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Family Planning and Child Health Care:

Effect of the Peruvian *Programa de Salud Reproductiva y Planificación Familiar*, 1996-2000[1]

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Abstract

Our study estimates the effects of exposure to a family planning program which promoted surgical contraception for the first time in Peru on women's use of birth-control methods and their children's health. While a broad program, the PNSRPF forced many indigenous women to undergo sterilization. We compare provinces affected by the program earlier with provinces affected later, before and after the policy. Overall, the results indicate that women in treated areas were more likely to use both temporary and permanent contraceptive methods and their children were less likely to die within their first year of life, partly due to longer breastfeeding. However, we observe heterogeneity by ethnicity. In treated provinces, nonindigenous children benefited from the policy regardless of their mothers' choice of contraceptive method, while there were few positive impacts for indigenous children whose mothers underwent sterilization. This suggests that coercive or aggressively implemented family planning programs may not confer health benefits on children.

Keywords: family planning; child health; ethnic minority; Latin America; Peru

JEL classification codes: J13, J15

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Introduction

Family planning plays a role in every country's social policy. Couples and individuals are enabled to freely and responsibly make decisions regarding their reproductive life, thanks partly to the provision of information and means by governments. Prior to the late 1990s, most governments had regarded family planning from a demographic perspective: it was intended to reduce high fertility rates and thus slow population growth. After the International Conference of Population and Development (ICPD) held in Cairo in 1994, family planning became more associated with human rights since reactions emerged against the demographic approach (Seltzer 2002). Currently, many family planning programs are still implemented, especially in developing countries, where there is an unmet need on the part of women who would like to delay, space, or limit their fertility but are not using any method of contraception. These programs often promote, along with the use of birth control methods, neonatal and under-5 care such as breastfeeding, immunization and attendance at delivery by trained personnel.

This paper aims to study the effects of being exposed to an unusual family planning program promoting voluntary surgical contraception (VSC) on women's use of birth-control methods and their children's mortality and health care. In addition, we investigate the heterogeneous effects by ethnicity and put forward some evidence on child health care behaviors correlated with permanent versus temporary interventions. We contribute to the existing literature by providing new evidence on the heterogeneous effects of family planning programs on child survival and child health. To our knowledge, we are also the first to document differential child health care behaviors correlated with permanent or temporary interventions reducing fertility, especially for those women for whom permanent birth control is aggressively promoted.

The program, called *Programa de Salud Reproductiva y Planificación Familiar* (PNSRPF), was carried out in Peru during 1996-2000. At the time, infant and child mortality rates were exceptionally high: in 1994 infant mortality was 45.8 per thousand live births and neonatal mortality was 23.5 per thousand live births, both higher than the South American average (World Bank 2018). Child mortality, together with the high prevalence of chronic malnutrition among children under five years (25.8 percent in 1996), were further challenges (UNICEF 2008). The PNSRPF was promoted by the Peruvian Ministry of Health with the stated purpose of addressing widespread poverty in the country through the reduction of fertility rates. It was a family planning program that provided information and means for the adoption of birth control methods together with the supply of maternal and child health care services. Its distinctive feature is that, for the first time, voluntary surgical contraception became legal and freely supplied by the Peruvian public health sector. Nonetheless, several sources reported irregularities during the program implementation: poor, indigenous women from rural areas were often sterilized without giving consent (Boesten 2007; Ballón 2014; Byker and Gutierrez 2012; Tamayo 1999), and the surgeries were mainly performed during health festivals by mobile sterilization teams (Ballón 2014; Tamayo 1999; Morrison 1998). The government administration refused to recognize such irregularities and did not officially report the existence of sterilization quotas. Despite the documented episodes of forced sterilization among indigenous women, it never publicly stated guidelines regarding which populations were targeted: there are no official data on who was actually exposed to the policy, nor at which point in time. We can only rely on aggregate official data on the number of sterilizations per region recorded by the Ministry of Health (Ministerio de Salud 2002).

In the absence of detailed official data on the implementation of the PNSRPF, for our identification strategy we take advantage of such irregularities. To identify who was affected by the PNSRPF, we combine the information provided by the Ministry of Health (Ministerio de Salud 2002) and other available official data collected by the Committee of Latin America and the Caribbean for the Defense

of Women's Rights - CLADEM (Tamayo 1999) with data provided by the Demographic and Health Surveys (DHS). Exposure to the program was not random. Nonetheless, to identify who was affected and at which point in time we can exploit the key feature of the PNSRPF, that is, the provision for the first time in the country of surgical contraception, and the widespread evidence that it was targeting a specific population. We therefore use the incidence and timing of surgical contraception among indigenous women in each province to identify the areas treated.[2] DHS data provide self-reported information on the year and month in which a woman was sterilized. We observe that indigenous surgical contraception mainly took place in one month per year per province, suggesting that mobile medical units could have reached those areas in that specific month, especially during health festivals. This information, corroborated by official data at the aggregate level (Ministerio de Salud 2002; Tamayo 1999), allows us to distinguish provinces affected by the program in its first year versus provinces affected in the following years. We can thus compare provinces where it is more likely that mobile health units arrived earlier (treated) with provinces in which they arrived later (control). Such identification forces us to look at the impact of the PNSRPF in the short term: the year when treated provinces were reached by the program. For robustness checks, we also look at whether the intensity of the program, measured by the percentage of sterilizations among indigenous women aged 15 to 49 in the province, affects differently our outcomes of interest.

It needs to be clear that the PNSRPF was not merely a sterilization program. The PNSRPF was a family planning policy that provided, along with other birth control methods, the possibility to use sterilization as a new contraceptive method and supplied child health services. We study its overall impact on the use of contraceptive methods and child mortality and child health care. Further, in the last part of the paper, we attempt to document with an exploratory analysis the consequences on child health care in cases when the PNSRPF was most likely aggressively implemented. First, we use a difference-in-difference analysis comparing mothers of children up to one year old and the same children in treated

and control provinces before and after the policy. All the children were born or conceived before the policy was introduced in the province where the mother lived. Concerns regarding compositional changes in the family and anticipation effects are mitigated in this context. The population of mothers and the probability of abortion do not change before and after the policy in treated and control areas. Additionally, we do not observe differences in the number of children born per mother between treated and control areas, which counters the idea that control provinces might have heard about the enforcement practices in the treatment provinces and therefore reduced the use of contraceptives in order to conceive another child before they were forced to become sterilized. The treated and control samples are well balanced on covariates, and the parallel trend assumption for outcomes holds. We confirm the validity of our identification by performing placebo tests for pre-policy years. For robustness checks, in another specification we also include the provinces not treated by the policy or where we observe indigenous sterilizations only after the slowdown in the sterilization campaign. These provinces consistently differ in terms of observable characteristics from the provinces included in our sample and do not make up a comparable control group. Nonetheless, our results are not sensitive to their inclusion, suggesting that focusing on a subsample does not bias our estimates. Second, we select only treated provinces and women who are more likely to be aggressively affected by the policy and provide suggestive evidence on differential child health care behaviors correlated with using one contraceptive method or another.

