Impacts of the Global Gag Rule: Evidence from Ghana

A thesis submitted by

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Abstract

The Mexico City Policy is a restriction on family planning aid to developing countries, blocking U.S. funds to NGOs that provide abortion-related services. This policy, first announced in 1984, has since been repealed during Democratic administrations and reinstated during Republican ones. Comparing Ghanaian women who are more or less exposed to the policy when the policy is effect and not, I use a difference-in-difference analysis to identify the relative impact of the policy. I find that rural women are differentially impacted by the policy in terms of some fertility outcomes, but no significant effect is seen on maternal or child mortality. From a policy perspective, the fertility results join prior studies of the policy's potential unintended consequences, while other findings may simply highlight the difficulty of isolating differential exposure and emphasize the need for further study.

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1 Introduction

Population assistance refers to funding to support provision of contraception and family planning in foreign countries, and the United States continues to be one of the largest donors of international population assistance (UNFPA 2004). However, a number of restrictions have been placed on population assistance funding, including the Helms Amendment (1973), the Biden Amendment (1981), the Siljander Amendment (1981), and the Kemp-Kasten Amendment (1985). Arguably the most well-known of these provisions is the Mexico City Policy, which was introduced by President Ronald Reagan at the 1984 International Conference on Population in Mexico City. Under the terms of the policy, foreign nongovernmental organizations (NGOs) must sign official affidavits agreeing not perform, lobby for, or educate clients about abortion as a means of family planning, or lose any population assistance they received from the United States through the United States Agency for International Development (USAID). The stated aim of the policy is to reduce the use of abortions as a means of family planning.

Efforts to investigate the effect of the policy on maternal and child outcomes in developing countries must overcome two major obstacles (Jones, 2011). First, data on abortions in developing countries, either at the individual or aggregate level, are not very common. The second problem is the difficulty in creating a reasonable comparison group. However, Bendavid, Avila and Miller (2011) and Jones (2011) use DHS data to show that abortion rates go up during policy years. They hypothesize that the policy reduces funding for other family planning services such as contraceptives, leading to an increase in unintended pregnancies and thus an increase in the demand for abortion. Also using DHS data, this paper builds off their work by using increased abortions and reduced contraceptive access as the mechanism through which the policy operates, and estimating the consequent effect on fertility and parity, as well as child mortality.

It is difficult to estimate exactly how much funding was lost due to the effect of the policy. As of 2017, all approved funding that remained unused because of the policy was used to fund other family planning-related activities (Blanchfield, 2017). Therefore, total funding provided through USAID did not necessarily decrease. In fact, total funding levels may have remained constant or even increased over time, but the distribution of funding changed, with funding flows from non-compliant to compliant NGOs. Anecdotal evidence suggests that rural outreach services were reduced and reproductive health clinics and shut down due to funding losses, meaning that rural women could be disproportionately affected by the policy. However, any effect that the policy has could theoretically by isolated by looking at variation in exposure, such as place of residence (Jones, 2011) at the individual level or dependence on U.S. funding (Bendavid et al, 2011) at the country level. Other forms of variation could include education and wealth. A more educated woman might be less exposed to the policy because her behaviour is less likely to change as a result of the policy, and her knowledge of alternative sources of birth control. Similarly, a wealthier woman could be less affected because she has the resources to obtain contraceptives even if she lives in a low-access area where NGOs, due to funding constraints, have reduced their supply of birth control.

Using the 2014 DHS survey in Ghana, I create a woman-year panel with 9,396 women, who I classify as more exposed to the policy if they live in rural areas or have below median wealth or education. The women in my dataset experienced 1.8 policy changes on average during their childbearing years. I employ a difference-in-difference framework to compare women in policy and non-policy periods, with time-varying controls as well as time and individual fixed effects. My coefficient of interest is the interaction term between the exposure indicator (1 if the woman is classified as more exposed to the policy, 0 otherwise) and the policy indicator, which takes the value 1 in policy years. I test the hypothesis that the coefficient on this variable is positive.

My results indicate that the policy increased fertility for women in rural areas compared to urban areas, and that women who were less educated were more likely to be impacted. For urban women, any effect was driven by wealth, i.e. less wealthy women had higher fertility, but not education. Child mortality, however, does not appear to have been impacted by the imposition of the policy. I also find small impacts on age at first birth.

I also analyze the impact of the policy on maternal mortality by creating a similar woman-year panel from the 2007 DHS survey conducted in Ghana. I utilize data from this round that asked respondents about the status (living or not) of their siblings, and included information about the cause of death. I find no significant impact on maternal mortality.

My lack of results for a number of outcomes that the policy could theoretically affect may imply that these exposure mechanisms are not the most appropriate ones for isolating policy impacts. Further analysis is limited by the nature and scope of the DHS surveys, but future studies could deal with other forms of exposure by combining this dataset with others providing more time-varying information by using geospatial data.

The remainder of this manuscript is organized as follows. Next, I introduce the Mexico City Policy before reviewing the relevant literature. Section 3 and 4 cover the data I use for my analysis and my empirical strategy respectively. My results are shown in Section 5, and supplementary analysis and robustness checks are covered in Section 6. Section 7 concludes.

2 Background and Literature Review

Population assistance is defined as funding to support provision of contraception and family planning in foreign countries. Schindlmayr (2004) divides donor trends for population assistance over time into distinct phases, with the first beginning in the mid-1960s, followed by the 'population panic' of the 1960s and 1970s. The third began with the Bucharest population conference, while the fourth started with the Mexico City Policy of 1984. The fifth covered the 1990s, and a possible sixth extends to the present day.

The population explosion panic, fuelled by dire warnings about food shortages and other resource scarcities, led to a large increase in population assistance, which died down in the early 1980s. Population assistance continues to be an important part of aid; in 2014, bilateral donor spending on family planning programs was valued at $1.4 billion (Kaiser Family Foundation, 2014), of which nearly half came from the United States. In 1995, the definition of population assistance was changed to include reproductive health. Arguably, this marked a shift in focus from reducing population growth to emphasizing the importance of birth spacing in order to promote the quality of life, with education and access to family planning services being the key for success. A more controversial aspect of family planning is abortion access.

In the wake of Roe v. Wade (1973), many in the U.S. aid establishment were concerned that increasing access to abortion would result in its use as a substitute for birth control. Outside the aid sector, this eventually led to the formation of groups that today form the pro-life movement. These groups not only supported anti-abortion policies domestically, but have also impacted population assistance and family planning aid provided to the developing world through USAID. The Helms Amendment (1973) was among the first of these, restricting the use of U.S. funds for abortion or abortion-related activities, stating 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." It was followed by the Biden Amendment (1981), which prohibited the use of U.S. funds for "biomedical research related to methods of or the performance of abortion as a means of family planning," and the Siljander Amendment (1981), which was intended to keep

NGOs from lobbying for or against abortion (critics have pointed out that lobbying for abortion may be more strictly regulated than lobbying against it). These measures were followed in 1984 by the Mexico City Policy. This section introduces and describes the Mexico City Policy, before turning to a discussion of the relevant literature that this paper contributes to, including but not limited to existing analyses of the policy.

2.1 What is the Mexico City Policy?

The Mexico City Policy places a number of restrictions on NGOs receiving family planning and reproductive health funding through USAID (White House Office of Policy Development, 1984). It disqualifies NGOs from receiving aid if they provide or promote abortion services, referrals, counseling, advice or information to patients in countries where it is legal, or if they advocate for legalization of abortion in countries where it is illegal. Recipient organizations are also barred from conducting public information campaigns about abortion as a means of family planning.

These NGOs can, however, provide the aforementioned abortion-related services in cases where the pregnancy risks the life of the mother, or is the result of incest or rape. In cases where a pregnant woman informs NGO staff that she has already decided to have an abortion and asks where a safe, legal abortion may be obtained, they may respond with the requested information. The indication is that actively providing medically relevant information is discouraged, but passively doing so is tolerated.

The terms of the policy apply only to NGOs and not to governments. It applies to international and regional NGOs based outside the U.S. and local NGOs in countries that receive U.S. assistance. While NGOs operating in the U.S. are not affected by the terms of the policy, they are required to ensure that they do not support non-compliant foreign NGOs. Prior to the enactment of the policy, foreign NGOs could use funds from other (non-USAID) sources for abortion-related purposes, as long as they maintained separate accounts for aid from the U.S. and funds from these other sources.

Under the original terms of the policy, NGOs could choose either to accept the policy or refuse; the latter would result in losing family planning funding from the United States. It should be noted that this does not necessarily decrease the total amount of family planning and reproductive health aid disbursed by USAID, as funding may be shifted from less compliant to more compliant NGOs (Camp, 1987).

2.2 Timeline of the Mexico City Policy

Figure 1 shows a simple historical timeline of the Mexico City Policy. After Reagan's initial introduction of the policy, it took effect in 1985, and stayed in place for the duration of his second term as well as the Bush Sr. (Republican) presidency (1989-1993). The policy was rescinded by President Clinton (Democrat) at the beginning of his term. Up till this point, policy changes had been brought about exclusively by executive action. However, congressional action restored the policy for a year of the Clinton presidency, between October 1999 and September 2000. The full impact of the policy was not felt as the then President was able to waive its conditions during this period.

