The Global Health Toll of the Global Gag Rule*

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Sept. 30th, 2025

Preliminary Draft, Please Do Not Circulate.

Abstract

The Global Gag Rule (GGR) is a pro-life policy that prohibits foreign non-governmental organizations receiving U.S. aid from providing or advocating for abortion-related services. First introduced by President Reagan in 1984, the policy has been rescinded by every Democratic administration and reinstated by every Republican. We examine the effects of the GGR on maternal mortality across two presidential transitions: Clinton to Bush (1993–2008) and Obama to Trump (2009–2020). Our empirical strategy employs a triple difference design that leverages cross-country variation in U.S. aid dependence and within-country variation in baseline clinic access to measure exposure. We find that, on average, the reinstatement of the GGR is associated with a 16.7 percent increase in maternal mortality in highly-exposed aid-receiving countries. We also uncover larger effects under the Trump administration, likely due to the policy's unprecedented expansion in 2017.

JEL codes: H51, I15, F35, D72
Keywords: US Presidential party, overseas aid, abortion, maternal mortality, Global Gag Rule, Mexico City Policy

*Sonia Bhalotra acknowledges financial support from European Research Council (ERC) Advanced Grant Evidence-VAW 885698. Damian Clarke acknowledges the support of ANID Chile via the Millennium Institute for Market Imperfections and Public Policy. We thank seminar participants at Warwick, IEPS, AEHESG (Oxford), BSE Summer Forum, WOLFE (York), and Royal Holloway for their comments and insights. We thank Sam Rawlings for kindly sharing data. Aleesha Joseph provided excellent research assistance. All remaining errors are our own.

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1 Introduction

The United States provided 38% of all Overseas Development Aid (ODA) for health, with bilateral payments of over 8.4 billion USD in 2018, of which around 6% is earmarked for family planning (IHME, 2018). In this paper, we leverage the systematic variation in US family planning aid, historically aligned with shifts in the US Presidential Party, to investigate the impacts of aid cuts on reproductive health outcomes such as maternal mortality in recipient countries.

In particular, we estimate the impacts of the switching on and off of the Mexico City Policy, also known as the Global Gag Rule (henceforth, GGR). The GGR is a US foreign policy that prohibits aid to overseas nongovernmental organizations (NGOs) offering abortion-related information or services. It was first implemented by President Reagan in 1984. Being a Presidential Policy, it has since been enacted under every Republican and revoked under every Democrat, typically on the first day of their Presidential term. Notably, under the first Trump administration (2017-2020), the policy was not only reinstated but also dramatically expanded, extending its restrictions to almost all U.S. bilateral global health assistance rather than assistance for family planning and reproductive health only.[1] The expansion suggests that even among Republican presidencies, the effects of the GGR can be heterogeneous, with Trump's implementation imposing a more extensive disruption on health service resources than previous iterations.

Existing empirical evidence indicates that GGR-related cuts in US aid have had a significant impact on health service resources in developing countries (Iversen, 2017). Several case studies and in-depth interviews with providers establish that the GGR has led to staff retrenchment, clinic downsizing or clinic closure, as well as contraceptive shortages (USAID, 2001; Maistrellis et al., 2022). Since delivery of reproductive health services is often bundled with other health services, the GGR has resulted not only in reduced access to safe abortion and contraception but also in reduced capacity for broader maternal health services, including HIV prevention and treatment (Mavodza et al., 2019).

The implementation of GGR under Republican Presidents has been presented as part of their pro-life agenda, aimed at limiting access to abortion. Previous research in the public health and

[1] In January 2017, Trump reinstated and renamed the GGR policy as "Protecting Life in Global Health Assistance" (PLGHA) to reflect its expanded scope. The expanded policy applied to almost $11 billion in U.S. global health assistance, including programs for HIV/AIDS (PEPFAR), maternal and child health, malaria, and global health security, in addition to family planning and reproductive health (Schaaf et al., 2019; Brooks et al., 2023).

economic literature demonstrates that the GGR has failed in its stated purpose because the closure of clinics providing abortion services has resulted in reduced access to contraception, an increase in (often unwanted) pregnancies, and thus an increase in the demand for abortion (van der Meulen Rodgers, 2018; Brooks et al., 2019; Jones, 2015; Miller and Valente, 2016).[2] Using high-frequency data from service delivery providers for eight sub-Saharan African countries, Brooks et al. (2023) also finds that health facilities provide fewer family planning services, reducing contraceptive use and increasing pregnancies under the more recent Trump expansion of the policy.

In this paper, we explore the empirical relationship between GGR reinstatement in the U.S. during 1993-2020 and maternal mortality in aid-receiving countries. To this end, we leverage fine-grained, within-country variation through a grid creation strategy combined with a triple-differences (DDD) research design. Because the GGR disrupts contraceptive access and maternal healthcare primarily by restricting U.S.-funded clinics, we posit that areas with higher baseline clinic access are more sensitive to funding cuts. To capture this exposure, we construct 0.5-by-0.5 degree grid cells for a sample of countries for which we have micro-level data from reproductive-aged women measuring their clinic access as a proxy for exposure to the GGR. Our DDD approach then exploits variation across time (before and after GGR implementation), between regions with different baseline levels of clinic access, and across countries with high versus low baseline reliance on U.S. aid. In brief, we show that the GGR leads to higher maternal mortality in aid-receiving countries. In particular, our estimates from the pooled sample (averaging across the Bush and Trump iterations) suggest there is a 16.7% increase in maternal deaths, which can be attributed to GGR when comparing high to low-exposure areas. Moreover, we also uncover larger effects under the Trump administration, as well as some evidence of effect reversal when GGR is switched off during the Bush-Obama transition. On mechanisms, we report suggestive evidence that GGR causes both disruptions in contraceptive access and a subsequent increase in unsafe abortions, as well as reductions in maternal care services during and after pregnancies.

We make three main contributions to the literature. First, we introduce a novel approach

[2] While based on a quite different policy intervention, broadly similar results have been documented considering the recent rollback of abortion access in the United States, with observed increases in fertility and in infant mortality rates (Bell et al., 2025; Gemmill et al., 2025), among other measures.

to identify the global health impacts of the GGR on health outcomes, beyond the conventional country-level analyses (Brooks et al., 2019; Kavakli and Rotondi, 2022).[3] Our contribution advances the methodological frontier by leveraging fine-grained, within-country variation of GGR exposure embedded in a triple-differences (DDD) research design. The approach not only refines the measurement of exposure but also enhances our ability to isolate the causal effects of the GGR on health service disruptions and health outcomes from other contemporary confounders, such as any country-level shock specific to U.S. partisan shifts (Spenkuch et al., 2023). Our first-stage results show that clinic access deteriorated by 0.22 standard deviations in high relative to low-exposure areas after the policy's reinstatement, consistent with the hypothesis that areas with initially high clinic access were more likely to experience clinic closures and service disruptions.

Second, our study contributes to the broader maternal mortality literature by investigating the GGR's impact on maternal well-being. A number of extant studies have highlighted the political determinants of maternal mortality and morbidity (Bhalotra et al., 2022; Clarke and Mühlrad, 2021; Farin et al., 2024). In particular, a recent study by Londoño-Vélez and Saravia (2025) exploits judge leniency in Colombia and finds that being denied a wanted abortion substantially increases the risk of maternal death within the following nine months, while also imposing long-term costs on educational and labor market outcomes of both mothers and children. We hypothesize that the GGR's reduction in service capacity increased demand for abortions at a time when already sparse services are being pared back, resulting in largely unsafe abortions. We find evidence consistent with this hypothesis. Unsafe abortion is hard to accurately track as it is often clandestine, but it is estimated to account for between 4.7% and 13.2% of maternal deaths (Mavodza et al., 2019; Ceschia and Horton, 2016). By linking the policy-induced disruptions in health services to maternal well-being, our research provides new evidence on the broader public health consequences of the GGR. Even if politics and religion divide views on abortion and contraception (Elías et al., 2017; Bhalotra et al., 2021a), most policymakers would agree that increases in maternal mortality represent a failure of the policy.

Lastly, we also contribute to a large literature on the effects and effectiveness of foreign aid (Easterly, 2003; Nunn and Qian, 2014; Qian, 2015; Child et al., 2024), including recent papers

[3] See Mavodza et al. (2019) and Kates and Moss (2024) for a scoping literature review of the impacts of GGR on global health.

that focus on aid in the health sector (Shastry and Tortorice, 2025) or Chinese aid in sub-Saharan Africa (Dreher et al., 2021; Mueller, 2025). By studying the Global Gag Rule, a recurring U.S. policy that restricts funding to foreign NGOs providing or promoting abortion-related services, we explore how donor-imposed conditionalities can cause aid disruptions and undermine aid effectiveness. Our results echo the findings of recent studies (Bendavid et al., 2011; Jones, 2015) which suggest that the GGR may paradoxically increase abortion rates and reduce access to family planning. We contribute to the literature by providing causal evidence of GGR on maternal mortality, a key indicator of health system quality and women's well-being.

The rest of the paper is organized as follows. Section 2 elaborates on the institutional background of the Global Gag Rule. Section 3 describes the data. Section 4 lays out the grid creation process and our empirical strategy. The main results and mechanisms are discussed in Sections 5 and 6. Section 7 presents the effects on other health outcomes. Finally, Section 8 concludes.

2 The Global Gag Rule

The Global Gag Rule, or "Mexico City Policy",[4] prohibits foreign NGOs from receiving US global health assistance funds for family planning if the NGO promotes abortion as a family planning method, advocates for abortion as a method of family planning, or provides referrals or counseling to women seeking abortion as a means to family planning. It is referred to by its critics as the "gag rule" due to its silencing effect on abortion advocacy. When the Global Gag Rule (henceforth GGR) is in place, NGOs are forced to choose between receiving U.S. funds and providing women with abortion counseling and care that they may want and need, especially if coping with an unwanted pregnancy. Even in cases where abortion is legal in the potential recipient country, NGOs are subject to the GGR, and so, paradoxically, the effects of the GGR may be largest in settings where access to induced or therapeutic abortion is legal.

The GGR was first implemented by Ronald Reagan in 1984 as an extension to the Helms Amendment, an amendment passed by the U.S. Congress in 1973 to limit the use of U.S. for-

[4] So named after the location of the UN conference where Ronald Reagan announced its first iteration in 1984.

eign assistance for abortion.[5] While the Helms Amendment is a law which (since 1973) has permanently restricted any U.S. government funds from being used to fund abortion services, the GGR is an executive order, and as such shifts based on Presidential decree. The GGR has been repealed by subsequent Democratic presidents of the United States and reinstated by Republican Presidents. These repeals and reinstatements are typically made at the very beginning of respective Presidential terms.[6] The GGR is also substantially broader in scope than the Helms Amendment, as rather than restricting how U.S. funds themselves are spent, it restricts how NGOs spend the entirety of *their own resources*, conditioning the receipt of any U.S. health aid on NGO's not directing any funds to abortion provision or counselling. To receive funds, NGOs must certify that they will comply with the GGR's conditions, and, if failure to comply is detected in audits, they are at risk of losing current funding, being barred from future funding, or having their received funds repaid. There is evidence that because enforcement can be strict, many NGOs go beyond the rule (self-censorship), cutting reproductive health information, referrals, or partnerships out of caution, leading to a further "chilling effect" from the policy (Ushie et al., 2020).

While initially, the GGR restricted access to U.S. aid specifically earmarked for family planning (a sub-class of all U.S. global health assistance), in its reinstatement in 2017, Trump's executive order extended the GGR to include restrictions on all types of global assistance for health. This is a clear increase in the incentive to comply with the policy: while around 6% of U.S. aid corresponds to family planning, 30% of U.S. aid is directed to health spending. In this paper, we focus on policy interactions that took place during the Clinton-Bush-Obama-Trump transitions spanning from 1993 to 2020.[7]

A book by van der Meulen Rodgers (2018) provides a comprehensive overview of the GGR's

[5]Named after North Carolina senator Jesse Helms, the amendment states that "no foreign assistance funds may be used to pay for the performance of abortion as a method of family planning or to motivate or coerce any person to practice abortions."

[6]Clinton rescinded the GGR on January 22, 1993; Bush reinstated the GGR on January 22, 2001; Obama repealed the GGR on January 23, 2009; and Trump reinstated the GGR on January 23, 2017. Along with a Presidential executive order, these policy decisions are accompanied by a document outlining their position, published in the Population and Development Review (Clinton, 1993; Bush, 2001; Obama, 2009; Trump, 2017).

[7]The GGR has been reinstated by the second Trump administration in January 2025. In addition, President Trump signed Executive Order 14169 to freeze all U.S. foreign development assistance for 90 days and launch a sweeping restructuring that effectively dismantled the U.S. Agency for International Development (USAID). In relation to the ongoing review of US foreign assistance programs, the DHS Program, whose data is key for the earlier period studies in this paper, is on pause as of late January 2025 (https://dhsprogram.com/).

scope. She suggests that of 64 countries receiving bilateral funding from the US in 2016, 37 of these countries had less restrictive abortion limits than those imposed by the GGR. Thus, NGOs in these 37 countries would not be able to provide abortion services (even with their own funds) if they apply for or continue receiving US health assistance. In the remaining 27 countries with abortion laws at least as restrictive as those imposed by the GGR, the policy would only limit NGOs from providing counseling and advocacy for abortion. van der Meulen Rodgers (2018) suggests that there is a lack of evidence regarding the impacts of the GGR on family planning and women's health,[8] citing just two quantitative studies of the impacts of the GGR on abortion and contraceptive supply (Bendavid et al., 2011; Jones, 2015), as well as herself proving a chapter extending the results of Bendavid et al. (2011) examining abortion rates.

Broader evidence of the impacts of the GGR points to a clear pattern by which the policy has historically interrupted the provision of key inputs into maternal health and family planning. Prior work has documented service disruption through reductions in NGO partnerships and clinic closures (Mavodza et al., 2019), declines in outreach activities including a reduced capacity of community health worker outreach (Jones, 2015; Sully et al., 2022), reductions in staffing and training (Ushie et al., 2020; Maistrellis et al., 2022), declines in collaborations between clinics and referral networks (Maistrellis et al., 2022), and contraceptive stock-outs, with corresponding declines in access to contraceptives among exposed populations (Bendavid et al., 2011; Jones, 2015), when organisations forgo U.S. funding or restructure programs to comply with the rule. Each of these channels conceivably has an impact on both access to healthcare and health outcomes themselves.

3 Data

US Health Aid. We obtain data on U.S. aid for maternal and reproductive health, and for the subcategory of family planning, from the Development Assistance for Health (DAH) Database, the Institute for Health Metrics and Evaluation (IHME, 2024). All receipts are expressed in current US dollars in 2022, thus accounting for inflation. During Obama (2009-2016), the average annual

[8] In particular, she states "Despite the importance of evidence on the global gag rule, there are very few other assessments of the actual impact of the policy on the provision of family planning services and on women's reproductive health. This limited amount of evidence makes it difficult to ascertain the plausibility of the most recent claims in the media following Trump's reinstatement of the policy." van der Meulen Rodgers (2018, p. 4).