The main results suggest that women in treated areas increased the use of contraceptive methods, both temporary and permanent, and that their children were less likely to die within their first year of life, partially thanks to an increase in the length of breastfeeding. More specifically, women in treated areas were 4.9 percentage points more likely to use temporary modern method of contraception and 5.1 percentage points more likely to use permanent methods compared to women in control areas. Sterilization, widely promoted by the PNSRPF, increased significantly more for indigenous women

compared to nonindigenous women, confirming that the former were the most targeted by the policy. Furthermore, children in treated provinces were less likely to die within their first year of life compared to children in control provinces. Neonatal mortality and infant mortality decreased by 5.2 percentage points and by 6.1 percentage points, respectively. These results can be partially explained by an increase in the time mothers breastfed their children. Mothers were 18.5 percentage points more likely to breastfeed their children more than the age-average length. The likelihood that children received the appropriate vaccination within their first year of life was not significantly affected by the policy. The results are robust at the intensive margin by using the percentage of indigenous sterilizations in each province and including provinces not affected by the program in the interval of time we study. The sex of the child is not relevant for any of the outcomes of interest. As an additional result, by comparing women in treated provinces who used a temporary contraceptive method and women who were sterilized to women who did not use any contraceptive method, we observe that nonindigenous children benefited from the policy regardless of the contraceptive method their mothers used. Conversely, almost all the positive impacts of the PNSRPF on child health care for indigenous children whose mothers were sterilized dissipated over time.

Literature

Our paper is primarily related to the literature on the effects of family planning programs on a range of health and fertility outcomes. Family planning has the potential to improve child health and survival rates by reducing the number of births associated with higher risks (Seltzer 2002). It promotes the reduction in the number of births that occur within approximately two years of a previous birth, in the number of high-order births (five or higher), and in the number of children born to very young mothers and to women in poor health. The effects of family planning programs on child mortality have been studied, among others, by Joshi and Schultz (2013) in Bangladesh, Beegle et al. (2011) in Ethiopia and Miller (2010) in Colombia. While the first two studies find that villages where a family planning

program was introduced had lower child mortality compared to control areas, Miller (2010) does not find clear evidence that the Colombian *Profamilia* program influenced infant and child mortality, although it improved women's socioeconomic status. In all these cases, as in most of the literature on family planning programs, child mortality is studied: birth control methods extend birth spacing and lead to a reduction in the proportions of high-risk births and thus of infant mortality. Nonetheless, we look at births occurring shortly after the family planning program was implemented. We can exclude that reduction in mortality is here due to changes in preceding birth intervals induced by the policy. It can instead be explained by better delivery conditions, increased breastfeeding and vaccination. The PNSRPF, like other, more recent, family planning programs (Aquino et al. 2009; Cleland et al. 2006), does not only provide information on birth control methods. It includes promotion of child health care and attendance at delivery by trained personnel. Byker and Gutierrez (2012) are the first to investigate the PNSRPF. They use propensity score with reweighting techniques to infer who was sterilized by the program and then look at the impact of being sterilized on household well-being outcomes in the long run. Their results show that when birth control is imposed, benefits from making choices about fertility may not accrue and that, in general, the decline in fertility does not involve substantial improvements in family well-being. They only find a small positive impact of mothers' sterilization on their daughters' height-for-age and school enrollment. Conversely to Byker and Gutierrez (2012), we look at the general impact of the policy on both sterilized- and non-sterilized women. Moreover, by using a difference-in-difference estimation strategy, we investigate the impact of the PNSRPF on child mortality, place of delivery, length of breastfeeding and vaccinations, which are the first outcomes affected by the program. We therefore complement their study by observing short-term child health care outcomes and confirm the overall non-positive impact of the program when birth control is imposed. We also contribute to the existing literature by providing a broader analysis of the impacts of the PNSRPF program and new evidence on the heterogeneous effects of family planning programs on child survival and on child health. As mentioned above, to our knowledge, we are the first to

document differential child health care behaviors correlated with permanent as opposed to temporary interventions reducing fertility. Typically, women are sterilized once they have achieved their desired family size. However, there are a few known cases when sterilization has been aggressively promoted, in which case the permanent intervention can be seen as an exogenous decrease in family size.[3] We provide here some suggestive evidence of the effects on child health care when that happens.

Our paper is also related to another strand of the literature focusing on Becker's quantity-quality model (Byker and Lewis 1973; Becker and Tomes 1976). In line with the theoretical predictions, child well-being is expected to improve when family size decreases. The empirical literature has largely estimated a child quantity-quality trade-off, where quality is usually measured in terms of investment in education, by using multiple births. It has overall been observed that children from larger families have lower academic performance than children from smaller families (among others, Hanushek 1992; Hill and O'Neill 1997; Conley and Glauber 2006). However, recent studies show that multiple births may have no impact on education. Black et al. (2005) use data from Norway and find no effects, possibly because, as suggested by Cáceres-Delpiano (2006), in a developed country families have more ways to augment or reallocate resources to protect child quality. Small effects have also been found by Angrist et al. (2010), who look at education and at a broader range of outcomes. They exploit multiple third births and the effects of sibling-sex composition in families with three or more children and find that an exogenous increase in family size at the second and higher births has little effect on first- and second- born children, though some estimates suggest that first-born girls from large families marry sooner. Our results are in line with these most recent predictions, suggesting that they do not provide strong evidence to support Becker's quantity-quality model. However, our contribution on this front can be only limited since in the PNSRPF the cost of child quality is not fixed: the government is also improving the supply of maternal and child-health services and reducing the cost of accessing these services at the same time as the decline in fertility.

The PNSRPF (1996-2000)

After the agreements reached at the International Conference of Population and Development (ICPD, Cairo 1994) and the World Conference on Women (Beijing, 1995), in 1995 voluntary surgical contraception was introduced into the Peruvian Public Health Sector. The Congress modified the National Law of Population to include VSC as a contraceptive method and emphasized that its adoption was based on the free exercise of personal will. The 1996-2000 PNSRPF was presented as part of the government's social development policy to fight poverty and gained the support from the United Nations Population Fund (UNFPA), USAID and several NGOs. Part of the funds went to *Movimiento Manuela Ramos*, a Lima-based NGO, to implement a participatory program to inform women on the use of birth control methods and empower them to become actively involved in the improvement of their reproductive health. The remaining funds were used by the government to provide information campaigns and family planning services, including sterilization (Boesten 2007). The general goals of the policy were to reduce the Total Fertility Rate (TFR) from 3.5 in 1995 to 2.5 by 2000[4] and to reduce maternal and perinatal mortality. A 100 percent usage of contraceptive methods among women with institutional help during delivery was expected. Provision of information on the use of birth control methods and the provision of family planning services were both planned without a fee.[5]

