Fertility and Family Well-being Effects of an Aggressive Family Planning Policy in Peru in the 1990s

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1 Introduction

In the mid-1990's President Fujimori of Peru initiated an aggressive family planning program with the stated purpose of addressing widespread poverty in the country. The 1991-1992 Peruvian Demographic and Health Survey (DHSII) provided evidence that seemed to bolster Fujimori's claim that there was a "the vicious circle [of] poverty–unwanted child–poverty" in Peru.[1] Table 1, based on data from DHSII, shows the strong negative correlation between wealth (and education) and fertility in Peru. The Peruvian DHSII also indicated an unmet need for contraception with 35 percent of all women who gave birth within the last five years responding that their latest birth was not wanted; this percentage of unwanted last births increases to 65 percent among women

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[1] Address by President Alberto Fujimori at the United Nations, New York, 1999.

with three or more children. Against this backdrop, Fujimori's plan initially had support from the United Nations (UNFPA), USAID and NGOs, if not from powerful conservative religious forces with in Peru. By early 1998, however, claims of sterilizations performed on poor rural women without consent had caused a political uproar in Peru and the controversy spread to the international community. Tubal ligation, a form of female sterilization, was a publicly stated element of the program, but anecdotal evidence suggests that health workers were given large sterilization quotas and often used "bribes," coercion, and even physical force to meet them.[2]

Sterilization quotas were not officially reported by the Fujimori administration and there were no publicly stated guidelines about which populations were targeted by the sterilization campaign. However, Peruvian Demographic and Health Surveys collected in 2000 (DHSIV) and 2004-2008 (DHSV) asked respondents about their current form of contraception and the date they initiated use.[3] Figure 1 shows a dramatic spike in female sterilizations in 1996 and 1997 and an equally dramatic fall by 1998 when the controversy erupted. We will consider 1996-1997 to be the policy window for our analysis.[4] Based on United Nations age-and gender-specific population tables, we estimate that the DHS reports of sterilization from 1996 to 1997 imply that nearly 172,000 women were sterilized in those two years–close to 5 percent of Peruvian women aged 25-49. If we consider the relevant population to be poor women, as reported, the proportion sterilized is much higher.

In this paper we will address three main research questions. Our first goal is to understand who was affected by the Fujimori sterilization policy using the nationally representative random sample of women in the DHS. The second aim of the paper is to estimate the causal impact of the policy on fertility: How many fewer children were born due to the policy? Third, we attempt to understand what, if any, impact a reduction in the counterfactual number of children had on women's employment and on household well-being for those affected by the policy using DHSIV to measure outcomes three years after the policy and DHSV to measure outcomes seven to eleven years after the policy. We tackle these questions sequentially, with each stage feeding into the next.

[2] A report by Guilia Tamayo from the NGO Flora Tristan published in 1999 provides evidence based on interviews with sterilized women and investigations of rural "health festivals." The post-Fujimori government of Alejandro Toledo also produced reports documenting human rights violations under the Fujimori sterilization program. The Toledo government was, however, reported to be opposed to birth control in general on religious grounds (Vasquez del Aguila 2006)

[3] The Demographic and Health Surveys are cross-sectional surveys. So that DHSIV and DHSV do not represent waves of a panel, but rather repeated cross sections.

[4] This evidence from the DHS on the timing of the policy is corroborated Tamayo (1999) and post-Fujimori Health Ministry reports.

We carefully outline the assumptions behind causal identification at each stage. We also attempt to explore and sign any potential bias in our estimates. We continue to conduct robustness checks that test the assumptions and credibility of our causal claims.

There is considerable debate about the causal direction of the correlation between poverty and family size in developing countries like Peru. Fujimori's own claim of a "vicious circle" points directly to the simultaneity—endogeneity— inherent in the study of the link between family planning and economic development. However, credible evidence is vitally important since Fujimori's logic that Peru needed to reduce family size in order to eliminate poverty was the driving force behind a misguided policy that lead to serious human rights violations. The challenges to identification in evaluating population programs are elaborated in recent papers by Shultz (2005) and Moffitt (2005). Both of these papers highlight the true difficulties of establishing causation in population research, but also the great policy importance of accepting these challenges, being honest and clear about assumptions, and seeking out mechanisms that can explain observed behaviors.

There are at least two major challenges specific to the Peruvian sterilization campaign that we must tackle in order to understand who was affected by the policy and then take the next step of identifying the policy's impacts. First, the details of the policy were secret. Second, there was a non-trivial and slightly increasing rate of female sterilization prior to the advent of the 1996-1997 Fujimori sterilization policy as can be seen in Figure 1. This underlying rate of sterilization likely continued during the policy, but we are unable to distinguish directly in the data which women would have been sterilized anyway, and which were sterilized because of the policy. We suspect that sterilizations that were not caused by the policy were more likely to be voluntary. If some underlying level of sterilization continued during 1996-1997, simply looking at all sterilizations that occurred during the policy window will conflate the impact of potentially voluntary and potentially coerced sterilizations—impacts that we suspect may be quite different. Our methodology aims to tackle both of these challenges using the rich information in the DHS to forensically uncover the characteristics of the population that was targeted by the policy. We use the complete birth histories and detailed geographic information available in the DHS along with timing of sterilization to construct a reweighting estimator along the lines of DiNardo et al. (1996). Our estimator of the treatment effect of the policy is modified, however, to account for the fact that the group of all women who were sterilized during the policy make up a "contaminated" treatment group–in the

data we know who was sterilized during the policy period, but among these women we do not know who was treated by the policy.

There is evidence based on hundreds of interviews that women were tricked, pressured, and even physically forced into sterilization procedures in 1996 and 1997 (Tomayo 1999). However, in our data we cannot determine any level of coercion or force during the policy. Furthermore, we cannot confirm that sterilizations that occurred outside of the policy were voluntary. Therefore, going forward, we refrain from using the terms "voluntary" and "coerced" or "forced," and we distinguish, rather, between sterilizations that we predict would have occurred even in the absence of the program, and those that were caused by the 1996-1997 policy. Given that our methodology is based on predictions of which women were in each category, we are further able to tackle the question of whether the impact of sterilization was different among women targeted by the policy compared to women whose sterilizations were not caused by the policy.

We find that women targeted by the Fujimori sterilization policy were on average 31 years old, had four children at the time of sterilization, and 5.6 years of schooling. We estimate that roughly half of the women treated by the policy lived in rural areas and a quarter were from rural mountain regions, but we also find that a significant proportion of treated women came from urban coastal areas like Lima. We estimte that being sterilized by the policy led these women to have 0. fewer children by 2000, and 0.85 fewer children by 2004. We find small and marginally significant impacts of the policy on women's and children's outcomes, with the exception of statistically significant improvements in the height for age and school enrollment of daughters of treated women.

The counterfactual comparisons we use in our estimation procedure rely on the assumption that all of the factors that lead women to be sterilized by the policy are observable and that we have properly controlled for them. This is a strong assumption, one that we continue to examine. For example, we might be concerned that women who were observationally the same as women sterilized by the policy, but who were not sterilized were different in unobserved ways that are correlated with fertility. In particular, we may worry that they are women who had a greater desire for additional children than those who succumbed to the policy. The DHS surveys asked women about the wantedness of all pregnancies in the past five years. We compare responses to these questions in DHS IV between the treatment and control groups created by reweighting and find that the percentages of women who wanted (or did not want) their last pregnancy are not identical,

but the reweighting improves the match considerably.[5] We are encouraged by these results and think this is suggestive evidence that while we are matching on observables, our treatment and control group may also match on unobserved characteristics.

2 Methodology

Our goal is to estimate the effect of being sterilized by the Fujimori Sterilization Policy (FSP) on fertility and on measures of family well being. Estimating *who* was treated by the policy is a crucial first step in accomplishing this goal. This is not the usual first step in treatment effects estimation, but it is required in this case because of the secrecy of the policy and the nature of the information we have about sterilized women. Recall, that we know if a woman was sterilized and when she was sterilized. However, we suspect that some of the women sterilized during the policy period were not treated by the policy–they would have been sterilized anyway. In other words, our information on who was sterilized during the policy period is contaminated information on treatment status. Finally, treatment was assigned based on criteria that is not publicly available and those criteria were likely far from random assignment. To motivate the modifications we make to the standard treatment effects estimation, we will begin by outlining the methods we would use if we had either "ideal" data or at least a more typical amount of information about a policy.