U.S. aid for family planning was around 695 million USD, while during Trump (2017-2020) it fell to 530 million USD (see Ferrière, 2024, for further discussion). We merged the aid data with data on health outcomes derived from the DHS by country. Figure 1 illustrates the evolution of U.S.-source family planning aid over time, in terms of aggregated aid volumes to the rest of the world, where the solid lines represent averages over presidential terms.[9] One can observe a clear pattern of aid volatility that coincides with partisan shifts. In particular, Panel A of Table 1 shows that the average family planning aid per capita falls by around 53% during the Clinton-Bush transition, and by around 14% during the Obama-Trump transition. It is important to note here that, while we *do* see shifts in aid receipts broadly lining up with the passage of the GGR, such shifts need not be observed for the GGR to have a substantial effect. Even if all NGO's certified their compliance with the policy and no change in aid receipts was observed, compliance with the GGR sharply changes the types of services and counselling available to women, and it is this policy effect that we are interested in capturing.

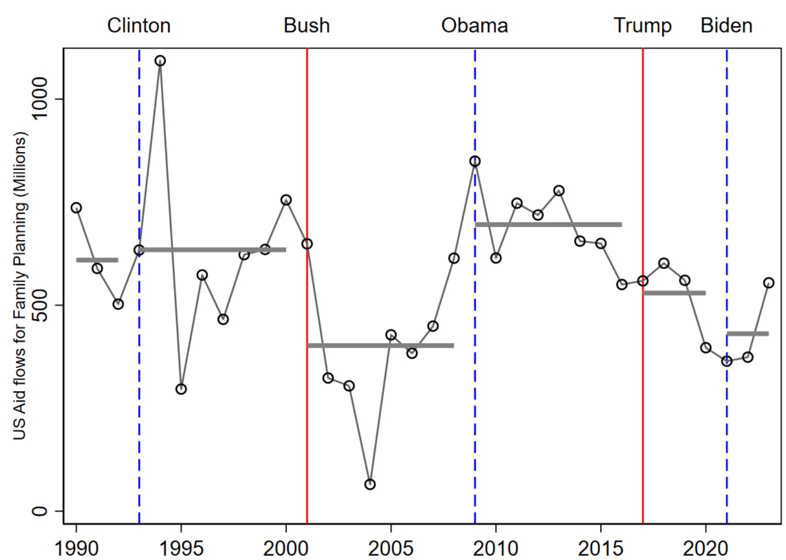


Figure 1: Yearly Variation of U.S. Family Planning Aid

Notes: This figure plots the yearly trends of aggregate U.S.-sourced health aid labeled as family planning. Vertical dotted lines mark the beginning of GGR repeal years, and solid lines mark the beginning of adoption years. The gray horizontal lines are within-presidency averages. *Source:* Development Assistance for Health (DAH) Database.

Health Outcomes. We construct individual records of maternal mortality across countries and over time using data from all publicly available Demographic and Health Surveys (DHS)

[9] In Figure A1, we additionally show the yearly trends for total US health aid as well as US health aid by other categories. Figure A2 illustrates the relative aid volumes by stacking the different categories. Data is yet unavailable for the last year of Biden's term in 2024 and the second Trump administration.

in which a maternal mortality module has been implemented. This includes 138 surveys conducted in 49 countries in Africa, Asia, and Latin America between 1990 and 2019. For empirical analysis of the GGR policy, we focus on both the Clinton-Bush transition (1993-2008) and the Obama-Trump transition (2009-2020).[10] We therefore restrict our final sample to countries with at least one DHS survey wave conducted during the Democratic regime before GGR implementation (Clinton or Obama) and another wave during the ensuing Republican regime post-GGR (Bush or Trump). This results in a final sample of 19 countries, 9 of which are observed during the Clinton-Bush transition and 13 during the Obama-Trump transition.[11]

The DHS follows the sisterhood method for measuring maternal mortality, asking every woman to list all her siblings, their survival status, and, in case of death, when the death occurred and from which causes. This allows us to form a retrospective panel covering all sisters of surveyed women between the ages of 15–49 (or up to their age at the time of survey or death), who are at risk of death during 1985-2019. In this panel, for each woman and year, we generate a binary variable indicating whether or not the woman died of causes relating to childbirth.[12] The maternal mortality rate is then defined as maternal deaths per 1,000 reproductive-aged women. From the DHS, we also construct other relevant health-related outcome variables, such as the infant mortality rate (IMR, defined as infant mortality per 1,000 live births), the exposure to intimate partner violence (IPV), along with a rich set of information on women's health-seeking behavior and access to maternal care services during previous pregnancies. In addition, for countries where the DHS survey includes a reproductive calendar, we observe the history of reproductive events, including conceptions, live births, pregnancy terminations, contraceptive use, etc., of all women surveyed at a monthly interval for around the past 5-6 years until the interview date.[13] We provide a detailed discussion of data generation along with the country and temporal coverage of these variables in Appendix B.1.

In order to exploit within-country variations, we partition countries into 0.5-by-0.5 degree

[10]We are unable to extend to earlier iterations beginning with President Reagan due to limited availability of the DAH aid data and the DHS GPS modules back in the 1980s.

[11]Countries observed during the Clinton-Bush transition are Benin, Cote d'Ivoire, Guinea, Madagascar, Malawi, Mali, Namibia, Senegal, and Togo. Countries observed during the Obama-Trump transition are Cameroon, Cote d'Ivoire, Gabon, Lesotho, Liberia, Mali, Mozambique, Nigeria, Rwanda, Senegal, Sierra Leone, Tanzania, and Zambia. Readers can refer to Appendix B.1 for additional details.

[12]Maternal mortality is defined by the World Health Organization (WHO) as deaths occurring during pregnancy or within 42 days postpartum, from any cause related to or worsened by pregnancy.

[13]The sample of countries included in this part of the analysis is Benin, Liberia, Malawi, Mali, Nigeria, Rwanda, Senegal, Sierra Leone, Uganda, Zambia, and Zimbabwe.

Table 1: Summary Statistics

Variable Name	Mean	SD	Min	Max	Observations	Num. of Countries
Panel A: Aid Volumes from DAH - Period Averages						
Population (in millions)	14.74	24.73	1.17	112.55	19	19
DAH per capita - Clinton	0.29	0.29	0.00	0.87	19	19
DAH per capita - Bush	0.17	0.12	0.01	0.39	19	19
DAH per capita - Obama	0.63	0.51	0.03	1.92	19	19
DAH per capita - Trump	0.79	0.63	0.04	2.33	19	19
Panel B: Health Variables from DHS - Yearly						
Num. of reprod-aged women	376.84	711.06	1.00	15006.00	25,440	19
MMR	5.11	9.09	0.00	147.06	25,440	19
IMR	64.74	87.58	0.00	1000.00	24,945	20
Sexual abuse last 12 months	4.18	9.33	0.00	254.94	5,183	22
Emotional abuse last 12 months	9.69	17.62	0.00	296.27	5,359	22
Physical abuse last 12 months	10.85	17.74	0.00	310.85	5,372	22
Any IPV last 12 months	15.50	24.67	0.00	418.12	5,166	22
Panel C: Health Variables from DHS - Quarterly						
Conceptions	179.79	79.88	0.00	1000.00	57,080	16
Live births	170.69	75.39	0.00	1000.00	57,080	16
Pregnancy terminations	12.00	17.21	0.00	402.76	57,080	16
Contraceptive use	786.74	804.83	0.00	6000.00	57,080	16
ln(Total antenatal visits)	1.23	0.56	-2.57	3.00	45,691	17
Any antenatal care	614.27	348.70	0.00	1000.00	50,933	19
Early antenatal care	240.45	280.32	0.00	1000.00	49,036	19
Antenatal care at facility	650.25	300.46	0.00	1000.00	35,719	19
Any delivery care	3.83	7.96	0.00	201.93	60,912	19
Delivery at facility	4.58	9.15	0.00	204.96	60,766	19
Postnatal check	3.17	6.26	0.00	149.48	29,397	19
Postnatal baby check within 2 months	2.59	5.34	0.00	102.00	36,769	19

Notes: This table reports summary statistics for key variables used in our analysis. Units of observation are the country in Panel A, the grid-year in Panel B, and the grid-quarter in Panel C. In Panel A, DAHperCap is U.S.-sourced family planning aid per capita (in millions of USD), which is reported as country-level period averages by presidential term. In Panel B, MMR is calculated as maternal deaths per 1,000 reproductive-aged women. IMR is calculated as infant deaths per 1,000 live births. IPV measures are reported as rates per 1,000 reproductive-aged women. In Panel C, Conceptions, live births, pregnancy terminations, and contraceptive use are reported as rates per 1,000 reproductive-aged women. Variables related to antenatal, delivery, and postnatal care services are reported as rates per 1,000 births.

grid cells.[14] We then match individual households to corresponding grid based on the geo-coordinates recorded in the DHS GPS module.[15] Panels B and C of Table 1 report the summary statistics of the key variables we use from the DHS. In particular, the average maternal mortality rate in our DHS sample at the grid level across countries and cohorts is 4.98 per 1,000 women.

[14]The size of each grid will be approximately 55.6km×55.6km around the equator and will vary slightly depending on the latitude.

[15]To protect respondent confidentiality, the DHS may displace clusters in rural areas up to 5 km (and up to 10 km for 1% of clusters) and those in urban areas up to 2 km. We think aggregating data into 0.5-degree grid cells is appropriate to absorb this noise and avoid misclassifications, but we also conduct additional robustness checks with respect to grid size. In addition, Figure B1 illustrates the distributions of the number of DHS clusters or households per grid cell over time, for some example countries in our sample.

4 Empirical Strategy

4.1 Grid Creation and Clinic Access

Existing studies of the health impacts of GGR rely on cross-country analysis, masking critical within-country variation in policy exposure (Brooks et al., 2019; Kavakli and Rotondi, 2022). This paper advances the literature by exploiting sub-national differences in clinic access to isolate the distributional consequences of GGR within countries.

Given that GGR disrupts contraceptive access and maternal healthcare primarily through its restrictions on U.S.-funded clinics, we posit that areas with higher baseline clinic access—particularly clinics reliant on foreign aid—are more exposed to GGR shocks. Conversely, areas with low clinic access (e.g., rural regions with sparse infrastructure) are less exposed, as fewer individuals will be exposed to any clinic closures. We thus seek to define within-country exposure to clinics in local areas.

For this reason, we generate local exposure grids at a sub-national level. Specifically, we partition DHS countries in our sample into 0.5-by-0.5 degree grid cells, and we then calculate clinic access at the grid level by matching respondents to their respective grids using the geocode recorded in the DHS GPS module. Given that clinic access is not directly measured in the DHS, we select a set of relevant survey questions to proxy for clinic access. For instance, the portion of individuals who report receiving contraceptives from a health clinic can be calculated as the following for grid j in country c:

$$\overline{\text{Clinic Access}}_{jc} \equiv \frac{\sum_i^{N_{jc}} \mathbb{1}\{i \text{ Reports Contraceptives from Clinic}\}}{N_{jc}},$$

where N_{jc} refers to the number of individuals living in the grid cell.

We construct a composite z-score index of clinic access related to family planning services based on the following survey questions in the DHS: whether the respondent reportedly knows any source for accessing male condoms, whether the respondent reports any private facility or NGO as a source of male condoms, and whether "knows no source, lack of access, or clinic too far" are listed as reasons for not using family planning. The exact survey questions selected from the DHS to construct the composite clinic access index and their descriptive statistics are elaborated in Appendix B.3.

Panels A and B in Figure B3 illustrate changes over time in the constructed clinic access index at the grid level, using Nigeria as an example.[16] In grids that overlap with DHS clusters,[17] some exhibit improved clinic access over time (denoted by blue grids), while others show deterioration (denoted by red grids), highlighting considerable heterogeneity within the country. Moreover, comparing Panel B to Panel A reveals that clinic access worsened overall between two DHS survey waves in 2013 and 2018 (when President Trump reinstated GGR in 2017) compared to the period 2009 and 2013, which occurred within the Obama administration. Panels C and D plot changes in our key outcome of interest: the maternal mortality rate. Maternal mortality is a rare event and therefore appears sparsely distributed across the country, yet we still observe more of an increase in MMR during 2013-2018 relative to 2009-2013, echoing the contemporary decline in clinic access post-GGR implementation.

Given that the impacts of the GGR are likely to vary both across and within countries, exploiting grid-level variation is central to our research design for two reasons. First, by mapping clinic access at the grid level, we capture this heterogeneity by pinpointing "hot spots" within a country that are most vulnerable to U.S. policy shifts. Second, and more importantly, we can isolate the impacts of GGR that are tied to clinic closures or general stresses from restrictions imposed by the GGR on clinic activities, from other contemporaneous confounding shocks that coincide with the presidential switches, a concern that would invalidate the design using cross-country variation only.

4.2 Triple-Differences Estimation Strategy

In the baseline, we implement the following triple-differences specifications at the 0.5-by-0.5 degree grid level:

[16] Nigeria is classified as a high exposure country to GGR in our sample during the Obama-Trump transition, as it was heavily reliant on U.S.-sourced reproductive health aid.

[17] Considering that the grids we create are exogenous, the degree of overlapping depends on the locations of DHS clusters in each country. The DHS uses a two-stage stratified cluster sampling design based on the most recent census (MEASURE DHS, 2012). In each administrative region and urban/rural stratum, enumeration areas (clusters) are selected with probability proportional to size, and a fixed number of households are then systematically sampled within each selected cluster. The procedure is probabilistic rather than purely random, but it yields samples that are statistically representative of the national and regional populations.

$$
\begin{aligned}
Y_{jct} &= \tau(GGR_t \times High_c^{Country} \times High_{jc}^{Grid}) \\
&\quad +\beta_1(High_c^{Country} \times GGR_t) + \beta_2(High_{jc}^{Grid} \times GGR_t) \\
&\quad +\beta_3(High_c^{Country} \times High_{jc}^{Grid}) + \mu_{cs} + \lambda_j + \varepsilon_{jct}, \qquad (1)
\end{aligned}
$$

where Y_{jct} stands for the outcome of interest for grid j (aggregated from all respondents i in the DHS clusters that fall into grid j) from country c and year t. GGR_t is a binary indicator for periods in which the GGR is switched on (Trump or Bush). $High_{jc}^{Grid}$ captures baseline clinic access at the grid level within country c, while $High_c^{Country}$ captures country-level exposure to U.S. aid cuts for country c. We proxy for aid exposure using the observed decline of U.S.-sourced reproductive health aid per capita between presidential transitions (Clinton to Bush or Obama to Trump).[18] For simplicity, we consider discretised values of measures of GGR exposure both across- and within-country: $High_c^{Country} = \mathbb{1}\{\Delta Aid_c \geq \overline{\Delta Aid}_{c,t_0}\}$ denotes high exposure (above-median aid decline) country c and $High_{jc}^{Grid} = \mathbb{1}\{Clinic_{jc} \geq \overline{Clinic}_{jc,t_0}\}$ denotes high exposure (above-median baseline clinic access) grid j in country c. Conceptually, our estimator can also be viewed as an analogue of a shift-share (Bartik) design, where the GGR serves as the aggregate "shift" and the exposure measures represent predetermined "shares".[19]

Our coefficient of interest in (1) is τ, which captures the differential change in outcomes (e.g., maternal mortality) in high-exposure areas (grids with greater baseline clinic access) relative to low-exposure areas within countries that are more affected by the GGR aid cuts, compared to analogous areas in countries less affected by the policy. Country-year fixed effects that control for all country-level shocks or trends (e.g., contemporary diplomatic policy, economic conditions) that vary over time are denoted by μ_{ct}. And λ_j reflects grid fixed-effects that account for

[18] One caveat of using aid declines to proxy for country-level GGR exposure is the potential misclassification of the low-exposure countries. Given that we do not observe NGO compliance with the GGR, countries that experience lower aid declines can still be exposed to GGR due to a high compliance rate, causing service restrictions in the absence of aggregate aid cuts. Our descriptive patterns and double-difference event study in Section 5.2 provide evidence against the possibility of such misclassification.

