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Evaluating the Mexico City Policy

How US Foreign Policy Affects Fertility Outcomes and Child Health in Ghana

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ABSTRACT

US development assistance represents a significant source of funding for many population programs in poor countries. The Mexico City policy, known derisively as the global gag rule, restricts activities of foreign nongovernmental organizations (NGOs) that receive such assistance. The intent of the policy is to reduce the use of abortion in developing countries—a policy that is born entirely of US domestic politics and that turns on and off depending on the political party in power. I examine here whether the policy achieves its aim, and how the policy affects reproductive outcomes for women in Ghana.

Employing a woman-by-month panel of pregnancies and woman fixed effects, I estimate whether a given woman is less likely to abort a pregnancy during two policy periods versus two nonpolicy periods. I find no evidence that any demographic group reduces the use of abortion as a result of the policy. On the contrary, rural women significantly increase abortions. This effect seems to arise from their increased rate of conception during these times. The policy-induced budget shortfalls reportedly forced NGOs to cut rural outreach services, reducing the availability of contraceptives in rural areas. The lack of contraceptives likely caused the observed 12 percent increase in rural pregnancies, ultimately resulting in about 200,000 additional abortions and between 500,000 and 750,000 additional unintended births. I find that these additional unwanted children have significantly reduced height and weight for age, relative to their siblings.

Rather than reducing abortion, this policy increased pregnancy, abortion, and unintended births, resulting in more than a half-million children of significantly reduced nutritional status.

Keywords: fertility, abortion, foreign aid, child health

JEL Codes: I15, J13, O19, F35

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1. INTRODUCTION

The United States is consistently one of the largest donors of international population assistance worldwide (UNFPA 2004).[1] In 1984, President Reagan issued an executive order that restricted such funding in the following way:

> U.S. support for family planning programs is based on respect for human life, enhancement of human dignity, and strengthening of the family. Attempts to use abortion . . . in family planning must be shunned . . . [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. (White House Office of Policy Development 1984)

This executive order is known as the Mexico City policy (MCP), based on its introduction at the International Conference on Population held in Mexico City in 1984. It requires foreign nongovernmental organizations (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).[2] It is the aim of this paper to ascertain whether or not the MCP achieves its primary objective, that is, to reduce the use of abortion abroad.

Such an investigation presents two key challenges. The first is that very few data exist on the use of abortion in poor countries, either at the individual level or in aggregate. I am benefited in this regard by one survey conducted by MEASURE DHS in Ghana in 2007, which explicitly asked women about the outcome of each of their past pregnancies, including induced abortion. Secondly, lacking any logical comparison group, it is difficult to say whether differences during the years of the policy are actually attributable to the policy or to other unobserved differences. To address this issue, I use the complete pregnancy histories collected in this special Demographic and Health Survey (DHS) to create a woman-by-month panel from 1981 to 2007. Given the age range of women at the time of the survey, I observe women moving both into and out of MCP periods during their reproductive years. The creation of this panel allows a within-woman estimation, which controls for unobservables at the individual level.

I find that, on average, a woman is no less likely to abort a pregnancy when the policy is in effect than at any other time. Examining demographic subgroups by location, wealth level, and education, I find no significant reduction for any group. However, I find startling evidence that rural women actually increase the use of abortion during MCP periods.

Advocacy organizations report that the implementation of the MCP resulted in significant losses of USAID funding for key reproductive health organizations. Such losses reportedly forced cutbacks in rural outreach services, reducing access to contraceptives in rural areas. In some countries, reproductive health clinics were forced to close (TGGRIP 2003).[3] I find evidence supporting such reports in estimates showing that rural women experienced an increase in conception during MCP periods. If these additional conceptions were unwanted pregnancies, this could induce an increase in abortions. I find that rural pregnancies increased by 12 percent and the rural abortion rate increased by 2.3 percent of pregnancies—suggesting that the remaining 9.7 percent increase in pregnancies resulted in unwanted births.

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[1] Population assistance is defined as funding to support the provision of contraception and family planning in foreign nations.

[2] 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 US monies for the provision of abortion. Therefore, it was the forbidding of organizations to use their own funds to education women about safe abortion options or lobby the government for legalization that earned the policy the derisive nickname *the global gag rule*.

[3] These reports also detail the breakdown of sector-specific government–NGO partnerships as a result of the policy. A separate investigative report further suggested a *chilling* effect, whereby even signatory organizations cut back on other reproductive health activities out of fear of also losing funding (Blane and Friedman 1990).

Because the poorest and least-educated women were the least likely to abort additional pregnancies, the increase in unintended births is disproportionately located among these groups. Each additional unintended birth can put further strain on already resource-strapped households, causing reduced investment in child nutrition and health. I therefore examine whether rural children conceived during policy periods have differential health outcomes. I additionally employ DHS data collected in Ghana in 2003 and 1993, focusing on the policy changes in 1993 and 2001. I find evidence that children conceived under the policy exhibit significant growth deficits: height for age and weight for age that are approximately 0.7 standard deviation (SD) lower than their siblings. There is also suggestive, though less robust, evidence that these children are more likely to be anemic and experience respiratory illness.

This study is among the first to assess the impact of this policy, which has absorbed extensive time and energy in US politics. In the 25 years since its imposition, the policy has been continually repealed and reinstated by Democratic and Republican administrations, respectively.[4] It has been the concern of several major court battles, one of which ended in the Supreme Court, and at least 20 congressional debates or votes have been taken on the matter (see Appendix Table A.1). Its 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.

Bendavid, Avila, and Miller (2011) have concurrently investigated the impact of this policy. They employ data on women from 20 Sub-Saharan African countries, using an algorithm to infer pregnancies that were aborted. They categorize countries by low or high exposure to the policy and conclude that women in high-exposure countries increased their use of abortion following the 2001 reimplementation of the policy. My findings are consistent with theirs, though my methodology differs in a number of ways: (1) I focus on one country, rather than many, which allows me to employ data on actual induced abortions, rather than estimating abortions via algorithm; (2) I estimate within woman rather than across countries; (3) I estimate a more comprehensive effect of the policy, including the first three changes in the policy, rather than just the 2001 reimplementation; (4) I show evidence that the pathway of the effect is indeed increased conception; and (5) I investigate the impact on child outcomes for the resulting unintended births.

In addition, this inquiry contributes to the literature on the impacts of international development assistance, more broadly. Sachs et al. (2004) argue that ever-increasing donor commitments are Africa's only way out of poverty. In contrast, Easterly and Williamson (forthcoming) warn of the potential ill effects of aid, and Moyo (2009) argues that the dependence caused by aid is at the root of Africa's troubles. The imposition of the MCP demonstrates the negative effects of both aid conditionality and aid removal. Though I cannot identify these effects separately, we can see from these results that the dependence of the reproductive health sector on international assistance makes it vulnerable to unpredictable budget changes based on the whims of donor-country domestic politics.

This work also speaks to the importance of family planning programs in poor countries. Joshi and Schultz (2007) and Sinha (2005) describe family planning as an investment in development, based on results from randomized experiments in Matlab, Bangladesh. They find that increasing access to contraceptives does reduce fertility, which generates other beneficial outcomes. In contrast, Pritchett (1994) argues strongly that contraceptive access is not a major factor in determining fertility. Based on an examination of historical and current cross-country data, he concludes that "it is fertility desires and not contraceptive access that matter." The results presented here fall squarely on the side of family planning effectiveness—I find that reduced access to low-cost contraceptives increased pregnancies, abortions, and births in rural areas.

Finally, the findings presented here on child outcomes contribute to a growing literature that investigates differential outcomes for planned versus unplanned births. Gipson, Koenig, and Hindin (2008) review this literature, which relies primarily on cross-family comparisons and parental reports of wantedness. Do and Phung (2010) discuss the important ways in which these factors can bias such estimates. Their paper is one of the first to overcome these challenges, as I do here, by employing an exogenous change that affects birth wantedness (in their case, birth in a gender-specific auspicious year in

[4] It has been officially in effect during the periods 1984–92 and 2001–09.