We will use notation standard in the treatment effects literature. We define an indicator S to denote whether a woman is sterilized, and an indicator D to denote if a woman is sterilized (treated) by a sterilization policy. We assume that each woman has two potential outcomes, Y_0 if she is not treated and Y_1 if she is treated. We only ever observe one of these potential outcomes, but we are able estimate the average treatment effect on the treated, $E[Y_{i1} - Y_{i0}|D = 1] = E[Y_{i1}|D = 1] - E[Y_{i0}|D = 1]$, under different assumptions given the type of data available and the way treatment was assigned.

Random assignment allows the most straightforward estimation strategy. If the only sterilizations that took place during the policy period were those caused by the policy and sterilizations were randomly assigned (and furthermore we had data on who was sterilized), we would simply

[5] We only make this comparison for women who had pregnancies in the last five years but before the policy. This restriction is necessitated by the range of data available and our desire to make a proper counterfactual comparison, but limits the sample size. Details can be found in Table 2.

compare the outcomes of sterilized women to those of non-sterilized women. In this case $S = D$ and $E[Y_{i1}|D = 1]$ is observable–the average outcome among sterilized women. The average outcome among non-sterilized women, $E[Y_i|D = 0]$ would be an unbiased estimate of $E[Y_{i0}|D = 1]$ because of random assignment. If there were other sterilizations occurring but we knew who was sterilized by the policy, we would not always have $S = D$ but we would apply the analysis to all women not previously sterilized.

Now consider a scenario where sterilizations were not randomly assigned, but that the only sterilizations taking place were caused by the policy (and we know who was sterilized). Again in this case $E[Y_{i1}|D = 1]$ is observed. But now without random assignment of the treatment, the average outcome of non-sterilized women is no longer an acceptable counterfactual. However, if we believe that we observe all of the characteristics that lead to selection into treatment, we can use those factors to estimate $E[Y_{i0}|D = 1]$ using the non-sterilized population. This could be done with ordinary least squares using a treatment dummy and the necessary controls. Propensity score methods like matching and reweighting would rely on estimating the probability of treatment based on observable characteristics of treated women. Both methods rely on the assumption of selection into treatment on observables and both use the characteristics of the treated group to create a conterfactual comparison group among the non-treated that resembles the treated group. If there were other sterilizations occurring during the policy period, but we knew which women were sterilized by the policy, the same analysis would be conducted by simply removing women sterilized outside of the program from the sample. [6]

In all of the scenarios described above, $E[Y_{i1}|D = 1]$ is observed and the researcher only needs to think of how to find an unbiased estimate of $E[Y_{i0}|D = 1]$. In our case, we do not directly observe $E[Y_{i1}|D = 1]$ because we do not observe D. So we must estimate both $E[Y_{i1}|D = 1]$ and $E[Y_{i0}|D = 1]$. We will use the rich information available in the DHS on birth and martial histories, geographic and demographic characteristics of women sterilized both before and during the policy to separately identify treated women from women who would have been sterilized in the absence of the program. We will use the information in the DHS to estimate propensity scores for probability of sterilization and proceed with a reweighting strategy. The next subsection describes the assumptions

[6] We focus on a propensity score based reweighting method as it makes the intuition of our process more clear and the weights we create allow us to show the characteristics of the women we hypothesize were in the different categories. In this way we can infer from our analysis who was affected by the policy–the first of our research goals.

and modifications to standard reweighting techniques that allow us to estimate both $E[Y_{i1}|D=1]$ and $E[Y_{i0}|D=1]$–the elements necessary to find the average treatment effect on the treated.

2.1 Estimating the treatment effect of the Fujimori sterilization policy

2.1.1 Notation and relevant probabilities

We begin by modifying the notation outlined above to accommodate the unique features of our identification strategy. Because we will distinguish between sterilizations that occurred before and during the policy, we now define an indicator variable S_t to denote whether a woman is sterilized during a given period. A woman who is sterilized in period t will have a value of one for S_t and a value of zero otherwise i.e. $s_t \in \{0,1\}$.[7] The index $t \in \{1,2\}$ equals one if the time period is prior to the FSP time period (1990-1994), and equals two if the time period coincides with the FSP time window (1996-1997).[8] Since sterilization is a permanent one-time procedure, a woman can only have $S_t = 1$ in one of the periods.[9] We are mute regarding sterilizations that happened after the FSP was dismantled. In other words, we assume that they would have happened regardless of the FSP and thus are included in our control groups.

Our treatment of interest is sterilization by the FSP, denoted by D, $d \in \{0,1\}$. Women who were sterilized because of the FSP have a value of one for D, and women who were not sterilized or whose sterilizations were not caused by the FSP have a value of zero. So that a woman who was sterilized in 1997 by the FSP would have $S_1 = 0$, $S_2 = 1$, $D = 1$ and a woman who was sterilized during the FSP window but would have been sterilized regardless of the policy would have $S_1 = 0$, $S_2 = 1$, $D = 0$.[10] Finally, we denote other observed variables by X.

Now we define a series of probabilities we will use in our estimation strategy and the assumptions required to estimate them given our data. Equation 1 gives the probability of a woman with observed characteristics x, becoming sterilized during the FSP time period (given that she was not sterilized before)

[7] As is conventional, capital letters denote random variables and small letters denote specific realizations of those random variables.

[8] We leave out the calendar year 1995 because it is possible that the FSP might have started in the later part of that year. But we should and are in the process of conducting robustness checks for sensitivity of our results to including 1995 - for DHSIV it doesn't seem to matter.

[9] In the data we do not find any sterilized women who give birth after the date they report being sterilized.

[10] Other relevant categories are women who were sterilized prior to the FSP who would have $S_1 = 1$, $S_2 = 0$, $D = 0$ and women who were never sterilized by the end of the FSP would have $S_1 = 0$, $S_2 = 0$ and obviously $D = 0$.

$$\begin{aligned} P(S_2 = 1|X = x) =& P(S_2 = 1,\ D = 0|X = x) + P(S_2 = 1,\ D = 1|X = x) \\ =& P(S_2 = 1,\ D = 0|X = x) + P(D = 1|X = x) \end{aligned} \tag{1}$$

The first equality holds because $D = 0$ and $D = 1$ are mutually exclusive and collectively exhaustive events. The second equality holds because $S_2 = 1$ for all cases where $D = 1$. Assumption 1 below allows us to exploit the information we have about women prior to the FSP.

Assumption 1: The probability of a woman being sterilized during the policy period who would have been sterilized even in the absence of the policy is
the same as the probability of sterilization before the FSP was implemented for a woman with similar observable characteristics (X).[11] In other words we assume that

$$P(S_2 = 1,\ D = 0|X = x) = P(S_1 = 1|X = x) \tag{2}$$

Under Assumption 1 we can re-write equation 1 to express the probability of being treated as:

$$P(D = 1|X = x) \quad = \quad P(S_2 = 1|X = x) - P(S_1 = 1, |X = x) \tag{3}$$

To simplify notation we re-write the probability of sterilization in the pre-policy period for a woman with observed characteristics x as $P_1(x)$ and the probability of sterilization in during the FSP as $P_2(x)$.[12] Thus equation 3 becomes

$$\begin{aligned} P(D = 1|X = x) =& P_2(x) - P_1(x) \\ =& \triangle P(x) \end{aligned} \tag{4}$$

We can also define the probability of being treated conditional on being sterilized during the FSP time period

$$P(D = 1|S_2 = 1,\ X = x) = \frac{P(D = 1|X = x)}{P(S_2 = 1|X_i = x)} = \frac{\triangle P(x)}{P_2(x)} \tag{5}$$

[11] We allow for a time trend in our estimation to capture the underlying national trend in sterilization take-up prior to the implementation of the FSP.

[12] $P_2(x)$ and $P_1(x)$ can be estimated in the data using a probit or a logit, and can be thought of as the propensity scores used in matching estimators.

Finally, we modify our notation regarding the potential outcome of interested Y_d, where d indexes by the state of the treatment variable D. The outcome that is realized (and observed) is Y, which is not indexed by d. Since we do not know who was sterilized by the FSP (treated) and who would have been sterilized even in the absence of the FSP (non-treated) among women who were sterilized during the years 1996-1997 (i.e. $S_2 = 1$) the observational rule is modified from the standard case. Equation 6 gives the modified expression for the observed outcome in terms of the relevant potential outcomes

$$\begin{aligned} Y =& s_2 \left[dY_1 + (1-d) Y_0 \right] + (1 - s_2) Y_0 \\ Y =& s_2 \tilde{Y} + (1 - s_2) Y_0 \end{aligned} \tag{6}$$

The term $\tilde{Y} = [dY_1 + (1-d) Y_0]$ highlights the fact that not all women sterilized from 1996-1997 were treated by the policy. In other words, $\widetilde{Y}$ represents the outcomes of the contaminated treatment group.