President Bush (Jr.) reintroduced the policy in 2001, but modified its terms to exempt NGOs that provided HIV/AIDs services. President Obama (Democrat) removed it in 2009, and President Trump (Republican) reinstituted it in 2017. The latter also modified the terms of the policy, applying it not just to family planning aid but to the "vast majority of U.S. bilateral global health assistance," including but not limited to HIV/AIDS and malaria (Kaiser Family Foundation MCP explainer, 2017). Family planning aid accounts for a small portion of the bilateral global health assistance provided by the U.S., meaning that this change drastically expands the scope of the policy. For example, in 2017, the U.S. contributed \$600 million in family planning assistance, about 7.5% of bilateral global health assistance disbursed that year (\$8 billion).

2.3 Consequences of the Policy

From the time that the policy was first introduced, with the intended aim of reducing the incidence of abortions in developing countries, opponents have argued that it could have unintended consequences. Non-compliant organizations could face reduced budgets, and thus have to reduce the services they provide. Two of the largest family planning providers internationally, the International Planned Parenthood Federation and Maria Stopes International, were among the first NGOs to refuse to comply with its terms, and consequently became ineligible for funding. Despite the fact that total funding levels might not be reduced, the reallocation from non-compliant to compliant organizations could lead to flows away from low-access areas, especially if NGOs operating in low-access areas are less likely to comply. Even if NGOs in low-access and high-access areas are equally likly to comply, women in low-access areas could still lose health services if non-complying organizations are their only source of family planning services; urban women, for example, might have more options in terms of sources of contraceptive access than rural women.

Even compliant organizations could be faced with new costs, including new administrative burdens related to monitoring the activities of their sub-grantees to ensure that they are also compliant, resulting in higher travel costs, increased time in the field, training, new systems and legal fees (Camp, 1987). Compliant organizations, in the years following the first introduction of the policy, were also less likely to fund new family planning projects in countries where abortion is legal for a number of reasons. A related issue is that many family planning and health care providers are funded by the government to provide abortion or counseling services (Camp, 1987). There are also indications that clinics were forced to close (Turnbull and Bogecho, 2003).

2.4 Contribution to the Literature

This thesis contributes to the existing literature on the empirical impact of the Mexico City Policy and is inspired by Bendavid et al (2011) and Jones (2011). These two papers are reviewed in the first section of my literature review. My thesis also contributes to two broader literatures, the first being family planning studies in developing countries. The second is the large literature related to the impact of global health aid. Even more broadly, this paper deals with contraception and fertility, a literature defined by the seminal work of Bailey (2006, 2010), among others. Each of these literatures are reviewed in brief.

2.5 Evaluating the Mexico City Policy

Bendavid, Avila and Miller (2011) used a linear regression with country and year fixed effects to find the association between a country's exposure to the Mexico City Policy and the odds of induced abortion among women of reproductive age, where exposure was defined as a country's reliance on U.S. aid; high exposure means that a country is above the median for aid received in non-policy years. They find that the annual abortion rate (number of abortions per 10000 woman-years) rose as a result of the 2001 enactment of the policy, and that this rise was driven by women in high-exposure countries. This heterogeneity was linked to another finding: that increases in contraceptive use slowed in high-exposure countries, while continuing at a stable rate in low-exposure countries. They hypothesized that the policy reduced contraceptive access in high-exposure countries, driving up unintended pregnancies and therefore abortions. A similar argument is made by Jones (2011).

Jones (2011) estimates the effect of imposing or removing the Mexico City Policy on the use of abortion as a method of birth control in Ghana. Data was collected from the 2007 DHS survey conducted in Ghana, one of a handful of surveys that collected information on pregnancy histories instead of birth histories, and thus contains explicit data on how and when each pregnancy ended, whether by live birth, miscarriage, induced abortion, etc. The key outcome variable was an indicator for whether a pregnancy ended in abortion, with a policy dummy as the key explanatory variable that turns on and off over the woman-year panel, and other controls such as quadratic functions of age and parity (previous number of births). Jones estimates the policy led to a 12% increase in rural pregnancies, resulting in 200,000 additional abortions and 500,000 to 750,000 additional unintended births between 1972 and 2007. The children classified by Jones as resulting from unintended pregnancies were found to have reduced weight and height for their age, as compared to their siblings. My analysis builds on the setup used by Jones by expanding the study into a difference-in-difference framework, with less stringest assumptions.

2.6 Access to Abortion and Contraception

To clarify the mechanisms theorized by Jones (2011) and Bendavid et al (2014), I turn to a broader literature on the impact of increasing access to abortion and contraception on women. Abortion access is associated with a decline in fertility (Levine et al, 2011), but restrictions on abortion are associated with declines in in-wedlock births but had no effect on out-of-wedlock births (Kane and Staiger, 1996). This suggests not only that abortion restrictions are only effective for married couples, but also that single mothers are no less likely to give birth under these circumstances. Guldi (2008) finds evidence that access to abortion led to a drop in young women's birth rates, and also that access to the birth control pill reduced birth rates for white women. Miller and Valente (2016) finds robust evidence of substitution between modern contraception and abortion. The implications of this is that expanding the supply of modern contraceptives may be an effective strategy to reduce the incidence of expensive and possibly unsafe abortions around the world.

In the broader literature on contraception, the advent of oral contraceptives has been associated with significant changes in the economic and social status of women in developed countries, and is credited with giving women increased control over their fertility, allowing them to make career decisions not possible before the introduction of the pill, and possibly increasing the age at first marriage by removing uncertainties related to pregnancy. Bailey (2010) suggests that at least 40% of the total change in the U.S. marital fertility rate between 1955 and 1965 can be attributed to the pill. Bailey (2006) indicates that legal access to the pill for women before the age of 21 reduced the probability of having a first birth before the age of 22, raised the number of women in the paid labor force and increased the annual number of hours worked.

2.7 Abortion and Contraceptive Use in Ghana and Sub-Saharan Africa

I now turn to access to abortion and contraception in Ghana, my study setting. In 1985, the criminal code of Ghana was amended to allow abortion in the cases of rape, incest or the "defilement of a female idiot", as well as if the life or health (both mental and physical) of the woman is in danger or if there is risk of fetal abnormality (Guttmacher, 2013). Prior to this, abortion was not legal for any reason, and remains illegal if the reason mentioned is “on request” or economic hardship (Finlay and Fox, 2013). The increased access to abortion services is associated with a fertility decline in Ghana (Finlay and Fox, 2013). Fertility levels fell more rapidly in urban areas than rural ones (Benefo and Pillai, 2005) following this policy change. Furthermore, wealthier, urban and better educated women are more likely to get abortions (Sundaram et al, 2012).

Despite improved access to safe abortions, unsafe abortions continue to be the second largest cause of maternal mortality (GSS, GHS, and Macro International, 2009), and are linked to 13% of maternal deaths worldwide (WHO, 2011), but mortality from unsafe abortions may in fact be underestimated if concerns regarding underreporting are true (Gerdts et al, 2013). Sundaram et al (2012) estimate that women without children had seven times the odds of seeking an abortion as did women with three or more children. Wealthier, urban and better educated women were more likely to get abortions. However, 45% of the Ghanaian women in their sample who had abortions in the previous five years had undergone unsafe procedures, with a majority of women under the age of 20 obtaining unsafe services while a majority of older women accessed safe services. Women whose partners paid some or all of the expenses were more likely to get safe abortions.

Analysis on contraceptive use in sub-Saharan Africa includes an experimental study in Zambia by Ashraf et al (2013), who assigned women to be given contraceptive access alone or with their husbands. Women in the latter group were 19% less likely to seek family planning services, 25% less likely to use concealable contraception and 27% more likely to give birth. Closer to my own research setting, Eliason et al (2014) estimated that the average contraceptive prevalence rate in Nkwanta, a district in Ghana, was 6.2% (at the time, the national average was 19%), but that awareness of modern family planning methods was over 90%, and that lack of formal education among women, socio-cultural beliefs and spousal communication had a significant impact on contraceptive use, as did distance to health facilities and favourable opening hours.

2.8 Global Health Aid and Developing Countries

The effectiveness of health aid in developing countries has been the subject of a number of studies. Health aid has been found to be associated with a reduction in the prevalence and severity of diarrhea (De and Becker, 2015), and a reduction in the prevalence of malaria and improvements in self-reported health (Marty, Dolan, Leu and Runfola, 2017) in Malawi, as well as a reduction of overall disease burden in Uganda (Odokonyero et al, 2015). Health specific aid has been found to reduce infant mortality (Mishra and Newhouse, 2009), maternal mortality (Pickbourn and Ndikumana, 2016), and HIV-prevalence and child-mortality in Africa (Yogo and Mallaye, 2015). Kotsadam et al (2018) find that geographical proximity to active aid projects reduces infant mortality in Nigeria, and that aid is more effective in reducing infant mortality in less privileged groups, but that aid is not necessarily reaching the populations in greatest need. This has implications in the context of my study: reductions in foreign aid could lead to adverse outcomes such as higher infant mortality for less privileged groups, such as rural households with low education or low incomes.