Vietnam). They find that children more likely to be planned receive two additional years of schooling vis-à-vis their siblings. My findings suggest that inequalities begin much earlier than schooling, appearing as growth deficits in early childhood. This is consistent with the findings of Chalasani, Casterline, and Koenig (2008) that unwanted births in Bangladesh have higher rates of neonatal and infant mortality.

In the following section I provide further history of the Mexico City policy and discuss the Ghana case in detail. In Section 3 I describe the data employed and the creation of the woman-by-month rolling panel. Section 4 presents empirical specifications and estimation results, as well as specification and robustness checks. Impacts on child outcomes are explored in Section 5. I discuss the findings and the implications for policy in the concluding Section, 6.

2. BACKGROUND

In August 1984, the United Nations held the International Conference on Population in Mexico City. The official statement of the United States at this conference unveiled a new policy regarding the use of American population assistance funds. The administration of President Reagan issued an executive order stating that any NGO receiving such funding must attest that it does not perform or actively promote abortion as a means of family planning.

Certainly, many NGOs were willing to make such attestations. However, some organizations were unwilling—in particular, those for which reproductive health and family planning were the foremost objective. These organizations saw the provision of safe abortion (as an alternative to pervasive unsafe abortions) and the fight for legalization of safe abortion as central to their mandate. NGOs that refused to sign the policy lost all funding from USAID, amounting to 10–60 percent of organizational budgets. This included large international organizations such as the International Planned Parenthood Federation (IPPF) and Marie Stopes International (MSI), as well as small local NGOs such as the Family Guidance Association of Ethiopia and the Family Planning Association of Kenya.

Funding shortfalls resulting from lost USAID funding began in early 1985. The policy remained in effect, virtually unchanged, until it was repealed by President Clinton in January 1993. A modified version of the policy was implemented in 1999, and the full policy was reinstated by President Bush in January 2001. The policy was extended to apply to State Department funds as well in August 2003. Despite many congressional votes on the matter, the policy remained in effect until it was rescinded by President Obama in January 2009. It is significant that Presidents Clinton, Bush, and Obama issued their changes to the policy's effectiveness on the first or second day following inauguration.

In the interim period 1993–2000, when the MCP was not in effect, the United States provided nearly 40 percent of population assistance worldwide (UNFPA 2004). On average, about half of that funding flowed to NGOs (PAI 1999). USAID documents from late 1999 list funds slated to specific NGOs, by country, for the 2001 fiscal year. The total per country slated to reproductive health NGOs represents the funds at risk for loss following the 2001 reimposition of the policy (see Appendix Table A.2. Reproductive health NGOs in Ghana were slated to receive US$1.8 million in fiscal year (FY) 2001. That is about average for countries receiving such funding, suggesting that potential funding losses in Ghana were roughly representative of this group (USAID 1999).

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 reimposition of the policy in 2001 provides some insight regarding the policy's effect. The Planned Parenthood Association of Ghana (PPAG) was (and is) the leading NGO provider of reproductive and sexual health services in Ghana. [5] As of late 1999, PPAG was slated to receive $565,000 from USAID in 2001 (USAID 1999). Pursuant to the executive order in January 2001, these funds would only be disbursed if the organization agreed to the Mexico City policy.[6]

Under normal circumstances, nearly all the 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. Therefore, USAID was slated to provide one-fourth 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 (TGGRIP 2005;IPPF 2002).

However, from 2001 to 2003 PPAG did experience significant budget losses, as its funding from IPPF was reduced by 54 percent (reducing the total budget by 40 percent) (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

[5] For further detail on family planning in Ghana, see Appendix Table A.3

[6] This organization also existed prior to the 1984 enactment of the policy and likely reacted similarly at that time.

organizations.[7] 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 the previous budget cuts passed along from IPPF. Funding from IPPF did not recover until after the repeal in 2009 (see Table 2.1).

Table 2.1—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 2001–09.

Data from a nationally representative Demographic and Health Survey (DHS) in 1998 suggest that of Ghanaian women using contraceptives at that time, 44 percent were acquiring them from private providers such as PPAG, and 48 percent from government providers.[8] Surveys of both government and NGO providers of family planning services in Ghana were undertaken in 1993, 1996, and 2002.[9] A comprehensive report based on these surveys suggests that contraceptive availability was lower during the years the policy was in effect (Hong et al. 2005). The availability of contraceptive methods (weakly) increased from 1993 to 1996 for five out of six methods and decreased from 1996 to 2002 for five out of six methods (see Table 2.2).

Table 2.2—Family planning commodity availability (as percent of clinics)

	1993	1996		2002	
	(MCP1)	(NoMCP)		(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%	**

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

[7] Prior to the 2001 reimposition of the Mexico City policy, USAID was providing 7.3 percent of income for IPPF (IPPF 2002). It is not clear why cuts to PPAG were so large relative to IPPF losses. Perhaps this reflects IPPF's displeasure with PPAG for agreeing to the policy from 2001 to 2003.

[8] The remaining 8 percent reported acquiring them from shops, churches, friends, or other.

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

3. DATA

Macro International's MEASURE project routinely conducts nationally representative Demographic and Health Surveys (DHS) in developing countries, focusing on women aged 15–49. In 2007, MEASURE conducted a nonstandard survey in Ghana composed of special modules on maternal mortality and abortion. 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 abortions. Although 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.[10]

The survey contains 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 and year it ended (from which one can deduce the month it began), how it ended (live birth, stillbirth, miscarriage, or abortion), 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, was 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 data, each observation was assigned the woman's age at the time, and whether or not she had ever been in union. Months in which the woman was at least 15 years of age compose the complete dataset.

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. Although wealth may vary throughout a woman's life, it seems that wealth quintile is somewhat stable. Nonetheless, one might prefer an alternative indicator of a woman's socioeconomic status, and for this I use educational attainment. This too is measured at the time of the survey only, but we can be reasonably sure that it has not changed since age 18 for most women. Based on the 1998 Ghana DHS, 82 percent of 18-year-old women are no longer in school. In the 2007 data, just over a quarter of women have never attended school. The remainders are classified as having attended primary (21 percent), middle (40 percent), or secondary school or higher (13 percent). In some specifications, I classify women as "low education" (primary school or no schooling) and "high education" (middle school or higher).

Each woman's panel begins when she turns 15 and ends when she is interviewed (maximum age is 49). There are 1.85 million observations from November 1972 to December 2007. Each woman has between 23 and 444 observations (mean is 185). Figure 3.1A shows the conception rate by age; that is, the share of fecund woman-months in which a conception occurred.[11] The conception rates are highest (over 2.5 percent) for women aged 22–27. A gradual decline begins around age 28 and becomes steeper at age 37. For women younger than age 17 or aged 40 and over, the chance of conception in a given month is less than 1 percent.

Figure 3.1B 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 percent 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 percent for women over age 25. Figure 3.1C shows the probability of having an abortion, by age. The combination of high conception rates and high abortion rates yields the greatest chance of having an abortion for women

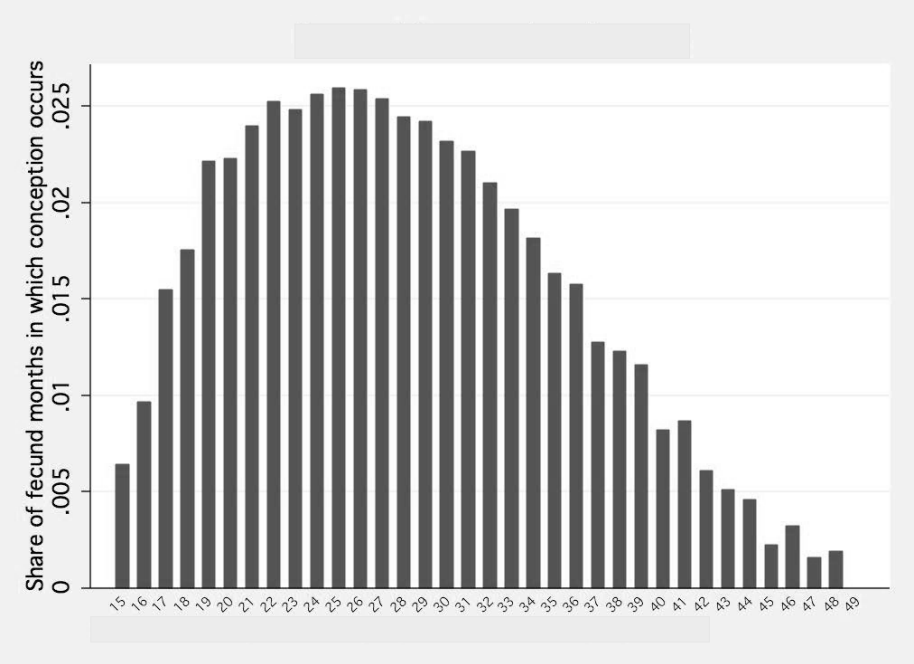
[10] Further, other surveys conducted after 2001 that include pregnancy histories are in countries unlikely to be as affected by the Mexico City policy: Armenia 2005, Azerbaijan 2006, Moldova 2005, Philippines 2008, and Ukraine 2007.