As discussed above, our goal is to estimate average treatment effect on the treated (ATT), i.e. the impact of the sterilization on those women who were sterilized by the FSP

$$\begin{aligned} ATT =& E[Y_1 - Y_0 | D = 1] \\ =& E[Y_1 | D = 1] - E[Y_0 | D = 1] \end{aligned} \tag{7}$$

2.1.2 Density Reweighing approach to estimate the treatment effect on women treated by the policy

Given our observational rule in equation 6, we cannot directly estimate the first term of equation 7, $E[Y_1|D = 1]$. However, note that

$$E[\tilde{Y}|D = 1] = E[Y_1|D = 1] \tag{8}$$

We observe $\tilde{Y}$ for women sterilized during the FSP period ($S_2 = 1$) and we can use the proba-

bilities derived above to estimate $E[\tilde{Y}|D=1]$, under certain assumptions. Note that

$$\begin{aligned} E[\tilde{Y}|D=1] &= \int \tilde{y} f(\tilde{y}|D=1) d\tilde{y} \\ &= \int\int \tilde{y} f(\tilde{y}, x|D=1) dx d\tilde{y} \end{aligned}$$

If we multiply and divide the integrand by $f(\tilde{y}\, x|S_2=1)$ and then apply Bayes Rule to the numerator and denominator we get

$$\begin{aligned} E[\tilde{Y}|D=1] &= \int\int \tilde{y} \frac{f(\tilde{y},\, x|D=1)}{f(\tilde{y},\, x|S_2=1)} f(\tilde{y},\, x|S_2=1) dx d\tilde{y} \\ &= \int\int \tilde{y} \frac{f(D=1|\tilde{y},\, x) f(\tilde{y},\, x) f(S_2=1)}{f(S_2=1|\tilde{y},\, x) f(\tilde{y},\, x) f(D=1)} f(\tilde{y}, x|S_2=1) dx d\tilde{y} \end{aligned} \tag{9}$$

Next we introduce the standard assumption in matching estimators:

Assumption 2: Strong Ignorability Assumption. We assume that after conditioning on X the probability of being treated and of being sterilized during the years of the FSP are independent of the potential outcomes $\{Y_0,\, Y_1\}$ and, thus, they are also independent of $\widetilde{Y}$. In other words, and invoking equation 4 we assume that

$$f(D=1|y_0,\, y_1,\, x) = f(D=1|\tilde{y},\, x) \quad = \quad f(D=1,|x) = \triangle P(x) \tag{10}$$

$$f(S_2=1|y_0,\, y_1,\, x) = f(S_2=1|\tilde{y}, x) \quad = \quad f(S_2=1|x) = P_2(x) \tag{11}$$

Using Assumption 2 we can re-express equation 9 as

$$\begin{aligned} E[\widetilde{Y}|D=1] &= \frac{f(S_2=1)}{f(D=1)} \int\int \tilde{y} \frac{f(D=1|x)}{f(S_2=1|x)} f(\tilde{y}, x|S_2=1) dx d\tilde{y} \\ &= \frac{P(S_2=1)}{P(D=1)} \int\int \tilde{y} \frac{\triangle P(x)}{P_2(x)} f(\tilde{y}, x|S_2=1) dx d\tilde{y} \end{aligned} \tag{12}$$

Since we have a sample from $f(\tilde{y},\, x|S_2=1)$ we can estimate the expected value in equation 12

with the finite sample estimator: [13]

$$\widehat{E[\tilde{Y}|D=1]} = \widehat{E[Y^1|D=1]} = \frac{\sum_{i=1}^{N} y_i w_{1i} s_{2,i}}{\sum_{i=1}^{N} w_{1i} s_{2,i}} \tag{13}$$

$$w_{1i} = \frac{\widehat{\triangle P(x_i)}}{\widehat{P_2(x_i)}} \times \phi_i \tag{14}$$

Where ϕ_i is the DHS sampling weight of woman i. Thus, the expected value $E[Y_1|D=1]$ is a weighted average of the observed outcome, Y, for women who were sterilized during the FSP time period. The weights are proportional to the probability that, conditional on being sterilized during that period, the woman was a induced to be sterilized by the policy (see equation 5).

Invoking the strong ignorability assumption described by Assumption 2 and following similar steps as before, it can be shown that $E[Y_0|D=1]$ is given by:

$$\begin{aligned}
E[Y^0|D=1] &= \int y_0 f(y_0|D=1)dy_0 \\
&= \int\int y_0 f(y_0,\, x|D=1)dxdy_0 \\
&= \int\int y_0 \frac{f(y_0,\, x|D=1)}{f(y_0,\, x|S_2=0)} f(y_0,\, x|S_2=0)dxdy_0 \\
&= \int\int y_0 \frac{f(D=1|y_0,\, x) f(y_0,\, x) f(S_2=0)}{f(T=0, S=0|y^0,\, x) f(y^0,\, x) f(D=1)} f(y_0,\, x|S_2=0)dxdy_0 \\
&= \frac{P(S_2=0)}{P(D=1)} \int\int y_0 \frac{\triangle P(x)}{1-P_2(x)} f(y_0,\, x|S_2=0)dxdy_0
\end{aligned}$$

And the sample estimator is given by:

$$\widehat{E[Y_0|D=1]} = \frac{\sum_{i=1}^{N} y_i w_{0i}(1-s_{2,i})}{\sum_{i=1}^{N} w_{0i}(1-s_{2,i})} \tag{15}$$

$$w_{0i} = \frac{\widehat{\triangle P(x_i)}}{1-\widehat{P_2(x_i)}} \times \phi_i \tag{16}$$

Thus, the expected value $E[Y_0|D=1]$ is a weighted average of the observed outcome, Y, for

[13] The finite sample estimator of equation 12 is given by:

$$\widehat{E[\tilde{Y}|D=1]} \quad = \quad \frac{P(S_2=1)}{P(D=1)} \frac{\sum_{i=1}^{N} y_i w_{1i} s_{2,i}}{\sum_{i=1}^{N} s_{2,i}}$$

However, note that the population value $\frac{P(S_2=1)}{P(D=1)}$ can be approximated in finite samples by $\left[\frac{\sum_{i=1}^{N} w_{1i} s_{2,i}}{\sum_{i=1}^{N} s_{2,i}}\right]^{-1}$, which gives the expression in equation 13.

women who were not sterilized during the FSP time period,where the weights allow us to construct a counterfactual control group for the treated group. We can re-write the weights to give them a clearer interpretation:

$$\begin{aligned} w_{0i} =& \frac{\widehat{P_2(x_i)}}{1-\widehat{P_2(x_i)}} \times \frac{\widehat{\triangle P(x_i)}}{\widehat{P_2(x_i)}} \times \phi_i \\ =& \frac{\widehat{P_2(x_i)}}{1-\widehat{P_2(x_i)}} \times w_{1i} \end{aligned} \tag{17}$$

In this form, it becomes evident that the weights are the result of a two-step (matching) procedure. In the first step, described by the term $\frac{\widehat{P_2(x_i)}}{1-\widehat{P_2(x_i)}}$, we reweigh the outcomes of women who were not sterilized during the FSP time period by giving higher weights to the outcomes of those women who are observationally more similar to women that were sterilized during the FSP time period. The second step is essentially the same as the reweighting performed before on the $S_2 = 1$ group and thus is described by the term w_{1i}. In other words, in the second step we give more weight to the outcomes of women with a higher counter-factual probability of being treated by the FSP (given the counterfactual of being sterilized at all during the FSP time period).

