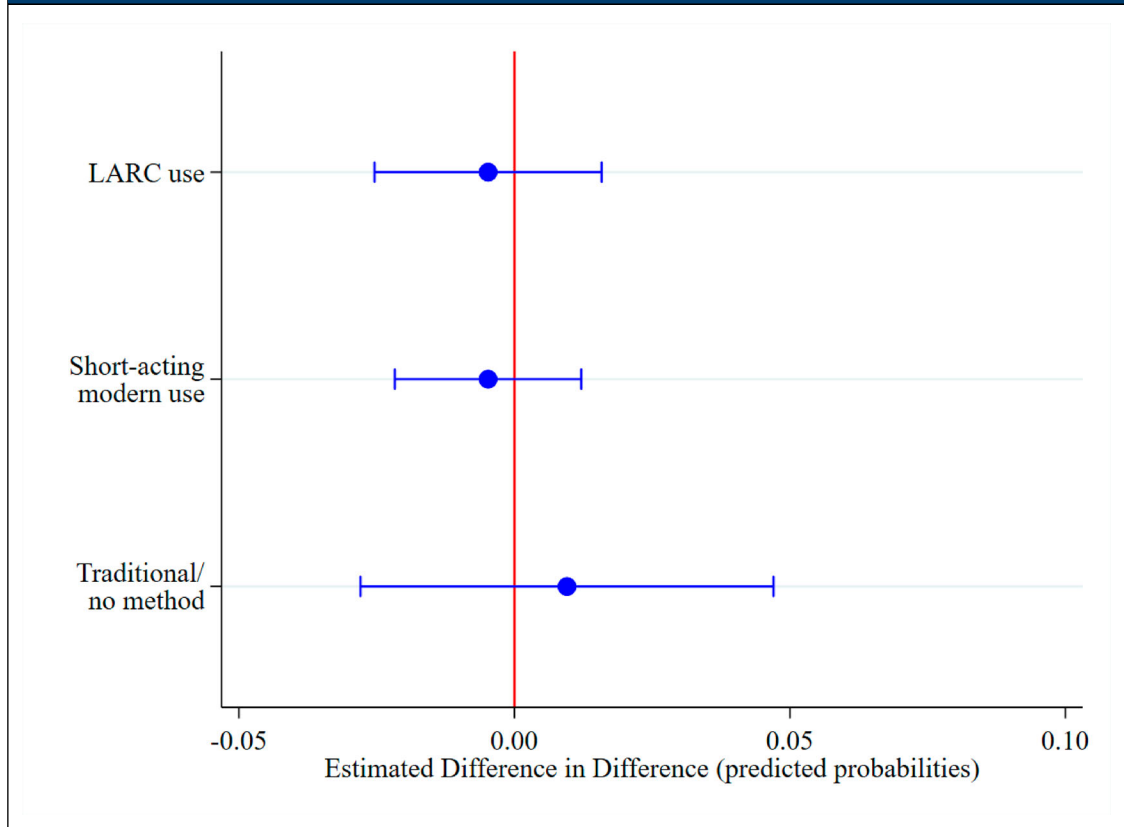
Figure 2. Trends in contraceptive use among Ugandan women using the retrospective contraceptive calendar data from the 2019 PMA2020 survey**



pregnancies common.^{27,28} More recently, Uganda has seen steady increases in modern contraceptive use, as well as decreases in the total fertility rate.^{29,30} The results of this study suggest that women in less exposed districts have continued to benefit from this progress, while positive trends for women in more exposed districts have slowed or levelled off as a result of the GGR.

