Contraceptive supply and fertility outcomes: evidence from Ghana

Jones, Kelly M.

International Food Policy Research Institute

2013

Contraceptive supply and fertility outcomes: Evidence from Ghana

Kelly M. Jones*

August 23, 2013

*International Food Policy Research Institute; 2033 K St NW, Washington, DC 20017; (202) 862-4641; k.jones@cgiar.org. The author gratefully acknowledges funding from the National Science Foundation's Doctoral Dissertation Research Improvement Grant, the William and Flora Hewlett Foundation's Fellowship in Population, Reproductive Health and Economic Development, the UC Berkeley Population Center (NICHD R21 HD056581), and the Center of Evaluation for Global Action. Thanks are owed to Elisabeth Sadoulet, Alain de Janvry, Jeremy Magruder, Ted Miguel, Pascaline Dupas, Craig McIntosh, Erick Gong, Gil Shapira, Melissa Hidrobo and participants of the PopPov research conference. Research assistance was provided by Aviva Lipkowitz. All remaining errors are my own.

Abstract

Total fertility rates in Sub-Saharan Africa are nearly double that of any other region in the world. Evidence is mixed on whether providing contraceptives has an impact on fertility. I exploit exogenous, intermittent reductions in contraceptive supply in Ghana, resulting from cuts in U.S. funding, to examine impacts on pregnancy, abortion, and births. Women are unable to fully compensate for the 22% supply reduction using traditional methods for preventing pregnancy, which increases by 10%. Only non-poor women offset these unwanted pregnancies with induced abortion. Using separate data, I find that poor women experience increases in realized fertility of 7-10%.

JEL Codes: I15, J13, O19, F35

1 Introduction

Sub-Saharan Africa has the highest fertility rates in the world, lagging behind every other region in terms of demographic transition. This region also has significant unmet need for family planning, with fertility rates outpacing wanted fertility by 24%. Unwanted fertility is highest among the poorest and least educated women, suggesting that achieving target fertility has significant implications for the welfare and productivity of the next generation of Africans.[1] Such figures suggest that family planning programs may play a key role in African development.

Yet whether or not contraceptive access actually affects fertility outcomes is a matter of considerable debate. Some have argued that fertility is driven purely by preferences, and that access to family planning has negligible impacts (Pritchett, 1994; Miller, 2010), while others have provided evidence that increased access to contraceptives does reduce fertility (Sinha, 2005; Molyneaux and Gertler, 2000).

In Sub-Saharan Africa, eighty percent of contraceptive supplies are provided by international population assistance (Ross, Weissman, and Stover, 2009). Such funds are not always reliable, as they are subject to the whims of donors, as in the case presented here. Further, the debate continues regarding whether international aid on the whole should be dramatically increased or should be scaled back, due to detrimental impacts and aid dependance (See Easterly and Williamson 2011; Moyo 2009). For these reasons, and because poor countries struggle with how to allocate their own funds, it is useful to understand the potential impacts (or lack thereof) of changes in funding for contraceptive provision.

If we assume that a woman has a desired fertility target, use of modern contraceptives is only one way of achieving that target. Traditional methods may also prevent pregnancy.[2]

[1] Based on the most recent Demographic and Health Surveys surveys from 41 countries in this region, average wanted fertility is 4.1 children per woman, whereas average realized fertility is 5.1 children per woman. Excess fertility, defined as the difference between wanted and realized fertility, is 1.3 children for the poorest (and for those with no education) and 0.6 children for the least poor (and those with secondary or higher education)(MEASURE-DHS, 2013).

[2] Modern contraception includes hormonal methods (e.g. pill, implant, injection, IUD), barrier methods (e.g. condom, diaphragm, sponge, spermicide), and permanent methods (i.e. sterilization). Traditional

In some contexts, induced abortion can also be used to prevent unwanted births. It thus remains an open question whether supply-side changes in contraceptive availability will directly translate into changes in fertility. A women may adjust her use of these other options in order to compensate for insufficient supply, leaving fertility unchanged. Or, if women are unable or unwilling to do so, fertility outcomes may respond to supply. Such behavioral questions are interesting theoretically, but also have significant implications for designing policies to achieve fertility transition and long-term development in Africa.

Equally interesting are questions regarding how such behavioral responses will differ depending on different demographic characteristics. What factors affect the degree to which a woman will engage in compensating behavior and thereby achieve her target regardless of supply? Are educated women better at this? Are rural women less willing or able to do so? Answers to these questions indicate which groups are most affected by supply-side factors of contraception provision. For example, Pop-Eleches (2010) finds that a drastic increase in the availability of modern contraceptives and abortion in Romania decreased fertility most for the least educated women. To the degree that demographic factors matter for behavioral response, a link may be drawn between contraceptive supply and the characteristics of resulting fertility. This is the first study, to my knowledge, that examines the individual-level behavioral response to broad-based, supply-side changes in contraceptives in the context of Sub-Saharan Africa.[3]

This paper exploits exogenous reductions in rural contraceptive availability to examine women's behavioral responses in terms of fertility decisions and outcomes in the context of Sub-Saharan Africa. The reductions resulted from significant cuts in U.S. funding to foreign NGOs that provide contraceptives. Consistently, the U.S. is the largest provider of such

methods include abstinence, withdrawal, rhythm, lactational amenorrhea and other folkloric methods.

[3]It is noted that several US-based studies have examined the impacts of access to contraception on a variety of outcomes (Bailey, 2006, 2010; Bailey, Hershbein, and Miller, 2012; Myers, 2012). Other studies studies based in the US or Romania have examined the impact of access to abortion (Currie, Nixon, and Cole, 1996; Kane and Staiger, 1996; Cook, 1999; Gruber, Levine, and Staiger, 1999; Pop-Eleches, 2006; Ananat, Gruber, and Levine, 2007; Mitrut and Wolff, 2011) or combined access to contraception and abortion (Guldi, 2008; Pop-Eleches, 2010).

international population assistance (Ashford, 2010). Much of this assistance flows to NGOs, who are primary providers of rural outreach for family planning in many poor countries (Turnbull and Bogecho, 2003). The funding cuts were a result of domestic politics in the U.S., discussed in detail in section 4, and were in place during the periods 1984-1992 and 2001-2008.

The context of the analysis is Ghana, based on the availability of individual-level data on induced abortion, which is necessary for examining women's decisions in the face of supply changes. I first provide evidence that contraceptive availability was reduced during policy periods and that rural contraceptive use was impacted as well. These findings are consistent with reports that the primary impact of the budget cuts was to reduce outreach for contraceptive provision in rural areas.

I then create a woman-by-month panel to examine the impact of these changes on individual fertility outcomes. Estimating within-woman, I find that conception significantly increased during policy periods for rural but not urban women. These results are concentrated in regions that experienced the greatest policy-induced reductions in contraceptive use. I also find an increase in the use of induced abortion in rural areas, but only for women in the top three wealth quintiles. The poorest women were either unable or unwilling to offset the increase in pregnancies with induced abortion. As a result these women experience increases in fertility that did not occur among other groups.

The following section reviews the debate on the role of contraceptive access in reducing unwanted fertility. Section 3 sets up a conceptual framework that predicts differential impacts across demographic groups. Section 4 gives background on the U.S. policy that provides exogenous variation for this study, with a focus on the policy's repercussions in Ghana. Evidence of reductions in contraceptive availability and use are presented in Section 5. Section 6 examines impacts on conception and abortion, and section 7 examines changes in resulting fertility. Section 8 discusses the results and their implications and concludes.

2 Existing Evidence

Becker (1993) argues that the major changes in fertility have been caused by changes in demand for children, rather than birth control methods. In agreement, Pritchett (1994) claims that a precedent exists for fertility transition without modern contraception. He cites crude birth rates in Europe around 1800 that were lower than the average rate in low-income countries in 1990. However, the 1800 rates cited are far from replacement fertility, and are in fact comparable are to rates in low-income countries today.[4]

In Romania and Ireland, fertility declined 30% following the legalization of contraceptives. However, abortion legalization in Romania, and changing social norms in Ireland were contemporaneous with contraceptive legalization, making it difficult to assign causality (Pop-Eleches, 2010; Bloom and Canning, 2003). More experimental evidence in the US suggests that contraceptive subsidies reduce fertility by a significant, but small, 2% (Kearney and Levine, 2009).

Experimental evidence on the impact of contraceptive access in low income countries begins with the 1978 Family Planning and Health Services program in Matlab, Bangladesh. Sinha (2005) estimates that women exposed to the program had lifetime fertility reduced by 14%. However, it is unclear how much of that impact is attributable to the family planning aspects of the program, rather than the introduction of other health services (Phillips et al., 1982). Smaller impacts are found in Colombia by Miller (2010), who estimates that the expansion of a large, government family planning program reduced fertility by 6-7%. Grant deems this a negligible change in total fertility. His view is consistent with Pritchett (1994), who claims that contraceptive prevalence has a significant, but negligibly small effect on realized fertility, as preferences account for 90% of differences across countries' fertility rates. In response, Bongaarts (1994) argues that reductions in fertility of 5-10% are not little, but rather, they represent a meaningfully large share of *unwanted* fertility.

[4]Europe in 1800 averaged 30 births per 1,000 population (Pritchett, 1994). Low income countries in 1990 averaged 41. By 2011, low income countries' average rate was 32; OECD countries average rate in was 12.2 (World Bank Development Indicators,2013).

Other works based on experiments or natural experiments have provided evidence on both sides of the argument. Molyneaux and Gertler (2000) instrument for family planning program placement in Indonesia and find that the reach of the supply network does have a significant impact on fertility. On the other hand, McKelvey, Thomas, and Frankenberg (2012) show that, in the same setting, exogenous variation in prices of contraceptives hardly affects use. An experiment by Ashraf, Field, and Lee (2010) provides evidence that a woman is more likely to use contraception and reduce unwanted births when the contraception is concealable from her husband. Like Pritchett, they argue that preferences play a dominant role in contraceptive use and thus provision of contraceptives is unlikely to change African fertility as long as men have *de facto* control over its use.

3 Conceptual Framework

Consider a production function for fertility

$$F = f\left(T, M, A, \varepsilon\right)$$

where a woman's realized fertility, F, is a function of her effort to prevent pregnancy by traditional methods, T (including abstinence, rhythm, *coitus interruptus*, lactational amenorrhea and folkloric methods), her use of modern contraception, M, her use of induced abortion, A, and an idiosyncratic term that includes both natural fecundity and random error, ε.[5] T, M, and A have negative and unique relationships with F. There are monetary costs associated with M and A, as well as time costs for acquiring them. Further, there are unique utility costs associated with using T, M, and A (which may include partner negotiation). In optimization, a woman will seek to minimize the difference between her realized fertility and her exogenously given target fertility, F^*, while minimizing the utility, monetary, and time costs

[5] In the SSA context, methods for increasing fecundity, such as *in vitro* fertilization, are not generally in a woman's set of options, and are thus excluded from this model.

associated with preventing F from exceeding F^*.[6]

This framework suggests that an increase in the monetary or time cost of M associated with reduced supply will spur re-optimization. If she reduces her use of M, she may choose to increase T and/or A in order to keep F as close to F^* as possible, depending on her utility costs of T, A, and $(F - F^*)$, and the functional form of f. This offers several predictions in terms of fertility outcomes.

Specifically, given an increase in the cost of M, we would expect to see increases in pregnancy for women who meet both of the two criteria:

1. Reduce their use of M, because either
   (a) she is unwilling/unable to forgo consumption to pay the increased price, due to high marginal utility of consumption (i.e. poor women), or
   (b) she is unwilling to commit additional time to acquiring M in the case of stock-outs, due to high opportunity costs (i.e. educated women), and
2. Are unable to fully compensate by increasing T, because either
   (a) her use of T is not effective for preventing pregnancy, due to low knowledge (i.e. uneducated women), or
   (b) her utility cost of using T is high (she or her husband may oppose abstinence/rhythm, *coitus interruptus*, etc.)

We would expect to see increases in realized fertility for women who both:

1. Experience increased pregnancy, and
2. Do not fully offset additional pregnancies with A because either

[6]This characterization assumes that the woman has sufficient fecundity to meet or exceed her desired fertility.