By joining the results of equations 13 and 15, we can estimate the ATT using:

$$\widehat{ATT} = \frac{\sum_{i=1}^{N} y_i w_{1i} s_{2,i}}{\sum_{i=1}^{N} w_{1i} s_{2,i}} - \frac{\sum_{i=1}^{N} y_i w_{0i} (1-s_{2,i})}{\sum_{i=1}^{N} w_{0i} (1-s_{2,i})} \tag{18}$$

We have focused so far on the impact of sterilizations that were caused by the FSP, but we could also, for comparison purposes, be interested in the impact of sterilizations that occurred outside of the policy. Using the same procedures as before, it can be shown that the ATT for sterilizations that occurred during 1996-1997 but would have occurred even in the absence of the policy can be estimated using equation (19):

$$E[Y_1 - Y_0 | \widehat{S_2} = 0, D = 1] = \frac{\sum_{i=1}^{N} y_i \widetilde{w}_{1i} s_{2,i}}{\sum_{i=1}^{N} \widetilde{w}_{1i} s_{2,i}} - \frac{\sum_{i=1}^{N} y_i \widetilde{w}_{0i} (1-s_{2,i})}{\sum_{i=1}^{N} \widetilde{w}_{0i} (1-s_{2,i})} \tag{19}$$

where $\widetilde{w}_{1i} = \frac{\widehat{P_1(x_i)}}{\widehat{P_2(x_i)}} \times \phi_i$ and $\widetilde{w}_{0i} = \frac{\widehat{P_1(x_i)}}{1-\widehat{P_2(x_i)}} \times \phi_i$

3 Data

3.1 Peruvian Demographic and Health Surveys

We investigate the Peruvian sterilization policy using the fourth, and fifth waves of the Demographic and Health Surveys for Peru (hereafter DHS IV, and DHS V.) The Demographic and Health Surveys are nationally representative cross sectional surveys. Both DHS IV and DHS V were conducted after the policy had ended and thus allow us to look at potential impacts on fertility and other household outcomes. DHS IV was conducted in 2000 and has a sample size of 27,843 women aged 15-49; and DHS V was collected continuously over the course of 2004 to 2008 and has a sample size of 41,648 women. The primary advantage of the survey for addressing our research questions is the information collected on birth control methods including sterilization and the date when the sterilization occurred. The surveys also include detailed birth histories and information on place of residence. Our analysis sample includes all women who were eligible to be sterilized during the policy period 1996 to 1997– ever-married women who had at least one child and who were not previously sterilized[14]–giving us a sample size of 14,430 eligible women in DHSIV, 707 of whom were sterilized during the policy period; and 16,673 eligible women in DHS V, 735 of whom were sterilized during the policy.

We estimate the impact of the FSP on fertility as well as on household outcomes. To measure fertility we use the number of surviving children in a given year. We can also use the birth histories to measure number of children ever born and child mortality, which are alternative outcomes we plan to pursue. Next we look at the impact of the policy on women's outcomes. The DHS has limited information on labor force outcomes, but we make use of a question asking whether the respondent is currently working and we use this as a proxy for labor force participation. We also estimate the impact on reports of domestic violence in the last 12 months as sterilization could impact a woman's bargaining power relative to her spouse.

We examine several outcomes of household children to test whether the policy impacted well-being as measured through health and education. We want to compare children whose mothers' were sterilized–and therefore had no more siblings–to counterfactual children whose mothers were

[14]There was an existing law prior to the policy requiring spousal consent for sterilization (Coe 2004) and all women who report sterilizations in both DHS were married (or had previously been married) and had at least one child at the time of sterilization.

not sterilized and therefore likely had younger siblings. This kind of comparison would allow us to test a quality/quantity trade-off. Therefore, we only look at outcomes for children born prior to the policy. Weight for height and height for age was collected for all children age four and under, so we are restricted to DHS IV for this outcome since all children under four were born after the policy by the time the DHS V survey began in 2004. In both DHS IV and DHS V we measure years of schooling and current enrollment (controlling for age) of household children under 15. In the DHS V we can also examine the education level of girls over 15 who are old enough to be survey respondents and but were children at the time of the policy. Having fewer younger siblings to help care for, could have allowed girls to stay in school.

One limitation of the Peruvian DHS is that is does not contain accurate information on respondents' ethnic group. One of the claims of human rights activists is that the Fujimori policy targeted indigenous women from the Quechua or Aymara groups. DHS IV and V ask respondents their language among which Quechua or Aymara are choices. But only 15% of eligible women DHS V responded that they spoke either of these languages. This variable clearly does not accurately measure ethnicity, as the Amerindian population is closer to 40% of the Peruvian population.

3.2 Descriptive Analysis of Sterilization in Peru 1990-1998

Table 1 presents summary statistics from DHS IV and DHS V (and from DHS II (1990-1002) prior to the policy) relating to fertility highlighting the strong negative correlation between family size and income (proxied by education) or wealth. We see that going from the highest to the lowest levels of mother's education doubles the number of living children for mothers over 40 from 2.2 children to more than 5.4 in 2000. This contrast is similarly strong across the wealth index. Rural households have substantially more children than urban households. Comparing number of children across the two surveys we see that fertility decreased at all education and wealth levels from 2000 to the 2004-2008 period.

Figure 1 shows the number of sterilizations reported in DHS IV and DHS V by year, and confirms the sharp increase in sterilizations during the policy period. In the analysis that follows we will consider 1996 and 1997 to be the aggressive sterilization "policy period." Using age- and gender-specific population estimates for Peru from the United Nations Population Division, we can estimate the number of actual sterilizations implied by the self reported sterilizations in the

nationally representative DHS surveys. Based on the UN estimated Peruvian population of women age 15 to 49 in 2000, the DHS IV is a 0.41 percent sample of the relevant population. Based on this sampling scale, the 417 sterilization reported in 1997 (representing 408 women when weighted) imply that 99,430 women were sterilized in that year, which is remarkably similar to the numbers reported by Fujimori's opponents. If we sum together all of the DHS IV reported sterilizations that occurred during the supposed policy period from 1996 to 1998, we estimate that roughly 218,626 women were sterilized. This implies that approximately 3.4 percent of women age 15 to 49 were sterilized, or 4.5 percent of women age 20 to 45 who were in their prime fertility years. If certain regional or demographic characteristics were specifically targeted the percentage of the relevant population that was sterilized could be much higher. There are some notable differences between the DHS IV and DHS V surveys in the reporting on female sterilization that occurred during the 1990s. Looking at Figure 1 we see that the increase in sterilizations from the pre-policy period to the policy period was more gradual in DHS V and less abrupt than in DHS IV, and similarly more gradual for the decrease in sterilizations after the policy ended. Part of the difference could be recall bias given that the DHS V survey took place eight to 12 years after the policy, while DHS IV was conducted only three years from the peak of the policy. DHS V collected a smaller nationally representative sample in each of the five years of the survey, which ranged from a 0.07 percent sample of the age-/gender-specific population in 2004, to a 0.2 percent sample in 2008. The implied number of sterilizations in 1997 based on DHS V reports is 79,752 and the total number of sterilizations over from 1996 to 1998 is 179,352, or 2.8 percent of women aged 15 to 49. It is possible that some of the sterilizations that actually occurred during the policy were mistakenly reported to occur in the year before or after the policy ended in DHS V. If this were the case, we would expect to find muted treatment effects using DHS V data. These discrepancies deserve further investigation.

3.3 Estimating the probability of treatment by the Fujimori sterilization policy using DHS IV and DHS V

We estimate the probability of sterilization in the pre-policy and policy periods using a pseudo panel constructed from the cross-sectional data in the DHS surveys. These probabilities, conditional on observable characteristics, are the propensity scores $P_1(x)$ and $P_2(x)$ described in the methodology section. We use the date of sterilization and other variables to construct a longitudinal history

for each woman describing her fertility and marital time path from the beginning of what we consider the pre-policy period, 1990 to the end of the policy period in 1998. Each woman has one observation for each year and dummy indicating whether she is sterilized in each year. Once she is sterilized she has no further observations. Using this type of quasi panel allows us to estimate the conditional probability of being sterilized in each year—the annual hazard of being sterilized given that one has not been sterilized up that point.[15] This approach takes account of the fact that a woman sterilized in 1997 was at risk of being sterilized in all previous periods and as such should be included in calculating the probability of being sterilized in 1994, for example. Furthermore, because of detailed birth and (somewhat) detailed marital histories provided in the DHS surveys we can use richer information about spacing of children in the pseudo panel than in a cross sectional estimation of probability of sterilization by year. We also include 56 regional categories starting with Peru's 25 departments and further differentiating by geography (jungle, mountain, coastal) and by urban and rural status. Other covariates are age, number and age of children, number of boys, infant mortality, age at first birth, and education. Finally, using the pseudo panel we are able to include a time trend in the logit to account for secular changes in fertility and sterilization that could be occurring within each period. We estimate the probability of sterilization in each period using a logit.