We hypothesised that LARC use might be most vulnerable to impacts of the GGR, as increasing access to LARCs was a major focus of the two NGOs in Uganda most affected by the policy. LARC use has been steadily increasing in Uganda for several years (4.7% in 2014 to 8.0% in 2018).^{31,32} Our DID results show that LARC use increased by a smaller magnitude among

women who resided in districts that were more exposed to the GGR, resulting in a small, negative DID point estimate, and post-hoc power calculations revealed that our study was not sufficiently powered to detect changes of this size. Our nationally representative descriptive analysis using calendar data supports the notion that the GGR is creating a stagnation in LARC uptake, showing a larger increase in LARC use among women in less exposed districts as compared to their more exposed counterparts, with a cross-over in the trends after the implementation of the GGR. These findings are also consistent with previous work investigating the impact of the Bush-era GGR; Bendavid et al. found that modern contraceptive use increased steadily over the eight

Figure 3. Estimated impact of the GGR on contraceptive outcomes for women in more exposed districts compared to women in less exposed districts in Uganda, 2019

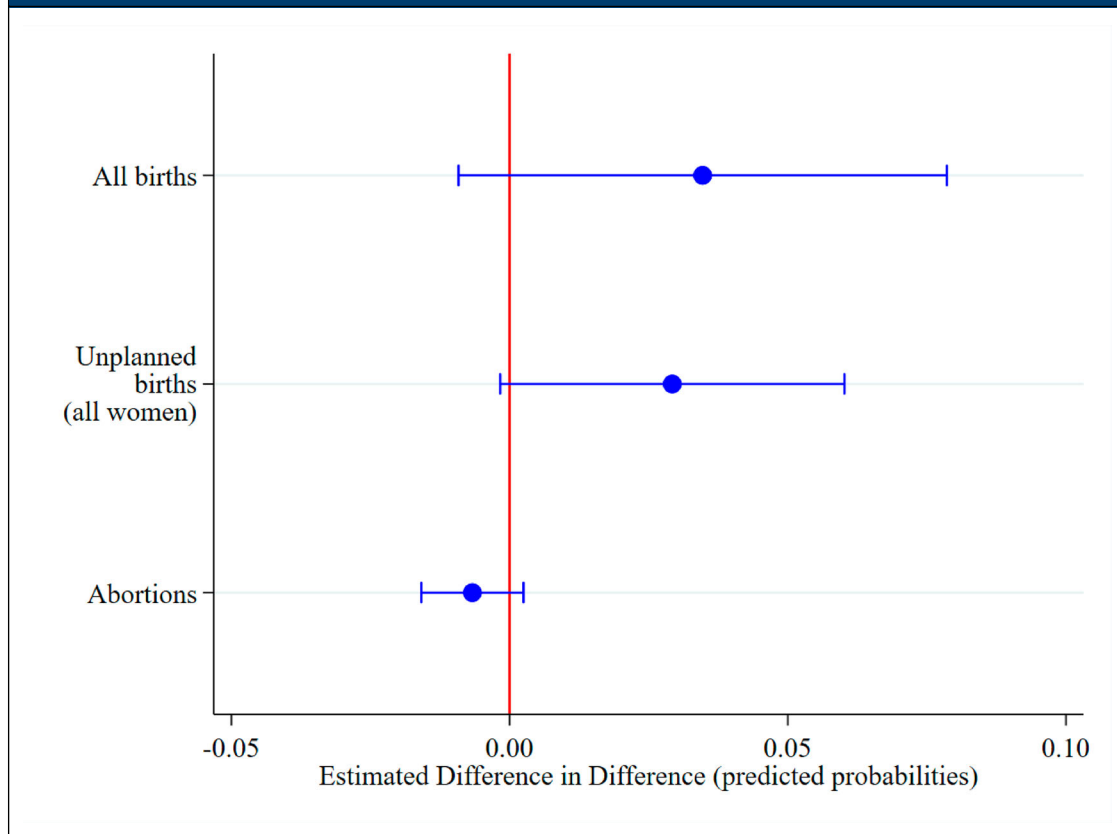
years that the policy was in effect in low exposure countries while it levelled off in high exposure countries.⁶

We observed a marginally significant impact of the GGR on births overall, and it appears that this change is primarily driven by changes in unplanned births. Women in less exposed districts experienced a decrease in unplanned births over time, which is consistent with recent trends in unintended pregnancy and unplanned birth rates in Uganda.^{29,30,33,34} Conversely, we observed little change in unplanned births for women in more exposed districts over the study period. While changes in contraceptive use are likely influencing these divergent trends by exposure status, they cannot completely account for the magnitude of our findings. Another possible contributor may be related to the GGR's impact on Community Health Workers (CHWs); previous research in

Uganda found that the policy resulted in a reduction in the average number of CHWs engaged by facilities on family planning.¹⁴ A main focus of these CHWs is to provide contraceptive education and counselling.¹⁵ It is possible that this reduction in CHWs may have negatively impacted levels of correct and consistent use of contraceptives among women in more exposed districts, further disrupting the downward trend in unplanned births.