(a) she is unwilling/unable to reduce other consumption in order to purchase an increased amount of A (i.e. poorer women), or

(b) she has a high utility cost of A, due to traditional values (i.e. uneducated women)

The predicted impacts on conception, abortion, and realized fertility by poverty and education group are summarized in Table 1. For non-poor, educated women, we expect no change in fertility as any possible change would be offset with abortion. For non-poor women with low education, we expect no change in conception as they would be likely to continue using M despite the increased monetary and time costs. For poor women with low education, we expect an increase in conception and realized fertility, with no offsetting by induced abortion. Finally, for the small group of women who are poor but educated, the predictions for conception are ambiguous, though we can say that realized fertility will move as conception does, as these women are also unlikely to offset with abortion.

4 Background on the Mexico City Policy

The United States is consistently one of the largest donors of international population assistance worldwide.[7] In 1984, President Reagan issued an executive order that restricted such funding in the following way:

> "[T]he United States does not consider abortion an acceptable element of family planning programs and will no longer contribute to those of which it is a part. ... Moreover, the United States will no longer contribute to separate nongovernmental organizations which perform or actively promote abortion as a method of family planning in other nations." [The White House Office of Policy Development, 1984]

[7]UNFPA (2004). Population assistance is defined as funding to support the provision of contraception and family planning in foreign nations.

This executive order is known as the Mexico City policy, based on its introduction at the International Conference on Population held in Mexico City in 1984. It requires foreign NGOs to sign official affidavits stating that they will not perform, lobby for, or educate clients about safe abortion. If they refuse, they forfeit any and all population assistance provided by the United States Agency for International Development (USAID).[8] Organizations unwilling to sign affidavits were those for which reproductive health and family planning were the foremost objective. Often, these organizations were not performing abortion (as it is illegal in many poor countries), but were providing a considerable share of contraceptive supply. Such groups lost all funding from USAID, amounting to 10-60% of organizational budgets. This included large, international organizations such as International Planned Parenthood Federation (IPPF) and Marie Stopes International (MSI), as well as small local NGOs such as Family Guidance Association of Ethiopia and Family Planning Association of Kenya (Turnbull and Bogecho, 2003).

Funding shortfalls resulting from lost USAID funds took effect in early 1985. The policy remained in effect until it was repealed by President Clinton in January, 1993. It was reinstated by President Bush in January, 2001.[9] Despite many Congressional votes on the matter, the policy remained in effect until it was rescinded by President Obama in January, 2009.[10] In the interim period 1993-2000, when the Mexico City Policy was not in effect, the U.S. provided nearly 40% of population assistance worldwide (UNFPA, 2004). On average, about half of that funding flowed to non-governmental organizations (PAI, 1999).

[8]At the time of the policy's creation, and still today, abortion on-request is not legal in many countries that receive US population assistance. Further, the 1973 Helms Amendment already forbade the use of U.S. monies for that purpose. Therefore, it was the forbidding of organizations to use their own funds to educate women about safe abortion options or lobby the government for legalization that earned the policy the derisive nickname "the global gag rule."

[9]The policy was extended to apply to State Department funds as well in August, 2003.

[10]For Presidents Clinton, Bush, and Obama, their change to the policy's effectiveness was issued on the first or second day following inauguration. It has been the concern of several major court battles, one of which ended in the Supreme Court; and at least twenty congressional debates or votes have been taken on the matter (see Appendix Table A.2). It's potential reinstatement in 2011 was one of the "policy riders" that created a roadblock in the Congressional budget negotiations, nearly shutting down the federal government.

Advocacy groups consistently report that the primary impact of the policy was to force cutbacks in rural outreach services, reducing access to contraceptives in rural areas. A recent evaluation of the policy by Bendavid, Avila, and Miller (2011) examined its impact on abortion use. They employ cross-country data and an algorithm to infer abortions and conclude that women in high-exposure countries increased their use of abortion following the 2001 reimplementation of the policy.[11]

Repercussions of the policy in Ghana

Information regarding NGO funding prior to the 1984 implementation of the Mexico City policy is not readily available. However, the situation surrounding the re-imposition of the policy in 2001 provides some insight regarding the policy's effect. USAID documents from late 1999 list funds slated to specific NGOs, by country, for the 2001 fiscal year. The total per country slated to foreign-based reproductive health NGOs represents the funds at risk for loss following the 2001 re-imposition of the policy (see Appendix Table A.1). In Ghana, such organizations were slated to receive nearly $800,000 in FY2001. That is approximately the median for countries receiving such funding, suggesting that Ghana is a fairly representative case (USAID, 1999).

Planned Parenthood Association of Ghana (PPAG) was (and is) the leading NGO-provider of reproductive and sexual health services in Ghana. As of late 1999, PPAG was slated to receive $472,952 from USAID in 2001 (USAID, 1999).[12] Upon the executive order in January 2001, these funds would only be disbursed if the organization agreed to the Mexico City policy.

[11] My findings are consistent with theirs, though my methodology differs in a number of ways: (i) I focus on one country, rather than many, which allows me to employ data on actual induced abortions, rather than estimating abortions via algorithm; (ii) I estimate within-woman rather than across countries; (iii) I estimate a more comprehensive effect of the policy, including the first 3 changes in the policy, rather than just the 2001 reimplementation; (iv) I show evidence that the pathway of the effect on abortion is indeed increased conception; and (v) I investigate the impact on fertility outcomes more broadly, including conception rates and fertility rates.

[12] The remainder of at-risk USAID funds slated for NGOs in Ghana were for Ghana Social Marketing Foundation, which also primarily provides contraceptives.

Under normal circumstances, the majority of funding for PPAG comes from the International Planned Parenthood Federation (IPPF). However, at this time, USAID was funding a large Community-Based Services (CBS) project through PPAG. As such, USAID was slated to provide 1/4 of PPAG's budget for FY2001. The CBS project was scheduled to run through 2003 and in order to preserve this project, PPAG agreed to the MCP to keep its USAID funding (Turnbull and Bogecho, 2003; IPPF, 2002).

However, from 2001 to 2003 PPAG did experience significant budget losses, as its funding from IPPF was reduced by 54% (reducing the total budget by 40%) (IPPF, 2002). As IPPF had refused to sign the policy, it had experienced budget cuts. Out of necessity, these were passed on to its member organizations.[13] In 2003, at the conclusion of the CBS project, PPAG rejected the policy and lost USAID funding (and in-kind donations of contraceptives) in addition to previous budget cuts from IPPF. Funding from IPPF did not fully recover until after the repeal in 2009 (see Appendix Table A.3). Many PPAG clinics were closed or consolidated during this period, especially those in rural areas, as shown in the before-and-after location maps in Figure 1.

Advocacy groups have claimed that the funding losses resulting from this policy primarily impacted the availability of contraceptives to poor, rural populations, rather than the provision of abortion services (Cincotta and Crane, 2001; Crane and Dusenberry, 2004). In particular, a report states that in Ghana, "the major cutbacks in PPAG staff and the loss of its community-based distributors have limited its outreach capabilities, particularly in the most remote areas of Ghana" (Turnbull and Bogecho, 2003). Such claims are supported by information received directly from PPAG, which shows that contraceptive provision via community-based distribution in rural areas, as measured in couple-years of protection, dropped by 45% as a result of funding losses (PPAG, 2012). While provision of post-abortion

[13] Prior to the 2001 re-imposition of the Mexico City policy, USAID was providing 7.3% of income for IPPF (IPPF, 2002). It's not clear why cuts to PPAG were so large relative to IPPF losses. Perhaps IPPF's funding cuts to member organizations were inversely proportional to the unilateral budget cuts suffered by the member.

care increased, no indication was given that availability of abortion services was impacted.[14]

5 Changes in Contraceptive Availability and Use

Surveys of government, private, and NGO providers of family planning services in Ghana were undertaken in 1993, 1996 and 2002.[15] A comprehensive report based on these surveys suggests that contraceptive availability was lower during the years the policy was in effect, as shown in Table 2 (Hong et al., 2005). The availability of contraceptive methods (weakly) increased for five out of six methods when the policy was rescinded (in 1996 vs. 1993), and decreased for five out of six methods after reinstatement (in 2002 vs. 1996). Surrounding the reinstatement, the magnitudes of decreases in availability range from 7% to 19% across contraceptive methods (13% on average).

Ghana Demographic and Health Surveys (DHS) regularly collect information on reproduction and health for a nationally representative, cross-sectional sample of women aged 15-49 (GSS, GHS, and ICF, 2009). Data from the 1998 DHS indicate that, of women having ever used contraception, 58% were rural and 44% were acquiring them from private providers such as PPAG. Given a national reduction of 13%, these suggest that reductions in rural provision by private providers was on the order of 50%, as reported by PPAG, and total rural provision was reduced by 22%.

To better examine heterogeneities across regions, I estimate changes in contraceptive use, as a proxy for changes in availability.[16] The data available for this exercise come from the DHS, in which contraceptive use is reported only as current use at the time of the interview,

[14] Abortion has been legal in Ghana, with significant restrictions, since 1985. Since a change in the legal status of abortion coincided with one of the policy changes of interest, estimates of the policy's impact on abortion use were also run excluding the 1985 policy change. These results do not differ from the results presented here, and are available upon request.

[15] In 1993 and 1996 by the Population Council's Africa Operations Research and Technical Assistance Project. In 2002 by Macro International as part of the MEASURE DHS+ project.

[16] The data used by Hong, et al. are no longer available, so no further analysis of changes in availability over time can be undertaken. The 2002 Demographic and Health Survey Service Provision Assessment by MACRO is available but cannot be used to see changes over time. The data from the Situation Analysis Studies in 1993 and 1996 are no longer available; they were corrupted while being stored on floppy disks (Arnold, 2013).

and whether the respondent has ever used.[17] While DHS data are available from 1988, 1993, 1998, 2003 and 2008, five data points are hardly sufficient to estimate the impact of the policy in a region.

A useful stylized fact is that, while many Ghanaian women never use modern contraception, the ones that ever do generally begin using it after the first birth. This is evident in the data on 24,500 women from the five surveys described above. Fig. 3 shows ever-use rates by parity for women in each of the five surveys. Among women in the three most recent surveys, there is a steep increase in ever-use following the first birth, while increases following subsequent births are negligible. Specifically, only 23% of nulliparous women were ever-users, this nearly doubles among uniparous women (42%) and the maximum ever-use rate among multiparous women is only slightly higher (46% at parity 4). The pattern is consistent but somewhat muted in the two earlier surveys (a doubling followed by a 30% increase).

I take advantage of this fact to use the timing of the first birth as an indicator for exposure to the policy in terms of contraceptive use. That is, I estimate whether a woman has ever used contraception at the time of her interview as a function of whether the policy was in place at the time of her first birth. I estimate the linear probability model

$$CPT_{iy} = \alpha_0 + \alpha_1 ON_{iy}^{FB} + X_{iy}^{'}\phi + M_{iy}^{'}\theta + \nu_y + \varepsilon_{iy} \quad (1)$$

where CPT_{iy} indicates that woman i has ever used modern contraception at the time of her interview in year y; ON_{iy}^{FB} indicates that the policy was in effect at the time of the first birth to woman i (from survey y); M_{iy} is a quadratic time trend (for the birth timing); and ν_y is a survey fixed effect. X_{iy} contains characteristics of woman i in year y, such as age, parity, ever-married status, poverty (defined as two poorest wealth quintiles specific the rural or urban sector), and low education (primary school or less). This is estimated for the full

[17] Although some DHS surveys include contraceptive calendars for the 5 years prior to the interview, none of those in Ghana include this module. The author is not aware of any other data that include information on contraceptive use in Ghana.

sample, for rural and urban sectors, and for each region individually.

Since the only outcome available is "ever-use," estimates of α_1 will be underestimates of the policy's impact on contemporaneous contraceptive use. While contraceptive use may have been prevented by exposure to the policy at first birth, some women may have begun use after the removal of the policy but before the survey, depending on the timing. This would result in an affirmative answer for ever-use at the time of the survey, even for women whose contraceptive use was prevented during the policy. Thus, the estimates for reductions in contraception are primarily useful for the assessing regional heterogeneities, and should be interpreted cardinally only as an extreme lower bound.