[19] More formally, our specification can be written as a reduced-form variant of a shift-share model, in which a common policy shock interacts with baseline exposure shares:

$$
Y_{jct} = \alpha + \tau\,(GGR_t \times S_{jc}) + \mu_{ct} + \lambda_j + \varepsilon_{jct},
$$

where $S_{jc} = High_c^{Country} \times High_{jc}^{Grid}$ captures baseline exposure. While in our implementation both the shift and the shares are binary, a continuous formulation (e.g., using continuous measures of aid dependence or clinic access) would yield a closer analogue to a conventional Bartik specification. See Borusyak et al. (2025) for discussion of identification in modern shift-share settings.

time-invariant differences between grids, such as baseline infrastructure or geographic characteristics. Standard errors are clustered at the grid level. In the baseline, we can "pool" both presidential transitions and estimate a stacked model with transition-specific fixed effects.[20]

It is worth emphasizing that our identification does not require that grids with high and low clinic coverage within a country would have counterfactual parallel trends, nor does it require that countries that are highly exposed to the GGR would have counterfactual parallel trends to less exposed countries. Instead, our approach relies on a weaker assumption that trends in high versus low clinic access grids in less-exposed countries serve as a valid counterfactual for trends in exposed countries in the absence of the GGR enactment, that is, parallel trends of the differences between grids. This accounts for the possibility of *catch-up* or *divergence* in clinic access across grids within the same country due to general development trends. In other words, our identification does not require the traditional "sample balance" assumption since time-invariant differences at the grid level are partialled out by the grid fixed-effects.

When the outcome is maternal mortality or other outcomes that are measured retrospectively, like fertility or antenatal care, we observe an extended timespan of different cohorts of women and can test for pre-trends and investigate dynamic effects using the following event-study specification:

$$Y_{jct} = \sum_{k=-K}^{L} \tau_k \left(High_c^{Country} \times High_{jc}^{Grid} \times \mathbb{1}(T_{ct} = k) \right) + \mu_{ct} + \lambda_j + \varepsilon_{jct} \qquad (2)$$

Importantly, we incorporate our event study design into a Poisson pseudo maximum likelihood (PPML) estimation framework when considering outcomes such as maternal mortality. Given that maternal mortality, especially when aggregated at geographic units, typically includes a non-trivial number of zero observations,[21] the Poisson model is preferred as it can accommodate the frequent zero observations as opposed to the log-level specification (Farin

[20] The design is similar to a stacked differences-in-differences design (Cengiz et al., 2019; Gardner, 2022). The difference is that instead of a conventional staggered adoption, we effectively stack different waves of policy iterations, whereby countries highly exposed to GGR in the Obama-Trump transition may have already been exposed in the previous Clinton-Bush transition. In other words, treatment may switch on and off and then on again for some countries, while the data does not allow us to track countries back to the initial pre-treatment period before 1984 (De Chaisemartin and d'Haultfoeuille, 2024). Instead, we treat each presidential transition as an independent event and mitigate potential lingering effects from previous iterations by restricting the timespan to begin with $T-4$ rather than $T-8$.

[21] Figure B2 suggests this is indeed the case when we measure maternal mortality rate at our defined grid cell level.

et al., 2024). In our main specification, we focus on k = −4, ..., 3, where the time unit is a year.[22] In addition, regressions are weighted by the number of reproductive-aged women in the corresponding grid in the baseline years prior to GGR implementation.

5 Main Results

5.1 First Stage: GGR and Clinic Access

Table 2 reports the results estimated from equation 1, which examines the evolution of clinic access over time as a response to the GGR policy. Such a model seeks to assess the relevance of using clinic access to measure exposure to the GGR. Namely, if the GGR affects clinic access, we would expect the triple difference term to be negative. Clinic access is proxied by access to family planning services, standardized to have a mean of one and a standard deviation of one, conditioning on country and survey year. Column 1 shows results for the pooled sample, columns 2 and 3 show results separately for the two periods of presidential shifts.

The coefficient on $GGR \times High^{Grid}$ captures changes in clinic access in periods following the GGR in grids with high clinic access at baseline, but in countries with low exposure to the GGR. The negative and statistically significant estimates suggest that, on average, higher baseline clinic access correlates with steeper declines in service availability in post-GGR periods. This result is consistent with a pattern of *catch-up*, in which access to clinics generally grows more over time in areas starting from a lower baseline (e.g., due to higher domestic investments targeting low-access areas, in line with a low-hanging fruit phenomenon).

The coefficient for the triple interaction term ($GGR \times High^{Country} \times High^{Grid}$), our key coefficient of interest, isolates the differential effect of the GGR in high clinic-access areas in countries with elevated reliance on U.S. aid (proxied by actual aid decline). The negative and significant estimates indicate that high-access grids suffer amplified declines in service availability under the GGR compared to low-access grids. The coefficients imply a 0.22 SD additional decrease in clinic access in the pooled sample, or a 0.10 and 0.42 SD decline during Clinton-Bush

[22] As the GGR policy has been switched on and off over time across presidential regimes, we restrict our time window to begin from $T - 4$ to mitigate potential inherited effects from previous GGR iterations.

Table 2: GGR and Clinic Access

	(1)	(2)	(3)
Estimation Sample	Pooled	Clinton-Bush	Obama-Trump
GGR × $High^{Grid}$	-0.311***	-0.176***	-0.392***
	(0.027)	(0.029)	(0.039)
GGR × $High^{Country}$ × $High^{Grid}$	-0.220***	-0.103**	-0.418***
	(0.041)	(0.046)	(0.059)
Country-Year FE	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes
F-Stat	227.7	48.6	215.2
R^2	0.68	0.73	0.67
Mean Dep. Var.	-0.118	-0.197	-0.081
SD Dep. Var.	0.374	0.381	0.365
Observations	4,469	1,795	2,674
Num. of Clusters	1,696	833	1,248

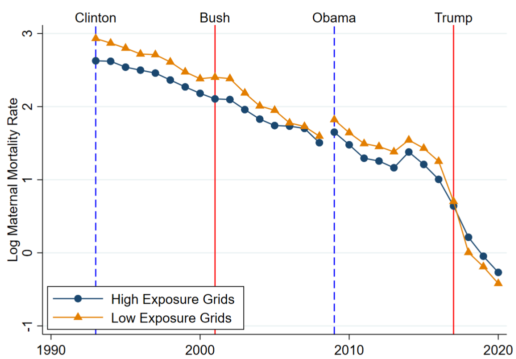
Notes: Estimated coefficients from specification 1, where the dependent variable is clinic access and the unit of observation is the grid. $High^{Country}$ equals 1 for countries with above-median family planning aid decline between Obama-Trump and/or Clinton-Bush. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. Standard errors, in parentheses, are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

and Obama-Trump transitions, respectively. This aligns with our hypothesis of a *compounded vulnerability*: clinics in high-access regions and high-exposure countries are more vulnerable to abrupt aid withdrawals. By contrast, low-access regions, with fewer clinics to lose, exhibit muted effects.

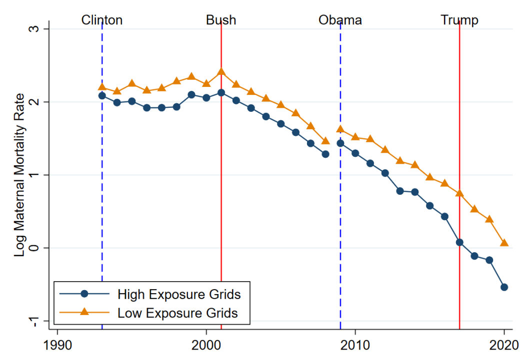
Overall, the results in Table 2 underscore the GGR's role in exacerbating spatial inequities, disproportionately harming areas where clinic infrastructure is both critical and precariously tied to U.S. policy shifts. Moreover, the magnitude of the effect seems to suggest that service disruptions were substantially worse under the expanded Trump-era GGR implementation. This result lines up with effects which have been noted in structured interviews and qualitative studies (Maistrellis et al., 2022; Mavodza et al., 2019).

5.2 The Effect of GGR on Maternal Mortality

We begin by exploring the dynamics in the raw data for our main outcome variable, i.e., maternal mortality. Panels A and B of Figure 2 display the raw data of log maternal mortality rate over the time window of 1993-2020 for high- and low-exposure countries in our sample, respectively. Over time, both groups witnessed a decline in maternal mortality. Within countries, low-exposure (low baseline clinic access) grids underperform compared to high-exposure grids on average, in line with lower health service coverage in these areas. However, the discrepancies narrow down among high-exposure countries during Republican regimes (Panel A), a pattern unobserved in low-exposure countries (Panel B). The figures provide a descriptive visual test for our identification strategy, as we observe parallel trends in the Democratic periods, and a negative impact on MMR when GGR sets in, but only in areas and countries most susceptible to the policy.



(A) High Exposure Countries



(B) Low Exposure Countries

Figure 2: Yearly Trends of Log MMR

Notes: This figure plots the yearly trends of log MMR averaged across grids, separately for high- and low-exposure grids and high- and low-exposure countries. *Source:* Authors' calculations from DHS data.

We identify these dynamics more precisely by plotting the point estimates obtained from the estimation of equation 2. Figure 3 shows the event-study estimates for the full sample pooling both Clinton-Bush and Obama-Trump transitions, restricting to the time window of [-4,3]. The figure shows that pre-trends are parallel in our preferred baseline event study specification as well, with the positive effect of GGR on MMR materializing immediately at the year of reinstatement and persisting in the subsequent years. It is worth noting that we use the full (unbalanced) sample of countries in the baseline to maximize power, but conduct robustness checks when restricting to a balanced sample and compare the results in Figure A4. In addition, we plot the event study when alternative continuous measures of exposure are adopted (Figure A5), and the results are broadly similar.

As a diagnostic check of our triple-differences framework, Figure 4 further reports the triple

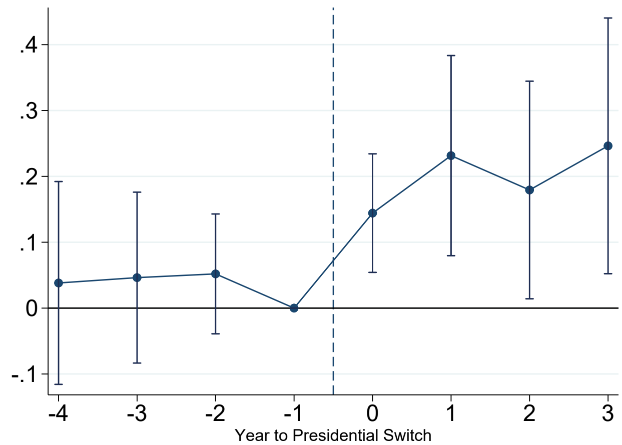


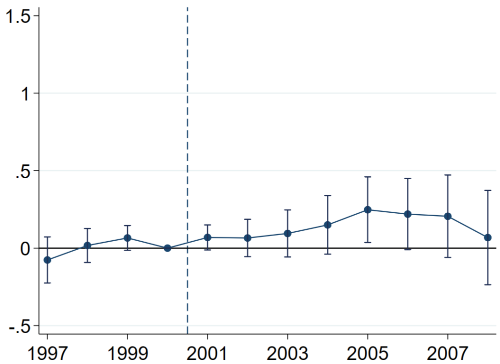
Figure 3: GGR and maternal mortality – Pooled Sample

Notes: This figure presents the event study estimates of the effects of GGR on MMR using the pooled sample, following specification 2, where the unit of time is the year. Reporting 95% confidence intervals.

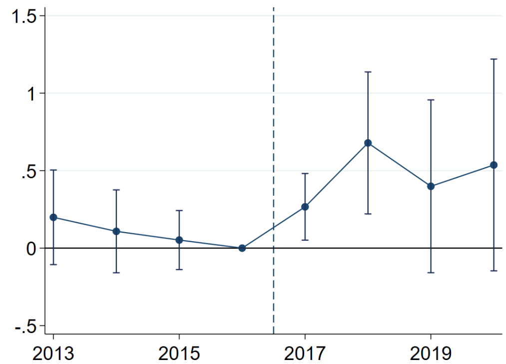
difference estimates separately by presidential term (Panels A and B), as well as the underlying double-difference estimates for high-exposure countries and low-exposure countries (Panels C and D). Consistent with our expectations, the positive effects of GGR on MMR are concentrated in high-exposure countries. Contrary to our expectations, the negative coefficients, although statistically insignificant, of low-exposure countries suggest *divergence* rather than *catching-up* between low- and high-exposure grids. One possible explanation is that some low-exposure countries actually received increased aid post-GGR switch-on, an idiosyncratic pattern observed particularly during the Trump iteration. In other words, the low-exposure countries are in fact "positively" exposed to the GGR. Overall, the double-differences patterns enhance additional credibility to our empirical design, where treatment effects manifest before we purge out any generalized differences across different groups of countries.

Taken together, our main results underscore that the adverse impacts of the GGR on maternal health outcomes are concentrated in areas that are more exposed—both at the local level (high baseline clinic access) and at the national level (high observed aid cuts). Our findings reinforce the argument that policy-induced disruptions can have significant downstream effects on public health, particularly in the most vulnerable regions.