When we calculate $\Delta P(x) = P_2(x) - P_1(x)$ as in equation 4 in some cases the value is negative leading $P(D = 1|S_2 = 1, X = x)$ (equation5) to be less than zero. Since this object is a probability, negaive values are not defined and we set these values to zero. The rationale is that such women, if sterilized, had a zero probability of of being treated by the policy. The number of observations for which we make this adjustment is noted in the results tables.

[15] This is based on a extension of proportional hazard models to discrete time proposed by Cox (1972). Estimating a logit regression on a set of pseudo observations generated from a cross-section amounts to fitting a discrete-time proportional-hazards model. See Allison 1982 and notes on this by German Rodriquez at Princeton: http://data.princeton.edu/wws509/notes/c7s6.html.

4 Results

4.1 Characteristics of women targeted by the Fujimori sterilization policy

Table 2, based on DHS IV, and Table 3, based on DHS V, show the characteristics of the sample of eligible women before and after reweighting. The first three columns of Tables 2 and 3 give the characteristics of the non-reweighted sample of women eligible to be sterilized during the policy—ever married women with at least one child who were not previously sterilized–separated by whether they were sterilized during the policy period. The findings based on DHS IV and DHS V are similar, so we will summarize them jointly. Column 2 gives the characteristics of what we have called the contaminated treatment group which includes both women treated by the FSP and women who were not treated by the policy and would have been sterilized even the absence of the program. We see that sterilized women are older, have more children, slightly less education than the average eligible woman, but that a roughly similar proportion of women sterilized during the policy live in rural areas.

In columns 4 and 5, we apply the weights described in the methodology section to create the effective treatment and control groups we use to estimate the impacts of the policy. The group of women who were sterilized during 1996-1997 are reweighted to represent only the group of women who were treated by the policy (ie we use weight ω_0). The non-sterilized women are reweighted to match the observable characteristics of the treated women (ie we use weight ω_1). The differences compared to column 2 are striking. The women we estimate to be in the treatment group are younger, considerably less educated, and much more likely to live in rural areas than the contaminated treatment group suggested. The last two columns of Tables 2 and 3, specifically highlight the differences between women who were sterilized by the policy and sterilized women who we estimate would have been sterilized even in the absence of the policy. Women sterilized outside of the policy are considerably more educated and more likely to live in urban areas, though they do not have substantially fewer children. If we suspect there may be heterogeneous treatment effects of sterilization by these characteristics, then these columns confirm the benefit of our method in separating these two types of sterilized women. Column 6 gives the demographic characteristics of the women we estimate were affected by the Fujimori Sterilization Policy and thus provide an answer to our first research question: who was targeted by the Fujimori Sterilization Policy? We

estimate that women targeted by the policy were on average 31 years old, had four children at the time of sterilization, and 5.6 years of schooling. Their average age at first birth was 19. Roughly half of these women lived in rural areas and a quarter were from rural mountain regions, but we also find that a significant proportion of treated women came from urban coastal areas like Lima.

4.2 Impact of the FSP on fertility

Table 4 shows the estimated impact of the FSP on fertility. The following pattern will be used in all of the subsequent results tables (unless otherwise noted): The first three columns give results based on DHS IV which was collected three years after the policy. The next section of three columns are based on DHS V which was collected seven to eleven years after the policy. The first column in each section gives the results of the standard reweighing estimation that only reweights observations of women not sterilized during the FSP. In other words these standard reweighting estimates do not accounting for the contamination of the treatment group and conflate the impact of sterilization on women treated by FSP and those who would have been sterilized even in the absence of the FSP. The second column gives our preferred specification– the estimated ATT for women sterilized by the FSP based on the reweighting technique described in the methodology section (in notation these women have $S_2 = 1$, $D = 1$.) Finally the third column is the estimated impact of sterilization on outcomes for women who were sterilized during the policy period but were not treated by the FSP (in notation these women have $S_2 = 1$, $D = 0$.)

In column 2, based on DHSIV,we estimate that by 2000, women treated by the FSP had 0.33 fewer children than the non-sterilized control group , and in column 5, based on DHSV, we estimate that by 2004, treated women had 0.85 fewer children. These estimates of the impact on fertility are larger than the standard reweighting estimates in columns 1 and 4, suggesting that the policy had a stronger impact on fertility among treated women than among women who would have been sterilized anyway. This is shown by the estimates in columns 3 and 6 which give the estimated impact of sterilization on women sterilized outside of the FSP. We find that women sterilized outside of the FSP had 0.22 few children by 2000, and 0.58 fewer children by 2004 than relevant counterfactual women. All of the coefficients in table 4 are negative, but could also be expressed positively as the number of additional children born to women in the control group(s). We think these results are large but plausible given the age and existing fertility of the treated women, the amount of time

since the policy, and limited access to contraception available in Peru.

4.3 Impact of the FSP on household outcomes

The remaining tables provide estimates of the impact of the FSP on women's, and children's outcomes. Since in the previous section we estimate that the policy led to a substantial decrease in fertility, we can hypothesize that any impacts on other outcomes were the result of lowered fertility. However, at this point we cannot rule out impacts of the nature of the policy itself, for example the trauma of a coercive act, on outcomes. In this summary of results, we will focus on the estimated impact of the FSP in columns 2 and 5 of the table. Table 5 provides estimates of the impact of the policy on women's labor force participation. Column 2 shows an increase in probability of working of five percent in 2000 based on reweighting which is significant at a 10 percent level. However, there is no significant impact on working by DHS V in 2004-2008 as seen in column 6. Table 6 shows estimates for the binary outcome of experiencing domestic violence (either physical or sexual) in the last 12 months (this information is only available in DHSV). We estimate that being sterilized by the FSP increased the likelihood of experiencing domestic violence by 5 percent. Table 3 shows that 13 percent of eligible women report domestic violence in the last 12 months. We need to further explore the mechanisms of this estimated impact, but changes in ability to bear children may impact women's bargaining power within the household. We view our estimated impact on domestic violence with caution, however, as it could be the case that women susceptible to domestic violence could also be those more susceptible to a coercive government policy.

Turning to the impact of the FSP on the children of sterilized women, in summary we find small and mostly non- or marginally significant impacts when we combine girls and boys. Table 7 gives the estimated impact on biometric measures of weight for height and height for age (in standard deviations from the reference median) based on DHSIV among children under four who were born prior to the policy. This table only shows results for DHSIV since children born prior to the policy in DHSV are over four years old, and the surveys only record biometric information for children under four. Point estimates on weight for height in column 5 are small and none are statistically significant. Impacts on height for age in column 2, which is a longer term measure of health, are somewhat larger, but again, for the most part, not significant. When we examine girls separately in Table 8, however, we find that daughters of women treated by the FSP had height for age that

was 0.29 standard deviations greater than counterfactual girls. This estimate is significant at a one percent level. Looking at column 3, we see that there is a similar, though not significant positive impact on daughters of women sterilized outside of the FSP.

In Tables 9 and 10, we find a small but significant positive impact on years of schooling and enrollment for children under age 15 in DHSIV three years after the policy.. While the magnitude of the impact is similar for DHSV, seven to 11 years after the policy, the estimates are not statistically significant. When we look just at girls enrollment in Table 11, we find that there is a 2.3 percent increase in school enrollment for girls of women sterilized by the FSP about double the impact found when we combined boys and girls. These small impacts are likely due to the high levels of enrollment of primary school children in Peru, even in rural areas, leaving little margin to increase schooling for these ages. However, we find no impact on education of older girls in Table 12 based on DHSV. These are girls who are aged 15 to 22 and are respondents to DHSV as adults, but were children at the time of the policy. The fact that their mothers were sterilized could mean they had fewer young siblings to help care for than counterfactual girls and were able to stay in school longer. We do not find evidence of this kind of impact. However, if this impact accrued to girls who then moved out of the house younger, we will not be able to measure the effect.

5 Conclusion

There is a continuing debate about the causal link between access to family planning and reductions in fertility in both the developed and developing world. Beyond any direct impact on the level of fertility, access to contraception clearly allows women to control the timing of fertility, which reduces constraints on choices about work and caring for existing children. Recent research in both the United States (Bailey 2006) and Columbia (Miller 2009) uses plausibly exogenous variation in access to show that contraception significantly increases female educational attainment and labor force participation by allowing women to delay first births. Our preliminary findings in Peru seem to confirm that the mere reduction of fertility that is not necessarily associated with substantial improvements in welfare in the context of potentially coerced sterilizations. We are finding that when birth control is imposed, the benefits of making choices about fertility may not accrue to women and their households. While we do find small improvements in height for age and school

enrollment for girls whose mothers were sterilized by the Fujimori sterilization policy, in general the substantial decrease in fertility caused by the policy does not seem to be associated with substantial improvements in family well-being.