Table 3 provides evidence that exposure to the policy at the time of first birth reduces the probability of having ever used contraception at the time of the interview. Overall, exposure reduces ever-use by 2 percentage points, about a 5% impact. However, the impact is concentrated among rural populations, where the impact is 7% to 8%. This is consistent with reports that contraceptive access in rural areas depends on the outreach services provided by groups such as PPAG. It is also consistent with PPAG's own report that funding cuts translated directly into the loss of rural distribution of contraceptives (Nerquaye-Tetteh, 2012).

The exclusion of woman characteristics, the time trend of the year of first birth, or the survey fixed effect do not measurably change the point estimate, which ranges from $-.023$ to $-.028$. In the two final columns, I check for differences according to a woman's wealth or education. In both cases I find that effects are not statistically different across groups; the point estimates indicate effect sizes of about 6% for the poor and uneducated and 8% for others.

In addition to differences across rural and urban sectors, I also examine heterogeneities across the ten administrative regions of Ghana. I estimate the equivalent of column 3 from Table 3 separately for each region, as shown in Table 4. With samples sizes ranging from 200 to 1150, only two regions have estimates that are statistically different from zero. These are

Volta and Ashanti, both of which experienced reductions in contraception ever-use greater than 15%, and are classified as high-impact regions. Western, Central, Brong-Ahafo, Upper East and Upper West all have point estimates suggesting reductions in contraception ever-use between 5% and 10%, and are categorized as medium-impact. Finally, three regions have positive point estimates and are classified as low-impact; these are Greater Accra, a densely populated urban area, its neighboring region, Eastern, and the Northern region, which has extremely low ever-use rates (12.5%; the next lowest region has more than double this rate). These categorizations will serve to examine differential impacts by policy intensity in the following section.

6 Impact on Conception and Abortion

6.1 Data

The data used in the preceding section are from standard DHS surveys collected every five years in many developing countries by Macro International's MEASURE project. In 2007, MEASURE conducted a non-standard DHS survey in Ghana composed of special modules on maternal mortality and abortion (GSS, GHS, and Macro, 2009). Unlike most DHS, which collect a woman's complete birth history, this survey queried each woman's complete *pregnancy* history, including pregnancies that ended in miscarriages, stillbirths and induced abortions. While a handful of other DHS also collect pregnancy (rather than birth) histories, the Ghana 2007 survey is the only one that explicitly records the use of induced abortion.[18] These data will be useful for determining if the supply changes resulting from the policy impacted pregnancy and abortion use at the individual level.

The data contain information for 10,370 women. For each pregnancy in a woman's lifetime, the following information is recorded: the duration of the pregnancy, the month

[18] Further, other surveys conducted after 2001 that include pregnancy histories are in countries unlikely to be as affected by the Mexico City policy: Armenia 2005, Azerbaijan 2006, Moldova 2005, Philippines 2008, and Ukraine 2007.

and year it ended (from which one can roughly deduce the month it began), how it ended, and further information about the child if it was a live birth. Using this, I create a woman-by-month panel. In each month, a woman has one of the following seven statuses: conceived, is pregnant, birthed a live child, had a stillbirth, miscarried, aborted a pregnancy, or was not pregnant. Moving consecutively through the months, summing the live births, I calculate her existing parity (number of children previously born) in each month. The survey also collects information regarding the woman's date of birth and month and year of first marriage (or cohabiting union). Using these, each woman-month observation is assigned the woman's age and parity and whether or not she has ever been in union at that time. Months in which the woman is at least 15 years of age compose the complete data set. There are 1.8 million observations from 1981 to 2007. Each woman has between 1 and 323 observations (mean is 144; only 5% of women have fewer than 10 observations).

Other information collected about the woman does not vary over time, but is useful for dividing women into demographic subgroups. A wealth index for her household is created based on a principle components analysis of information about housing quality, drinking water source, toilet facilities and durable assets (Filmer and Pritchett, 2001). From this, women are classified by wealth quintiles, specific to rural and urban sectors. While wealth may vary throughout a woman's life, it seems that wealth *quintile* is likely somewhat stable. Additionally, the data include educational attainment. This too is measured at the time of the survey only, so I classify women according to whether or not they had any schooling beyond primary school (grade 6), an indicator that is likely unchanging by age 15.

Balancing the panel A primary concern in creation of panel data from cross-sectional data is the loss of representativeness. The survey is nationally representative of women aged 15 to 49 in 2007 (mean age is 29). For each woman, her panel begins when she turns 15 (or in 1981 if she is already 15 or older) and ends when she is interviewed in 2007 (max age is 49). However, the resulting panel is not representative of women of these ages for each year.

For example, in 1981, the data only contain women aged 15 to 24 (mean 18); in 1997 they contain women aged 15 to 40 (mean 25). This "aging" of the sample confounds comparisons of births from different time periods. In order to limit the bias arising from the conversion of cross-sectional to panel data, I limit the age range of women in the sample. This effectively creates a "rolling panel" where women age into and out of the sample over time. But what is the appropriate age range for this analysis?

Figure 2.A shows the conception rate by age; that is, the share of fecund woman-months in which a conception occurred. The conception rates are highest (over 2.5%) for women aged 22 to 27. A gradual decline begins around age 28, becoming steeper at age 37. For women younger than age 17, or aged 40+, the chance of conception in a given month is less than 1%. Figure 2.B shows the abortion rate by age; that is, the share of pregnancy conclusions that are abortions. The likelihood of aborting a pregnancy is greatest for the youngest women; over 15% for 15 year olds. However, considering their low number of pregnancies, this represents a small share of total procedures. The likelihood of aborting a pregnancy declines with age, generally remaining below 5% for women over age 25. Figure 2.C shows the confluence of likelihood of conception and likelihood of aborting an existing pregnancy, that is, the probability of having an abortion, by age. The combination of high conception rates and high abortion rates yield the greatest chance of having an abortion for women aged 18 to 20: about 2% per year (.0018*12). Women outside the 17 to 25 age range have a considerably lower probability: less than 1% per year.

Given the focus of the analysis on conception and abortion decisions, I select the 17 to 25 age range as the default for the analysis, varying this in robustness checks. In order to keep the sample consistent, I employ data only from months in which the sample is representative of this age range (1981 to 2007).[19] These data are divided according to policy periods:"PRE" from 1981 to 1984, "ON 1" from 1985 to 1992, "OFF" from 1993 to 2000, and "ON 2" from 2001 onward. Appendix Table A.4 shows how the mean age for the full sample is significantly

[19]While the respondents report pregnancies as far back as 1977, the panel used for this analysis begins in 1981, for reasons of representativeness as discussed.

increasing over the periods. The last two columns show that when restricting observations to those for women aged 17 to 25, the mean age is much more similar across periods. Further restricting to the group in which most abortions occur (18 to 20 year olds) produces mean ages nearly identical across periods.

There are 8,334 women that are observed while aged 17 to 25. The conception analysis includes all of these women, in each month when she is in this age range. Of the 7,489 women that are ever-pregnant in the sample, 91% had a pregnancy while aged 17 to 25, yielding an effective sample size for the abortion analysis of 6,818 women.[20] However, it is useful to note that, when employing woman-fixed effects, the identification of policy effects on abortion use arises from women who have at least two pregnancies in that age range, with variation across the pregnancies in the status of the policy. 68% of the effective sample have at least two pregnancies during the nine year period when aged 17 to 25.[21] This potentially introduces some selection bias, so it is important to note that the estimated effects are specific to women having two or more conceptions during that period of life. The source of identification is further reduced by the fact that only 50% of these women have at least one pregnancy during a policy period and at least one during a non-policy period. However, while this reduces the size of the sample used for identification, it does not introduce any further bias. For each woman, the timing of this nine-year period in her life is orthogonal to the imposition and removal of the policy. Appendix Table A.5 shows the effective sample sizes for the analysis of conception and abortion use.

Measurement error One concern regarding these data is noisy measurement of pregnancy timing due to recall error. One expects that reporting accurate dates for pregnancy conclusions is more difficult for events further in the past. Less recent pregnancies will therefore have a higher probability of incorrect reporting of the month of the conclusion; in some cases even the year of conclusion may be incorrect. While the selection of respondents that

[20] This figure is 93% for the rural sample.
[21] 72% in the rural sample.

misreport may be non-random, we expect that the direction of mis-reporting (too early or too late) will be random. As such, the direction of mis-classification of pregnancies (regarding whether they were conceived under the policy or not) will also be random. Therefore, this introduces classical measurement error, which will attenuate the estimates toward zero.

Another artifact of recall error is that less recent pregnancies are more likely to be unreported due to forgetting, especially if the pregnancy ended in miscarriage, stillbirth or abortion. Therefore, I include a fixed effect for the policy change nearest in time to the month of observation. In this way, observations are directly compared only with others within the same eight-year time span, so that misreporting should be more consistent within these groups. However, even estimating within-period, this recall bias may still yield an artificially lower rate of conception or abortion in earlier years. For periods surrounding a policy change from on-to-off (off-to-on), this could artificially associate the policy with lower (higher) conception and abortion rates. For this reason, it is important that the analysis here includes both an on-to-off period, as well as off-to-on periods. As a robustness check, I restrict the analysis to just one of each type of period and the results are not significantly different.

Summary statistics Table 5 presents summary statistics regarding conception and abortion rates for various demographic sub-samples. In addition to disaggregation by rural/urban status, the rural population is divided into wealth and education groups. The two lowest rural wealth quintiles are labeled "poorest." It is notable that 70% of those in the top three rural quintiles are still poor by international standards (less than $2/day), and so this group is labeled "less poor." The population is also divided into those with any schooling beyond primary school ("high education," 39%), and those without ("low education," 61%).

Overall, for women aged 17 to 25, the probability of conception in a given month is .022. This differs significantly between rural and urban populations (.025 vs. 018), but differs less between the rural sub-groups shown. However, interesting differences across rural sub-groups exist in use of both contraception and abortion. While 47% of rural women have ever used modern contraception, this figure is 28% for the uneducated poor and 62% for the educated less-poor. Across all rural women, 4.8% of pregnancies are aborted. However, the uneducated have rates of 1.6% and 3.7%, depending on wealth, whereas the educated use abortion at a much higher rates (7% and 9.7%). On the whole, poorer and less educated women are less likely to have ever used modern contraception and are less likely to end unwanted pregnancy with abortion as compared to other rural women.[22]

6.2 Estimation

In estimating the impact of changes in contraceptive supply on conception and abortion, one concern is the degree to which conception and abortion are affected by environmental and situational concerns beyond the change in supply. For example, birth rates fluctuate in tandem with business cycles, as couples are more reluctant to have children during recessions (Kirk and Thomas, 1960). Fertility decisions are also affected by seasonal changes. For this reason, it is important to control for other, unobservable factors changing over time. To deal with seasonality, I include calendar month fixed effects. However, because the imposition (or removal) of the policy always coincided with the change in calendar year, year fixed effects would be perfectly collinear with an indicator for the policy. I employ several alternatives to deal with this concern. First, I include a cubic time trend in all specifications. Second, I focus the estimation on a fixed time window on either side of each policy change to reduce the impact of time-varying unobservables. The default window includes observations within four years of a policy change, and this is narrowed in robustness checks. Third, I include a fixed-effect for the policy change that is nearest in time, effectively estimating within-change. For example, an observation occurring in January 1990 is nearer the 1993 policy change than

[22] These statistics may also reflect a differential willingness to report abortion. If poorer or less educated women perceive a greater stigma associated with abortion, they may be less willing to report in general. Lower levels of reporting would act similarly to low levels of use, in that it makes any effect of the policy more difficult to detect. Note, however, that this does not bias the results given the inclusion of woman-fixed effects.

the 1985 policy change. In this way, I compare observations just before a change to those just after it, rather than comparing observations from, say, 1982, to those from 2004.