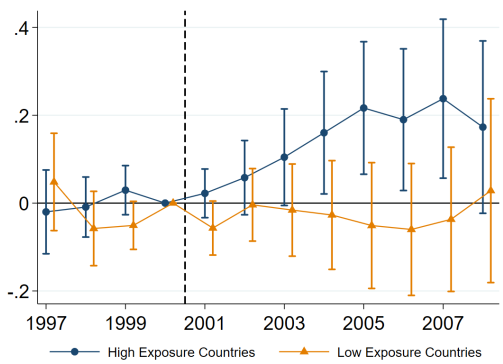
The effects of switch-off. We also examine the "mirroring" effects of GGR switch-off, using data available from the Bush-Obama transition. The event study results for both the triple differ-



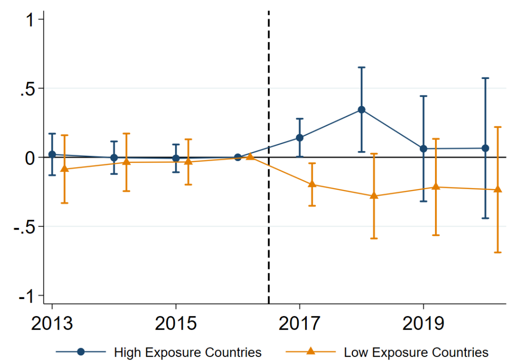
(A) Clinton-Bush: DDD



(B) Obama-Trump: DDD



(C) Clinton-Bush: Double Differences

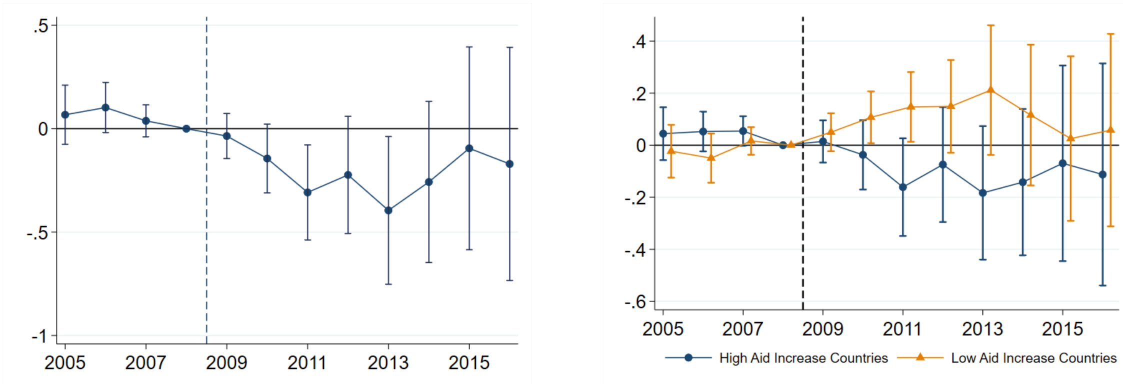


(D) Obama-Trump: Double Differences

Figure 4: GGR and MMR - Decomposition

Notes: This figure presents the event study estimates of the effects of GGR on MMR separately by presidential transition, following specification 2 (Panels A and B), along with their double-difference decompositions (Panels C and D). Reporting 95% confidence intervals.

ence and the double differences are reported in Figure 5. The results suggest a certain degree of effect reversal: MMR gradually decreases when GGR is switched off, even though this effect is not immediate but rather takes two to three years to materialize. Moreover, the uptick of low-exposure (defined as below-median aid expansion during the Bush-Obama transition) countries in Panel B does suggest *catch up* between high- and low-exposure grids in the absence of GGR influence. We believe this finding to be of key importance to understanding the long-term dynamic effects of GGR policy cycles, as it indicates some degree of recoverability of the harm done by GGR (at least during Bush-Obama) when reproductive health aid and services are gradually restored.



(A) Bush-Obama: DDD

(B) Bush-Obama: Double Differences

Figure 5: GGR and MMR - Switching off

Notes: This figure presents the event study estimates of the effects of GGR switch-off on MMR, using data from the Bush-Obama transition, following specification 2. Reporting 95% confidence intervals.

Effect magnitude. We interpret the magnitude of the effects by estimating the triple-differences equation 1, reported in Table 3. Column 1 corresponds to the sample pooling both transitions of GGR switch-on, while columns 2-3 report by presidential transition. We see that GGR has a positive and statistically significant impact on MMR across different periods of policy iteration. To interpret the size of the effect, we transform the coefficients from Poisson regressions into percentage-change effects (calculated as $exp(\tau)$, the incidence rate ratio, minus 1). For the pooled sample, GGR increases the maternal mortality rate by around 20%. A simple back-of-the-envelope calculation would suggest that about 16.7% of all post-GGR maternal deaths are attributable to GGR. The effect magnitude is larger for the Obama-Trump transition (40.5%) compared to the Clinton-Bush transition (14.2%). On the other hand, column 4 shows that when GGR is switched off during Bush-Obama, we observe an effect reversal of a 27.9% relative decrease in MMR.[23] A recent study shows that denial of a wanted abortion in Colombia raised women's risk of death by 2.5 percentage points or 161% (mainly from unsafe procedures) within the next nine months (Londoño-Vélez and Saravia, 2025). Another study by Farin et al. (2024) finds that abortion legalization in the U.S. during 1969-1973 lowered non-White maternal mortality by 30-50 per cent. In comparison, the magnitudes of our estimates do not seem to be implausibly large, considering our treatment is the potential reduction in contraceptive and abortion access as a result of GGR-related aid cuts, rather than abortion access itself.

[23] A caveat in comparing the magnitudes across columns is that the sample of countries observed during different presidential transitions can be different due to DHS survey availability.

Table 3: Effects of GGR on Maternal Mortality

	GGR Switch-on			GGR Switch-off
	(1)	(2)	(3)	(4)
Estimation Sample	Pooled	Clinton-Bush	Obama-Trump	Bush-Obama
GGR $\times$ $High^{Grid}$	-0.052	-0.017	-0.157	0.121
	(0.051)	(0.053)	(0.123)	(0.079)
GGR $\times$ $High^{Country}$ $\times$ $High^{Grid}$	0.182**	0.133*	0.340**	-0.246**
	(0.075)	(0.080)	(0.167)	(0.122)
Country-Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
$exp(\tau) - 1$	20.0%	14.2%	40.5%	-27.9%
Mean Dep. Var.	6.042	10.759	3.821	6.585
Median Dep. Var.	3.644	9.315	2.147	4.862
SD Dep. Var.	6.970	7.359	5.527	5.841
Observations	9,626	5,499	4,127	6,111
Num. of Clusters	977	461	647	534

Notes: Estimated coefficients from specification 1, where the dependent variable is MMR and the unit of observation is the grid. $High^{Country}$ equals 1 for countries with above-median family planning aid decline between Obama-Trump and/or Clinton-Bush, and it equals 1 for countries with above-median family planning aid increase between Bush-Obama. GGR equals 1 for years when GGR is switched on in columns 1-3 and for years when GGR is switched off in column 4. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. Standard errors, in parentheses, are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Robustness to additional controls. We assess the robustness of our main results by including a set of additional control variables at both the cross-country and within-country grid level.

We begin by exploring the role of alternative aid donors. Existing studies demonstrate that different donors may exhibit different tendencies to compensate for US policy shifts or to follow suit (Ferrière, 2024), both of which may affect the degree to which US aid cuts impact resource availability on the ground. Figure A3 shows that, on the aggregate, alternative non-U.S. donors in family planning aid tend to follow suit with the U.S. during the Clinton-Bush transition, while there is a stronger tendency to compensate during Obama-Trump. To account for the role of potential donor compensation, we include an additional country-level measure of aid exposure to non-US reproductive health aid declines during the same transition periods in equation 1. Column 3 of Table A1 shows that once this is controlled for, the adverse effects of the GGR vanish during the Clinton–Bush transition, consistent with non-U.S. donor behavior moving in tandem with U.S. aid. By contrast, columns 4–5 indicate that during Obama–Trump, the estimated effects remain even after accounting for compensation, suggesting that alternative donor efforts fell short of fully offsetting the negative consequences of U.S. aid cuts.

Next, we examine the robustness of our main results to the inclusion of local economic controls to mitigate concerns about remaining time-varying confounders (such as fluctuations of aid in other sectors) at the grid level. We collect global GDP estimates at the 0.5-by-0.5 grid-level from Rossi-Hansberg and Zhang (2025). There are two caveats in utilizing their dataset. First, the timespan of their data covers 2012-2021, which restricts our robustness test to the Obama-Trump period only. Second, the gridding approaches differ slightly due to alternative reference points.[24] In columns 1-2 of Table A2, we first compare the baseline triple-difference estimates using the two alternative gridding methods, to ensure our results are not sensitive to arbitrary ways of gridding. We then test and confirm the robustness of this result to the inclusion of local population and predicted GDP measures in columns 3-5. An additional caveat of using the decline of U.S. health aid per capita to define country-level exposure is that population is potentially endogenous when the outcome is mortality. Columns 3 and 5 alleviate this concern by showing that the results are robust to controlling for grid population.

Even though there is ongoing debate on the overall economic consequences of foreign aid in recipient countries (Qian, 2015), recent studies focusing on Chinese aid projects in Africa point out that Chinese aid is positively associated with local economic growth (Dreher et al., 2021; Mueller, 2025). While we lack direct aid data at the local grid cell level, the evidence presented above helps alleviate concerns where the convoluted roles of alternative aid donors or non-reproductive health aid pose a threat to our empirical identification.

Spatial spillovers. In the absence of GGR, we expect some degree of positive spatial spillovers to low-exposure grids that border high-exposure grids for two reasons. First, women who reside in low-exposure grids could be traveling to nearby high-exposure grids for clinic access. Second, considering that NGOs often allocate programs in contiguous clusters, low bordering grids may still receive spillovers from shared catchment areas, outreach teams, or supply chains. Both channels are hampered by the disruptions of clinic access in high-exposure grids following the GGR.

Table A3 examines the geographic spillover effects in detail, where we include grid-level measures of whether a grid borders high-exposure grids as per the definition of queen's con-

[24] To be specific, Rossi-Hansberg and Zhang (2025) uses the world map where grids start at coordinates (0,0), while we use maps for each individual country in our DHS sample where the starting coordinates differ by country.

tiguity (each grid has a maximum of 8 neighbors) in equation 1. Columns 2-3 show evidence of "negative" spillovers where grids bordering high grids also saw higher maternal mortality, both when the exposure measure is binary (having any high-exposure neighbor) or continuous (the total number of high-exposure neighbors), even though the effects are not statistically significant.[25] Reassuringly, the results in columns 4-5 show that the direct effects of GGR on MMR are robust conditioning on these potential indirect spillovers. To mitigate a related concern that our results are sensitive to the arbitrary choice of grid size, we conduct robustness checks by varying the grid size and report the pooled event study in Figure A6.

Heterogeneity analysis. We explore effect heterogeneity by baseline observable characteristics such as age and women's status within the household.[26] To take into account the possibility that younger women may face different constraints in contraceptive access due to greater social stigma, legal and provider restrictions, and lower autonomy in decision-making, we adjust our baseline grid-level measure of clinic access to be age-group-specific. Table A4 shows limited heterogeneity in treatment effects by age group, though splitting the sample along age lines reduces our statistical power to detect such differences.

We also categorize grids into high- versus low-status by aggregating survey responses on women decision power within the household and adopt the median threshold in the baseline year when GGR is switched off.[27] Table A5 summarizes the findings. Columns 1-2 suggest that the effects appear to be concentrated in grids where women possess greater decision power in the baseline. The result is consistent with differential exposure to disrupted services: higher-status women are more likely to reside in high-exposure grids and utilize facility-based obstetric

[25] We want to emphasize that we observe a fair degree of spatial clustering (example of high-exposure countries illustrated in Figure B4), in addition to grids with missing information from the DHS. In practice, this means our bordering-grid coefficients may partly capture correlated shocks rather than true cross-border spillovers. We therefore interpret the spillover estimates as reduced-form correlations conditional on spatial structure, not as fully identified causal effects.

[26] We also conjecture that the impacts of GGR can be heterogeneous based on the baseline legal status of abortion across countries (van der Meulen Rodgers, 2018; Clarke and Mühlrad, 2021; Farin et al., 2024). Unfortunately, we do not have substantial variation in terms of baseline abortion legality to rigorously analyze such effect heterogeneity. Out of the 19 countries included in our main analysis, only two (Mozambique and Zambia) possessed permissive abortion laws at the time of the GGR initiation by Trump in 2017. All other countries possess restrictive abortion laws where abortion is permitted only to save a person's life or to preserve health (Center for Reproductive Rights, 2020). Our results indicate that even in countries with already restrictive abortion laws, the GGR can still impose additional constraints on the delivery of abortion-related services and information.

[27] The specific questions used to construct the proxy are if the woman has sole or joint decision power over making large household purchases, visits to family or relatives, spending woman's earnings, and on woman's health care. We aggregate these measures into a composite index.

care. However, these results need to be interpreted with caution. Columns 3-4 show that, once we interact the treatment with baseline status measures in a more saturated way, the coefficients of the interaction term are of opposite signs and statistically insignificant, suggesting limited power for us to detect heterogeneity in this dimension.

6 Mechanisms of Action

The previous results trace a link between the imposition of the GGR and extreme measures of maternal health (namely, survival). There are a number of channels that may mediate such effects, and we explore the relevance of such channels here. A first is reduced access to contraception and safe termination of pregnancies, which may increase the incidence of unsafe abortions and high-risk pregnancies. And a second is reduced availability or quality of maternal healthcare services, including antenatal care, skilled attendance at delivery, and postnatal care.

6.1 DHS Reproductive Calendar

Unsafe abortion is a major cause of maternal mortality (Girum and Wasie, 2017), particularly in sub-Saharan Africa. We gather information from the DHS reproductive calendar, which allows us to create a longitudinal record of all reproductive events (conception, birth, termination of pregnancy, etc.) for each woman in each calendar month for the past 5-6 years. We note two limitations of this analysis. First, as discussed in section 3, given the availability and the cohort coverage of the calendar data, we only observe a sufficient sample of countries for the Obama-Trump transition in this part of the analysis. Second, different from the retrospective sibling module used in our main analysis, the sample in the calendar data is all reproductive-aged women directly surveyed, which might be subject to *survivor bias*.[28]

As we observe the reproductive history of all women at the monthly level, we can obtain event-study estimates using equation 2, where the unit of time is the quarter and the outcomes

[28]For example, survivor bias can arise as women who potentially died from unsafe abortions are not observed in the data, leading to a downward bias in the estimated effects of GGR on unsafe abortions.

are relevant reproductive events.[29] The event-study plots are reported in Figure 6 and the corresponding table estimates are summarized in Table A6. In sum, Panel A illustrates a drop in conceptions with the effect setting in gradually in the second year. Panels B and C show an immediate rise in total live births and pregnancy terminations the year post-GGR, and Panel D illustrates a persistent decline in overall contraceptive use. The decline in contraceptive use is consistent with the evidence of deteriorating clinic access we document in the first stage. The temporary rise in live births and pregnancy terminations can reflect the downstream consequences of initial disruptions in family planning and abortion services, where more pregnancies are either "forced" to be carried to term or result in unsafe abortions.[30] The decline in new pregnancies is in line with the interpretation that women adjust their reproductive behavior because, over time, they perceive access to safe abortion or reliable contraception to be more precarious under the GGR. Overall, albeit suggestive, the results in Figure 6 show that disruptions in contraceptive access and unsafe abortions line up with the elevated rates of maternal death documented previously. Combined with these factors' well-documented links with elevated rates of maternal mortality (Ahmed et al., 2012; Londoño-Vélez and Saravia, 2025), this result suggests a clear moderating effect linking GGR passage to elevated mortality rates.

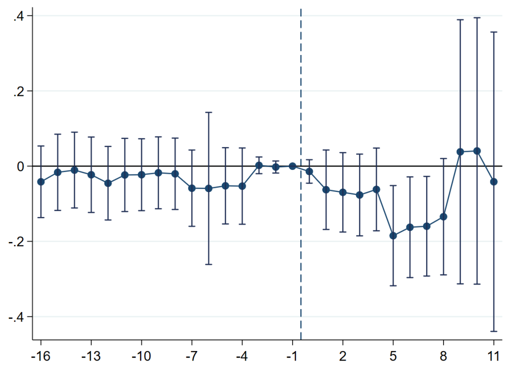
6.2 Maternal Healthcare Services

Other than unsafe abortions, maternal mortality can also arise from causes such as hemorrhage, sepsis, eclampsia, obstructed labor, along with other complications from delivery (Ronsmans and Graham, 2006; Musarandega et al., 2021). Therefore, poor availability or quality of maternal care, including antenatal and postnatal care, as well as skilled attendance during delivery, can increase the risk that pregnancy-related complications go untreated, thereby raising mortality from such causes.