The PNSRPF was locally implemented by regional or subregional health authorities with the help of private firms with public contracts. It was introduced gradually, mainly through the celebration of health festivals (Aramburú 2002). For a few days, mobile medical teams visited the villages, provided information and performed surgeries.[6] Since no official data are available, there is limited knowledge of how the program was implemented. What has been documented, mostly through interviewed

witnesses and other informal documents (health festivals advertisements and internal documents from the Ministry of Health), is that fairs lasted about two days. Mobile medical teams then moved to another village in the province. During those days, family planning workers provided information on health care and contraceptive, giving special emphasis to the use of sterilization, and supplied health care services. Depending on the village health infrastructure, interventions occurred in the village health clinic/center or in the nearest hospital that had the relevant equipment, or inside ambulances that served as mobile sterilization units. Almost every woman in each village had been exposed to the program, since family planning workers were visiting them at home or were approaching their husbands. All health services were free of cost. Nonetheless, statements of the doctors themselves admit irregularities in implementing the PNSRPF (Ballón 2014). Several nongovernmental sources (i.e., the Ombudsman, NGO Flora Tristán and CLADEM) have reported that VSCs were realized rather forcefully. According to human rights agencies and international investigations, the government established a quota system through which poor, indigenous women (especially Quechua speakers) from rural areas were sterilized under coercion (Tamayo 1999). These women were often pressured to have surgery through household harassment and/or were offered money or food in exchange. Additionally, many of these surgeries were performed without women giving an explicit consent and without doctors giving medical information about the results of the surgery or post-surgery monitoring. None of the irregularities nor the existence of sterilization quotas during the campaigns have been recognized by the government or the Ministry of Health. Nonetheless, among the 277,793 women who were sterilized during 1996-2000 by the Ministry of Health (Velikoff 2011), it is estimated that only 10 percent gave explicit consent (Tamayo 1999).[7]

The peak years of the family planning campaign were 1996 and 1997. Until the end of 1997, the PNSRPF was implemented with the same modus operandi. Starting in 1998, its implementation changed. The Committee of Latin America and the Caribbean for the Defense of Women's Rights

(CLADEM) accused President Fujimori's family program of forced sterilization through a public statement, the Ombudsman gathered several reports on sterilizations, and the first international investigation started. The Peruvian Ministry of Health replied to the accusations and decided to make changes to the way the goals of the program were stated (Aramburú, 2002).[8]

Data and Descriptive Statistics

We use the Peruvian Demographic and Health Surveys (DHS) data for the years 1996 to 2012, available data of the Ministry of Health on the number of sterilizations per region registered between 1990 and 1999 (Ministerio de Salud 2002) and information collected by CLADEM (Tamayo 1999). There is also DHS information available for the years 1986 and 1992, but unfortunately, we cannot use these waves. In 1986, there was no direct information on ethnicity[9] and in 1992, according to DHS data specialists, "the survey was done at the height of *Sendero Luminoso* (Shining Path) activity, in which the real IDs were left off to protect the respondents,"[10] so we do not know the provinces where women were living. Each of the eight waves of the DHS survey is a cross-section with detailed information about women of reproductive age (15-49 years). We also have information on their children's health and on sex and age for children who died. The survey asks questions about women's use of contraceptive methods. Thus, we know who was sterilized and when. We use this information to identify the areas that were reached by the program and its timing. However, we cannot determine any level of coercion or force during the PNSRPF. What we observe is that the percentage of sterilizations by year among women aged 15-49 strongly increased during 1996-1997, the peak years of the family planning program (Figure 1). Additionally, if we look at the percentage of sterilizations among nonindigenous and indigenous women separately, we find that indigenous women were less likely to choose to be sterilized both before and after the family planning program (Figure 2). While nonindigenous women were being sterilized even before the PNSRPF (left panel of Figure 2), indigenous women mainly started with the program and stopped thereafter (right panel of Figure 2).

This exploratory analysis provides support to the documented information that indigenous women were targeted by the program.

[insert FIGURES 1 and 2 here]

In the DHS data, we also observe that indigenous sterilizations mainly took place in one month per year per province, supporting the idea that, through the celebration of health festivals, mobile health units reached those areas in that specific month.[11] Data from other sources - official data from the Ministry of Health (Ministerio de Salud 2002) and CLADEM (Tamayo 1999) - corroborate this hypothesis. We use information on when and where indigenous women were sterilized in order to define the provinces affected by the family planning policy. The provinces in which we do not observe indigenous sterilizations before the implementation of the program constitute our sample. We are interested in the effect of the PNSRPF, and we want to be sure that the sterilizations we observe occur for the first time due to the family planning program. Among the provinces in our sample, we define as treated the provinces in which we observe indigenous sterilizations starting in 1996 and as control the provinces in which we observe indigenous sterilizations starting in 1997, exploiting thus the gradual implementation of the program. There are 46 treated provinces and 17 control provinces. The provinces not included in the sample are provinces with no indigenous sterilizations at all or provinces in which we observe indigenous sterilizations before 1996 or only after 1998, the year in which the implementation of the program changed. For robustness checks, we also include in the analysis these provinces. Nonetheless, they consistently differ in terms of observable characteristics from those in our main sample (Table A2 in the Appendix containing online supplemental materials), confirming that it would be preferable not to include them in our main analysis. Figure 3 reports where treated and control provinces are located.

[insert FIGURE 3 here]

Mothers' and Children's Predetermined Characteristics

Table 1 reports the observable predetermined characteristics of the selected sample (column (1)), those of the treated and control groups (columns (2) and (3), respectively), the p-values of their differences (column (4)) and the normalized differences (column (5)).

[insert TABLE 1 here]

In the analysis we control for women's characteristics, such as age, education, marital and labor force status, ethnicity, household wealth and differences in the place and in the geographical region of residence.[12] We also control for children's characteristics, such as their ranking among siblings, their sex, and the quarter and year of birth. All children were born and conceived before the policy was introduced in the province where the mother resided. More precisely, children in our sample were born either in 1995 or in 1996. If the mother lived in a treated area, children born in 1996 were exposed to the PNSRPF, while children born in 1995 were not. For mothers in control areas, both children born in 1995 and 1996 were not exposed to the policy since it reached these provinces only in 1997. In this time span, each mother had on overage 1.04 children, with a maximum of 2. For children born in 1996 in treated areas, we need to also consider the date when the policy arrived and the date when the mother was interviewed in DHS 1996. We define the month when the first indigenous sterilizations occurred in the province as the date of arrival of the policy to that province. Recall that indigenous sterilizations mainly took place in one month per year per province.[13] Then, using the information we have on the woman's DHS date of interview, we identify whether she was exposed or not to the policy at the time of the survey based on whether she was interviewed before or after the policy arrived to her province of residence. For example, assume that a child was born in March 1996 and the policy arrived in her

province in July 1996. If her mother was interviewed in June 1996, she is considered not treated. If in this same province, her mother was interviewed in November 1996, she is considered treated.