3 Data

Demographic and Health Surveys (DHS) are conducted by Macro International's MEASURE project in developing countries and focus on women between the ages of 15 and 49. They are nationally representative and can be either standard surveys that collect information on a woman's complete birth history, or special modules focusing on abortion and maternal mortality. I use data from two DHS survey rounds conducted in Ghana in 2007 and 2014. The 2014 DHS round contains detailed information not only on the female respondents themselves as well as their households, but not on maternal mortality. It is therefore suitable for most of the outcomes I am concerned with, other than maternal mortality. The 2007 DHS round, however, was a special survey focusing on abortion and maternal mortality, but contained little information on household characteristics and other information. I use this dataset only for the maternal mortality outcome.

3.1 2014 Survey Round

The 2014 standard DHS survey data from Ghana surveyed 9,396 women. For each respondent, information was recorded on the month and year of each birth, whether the child was alive at the time of the survey, age at death, and succeeding and preceding birth intervals for each birth. I used these observations to create a woman-year panel with variables for number of births per woman-year, number of deaths of children under the ages of 5 and 2 per woman-year, and total parity (total number of births up to that point). Each observation was also assigned the woman's age and whether she had been sexually active, cohabitating or married at that point in time. These classifications were made using survey questions that asked about the interval from marriage to first birth and age of first sexual activity. For women who reported having their first child before their marriage (negative interval), no further information was available, so they are shown in the panel as being married from the time of birth of their first child.

These are the only time-varying characteristics, but cross-sectional information can be used to divide the women into demographic subgroups and therefore serve as useful controls. I make use of the wealth index variable that divides women into "poorest", "poorer", "middle", "richer", and "richest"; these can be used to divide the population into quintiles (above or below median wealth). Wealth is measured only at the time of the survey and could reasonably vary over the course of a woman's life, but previous research suggests that wealth quintiles show more stability (Jones, 2011).

An alternative indicator is education, which is also measured only at the time of the survey, but would not be expected to change after adulthood for the majority of the sample; 82% of the women sampled for 1998 Ghana DHS were no longer in school (Jones, 2011). In my sample, slightly under 24% of the women have never attended school, 13% have incomplete primary education, 6% completed their primary eduation, 42% have incomplete secondary education, 10% completed their secondary education, and the remaining 5% had higher education. Women can also be classified as having education above or below median. For my wealth variable, I use a binary indicator which takes the value 1 if the woman's wealth index is below the median. Similarly, the education indicator takes the value 1 for women who have never attended school, or only attended primary school.

My dataset contains information on 23118 births. Figure 2 shows the share of births by age. For example, 6% of births occur to mothers of age 20. The largest share is between the ages of 20 and 25, after which there is a gradual decline, followed by a steeper decline at age 30. I include a number of different age specifications: one between the ages of 17-25, which is the major group for abortions as per Jones (2011), as well as ages which account for more than 4% of births (18-30) or 3% of births (17-33). The main specification is 15-49, but robustness to other age specifications is also shown. Fertility trends for rural and urban women separately are shown in Figure 4. Panel (a) shows the full sample with time trend, while (b) shows the sample sample with the time trend removed. Panel (c) restricts the sample to women experiencing their first policy change, and includes a time trend. Panel (d) shows fertility for the same sample, but with the time trend removed.

Average fertility is increasing over the time for the sample, but not necessarily for Ghana as a whole. This is due to the fact that women are aging into the panel, and fertility rises in the sample as it ages. It is clear, however, that there are distinctly different trends for these two groups of women, with average fertility rising much faster over time for rural women as opposed to urban women.

The data also contains information on 2256 child deaths. Figure 3 shows the distribution of share of deaths by the child's age (in months). Almost 60% of deaths occur between birth and age 2; the probability of dying declines sharply after the first few months of birth. For my analysis, I focus on deaths under the ages of 2 and 5, i.e. whether the probability of dying under the ages of 2 and 5 is affected by being born in a policy year as opposed to a non-policy year.

The dataset consists only of years when the woman was at least 15 years old, meaning that women age into the sample. From 1980 to 2014, there are 147,530 woman-year observations. Each woman has between 1 and 35 observations (the average is 16). Summary statistics for the 2014 survey round is shown in Table 1. 51% of the women in my sample are rural, and 43% have less than secondary education. 68% have ever been married or cohabitated with a man. The 6511 women who have given birth have an average of 3.55 children, and the average age in the full sample is 29.7 years.

In relation to the Mexico City Policy, a panel dataset that runs from 1980 to 2014 allows us to consider a number of different time periods. 1980 to 1984 are years before the policy was first implemented and are denoted by PRE. 1985 to 1992, the first implementation of the policy, are denoted by ON1, 1993 to 2000 are OFF, 2001 to 2007 are ON2, and 2008 onwards are OFF. These periods cannot be compared directly, as various sample characteristics are changing over time. Furthermore, as women age into the sample, there are considerably more observations for later periods than earlier ones, ranging from 2,436 observations in the PRE period to 58,183 for the period 2008 to 2014. The mean age of the sample is also increasing, from 16.3 years in the PRE period to 28.6 years in the final off period. Restricting the sample to women 17-25 and 17-33 makes the mean ages during each period more similar. Focusing on only the 18-20 age group, however, gives us almost identical mean ages. Table 2 shows the mean age of the sample by period.

Finally, contraceptive use in the sample is distinctly different for various rural and urban subgroups. Table 3 shows contraceptive use in sample for each subgroup, and includes percentages of the sample that are currently using contraceptives as well as those who have ever used. Sample sizes for each subgroup are also reported. As the "currently using" measure is affected by the age composition of the sample, the "ever used" measure is a more reliable measure of contraceptive use and prevalence. Urban women are more likely to have ever used contraceptives than rural women. Focusing on rural subgroups, women in the middle, richer and richest groups are more likely to have ever used contraceptives than women in the poorest group.

3.2 2007 Survey Round

The DHS round conducted in Ghana in 2007 contains detailed information not just on the women surveyed, but also on their siblings, namely whether each sibling is still alive or not, as well as year and cause of death, including whether female siblings died while pregnant, in childbirth or within two months of delivery. Using this data, I create a maternal mortality panel with my dependent variable of interest being an indicator for whether a woman in the sample lost a female sibling while the latter was pregnant, giving birth or two months from delivery. I assume that the education level, place of residence (urban or rural) and wealth are the same for siblings as they are for women surveyed, as information on the siblings' education and residence is not available.

Table 4 shows the summary statistics for the 2007 DHS round. 52% of the sample are rural, and 46% have only primary eduation or less. 2% of the sample have lost siblings for reasons related to maternal mortality. Maternal mortality accounts for 164 sibling deaths, and the 159 respondents who lost siblings during pregnancy, in childbirth or 2 months from delivery lost an average of 1.03 siblings with a maximum of 2. The average age of respondents is 29.3 years.

4 Empirical Strategy

The aim of this analysis is to investigate whether the imposition or removal of the Mexico City Policy had an effect on fertility (yearly as well as total). Jones (2011) finds that both conception and abortion rates go up, but that abortion rates do not rise enough to completely counteract conception rates. I look for similar evidence by looking at annual fertility at the woman-year level as well as total parity. I focus on Ghana primarily as DHS data from Ghana is relatively plentiful and easy to access, but also because empirical work on the Mexico City Policy has been conducted using DHS data from Ghana in the past (Jones, 2011), allowing for a comparison of results. Ghana can also be classified as aid-dependent on the U.S., as it received above median assistance in non-policy years (Bendavid, Avila and Miller, 2011).

I expect that birth rates are affected by many observable and unobservable factors other than the policy itself. To control for these, I use a difference-in-difference specification with an interactive time trend and individual and year fixed effects. My control group for each specification consists of women who are less vulnerable to loss of contraceptive access due to the policy, such as wealthier women, more educated women, or urban women. Due to the nature of the data, it is difficult to show satisfactory parallel trends (only the years from 1979 to 1985 comprise the pure pre-policy period, and these years have the lowest fertility rates as most women have not yet aged into the sample), but panels (a) and (c) in Figure 4 suggest similar pre-trends. Another reason for parallel trends not holding, however, is that it could be reasonably expected that urban and rural women are different to begin with, even before the policy was first introduced. The policy could, however, be expected to create a wider gap between the groups if rural women are truly more likely to be exposed to the policy.

The primary estimation is as follows:

$$Outcome_{it} = \alpha + \beta Policy_t + \gamma Exposure_i + \delta Exposure_i * Policy_t + \theta Controls_{it} + V_i + V_t + \varepsilon_{it}$$

The policy variable is in a binary indicator for whether the policy is on (1) or off (0). Exposure is a binary indicator for whether a woman is more likely to be exposed to the policy (1) or not (0), based on her place of residence, wealth status or educational attainment. In different specifications, a woman is classified as more exposed to the policy if she lives in a rural area, has below median wealth or has below median education. Time-varying controls include marital status and age. Fixed effects at the year, woman and policy level are also included, as is a cubic time trend.