[11] Women are considered fecund (capable of conceiving) if they are not already pregnant or concluding a pregnancy. Information about an individual's natural fecundity or menopausal status is not available.

aged 18–20: about 2 percent per year (.0018*12). Women outside the 17–25 age range have a considerably lower probability: less than 1 percent per year.

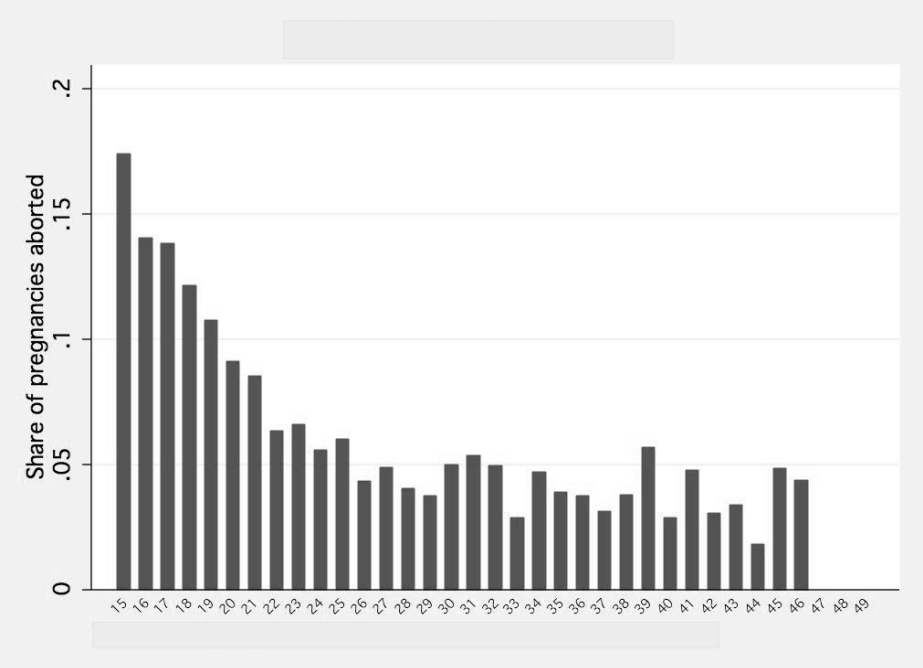
Figure 3.1—Conception and abortion statistics

3.1a—Rate of conception, by age



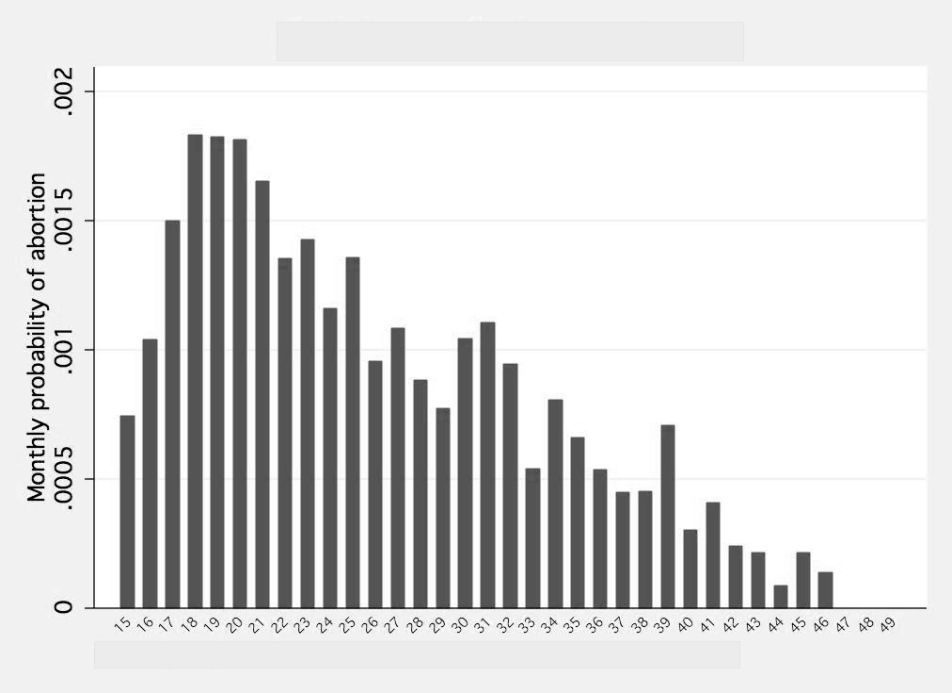
Notes: Fecund indicates not pregnant or concluding pregnancy in that month. Sampling weights applied.

3.1b—Abortion rate, by age



Note: Sampling weights applied.

3.1c—Probability of abortion, by age



Source: Author's calculations based on DHS data
Note: Sampling weights applied.

Creating a panel dataset that encompasses the years 1981 to 2007 allows for examination of four different periods in relation to the policy[12]: period 0 ("PRE"), from 1981 to 1984; period 1 ("ON 1"), from 1985 to 1992; period 2 ("OFF"), from 1993 to 2000; and period 3 ("ON 2"), from 2001 onward. However, a simple comparison of abortion statistics across these periods would be misleading, as the sample characteristics differ across the periods as well. Table 3.1 shows that the mean age for the full sample is significantly increasing over the periods. In order to keep the sample more consistent across periods, one can restrict the age range, effectively creating a rolling panel. The last two columns of Table 3.1 show that when observations are restricted to those for women aged 17–25 (the primary group for abortion procedures), the mean age is much more similar across periods. Further restricting the sample to the group in which most abortions occur (those aged 18–20) produces mean ages nearly identical across periods. The 17–25 age range is the default used for this analysis; robustness to other range selections is shown.

Table 3.1—Mean age of sample, by period

Period	Years	Full Sample	17–25 Years	18–20 Years
PRE	1972–84	18.3	19.7	18.9
ON 1	1985–92	21.9	20.7	19.0
OFF	1993–2000	25.1	20.8	19.0
ON 2	2000–07	28.2	20.9	19.0

Source: Author's calculations based on DHS data

[12] Note that prior to 1981, there are too few pregnancy observations per year to be representative.

Table 3.2 shows the effective sample sizes. Of the 7,489 women who were ever pregnant in the sample, 91 percent had a pregnancy while aged 17–25, yielding an effective sample size of 6,818 women.[13] However, it is useful to note that the identification of policy effects arises from women who had at least two pregnancies in that age range, with variation in the status of the policy across the pregnancies. Sixty-eight percent of the effective sample had at least two pregnancies during the eight-year period from age 17 to 25.[14] 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 unfortunately further reduced by the fact that only 50 percent of these women had at least one pregnancy during a policy period and at least one during a nonpolicy period. However, although this reduces the size of the sample used for identification, it does not introduce any further bias. The timing of this eight-year period in each woman's life is orthogonal to the imposition and removal of the policy.

Table 3.2—Effective sample size

	At any time	While aged 17-25
Women in sample	9,097	8,344
Had any pregnancy	6,470	{5,860}
Had 2+ pregnancies	5,273	3,889
Had variation in policy across pregnancies	4,477	2,286

Source: Author's calculation based on DHS data
Note: Includes women aged 15–49 between 1981 and 2007.