The treated and control samples are well balanced on covariates: only few tests yield a p-value below .05, and the p-value of the F-test of joint significance of all balance variables is 0.1929. More important, as reported in column (5), all but one of the normalized differences are smaller than 1/4th of the combined sample variation, suggesting that linear regression methods are unlikely to be sensitive to specification changes (Imbens and Wooldridge 2009).[14] The only individual characteristic that differs between treated and control provinces is the proportion of indigenous women, which is significantly higher in treated areas. We know that the PNSRPF was targeting indigenous women, so we expect provinces with a higher proportion of indigenous women to be reached before others. Moreover, as reported in Table A3 in the online Appendix, these women were significantly poorer and less educated than nonindigenous women and lived in rural areas. Therefore, once we interact the treatment with being indigenous, we take into account observable differences between the two groups that can potentially bias the results. Note also that for indigenous women the treated and control samples are well balanced on covariates, as reported in Table A4 in the Appendix: all the normalized differences are smaller than 1/4th of the combined sample variation. If we restrict the sample to indigenous women, the results are consistent with those of our main analysis.

Mothers' Contraception and Child Health Care

Our outcomes of interest are contraceptive methods used by the mother and a series of child health care measures for the last children born to the mother up to 1 year of age, namely, neonatal and infant mortality, the probability of being delivered at home, the length of breastfeeding and the probability of being fully immunized. Table 2 reports the predetermined outcomes of the selected sample (column

(1)), those of the treated and control groups (columns (2) and (3)), the p-value of their differences (column (4)) and the normalized differences (column (5)).

[insert TABLE 2 here]

Women could use either traditional or modern temporary methods, be sterilized or not use any contraceptive method.[15] The DHS survey asks for the current and the last method used and does not provide chronological information on the use of contraceptives prior to the last and current method. Since we need information for the pre-policy contraceptive methods, we can only use information provided by DHS 1996. Before the policy, almost 30 percent of women in our sample were using temporary methods and 0.5 percent were sterilized. The outcomes are well balanced between treated and control groups (Panel A).

Neonatal and infant mortality refer to the probability of dying before 1 month of age and before 1 year of age, respectively. Before the policy, these rates were 55 and 75 deaths per 1,000 live births (Panel B).[16] The probability of being delivered at home is a dummy equal to 1 if the child is delivered at home and 0 at the hospital or a health center. On average, 67.5 percent of women delivered at home before the policy. The length of breastfeeding refers to the number of months a woman has breast-fed her child. We create a dummy variable equal to 1 if the mother has breast-fed her child for a number of months higher than the pre-policy average months and 0 otherwise. We control for the age of the child in months. The probability that a child was fully immunized is measured by the probability of receiving the required vaccinations at birth and by the first year of life. They are dummies taking value 1 if the child receives all relevant vaccinations for her age.[17] Before the policy, 39 percent of children received the required vaccinations at birth and 29 percent by age 1. Information on place of delivery, breastfeeding and vaccinations were only collected for children under 5 years old at the time of the

survey. Since we look at children born in either 1995 or 1996, such information is only provided in DHS 1996 and 2000 and not in the following waves. This explains why we have fewer observations than for mortality outcomes. The results are consistent if we restrict the sample to the DHS waves of 1996 and 2000 for the mortality outcomes too.

The treated and control samples are comparable in the outcomes of interest before the policy was introduced. [18] All the tests yield a p-value below .05, and all the normalized differences are smaller than 1/4th of the combined sample variation.

Empirical Strategy

To identify the effects of the PNSRPF on the outcomes of interest, we implement a difference-in-difference analysis. Recall that the family planning program includes both information on the use of birth control methods and the provision of family planning services. Being exposed to it means being resident in a province where health festivals were held. The timing of the health festivals identifies treated and control provinces. Women in treated provinces could or could not be sterilized, but they all received information on family planning. We look at the short-run impacts of the program: in the year when the treated provinces were reached by it. First, we compare the outcomes before and after the policy was introduced in treated and control provinces for mothers whose children were born from January 1995 to December 1996. The specification is defined as follows:

$$Y_{jpt} = \alpha + \beta_1 Treat_{jpt} + \beta_2 Post_{jpt} + \beta_3 Treat_{jpt} * Post_{jpt} + X'_{jpt}\gamma + \eta_r + u_{jpt} \qquad (1)$$

where Y_{jpt} is whether the mother *j*, in province *p*, at time *t* uses a contraceptive method and whether it is temporary or permanent. $Treat_{jpt}$ is a dummy equal to 1 if the child's mother lives in a treated province; $Post_{jpt}$ is equal to 1 in 1996 and 0 in 1995; and β_3 is our coefficient of interest. The vector of

control variables (X'_{jpt}) include maternal and household characteristics, namely, mother's age and age squared, her years of education, whether she is married and she is working, whether she is indigenous, household wealth, the household's location - urban (city or town) or rural area - and geographical region. We also include regional fixed effects (η_r). Robust standard errors are clustered at the province level.

Second, we compare health care outcomes before and after the policy was introduced in treated and control provinces for children born between January 1995 and December 1996 and whose mothers are considered in specification (1). We proceed as follows:

$$Y_{ipt} = \alpha + \beta_1 Treat_{ipt} + \beta_2 Post_{ipt} + \beta_3 Treat_{ipt} * Post_{ipt} + X'_{ipt}\gamma + \eta_r + u_{ipt} \qquad (2)$$

where Y_{ipt} is equal to our outcomes of interest for child *i*, in province *p*, at time *t*. $Treat_{ipt}$ is a dummy equal to 1 if the child's mother has been exposed to the policy (treated province); $Post_{ipt}$ is equal to 1 if the child was born in 1996 and 0 in 1995; β_3 is our coefficient of interest. The outcomes we are interested in are: neonatal and infant mortality, place of delivery, length of breastfeeding and the probability to be fully immunized. For infant mortality and vaccination by the first year of age, we restrict the analysis to children aged at least 12 months at the date of the survey. For neonatal mortality, we restrict the analysis to children older than one month at the date of the survey. So, for example, assume that the policy arrives in a province in July 1996. For neonatal mortality, we consider as treated those children aged more than one month at the date of the survey born from August to December 1996. The vector of control variables (X'_{ipt}) includes maternal and household characteristics as before and child characteristics, such as a child's ranking among siblings, gender and the quarter of birth. We also include regional fixed effects (η_r) and clustered robust standard errors at the province level.

To investigate if there are differential impacts between indigenous and nonindigenous women, we modify the previous specifications as follows:

$$Y_{i(j)pt} = \alpha + \beta_1 Treat_{i(j)pt} + \beta_2 Post_{i(j)pt} + \beta_3 Treat_{i(j)pt}*Post_{i(j)pt} + \beta_4 Indigenous_{i(j)pt} + \beta_5 Treat_{i(j)pt}*Indigenous_{i(j)pt} + \beta_6 Post_{i(j)pt}*Indigenous_{i(j)pt} + \beta_7 Treat_{i(j)pt}*Post_{i(j)pt}*Indigenous_{i(j)pt} + X'_{i(j)pt}\gamma + \eta_r + v_{i(j)pt} \quad (3)$$

where the outcomes of interest are as before (Y_{jpt} for the mother *j* contraceptive method, and Y_{ipt} for child *i* outcomes) and β_7 is now our coefficient of interest. The vector of control variables ($X'_{i(j)pt}$) is the same as before, excluding whether the mother is indigenous.