I analyze a number of outcomes of interest. The first is fertility per woman-year, defined as the number of children woman i gave birth to in year t. I also consider child mortality outcomes using the same specification. Restricting the sample to woman years with at least one child under the age of 2, the outcome variable is an indicator for whether a child of woman i under the age of 2 died in year t. A similar outcome variable is under 5 mortality, an indicator for whether a child of woman i under the age of 5 died in year t. The hypothesized effect is that if the policy is increasing unintended births for women in low access areas, then this could lead to constrained housedhold budgets and a reduction in investment in child health, leading to adverse health outcomes, or, in the extreme case, higher child mortality. A positive δ would be evidence of this.

I consider two other outcomes namely maternal mortality and age at first birth. For the first, I use a similar difference-in-difference regression to the those I used to look at fertility and child mortality to look at whether the probability of losing a sibling is different in policy versus no-policy years. To analyze the policy's impact on age at first birth, I use a simple Cox proportional hazards model.

5 Results

For this analysis, women whose wealth index is below median are classified as low income, and those above the median are classified as high income. By education, I classify women with no education versus women with some education. For fertility per-woman year, I use the woman-year panel, while for age at first birth I conduct the hazard model analysis on the cross-sectional data. Before the difference-in-difference analysis, however, I show OLS results to motivate my strategy (Table 5). Column 1 shows a simple difference-in-means of fertility per woman-year in policy vs. non-policy years. It suggests that sampled women have 0.005 more children in policy years as compared to non-policy years, and this difference is significant at 1%. Adding a linear time trend and year fixed effects increases the coefficient to 0.042, but adding in time varying woman level controls such as age squared, marital status, an indicator for sexual activity and parity reduces it to 0.021. This is still significant. Finally, adding woman fixed effects reduces the coefficient to 0.003, and all significance is lost. This is expected since the sources of exposure for the policy (living in a rural area, having low education and being poor) are all time-invariant, and therefore soaked up by the woman fixed effects.

5.1 Fertility Outcomes

The hypothesized effect of the policy is that it could reduce the supply of contraceptives provided by NGOs. This could be expected to affect rural women disproportionately, as urban women are more likely to have a wider variety of options to access contraception. This could drive up the incidence of unintended pregnancies if women do not substitute modern contraceptives with abstinence. Furthermore, since previous research (Jones, 2011) suggests that rural women are in fact less like to access abortions, these unintended pregnancies would most likely translate to births. In addition to fertility per woman-year, I look at age at first birth. Another outcome that could be considered but is not shown here is birth spacing. Access to contraceptives could help women space their births farther apart, which could lead to better health outcomes for the child.

Pregnancies that begin less than 18 months after the previous birth have been found to be associated with a number of adverse outcomes, including neonatal morbidity (DeFranco et al, 2015). Previous studies linking contraceptive use to birth spacing such as Feldman et al (2009), make use of a Cox proportional hazard model, and a similar approach would be appropriate in this setting.

5.1.1 Fertility Per Woman-Year

I consider the full sample (i.e. all observations with ages between 15 and 49). Running a difference-in-difference regression comparing urban and rural women across policy and non-policy years, with a linear time trend and a quadratic age trend, marital status, total parity and sexual activity as time varying controls, I see a significantly positive coefficient for the interaction term (Table 6). Woman and year fixed effects are also included, and an interacting time trend allows trends to differ for urban and rural women. Rural women in policy years have 0.011 more children per year than urban women in policy years (column 1), significant at 5%. Restricting the sample to rural women and comparing low education (no education, incomplete primary and complete primary) to high education (incomplete secondary, complete secondary and higher) women in non-policy periods, the interaction term is again positive and significant, suggesting that lower education women have 0.013 more children per year than women with higher education in policy years (column 2), significant at 10%. Comparing rural poor women to less poor women in policy versus non-policy years, the results are insignificant (column 3). This suggests that though rural women are more vulnerable to the policy than urban women, this may be driven by less-educated women and not less-wealthy women. Looking at urban subgroups, women with lower education (column 4) and lower wealth (column 5) show no policy impacts..

5.1.2 Age at First Birth

Another outcome of interest is the age at which the women in the sample begin childbearing, as this could affect a number of demographic and non-demographic conditions in the woman's life (Rao and Balakrishnan, 1988). The Mexico City Policy could theoretically reduce the age at first birth for women, and through this impact education and other outcomes that could persist throughout the woman's lifetime. Lengthening of time between marriage and first birth was argued to have led to declining fertility in the United States (Pebley, 1981). Since the dataset provides retrospective information collected in a cross-sectional survey, I use a proportional hazards model to analyze the effect of the policy on age at first birth, informed by a similar study by Rao and Balakrishnan (1988) in Canada.

I consider a number of time-varying predictors, including age at first cohabitation, education, an indicator for whether the woman lives in an urban area, wealth and a policy indicator. The Nelson-Aalen cumulative hazard estimate is presented in Figure 5. For the Nelson-Aalen cumulative hazard model, the horizontal axis shows age at first birth and the vertical axis shows the inverse of number of subjects at risk, with the number at risk being defined as the number of subjects just before the first birth that are still observed to not have had any children.

Regression results are presented in Table 7. Column 1 includes the entire sample and an interaction term between the policy indicator and a dummy for whether the woman is rural or not. The coefficient on the interaction term is negative, suggesting that the policy reduces age at first birth for rural women by -0.176 years relative to its effect on urban women. Column 2 restricts the sample to rural women and includes and education-policy interaction. Coefficients for neither the policy indicator nor the interaction are significant. Column 3 again looks at rural women, but with a wealth-policy interaction. Again, none of the coefficients of interest are significant. It should be noted for each of these, however, that the p-value is slightly over 0.1, so the lack of significance may be driven by low statistical power rather than the absence of an effect. Columns 4 and 5 restrict the sample to urban women, again considering education-policy and wealth-policy interactions respectively. Again, coefficients for the variables of interest are not significant, with large standard errors.

5.2 Mortality Outcomes

An increase in births arising from unintended pregnancies could impose a financial burden on families, leading to lower investment in children's health. An extreme consequence of this could be a higher incidence of child mortality under the age of 5, when children are most vulnerable to a variety of disease and environmental factors. However, if the imposition of the policy led to an increase in the use of unsafe abortion, then it could also raise maternal mortality. For child mortality outcomes, the woman cross-section from the 2014 DHS round is converted into a child cross-section, with a binary variable indicating death under 5 or under 2. The outcome is regressed on an indicator for birth in a policy year, and an interaction with the exposure indicator (rural, uneducated and poor). Controls include whether the mother was married at the time of birth, mother's age at birth, and mother's parity at birth. Woman fixed-effects are included, and standard errors are clustered at the woman level.

5.2.1 Under 5 Mortality

A comparison of rural versus urban women in policy versus non-policy years does not yield any meaningful results. For each urban and rural subgroup, none of the coefficients are significantly different from 0. Results are presented in Table 8.

5.2.2 Under 2 Mortality

Using an indicator for under 2 mortality does not yield any meaningful results. The policy does not appear to have had an effect on child mortality under the age of 2. Results are presented in Table 9.

5.2.3 Maternal Mortality

A difference in difference using the maternal mortality panel constructed from the 2007 DHS round does not yield significant results. The only significant coefficient is age at death, with a marginal increase in age at death being associated with a 3.14% increase in dying. Results are presented in Table 10. Coefficients are each multiplied by 100 as they are small in magnitude.

6 Supplementary Analysis

For this section, I focus on results presented in section 5.1.1 (fertility per woman-year). I do not consider child or maternal mortality since I did not previously find any significant results. Since I originally restricted the fertility sample to women between the ages of 15 and 49, I first check that my results are not driven by the choice of age, by looking at two other age ranges, namely 17-33 and 18-20. I then focus on individual policy changes, focusing only the period before and after the changes in 1985, 1993, 2001 and 2008. Third, I restrict the sample only to women who are being exposed to their first policy change, i.e. for each woman, I consider only the first time the policy turns on during her childbearing years. A final supplementary analysis that is not shown here is a placebo test that would randomly reassign the policy blocks.

6.1 Other Age Selections

I previously ran fertility analysis with the age range of 15 to 49 The same analyses can be run with ranges of 17 to 33 and 18 to 20, the former being broader and the latter far narrower. Table 11 shows the results with different age specifications. The 17-33 age restriction results are shown in Column 2, and the 18-20 age restriction results are shown in Column 3. None of the interactions are significant. A possible reason for this is that for the sub-sections, abortion use increases to the point whether the effect of unintended pregnancies is negated. Women between the ages of 18 and 20 are also less likely to be married or sexually active, putting them less at risk.

6.2 Individual Policy Changes

Focusing on individual policy changes allows us to run a two-period difference-in-difference analysis. For these, I include year fixed effects and a linear time trend and rural interaction.. Since each policy change is different, focusing on one at a time could lead to a better understanding of their impacts. I consider the changes in 1985, 1993, 2001 and 2009. The policy variable is coded as before, taking the value 0 in non-policy years and 1 in policy-years. The only source of exposure shown is rural residence. The full sample (ages 15 to 49) is used. Results are shown in Table 12.