[13] This figure is 93 percent for the rural sample.
[14] This figure is 72 percent for the rural sample.

4. ESTIMATION

It is the intention of this estimation to determine whether the imposition (or removal) of the Mexico City policy had any discernible effect on the degree to which abortion is used as a method of birth control in developing countries. Ideally, this estimation would encompass all recipient countries of USAID population assistance. However, given the existence of detailed pregnancy history and abortion data for only one of these countries (Ghana), the estimation is thus restricted. Nonetheless, Ghana seems to be a reasonable test case for this question, given that the potential for funding losses in Ghana was close to the average across recipient countries.

One concern is the degree to which conception and abortion are affected by environmental and situational concerns beyond the policy of focus. 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 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 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. Third, I present some specifications in which the sample is narrowed to a fixed window on either side of the policy change to reduce the impact of time-varying unobservables.

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 often 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.

The primary estimation is

$$A_{imyc} = \alpha + \beta ON_{my} + X'_{imy}\emptyset + M'_{my}\gamma + V_i + V_m + V_c + \varepsilon_{imyc} \quad (1)$$

where A_{imyc} 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 live birth, stillbirth, or miscarriage. The index *c* takes the value 1, 2, or 3, representing which policy change is within the fewest months of my. For example, *c* = 1 represents the change in late 1984 from PRE to ON1, including observations from 1981 to 1988.
c = 2 represents the change in 1993 from ON1 to OFF, including observations from 1989 through 1996. *c* = 3 represents the change in 2001 from OFF to ON2, including observations from 1997 through 2005. X_{imy} is a vector containing quadratic functions of age and parity specific to the 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 V_i, V_m, and V_c, respectively.

The independent regressor of interest is ON_{my}, which indicates that the policy was in effect in month-year *my*. If β is significantly less than zero, this would indicate that, conditional on age, existing parity, and ever-unioned status, a given woman is less likely to abort a pregnancy when the policy is in effect than at other times. Such a finding would provide evidence that the policy achieves this primary objective. If I fail to reject that β is zero, it will be difficult to say whether the policy has any effect. In this case, interactions of demographic indicators with ON_{my} can be used to check for significant effects for separate subgroups.

5. RESULTS

Table 5.1 presents summary statistics regarding conception and abortion rates for various subsamples discussed below. Overall, for women aged 17–25, the probability of conception in a given month (when not already pregnant) is .022. Over the course of a year, the summation of probabilities over each month, conditional on nonconception in the previous months, yields an annual probability of conception of .29. This differs significantly between rural and urban populations (.286 vs. .293) but differs little between the rural subgroups shown. In contrast, the share of pregnancies aborted differs significantly between rural and urban sectors and between rural subgroups. In total, 4.8 percent of rural pregnancies are aborted. However, the poorest of the poor have a rate of 2.6 percent, versus 6.2 percent for the less poor; and those not completing primary school have a rate of 2.7 percent, versus 9.1 percent for those who have completed primary school. Based on these data, it would be surprising to find any effect on abortion use for the poorest (and least educated) of the rural populations, since they are considerably less able (or willing) to access this service in general.

Table 5.1—Summary of contraceptive use, conception, and abortion

	Age 17–25			
	N	Ever Used Contraceptives	Rate of Conception	Aborted Share of Pregnancies
All	10,370	52.5%	2.2%	8.6%
Urban	5,410	59.0%	1.8%	15.3%
Rural	4,960	46.8%	2.5%	4.8%
Rural subgroups				
Poorest	2,166	34.5%	2.7%	2.6%
Less poor	3,244	54.2%	2.5%	6.2%
Less than primary school	3,271	37.6%	2.7%	2.7%
At least primary school	2,139	60.7%	2.3%	9.1%

Source: Author's calculation based on DHS data

Notes: Rates of conception and abortion are for months when women are aged 17–25. Rate of conception is the probability of conception in a month when not already pregnant. "Less poor" is the top three wealth quintiles in the rural population; note that 70% of this group is poor by the international standard of $2/day. "Less than primary school" includes those with no education and those attending some primary school but not completing grade 6.

Table 5.2 shows results from estimations of equation (1) for the full sample and several subgroups. For the full sample, the coefficient is positive, though not statistically different from zero at a standard level. However, based on the confidence interval, we can reject with 95 percent confidence that the policy reduced abortion by more than 0.3 percentage points.

The lack of precision in the full sample results reflects the differences between the policy's effect in urban versus rural areas. The point estimate for the urban population is also positive, but even less precise. We cannot reject that the effect in urban areas is zero. Four urban subgroups are explored, and none shows significant reductions in abortion as a result of the policy (shown in Appendix Table A.4). However, in rural areas, the estimation suggests that the policy increased the use of abortion by 2.35 percentage points; we can reject with 95 percent confidence that this effect is zero. Given that only 4.7 percent of pregnancies are aborted in rural areas, this change reflects a 50 percent increase in the use of abortion—a surprisingly large effect, the potential cause of which is discussed shortly.

Table 5.2—Policy's effect on share of pregnancies ending by abortion

				Rural Subgroups			
	All	Urban	Rural	Poorest	Less Poor	<Primary	Primary+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Policy	.0144	.0044	.0235**	-.0019	.0393***	.0140	.0478**
	(.009)	(.020)	(.009)	(.013)	(.013)	(.009)	(.023)
N	12,439	4,945	7,494	3,155	4,339	5,255	2,239
Individual fixed effects	5860	2434	3426	1427	1999	2316	1110
R^2	.132	.239	.074	.067	.090	.054	.169
Mean of dependent variable	0.089	0.152	0.047	0.026	0.059	0.027	0.089

Source: Author's regression estimates based on DHS data

Notes: Samples include all pregnancy conclusions for women 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. Note that 70% of the top three quintiles are still poor by international standards ($2/day). "<Primary" and "Primary+" delineate whether or not the woman *completed* primary school. Sampling weights are employed. Standard errors are shown in parentheses, clustered at the cluster level.

***; **, and * indicates statistical significance at the 1%, 5%, and 10% level respectively.

In order to explore this surprising effect in the rural population, the last four columns of Table 5.2 present results for rural subgroups.[15] For the poorest two quintiles of the rural population, shown in column 4, the magnitude of the effect is slightly below zero, with a very wide confidence interval. This is consistent with the fact that abortion is prohibitively costly for the poorest of the poor, as evidenced by low use rates shown in Table 5.1. For the remainder of the rural population (column 5), the policy increases the share of pregnancies aborted by 3.9 percentage points. It is notable that while this subpopulation is less poor than those in column 4, 70 percent of them are still poor by international standards (less than $2 per day).

Because wealth quintiles are based on the wealth indicators of the household at the time of the survey, these groups may not reflect the wealth of the woman at the time of each pregnancy. In particular, one might be concerned that the decision to abort an early (or an additional) pregnancy may increase a woman's potential for future wealth. Therefore I employ education as an alternative indicator of socioeconomic status, focusing on whether or not the woman completed primary school. The primary school completion rate is 40 percent in the rural population. This is a characteristic of a woman that is unchanging over time, after about age 12, and certainly by age 16.

The last two columns of Table 5.2 show estimates of the policy's effect for each of the education subgroups. The effect is 4.78 pp for women who have completed primary school. For those who have not, the effect is 1.4 percentage points but is only distinguishable from zero with 85 percent confidence. This likely reflects the very low use of abortion in general for the rural population with less than primary school education, as shown in Table 5.1.

For the rural sample, the less-poor rural sample, and the rural sample that completed primary school, estimated effect sizes suggest that the policy increased the share of pregnancies aborted by 50–65 percent. This effect is in the opposite direction of what was expected, based on the intent of the policy, and is astoundingly large. An explanation for these surprising findings is explored in the next subsection.

[15] In order to check thoroughly for any subpopulation that could potentially exhibit the intended effect of the policy (a reduction in abortion use), estimations for urban subgroups are shown in Appendix Table A.4; none of which is statistically distinguishable from zero.