For our analysis to be valid, we need our outcomes of interest to satisfy the parallel trend assumption: in the absence of treatment, the difference between the treated and control provinces is constant over time. The vertical lines in Figure 4a show the years when the policy started (1996) and when the first international investigation of its irregularities began (1998). Recall that the first DHS wave we can use is from 1996 and that information on place of delivery, vaccination and breastfeeding were only collected for children under 5 years old at the time of the survey. Thus, we do not have data before 1991. For consistency, we report the parallel trends for all the outcomes starting from 1991. Moreover, we cannot report parallel trends for contraceptive methods since DHS do not provide chronological information on the use of contraceptives prior to the last and current method. However, we know when the last method was adopted. If we assume that the previous contraceptive method was used continuously before the current one was adopted, we can approximately show for each year the proportion of women using each birth control method by treatment status. Figure 4b shows proxy pre-trends for no contraception at all, temporary and permanent contraception.

[insert FIGURES 4a and 4b here]

All the outcomes show parallel trends before the program with some convergence between treated and control provinces starting after the policy was implemented. We also perform placebo tests for pre-policy years for all the outcomes of interest (Tables A6 and A7 in the Appendix). They confirm the validity of our identification. Furthermore, we compare the differences-in-differences estimates between treated and control groups for every birth cohort, i.e., 1996, 1995, 1994, and 1993, with the birth cohort in 1991 (Table A8 in the Appendix). The results are again consistent with our main ones and further confirm the validity of our identification.

For robustness checks, in another specification we also include the provinces with no indigenous sterilizations at all and the provinces where we observe indigenous sterilizations before 1996 or only after the slowdown in the sterilization campaign. We also look at whether the intensity of the program, measured by the percentage of sterilizations among indigenous women aged 15 to 49 in the province, and the gender of the child affect differently our outcomes of interest.

Results

PNSRPF and Contraceptive Methods

We first look at the impact of the PNSRPF on the use of contraceptive methods for women whose children were born between January 1995 and December 1996. All estimates are based on OLS.[19] Concerns regarding compositional changes in the family and anticipation effects are mitigated in this context. The probability of abortion does not change before and after the policy in treated and control areas,[20] and the differences in the number of children born per woman between the areas rule out the possibility that control provinces might have heard about the enforcement practices in the treatment provinces and therefore reduced the use of contraceptives in order to conceive another child in 1996

or 1997, before they would be forced to undergo sterilization. In control provinces, the number of children born per woman did not increase in late 1996 or in 1997, compared to the previous years.[21]

The results on the use of contraceptive methods are reported in Table 3. Columns (1) and (2) report whether a woman uses contraceptive methods, columns (3) and (4) whether she uses temporary methods and columns (5) and (6) whether she was sterilized.

[insert TABLE 3 here]

Overall, women in treated provinces were 9.9 percentage points more likely to use any method of contraception: they were 4.9 percentage points more likely to use modern temporary methods, and they were 5.1 percentage points more likely to receive surgical contraception, compared to women in control areas. That is, the percentage of women using temporary methods was 31 percent in control areas as compared to approximately 36 percent in treated areas, and the percentage of women sterilized was 5.3 percent in control areas as compared to 10.4 percent in treated areas. As reported in column (6), in treated provinces, indigenous women were 8.3 percentage points more likely to be sterilized than nonindigenous women, confirming the targeted nature of the policy.[22] Meta-analysis suggests that family planning programs increase the use of contraceptive methods on average by approximately 20 percentage points (Angeles et al., 2001). Our results are therefore slightly less extreme but in line with previous literature.

PNSRPF and Child Health Care

Second, we look at the probability of the most recently born children to die before one month of age and before one year of age and on the probability of being delivered at home. Recall that children were born in either 1995 or 1996. Results are presented in Table 4.

[insert TABLE 4 here]

We find statistically significant effects of the program on rates of both neonatal and infant mortality. They decrease for all children whose mothers are affected by the policy by 5 to 6 percentage points. Such reduction partially explains the national average decrease in neonatal mortality from 23.5 in 1994 to 19.5 deaths per 1,000 live births in 1997 and in infant mortality from 45.8 to 37 deaths per 1,000 live births (The World Bank 2018).[23] This impact is not significantly different between indigenous and nonindigenous women, as reported in columns (2) and (4). Conversely, although the coefficients in column (5) suggest a positive impact of the policy, there are no statistically significant differences in the probability of home delivery between treated and control provinces, nor between indigenous and nonindigenous women (column (6)). At the end of the table, we include the pre-trend results. We regress the change in each outcome on the treatment and any control variables included in specification (2). Treatment does not explain the change in the pre-trend. Table A6 in the Appendix reports the placebo coefficients for the years 1995 to 1991, before the PNSRPF was introduced. In the years previous to the program, there are no statistically significant differences between treated and control provinces, while indigenous women always show higher levels of child mortality and home delivery than nonindigenous women. Furthermore, we compare the differences-in-differences estimates between the two groups for every birth cohort, i.e., 1996, 1995, 1994, and 1993, to the birth cohort in 1991 (Table A8 in the Appendix). The placebo results are robust and in line with our main results.

Third, we focus on the probability of being breast-fed longer than the average and on the probability of being fully immunized (at birth and by the first year). We expect breastfeeding and children's vaccination to explain part of the effects we observe on child mortality. Medical research indicates that both vaccination and breastfeeding lower child mortality. Childhood vaccination, and in particular

measles and tetanus vaccination, is associated with substantial reductions in child mortality (McGovern and Canning 2015; World Health Organization 2017). Human milk has immunological benefits (Morrow 2005), and breastfeeding protects against water- and food-borne disease (Habicht et al. 1998). As shown by Victoria (1987) and Jayachandran and Kuziemko (2011), among others, breastfeeding is associated with lower rates of infant mortality from diarrheal disease and acute respiratory infection, and child-mortality patterns mirror those of breastfeeding (Betran et al. 2001; Chen, Yu, and Li 1988). More generally, the World Health Organization (2000) estimates that in developing countries, mortality risk between ages one and two is twice as high if a child is not being breastfed.

[insert TABLE 5 here]

As reported in Table 5, women affected by the policy breastfed their children longer than women in control areas. They were 18.5 percentage points more likely to breastfeed their children more than the average length for their months of age. This increase corresponds to approximately 3 weeks: the average length of breastfeeding goes from 10.2 to almost 11 months. However, there are not statistical significant impacts of the PNSRPF on vaccines. At the end of the table, we include the pre-trend results as in Table 4. Treatment does not explain the change in the pre-trend. Table A7 in the Appendix reports the placebo coefficients for the years 1995 to 1991, and Table A8 reports the differences-in-differences estimates between treated and control groups for every birth cohort to the birth cohort in 1991. Again, the placebo results are consistent with our main results.

Since breastfeeding is associated with lower child mortality, we perform an additional exercise and provide evidence that children breastfed for longer due to the policy had a lower probability of dying in their first year. The estimates in Table A9 in the Appendix are obtained with an instrumental variable method where the instrument is equal to 1 if the woman lived in a treated province. They

suggest that an increase of one month in breastfeeding due to the policy reduced neonatal mortality by 2 percentage points and infant mortality by 1.6 percentage points, on average. We cannot perform the corresponding exercise for vaccination since treatment is a weak instrument for this outcome.