First, I look at the years around the initial policy implementation in 1985. I therefore consider all years up to and including 1992 (the policy was rescinded by President Clinton in 1993). The policy coefficient is significant but negative (Column 1) and the rural-policy interaction is insignificant. The negative policy coefficient can be explained by the fact that the policy years happened after the non-policy years for President Reagan, and fertility was declining over time. Next, I consider the first removal of the policy by President Clinton in 1993 (Column 2). The sample is restricted to years between 1986 and 2000. None of the coefficients are significant.

In 2001, the policy was reinstituted by the Bush administration, but exemptions were made for NGOs that provided HIV/AIDS services. Theoretically, this could mean a lower impact for this iteration of the policy. Restricting the data to years between 1994 and 2008, the interaction term is insignificant(Column 3), but the policy coefficient is once again negative. For the final policy removal included in this dataset (Obama in 2009), I look at years from 2002 to 2014. The interaction term is insignificant (Column 4).

A possible confounding factor for this kind of supplementary analysis is that the policy indicator could be picking up other "presidency effects", i.e. the effects of other aid-related legislation made during their presidential terms, since the policy variable takes the value 0 for the Obama years and 1 for the Bush years.

6.3 Restriction of Sample to First Policy Change

Due to the nature of this dataset, women move in and out of policy periods, with the oldest women in the sample experiencing all 4 policy changes and the youngest experiencing none. There is some concern that the impact of the first policy a woman is exposed to during her childbearing years may affect her throughout her reproductive life and through subsequent policy changes. I restrict the sample to look at only the average effect of the first policy faced by each of the women in my sample. Results are shown in Table 13.

The rural-policy interaction is insignificant (column 1), but the education-policy interaction is positive and significant for the rural subgroup (column 2). The low wealth-policy interaction, however, is negative and significant for the rural subgroup (column 3).

6.4 Event Study Analysis

A final supplementary analysis examines the data with the use of an event study. I examine the effect of years since the policy's enactment on fertility per woman-year, the outcome of interest. Results are shown in Table 14, without individual fixed effects (column 1) and with (column 2), and are graphed in Figure 6.

7 Discussion

My analysis differs from Jones (2011) in a number of ways. The methodological framework is different; while Jones uses woman and year fixed effects to directly identify the effect of the policy, I employ a difference-in-difference setup in addition, and identify the relative effect of the policy using the exposure interaction, but not the effect of the policy directly. A major reason for this is the datasets used. Jones uses DHS 2007 data, which contains detailed information on each pregnancy, while I use DHS 2014 data, which contains information on births but not on pregnancies. Jones is able to use this more detailed dataset to create a woman-month panel that follows women through conception and into childbirth, miscarriage or abortion. The assumption that Jones can justifiably make, but that I cannot, is that most, if not all, non-policy variation can be removed by employing woman and year fixed effects.

My results also differ, which could either be due to the different methods I use or the different dataset. Unlike Jones, I also do not employ sample weights. Though I do find that fertility increases for rural women relative to urban women and for rural less educated women relative to rural educated women, I do not find the wealth interaction to be significant. If we take the rural fertility coefficient of 0.011 (Table 6, Column 1) at face value, however, and multiplying by 35 (the time period of the panel), we have about 0.385 children per rural women over 35 years. This translates to about 1845 "unintended" children for the 4790 rural women in my panel. This is about 0.19 children per woman for the entire sample of 9396 women. Jones's estimated number for unintended births as a result of the policy was between 500,000 and 750,000. In 2007, the population of Ghana was 22.7 million, of which about 11,395,400 (50.2%) were female. The unintended birth-woman ratio, then, is between 0.04 and 0.07, which is far lower than 0.19. A possible reason for this could be the application of sample weights, thus making Jones's results more generally applicable.

8 Conclusion

The Mexico City Policy has been a political tool since its first implementation, and has active detractors on the left and equally vocal supporters on the right in the United States. However, though many consequences of the policy have been theorized, including an impact on maternal mortality (Law and Rackner, 1987), very little empirical evidence exists beyond those mentioned here. One of the reasons for this is that very few datasets collect information conducive to such an analysis.

Another is that the Mexico City Policy could be interacting with several other domestic and international policies in developing countries, and the effects of these are difficult to separate. Information on funding flows to individual NGOs is also difficult to find, other than high-profile ones such as the International Planned Parenthood Federation or Maria Stopez. Qualitative evidence of the impact of the policy includes a research project by the Population Crisis Committee in 1987 (Camp, 1988), which identified thirty-one agencies that had ongoing population or family planning projects funded by USAID. Thirteen of them were exempt from the policy because they had cost-reimbursable contracts, while seven had agreements from before the policy was enacted that had not yet come up for renewal. In fact, only nine of the surveyed organizations were now operating under the new restrictions, and three of them were able to successfully negotiate with USAID to exclude most of their work in developing countries from the policy. However, family planning organizations were faced with a new cost: that of monitoring the activities of their subgrantees and sub-sub-grantees to ensure their compliance. Inadvertent violations in “lapsed law” countries were also a concern. Therefore, the true effect of the policy on funding is unclear; it is difficult to tell to what extent avoidance methods were practiced or for which NGOs they were more or less successful.

This, combined with lack of time-varying individual data on contraception use, make identifying the impact of the policy troublesome, though Bendavid et al (2011) and Jones (2011) are both able to use interesting forms of variation in exposure to the policy to create plausible estimates. My thesis examines the effect of the policy on fertility outcomes, and finds a possible impact on the policy on fertility per woman-year, and a slight reduction in age at first birth, but no other outcomes of interest. However, this does not mean that the policy has had no effect on other outcomes such as maternal and child mortality. Power issues could be a concern, especially in the case of maternal mortality, where only 158 of the sample of 10,370 have lost siblings.

It seems possible that any effect of the policy will be magnified in coming years as the newest enactment of the policy (Trump, 2017). For the first time, this incarnation applies to all global health aid provided by the United States, and not just family planning funding, drastically expanding the scope of the Mexico City Policy. Understanding the policy and its impacts better may have consequences for international aid policy, allowing other donor countries to better offset funding shortfalls. It may also encourage recipient nations to change domestic policies regarding abortion and family planning to counteract this measure. More high-quality data, such as DHS and LSMS, are critical to this endeavour. For example, a more reliable source of exposure for an analysis such as mine would be time-varying contraceptive use at the woman level, or access to health clinics at various points in time.