Policy Effects on Conception Rates

Given that urban populations in this sample have a fairly high abortion rate, the lack of a policy effect in this sector is surprising. Further, considering that the policy was intended to decrease the use of abortion as a means family planning, the significant increase in usage for rural populations is surprising.

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; Turnbull and Bogecho 2003). 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" (TGGRIP 2005, p 6). If such claims are true, we would expect that the reduction in access to contraception would increase rates of conception.

Table 5.3 provides estimates of the policy's effect on the probability of conception in a month when a woman is not already pregnant, for women aged 17–25. Overall, the probability of conception per month increased by 0.0014, representing a 6.4 percent increase that is significant at the 10 percent level. This is consistent with previous estimates of how changes in contraceptive availability affect pregnancy rates.[16] Results for the urban population are not significantly different from zero, reflecting the fact that contraceptives are more broadly available in urban areas. However, according to reports, contraceptive access in rural areas depends on the outreach services provided by groups such as PPAG. The estimates show that when this NGO lost funding as a result of the policy, the probability of conception per month in rural areas increased by 0.0031. This represents a 12 percent increase in pregnancies and is statistically different from zero with 99 percent confidence.

Table 5.3—Policy's effect on probability of conception per month

				Rural Subgroups			
	All	Urban	Rural	Poorest	Less Poor	<Primary	Primary+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Policy	.0014*	-.0006	.0031***	.0032*	.0028*	.0032**	.0027+
	(.001)	(.001)	(.001)	(.002)	(.001)	(.001)	(.002)
N	583,437	284,058	299,379	119,977	179,402	198,316	101,063
R^2	.018	.016	.021	.025	.019	.022	.020
Mean of dependent variable	0.022	0.018	0.026	0.028	0.025	0.028	0.023
Effective increase in pregnancy	6.4%		12%	11.4%	11.2%	11.4%	11.7%

Source: Author's regression estimates based on DHS data.
Notes: Samples include all months in which a woman was aged 17–25 and not already pregnant or concluding a pregnancy. 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 5.2. Sampling weights are employed. Standard errors are shown in parentheses, clustered at the cluster level.
***; **, and * indicates statistical significance at the 1%, 5%, and 10% level respectively.
+ indicates 11% borderline significance.

Columns 4–7 of Table 5.3 show similar estimations for rural subgroups. I find that the policy affected conception rates fairly consistently across groups. For each subgroup I find an 11 to 12 percent increase in pregnancy as a result of the policy; these effects are statistically significant at 15, 10, and 5 percent levels. It seems that rural women were more likely to conceive while this policy was in place—a policy reported to reduce their access to contraception. This suggests that rural women experienced

[16] Molyneaux and Gertler (2000) estimate that, after controlling for changes in demand for contraception, variation in family planning availability explained between 4 and 8 percent of fertility decline in Indonesia from 1982 to 1987. Pritchett (1994) estimates that differences in family planning efforts explain about 5 percent of differences in fertility across countries.

additional unwanted or unplanned pregnancies as a result of the policy. As shown in Table 5.2, these women responded by aborting an additional 2.35 percent of pregnancies. That is, of the additional unwanted pregnancies resulting from this policy, 1 in 6 were aborted.

This certainly suggests that the policy did not achieve its purpose of reducing the use of abortion. Yet a further unintended consequence is perhaps of greater import: 5 out of 6 of those additional pregnancies became unwanted or unplanned births. These additional births are concentrated among the poorest and least educated mothers. Other women allowed unplanned pregnancies to become unplanned births at an estimated rate of about 66 percent. Those without primary education did so at a rate of 88 percent, and the poorest of the poor at 100 percent. Given the evidence that maternal poverty and education are significant predictors of child outcomes, it seems this policy increased the number of children at risk for poor health.

Specification Tests

In the specifications presented above, I include all years within four years of a policy change (that includes all years in the 1981–2007 period). However, one might expect that the effect of a policy (or its removal) would be most salient within a narrower time frame. Table 5.4 shows the estimation of policy impacts on abortion use for rural women, excluding the poorest of the poor, using four different windows of estimation. 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. Column 1 shows that this window provides a larger estimate of policy impact (.0728*). As the window is expanded the effect becomes gradually smaller (.0634***, .0479**, and .0399***).

Table 5.4—Variation in window around policy change

	24 Mos.	30 Mos.	36 Mos.	42 Mos.
	(1)	(2)	(3)	(4)
Policy	.0728**	.0634***	.0479**	.0399***
	(.036)	(.024)	(.019)	(.014)
N	2,252	2,820	3,334	3,826
R^2	.114	.094	.095	.087

Source: Author's regression estimates based on DHS data.
Notes: Samples include pregnancy conclusions within the specified number of months of a policy change for rural women 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 cluster level. ** indicates statistical significance at the 5% level; * 10%.

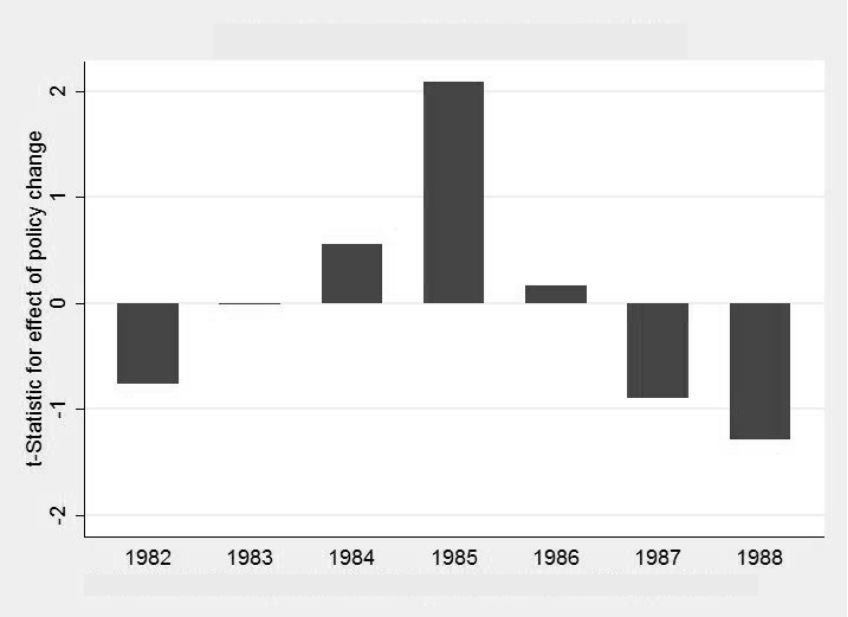
In order to further ensure that the effects I am estimating are due to the policy, I perform a thorough placebo test. This entails re-estimating the primary equation under the false assumption that policy changes happened in years that they did not. First, for each of the three policy changes, I estimate its individual effect, employing all the surrounding years up to the adjacent changes. For example, I estimate the impact of the 1993 change using dates 1986 to 2000, in order to not overlap with the other changes. The t-statistics for the policy effect based on these estimations are shown as the center columns of Figures 5.1A, 5.1B, and 5.1C. While each of these estimations suffers from a large reduction in sample size vis-à-vis the pooled estimations shown in Table 5.2, the t-statistics remain near or above 1.96.