Robustness Checks

Consistent results are found when we include provinces where there were never indigenous sterilizations, provinces where there were indigenous sterilizations before 1996 and the few provinces where there were indigenous sterilizations only starting from 1998. In Panel A of Table 6, we compare treated provinces, control provinces - which were then treated in 1997 - and provinces not included in the main analysis. The latter category comprises all remaining Peruvian provinces. The category omitted is control provinces treated later, that is, in 1997. In Panel B, control provinces treated later and provinces not included are collapsed into a single category. Treatment is a dummy equal to 1 if the woman lives in a treated province. The results in both panels are consistent with the findings of the main analysis.[24]

[insert TABLE 6 here]

To further study the impacts of the PNSRPF on mothers' contraception and children's health care, we analyze whether there are heterogeneous effects based on the intensity of the program and children's sex.[25] The results for intensity are consistent with the main ones. We consider the percentage of sterilizations among indigenous women aged 15 to 49 in each treated province (*intensity*): as reported in Table A10 in the Appendix, the intensive margin is as relevant as the extensive margin. The children's sex is never relevant when looking for differential impacts of the policy on our outcomes of interest (Table A11 in the Appendix).

Permanent versus Temporary Contraception. Some Suggestive Evidence.

Since we are also interested in understanding the possible differential child health care behaviors correlated with permanent versus temporary interventions, we perform a second exploratory analysis where we focus on the effects that each type of intervention might have on the primary outcomes of interest: child mortality, place of delivery, breastfeeding and vaccination. Had the policy affected the use of these methods only in treated provinces, we restrict the sample to those provinces. We first define the specification as follows:

$$Y_{ip} = \lambda + \delta_1 Temporary_{ip} + \delta_2 Permanent_{ip} + X'_{ip}\rho + \nu_{ip} \qquad (4)$$

where Y_{ip} is equal to our outcomes of interest for child *i* born up to 1 year before the policy in province *p*. $Temporary_{ip}$ is a dummy equal to 1 if the mother uses a temporary contraceptive method; and $Permanent_{ip}$ shows if she was sterilized. The reference category is $None_{ip,}$ which is a dummy equal to 1 if the mother does not use any contraceptive method. The vector of control variables (X'_{ip}) is the same as in specification (2). Robust standard errors are clustered at the province level. As before, we also look at differential behaviors by ethnicity.

The δ_1 and δ_2 coefficients in specification (4) could, however, not satisfy our goal: the policy promoted VSC for the first time in the country, but it also aimed to achieve a higher prevalence in the use of other modern contraceptive methods. The reason to use one method or another can be an individual choice correlated with unobservables that also affect the outcomes of interest and thus may not be exogenously due to the policy. To reduce the noise of the analysis and consider as exogenous the contraceptive method adopted, we restrict the sample to young women. These women have rarely reached their desired number of children and thus are less likely to choose sterilization. Using a temporary or permanent contraceptive method is less likely a voluntary choice and is more likely due

to the reported irregularities during the program implementation. More precisely, we restrict the sample to women aged less than 31. Thirty-one years old is the median age of sterilization before the policy. According to the DHS, before 1996, 75 percent of women were sterilized after the age of 29 years. During the peak period of the policy, more young women were sterilized: 75 percent were sterilized after age 26.[26] The choice to restrict to women aged less than 31 is also supported by the findings of Byker and Gutierrez (2012). They estimate that women targeted by the policy were indigenous women of, on average, 31 years old.[27] Although we cannot claim with certainty that the effects we observe are due to an exogenous variation in family size, we believe that by restricting the sample to young women, we are able to provide suggestive evidence of the impacts on child health care when a policy intervention is aggressively implemented.

[insert TABLE 7 here]

As reported in Panel A of Table 7, there are differences between indigenous and nonindigenous children in treated provinces. Nonindigenous children benefited from the fact that their mothers used contraception, regardless of the method used. If women adopted temporary methods, their children were 17.6 percentage points less likely to be delivered at home and 7.1 percentage points less likely to die in the first year of life compared to children whose mothers did not use any contraceptive method. If women were sterilized, their children were 23.1 percentage points more likely to be breastfed longer than the average compared to children whose mothers did not use any contraceptive method. In the case of indigenous children, there were positive effects on child health care if their mothers used temporary methods but much weaker ones if they were sterilized. Children whose mothers used temporary contraception were 16.8 percentage points more likely to be breastfed longer than the average (roughly 1 month more) and 26.1 percentage points more likely to receive the required vaccinations at birth, compared to children whose mothers did not use any contraception. Permanent

interventions were less beneficial for child health care. Although children of young indigenous women who were sterilized were less likely to die than children of young indigenous women who did not use any birth control method, this appears to have been mainly related to the fact that they were delivered in safer conditions and not because they received better child care after delivery. The anecdotal evidence can help us interpret the results: most indigenous mothers experienced surgical interventions after giving birth either during health festivals or at health centers (Ballón, 2014). Their children were born in a safer environment since they were attended by doctors. They might have fewer complications during the delivery and thus be less likely to die by their first year of life. Nonetheless, columns (4) to (6) of Panel A of Table 7 suggest that they were not breastfed for longer, nor did they receive the appropriate vaccinations by their age. Panel B of Table 7 report the results for the whole sample. They are suggestive of the behaviors women would have had if no policy had been aggressively implemented. Interestingly, nonindigenous children still benefited from the fact that their mothers used contraception, with effects comparable to those of children whose mothers were aged less than 31. Their children were more likely to receive complete vaccination at birth and were less likely delivered in unsafe conditions.[28] In contrast, while for indigenous children whose mother used a temporary method we still observe higher breastfeeding and complete vaccination at birth, we observe almost no difference between children of mothers who were sterilized and children whose mothers did not use any contraceptive method. Children of indigenous sterilized women were neither breastfed for longer, nor received more vaccinations, and all the effects of sterilization on mortality disappeared.

We are aware that the sample shrinks when we look at the differential child health care behaviors associated with the contraceptive methods used. We acknowledge that we cannot derive strong conclusions and claim some causal interpretation of the impact of contraception on child health since we do not know from the survey who freely decided to undergo sterilization and who was coerced. Nevertheless, we believe these results are informative of different reactions to the policy by ethnicity

and that such effects comply with the anecdotal evidence that the PNSRPF aggressively targeted indigenous women.

Conclusions

The PNSRPF was carried out in Peru from 1996 to 2000. At the time, infant and child mortality rates were exceptionally high and there was high prevalence of chronic malnutrition among children under five years. With our study, we have quantified the short-run impacts of this family planning program on women's use of birth-control methods and their children's mortality. The program's short-run impact was estimated by comparing two types of provinces (treated vs. control) in the year when the treated provinces were reached by the program (1996) compared to before (1995). The main results show that women in treated areas were more likely to use contraceptive methods compared to women in control areas and that their children were less likely to die within their first year of life. This reduction in mortality can be partially explained by the longer time mothers breastfed their children in treated provinces. The likelihood that children received the appropriate vaccinations for their age was not significantly affected by the policy. The results are robust when a measure of intensity of the program is introduced and when provinces not affected by the program are included in the control group. The sex of the child is not relevant for any of the outcomes of interest.