Finally, in order to control for the host of unobservable characteristics about each woman that certainly affect such decisions, I employ woman-fixed effects to compare each woman only with herself. Further, because a woman's preference for having a child changes throughout her life, I include controls for time-varying characteristics that strongly predict conception and childbirth: quadratic functions of her age and parity (previous number of births), and whether she has ever lived in union with a man.[23]

The primary estimations are

$$C_{imyc} = \beta_0 + \beta_1 ON_{my} + X'_{imy}\phi + M'_{my}\theta + \nu_i + \nu_m + \nu_c + \varepsilon_{imyc} \quad (2)$$

$$A_{imyc} = \gamma_0 + \gamma_1 ON_{my} + X'_{imy}\phi + M'_{my}\theta + \nu_i + \nu_m + \nu_c + \varepsilon_{imyc} \quad (3)$$

where C_{imyc} indicates that woman i conceived in month m of year y. The index c takes the value 1, 2, or 3, representing which policy change is within the fewest months of my. Specifically, $c = 1$ represents the change in late 1984 from PRE to ON1, including observations in the window from 1981 through 1988. $c = 2$ represents the change in January 1993 from ON1 to OFF, including observations from 1989 through 1996. $c = 3$ represents the change in January 2001 from OFF to ON2, including observations from 1997 through 2004. X_{imy} is a vector containing quadratic functions of age and parity specific to woman and month, plus an indicator for whether she has ever been in a cohabiting union. M represents a cubic time trend. Fixed effects for the individual, the calendar month, and the nearest policy change are included as ν_i, ν_m, and ν_c, respectively. $A_{imyc} = 1$ indicates that the pregnancy of woman i that ended in month m in year y was aborted. $A_{imyc} = 0$ for all pregnancies ending in

[23]Note that having ever been in union will be equal to zero for a woman in all months preceding her first union and will be equal to one in all months following that. Note also that the results do not depend on the inclusion of these controls, as show in section 6.3.

live birth, stillbirth, or miscarriage. For the estimation of equation 3, all woman-months are included when the woman is concluding a pregnancy while aged 17 to 25.

The independent regressor of interest is ON_{my}, which indicates that the policy was in effect in month-year my. β_1 and γ_1 indicate the estimated impacts of the policy on conception and abortion, respectively, of the sub-group for which Eq. 2, or 3, is estimated, conditional on age, existing parity, ever-unioned status, and the secular trend.

6.3 Results

Conception

Table 6 provides estimates of the policy's effect on the probability of conception for women aged 17 to 25.[24] Overall, the probability of conception per month increased by 0.0011, representing a 5.6% increase that is significant at the 5% level. Results for the urban population are not significantly different from zero, consistent with the findings on contraceptive use in section 5. This reflects the fact that contraceptives are more broadly available in urban areas, so that changes in NGO provision are less salient.[25] For the rural sample, however, the probability of conception per month in rural areas increased by 0.0022 when the policy was in place. This represents a 10% increase in pregnancies and is statistically different from zero with 99% confidence. Recall that the estimated change in ever-use of contraceptives as a result of the policy was 7% for rural areas, which serves as a lower bound for the effect on contemporaneous use. Thus, it seems feasible that the true change in contemporaneous use could result in a 10% increase in pregnancy.

The conceptual framework laid out in section 3 suggests that, even within the rural sector, we may observe differences in these impacts across wealth and education groups. Columns 4 - 7 of Table 6 explore these differences. Cols. 4 and 5 estimate using only the Poorest two

[24]None of the estimates of the policy's impact on conception is different when restricting the sample to months when a woman is not already pregnant or concluding a pregnancy. Results available upon request.

[25]Four urban sub-groups are explored and none shows significant changes in conception as a result of the policy (results not shown).

quintiles, where Col. 5 adds an interaction term to examine the differential impact of the policy according to education. Cols. 6 and 7 are parallel estimations using the Less Poor sub-sample. The linear combination effects for the educated groups are reported below the estimated coefficients.

I find that conception increased by 10% for women with low education, regardless of wealth. This was expected for the uneducated poor, confirming that they are both unable to pay the increased cost of contraception and unable to effectively prevent pregnancy with traditional methods. However, this is surprising for the uneducated less-poor and suggests that the assumption that the less-poor would absorb the price increase was incorrect. For women with more education, there was no clear prediction for changes in conception. I find that conception increased 11% for the educated less-poor; this would provide support for the assumption that stock-outs increased the time required for accessing supply, which is a greater opportunity cost for educated women. However, given that the monetary price increase acted as a barrier for less-poor women too, I cannot determine whether the time cost is also a barrier or not. Conception increases by these women also imply that either i) these couples have high utility costs for using traditional methods, or ii) traditional methods are not effective at preventing pregnancy, even for educated women.[26] For the educated poor, I cannot reject that the impact on conception was zero, and this is not consistent with the model's predictions. Drawing conclusions from this comes with the caveat that this is a small and highly selected group of individuals (8% of rural population). Nonetheless, this may suggest that effective use of traditional methods takes both education and practice, the latter being something poor women are more likely to have than their richer counterparts.

In total, the 10-11% increase in conception estimated for 92% of the rural population is fairly consistent with the evidence in section 5, which indicates significant reductions in

[26]Unfortunately, there are no data, to my knowledge, that track individuals' use of traditional or modern contraceptive methods over a time period relevant to these policy changes. Standard DHS surveys query only current and ever use of each method. The only Ghana DHS with a full calendar of contraceptive use was in 2008; however, the calendar covers the preceding five years during which time there were no changes in the policy.

contraceptive use that are not noticeably different across rural demographic groups.

Induced abortion

Table 7 shows the estimates of the policy's impact on the share of pregnancies ending in abortion for the full sample and the same subgroups.[27] For the full sample, the coefficient is positive, though not statistically different from zero at a standard level. The point estimate for the urban population is also positive and imprecise, and we cannot reject that the effect in urban areas is zero.

In rural areas, the estimation suggests that the policy increased the use of abortion by 2.35 percentage points, an estimate significant at the 5% level. Given that only 4.7% of pregnancies are aborted in rural areas, this change reflects a 50% increase in the use of abortion. However, this additional 2.35% of pregnancies aborted only partially offsets the estimated 10% increase in pregnancies estimated by equation 2. That is, of the additional unwanted pregnancies resulting from the policy, 1 in 5 were aborted.

The last four columns of Table 7 present results for rural sub-groups. For the poorest two quintiles of the rural population, shown in Cols. 4 and 5, the effect is not distinguishable from zero, regardless of education status. This is consistent with the prediction that abortion is prohibitively costly for the poorest of the poor. For the Less Poor population, the policy increases the share of pregnancies aborted by 3 to 4 percentage points. For the uneducated, the estimate is significant at the 1% level; for the educated the point estimate is marginally significant (p-value = .115). For the Less Poor, the increase in abortion offsets 30-40% of conceptions that result from the supply reduction. The fact that education status has no discernible effect on the policy's impact on abortion use implies that the assumption that uneducated women would hold traditional values preventing them from increasing use is

[27] What may also be of interest is the unconditional change in occurrence of abortion, that is the probability of having an abortion in any month, regardless of pregnancy status. For the rural population, this probability is only .0009, which makes changes in this low rate very difficult to detect. Nonetheless, I find that the policy increased the unconditional probability of abortion for rural women by .0003, a 33% increase with a p-value of .117. These results are shown by rural sub-group in Appendix Table A.6.

false. Though education is generally a stronger predictor of abortion use than is wealth (see Table 5), using abortion to compensate for reductions in contraception is not driven by educational attainment. The results on conception and abortion by demographic sub-group are summarized in Table 8.

Results by Regional Policy Intensity

To provide suggestive evidence that contraceptive use is the pathway by which the policy impacted pregnancy and abortion rates, I estimate the policy's impact separately by intensity of impact in one's region. I replicate column 3 of Table 6 (Eq. 2 using the full rural sample), and column 6 of Table 7 (Eq. 3 using the rural, less poor sample). To each of these I add interactions between the policy and indicators for medium and low impact regions, as estimated in section 5.

I do find that the increases in pregnancy and abortion increase monotonically with the intensity of policy impact on contraceptive use. In Table 11, the main effect ("policy") represents the impact in (high) regions where the policy induced the greatest reductions in contraceptive use. These regions experienced a 14% increase in pregnancy. The linear combinations of the main coefficient with the subsequent interaction terms are shown in the second panel. These suggest that medium impact regions experienced pregnancy increases of 12%, significant at the 5% level; the impact for low impact regions is not distinguishable from zero. Similarly, the point estimate for change in share of pregnancies aborted is .05 in high impact regions, .04 in medium impact regions, and .03 in low impact regions, all statistically different from zero at the 5% level. Though the estimates are generally not statistically distinguishable from each other, the difference between increases in pregnancy between high and low regions is nearly significant (p=0.101). This provides suggestive evidence that, in regions where the policy had the greatest impact on contraceptive use, it also had the greatest impact on conceptions and abortions.

Specification & Placebo Tests

Varying estimation window In the specifications presented above, I include all years within four years of a policy change (which includes all years 1981 through 2004). However, one might expect that the effect of a policy (or its removal) would be most salient within a narrower time frame. Table 9 shows the estimation of policy impacts on conception and abortion for rural women using successively tighter windows of estimation. The estimates for abortion use are focused on women in the upper three quintiles of wealth, as the poorest of the poor showed no significant effect. I gradually narrow the window by six months at a time (42 months, 36 months, 30 months, 24 months). The smallest feasible window that allows enough women to have at least two pregnancies, and thereby allows the use of woman-fixed effects, employs dates within 24 months of a policy change. The estimates show that as the window is narrowed, the effect becomes gradually larger for estimates of abortion use. For conception, estimates remain in the neighborhood of the original estimate of .0022.[28]

Robustness checks During the period of this analysis, there were other policies changing in Ghana that may have affected fertility decisions. In particular, in 2003 the government introduced a national health insurance scheme (NHIS) that made all prenatal and maternity care available free of charge. To check whether this policy is affecting the results presented here, I include an indicator for the presence of the NHIS in the specification shown in columns 1 and 8 of Table 10. Its inclusion does not affect the point estimate in magnitude or precision for either conception or abortion.

Another potential concern is that women are both more likely to conceive and more likely to abort as they age through the 17 to 25 years-of-age period. Therefore a policy change from off to on, viewed in isolation, will mechanically indicate that the policy increased both conception and abortion. Since the primary estimates presented here include two changes of this nature, and only one of the opposite nature, one may be concerned about this mechanical

[28] For conception estimations employing only months when a woman is not already pregnant or concluding a pregnancy, the coefficients do increase as the window is narrowed. Results not shown.

bias. Therefore, I restrict the estimation sample to the time windows surrounding just the first two policy changes, so that the sample is balanced in terms of direction of policy change. Columns 2 and 9 show the estimation including only the years surrounding the changes in 1985 and 1993 and the estimated effects are not significantly different than the original estimates.

In section 6.1, I discuss the need to restrict the age range of women in included observations. The default age range is 17 to 25, based on the natural breaks in abortion use on either side of this range. Columns 3 (and 4) and 10 (and 11) of Table 10 present results under the larger age range of 16 to 26 (and 15 to 27). None of these differs significantly from the primary estimation. Finally, columns 5 and 12 present the estimation without employing the sampling weights and columns 6 and 13 estimate without the woman-level controls for age, parity, and marital status; the estimates are not significantly different from the original point estimates and remain statistically significant.

Placebo test Perhaps there is something about U.S. presidential elections that affects fertility outcomes in Ghana by some mechanism outside the Mexico City Policy. Perhaps elections affect business cycles in the U.S., which affect economies elsewhere. Or perhaps there are expectations in the months approaching an election regarding what *might* change about the policy. In order to test for these possibilities, I take advantage of the 1988 U.S. presidential change-over (from Reagan to G.H. Bush) that did not change the policy in any way. I estimate the effect of the 1988 election on conception and the use of abortion and find effects that are not statistically distinguishable from zero (columns 7 and 14 of Table 10).