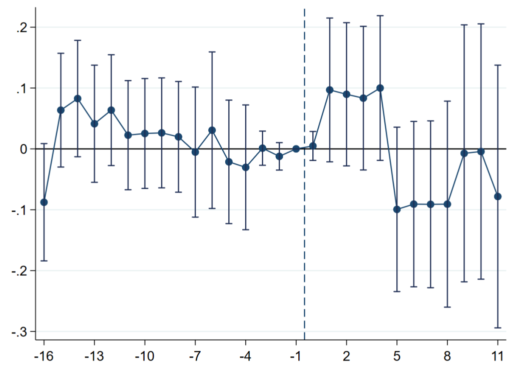
To investigate whether the onset of GGR disrupted maternal care services more generally, we use DHS survey responses from all reproductive-aged women regarding maternal care services

[29] We choose the trimester as the preferred time unit, as it is less noisy compared to monthly data but provides more refined dynamics compared to yearly data. Even though GGR was reinstated by Trump in January 2017, we define the last quarter of 2016 as $t = 0$ to take into account potential anticipation effects after the election outcome is known.

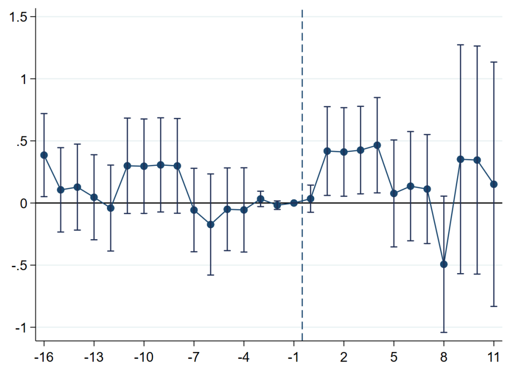
[30] Strictly speaking, pregnancy terminations include both spontaneous and induced terminations (abortions). Abortion is rarely directly asked in the DHS and is typically under-reported. However, one can also follow the algorithm proposed in Magnani et al. (1996) and implemented in Brooks et al. (2019) to indirectly infer abortions.



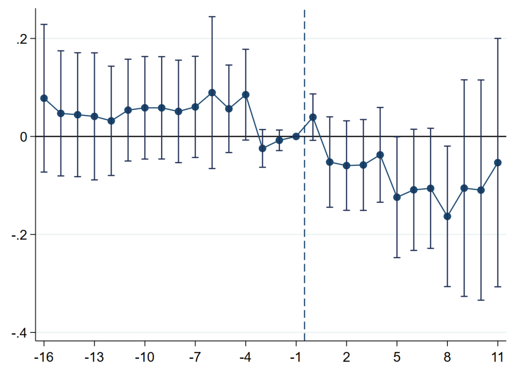
(A) New Pregnancies per 1,000 Women



(B) Live Births per 1,000 Women



(C) Pregnancy Terminations per 1,000 Women



(D) Contraceptive Use per 1,000 Women

Figure 6: DHS Calendar Outcomes - Quarterly Event-Study

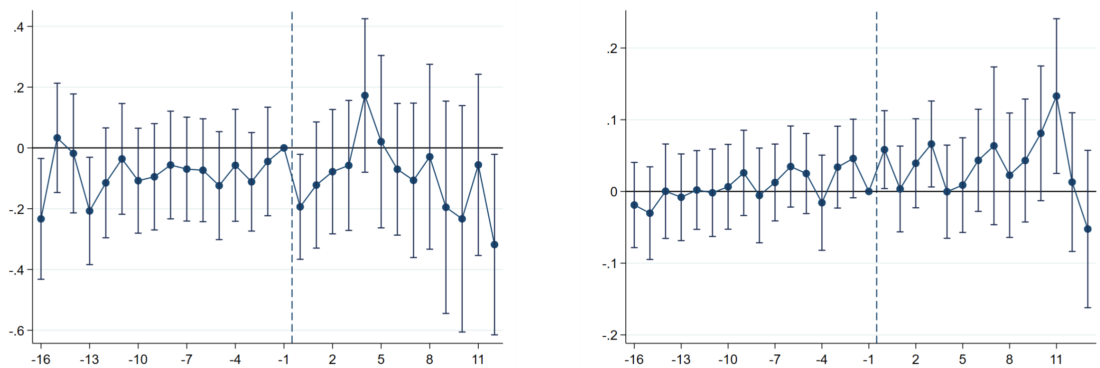
Notes: This figure presents the event study estimates of the effects of GGR on reproductive events recorded in the DHS calendar, following specification 2 where the unit of time is the quarter. Reporting 95% confidence intervals.

they reportedly received for their last recalled births (up to six previous births per woman). We construct two composite indices, one for antenatal care and one for delivery and post-delivery care.[31] Unlike the calendar records, these data provide sufficient coverage for both the Clinton–Bush and Obama–Trump transitions. In addition, we can construct a retrospective panel at the grid–quarter level by pooling all woman-birth pairs according to the calendar month of birth. For antenatal care variables, we use the quarter of conception as the reference time for GGR exposure. For delivery and post-delivery variables, we use the quarter of birth as the reference.

Figure 7 presents the event-study estimates for the effects of GGR on the two composite in-

[31]The variables related to antenatal care include whether antenatal care was provided by health professionals, whether it happened at a health facility, any antenatal care in the first three months of pregnancy, and the total number of antenatal visits. The composite index is constructed as a Z-score. The variables related to delivery and post-delivery care include delivery by health professionals, delivery at a facility rather than home, post-delivery health check, and postnatal baby check within two months. The composite index is constructed as described by Anderson (2008), as all variables are binary.

dices of maternal care service access. Panel A shows a slight decline in antenatal care access post-GGR, while Panel B indicates an increase in delivery and post-delivery care access, yet neither effect appears statistically distinct from zero. We also report the triple-difference coefficients on all relevant indicators in the coefficient plots in Figure 8, including when the sample is all births or restricted to the more recent births only (within six years prior to the interview date).[32] The coefficients again suggest that GGR has, if anything, a marginally significant negative effect on antenatal care services, and no significant effects on delivery care services. In fact, only the coefficients on postnatal care are negative, yet they are noisily estimated.



(A) Antenatal Care per 1,000 Births

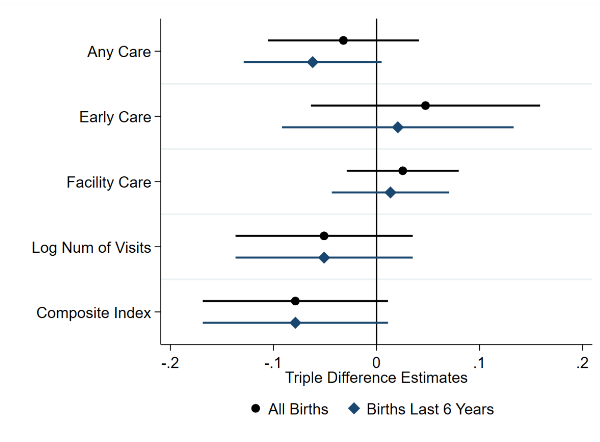
(B) Delivery & Post-Delivery Care per 1,000 Births

Figure 7: Maternal Care Services - Quarterly Event-Study

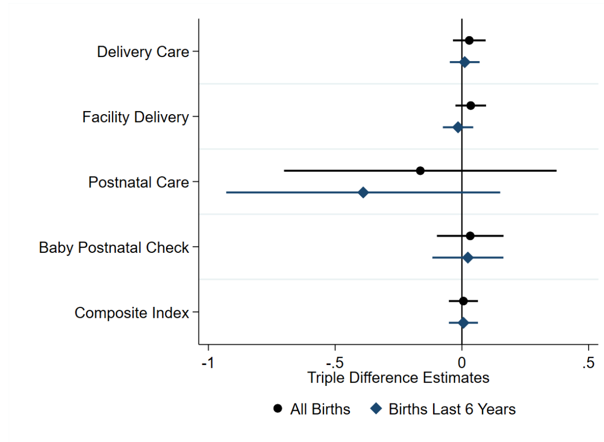
Notes: This figure presents the event study estimates of the effects of GGR on reported access to maternal care services, pooling our births recorded in the DHS, following specification 2 where the unit of time is the quarter. Reporting 95% confidence intervals.

The overall evidence in this section is more consistent with changes in reproductive autonomy as well as access to contraceptives and safe abortion services. However, considering that the sample does not perfectly align with that of the MMR module, together with potential *survivor bias*, we cannot rule out a contribution from reductions in maternal care services in the meantime. Both mechanisms may be driving the rise in maternal mortality following the GGR.

[32] Restricting to the births within six years prior to the interview only may help mitigate potential *recall bias* from DHS survey responses, which also makes the timeline more comparable to that of the DHS contraceptive calendar.



(A) Antenatal Care Indicators



(B) Delivery & Post-Delivery Care Indicators

Figure 8: Maternal Care Services - Coefficient Plots

Notes: This figure presents the triple-difference coefficients of the effects of GGR on reported access to maternal care services, following specification 1. Reporting 95% confidence intervals.

7 Broader Impacts on Health and Well-being

Infant and child mortality. We also examine the effects of GGR on infant mortality rate (IMR), defined as the number of deaths of infants under one year of age per 1,000 live births in a given year. On one hand, maternal health deterioration and maternal deaths may have spillover effects on infant health and survival. Similarly, increased rates of unplanned birth are likely to have direct effects on child health outcomes (Mayol and Pereira, 2023). On the other hand, Panel C of Figure A1 shows that, unlike reproductive health aid, the neonatal and child health aid from the US does not fluctuate with GGR but instead rises over time, which can potentially improve infant outcomes directly.

To empirically disentangle these effects, we extend equation 1 by incorporating an additional country-level exposure measure capturing the "relative decline" of US aid for neonatal and child health, where $High_{nch}^{country} = 1$ indicates a lower relative increase in this aid category. To begin with, Panel A of Table A7 shows that total fertility is not affected by the decline in reproductive health aid, but saw a relative decline where neonatal and child health aid increased less. Panel B then illustrates the counteracting roles of reproductive health aid and neonatal/child health aid: regions that experienced a rise in maternal mortality also saw a relative decline in infant mortality, even after accounting for volatility in neonatal and child health aid. The pattern is more salient during Trump (column 5), where $High_{nch}^{country}$ and $High^{country}$ negatively correlate with each other. Similar patterns are observed for the under-five mortality rate (Panel C). We interpret these patterns as plausible if there exists a reallocation of limited resources toward neonatal and child health that partly offsets the deterioration in maternal services. To support this claim, our results from Figure 8 also show positive (though non-significant) effects on post-natal baby check two months after delivery.

Placebo tests of other mortality measures. To strengthen our belief that the effects on MMR are not driven by other confounders, we conduct placebo checks using the same specification on non-maternal related female mortality rate as well as all-cause male mortality rate. Both mortality measures are constructed from the same DHS sibling module using siblings who died in the age range of 15-49. The results are summarized in Table A10. Reassuringly, we do not see significant impacts of GGR on these other mortality measures. This suggests that the disruptions caused by GGR are concentrated in contraceptive access and maternal health-related services, rather than general healthcare access.

Intimate partner violence and female empowerment. Next, we examine the impacts of GGR on reported intimate partner violence (IPV). A subset of DHS countries includes a standardized domestic violence module where women are asked about any specified physical, emotional, or sexual abuse experienced in the past 12 months by their current or former partners. The DHS surveys aim to minimize potential under-reporting through random woman selection, interviewer training, and interview technique when asking questions of such a sensitive nature (Bhalotra et al., 2021b). However, because the IPV module is not implemented in all survey rounds, our analysis is limited to the Obama–Trump transition. Moreover, as the IPV questions are tagged to specific survey years for each country, we are not able to exploit dynamic effects as with the mortality measures. We thus take the evidence in this section as suggestive.

Table A8 shows that GGR during the Trump period is associated with a lower incidence of reported IPV in the past 12 months. These negative associations remain when we control for fertility-related variables, including whether the woman was pregnant in the last year and whether her fertility preferences align with those of her partner (column 5). Although the negative estimates appear at odds with several recent studies showing that national abortion restrictions raise IPV risks (Dave et al., 2025; Muratori, 2025), we think they can be supported by two additional pieces of evidence. First, Table A9 shows some suggestive evidence of female empowerment and increased cooperation within the household following GGR. Second, Panel A of Figure 6 documents a short-run decline in new pregnancies, which may have reduced sources of intra-household conflict associated with fertility disagreements or unplanned births.

8 Conclusion

The Global Gag Rule has generated intense public policy debate and media coverage, attracting the support of Right to Life campaigners while alienating supporters of development aid and women's rights. The recent tightening of the GGR under President Trump (Kates and Moss, 2017) has been referred to as a "war on women". Our findings are relevant to the UN Sustainable Development Goals for Health and Gender Equality. Specifically, our results indicate that the expanded GGR is associated with an increase in maternal deaths in areas and countries most vulnerable to U.S. aid cuts. Comparing different presidential terms and policy iterations, we also uncover a much larger effect during the (first) Trump administration.

All in all, our findings suggest that U.S. policy decisions can have noteworthy impacts on global health measures over the relatively short time frame of presidential administrations. Maternal death is only the tip of the iceberg of maternal complications, including hemorrhage, sepsis, abdominal and reproductive tract infections, uterine perforation, cervical tears, chronic pain, infertility, and elevated risks in subsequent pregnancies (WHO, 2019). Access to safe abortion is a fundamental right of women. Our work underlines the significant and systematic loss of life and life quality among millions of women and families that emerges from the lottery of U.S. election outcomes.

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Online Appendices for:

“The Global Health Toll of the Global Gag Rule”

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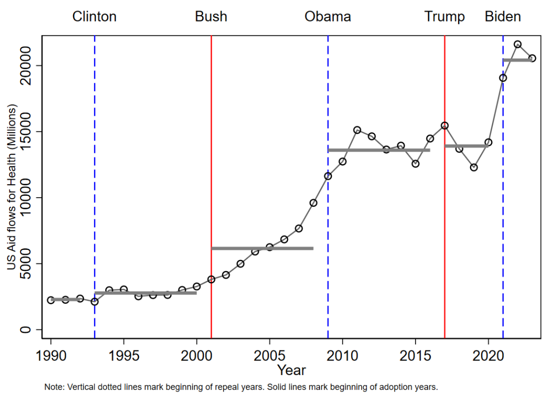
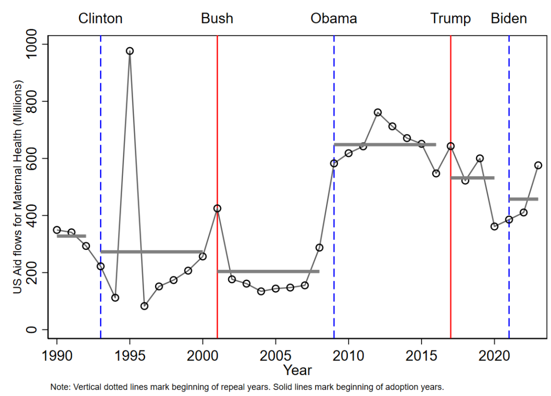
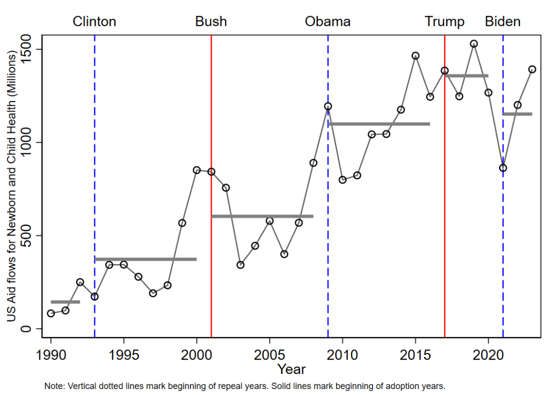
A Additional Tables and Figures



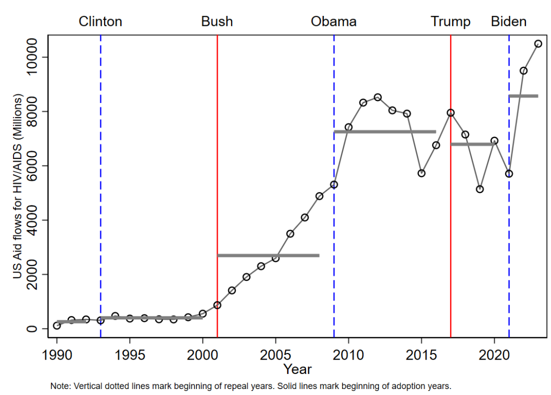
(A) US Total Health Aid



(B) US Maternal Health Aid



(C) US Neonatal and Child Health Aid



(D) US HIV Aid

Figure A1: US Health Aid – Other Categories

Notes: This figure plots the yearly trends of aggregate U.S.-sourced health aid overall and in other categories. *Source:* Development Assistance for Health (DAH) Database.