It is clear to us that the Fujimori sterilization policy involved eggregious human rights violations. Our goal is to document the impact of this policy in the hopes that future family planning policies will focus on improving the choices available to women and their families rather than imposing a single contraceptive alternative.

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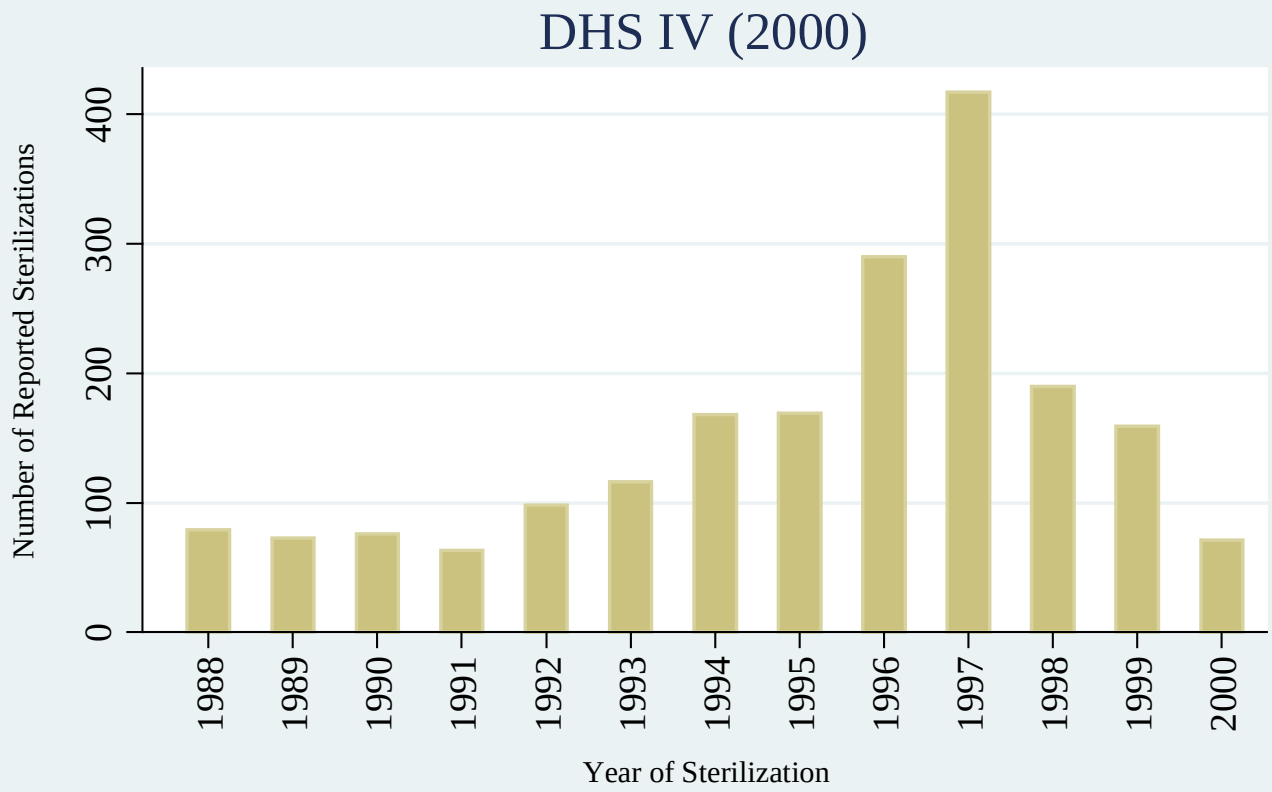
Figure 1. Number of Reported Sterilizations by Year

Peruvian Demographic and Health Surveys



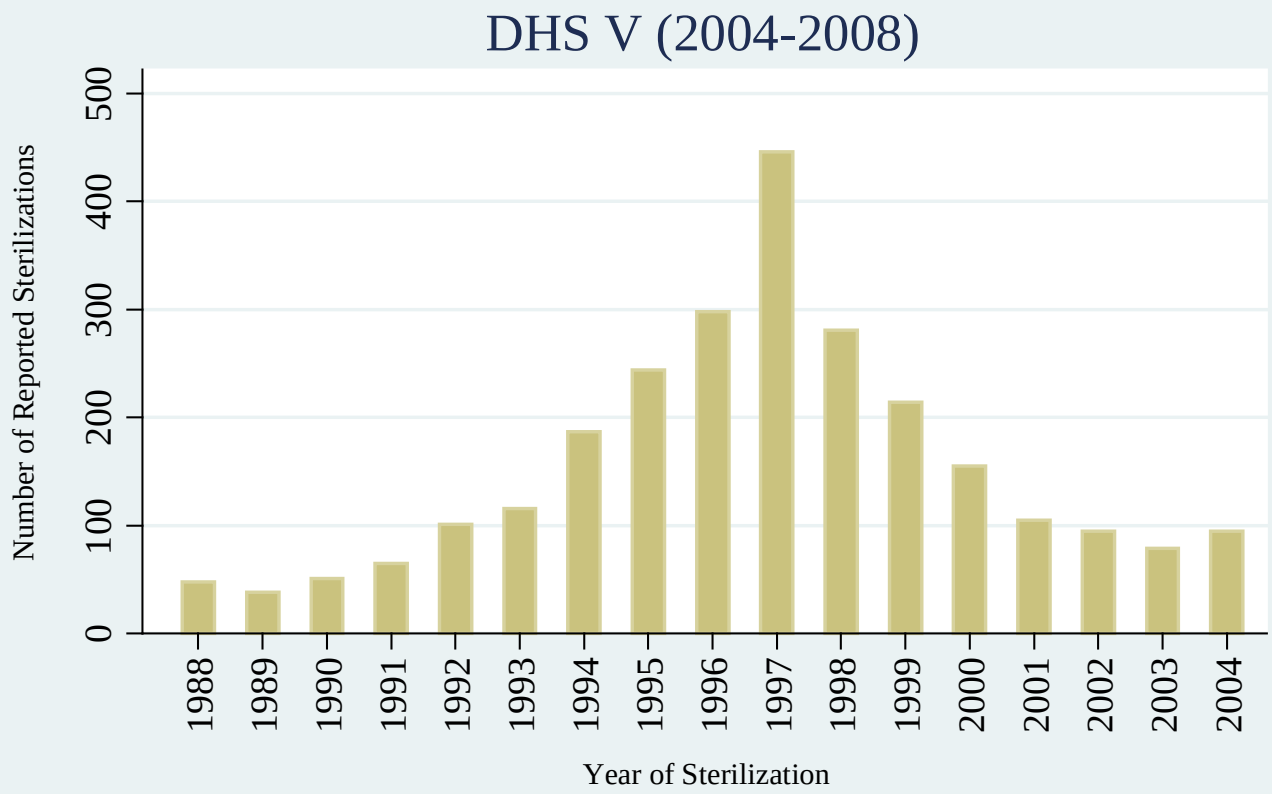


Table 1. Mean Number of Living Children for Mothers over 40 by Demographic Characteristics

	DHS II (1991-1992)		DHS IV (2000)		DHS V (2004-2008)	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
By Mother's Education						
no education	5.64	2.60	5.43	2.48	5.18	2.49
primary	4.42	2.34	4.80	2.39	4.52	2.26
secondary	3.24	1.79	3.22	1.80	3.05	1.72
higher	2.61	1.60	2.16	1.35	2.10	1.30
Total	4.39	2.55	3.91	2.37	3.48	2.19
By Place of Residence						
rural	5.82	2.72	5.27	2.52	4.75	2.44
urban	3.95	2.32	3.31	2.03	2.99	1.87
Total	4.39	2.55	3.91	2.37	3.49	2.19
By Qntls of Wealth Index						
lowest q	6.08	2.75	5.73	2.56	5.32	2.61
second q	5.43	2.54	5.05	2.33	4.82	2.34
middle q	4.80	2.38	4.06	2.25	3.90	2.05
fourth q	3.87	2.22	3.28	1.89	3.10	1.85
highest q	2.94	1.72	2.59	1.62	2.40	1.47
Total	4.39	2.55	3.84	2.35	3.48	2.19