7 Net Impact on Fertility

The total fertility rate (TFR) of a population in a given year is the average number of live births a woman would have in her lifetime if she were to experience the given year's age-specific fertility rates (ASFR) for each age throughout her lifetime.[29] This is the summation of the single-age ASFRs in the given year. In order to calculate the TFR of a population, one needs a representative sample of reproductive-aged women in the year of interest.

MEASURE DHS has collected fertility information on representative samples of Ghanaian women aged 15 to 49 in 1988, 1993, 1998, 2003, and 2008. These data are repeated cross-sections, but collection of retrospective birth histories allows for the creation of a woman-by year panel, similar to that described in section 6. This allows for a reasonable calculation of the TFR for women aged 15 to 45 in Ghana for the years 1984 to 2007.[30] Note that, while the fertility histories of respondents stretch back to 1951, the first year in which the sample is representative of women aged 15 to 45 is 1984.[31]

A longer panel of fertility rate history can be achieved by focusing on age-specific fertility rates. Given the focus in section 6 on women aged 17 to 25, I also calculate the ASFR for this age group. The 17 to 25 ASFR for a given year is the number of children a woman would have while she was aged 17 to 25 if she experienced the relevant single-age ASFRs for the given year at each of those nine ages in her life. This can be calculated from 1965 to 2007, as women in this age range in 1965 would be fully represented in the 1988 survey (as women aged 41 to 49). I calculate the 17 to 25 ASFR separately for each of the eight demographic subgroups created by pairwise combinations of poverty and education status for both rural and urban sectors.[32]

For all women, there is a clear declining secular trend in fertility from 1965 to 2007 (see Appendix Table A.7). Therefore, an estimation of the impact of the policy on fertility rates must carefully account for this, including as many years as possible. Focusing on the 17 to

[29] Assuming no mortality of reproductive-age females.

[30] Only survey years are representative of women up to age 49; the interim years are only consistently representative up to age 45. For example, any woman aged 46 or older in 1994 would not be enumerated in 1998, as she would be over 49 at that time. As the births for 1994 are drawn from surveys in 1998 or later, these women's births in 1994 would be excluded.

[31] This is because women aged 45 in 1983, for example, or at younger ages in earlier years, were not included in any surveys as they were past the target age by 1988.

[32] Appendix Table A.7 summarizes the fertility trends in Ghana by rural and urban sectors (since 1989) and by education level within the rural sector for the 17 to 25 ASFR (since 1970).

25 ASFR for demographic sub-groups, I estimate

$$ASFR_{yg} = \delta_0 + \delta_1 ON_y + Y_y^{'}\theta + \varepsilon_{yg} \tag{4}$$

where $ASFR_{yg}$ is the year- and group-specific ASFR for the 17 to 25 year old age range. The estimation includes ASFR for eight groups across 43 years, for a total of 344 observations; estimations are weighted according to the size of each sub-group. ON_y is interacted with indicators for urban, non-poor and high education statues in successive estimations. δ_1 estimates the impact of the presence of the policy on the 17 to 25 ASFR; summations of δ_1 with coefficients from successive interaction terms provide estimates of this impact for specific sub-groups. Y represents a quadratic time trend, and ε is a normally distributed error.

Table 12 shows the results of estimations of variations of Eq. 4. Column 1 shows a positive effect for the full population that is not distinguishable from zero. In Col. 2, we see that the impact on rural fertility is positive and significant, and the impact on urban fertility is negative but insignificant. Col. 3 suggests that the only positive and significant effect is for poor rural women. Col. 4 confirms this, additionally showing that education status does not change the impact among poor, rural women. The estimated effect for each of the eight sub-groups is calculated via summation of the relevant coefficients and presented in Table 13. No significant changes were found among urban women. Positive impacts are estimated for all rural women, but only for poor women are these distinguishable from zero.

For the less poor rural women, estimated increases are 3-6%, which are consistent with a 10% increase in conception, where 3 to 4 percentage points are offset with induced abortion. However, given the standard errors, we cannot reject that these effects are zero.[33] For the poorest rural women, fertility increased by 7.4% for the uneducated and by 10.7% for the educated, net of the secular trend. For the uneducated women, this is broadly consistent with

[33]Large standard errors likely result from noise, as some annual ASFRs are estimated based on as few as 166 women per year of age. In order to calculate a fertility rate, one needs an accurate estimate of the percent of women giving birth in each 1-year age group.

the 10% observed increase in conception, with little-to-no offsetting with induced abortion. As before, the small group of educated poor women present a puzzle: point estimates for both conception and abortion are negative and insignificant, but estimates show an increase in fertility. However, for 92% of the rural population, the findings are both consistent with the conceptual framework proposed and consistent across estimation of different outcomes, even using different data sources.

8 Conclusion

This study has examined Ghanaian women's response to a reduction in the availability of modern contraceptives in terms of resulting pregnancies, use of induced abortion, and resulting births. The exogenous change in availability results from a U.S. policy, driven entirely by domestic politics, that cut funding to NGOs providing reproductive health services in poor countries. These organizations report that the primary change in service provision resulting from the funding cuts was a reduction in contraceptive supplies and outreach in rural areas. These claims are supported by method availability data from clinic surveys, and lower-bound estimations of changes in contraceptive use during policy periods. I find that provision of contraceptives in rural areas fell by 22% during policy periods.

Using reproductive histories that are unusually detailed for a poor-country setting, I find that the vast majority of rural women were either unwilling or unable to fully offset the reduction in modern contraception with traditional methods for preventing pregnancy. These women experienced increases in pregnancies of 10%.

I also examine an additional potentially mediating behavior, that is, whether induced abortion is used to offset increases in unplanned pregnancies. As predicted by the conceptual framework, the poorest women did not increase abortion use. However, women in the upper three wealth quintiles aborted 4 out of every 10 additional pregnancies that resulted from the supply reduction. This compensating behavior did not differ according to educational attainment.

Given that the increase in pregnancy was only partially offset by abortion for some women, and not at all for others, we would expect to see fertility rates increase in response to the supply reductions as well. Using separate data to calculate age-specific fertility rates, I find increases in fertility that are remarkably consistent with the estimates of impact on conception and abortion. The poorest women realize fertility that is 7-10% higher than predicted by the secular trend. The less-poor women experienced increases of 3-6%, though these estimates are not as precise. Taken together, these changes in fertility are consistent with previous estimates of the impact of contraceptive availability on fertility outcomes. Molyneaux and Gertler (2000) estimate that, after controlling for changes in demand for contraception, variation in family planning availability explained between 4 and 8% of fertility decline in Indonesia from 1982 to 1987. Pritchett (1994) estimates that differences in family planning efforts explain about 5% of differences in fertility across countries.

The pattern of differential impacts by demographic group provides some interesting insights. It seems the increase in the price of contraceptives acted as a barrier not only for the poorest women, but for less-poor women as well, as there is no difference in conception impacts by wealth for uneducated women. Given this dominance of the monetary price effect, I cannot identify whether increased time costs required for accessing contraceptives were a significant barrier or not. Further, it seems that education alone does not mean a woman will be successful at preventing pregnancy with traditional methods, as there is no difference in conception impacts by education for non-poor women. However, educated poor women were able to avoid increases in pregnancy, suggesting that a combination of education and experience may improve effectiveness of traditional methods. Finally, the assumption that uneducated women hold traditional values, preventing them from increasing abortion in response to the supply change does not hold; among non-poor women, the uneducated were no less likely to compensate for the supply reduction by increasing abortion use.

The supply of contraceptives in many African countries is heavily depending on donor funding. As shown here, such funds are subject to the whims of donors, and may change unpredictably. While some researchers have argued that contraceptive availability is not a significant factor in fertility outcomes, the results of this study suggest otherwise. In the face of supply reductions, nearly all rural women experienced increased pregnancy. Among those that could afford to respond by increasing their use of induced abortion, even those with *ex-ante* low use rates chose to do so. Increases in realized fertility, relative to the secular trend, arguably represent increases in unwanted or unplanned births. While a 5-10% increase in total fertility may seem negligibly small as some have claimed, in fact, this represents a significant 20-40% increase in unwanted fertility.

The burden of these additional unwanted births fell disproportionately on the poorest women. These are significant impacts of a supply reduction that may have long-term implications. Increasing the use of induced abortion puts women at greater risk of unsafe procedures and long-lasting complications, in a setting that already has the highest maternal mortality in the world. And adding additional unplanned births in poor households strains their ability to raise a healthy and productive next generation.

In Africa, total fertility is higher than anywhere else in the world, and 25% of births are unwanted. International funding for contraceptive supplies clearly has role in changing these statistics, and may have distributional impacts as well. However, the reproductive health sectors in poor countries must eventually develop donor independence to ensure reliability of provision in the longer run. The results presented here suggest that interruptions or reductions in supply can hinder the demographic transition of Africa and result in a poorer, less healthy and less productive population in the future.

References

Ananat, E.O., J. Gruber, and P. Levine. 2007. "Abortion Legalization and Life-Cycle Fertility." *Journal of Human Resources* 42:375 – 397.

Arnold, F. 2013. Email communication with Fred Arnold, Senior Fellow at ICF International.

Ashford, L. 2010. "Resource Flows for International Population Assistance and UNFPA: Background paper prepared for the Center for Global Development." Available online http://www.cgdev.org/doc/Resources-at-UNFPA.pdf.

Ashraf, N., E. Field, and J. Lee. 2010. "Household Bargaining and Excess Fertility: An Experimental Study in Zambia." BREAD Working Paper 282.

Bailey, M.J. 2010. "'Momma's Got the Pill': How Anthony Comstock and Griswold v. Connecticut Shaped US Childbearing." *American Economic Review* 100:98 – 129.

—. 2006. "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Lifecycle Labor Supply." *Quarterly Journal of Economics* 121.

Bailey, M.J., B. Hershbein, and A.R. Miller. 2012. "The Opt-In Revolution? Contraception and the Gender Gap in Wages." *American Economic Journal: Applied Economics* 4:225 – 254.

Becker, G. 1993. *A Treatise on the Family*, enlarged ed. Harvard University Press.

Bendavid, E., P. Avila, and G. Miller. 2011. "United States aid policy and induced abortion in sub-Saharan Africa." *Bulletin of the World Health Organization* 89:873–880, Published Online: October 14 2011.

Bloom, D.E., and D. Canning. 2003. "Contraception and the Celtic Tiger." *Economic and Social Review* 34:229 – 247.

Bongaarts, J. 1994. “The Impact of Population Policies: Comment.” *Population and Development Review* 20:616–20.

Cincotta, R.P., and B.B. Crane. 2001. “Public Health: The Mexico City Policy and U.S. Family Planning Assistance.” *Science* 294:525–526.

Cook, P.J.a. 1999. “The Effects of Short-Term Variation in Abortion Funding on Pregnancy Outcomes.” *Journal of Health Economics* 18:241 – 257.

Crane, B.B., and J. Dusenberry. 2004. “Power and Politics in International Funding for Reproductive Health: The US Global Gag Rule.” *Reproductive Health Matters* 12:128–137.

Currie, J., L. Nixon, and N. Cole. 1996. “Restrictions on Medicaid Funding of Abortion: Effects on Birth Weight and Pregnancy Resolutions.” *Journal of Human Resources* 31:159 – 188.

Easterly, W., and C. Williamson. 2011. “Rhetoric versus Reality: The Best and Worst of Aid Agency Practices.” *World Development* 39:1930–1949.

Filmer, D., and L. Pritchett. 2001. “Estimating Wealth Effects Without Expenditure Data Or Tears: An Application to Educational Enrollments in States of India.” *Demography* 38:115–132.

Gruber, J., P. Levine, and D. Staiger. 1999. “Abortion Legalization and Child Living Circumstances: Who is the ”Marginal Child”?” *The Quarterly Journal of Economics* 114:263–291.

GSS, GHS, and ICF. 2009. *Ghana Demographic and Health Surveys 1988-2008*. Calverton, Maryland: Ghana Statistical Service, Ghana Health Service, and ICF Macro International [Producers]. ICF International [Distributor], 2009.

GSS, GHS, and Macro. 2009. *Ghana Maternal Health Survey 2007 [Dataset].*

GHIQ51DT.DTA. Calverton, Maryland: Ghana Statistical Service, Ghana Health Service, and Macro International [Producers]. ICF International [Distributor], 2009.