The PNSRPF marked the first use of Voluntary Surgical Contraception, or sterilization, in the country, and there were reported irregularities in the form of coercion, especially affecting indigenous women. In treated provinces we find suggestive evidence of differential child health care behaviors by ethnicity. While nonindigenous children benefited from the policy regardless of the contraceptive method their mothers used, the PNSRPF had few positive impacts on child health care for indigenous children whose mothers were sterilized. In line with Byker and Gutierrez (2012), this suggests that when birth control is aggressively imposed, there may be little or no health benefits for children.

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Notes

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[2] In our analysis, provinces are preferred to districts due to their higher population representativeness.

[3] During the Indian Emergency (1975-1977), in response to the unprecedented population growth of the 1960s, aggressive sterilization camps were held all over the country, and about 8.3 million sterilizations, mainly vasectomies, were carried out. In China, to achieve fertility-related targets, starting from 1982, a policy advocating sterilization was strongly promoted by the government. Several researchers have suggested that, at times, birth planning officials under pressure to meet quotas have coerced women into accepting sterilization (UNDESA 2013).

[4] Total Fertility Rate decreased as expected, as reported in Table A1 in the Appendix.

[5] The number of health posts, health clinics and health centers run by the Ministry increased by over 50% between 1995 and 2000, and over 10,000 medical and paramedical staff were added across the country. Annual government spending on health increased by 40% (Gribble et al. 2007). However, according to Boesten (2007), no improvements in the quality of rural healthcare services, such as the provision of a hygienic working environment, medical supplies or even beds, were provided. Instead, the government improvised mobile medical services for rural areas.

[6] Teams of doctors and nurses gathered, usually in Lima, and were then sent around the country. Doctors from Colombia and India are reported to have been brought in to train Peruvian doctors and officials in how to run campaigns (Morrison 1998).

[7] Based on the United Nations age and gender specific population tables, Byker and Gutierrez (2012) estimate that the DHS reports of sterilizations from 1996 to 1997 imply that nearly 172,000 women were sterilized in those two years (5 percent of women aged 25-49). According to CLADEM, the government forced sterilization on almost 1.5 million of women.

[8] In Figure 1 it can be observed that the percentage of sterilizations in 1998 already went back to the pre-policy rate.

[9] 1986 Peruvian DHS only provides information on the language in which the interview was conducted and not on the mother tongue. The language of the survey is hardly a good proxy for being indigenous since interviews can be performed in Spanish even by indigenous people. Moreover, there is not enough variation in this outcome: only 1% of women responded to the survey in an indigenous language.

[10] Between 1980 and 1993, the rebel group Partido Comunista del Perú - Sendero Luminoso (PCP - SL), a communist militant group, and the national army have been involved in a dramatic internal conflict that caused an intense period of violence for Peru. Since the capture of its leader Abimael Guzmán in 1992, the Shining Path sharply reduced its activity, and in the period we are considering, its power within the country was limited.

[11] Examples can be found in Figure A1 in the Appendix.

[12] Household wealth is defined through the principal component analysis and is a proxy for household durables and utilities. There are three geographical regions in Peru: jungle (*selva*), highlands (*sierra*) and coast (*costa*). The dummy Jungle is equal to 1 if the household is in the jungle and 0 otherwise.

[13] The most frequent months of the arrival of the policy in year 1996 are reported in Figure A2 in the Appendix. Some 60% of the provinces received the policy by August. The month with the least provinces reached is December.

[14] Normalized differences are for each covariate, the difference in averages by treatment status, scaled by the square root of the sum of the variances, as a scale-free measure of the difference in distributions. The reason to focus on the normalized difference, column (5), rather than on the t-statistic, column (4), comes from their relation to the sample size. The sample sizes in treated and control groups are indeed relatively small and unbalanced.

[15] Traditional contraceptive methods are defined as periodic abstinence and withdrawal, and they also include folkloric methods based mostly on herbal beliefs. Modern methods include the pill, IUD, injections, diaphragm/foam/jelly, condoms and Norplant.

[16] The World Bank data are lower than our data estimates. The regions we focus- on have a higher number of indigenous women than the average and thus, not surprisingly, higher mortality rates. On average, in our sample infant mortality for indigenous children is 0.09 and neonatal mortality is 0.066. For nonindigenous children, infant mortality is 0.062 and neonatal mortality is 0.046.

[17] For Peru, the general immunization schedule includes, at birth, Bacille Calmette-Guérin (BCG) and oral polio vaccine-neonatal dose (OPV-0); at month two, OPV, dose 1 (OPV-1) and diphtheria-pertussis-tetanus, dose 1 (DPT-1); at month three, OPV-2 and DPT-2; at month four, OPV-3 and DPT-3; and at month twelve, measles vaccine (World Health Organization 2004).

[18] Table A5 in the Appendixl reports the predetermined outcomes for the years 1994 to 1991. They further confirm that treated and control samples are comparable before the policy was introduced.

[19] The results using probit estimates are comparable and available upon request.

[20] Results are available upon request.

[21] In control provinces, the number of children born per woman is equal to 0.10 in 1995, 0.10 in 1996 and 0.086 in 1997. We do not observe changes in treated provinces: the number of children born per woman is 0.11, 0.12 and 0.09, respectively.

[22] As mentioned before, we cannot provide placebo tests for contraceptive methods since we do not have chronological information on the use of birth control methods prior to the last and current method used. We can only provide proxy pre-trends in Figure 4b.

[23] The World Bank data are reported for the entire country and are lower than our estimates for the initial period. This is not surprising since we focus on regions where, at the beginning of the policy, mortality rates are higher than the country average.

[24] The only lies in the coefficient of the mother's use of permanent contraceptive methods, which is now not statistically significant. This is not worrisome since among the provinces not included in the main sample, there are also provinces with indigenous sterilizations before 1996 and with an overall higher number of sterilizations. These sterilizations, not imputable to the policy, are more than in our main control group: 10% of women in the provinces not included or treated later (bottom line of Panel B of Table 6) versus 5% in the provinces only treated later (main control group).

[25] We also control for children's ranking among siblings. The results are not particularly informative: there is not a clear pattern suggesting that birth order matters for our outcomes of interest. They are available upon request.

[26] The results hold if we use as thresholds 29, 30 or 32 years old. They are available upon request.

[27] Ideally, we could consider the impact of contraceptive method on the outcomes of interest by using an instrumental strategy with exposure to the policy as an instrument for the use of contraceptive methods. However, we are interested in the differential impact of contraceptive methods, and the policy would be a suitable instrument for both temporary and permanent methods, therefore excusing us from using IV methods. The coefficients would be underidentified, the instruments being fewer than the endogenous regressors.

[28] Infant and neonatal mortality of nonindigenous children whose mothers were not using birth control methods are small on average (neonatal: 8 versus 17 per 1000 live births among indigenous children; infant: 49 versus 70 per 1000 live births among indigenous children). Thus, it is not surprising that our results, although positive, are not statistically significant.