Note: Using Sampling Weight Provided by DHS

Table 2. DHS IV : Characteristics of Eligible Women - Reweighted Estimates of Characteristics of Treated Women

	Among All Eligible Women					Among Women Sterilized 1996-1997 $S_2 = 1$	
	Not Reweighted			Reweighted to represent D=1		Reweighted	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	all	$S_2 = 1$	$S_2 = 0$	$S_2 = 1$	$S_2 = 0$	D = 1	D = 0
Pre-Policy Characteristics							
Age in 1996	31.48	32.36	31.44	31.24	31.05	31.24	32.62
# Kids in 1996	2.86	3.91	2.81	4.08	3.91	4.08	3.81
Years of education	7.85	7.12	7.89	5.65	5.88	5.65	7.95
Age at first birth	20.69	20.08	20.72	19.18	19.28	19.18	20.52
rural	0.36	0.35	0.36	0.49	0.48	0.49	0.25
coast	0.51	0.58	0.51	0.48	0.47	0.48	0.72
mountain	0.36	0.26	0.36	0.33	0.34	0.33	0.16
jungle	0.13	0.15	0.13	0.18	0.19	0.18	0.13
urban coast	0.45	0.48	0.45	0.37	0.38	0.37	0.62
rural coast	0.06	0.10	0.06	0.11	0.10	0.11	0.10
urban mountain	0.12	0.09	0.13	0.08	0.08	0.08	0.05
rural mountain	0.23	0.17	0.24	0.25	0.26	0.25	0.10
urban jungle	0.06	0.07	0.06	0.06	0.07	0.06	0.08
rural jungle	0.07	0.08	0.07	0.12	0.12	0.12	0.05
Outcomes and other variables							
Wanted last Prenancy[1]	0.45	0.30	0.47	0.28	0.35	0.28	0.30
Wanted last Prenancy Later	0.22	0.14	0.22	0.16	0.15	0.16	0.14
Did not Want last Prenancy	0.33	0.55	0.31	0.57	0.51	0.57	0.56
Labor Force Participation	0.63	0.61	0.63	0.63	0.59	0.63	0.58
Wealth Index	0.27	0.14	0.27	-0.19	-0.16	-0.19	0.36

Notes: All observations are weighted with DHS sampling weights. Reweighting refers to the propensity-score reweighting technique

Table 3. DHS V : Characteristics of Eligible Women - Reweighted Estimates of Characteristics of Treated Women

	Among All Eligible Women					Among Sterilized women	
	Not Reweighted			Reweighted to represent D=1		Reweighted	
	(1) all	(2) $S_2 = 1$	(3) $S_2 = 0$	(4) $S_2 = 1$	(5) $S_2 = 0$	(6) D = 1	(7) D = 0
Pre-Policy Characteristics							
Age in 1996	29.09	31.11	29.00	29.75	29.68	29.75	30.89
# Kids in 1996	2.51	3.71	2.45	3.70	3.73	3.70	3.86
Years of education	7.96	6.88	8.01	5.23	5.41	5.23	6.81
Age at first birth	20.53	20.31	20.53	19.97	19.82	19.97	20.11
rural	0.34	0.36	0.34	0.56	0.53	0.56	0.36
coast	0.50	0.54	0.50	0.38	0.37	0.38	0.55
mountain	0.37	0.32	0.37	0.42	0.43	0.42	0.30
jungle	0.13	0.15	0.13	0.20	0.20	0.20	0.15
urban coast	0.45	0.44	0.45	0.25	0.26	0.25	0.42
rural coast	0.05	0.10	0.05	0.13	0.11	0.13	0.13
urban mountain	0.14	0.11	0.15	0.10	0.11	0.10	0.12
rural mountain	0.23	0.20	0.23	0.32	0.32	0.32	0.18
urban jungle	0.07	0.08	0.06	0.09	0.09	0.09	0.10
rural jungle	0.06	0.06	0.06	0.11	0.11	0.11	0.05
Outcomes and other variables							
Domestic Violence in last 12 months	0.13	0.15	0.13	0.18	0.14	0.18	0.13
Labor Force Participation	0.73	0.72	0.73	0.77	0.75	0.77	0.69
Wealth Index	0.49	0.44	0.49	0.08	0.00	0.08	0.39

Table 4. Fertility Impact - Number of Children

	DHS IV (2000)			DHS V (2004-2008) - # of Kids in 2004		
	Standard Reweighting	Modified Reweighting		Standard Reweighting	Modified Reweighting	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0
S_2=1	-0.275***	-0.327***	-0.218***	-0.693***	-0.846***	-0.582***
	(0.013)	(0.017)	(0.013)	(0.021)	(0.025)	(0.023)
Age	-0.011***	-0.011***	-0.012***	-0.029***	-0.026***	-0.028***
	(0.001)	(0.002)	(0.001)	(0.003)	(0.004)	(0.003)
Years of Schooling	0.003	0.005	0.000	-0.031***	-0.026**	-0.030***
	(0.007)	(0.008)	(0.006)	(0.008)	(0.010)	(0.009)
# of children in 1997	1.007***	1.004***	1.009***	1.020***	1.010***	1.023***
	(0.005)	(0.006)	(0.006)	(0.010)	(0.011)	(0.012)
Age at first birth	0.003	0.001	0.004**	0.007	0.006	0.007
	(0.002)	(0.003)	(0.002)	(0.004)	(0.005)	(0.005)
Geographic controls	yes	yes	yes	yes	yes	yes
Constant	0.543***	0.601***	0.482***	1.534***	1.531***	1.490***
	(0.084)	(0.128)	(0.080)	(0.188)	(0.274)	(0.178)
R-squared	0.958	0.948	0.969	0.893	0.892	0.905
Observations	14430	14430	14430	16673	16673	16673
Observations with positive $\Delta P(x)$	NA	10607	10607	NA	14509	14509

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 5. Impact on Women's Labor Force Participation

	DHS IV (2000)			DHS V (2004-2008)		
	Standard Reweighting	Modified Reweighting		Standard Reweighting	Modified Reweighting	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0
S_2=1	0.022	0.049*	-0.009	-0.007	0.013	-0.023
	(0.025)	(0.027)	(0.030)	(0.026)	(0.026)	(0.033)
Age	0.011***	0.012***	0.010***	0.000	0.004	-0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Years of Schooling	0.016	0.002	0.031**	0.007	-0.001	0.007
	(0.010)	(0.012)	(0.012)	(0.011)	(0.012)	(0.013)
# of children in 1997	-0.022**	-0.034***	-0.008	0.005	-0.006	0.009
	(0.010)	(0.012)	(0.011)	(0.011)	(0.013)	(0.013)
Age at first birth	-0.012***	-0.011**	-0.014***	-0.003	-0.005	-0.004
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Geographic controls	yes	yes	yes	yes	yes	yes
Constant	0.658***	0.370**	0.760***	1.005***	0.913***	1.037***
	(0.172)	(0.187)	(0.169)	(0.115)	(0.127)	(0.149)
R-squared	0.120	0.146	0.113	0.075	0.112	0.072
Observations	14430	14430	14430	16673	16673	16673
Observations with positive $\Delta P(x)$	NA	10607	10607	NA	14509	14509

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 6. Impact on Domestic Violence (reported in the last 12 months)

	DHS V (2004-2008)		
	Standard Reweighting	Modified Reweighting	
	(1)	(2)	(3)
Treatment	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0
S_2=1	0.014	0.050*	-0.010
	(0.021)	(0.027)	(0.023)
Age	-0.003	-0.003	-0.003
	(0.003)	(0.004)	(0.003)
Years of Schooling	-0.002	-0.006	-0.001
	(0.008)	(0.011)	(0.009)
# of children in 1997	0.013	0.020	0.009
	(0.011)	(0.015)	(0.013)
Age at first birth	-0.004	-0.004	-0.004
	(0.003)	(0.005)	(0.004)
Geographic controls	yes	yes	yes
Constant	0.120*	0.094	0.167**
	(0.072)	(0.092)	(0.082)
R-squared	0.054	0.090	0.056
Observations	13381	13381	13381
Observations with positive $\Delta P(x)$	NA	11825	11825

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 7. Impact on Children's Biometrics - DHS IV (Kids=<4 born prior to policy)