Guldi, M. 2008. "Fertility Effects of Abortion and Birth Control Pill Access for Minors." *Demography* 45:817 – 827.

Hong, R., N. Fronczak, A. Chinbuah, and R. Miller. 2005. "Ghana Trend Analysis for Family Planning Services, 1993, 1996 and 2002." DHS Trend Report No. 1, ORC Macro, January.

IPPF. 2002. "Financial Statements: Annual Report of the Governing Council for the Year Ended 31 December 2001." International Planned Parenthood Federation, May.

Kane, T.J., and D. Staiger. 1996. "Teen Motherhood and Abortion Access." *Quarterly Journal of Economics* 111:467 – 506.

Kearney, M.S., and P.B. Levine. 2009. "Subsidized Contraception, Fertility, and Sexual Behavior." *Review of Economics and Statistics* 91:137 – 151.

Kirk, D., and D.S. Thomas. 1960. *Demographic and Economic Change in Developed Countries*, UMI, chap. The Influence of Business Cycles on Marriage and Birth Rates.

McKelvey, C., D. Thomas, and E. Frankenberg. 2012. "Fertility Regulation in an Economic Crisis." *Economic Development and Cultural Change* 61:7–38.

MEASURE-DHS. 2013. "Demographic and Health Surveys STATcompiler." www.statcompiler.com.

Miller, G. 2010. "Contraception as Development? New Evidence from Family Planning in Colombia." *Economic Journal* 120:709 – 736.

Mitrut, A., and F.C. Wolff. 2011. "The Impact of Legalized Abortion on Child Health Outcomes and Abandonment: Evidence from Romania." *Journal of Health Economics* 30:1219 – 1231.

Molyneaux, J.W., and P.J. Gertler. 2000. "The Impact of Targeted Family Planning Programs in Indonesia." *Population and Development Review* 26:61–85.

Moyo, D. 2009. *Dead Aid: Why Aid is Not Working, and How there is a Better Way for Africa*. Farrar, Straus and Giroux.

Myers, C.K. 2012. "Power of the Pill or Power of Abortion? Re-Examining the Effects of Young Women's Access to Reproductive Control." IZA Discussion Paper 6661.

Nerquaye-Tetteh, J. 2012. Direct communitcation with Joana Nerquaye-Tetteh, Executive Director of PPAG 1995-2006.

PAI. 1999. "Donor Report Card and Country Profiles." Population Action International.

Phillips, J., W. Stinson, S. Bhatia, M. Rahman, and J. Chakraborty. 1982. "The Demographic impact of Family Planning - Health Services Project in Matlab, Bangladesh." *Studies in Family Planning* 13:131–140.

Pop-Eleches, C. 2006. "The Impact of an Abortion Ban on Socioeconomic Outcomes of Children: Evidence from Romania." *Journal of Political Economy* 114:744 – 773.

—. 2010. "The Supply of Birth Control Methods, Education, and Fertility: Evidence from Romania." *Journal of Human Resources* 45:971–997.

PPAG. 2012. Internal historical documents of Planned Parenthood Association of Ghana, Provided by Joana Nerquaye-Tetteh, executive director of PPAG 1995-2006.

Pritchett, L.H. 1994. "Desired Fertility and the Impact of Population Policies." *Population and Development Review* 20:1–55.

Ross, J., E. Weissman, and J. Stover. 2009. "Contraceptive Projections and the Donor Gap." Working paper, Reproductive Health Supplies Coalition.

Sinha, N. 2005. "Fertility, Child Work, and Schooling Consequences of Family Planning Programs: Evidence from an Experiment in Rural Bangladesh." *Economic Development and Cultural Change* 54:97–128.

The White House Office of Policy Development, . 1984. "US Policy Statement for the International Conference on Population." *Population and Development Review* 10:pp. 574–579.

Turnbull, W., and D. Bogecho. 2003. "Access Denied: U.S. Restrictions on International Family Planning." Population Action International.

UNFPA. 2004. "Financial Resource Flows for Population Activities 2004." Available online: http://www.resourceflows.org/.

USAID. 1999. "Contracts and Grants and Cooperative Agreements with Universities, Firms and Non-Profit Institutions by Country/Region active as of October 1, 1999: Fiscal Year 2001." Yellow book, Office of Procurement, U.S. Agency for International Development.

World Bank. 2013. "World Development Indicators." Online at http://databank.worldbank.org.

Tables & Figures

Table 1: Predicted Impact of Contraceptive Supply Reduction by demographic group

	Low Education	High Education
Poor	conception: increase abortion: no change fertility: increase	conception: ambiguous abortion: no change fertility: increases iff conception increases
Non-Poor	conception: no change abortion: no change fertility: no change	conception: ambiguous abortion: increase iff conception increases fertility: no change

Table 2: Family Planning Commodity Availability (as percent of clinics)

	1993 (MCP1)	1996 (NoMCP)		2002 (MCP2)	
Combined Pill	92%	92%		82%	**
Progesterone Pill	62%	86%	**	75%	**
Condom	85%	93%	**	87%	**
Injectable	94%	90%	*	93%	
Spermicide	85%	91%	*	74%	**
IUD	89%	89%		76%	**
Implant	0%	85%	**	74%**	
N	399	313		428	

Note: *Indicates that the measure is significantly different from the measure in the previous survey at the 5% level (** 1%). Source: Hong et al. (2005)

Table 3: Impact of policy at time of first birth on ever-use of modern contraception

				Rural				
	All	Urban	Rural				1998,2003,2008	1988,1993
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy ON at time of first birth	-.0205**	-.0183	-.0215**	-.0234**	-.0284***	-.0269***	-.0288**	-.0418**
	(.009)	(.015)	(.010)	(.011)	(.011)	(.010)	(.014)	(.021)
Policy ON * Poorest							.0165	
							(.020)	
Policy ON * Low Education								.0268
								(.024)
Woman-level controls	Yes	Yes	Yes	No	No	No	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Quadratic time trend	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes
Observations	12521	4277	8244	8278	8278	8278	8244	8244
R^2	.084	.071	.080	.035	.008	.035	.080	.080
Mean of Dep.Var	.3981	.5085	.3409					

Notes: Dependent variable is a binary indicator for ever-use of modern contraception at interview. Data are from standard cross-sectional DHS 1988, 1993, 1998, 2003, and 2008. Standard errors are shown in parentheses. *** indicates significance at the 1% level; ** 5%; * 10%.

Table 4: Impact of policy on ever-use, by region

	West (1)	Central (2)	Accra (3)	Volta (4)	East (5)	Ashanti (6)	B Ahafo (7)	North (8)	UEast (9)	UWest (10)
Policy ON at time of first birth	-.0332	-.0296	.1250	-.1022**	.0448	-.0625*	-.0398	.0332	-.0198	-.0161
	(.327)	(.428)	(.079)	(.002)	(.185)	(.031)	(.276)	(.101)	(.572)	(.587)
N	841	707	200	957	907	1152	830	1123	686	875
R^2	.035	.068	.149	.076	.051	.074	.032	.025	.116	.104
Mean of Dep.Var	.3615	.3494	.4050	.4222	.4157	.3681	.4386	.1264	.3499	.2640
% Effect	-9%	-8.5%	+31%	-24%	+11%	-17%	-9%	+26%	-5.5%	-6%
Classification	Med	Med	Lo	Hi	Lo	Hi	Med	Lo	Med	Med

Notes: Dependent variable is a binary indicator for ever-use of modern contraception at interview. Data are from standard cross-sectional DHS 1988, 1993, 1998, 2003, and 2008. Estimates include controls for age, parity, ever married, poorest, and low education, as well as a quadratic time trend and survey fixed effects. Standard errors are shown in parentheses. *** indicates significance at the 1% level; ** 5%; * 10%.

Table 5: Summary of Contraceptive Use, Conception and Abortion

	N	Ever Used Contraceptives	Ages 17 - 25: Rate of Conception	Ages 17 - 25: Aborted share of Pregnancies
All	10,370	52%	2.2%	8.6%
Urban	4,960	59%	1.8%	15.3%
Rural	5,410	47%	2.5%	4.8%
Rural Sub-groups				
Poorest Low Education	1,676	28%	2.7%	1.6%
Poorest Hi Education	490	55%	2.5%	7.0%
Less Poor Low Education	1,595	46%	2.7%	3.7%
Less Poor Hi Education	1,649	62%	2.2%	9.7%

Note: Data are from special DHS 2007. Rates of conception and abortion are for months when women are aged 17 - 25. Rate of conception is the probability of conception in a single month. "Less Poor" is the top 3 wealth quintiles in the rural population; note that 70% of this group is poor by the international standard of $2/day. "Low Education" indicates those with no education beyond primary school (grade 6). Sampling weights are employed.

Table 6: Policy's Effect on Probability of Conception per Month

				Rural Subgroups			
	All	Urban	Rural	Poorest		Less Poor	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Policy	.0011**	-.0003	.0022***	.0019*	.0024**	.0021**	.0020*
	(.000)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Policy*Schooling					-.0032*		.0003
					(.002)		(.001)
Linear Combination					-.0007		.0023**
					(.0020)		(.0012)
N	686449	322506	363943	147832	147832	216111	216111
R^2	.007	.007	.008	.010	.010	.008	.008
Mean of Dep.Var	.0195	.0164	.0217	.0228	.0228	.0210	.0210
Effective Increase in Pregnancy (main effect)	5.6%	..	10.1%	8.3%	10.5%	10%	9.5%

Note: Dependent variable is a binary indicator for conceiving in a given month. Data are from special DHS 2007. Samples include all months in which a woman was aged 17-25. All specifications include woman-fixed effects, woman level controls as described in the text, a cubic time trend, calendar month fixed effects, and indicators for which policy change is relevant. "Poorest" indicates the lowest two wealth quintiles of the rural sample. "Less Poor" is the top three quintiles in the rural sample. "Schooling" indicates having education beyond primary school (grade 6). Sampling weights are employed. Standard errors are shown in parentheses, clustered at the village level. *** indicates statistical significance at the 1% level; ** 5%; * 10%.

Table 7: Policy's Effect on Share of Pregnancies ending by Abortion

				Rural Subgroups			
	All	Urban	Rural	Poorest		LessPoor	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Policy	.0144+	.0044	.0235**	-.0019	.0059	.0393***	.0443***
	(.009)	(.020)	(.009)	(.013)	(.013)	(.013)	(.013)
Policy*Schooling					-.0470		-.0135
					(.037)		(.019)
Linear Combination					-.0411		.0308
					.0369		.0195
N	12439	4945	7494	3155	3155	4339	4339
R^2	.132	.239	.074	.067	.070	.090	.091
Mean of Dep.Var	.0823	.1484	.0457	.0263	.0263	.0585	.0585

Note: Dependent variable is a binary indicator for whether a pregnancy conclusion was an abortion. Data are from special DHS 2007. Samples include all pregnancy conclusions for women aged 17 to 25. All specifications include woman-fixed effects, woman level controls as described in the text, a cubic time trend, calendar month fixed effects, and indicators for which policy change is relevant. For descriptions of subgroups by schooling and wealth, see notes to Table 6. Sampling weights are employed. Standard errors are shown in parentheses, clustered at the village level. *** indicates statistical significance at the 1% level; ** 5%; * 10%.

Table 8: Summary of Impacts on Conception and Abortion

	Low Education	High Education
Poorest	[population share: 33%] Conception: 10.5% increase Abortion: c/n reject no change	[population share: 8%] Conception: c/n reject no change Abortion: c/n reject no change
Less Poor	[population share: 29%] Conception: 9.5% increase Abortion: increased; offset 4 of 10 add'l pregnancies	[population share: 30%] Conception: 11% increase Abortion: increased; offset 3 of 10 add'l pregnancies

Note: For descriptions of sub-groups by schooling and wealth, see notes to Table 6.