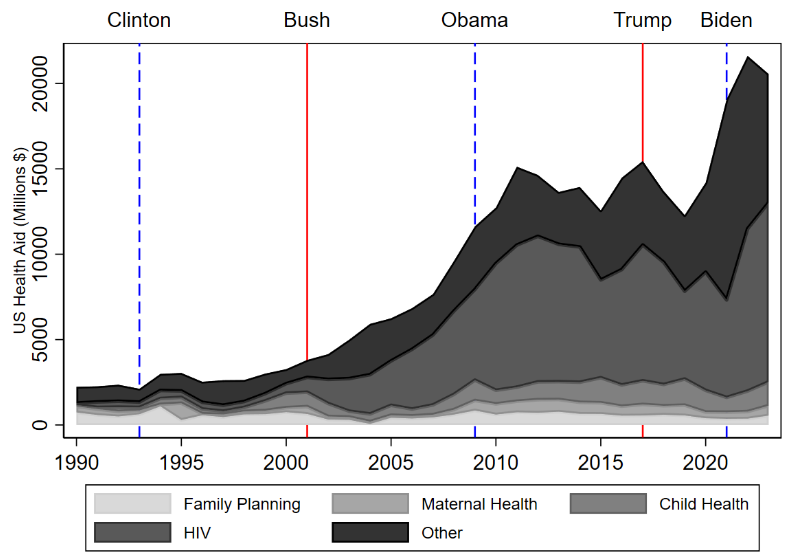


Figure A2: U.S. Health Aid Stacked by Category

Notes: This figure plots the yearly trends of aggregate U.S.-sourced health aid, stacked by category. Vertical dotted lines mark the beginning of GGR repeal years, and solid lines mark the beginning of adoption years. *Source:* Development Assistance for Health (DAH) Database.

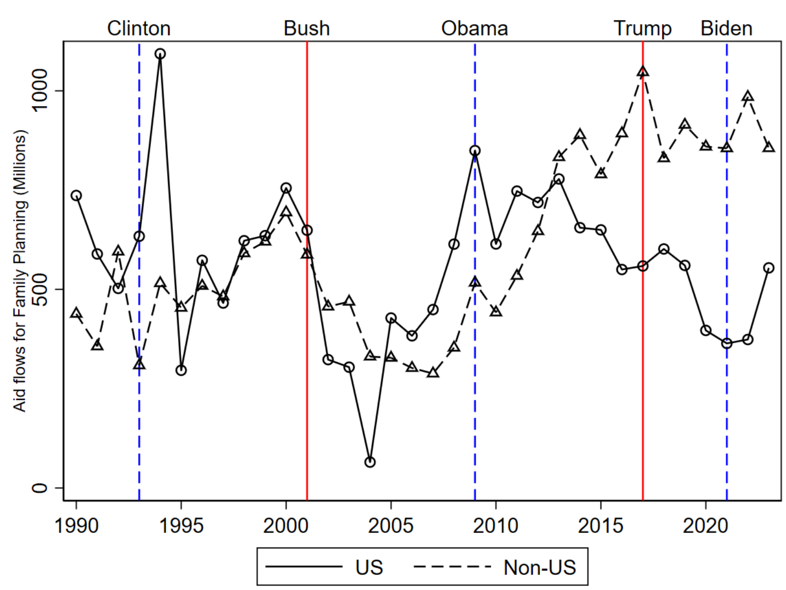
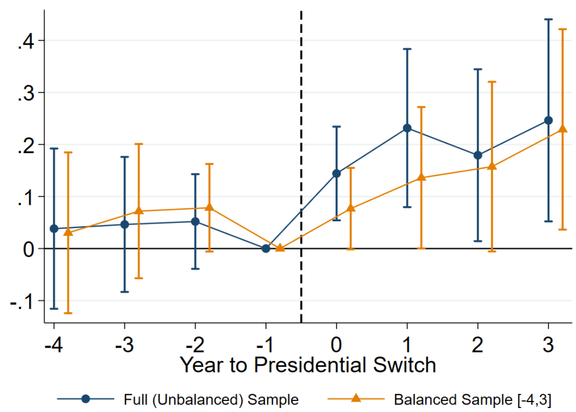
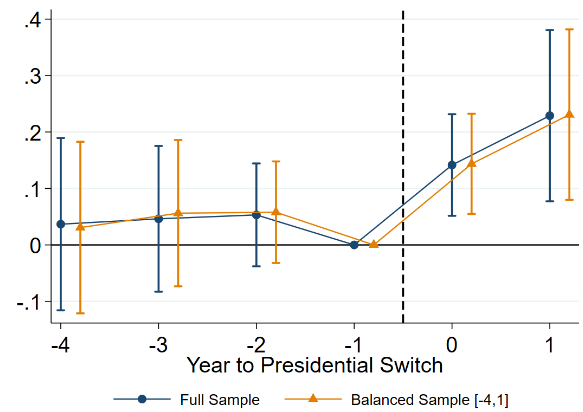


Figure A3: U.S. vs Non-U.S. Aid in Family Planning

Notes: This figure plots the yearly trends of aggregate U.S.-sourced versus non-U.S.-sourced health aid labeled as family planning. Vertical dotted lines mark the beginning of GGR repeal years, and solid lines mark the beginning of adoption years. *Source:* Development Assistance for Health (DAH) Database.

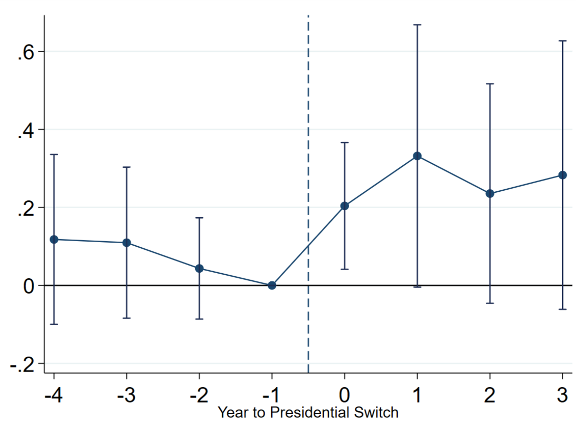


(A) Long Timespan

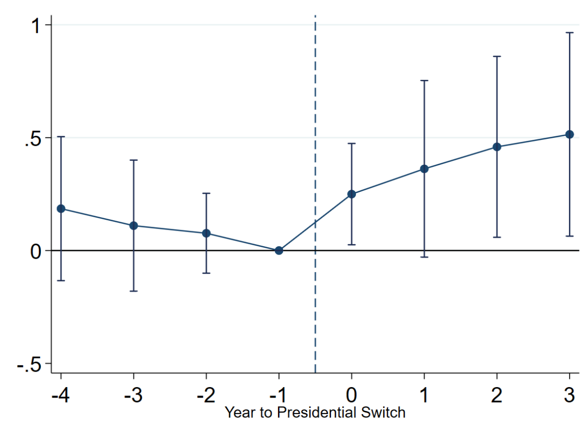


(B) Short Timespan

Figure A4: GGR and MMR - Pooled - Sample Balance

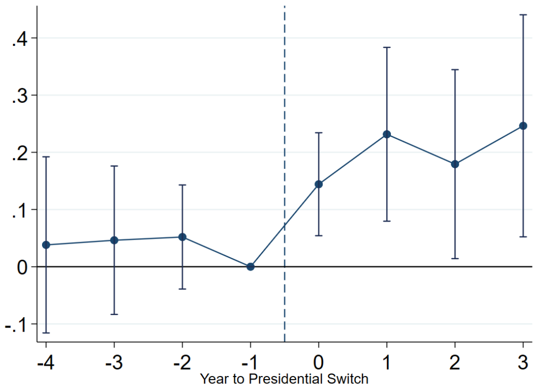


(A) Continuous Clinic Access × Binary Aid Exposure

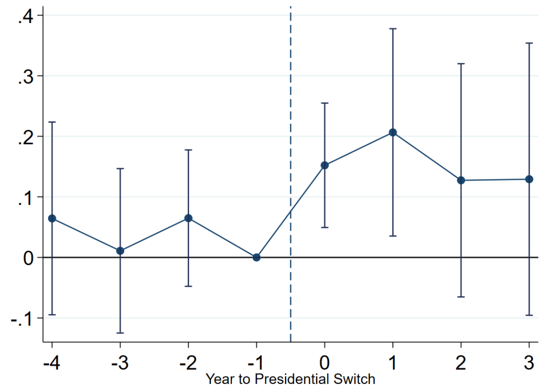


(B) Binary Clinic Access × Continuous Aid Exposure

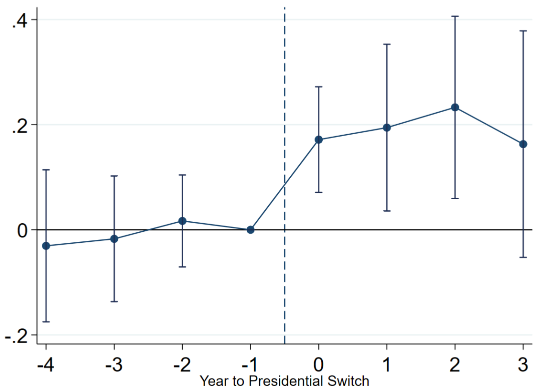
Figure A5: GGR and MMR - Pooled - Alternative Measures of Exposure



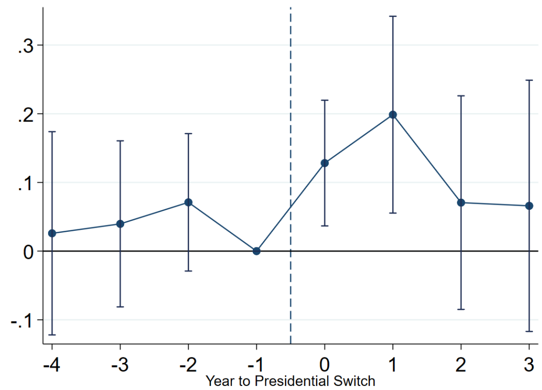
(A) 0.5-by-0.5 (Benchmark)



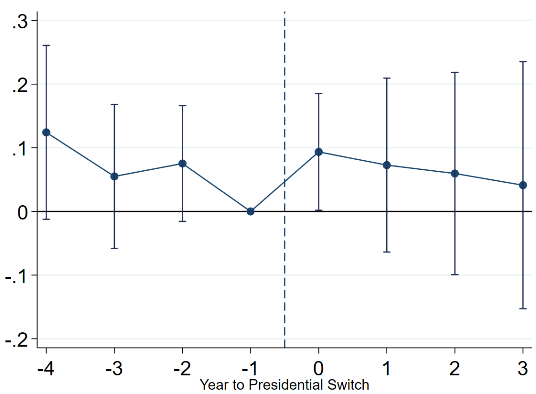
(B) 0.25-by-0.25



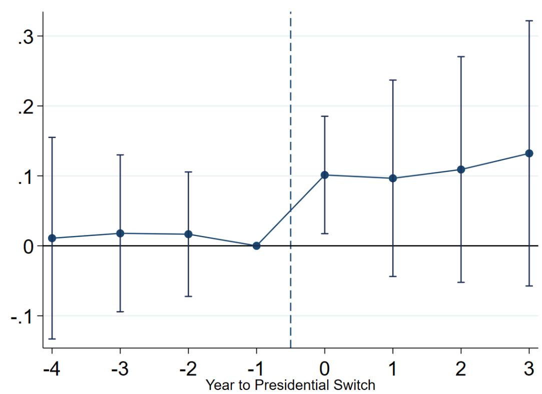
(C) 0.4-by-0.4



(D) 0.6-by-0.6



(E) 0.75-by-0.75



(F) 1.0-by-1.0

Figure A6: GGR and MMR - Pooled - Alternative Grid Sizes

Table A1: GGR and MMR - Alternative Donors

	(1)	(2)	(3)	(4)	(5)
Estimation Sample	Pooled	Pooled	Pooled	Clinton-Bush	Obama-Trump
GGR $\times$ $High^{Grid}$	-0.052	-0.010	-0.066	-0.034	-0.162
	(0.051)	(0.052)	(0.052)	(0.054)	(0.141)
GGR $\times$ $High^{Country}$ $\times$ $High^{Grid}$	0.182**		0.146	0.027	0.341**
	(0.075)		(0.098)	(0.128)	(0.164)
GGR $\times$ $High^{Country}_{oth}$ $\times$ $High^{Grid}$		0.143*	0.078	0.172	0.010
		(0.074)	(0.096)	(0.130)	(0.161)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	6.042	6.042	6.042	10.759	3.821
Median Dep. Var.	3.644	3.644	3.644	9.315	2.147
SD Dep. Var.	6.970	6.970	6.970	7.359	5.527
Observations	9,626	9,626	9,626	5,499	4,127
Num. of Clusters	977	977	977	461	647

Notes: $High^{Country}$ equals 1 for countries with above-median US-sourced family planning aid decline between Obama-Trump and/or Clinton-Bush. $High^{Country}_{oth}$ equals 1 for countries with above-median non-US-sourced family planning aid decline between Obama-Trump and/or Clinton-Bush. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. Standard errors are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: GGR and MMR - Local Economic Controls

	(1)	(2)	(3)	(4)	(5)
Estimation Sample	Obama-Trump				
Gridding Method	Benchmark	Rossi-Hansberg and Zhang (2025)			
GGR $\times$ $High^{Grid}$	-0.157	-0.198	-0.199	-0.196	-0.201
	(0.123)	(0.136)	(0.135)	(0.136)	(0.135)
GGR $\times$ $High^{Country}$ $\times$ $High^{Grid}$	0.340**	0.301*	0.301*	0.300*	0.302*
	(0.167)	(0.172)	(0.172)	(0.172)	(0.172)
Log Population			-0.079		-0.407**
			(0.155)		(0.195)
Log GDP				0.127	0.388**
				(0.132)	(0.168)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	3.821	3.741	3.741	3.741	3.741
Median Dep. Var.	2.147	2.036	2.036	2.036	2.036
SD Dep. Var.	5.527	5.488	5.488	5.488	5.488
Observations	4,127	4,018	4,018	4,018	4,018
Num. of Clusters	647	626	626	626	626