The observed change in unplanned births could also be driven by changes in induced abortion. However, a major limitation of our study is our inability to draw conclusions about the GGR's impact on induced abortion. It is well documented that induced abortions are dramatically underreported in surveys,³⁵ which we also observed in the PMA surveys; the direct-report one-year incidence rate during the study pre-

Figure 4. Estimated impact of the GGR on birth and induced abortion outcomes for women in more exposed districts compared to women in less exposed districts in Uganda, 2019



period (2018) was 8.6 per 1000, which is markedly lower than the 2013 incidence estimate for Uganda (39 per 1000).^{28,36} The biased nature of women who self-report induced abortions in surveys, coupled with the very small number of women who did report an abortion in PMA surveys, make it difficult to interpret the results of our DID model estimating the impact of the GGR on self-reported abortion. In anticipation of this limitation, we included two indirect methods for measuring induced abortion in the PMA surveys (the Confidante Method and the Network Scale-Up Method (NSUM)) in the hopes of using these indirect estimates to understand how the GGR may have impacted abortion in Uganda.^{31,32} However, we discovered several limitations to the use of the NSUM and Confidante Method in this analysis, including potentially inappropriate

assumptions about geographic locations of social networks, the reliability of these methods when used in repeated surveys, and the validity of the incidence estimates themselves.^{36,37} As a result, we are unable to draw conclusions about the impact of the GGR on induced abortions in Uganda using the study data.

In light of these limitations, we must consider several possible relationships between the GGR and induced abortion. One explanation is that abortions increased as a result of the GGR. In this scenario, decreases in contraceptive use would lead to increases in unintended pregnancies, which would also lead to an increase in induced abortions. Previous research supports this causal pathway, showing that prior iterations of the GGR resulted in an increase in induced abortions in sub-Saharan Africa.⁶ However,

research in Ghana after the Bush-era GGR was implemented suggests a more nuanced impact of the policy on fertility outcomes, documenting shifts between unplanned births and abortion;⁸ after access to contraceptive services declined in rural Ghana, non-poor women experienced an increase in induced abortion while unintended pregnancies were more likely to result in a birth among poor women. In the context of this study, the observed excess unplanned births among women in more exposed districts may be caused by either mechanism described above. However, the limitations in our study data make it difficult to determine the true relationship between the GGR and abortion incidence in Uganda. Future research is needed to better understand how women's need for and access to abortion services is impacted by the GGR, especially in settings such as Uganda where abortion remains highly restrictive.

Additional limitations

There was differential loss-to-follow-up in the creation of our study panel. In response, we calculated propensity score weights that accounted for the resulting imbalance between exposure groups during the pre-period. By using these weights as well as controlling for key socio-demographic factors in our models, we produced doubly robust DID estimates for the impact of the GGR on our study outcomes. Despite these efforts, our DID estimates may not be generalisable to all women in Uganda, as our panel is not nationally representative. Given that women lost to follow-up were more economically and educationally advantaged than the panel women, and therefore might be less susceptible to negative impacts of the GGR, it is possible that our estimates are biased away from the null.

Our panel sample only includes two observation points (one pre- and one after the policy's implementation), which is not ideal for a DID analysis. Having multiple observation points in the pre- and/or implementation period would have provided a more robust estimation of the outcome trajectories for our exposure groups, as well as a better determination of whether the parallel trends assumption holds for our analysis.³⁸ However, our descriptive analysis using the prior rounds of PMA data lead us to conclude that the parallel assumptions trend likely holds for our study.