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9 Tables and Figures

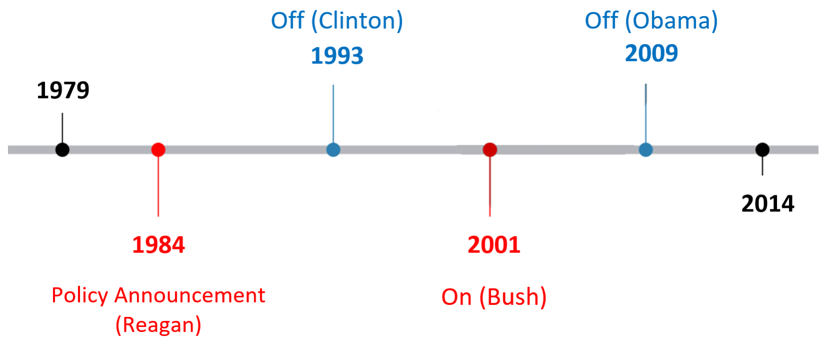

Figure 1: Timeline of Mexico City Policy

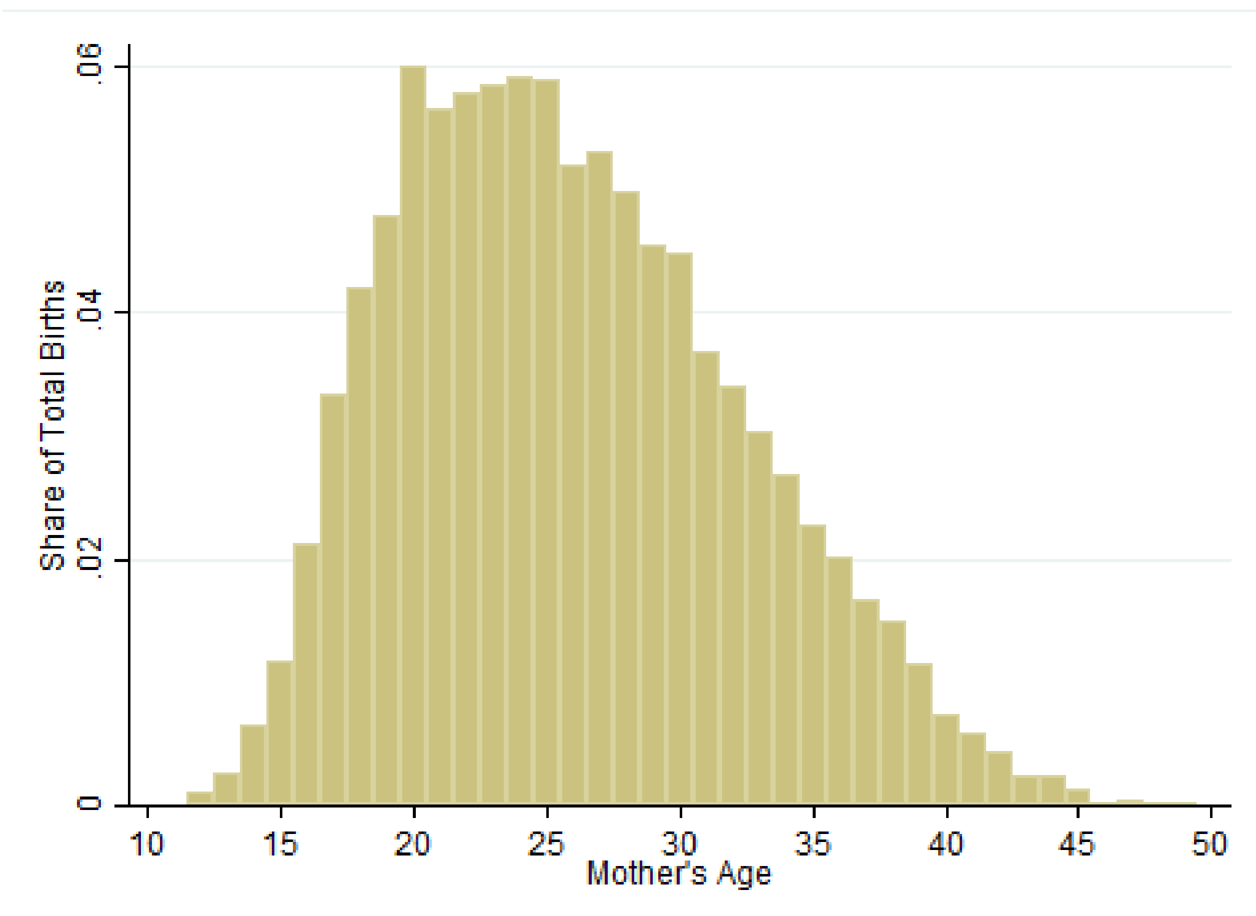

Figure 2: Share of Total Births by Mother's Age

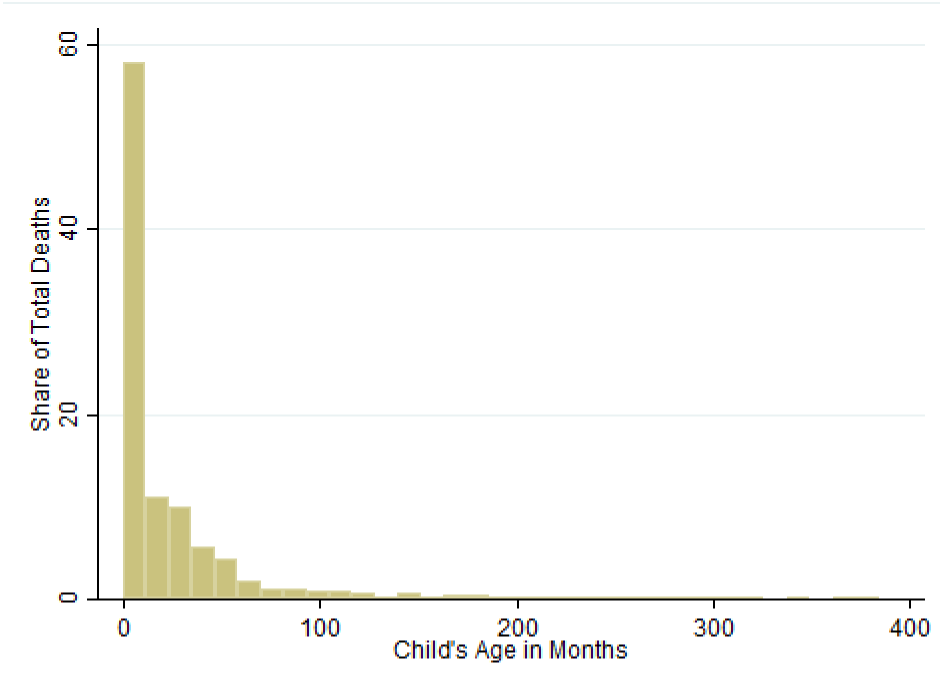

Figure 3: Share of Total Deaths by Child's Age (Months)

Note:This figure should be viewed as follows: for example, almost 60% of child deaths occurred between birth and 8 months of age for this particular sample.