Notes: $High^{Country}$ equals 1 for countries with above-median US-sourced family planning aid decline between Obama-Trump. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. Standard errors are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: GGR and MMR - Spatial Spillovers

	(1)	(2)	(3)	(4)	(5)
Estimation Sample			Pooled		
GGR × $High^{Grid}$	-0.057			-0.050	-0.067
	(0.051)			(0.049)	(0.050)
GGR × $High^{Grid}_{Neighbor}$		-0.175		-0.164	
		(0.133)		(0.130)	
GGR × Num $High^{Grid}_{Neighbor}$			0.005		0.011
			(0.017)		(0.017)
GGR × $High^{Country}$ × $High^{Grid}$	0.188**			0.187**	0.191**
	(0.074)			(0.073)	(0.079)
GGR × $High^{Country}$ × Any $High^{Grid}_{Neighbor}$		0.178		0.104	
		(0.172)		(0.168)	
GGR × $High^{Country}$ × Num $High^{Grid}_{Neighbors}$			0.012		-0.007
			(0.021)		(0.021)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	6.693	6.693	6.693	6.693	6.693
Median Dep. Var.	4.558	4.558	4.558	4.558	4.558
SD Dep. Var.	6.896	6.896	6.896	6.896	6.896
Observations	9,578	9,578	9,578	9,578	9,578
Num. of Clusters	973	973	973	973	973

Notes: $High^{Country}$ equals 1 for countries with above-median US-sourced family planning aid decline between Obama-Trump and/or Clinton-Bush. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. $High^{Grid}_{Neighbor}$ equals 1 for grids that borders any $High^{Grid}$ as per definition of queen contiguity. Standard errors are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: GGR and MMR - By Age Group

	Heterogeneity by Age Group: Pooled Sample			
	(1)	(2)	(3)	(4)
Estimation Sample	15-19	20-29	30-39	40-49
GGR $\times$ $High^{Grid}$	0.106	-0.034	0.007	0.196
	(0.185)	(0.110)	(0.148)	(0.298)
GGR $\times$ $High^{Country}$ $\times$ $High^{Grid}$	0.031	0.131	0.128	0.225
	(0.273)	(0.140)	(0.187)	(0.372)
Country-Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Mean Dep. Var.	8.896	7.837	8.335	10.979
Median Dep. Var.	5.817	4.822	4.018	3.488
SD Dep. Var.	13.066	9.967	12.934	35.586
Observations	5,584	7,669	6,779	3,030
Num. of Clusters	520	750	673	326

Notes: $High^{Country}$ equals 1 for countries with above-median US-sourced family planning aid decline between Obama-Trump. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. Standard errors are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: GGR and MMR - By Woman's Status

	Heterogeneity by Woman Status: Pooled Sample			
	(1)	(2)	(3)	(4)
Estimation Sample	Low Status	High Status	Interaction	Interaction
GGR $\times$ $High^{Grid}$	-0.020	-0.012	-0.052	-0.383
	(0.092)	(0.090)	(0.095)	(0.294)
GGR $\times$ $High^{Country}$ $\times$ $High^{Grid}$	-0.007	0.206*	0.050	0.567*
	(0.122)	(0.116)	(0.125)	(0.316)
GGR $\times$ $High^{Country}$ $\times$ $High^{Grid}$ $\times$ High Status			0.218	
			(0.168)	
GGR $\times$ $High^{Country}$ $\times$ $High^{Grid}$ $\times$ Status				-0.558
				(0.401)
Country-Year FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Mean Dep. Var.	6.029	4.971	5.434	5.434
Median Dep. Var.	3.488	2.847	3.181	3.181
SD Dep. Var.	7.035	6.070	6.530	6.530
Observations	3,743	3,744	7,487	7,487
Num. of Clusters	444	448	870	870

Notes: $High^{Country}$ equals 1 for countries with above-median US-sourced family planning aid decline between Obama-Trump. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. Standard errors are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: GGR and DHS Calendar Outcomes

	DHS Calendar Outcomes (Quarterly): Obama-Trump			
	(1)	(2)	(3)	(4)
Dep. Var.	Conceptions	Live Births	Preg. Terminations	Contraceptive Use
GGR × $High^{Grid}$	0.011	0.008	0.007	-0.162***
	(0.025)	(0.024)	(0.121)	(0.026)
GGR × $High^{Country}$ × $High^{Grid}$	-0.063*	0.014	0.195	-0.145**
	(0.034)	(0.036)	(0.150)	(0.060)
Country-Year-Quarter FE	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes
Mean Dep. Var.	146.855	139.035	11.926	968.777
Median Dep. Var.	141.898	134.074	10.626	734.595
SD Dep. Var.	58.520	52.396	10.469	749.883
Observations	22380	22391	20592	21997
Num. of Clusters	1300	1303	1097	1275

Notes: $High^{Country}$ equals 1 for countries with above-median family planning aid decline between Obama-Trump and/or Clinton-Bush. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. Standard errors are clustered at the grid level. Dependent variables in columns 1 and 3 are log-transformed, and those in columns 2 and 4 are inverse-hyperbolic-sine-transformed. SRB stands for sex ratio at birth, calculated by dividing the number of male births by the number of female births and then multiplying by 100.

Table A7: GGR and Infant and Child Mortality

	(1)	(2)	(3)	(4)	(5)
Estimation Sample	Pooled	Pooled	Pooled	Clinton-Bush	Obama-Trump
Panel A: Log Num of Live Births (TWFE)					
GGR × $High^{Grid}$	0.018	0.084	0.059*	0.049	0.065
	(0.021)	(0.051)	(0.031)	(0.038)	(0.051)
GGR × $High^{Country}$ × $High^{Grid}$	0.027		0.048	-0.023	0.094
	(0.051)		(0.057)	(0.078)	(0.070)
GGR × $High^{Country}_{nch}$ × $High^{Grid}$		-0.102*	-0.111*	-0.064	-0.110
		(0.055)	(0.062)	(0.076)	(0.078)
R^2	0.97	0.97	0.97	0.98	0.96
Mean Dep. Var.	4.069	4.069	4.069	4.589	3.779
Median Dep. Var.	4.143	4.143	4.143	4.635	3.871
SD Dep. Var.	1.401	1.401	1.401	1.286	1.379
Observations	14407	14407	14407	7356	7051
Num. of Clusters	1480	1480	1480	640	1122
Panel B: Infant Mortality Rate (PPML)					
GGR × $High^{Grid}$	0.140*	-0.048	0.096	0.114	0.011
	(0.074)	(0.052)	(0.075)	(0.085)	(0.149)
GGR × $High^{Country}$ × $High^{Grid}$	-0.224***		-0.279***	-0.188*	-0.335**
	(0.082)		(0.087)	(0.106)	(0.143)
GGR × $High^{Country}_{nch}$ × $High^{Grid}$		0.078	0.161**	0.050	0.280**
		(0.070)	(0.073)	(0.075)	(0.137)
Mean Dep. Var.	63.398	63.398	63.398	87.776	49.052
Median Dep. Var.	52.421	52.421	52.421	79.928	38.245
SD Dep. Var.	58.427	58.427	58.427	57.103	54.304
Observations	13,154	13,154	13,154	7,120	6,034
Num. of Clusters	1,285	1,285	1,285	611	939
Panel C: Under-5 Mortality Rate (PPML)					
GGR × $High^{Grid}$	0.156**	-0.008	0.127*	0.136*	0.050
	(0.064)	(0.042)	(0.065)	(0.074)	(0.141)
GGR × $High^{Country}$ × $High^{Grid}$	-0.218***		-0.259***	-0.194**	-0.289**
	(0.070)		(0.073)	(0.086)	(0.133)
GGR × $High^{Country}_{nch}$ × $High^{Grid}$		0.033	0.114*	0.028	0.230*
		(0.059)	(0.059)	(0.058)	(0.126)
Mean Dep. Var.	86.326	86.326	86.326	128.383	61.744
Median Dep. Var.	71.196	71.196	71.196	120.360	47.527
SD Dep. Var.	74.172	74.172	74.172	73.492	62.634
Observations	13,447	13,447	13,447	7,196	6,251
Num. of Clusters	1,318	1,318	1,318	619	972
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes

Notes: $High^{Country}$ equals 1 for countries with above-median family planning aid decline between Obama-Trump and/or Clinton-Bush. $High^{Country}_{nch}$ equals 1 for countries with above-median neonatal and child health aid "decline" between Obama-Trump and/or Clinton-Bush. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. The specification used is two-way fixed-effect in Panel A and Poisson maximum likelihood in Panels B and C. Standard errors are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8: GGR and Intimate Partner Violence

GGR and IPV Exposure in Last 12 Months					
	(1)	(2)	(3)	(4)	(5)
Dep. Var.	Physical	Emotional	Sexual	Any IPV	Any IPV
GGR × $High^{Grid}$	0.051	0.081	-0.034	0.066	0.036
	(0.110)	(0.106)	(0.164)	(0.087)	(0.088)
GGR × $High^{Country}$ × $High^{Grid}$	-0.398**	-0.512***	-0.247	-0.428***	-0.408***
	(0.157)	(0.157)	(0.235)	(0.132)	(0.132)
Pregnancy last 12 months					0.790**
					(0.394)
Desired fertility aligns					-0.033
					(0.170)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	120.872	140.263	58.459	196.241	196.241
Median Dep. Var.	112.995	124.908	42.639	185.009	185.009
SD Dep. Var.	82.435	104.850	65.116	125.193	125.193
Observations	822	830	744	832	832
Num. of Clusters	411	415	372	416	416

Notes: $High^{Country}$ equals 1 for countries with above-median family planning aid decline between Obama-Trump and/or Clinton-Bush. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. The dependent variables are reported exposure to sexual abuse, emotional abuse, physical abuse, or any of the three in the last 12 months. Standard errors are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9: GGR and Female Empowerment

	(1)	(2)	(3)	(4)	(5)
	Woman has sole or joint decision power over				
Dep. Var.	HH expenditures	Visiting family	Woman's earnings	Woman's healthcare	Composite Index
GGR × $High^{Grid}$	-0.065***	-0.018	-0.032*	-0.042***	-0.031**
	(0.016)	(0.018)	(0.018)	(0.016)	(0.013)
GGR × $High^{Country}$ × $High^{Grid}$	0.076***	0.026	0.045**	0.094***	0.050***
	(0.024)	(0.027)	(0.022)	(0.024)	(0.018)
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes
R^2	0.89	0.84	0.63	0.89	0.90
Mean Dep. Var.	0.480	0.563	0.858	0.539	0.604
SD Dep. Var.	0.269	0.260	0.164	0.275	0.253
Observations	3,172	3,172	3,578	3,172	3,692
Num. of Clusters	1,257	1,257	1,384	1,257	1,433

Notes: $High^{Country}$ equals 1 for countries with above-median family planning aid decline between Obama-Trump and/or Clinton-Bush. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. Standard errors are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10: GGR and Other Mortality Measures

	Other Female Mortality			Male Mortality		
	(1)	(2)	(3)	(4)	(5)	(6)
Estimation Sample	Pooled	Clinton-Bush	Obama-Trump	Pooled	Clinton-Bush	Obama-Trump
GGR × $High^{Grid}$	-0.004	-0.016	0.029	-0.028	-0.020	-0.048
	(0.029)	(0.033)	(0.061)	(0.025)	(0.028)	(0.050)
GGR × $High^{Country}$ × $High^{Grid}$	0.044	0.062	-0.012	0.041	0.041	0.036
	(0.040)	(0.045)	(0.088)	(0.033)	(0.037)	(0.071)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Grid FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	20.416	40.867	12.227	28.302	55.026	17.740
Median Dep. Var.	12.665	31.250	8.705	18.250	46.586	13.051
SD Dep. Var.	21.904	26.731	12.301	26.702	30.401	15.356
Observations	14,015	6,767	7,248	14,761	7,008	7,753
Num. of Clusters	1,507	569	1,140	1,600	596	1,220

Notes: $High^{Country}$ equals 1 for countries with above-median family planning aid decline between Obama-Trump and/or Clinton-Bush. GGR equals 1 for years when GGR is switched on. $High^{Grid}$ equals 1 for grids within a given country with above-median baseline clinic access. Standard errors are clustered at the grid level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B Data Appendix

B.1 Additional Details on Data Generation

Table B1 provides a summary of all DHS survey waves used in our analysis, including both the sibling (MMR) modules and reproductive calendar modules, as well as the corresponding sample sizes.

Table B1: DHS Data Summary

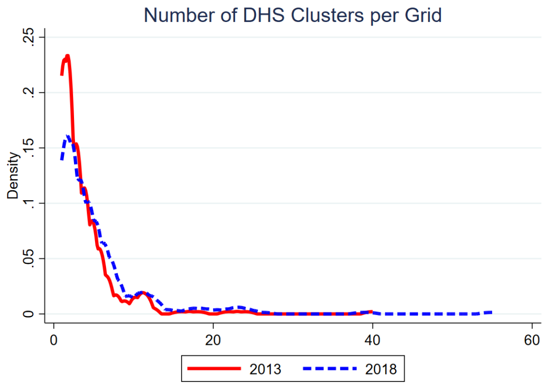
DHS Country	Survey Wave	MMR Module	Num. of Sisters	Reproductive Calendar	Num. of Women
Benin	1996	✓	8,743		
	2017	✓	21,428	✓	15,326
Cameroon	2011	✓	32,494		
	2018	✓	26,048		
Cote d'Ivoire	1994	✓	14,052		
	2012	✓	20,763		
	2021	✓	26,566		
Gabon	2012	✓	15,994		
	2019	✓	16,883		
Guinea	1999	✓	9,976		
	2012	✓	15,448		
Lesotho	2009	✓	8,896		
	2014	✓	9,359		
	2023	✓	8,514		
Liberia	2013	✓	16,680	✓	9,239
	2019	✓	14,776	✓	7,961
Madagascar	1997	✓	15,307		
	2008	✓	37,292		
Malawi	2000	✓	24,190	✓	13,150
	2004	✓	15,846	✓	11,514
	2010	✓	44,710	✓	22,480
	2015	✓	46,946	✓	24,500
Mali	1996	✓	15,418		
	2001	✓	21,774	✓	12,750
	2006	✓	25,940	✓	14,419
	2012	✓	16,169	✓	10,407
	2018	✓	18,356	✓	9,984
Mozambique	2011	✓	23,081		
	2022	✓	19,776		
Namibia	2000	✓	13,887		
	2013	✓	18,506		
Nigeria	2013	✓	70,110	✓	38,533
	2018	✓	78,537	✓	41,571

Rwanda	2005	✓	24,240	✓	11,158
	2010	✓	29,822	✓	13,645
	2014	✓	28,658	✓	13,497
	2019	✓	30,314		
Senegal	1993	✓	6,976		
	2005	✓	28,054		
	2010	✓	29,417		
	2012			✓	13,819
	2015			✓	8,793
	2016			✓	8,865
	2017	✓	35,559	✓	16,787
	2023	✓	29,697		
Sierra Leone	2013	✓	23,877	✓	16,631
	2019	✓	24,453	✓	14,990
Uganda	2000			✓	6,404
	2006			✓	7,693
	2011			✓	8,582
	2016			✓	18,231
Tanzania	2010	✓	21,395		
	2015	✓	28,793		
	2022	✓	31,339		
Togo	1998	✓	15,855		
	2013	✓	19,211		
Zambia	2007	✓	15,492		
	2013	✓	36,091	✓	16,334
	2018	✓	27,536	✓	13,421
Zimbabwe	1999			✓	5,721
	2005			✓	8,816
	2010			✓	8,859
	2015			✓	9,885

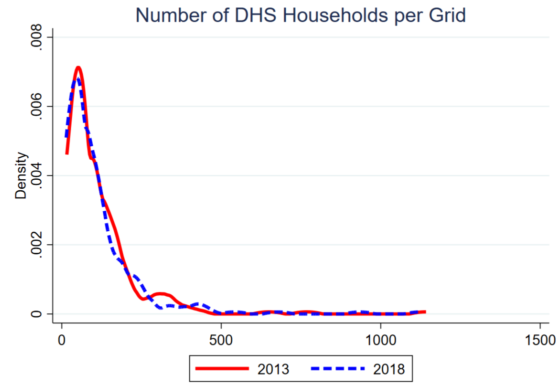
Source: Authors' own calculation from the DHS. Sample is restricted to women who gave birth during the period of interest (1993-2020).