Another potential limitation of our study is the statistically significant difference in unplanned births at baseline, with a larger proportion of women from less exposed districts reporting an unplanned birth in the pre-period. However, this does not necessarily pose a threat to the validity of our results; researchers have argued that it is important to describe the underlying cause of any observed baseline differences and confirm that this mechanism would not additionally impact trends during the study period.³⁹ In the case of unplanned births in Uganda, the observed difference is likely due to district-specific variations in women's socio-economic status; women in less exposed districts were more likely to live in rural areas, be married, live in lower wealth households, and have lower levels of education, all of which have been shown to be positively associated with unintended pregnancy in sub-Saharan Africa.⁴⁰ If anything, these economic differences might suggest that women in less exposed districts would be less likely to experience a decrease in unplanned births over time given these economic disadvantages. Yet, we observed the opposite trend, with women in more exposed districts experiencing no change in unplanned births and women in less exposed districts experiencing a decline. However, having more years of pre-exposure data to confirm that the parallel assumption was not violated would have strengthened our results.

There are some limitations related to our measurement of exposure to the GGR. A more robust measure of exposure would have included changes in US government family planning funding flows across geographic regions in Uganda, for which detailed data were not available at the time of this study. While our exposure variable measures proxies for this change, GGR-related funding changes may impact service delivery in additional ways that were not captured in stakeholder meetings. In addition, our assignment of exposure status assumes that women primarily access family planning and other SRHR services from facilities and programmes located within the same districts that they live. While this assumption is almost certainly met, we did not have data to confirm that this is the case. As such, there is a possibility that some women's exposure status, especially those who live near the border of their district, may have been misclassified. Further, the GGR may be affecting aspects of SRHR service delivery across the whole

country, potentially leading to our underestimating the true impact of the GGR.

Finally, several donor governments provided stop-gap funding in many affected countries shortly after the policy went into effect, which may have mitigated potential impacts of the GGR. In the case of Uganda, interviews with key stakeholders suggested that stop-gap funding was secured for several programmes, albeit piecemeal and slowly over time. Given that data collection finished in mid-2019, the influence of stop-gap funding on our results is likely small.

Conclusions

This study adds to the evidence suggesting that the GGR negatively impacts SRHR. Our findings also highlight the difficulty in measuring the true impact of the policy on outcomes that are notoriously difficult to measure, namely unintended pregnancies and abortion. Future research on the development and refinement of methods for more accurately measuring stigmatised and/or hidden outcomes is needed. Without robust data, it will be difficult to fully understand the mechanisms through which the GGR affects women.

Despite these limitations, the GGR is likely adversely impacting women in Uganda. Over the past several decades, the global health community, national governments, and local civil society groups have come together to create improvements in the lives of women in low- and middle-income countries. While the stated intention of the PLGHA policy was to cease US funding for organisations that support abortion, its actual consequences may be a disruption in years of progress in increasing access to contraceptive methods and decreasing unplanned fertility outcomes. Although this most recent version of the GGR was rescinded by the Biden administration after our study ended, it is possible that the impact of these disruptions could be felt for years to come. Future research should continue to evaluate both the shorter and longer-term impacts of this policy on SRHR service delivery and women's outcomes.

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Data availability statement

The data collected by the authors and by PMA2020 and used in this study are publicly available. The 2018 and 2019 PMA female surveys can be requested here: <https://www.pmadata.org/data/available-datasets>. The exposure classification data collected by the authors and used in this study can be accessed under DO:10.6084/m9.figshare.

Supplementary materials

Supplemental data for this article can be accessed online at <https://do.org/10.1080/26410397.2022.2122938>.