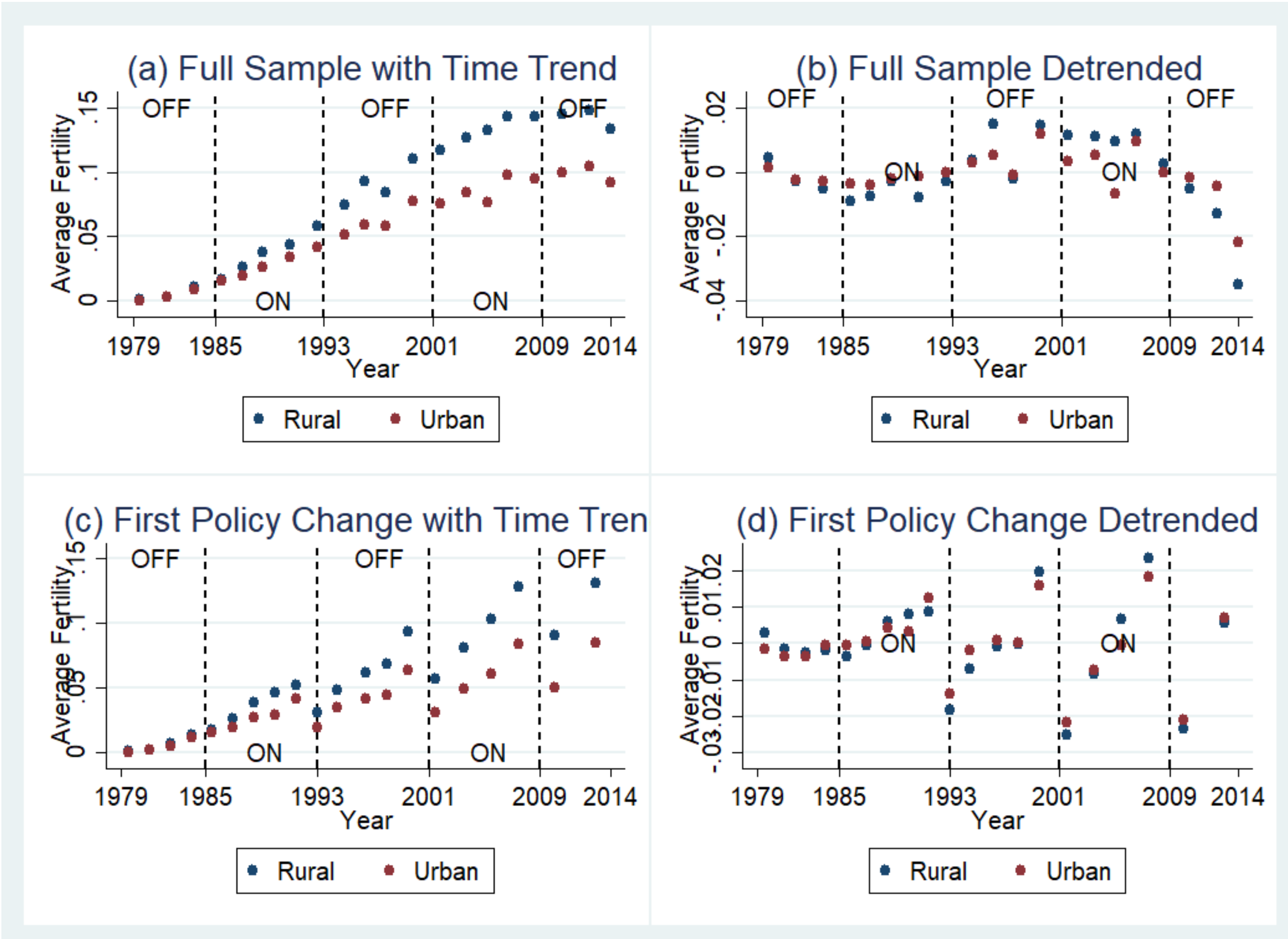

Figure 4: Rural and Urban Fertility Trends

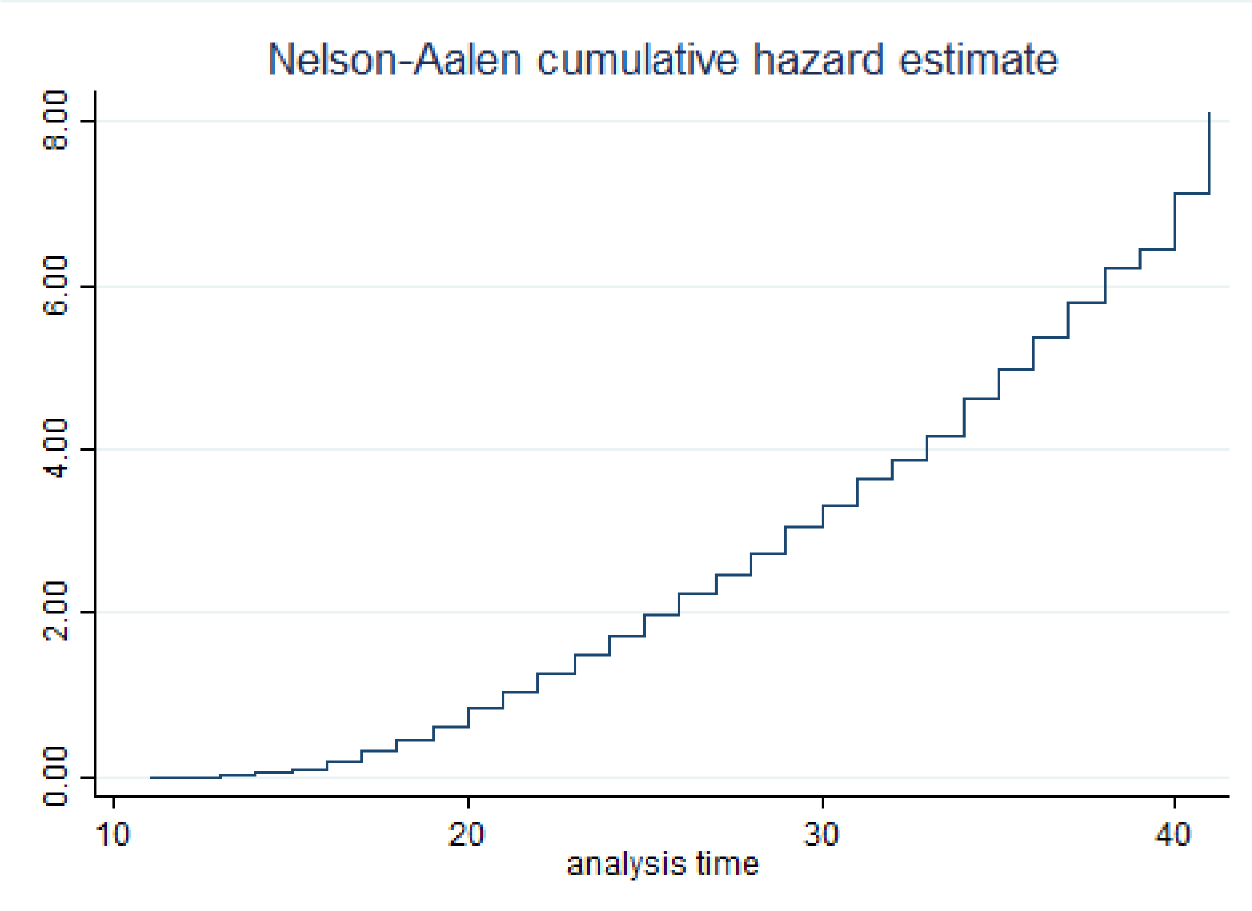

Figure 5: Nelson-Aalen Cumulative Hazard Estimate

In the staircase function above, each step is placed at each observed age at first birth (for example, there are no observations at age 10). The vertical size of each step is the inverse of the number at risk, i.e. the number of subjects just before the first birth that are still observed to not have had any children. For this graph, the interpretation is that conditional on not having had the first child at age 20, the number of women at risk at age 21 is 1.

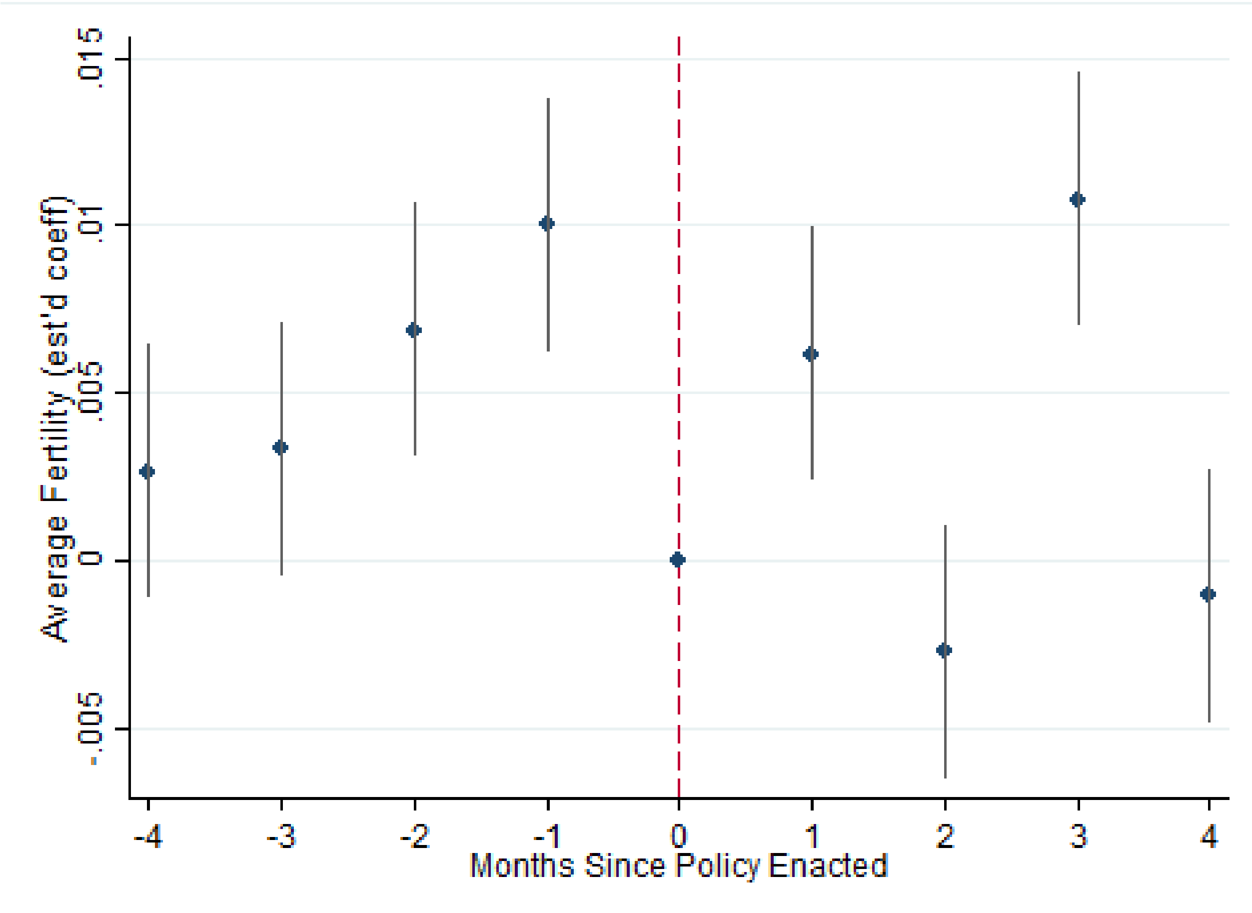

Figure 6: Event Study Analysis

Table 1: Summary Statistics (DHS 2014)

Variable	Mean	SD	Min	Max	N
Rural	0.51	0.50	0	1	9396
Education	0.43	0.49	0	1	9396
Wealth	0.50	0.50	0	1	9396
Ever Married	0.68	0.47	0	1	9396
Births	3.55	2.19	1	13	6511
Age	29.70	9.71	15	49	9396

Table 2: Mean Age of Sample, by Period

		Age: 15-49		Age: 17-33		Age: 18-20	
Policy	Years	Mean	N	Mean	N	Mean	N
PRE	1980-1984	16.3	2,436	17.7	940	18.3	468
ON1	1985-1992	19.2	14,214	20.4	10,763	19.0	4,466
OFF	1993-2000	22.5	30,271	23.5	25,460	19.0	5,932
ON2	2001-2008	25.8	49,751	24.4	35,551	19.0	7,297
OFF	2009-2014	28.8	50,858	24.6	30,396	19.0	5,715

Note: Columns 1, 3, and 5 show average sample age when the data is restricted to women of ages 15-49 (full sample), 17-25, 17-33 and 18-20 respectively. These are sample restrictions. Columns 2, 4, and 6 provide the relevant sample sizes.

Table 3: Contraceptive Use in Sample

	N	Currently Using	Ever Used
Full Sample	9396	18.5%	42.7%
Urban	4602	16.8%	43.6%
Rural	4794	20.0%	41.9%
Urban Subgroups			
Low Education	1337	18.9%	41.5%
High Education	3265	22.7%	44.4%
Poor	856	16.7%	35.3%
Rich	3746	22.7%	45.5%
Rural Subgroups			
Low Education	2691	20.4%	40.3%
High Education	2103	25.5%	44.0%
Poor	3842	21.6%	40.1%
Rich	952	27.0%	49.2%

Table 2 shows the number of women at different education and wealth levels, as well as by place of residence. The percentage of women currently using contraceptives is shown, as is the percentage who have ever used contraception.