Next, for each policy change, I re-estimate this equation assuming it occurred one, two, or three years before or after it actually did. The t-statistics for the policy effect from each of these estimations are shown as the other bars in Figure 5.1. In none of these estimations does any false year present a positive and statistically significant result. However, the third years following changes 2 and 3 do present statistically significant negative coefficients. This likely reflects the diminishing saliency of the change, consistent with

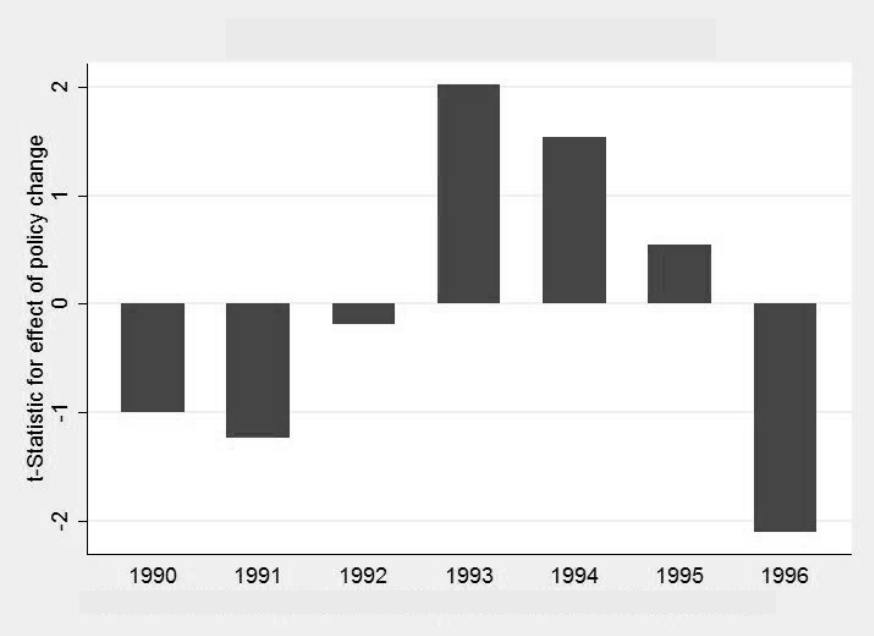
results shown in Table 5.4. Since the first three years following the change are the most effective, assuming the change happened after that reverses the estimated effect.

Figure 5.1—Placebo tests: t-statistics for effect of policy change, under false assumptions of policy timing

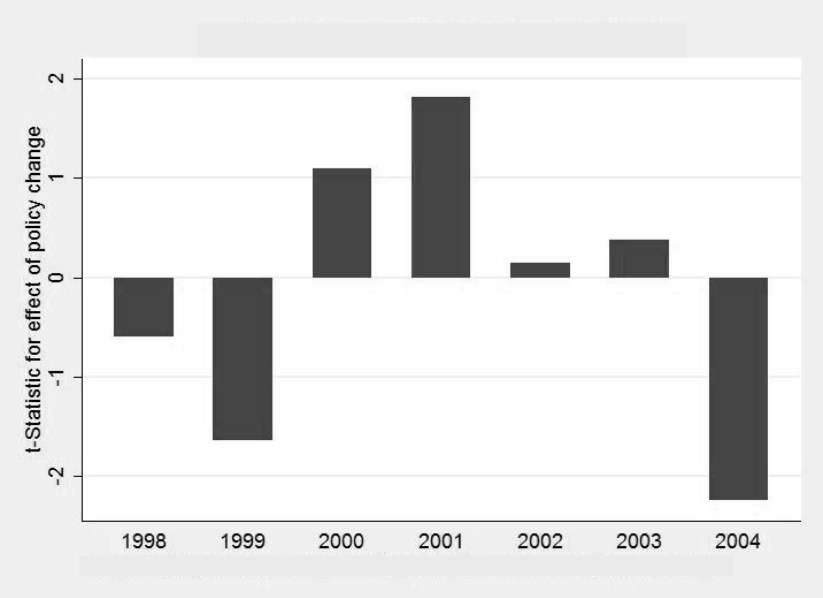
5.1a—Placebo tests for change 1, 1985



5.1b—Placebo test for change 2, 1993



5.1c—Placebo tests for change 3, 2001



Source: Author's regression estimates based on DHS data.
Note: Estimations (falsely) assume the 2001 policy change occurred in the year shown.

Robustness Checks

In order to check for the sensitivity of the results to the assumptions made herein, I present results in Table 5.5 for rural women, excluding the poorest of the poor, under slightly different assumptions.

Table 5.5—Robustness checks

	Excl. 1985	No Weights	Ages 16–26	Ages 15–27
	(1)	(2)	(3)	(4)
Policy	.0459**	.0377***	.0289**	.0342**
	(.018)	(.012)	(.013)	(.015)
N	3,143	4,339	5,021	3,515
R^2	.106	.085	.083	.107

Source: Author's regression estimates based on DHS data.
Notes: Samples include pregnancy conclusions for rural women not in the poorest two quintiles. All specifications include woman fixed effects, calendar month fixed effects, indicators for which policy change is relevant, woman–month level controls, and a cubic time trend as noted. Sampling weights are employed, except where noted. Standard errors are shown in parentheses, clustered at the cluster level. ** indicates statistical significance at the 5% level; * 10%.

A first concern is that the policy change in late 1984 coincided with a liberalization of abortion law in Ghana in 1985. The change in the law allowed abortions to be performed by qualified medical professionals only in cases of rape, concerns for the health of the fetus, or concerns for the physical or mental health of the mother. Prior to this, abortion was not allowed under any circumstance. Because this could induce an upward bias in the estimated effect of the policy, I re-estimate employing only the 1993 and 2001 policy changes. Column 1 shows the estimation excluding years prior to 1989, and the estimated effect is in fact slightly larger than the estimate shown in Table 5.2. Column 2 presents the estimation without employing the sampling weights; the estimate is nearly identical to the original.

In the opening of Section 4, I discuss the need to restrict the age range of women in included observations. The default age range is 17–25, based on the natural breaks in abortion use on either side of

this range. Columns 3 and 4 of Table 5.5 present results under successively larger age ranges. Neither of these differs significantly from the primary estimation.

Impact on Child Outcomes

As discussed above, the evidence suggests that the policy reduced access to contraception, thereby increasing the incidence of pregnancy among rural women. The additional pregnancies were partially, but not fully, offset by increased use of abortion. The estimates suggest that births increased 9.7 percent for rural women overall—additional births that were presumably unintended. For the poorest women, and those without primary education, births increased by about 11 percent, compared to 7 percent for other rural women.

A significant literature has investigated whether unplanned children fare worse than planned children. Of course, many of these studies suffer confounding by unobservable parental characteristics that are correlated with control over fertility outcomes and investment in children. Further, relying on parents to report which children were planned or not can introduce significant bias. This policy results in an exogenous and significant increase in unintended births, the identification of which does not rely on parental reporting of birth planning. In this section, I investigate whether the additional births resulting from this policy exhibit a lower health status compared to their siblings.

Unfortunately, the 2007 DHS data employed for the bulk of this study did not include indicators of child well-being. However, other rounds of DHS data in Ghana collect anthropometric data and information on recent illness for young children. For this investigation, I need data only on children born (not on all pregnancies) and so these other rounds are useful. In particular, the 2003 GDHS collects this information for all children under age five, thereby including children conceived just before and just after the policy change in 2001.

I restrict the analysis to the rural population, based on the primary findings of this study. In the 2003 data, there are 2,596 children under age five at the time of the survey. These children were conceived between September 1998 and October 2003; 40 percent of them were conceived after the policy was reinstated in 2001. The outcomes I examine from this data are the child's standard deviation from the reference mean for height for age and weight for age, severe anemic status, and whether the child has had fever, cough, or diarrhea in the two weeks prior to the survey.

I estimate an equation designed to match equation (2) as closely as possible. That is,

$$H_{imy} = \alpha + \beta ON_{my} + X'_{imy}\emptyset + M'_{my}\gamma + V_i + V_m + V_c + \varepsilon_{imyc} \quad (2)$$

where H_{imy} is a health indicator for the child conceived in month *m* and year *y* by woman *i*.[17] X_{imy} is a vector containing quadratic functions of age and parity specific to woman and month-year, plus an indicator for whether she has ever been in a cohabiting union. *M* represents a cubic time trend. Fixed effects for both the mother and the calendar month of conception are included as V_i and V_m, respectively.

Given the dates of conception for children in this dataset, estimates of differentials in health status will be based solely on the reimplementation of the policy in 2001. One might be concerned that children conceived under the policy in this data are also younger, *mechanically*[18], and may have differences in health indicators for this reason alone. I therefore endeavor to also show results from the 1993 removal of the policy, in which case children conceived under the policy would be older rather than younger.