	Height for Age (in sd from reference median)			Weight for Height (in sd from reference median)		
	Standard Reweighting	Modified Reweighting		Standard Reweighting	Modified Reweighting	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0
S_2=1	0.136	0.118	0.141	-0.067	-0.048	-0.080
	(0.099)	(0.110)	(0.108)	(0.080)	(0.091)	(0.092)
Age	0.043***	0.037**	0.045***	-0.010	-0.011	-0.009
	(0.014)	(0.017)	(0.015)	(0.011)	(0.015)	(0.011)
Years of Schooling	0.062	0.052	0.057	-0.025	-0.045	-0.017
	(0.040)	(0.049)	(0.042)	(0.031)	(0.041)	(0.030)
# of children in 1997	-0.154***	-0.145**	-0.164***	0.038	0.052	0.023
	(0.050)	(0.061)	(0.050)	(0.047)	(0.058)	(0.041)
Age at first birth	-0.027	-0.013	-0.030*	0.004	-0.012	0.009
	(0.019)	(0.024)	(0.018)	(0.014)	(0.018)	(0.013)
Geographic controls	yes	yes	yes	yes	yes	yes
Controls for child's age	yes	yes	yes	yes	yes	yes
Constant	-1.006*	-0.927	-0.547	1.165**	0.523	0.995**
	(0.568)	(0.599)	(0.522)	(0.484)	(0.335)	(0.427)
R-squared	0.318	0.315	0.316	0.143	0.146	0.164
Observations	2899	2899	2898	2899	2899	2898
Observations with positive $\Delta P(x)$	NA	2160	2160	NA	2160	2160

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 8. Impact on Girls Biometrics - DHS IV (Girls=<4 born prior to policy)

	Height for Age (in sd from reference median)			Weight for Height (in sd from reference median)		
	Standard Reweighting	Modified Reweighting		Standard Reweighting	Modified Reweighting	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0
S_2=1	0.276**	0.285**	0.228	-0.026	-0.047	-0.029
	(0.129)	(0.131)	(0.144)	(0.106)	(0.119)	(0.124)
Age	0.040**	0.045**	0.036	0.006	0.021	-0.001
	(0.020)	(0.020)	(0.024)	(0.015)	(0.021)	(0.015)
Years of Schooling	0.052	0.002	0.074	-0.038	-0.035	-0.035
	(0.052)	(0.058)	(0.062)	(0.043)	(0.051)	(0.046)
# of children in 1997	-0.174***	-0.162***	-0.195***	-0.001	-0.034	0.003
	(0.056)	(0.058)	(0.070)	(0.061)	(0.076)	(0.054)
Age at first birth	-0.032	-0.026	-0.034	0.007	-0.021	0.014
	(0.022)	(0.026)	(0.027)	(0.019)	(0.028)	(0.017)
Geographic controls	yes	yes	yes	yes	yes	yes
Controls for child's age	yes	yes	yes	yes	yes	yes
Constant	-0.613	-1.167**	-0.048	-0.058	0.077	0.241
	(0.421)	(0.554)	(0.550)	(0.382)	(0.421)	(0.477)
R-squared	0.352	0.376	0.345	0.212	0.267	0.219
Observations	1457	1457	1457	1457	1457	1457
Observations with positive $\Delta P(x)$	NA	1084	1084	NA	1084	1084

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 9. Impact on Years of Schooling for Own Children ages 5-14 (born prior to policy)

	DHS IV (2000)			DHS V (2004-2008)		
	Standard Reweighting	Modified Reweighting		Standard Reweighting	Modified Reweighting	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0
S_2=1	0.084**	0.093**	0.077	0.032	0.108	-0.034
	(0.039)	(0.041)	(0.050)	(0.085)	(0.094)	(0.103)
Age	0.042***	0.040***	0.046***	0.026**	0.043**	0.016
	(0.006)	(0.008)	(0.006)	(0.013)	(0.018)	(0.015)
Years of Schooling	0.046***	0.060***	0.024	0.122***	0.138***	0.110***
	(0.016)	(0.018)	(0.021)	(0.036)	(0.048)	(0.040)
# of children in 1997	-0.128***	-0.116***	-0.143***	-0.179***	-0.204***	-0.161***
	(0.018)	(0.022)	(0.019)	(0.038)	(0.052)	(0.042)
Age at first birth	-0.019***	-0.014	-0.023***	-0.009	-0.037*	0.013
	(0.007)	(0.010)	(0.007)	(0.016)	(0.021)	(0.017)
Geographic controls	yes	yes	yes	yes	yes	yes
Controls for child's age	yes	yes	yes	yes	yes	yes
Constant	-0.169	-0.383	-0.291	0.230	1.930***	0.294
	(0.383)	(0.265)	(0.216)	(0.820)	(0.508)	(0.569)
R-squared	0.807	0.792	0.826	0.366	0.329	0.431
Observations	22520	22513	22513	14021	14016	14016
Observations with positive $\Delta P(x)$	NA	18886	18886	NA	12329	12329

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 10. Impact on School Enrollment for Children ages 5-14 (born prior to policy)

	DHS IV (2000)			DHS V (2004-2008)		
	Standard Reweighting	Modified Reweighting		Standard Reweighting	Modified Reweighting	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0
S_2=1	0.005	0.016**	-0.009	0.001	0.009	-0.005
	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.011)
Age	-0.000	-0.000	-0.001	0.002**	0.002	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years of Schooling	0.009***	0.011***	0.007**	0.008**	0.007	0.007*
	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
# of children in 1997	-0.004	-0.005	-0.003	-0.009***	-0.006	-0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Age at first birth	0.002	0.001	0.002	-0.000	0.000	-0.000
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Geographic controls	yes	yes	yes	yes	yes	yes
Controls for child's age	yes	yes	yes	yes	yes	yes
Constant	-0.032	-0.099	-0.037	0.964***	0.958***	0.962***
	(0.071)	(0.103)	(0.058)	(0.051)	(0.036)	(0.052)
R-squared	0.649	0.632	0.681	0.126	0.128	0.150
Observations	22554	22554	22547	14021	14021	14016
Observations with positive $\Delta P(x)$	NA	18917	18917	NA	12329	12329

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 11. Impact on School Enrollment for Daughters ages 5-14 (born prior to policy)

	DHS IV (2000)			DHS V (2004-2008)		
	Standard Reweighting	Modified Reweighting		Standard Reweighting	Modified Reweighting	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0
S_2=1	0.009	0.023*	-0.011	0.005	0.016	-0.007
	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)	(0.015)
Age	0.000	0.001	-0.001	0.004***	0.003	0.004***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
Years of Schooling	0.015***	0.017***	0.012**	0.005	0.010*	0.001
	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)
# of children in 1997	-0.005	-0.006	-0.004	-0.010**	-0.008	-0.009
	(0.005)	(0.006)	(0.004)	(0.004)	(0.005)	(0.006)
Age at first birth	0.002	0.001	0.002	-0.001	0.000	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Geographic controls	yes	yes	yes	yes	yes	yes
Controls for child's age	yes	yes	yes	yes	yes	yes
Constant	-0.023	-0.015	-0.041	0.980***	0.900***	0.992***
	(0.048)	(0.050)	(0.049)	(0.050)	(0.056)	(0.067)
R-squared	0.649	0.632	0.681	0.126	0.128	0.150
Observations	11008	11008	11005	6905	6905	6903
Observations with positive $\Delta P(x)$	NA	9176	9176	NA	6153	6153

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 12. Impact on Education of Daughters 15-22

	DHS V (2004-2008)		
	Standard Reweighting	Modified Reweighting	
	(1)	(2)	(3)
Treatment	Contaminated Treatment Group D=1 & D=0	Sterilized by FSP D=1	Sterilized outside FSP D=0
S_2=1	-0.140	-0.061	-0.155
	(0.129)	(0.186)	(0.131)
Age	0.001	-0.043	0.007
	(0.021)	(0.031)	(0.022)
Years of Schooling	0.119**	0.284***	0.047
	(0.054)	(0.078)	(0.055)
# of children in 1997	-0.268***	-0.160**	-0.290***
	(0.058)	(0.067)	(0.068)
Age at first birth	0.019	0.065**	0.011
	(0.023)	(0.033)	(0.026)
Geographic controls	yes	yes	yes
Constant	10.320***	13.246***	12.350***
	(1.452)	(1.634)	(0.947)
R-squared	0.476	0.425	0.526
Observations	4304	4304	4304
Observations with positive $\Delta P(x)$	NA	3490	3490

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1