Table 4: Summary Statistics (DHS 2007)

Variable	Mean	SD	Min	Max	N
Rural	0.52	0.50	0	1	10370
Low Education	0.46	0.50	0	1	10370
Lost Sibling	0.02	0.12	0	1	10370
Siblings Lost	1.03	0.18	1	2	159
Age	29.30	9.73	15	49	10370

Table 5: OLS Regressions

	(1) OLS	(2) Time Trend and F.E.	(3) Controls	(4) Woman F.E.
Policy	0.005***	0.042***	0.021***	0.003
	(0.001)	(0.004)	(0.005)	(0.015)
Time Trend		0.003***	0.001***	-0.020***
		(0.000)	(0.000)	(0.001)
Age Squared			-0.000***	-0.000***
			(0.000)	(0.000)
Married			0.147***	0.250***
			(0.003)	(0.003)
Sexually Active			0.081***	0.120***
			(0.002)	(0.002)
Parity			0.061***	0.105***
			(0.001)	(0.002)
N	338256	338256	147530	147530
mean	0.068	0.068	0.068	0.068

(1) shows results for a simple OLS regression of fertility variable on policy indicator

(2) adds a time trend and year fixed effects

(3) adds time-varying woman controls

(4) adds woman fixed effects.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Policy Effect on Fertility per Woman-Year

	(1) Full Sample	(2) Rural Education	(3) Rural Wealth	(4) Urban Education	(5) Urban Wealth
Policy	0.000	-0.026	-0.005	0.035	0.028
	(0.015)	(0.022)	(0.022)	(0.022)	(0.021)
Rural x Policy	0.011***				
	(0.003)				
Rural x Trend	-0.005***				
	(0.000)				
Low Education x Policy		0.013**		-0.001	
		(0.005)		(0.005)	
Education x Trend		-0.006***		-0.005***	
		(0.001)		(0.001)	
Poor x Policy			-0.008		0.004
			(0.006)		(0.006)
Wealth x Trend			-0.007***		-0.006***
			(0.001)		(0.001)
N	147530	74712	74712	72818	72818
Mean	0.068	0.085	0.085	0.064	0.064

Standard errors (clustered: woman level) in parentheses

All regressions include woman and year fixed effects

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Hazard Model: Age at First Birth

	(1) Full Sample	(2) Rural	(3) Rural	(4) Urban	(5) Urban
main					
Low Education	-0.153***	-0.119**	-0.168***	-0.121**	-0.147***
	(0.030)	(0.055)	(0.041)	(0.059)	(0.044)
Poor	-0.098***	-0.001	-0.062	-0.251***	-0.315***
	(0.036)	(0.046)	(0.064)	(0.056)	(0.076)
Rural	0.203***				
	(0.043)				
policy=1	0.616	0.734	0.804	0.167	0.262
	(0.407)	(0.571)	(0.571)	(0.579)	(0.572)
Rural x Policy	-0.176***				
	(0.053)				
Low Education x Policy		-0.101		-0.055	
		(0.075)		(0.081)	
Poor x Policy			0.125		0.124
			(0.085)		(0.104)
N	6008	3293	3293	2715	2715
Mean	19.63	18.74	18.74	20.67	20.67

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Policy Effect on Under 5 Mortality per Woman-Year

	(1) Full Sample	(2) Rural	(3) Rural	(4) Urban	(5) Urban
Policy Birth	0.008	-0.008	0.006	0.008	0.012**
	(0.005)	(0.007)	(0.009)	(0.007)	(0.006)
Policy Birth x Rural	-0.006				
	(0.006)				
Policy Birth x Uneducated		0.014		0.000	
		(0.008)		(0.010)	
Policy Birth x Poor			-0.004		-0.015
			(0.010)		(0.011)
N	23118	13946	13946	9172	9172
Mean	0.047	0.048	0.048	0.046	0.046

(1) shows full sample under 5 mortality results

(2) and (3) consider the rural subgroup only

(4) and (5) consider the urban subgroup only

Standard errors (clustered by woman) in parentheses

Includes woman-fixed effects

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Policy Effect on Under 2 Mortality

	(1) Full Sample	(2) Rural	(3) Rural	(4) Urban	(5) Urban
Policy Birth	0.005	-0.007	0.007	0.006	0.009
	(0.005)	(0.007)	(0.009)	(0.007)	(0.006)
Policy Birth x Rural	-0.002				
	(0.006)				
Policy Birth x Uneducated		0.013		-0.003	
		(0.008)		(0.010)	
Policy Birth x Poor			-0.005		-0.015
			(0.010)		(0.010)
N	23118	13946	13946	9172	9172
Mean	0.040	0.040	0.040	0.040	0.040

(1) shows full sample under 5 mortality results
(2) and (3) consider the rural subgroup only
(4) and (5) consider the urban subgroup only
Standard errors (clustered by woman) in parentheses
Includes woman-fixed effects
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Maternal Mortality

	(1) Full Sample	(2) Rural	(3) Urban
Policy	-0.008	0.001	-0.002
	(0.000)	(0.000)	(0.000)
Rural x Policy	0.002		
	(0.000)		
Age at Death	3.138***	3.170***	3.096***
	(0.001)	(0.001)	(0.001)
Low Education x Policy		-0.001	0.002
		(0.000)	(0.000)
N	383690	200170	183520
Mean	0.0004	0.0005	0.0004

Standard errors in parentheses and clustered at woman level
All regressions include woman and year fixed effects
Restricted to years after 1970
Maternal death indicator=1 for death while pregnant, in childbirth or within two months of delivery
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Different Age Specifications

	(1) 15-49	(2) 17-33	(3) 18-20
Policy	0.000	0.001	-0.212***
	(0.015)	(0.023)	(0.033)
Rural x Policy	0.011***	0.007	0.021
	(0.003)	(0.005)	(0.017)
Rural x Trend	-0.005***	-0.012***	-0.054***
	(0.000)	(0.001)	(0.005)
Age Squared	-0.000***	-0.001***	-0.008***
	(0.000)	(0.000)	(0.003)
Married	0.248***	0.249***	0.146***
	(0.003)	(0.003)	(0.005)
Sexually Active	0.121***	0.155***	0.153***
	(0.003)	(0.003)	(0.004)
Parity	0.111***	0.207***	0.858***
	(0.002)	(0.003)	(0.007)
N	147530	103110	23878
Mean	0.068	0.184	0.145

(1) shows full sample fertility results

(2) restricts the sample to women between 17 and 33

(3) restricts the sample to women between 18 and 20

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Individual Policy Changes

	(1) Reagan	(2) Clinton	(3) Bush	(4) Obama
Policy	-0.384***	0.212*	-0.272***	0.070
	(0.117)	(0.110)	(0.083)	(0.055)
Rural x Policy	0.021	-0.009	0.016*	0.011
	(0.019)	(0.011)	(0.009)	(0.008)
Married	0.204***	0.235***	0.223***	0.216***
	(0.007)	(0.005)	(0.004)	(0.004)
Sexually Active	0.134***	0.131***	0.145***	0.178***
	(0.007)	(0.005)	(0.004)	(0.004)
Parity	0.299***	0.239***	0.225***	0.219***
	(0.008)	(0.005)	(0.003)	(0.003)
Age Squared	-0.003***	-0.001***	-0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
N	16650	43448	77194	95497
Mean	0.020	0.055	0.095	0.116

Standard errors (clustered at woman level) in parentheses
All regressions contain woman and year fixed effects
Results are shown for full sample only
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Restriction to First Policy Change During Childbearing Years

	(1) Full Sample	(2) Rural Education	(3) Rural Wealth	(4) Urban Education	(5) Urban Wealth
Policy	-0.009	-0.035	0.006	-0.001	-0.009
	(0.017)	(0.025)	(0.025)	(0.024)	(0.023)
Rural x Policy	0.007				
	(0.006)				
Rural x Trend	-0.015***				
	(0.001)				
Low Education x Policy		0.024**		0.014	
		(0.010)		(0.009)	
Education x Trend		-0.015***		-0.018***	
		(0.002)		(0.001)	
Poor x Policy			-0.029**		0.010
			(0.012)		(0.010)
Wealth x Trend			-0.015***		-0.013***
			(0.002)		(0.002)
N	85938	43116	43116	42822	42822
Mean	0.068	0.081	0.081	0.055	0.055

Standard errors (clustered: woman level) in parentheses

All regressions include woman and year fixed effects

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Event Study Fertility Results

	(1)	(2)
-4	0.003	0.003
	(0.002)	(0.002)
-3	0.003	0.003
	(0.002)	(0.002)
-2	0.007***	0.007***
	(0.002)	(0.002)
-1	0.010***	0.010***
	(0.002)	(0.002)
1	0.006***	0.006***
	(0.002)	(0.002)
2	-0.003	-0.003
	(0.002)	(0.002)
3	0.011***	0.011***
	(0.002)	(0.002)
4	-0.001	-0.001
	(0.002)	(0.002)
N	169128	169128
mean	0.055	0.055

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$