The 1993 GDHS collected anthropometric and illness information only for children younger than three years. The data contain records on 1,505 rural children under age three, conceived between February 1990 and February 1993. Unfortunately, due to the timing of data collection, only 4 percent of these children were conceived after the removal of the policy in early January 1993. It is unlikely that a 4 percent

[17] Note that only birth dates are given in the data. Conception dates are assumed to be nine months prior.

[18] These data are only for children conceived 1998 to 2003, those conceived under the policy are those conceived in 2001 or later. They are therefore younger than other children in these data, not by some unobservable selection, but *mechanically*.

variation in the variable of interest will provide the statistical power necessary to identify resulting health differentials. Nonetheless, I employ these data, as no other datasets that I am aware of contain anthropometric and health information for rural Ghanaian children before and after the 1993 policy change. These data do not have measures of anemia, but do additionally have the child's standard deviation from the reference mean in terms of arm circumference for height.

Table 5.6 shows estimates of equation (2) for six health indicators, separately for the 2003 and 1993 datasets. In the top panel, estimates from the 2003 data suggest that both height for age and weight for age are significantly reduced for children conceived after the policy was reinstated, compared to their siblings. These measures are reduced by 0.7 and 0.6 standard deviation (SD), respectively. This is consistent with the literature on how child growth responds to hardship. Hoddinott and Kinsey (2001) find that children aged 12–24 months during a significant drought in Zimbabwe have height for age reduced by 0.6 SD compared to their siblings, and as much as 0.9 SD among the poorest households.

It is not uncommon for younger children to perform worse on such measures, as stunted or wasted children can *catch up* with age. However, estimates in the lower panel confirm a statistically significant growth deficit for children conceived under the policy (*policy babies*). Statistical significance in the 1993 estimates is surprising, given the minimal variation in the independent variable of interest. Nonetheless, these coefficients confirm that the estimates shown in the upper panel are in fact due to the policy, rather than to differentials in younger versus older siblings.

Table 5.6—Differential outcomes for children conceived under the policy

	(1)	(2)	(3)	(4)	(5)	(6)
2001 Change	Height for age	Weight for age	Anemia	Fever	Cough	Diarrhea
Conceived under policy	-.7285**	-.6098**	.0937+	.1882**	.1279*	-.1608
	(.282)	(.291)	(.062)	(.082)	(.072)	(.160)
N	2,261	2,261	2,339	2,495	2,509	2,513
R^2	.399	.273	.074	.094	.084	.095
Mean of dependent variable	-1.4524	-1.2129	.0637	.2136	.2188	.3526

1993 Change	**(1) Height for age**	**(2) Weight for age**	**(3) Arm for height**	**(4) Fever**	**(5) Cough**	**(6) Diarrhea**
Conceived under policy	-1.5404*	-1.1762*	-.5803	-.1216	.1046	-.2493
	(.862)	(.640)	(.521)	(.253)	(.125)	(.289)
N	1,297	1,297	1,197	1,463	1,472	1,471
R^2	.469	.393	.071	.176	.170	.215
Mean of dependent variable	-1.2589	-1.3173	-.7866	.3021	.1101	.3243

Source: Author's regression estimates based on DHS data.

Notes: The top panel employs data on rural children as available in the 2003 GDHS. 2,596 living children, conceived between September 1998 and October 2003, were eligible for collection of anthropometric and illness information. 40% of these children were conceived after the policy was implemented in January 2001. The lower panel employs data on rural children as available in the 1993 GDHS. 1,505 living children, conceived between February 1990 and February 1993, were eligible for collection of anthropometric and illness information. Power for detection of differences is low in this sample, as only 4% were conceived after the policy was lifted. All specifications include mother fixed effects, calendar month of birth fixed effects, time-varying mother characteristics, and a cubic time trend. Sampling weights are employed. Standard errors are shown in parentheses, clustered at the cluster level. ** indicates statistical significance at the 5% level; * 10%.

The findings on anemia and arm circumference for height are less precise. The coefficient on anemic status suggests that policy babies have the probability of severe anemia increased by .094. This effect is very large relative to the 6.4 percent prevalence of severe anemia in the sample and is marginally

significant at the 0.15 level. The point estimate for the policy's impact on arm circumference is negative and large but has a very wide confidence interval, and thus is not very suggestive.

Estimates from the 2003 data also suggest that policy babies have an increased probability of suffering fever (by 0.19) and cough (by 0.13) (columns 4 and 5). These effects are distinguishable from zero with 95 and 90 percent confidence, respectively. The point estimate for the policy's effect on cough is consistent in the 1993 data (0.11) but considerably less precise (as expected, given the discussed characteristics of these data). However, the coefficient for fever in the 1993 data is negative with a wide confidence interval. Coefficients for diarrhea were negative and imprecisely estimated in both samples.

The evidence presented here indicates that the additional children born as a result of the Mexico City policy, likely unintended births, exhibit significant growth deficits in terms of height and weight for age. The evidence also hints that these children are more likely to become anemic or suffer cough, but these results are less robust.

6. DISCUSSION

This exercise has endeavored to show whether or not the Mexico City policy accomplished its aim of reducing the use of abortion in foreign nations that receive USAID funding. Lack of data on abortion use in most recipient countries prevents the answering of this question comprehensively. Nonetheless, available data for Ghana allow an analysis of the policy's effect in one country. This provides suggestive evidence of the policy's effect more broadly, as Ghana's USAID funding to reproductive health NGOs was average for such countries prior to policy implementation.

The richness of the Ghana DHS data enables the creation of a woman-by-month panel dataset of conception, pregnancy, and various types of pregnancy conclusions, including abortion. Because the policy was implemented in 1984, rescinded in 1993, and reinstated in 2001, there are three clear breaks in the policy that can be exploited for analysis. Using woman fixed effects, I estimate whether a given woman is more or less likely to abort a pregnancy that occurs under the policy than a pregnancy occurring when the policy is not in place.

Despite the fact that most abortions in Ghana occur among the urban population, the policy did not have a discernible effect in urban areas. This likely reflects the fact that women in urban areas have many options for pregnancy prevention and conclusion, including public provision as well as numerous private providers. Budget cuts to PPAG would be unlikely to significantly alter service provision in urban areas. Given the standard errors in the estimations, I cannot reject the possibility that the policy slightly increased or decreased abortion use in urban areas. Nonetheless, I find no statistically significant evidence that the US policy reduced the use of abortion among urban women in Ghana.

The situation among rural women in Ghana appears to be quite different. According to advocacy groups, it was this sector that was reportedly most affected by the budget cuts resulting from the policy—primarily by reduced access to contraception. I find evidence that this did occur; the conception rate among rural women increased by 12 percent when the policy was in effect.

With pregnancy increasing at a time when contraceptive access is restricted, one assumes that the additional pregnancies are unwanted, or at least unplanned. This is borne out in the results for abortion use. Nearly 20 percent of the additional pregnancies of rural women ended in abortion, yielding a 50 percent increase in the share of pregnancies aborted. This response is less prevalent for the poorest of the poor and for women without a primary school education. For these women, 90 to 100 percent of the additional unplanned pregnancies became unintended births. This suggests that rural Ghana experienced a 9 to 12 percent increase in unplanned births, most of which were born to the poorest and least-educated mothers.

These *policy babies* seemingly suffered from reduced nutrition or healthcare as a result of their unwantedness. Children conceived while the policy was in place exhibit reduced height and weight for age, relative to their siblings, by 0.7 SD. This finding is robust to effects of the policy turning on or off. Additional evidence hints that these children are also more susceptible to anemia and respiratory illness, though these findings are less robust. Given previous findings in the literature that unwanted children suffer higher infant mortality and receive less schooling, it is not surprising to find that an increase in unintended births produces children with poorer health (Chalasani, Casterline, and Koenig 2008; Do and Phung 2010).

If the intent of the proponents of the Mexico City policy is to reduce the use of abortion as a means of family planning, it appears that this policy misses its mark. For no demographic group was evidence found of a significant reduction in abortion during the periods in which the policy was effective. On the contrary, because organizations affected by the policy are also those that provide contraceptives in rural areas, the policy increased the occurrence of unwanted pregnancy for rural women. As unwanted pregnancies increased, the use of abortion increased, particularly among women with the means to obtain it.