Table 9: Narrowing the Window Around Policy Changes

	Conception				Abortion			
	42mos	36mos	30mos	24mos	42mos	36mos	30mos	24mos
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy	.0016**	.0018**	.0026***	.0025**	.0399***	.0479**	.0634***	.0728**
	(.001)	(.001)	(.001)	(.001)	(.014)	(.019)	(.024)	(.036)
N	322152	277406	232915	187891	3826	3334	2820	2252
R^2	.008	.008	.008	.008	.087	.095	.094	.114

Note: Dependent variables are shown as multi-column headers. Data are from special DHS 2007. Samples for columns 1 - 4 include months when a rural woman is 17 to 25 year old, which are within the specified number of months from a policy change. Samples for columns 5 - 8 include pregnancy conclusions within the specified number of months of a policy change for rural women aged 17 to 25 who are not in the poorest two quintiles. All specifications include woman-fixed effects, woman level controls as described in the text, a cubic time trend, calendar month fixed effects, and indicators for which policy change is relevant. Sampling weights are employed. Standard errors are shown in parentheses, clustered at the village level. *** indicates statistical significance at the 1% level; ** 5%; * 10%.

Table 10: Robustness & Placebo Checks

CONCEPTION	Controlling for NHIS (1)	Excluding 2001 change (2)	Ages 16-26 (3)	Ages 15-27 (4)	No Weights (5)	No Controls (6)	Placebo Election (7)
Policy	.0023***	.0029***	.0014**	.0019**	.0020***	.0015**	
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	
Placebo Policy							-.0020
							(.002)
N	363943	217817	441720	285348	363943	363943	111031
R^2	.008	.008	.008	.009	.008	.000	.008

ABORTION	Controlling for NHIS (8)	Excluding 2001 change (9)	Ages 16-26 (10)	Ages 15-27 (11)	No Weights (12)	No Controls (13)	Placebo Election (14)
Policy	.0399***	.0525***	.0289**	.0342**	.0377***	.0405***	
	(.014)	(.016)	(.013)	(.015)	(.012)	(.013)	
Placebo Policy							.0277
							(.035)
N	4339	2643	5021	3515	4339	4339	1413
R^2	.090	.092	.083	.107	.085	.014	.128

Note: Dependent variables are shown as left-hand table headers. Data are from special DHS 2007. **Upper Panel**: Samples include rural women in months when aged 17 to 25. **Lower Panel:** Samples include pregnancy conclusions for rural women in the upper three rural wealth quintiles, aged 17 to 25. **Both Panels**: The final column includes observations 4 years before or after a placebo policy change (to OFF) in 1988, coinciding with a U.S. presidential election. All specifications include woman-fixed effects, calendar month fixed effects, indicators for which policy change is relevant, a cubic time trend, and woman-month level controls, except as noted. Sampling weights are employed, except as noted. Standard errors are shown in parentheses, clustered at the village level. *** indicates statistical significance at the 1% level; ** 5%; * 10%.

Table 11: Impact of policy on conception and abortion, by regional impact of policy on use of modern contraception

	Conceive (1)	Conceive (2)	Abort (3)	Abort (4)
Policy	.0022***	.0031***	.0393***	.0474**
	(.00064)	(.00106)	(.01291)	(.01973)
Policy*Medium Impact Region		-.0004		-.0071
		(.00145)		(.02224)
Policy*Low Impact Region		-.0021		-.0184
		(.00125)		(.02001)
Lin. Combination for Med		.0028**		.0403**
		(.00106)		(.01775)
Lin. Combination for Low		.0011		.0290**
		(.00090)		(.01361)
Observations	363943	363943	4339	4339
R^2	.008	.008	.090	.091
Mean of Dep. Var	.0217		.0585	

Notes: Dependent variables are shown as column headers. Data are from special DHS 2007. Indicators of policy impacts are those estimated in Table 11. Cols 1 and 2 use rural woman-months; cols 3 and 4 use pregnancy conclusions of rural, less poor women. All columns include woman fixed effects, quadratic functions of age and parity, an indicator for ever married, a cubic time trend, calendar month fixed effects, and policy change fixed effects. Standard errors are shown in parentheses, clustered at the village level. *** indicates significance at the 1% level; ** 5%; * 10%.

Table 12: Policy Impact on Fertility Rates

	(1)	(2)	(3)	(4)
Policy	.0487	.1202*	.1715*	.1644*
	(.043)	(.061)	(.089)	(.094)
Policy*Urban		-.1921**	-.1916**	-.2038**
		(.084)	(.084)	(.082)
Policy*Non Poor			-.0937	-.1024
			(.091)	(.091)
Policy*Primary Ed				.0397
				(.080)
Linear Combination: Urban		-.0719	-.0201	See Table 13
		.0588	.0765	
Linear Combination: Nonpoor			.0778	See Table 13
			.0660	
N	344	344	344	344
R^2	.607	.614	.615	.616

Note: Dependent variable is the age-specific fertility rate (ASFR) for 17 to 25 year olds in years 1965 to 2007, calculated separately for eight demographic groups (by rural/urban, poor/non-poor, and low/high education). Data are from standard DHS cross-sectional surveys in 1988, 1993, 1998, 2003, and 2008. For descriptions of sub-groups by schooling and wealth, see notes to Table 6. Estimations are weighted by the size of the demographic group; sizes given in Table 13. All linear combinations from Col 4 are shown in Table 13. Standard errors are given in parentheses. *** Indicates significance at 1% level; ** 5% level; * 10% level.

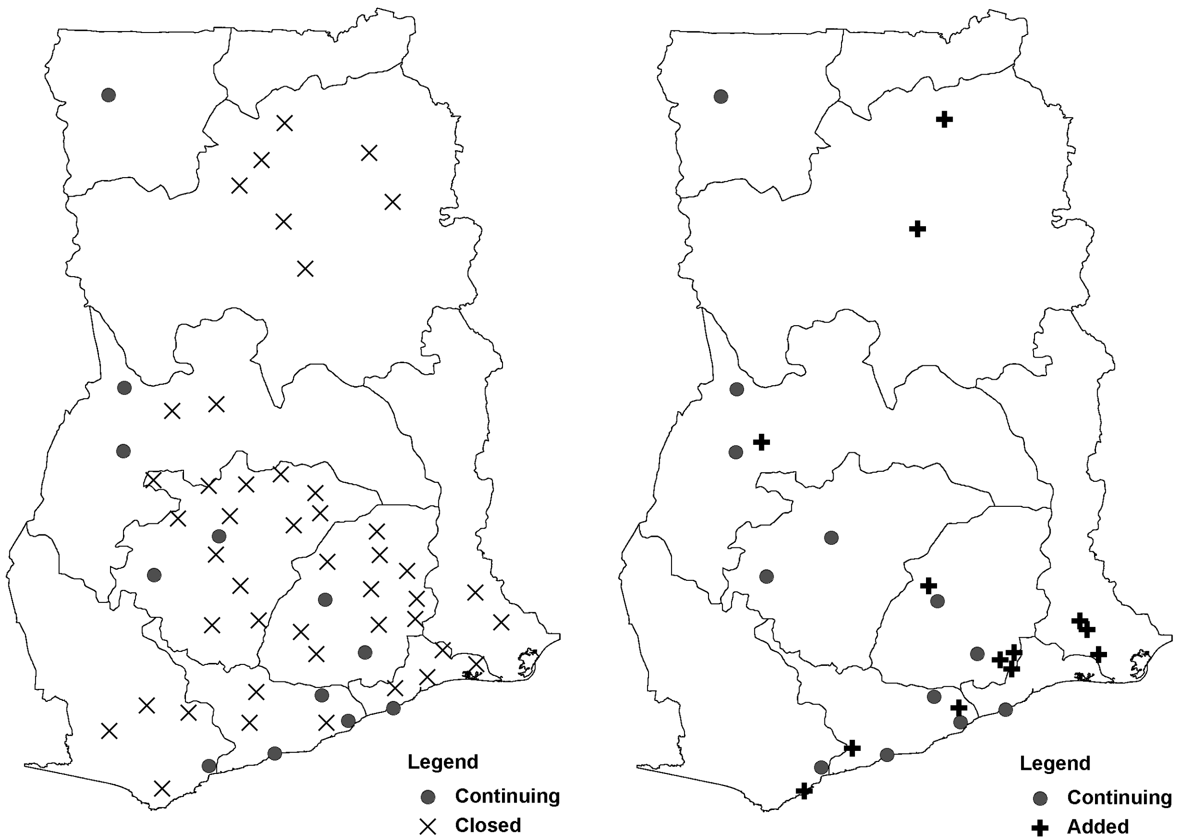
Table 13: Summary of Impacts on Fertility Rates

			No/Low Education		High Education
Rural	Poorest	Effect	**.1644***	Effect	**0.2041****
			(.094)		(.094)
		ASFR:	2.23	ASFR:	1.9
		% effect:	**7.4%**	**% effect:**	**10.7%**
		Pop share:	23%	Pop share:	5%
Rural	Less Poor	Effect	0.0620	Effect	0.1018
			(.080)		(.071)
		ASFR:	2.19	ASFR:	1.71
		% effect:	2.8%	% effect:	6.0%
		Pop share:	21%	Pop share:	14%
Urban	Poor	Effect	-0.0393	Effect	0.0004
			(.089)		(.090)
		ASFR:	1.99	ASFR:	1.43
		Pop share:	8%	Pop share:	8%
Urban	Non Poor	Effect	-0.1417	Effect	-0.1020
			(.077)		(.077)
		ASFR:	1.69	ASFR:	1.08
		Pop share:	6%	Pop share:	14%

Note: Data are from standard DHS cross-sectional surveys in 1988, 1993, 1998, 2003, and 2008. Estimated effects shown are linear combinations from Col 4 of Table 12. For descriptions of sub-groups by schooling and wealth, see notes to Table 6. Standard errors are given in parentheses. *** Indicates significance at 1% level; ** 5% level; * 10% level.

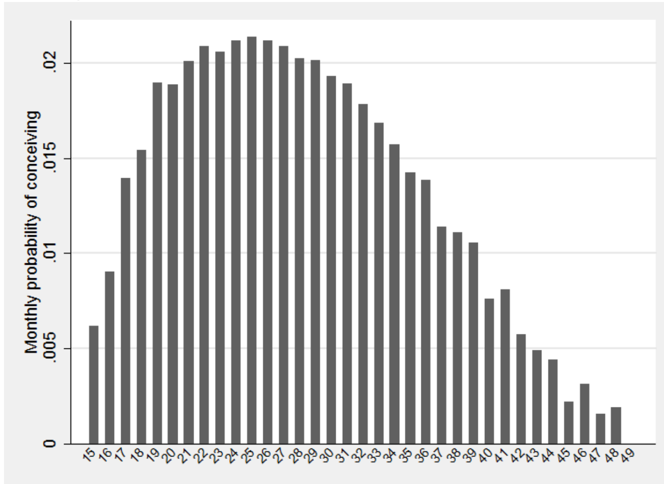
Figure 1: PPAG service locations, before and after re-implementation of the policy in 2001

Before After

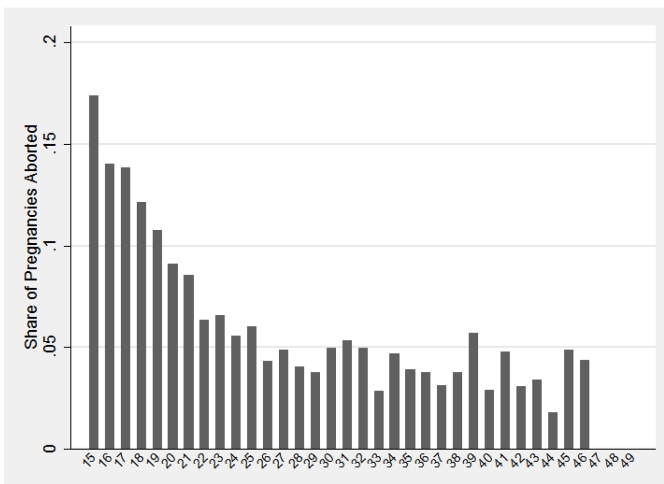


Source: Provided to author by former PPAG director

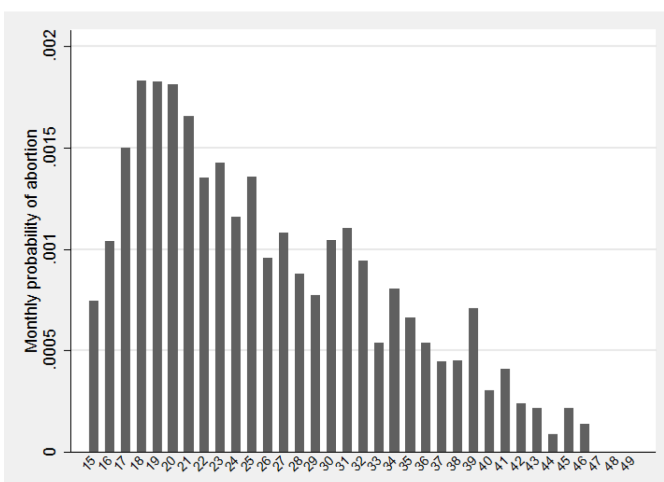
Figure 2: Conception & Abortion Statistics