ORCID

Elizabeth Sully  <http://orcid.org/0000-0002-0736-2237>

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Résumé

En 2017, l'administration Trump a rétabli la « Global Gag Rule » (ou règle du bâillon mondial), qui interdit aux organisations non gouvernementales non américaines de bénéficier de l'aide sanitaire internationale du Gouvernement des États-Unis si elles fournissent un accès à l'avortement ou des informations sur cette intervention. On sait peu de choses sur l'impact de cette règle de l'administration Trump sur les femmes. Les données utilisées pour cette analyse proviennent d'un groupe de femmes ayant fait l'objet d'une enquête en 2018 et 2019 en Ouganda ($n = 2755$). Nous avons aussi utilisé des informations

Resumen

En 2017, el gobierno de Trump restableció la Ley Mordaza (LM), que dictó que las organizaciones no gubernamentales fuera de Estados Unidos son inelegibles para recibir asistencia de Estados Unidos para la salud mundial si proporcionan acceso a servicios de aborto o información sobre el aborto. No se sabe mucho sobre el impacto de la LM durante el gobierno de Trump en los resultados para las mujeres. Los datos para este análisis provienen de un panel de mujeres encuestadas en 2018 y 2019 en Uganda ($n = 2755$). Además, utilizamos datos de reuniones con partes interesadas clave para crear una medición

recueillies lors de réunions avec des acteurs clés pour créer une mesure détaillée de l'exposition à cette règle en Ouganda et nous avons classé les districts comme plus ou moins exposés à la règle du bâillon mondial. Des modèles de régression multivariée ont été adoptés pour évaluer les changements dans l'emploi de contraceptifs, toutes les naissances, les naissances non planifiées et les avortements entre la période précédant la mise en œuvre de la règle et la période d'application de celle-ci. Les estimations des doubles différences ont été déterminées en calculant les probabilités prédites d'après les conditions d'interaction pour le cycle exposition/enquête. Les analyses descriptives ont montré que l'emploi d'une méthode contraceptive réversible de longue durée augmentait plus rapidement chez les femmes dans les districts moins exposés après la mise en œuvre de la règle. Les estimations des doubles différences pour l'utilisation de contraceptifs étaient de faible amplitude. Nous avons observé une estimation des doubles différences de 3,5 (IC 95% –0,9-7,9) pour toutes les naissances et de 2,9 (IC 95% –0,2,6,0) pour les naissances non planifiées chez les femmes dans les districts plus exposés pendant la période d'application de la politique. D'après nos résultats, il semble que la règle du bâillon mondial ait sapé les progrès accomplis récemment par l'Ouganda pour améliorer la santé et les droits sexuels et reproductifs; les femmes qui vivent dans les districts les moins exposés ont continué à bénéficier de ces progrès, alors que les tendances précédemment à la hausse pour les femmes dans les districts plus exposés se sont stabilisées. Même si la règle du bâillon mondial a été abrogée en janvier 2021, il est possible que l'impact de ces perturbations se fasse sentir pendant les années à venir.

développée de la exposition à la LM en Ouganda et en districts classifiés comme plus ou moins exposés à la LM. Se utilizaron modelos de regresión multivariada para evaluar los cambios en el uso de anticonceptivos, todos los nacimientos, nacimientos no planeados y aborto desde antes de la aplicación de la LM hasta durante la misma. Se determinaron las estimaciones de diferencia en diferencias (DD) calculando probabilidades previstas de términos de interacción para la ronda de exposición/encuesta. Análisis descriptivos mostraron que el uso de anticonceptivos reversibles de acción prolongada aumentó con mayor rapidez entre las mujeres en distritos menos expuestos después de aplicada la LM. Las estimaciones de DD para el uso de anticonceptivos fueron de pequeña magnitud. Observamos una estimación de DD de 3.5 (IC de 95% –0.9,7.9) para todos los nacimientos y de 2.9 (IC de 95% –0.2,6.0) para nacimientos no planeados entre mujeres en distritos más expuestos durante el período en que la política estaba en vigor. Nuestros resultados indican que la LM posiblemente atenuó el reciente progreso logrado en Ouganda para mejorar los resultados de SDR, durante el cual las mujeres en distritos menos expuestos continuaban beneficiándose de este progreso, mientras que las tendencias anteriormente en alza entre las mujeres en distritos más expuestos se nivelaron. Aunque la LM se revocó en enero de 2021, es posible que se sienta el impacto de estas interrupciones durante muchos años.