While the *pro-life* contingent in the United States would deem the increase in abortion to be the greatest downfall of this policy, further harm is done by it as well. The increase in pregnancy resulting from reduced access to contraception was only partially offset by the use of abortion. The majority of these unplanned pregnancies resulted in children being brought into the world, on average into poor, rural homes without the ability to care for them comfortably or provide them with basic education. This is borne out by

evidence, shown here, that the additional children born as a result of this policy are worse off in terms of nutritional and health status. Further, women who would otherwise have chosen to have no more children experienced the unnecessary risk of additional childbirth. And finally, young women who would otherwise choose to continue their education or further their career were forced into early motherhood. Estimates of the policy's other effects on mothers I leave for future work.

I cannot conclude based on Ghana alone that the policy is wholly unsuccessful in its aims worldwide, or that the unintended consequences are widespread. In many recipient countries the conditions for legal abortion are much more restrictive than in Ghana. In such countries, it is possible that we would not observe the offsetting of increased pregnancy with increased abortion. In these cases, the policy may not increase the use of abortion but would increase the occurrence of unwanted births even more. It is important to note that under normal circumstances USAID funding to PPAG is comparable to funding of reproductive health NGOs in other countries. Therefore, although it may not have increased abortion in some of the other recipient countries, it seems unlikely that it could decrease it, if it did not do so in Ghana.

Much of the American public holds strong opinions on the issue of abortion, on both ends of the spectrum. Therefore, it is common for both political parties to use this issue to engage their constituents. Each party enacts or repeals this policy as a means of garnering popular support. The evidence provided here suggests that such efforts are merely theatrics, as the policy does not seem to accomplish its most basic objective. On the contrary, its imposition has the potential to produce considerable unintended consequences that both parties would agree are undesirable. Following the presentation of this evidence, any further efforts to reinstate this policy could only be considered a wrongheaded political stunt.

APPENDIX: SUPPLEMENTARY TABLES

Table A.1—USAID funds slated to reproductive health NGOs for FY2001

Country	Total Funds	Organizations
Dominican Republic	$ 431,117	Profamilia
Paraguay	$ 598,404	Population Services International (PSI)
Madagascar	$ 600,000	PSI
Kenya	$ 763,088	Family Planning Association of Kenya
Egypt	$ 1,157,543	Center for Development and Population Activities (CEDPA)
Mali	$ 1,192,351	CEDPA
Peru	$ 1,422,215	Profamilia, Asociacion Pro-Bienestar de la Familia (APROFAM), Planification Familiar (Puno)
Nepal	$ 1,490,361	Family Health International (FHI)
Mozambique	$ 1,788,494	PSI
Ghana	$ 1,811,517	Planned Parenthood Federation of Ghana , Association for Voluntary Surgical Contraception (AVSC), Ghana Social Marketing Foundation
Senegal	$ 1,906,493	FHI, Santé de la Famille
Boliva	$ 2,350,000	PSI
Nicaragua	$ 3,549,844	FHI
Guatemala	$ 4,131,624	APROFAM
Bangladesh	$ 4,623,956	AVSC
Indonesia	$ 4,660,801	FHI
Average per Country	$ 2,029,863	

Source: USAID 1999.

Notes: Funds are aggregated by country, in some cases slated to multiple NGOs, as shown. Figures were published in 1999, before the reimposition of the Mexico City policy in early 2001. In some cases, multiyear grants have been divided to represent funds specific to 2001.

Table A.2—Presidential, congressional, and 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 Washington, DC. Case fails in 1989.
1987		Lawsuit	Planned Parenthood Federation of America sues USAID. Case fails in Supreme Court in 1990.
1990		Lawsuit	USAID is sued by coalition of organizations* in US District Court in Washington, DC.
1991		Debate	House debates reversal of policy in Foreign Aid Authorization Bill.
1992	October	Vote	Congress approves language in Foreign Aid Appropriations (FAA) Bill that reverses policy; language dropped under threat of veto by President G. W. Bush.
1993	January	Repeal	President Clinton repeals 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, US House leadership elicits agreement from President Clinton to reimpose modified version of policy.
2000	Fall	Vote	Foreign Operations Appropriations (FOA) Act delays USAID 2001 funding decisions until February 2001.
2001	January	Enact	President G. H. W. Bush reinstates policy.
2001	February	Debate	Bills are sponsored in House and Senate to repeal policy.
2001	March	Enact	President Bush issues memorandum preventing Congress from challenging policy.
2001	May	Vote	Lee Bill attempts to amend Foreign Relations Authorization Act to repeal policy; it fails in House. Similar attempt fails in 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. It fails in 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 House.
2003	August	Enact	President Bush extends policy to apply to State Department funding.
2003	October	Vote	Senate passes Foreign Operations Bill overturning policy; bill fails in House.
2005	April	Vote	Senate approves amendment to overturn policy; bill fails in House.
2005	November	Vote	Senate approves amendment to FOA bill to exempt contraceptives from policy; bill fails in House.
2006	June	Vote	Senate repeals policy; bill fails in House.
2007	June	Vote	House votes to exempt contraceptives from policy.
2007	September	Vote	Senate votes to exempt contraceptives and repeal policy entirely.
2007	December	Veto	Despite votes to exempt contraceptives in both chambers, bill is dropped due to threat of veto by President Bush.
2008	July	Vote	Senate Appropriations Committee adopts full repeal of policy; House is silent on matter.
2009	January	Repeal	President Obama rescinds policy.
2009	January	Vote	Senate defeats amendment proposed to nullify presidential repeal.
2010	July	Vote	Senate Appropriations Committee adopts bill to make future enactments of policy impossible.
2011	April	Vote	House adds language reinstating policy to federal budget bill. Senate refuses to pass budget due to policy riders. Federal government comes within hours of shutdown when Congress cannot pass budget.

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

Notes: *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—History of family planning in Ghana

Year	Event
1961	Christian Council of Churches begins providing family planning information.
1966	Small-scale family planning program emerges in clinics.
1967	Planned Parenthood Association of Ghana (PPAG) is established.
1968	USAID supports Family Planning and Demographic Data Development Project in FY1968–70.
1970	Ghana National Family Planning Program is established, with a secretariat to coordinate all ministries. Between 1970 and 1976, 306 new family planning clinics are registered with the Ministry of Health (MOH).
1971	USAID Phase I assistance to the Government of Ghana in 1971–1975 trains providers and provides contraceptives and informational materials.
1979	Continued USAID support in Phase II (1976–1982) and many centrally funded projects increase access to family planning.
1981	More than 5,000 providers have been trained in family planning.
1985	Ghana Social Marketing Program is established. The $7 million Contraceptive Supplies Project (1985–1990) increases access to modern methods through improved logistics; clinical training; and information, education, and communication in public and private sectors.
1990	MOH and nongovernmental organizations (NGOs) are trained in family planning, especially Ghana Registered Midwives Association and PPAG.
1991	Ghana Family Planning and Health Program (FPHP), a six-year, $30 million USAID-funded project, begins (and continues until 1996); it includes $6.5 million for contraceptive procurement.
1992	National Population Council reporting directly to the president is established.
1994	Navrongo Community Health and Family Planning Project (CHFP) is launched. USAID funds 10-year, $6 million project, Improving Access and Quality of Clinical Family Planning Services in the Public and Private Sectors in Ghana.
1995	Ghana Population and AIDS Project (GHANAPA), a $45 million project, begins. It operates from 1995 to 2000 and is extended to 2002.
1999	National Reproductive Health Service Protocols are established.
2001	Life Choices behavior change campaign for family planning is launched.
2004	Vasectomy promotion campaign begins.

Source: Solo et al. 2005.

Table A.4—Policy's effect on abortion use in urban subpopulations

	(1)	(2)	(3)	(4)
	Poor	Nonpoor	<Primary	Primary+
Policy	.0394*	-.0271	-.0016	.0051
	(.022)	(.033)	(.023)	(.031)
N	2,216	2,729	2,030	2,915
R^2	.207	.289	.134	.327
Mean of dependent variable	.0903	.1995	.0626	.2076

Source: Author's regression estimates from DHS data.

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