2.A

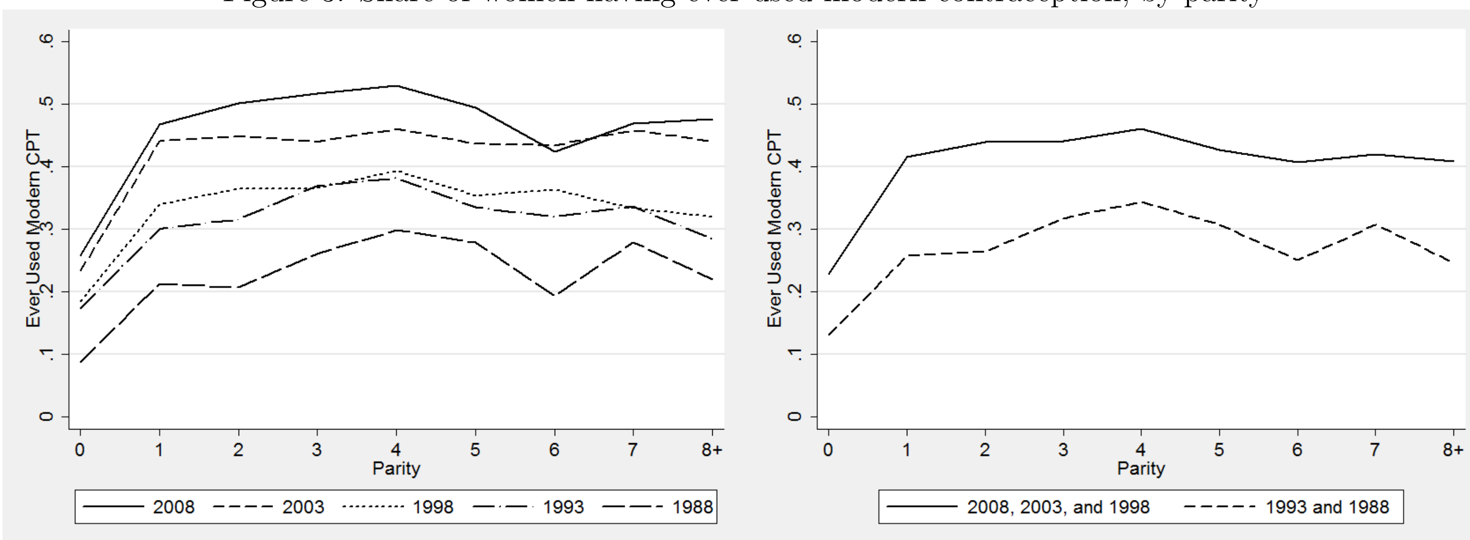


2.B



2.C

Figure 3: Share of women having ever used modern contraception, by parity



Appendix

Table A.1: USAID Funds Slated to Foreign Reproductive Health NGOs for FY2001

Country	Total Funds	Organizations
Nicaragua	$ 5,916,406	APROFAM
Guatemala	$ 3,427,370	APROFAM
Peru	$ 2,006,509	APROFAM, PUNO, PROFAMILIA
Kenya	$ 1,852,803	Family Planning Association of Kenya
Ghana	**$ 797,014**	Planned Parenthood Association of Ghana, GSMF
Philippines	$ 686,498	Friendlycare Foundation
Paraguay	$ 581,057	Centro Paraguayo de Estudios de Poblacion
Dominican Republic	$ 318,871	Asociacion Dominicana de Planificacion familiar
Romania	$ 192,161	FTPT, Society for Contraceptive & Sexuality Education
Senegal	$ 31,915	SANFAM
Median	$ 741,756	

Source: USAID (1999). Note: Funds are aggregated by country, in some cases slated to multiple NGOs, as shown. Figures were published in 1999, before the re-imposition of the policy in early 2001. In some cases, multi-year grants have been divided to represent funds specific to 2001.

Acronyms for Tables A.1 and A.2:

APROFAM: Asociacion Pro-Bienestar de la Familia

FAA: Foreign Aid Appropriations

FOA: Foreign Operations Appropriations

FRA: Foreign Relations Authorization PSI

FTPT: Fundatia Tineri Pentru Tineri GSMF

GSMF: Ghana Social Marketing Foundation

IPPF: International Planned Parenthood Federation

PPFA: Planned Parenthood Federation of America

PROFAMILIA: Programa de planificacion familiar y salud materno infantil del cusco

PUNO: Planification Familiar

SANFAM: Sante de la Famille

USAID: United States Agency for International Development

Table A.2: Presidential, Congressional & Litigatory Actions Regarding the Mexico City Policy

Date		Action	Details
1984	August	Enact	US delegation to International Conference on Population in Mexico City announces policy as executive order.
1985	January	Lawsuit	DKT Memorial Fund brings legal challenge to US Court of Appeals in DC. Case fails in 1989.
1987		Lawsuit	PPFA sues USAID. Case fails in Supreme Court in 1990.
1990		Lawsuit	USAID is sued by coalition of organizations* in US District Court in DC
1991		Debate	House debates reversal of policy in Foreign Aid Authorization Bill
1992	October	Vote	Congress approves language in FAA Bill that reverses policy; Language dropped under threat of veto by President G. W. Bush
1993	January	Repeal	President Clinton repeals the policy
1996	February	Vote	Population funding is capped and release of funds blocked; requiring special congressional votes to release funding.
1999	Fall	Bargain	In bargaining over other matters, U.S. House leadership elicits agreement from President Clinton to re-imposition of a modified version of the policy
2000	Fall	Vote	FOA Act delays USAID 2001 funding decisions until February 2001
2001	January	Enact	President G. H. W. Bush reinstates the policy
2001	February	Debate	Bills sponsored in House and Senate to repeal the policy
2001	March	Enact	President Bush issues memorandum preventing Congress from challenging the policy
2001	May	Vote	Lee Bill attempts to amend FRA Act to repeal the policy; fails in the House. Similar attempt fails in the Senate.
2001	June	Lawsuit	Center for Reproductive Law and Policy sues President Bush. Fails in US Court of Appeals 2nd Circuit.
2001	October	Vote	Senate approves language overturning policy. Fails in the House.
2003	February	Debate	White House proposes expansion of policy
2003	March	Debate	In face of opposition, White House abandons expansion of policy
2003	July	Vote	Senate votes to overturn policy. Under threat of presidential veto, bill fails in the House.
2003	August	Enact	President Bush extends policy to apply to State Departement funding
2003	October	Vote	Senate passes Foreign Operations Bill overturning policy; bill fails in the House.
2005	April	Vote	Senate approves ammendment to overturn policy; bill fails in the House.
2005	November	Vote	Senate approves ammendment to FOA bill to exempt contraceptives from policy; bill fails in the House
2006	June	Vote	Senate repeals policy; bill fails in the House
2007	June	Vote	House votes to exempt contraceptives from the policy
2007	September	Vote	Senate votes to exempt contraceptives and repeal policy entirely
2007	December	Veto	Despite votes to exempt contraceptives in both chambers, the bill is dropped due to threat of veto by President Bush
2008	July	Vote	Senate Appropriations committee adopts a full repeal of the policy; House is silent on the matter
2009	January	Repeal	President Obama rescinds the policy
2009	January	Vote	Senate defeats amendment proposed to nullify the Presidential repeal
2010	July	Vote	Senate Appropriations committee adopts bill to make future enactments of the policy impossible
2011	April	Vote	House adds language reinstating the policy to federal budget bill. Senate refuses to pass the budget due to policy riders. Federal government comes within hours of a shut-down when Congress cannot pass a budget.

Source: Population Action International's Global Gag Rule Timeline. Online at http://www.populationaction.org/Publications/Reports/The_Global_Gag_Rule/Index.shtml

*The coalition of organizations bringing suit in 1990 was The Pathfinder Fund, The Population Council, and the Association for Voluntary Surgical Contraception.

Table A.3: IPPF Funding to Planned Parenthood Association of Ghana

Allocation Year	Funding from IPPF	As percent of funding in 2000
2000	$1,694,592	
2001	$926,706	55%
2002	$780,000	46%
2003	$902,851	53%
2004	$1,199,589	71%
2005	$1,114,402	66%
2006	$1,125,598	66%
2007	$1,148,371	68%
2008	$1,270,742	75%

Source: IPPF financial statements 2001-2009

Table A.4: Mean Age of Sample, by period

Period	Years	Full Sample	17 - 25 yos	18 - 20 yos
PRE	1981-1984	18.3	19.7	18.9
ON 1	1985-1992	21.9	20.7	19.0
OFF	1993-2000	25.1	20.8	19.0
ON 2	2000-2007	28.2	20.9	19.0

Note: Data are from 2007 special DHS. The time periods are restricted to match the 8-year time windows surrounding the policy changes, as in the analysis.

Table A.5: Effective Sample Size for Analysis of Abortion Use

	At any time	While aged 17-25
Women in sample	9,094	**{8,334}**
Had any pregnancy	6,470	**{5,860}**
Had 2+ pregnancies	5,273	3,889
Has variation in policy across pregnancies	4,477	2,286

Note: Data are from 2007 special DHS. Samples includes women aged 15 to 49 between 1981 and 2004.

Table A.6: Policy's Effect on Unconditional Probability of Abortion

	All	Poorest		Less Poor	
	(1)	(2)	(3)	(4)	(5)
Policy	.0003	.0001	.0003	.0004	.0005*
	(.000)	(.000)	(.000)	(.000)	(.000)
Policy*Schooling			-.0010		-.0003
			(.001)		(.000)
p-value (main)	0.117	0.664	0.266	0.141	0.059
Linear Combination			-.0007		.0002
SE			(.0007)		(.0004)
N	363943	147832	147832	216111	216111
R^2	.000	.000	.000	.000	.000
Mean of Dep.Var	.0009	.0006	.0006	.0012	.0012

Note: Dependent variable is whether woman i had an abortion in the given month, irrespective of pregnancy status. Data are from special DHS 2007. Samples are denoted in column headers and include all months in which a women was aged 17 to 25. All specifications include woman-fixed effects, woman level controls as described in the text, a cubic time trend, calendar month fixed effects, and indicators for which policy change is relevant. For descriptions of sub-groups by schooling and wealth, see notes to Table 6. Sampling weights are employed. Standard errors are shown in parentheses, clustered at the cluster level. *** Indicates significance at 1% level; ** 5% level; * 10% level.

Table A.7: Historical Fertility Rates in Ghana

	TFR		17 to 25 ASFR (Rural)	
Year	Urban	Rural	Low Education	High Education
1970			2.4	2.7
1975			2.1	2.3
1980			2.1	2.2
1985			2.2	2.1
1990	4.6	6.6	2.4	1.8
1995	3.4	6.1	2.2	1.7
2000	3.4	6.5	2.4	1.8
2005	3.0	4.7	1.8	1.4

Note: Data are from standard DHS cross-sectional surveys in 1988, 1993, 1998, 2003, and 2008. TFR is total fertility rate in births per woman over her lifetime; 17 to 25 ASFR is age-specific fertility rate – births per woman while she is between the ages of 17 and 25. Accurate TFR cannot be calculated prior to 1989 based on these data.