B.2 Descriptive Statistics and Figures

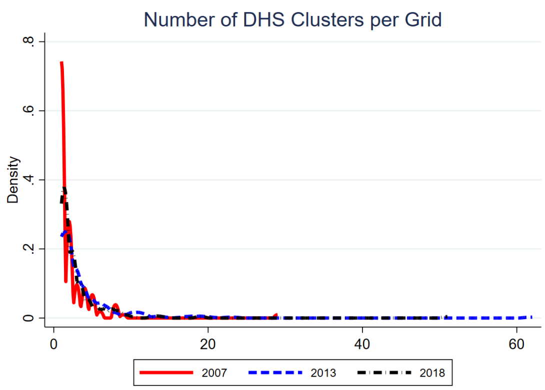
Distribution of clusters/households per grid. The DHS Program uses a cluster sampling method, where clusters (groups of households) are selected as a first step in a stratified sampling process. Most DHS surveys use a fixed sample of households per cluster of about 25-30 households, determining the number of clusters to be selected. Figure B1 plots the kernel density of the number of DHS clusters or households per 0.5-by-0.5 grid cell, for example, countries Nigeria and Zambia over their respective DHS survey waves. The figures illustrate that the distributions are relatively stable over survey waves, boosting credibility to the pseudo-panel we have constructed at the grid level.



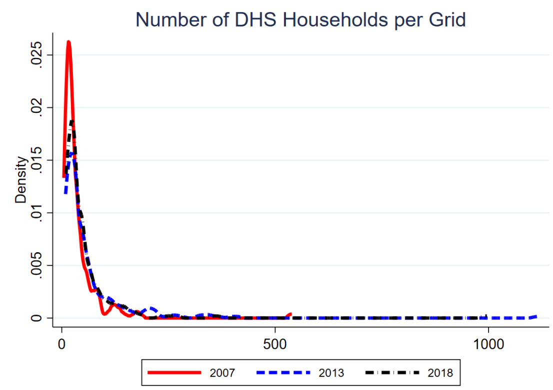
(A) Nigeria



(B) Nigeria



(C) Zambia



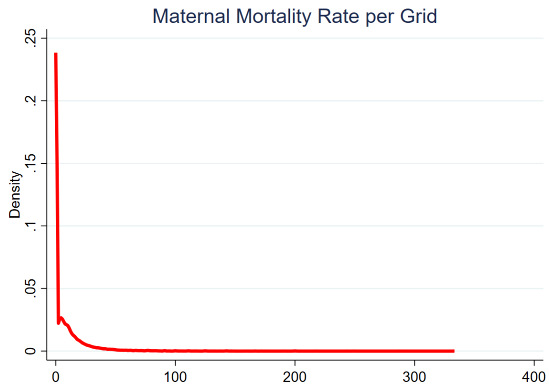
(D) Zambia

Figure B1: Number of DHS Clusters/Households per Grid

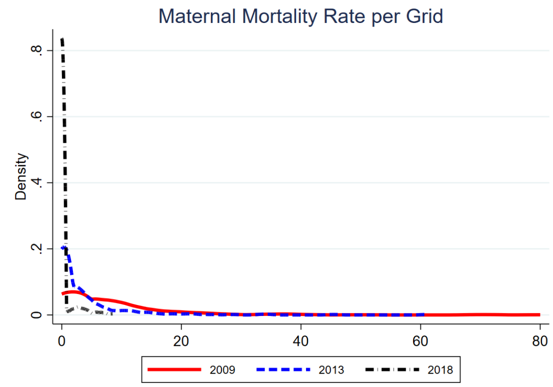
Distribution of maternal mortality across grids. Figure B2 plots the kernel density of maternal mortality rate per 0.5-by-0.5 grid cell, for the pooled sample of all country-year observations (Panel A) and for specific exposure years in Nigeria (Panel B). The figures illustrate the significant presence of zero observations, suggesting that maternal mortality is a rare event. Moreover, the share of zero observations increases over time (Panel B).

Spatial distribution of clinic access and maternal mortality. Figure B3 illustrates the spatial distribution of some key outcome measures across 0.5-by-0.5 grid cells. Panels A and B document changes over time in the constructed clinic access index at the grid level, using Nigeria as an example. Panels C and D repeat for the maternal mortality rate, our core outcome of interest.

Spatial clustering of high and low-access grids. Figure B4 illustrates the spatial clustering pattern across grids that we observe in some example DHS survey waves. In particular, the black (white) grids are those with high (low) baseline clinic access, while the gray grids are the ones with missing information (no households located).

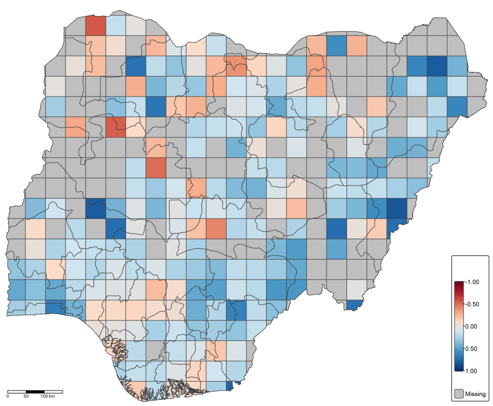


(A) Pooled Sample

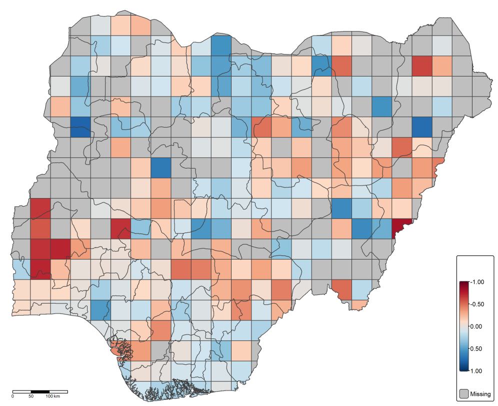


(B) Nigeria

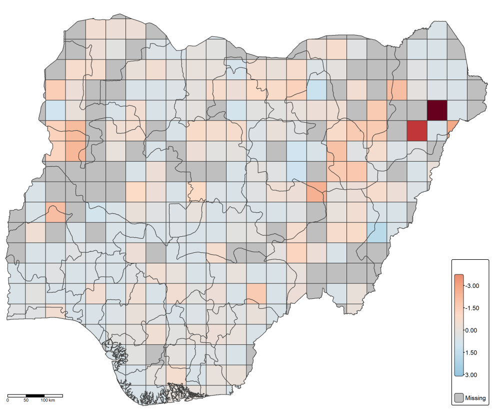
Figure B2: Maternal Mortality Rate per Grid



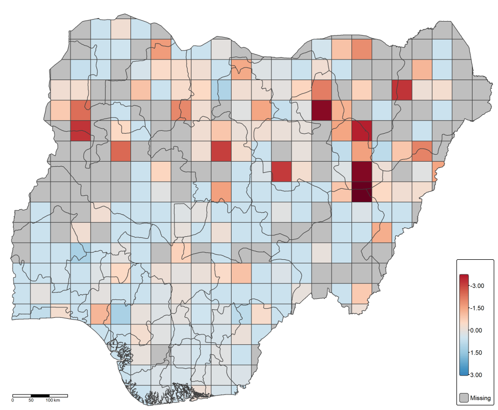
(A) Clinic Access Δ 2013-2009



(B) Clinic Access Δ 2018-2013



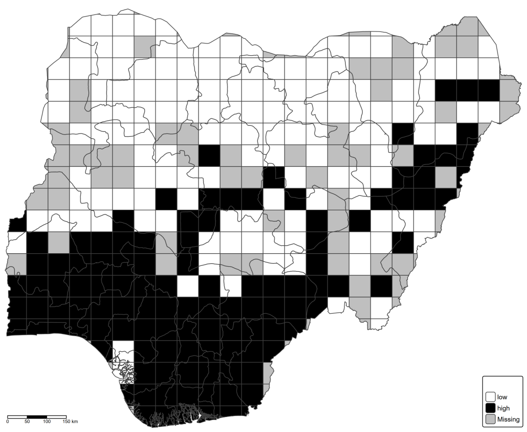
(C) MMR Δ 2013-2009



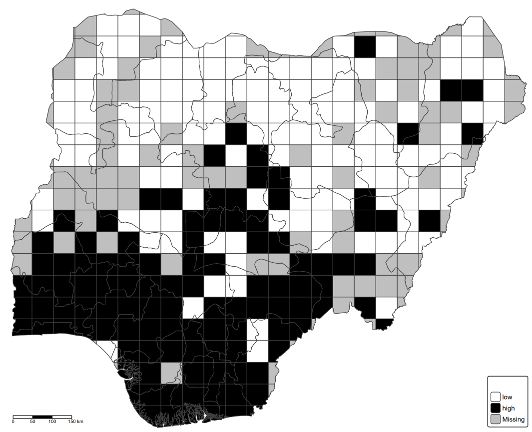
(D) MMR Δ 2018-2013

Figure B3: Changes in Clinic Access & MMR in Nigeria

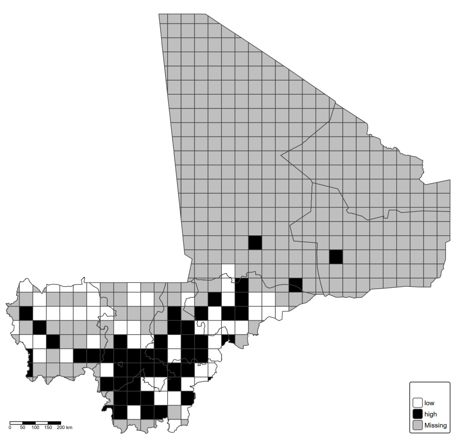
Notes: This figure plots the changes in the distribution of our measures of clinic access and maternal mortality rates across 0.5-by-0.5 degree grid cells in Nigeria over three DHS survey waves. *Source:* Authors' calculations from DHS data.



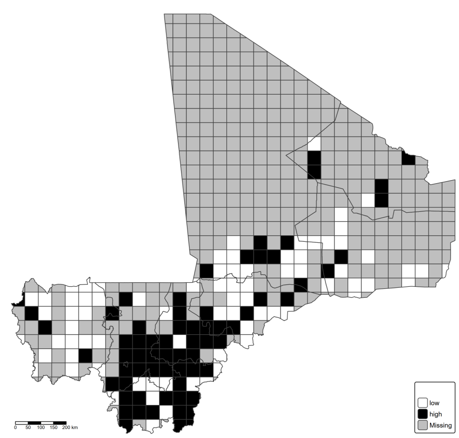
(A) Nigeria - 2008



(B) Nigeria - 2013



(C) Mali - 1996



(D) Mali - 2006

Figure B4: Spatial Clustering of Grids

B.3 Measuring Clinic Access from the DHS

This Appendix section elaborates on how we construct grid-level clinic access from survey questions in the IPUMS-DHS database. The variables selected to proxy for local clinic access are listed as follows. All variables are binary and harmonized to reflect that 1 stands for "better" clinic access.

- *fpynotany*: (for women who are not currently using a method of contraception and are at risk of pregnancy) reason not using family planning: knows no source or lack of access
- *hcmpub*: woman-reported source of male condoms: public[33]
- *hcmpri*: woman-reported source of male condoms: private
- *hcmngo*: woman-reported source of male condoms: NGO
- *hcmcom*: woman-reported source of male condoms: commercial or informal sources
- *hcmknow*: woman-reported source of male condoms: knows ANY source
- *bhcdistance*: woman-reported barrier to health care: distance to facility
- *bhctaketran*: woman-reported barrier to health care: having to take transport

Based on these survey questions, we created two composite indices for baseline clinic access:

- *clinic_access_fp*: Access to family planning- z-score composite index calculated from *hcmpri*, *hcmngo*, *hcmknow*, and *fpynotany*. Standardization is done conditioning on country and survey year.
- *clinic_access_hc*: Access to general health care- 1 indicates no barrier to health care facilities, either due to distance or transport

In our preferred specification, we use *clinic_access_fp* as a proxy for baseline clinic access at the grid level.

Table B2 presents summary statistics of these variables from the entire IPUMS-DHS sample and from the DHS sample (once imposing the restriction that a DHS survey is conducted during both the Republican regime and the preceding Democratic regime), respectively.

[33] We aggregate public hospitals, public health centers, family planning clinics, public field workers, and other public into "public", as opposed to private hospitals or private clinics as in "private" or shops, churches, and family or relatives as "informal".

Table B2: Summary Statistics of Clinic Access Variables

Variable Name	Mean	SD	Min	Max	Observations	Num. of Countries
Panel A: All IPUMS-DHS Countries						
my_fpynotany	0.97	0.17	0.00	1.00	696,169	46
my_hcmpub	0.33	0.47	0.00	1.00	1,421,341	40
my_hcmpriv	0.27	0.44	0.00	1.00	1,429,083	40
my_hcmngo	0.00	0.05	0.00	1.00	2,570,320	50
my_hcmcom	0.18	0.39	0.00	1.00	1,400,341	39
my_hcmknow	0.48	0.50	0.00	1.00	1,426,048	40
my_bhcdistance	0.33	0.47	0.00	1.00	2,980,742	46
my_bhctaketran	0.30	0.46	0.00	1.00	2,198,491	32
clinic_access_fp	0.00	0.87	-48.69	4.00	1,816,292	47
clinic_access_hc	0.63	0.48	0.00	1.00	2,980,990	46
Panel B: Restricted DHS Sample						
my_fpynotany	0.96	0.20	0.00	1.00	267,688	22
my_hcmpub	0.38	0.48	0.00	1.00	718,582	22
my_hcmpriv	0.24	0.42	0.00	1.00	718,514	22
my_hcmngo	0.00	0.02	0.00	1.00	1,125,158	22
my_hcmcom	0.24	0.43	0.00	1.00	718,582	22
my_hcmknow	0.51	0.50	0.00	1.00	718,514	22
my_bhcdistance	0.37	0.48	0.00	1.00	840,871	22
my_bhctaketran	0.39	0.49	0.00	1.00	384,365	19
clinic_access_fp	-0.01	0.84	-13.58	3.36	810,851	22
clinic_access_hc	0.60	0.49	0.00	1.00	841,080	22

Notes: Observations refer to the sample where the question is relevant, for instance, *fpynotany* is asked for women who reported that they need family planning but are not currently using any family planning method.