1. Tables

TABLE 1 Predetermined Characteristics

Variable	(1) All	(2) Treatment	(3) Control	(4) P-value (2)-(3)	(5) Normalized Differences
a. Mother's characteristics					
Age mother	27.639	27.755	27.245	.307	.059
	(6.132)	(6.176)	(5.967)		
Age mother squared	1337.35	1338.425	1333.711	.929	.006
	(535.619)	(537.136)	(530.708)		
Years of education	5.321	4.991	6.443	.021	-.238
	(4.344)	(4.289)	(4.345)		
Indigenous (=1)	.449	.512	.237	.014	.419
	(.497)	(.5)	(.425)		
Married (=1)	.554	.587	.442	.010	.208
	(.497)	(.492)	(.497)		
Working (=1)	.382	.366	.436	.712	-.102
	(.486)	(.482)	(.496)		
b. Household characteristics					
Household wealth[a]	-.514	-.609	-.192	.192	-.203
	(1.434)	(1.392)	(1.151)		
City (=1)	.196	.181	.246	.386	-.112
	(.397)	(.385)	(.431)		
Town (=1)	.115	.092	.192	.011	-.205
	(.319)	(.289)	(.394)		
Rural (=1)	.689	.726	.562	.141	.247
	(.423)	(.446)	(.496)		
Jungle (=1)	.104	.077	.195	.330	-.247
	(.306)	(.267)	(.397)		
Observations	4243	3276	967		
c. Child's characteristics					
Ranking siblings	3.553	3.624	3.312	.278	.095
	(2.356)	(2.386)	(2.236)		

Female (=1)	.487	.495	.464	.335	.044
	(.5)	(.5)	(.499)		
Quarter of birth 1st	.28	.29	.248	.012	.068
	(.449)	(.454)	(.432)		
Quarter of birth 2nd	.285	.29	.27	.260	.031
	(.452)	(.434)	(.444)		
Quarter of birth 3rd	.231	.231	.23	.895	.001
	(.421)	(.422)	(.421)		
Quarter of birth 4th	.203	.189	.252	.007	-.107
	(.402)	(.392)	(.434)		
Observations	4275	3302	973		

[a] The wealth index ranges between -2.814 and 3.463.

Columns 1 to 3 report means with standard deviation in parentheses, based on children and women in all, treatment and control provinces respectively. Column 4 reports the p-value of the test of equal means, allowing for standard errors to be clustered by province. Column 5 reports normalized differences computed as the difference in means in treatment and control villages divided by the square root of the sum of the variances.

TABLE 2 Predetermined Children Outcomes

Variable	(1) All	(2) Treatment	(3) Control	(4) P-value (2)-(3)	(5) Normalized Differences	(6) Observations
a. Mother's contraception						
Any (=1)	.304	.252	.395	.102	.141	1747
	(.460)	(.434)	(.489)			
Temporary (=1)	.299	.248	.387	.115	-.188	1747
	(.458)	(.432)	(.487)			
Permanent (=1)	.005	.003	.007	.404	.062	1747
	(0.071)	(0.059)	(0.088)			
b. Children's outcomes						
Neonatal mortality (=1)	.055	.059	.036	.240	.079	1695
	(.228)	(.237)	(.186)			
Infant mortality (=1)	.075	.080	.052	.116	.080	1695
	(.263)	(.273)	(.223)			
Home delivery (=1)	.675	.698	.584	.810	.170	1024
	(.468)	(.459)	(.414)			
Breastfeeding (=1)	.718	.705	.753	.065	-.077	864
	(.45)	(.456)	(.432)			
Vaccines at birth (=1)	.392	.403	.350	.158	.077	1023
	(.488)	(.491)	(.478)			
Vaccines by 1st year (=1)	.294	.303	.262	.395	.064	711
	(.456)	(.460)	(.441)			

Columns 1 to 3 report means with standard deviation in parentheses, based on children and women in all, treatment and control provinces respectively. Column 4 reports the p-value of the test of equal means, allowing for standard errors to be clustered by province. Column 5 reports normalized differences computed as the difference in means in treatment and control villages divided by the square root of the sum of the variances.

TABLE 3 Mothers' Contraceptive Methods

	(1)	(2)	(3)	(4)	(5)	(6)
	Any		Temporary		Permanent	
treat*post	0.099***	0.070*	0.049***	0.029	0.051***	0.041*
	(0.025)	(0.038)	(0.018)	(0.026)	(0.016)	(0.024)
treat	-0.071**	-0.102***	-0.061**	-0.092***	-0.011	-0.010
	(0.028)	(0.038)	(0.027)	(0.036)	(0.010)	(0.013)
post	-0.070***	-0.075***	-0.161***	-0.200***	0.091***	0.125***
	(0.020)	(0.027)	(0.015)	(0.019)	(0.012)	(0.016)
Indigenous	-0.052*	-0.134***	-0.075***	-0.213***	0.023***	0.079***
	(0.027)	(0.045)	(0.026)	(0.044)	(0.006)	(0.011)
treat*indigenous		0.084*		0.116**		-0.032**
		(0.049)		(0.047)		(0.015)
post*indigenous		0.016		0.141***		-0.125***
		(0.032)		(0.026)		(0.016)
treat*post*indigenous		0.041		-0.041		0.083***
		(0.046)		(0.033)		(0.027)
Observations	3434	3434	3434	3434	3434	3434
Mean in control	0.637	0.637	0.310	0.310	0.053	0.053

Robust standard errors clustered at the province level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In all the columns, we control for mother's characteristics (age, age squared, years of education, whether she is indigenous, whether she is married and whether she is currently working), household's characteristics (wealth and location) and region fixed effects.

TABLE 4 Infant and Neonatal Mortality and Place of Delivery

	(1)	(2)	(3)	(4)	(5)	(6)
	Mortality					
	neonatal		infant		Home delivery	
treat*post	-0.052***	-0.046***	-0.061***	-0.057***	0.027	0.062
	(0.010)	(0.011)	(0.013)	(0.016)	(0.030)	(0.042)
treat	0.025***	0.022**	0.026*	0.027*	-0.015	-0.044
	(0.010)	(0.011)	(0.013)	(0.013)	(0.039)	(0.049)
post	-0.004	-0.005	-0.009	-0.008	-0.009	-0.003
	(0.008)	(0.009)	(0.011)	(0.012)	(0.025)	(0.031)
Indigenous	0.014**	0.011	0.020**	0.032	0.136***	0.106**
	(0.006)	(0.014)	(0.008)	(0.022)	(0.029)	(0.050)
treat*indigenous		0.008		-0.007		0.074
		(0.015)		(0.023)		(0.062)
post*indigenous		0.004		-0.007		-0.022
		(0.013)		(0.019)		(0.035)
treat*post*indigenous		-0.014		-0.003		-0.052
		(0.017)		(0.024)		(0.047)
Observations	8688	8688	7556	7556	2080	2080
Mean in control	0.033	0.033	0.058	0.058	0.588	0.588
Pre-trend *beta*[a]	-0.010	-0.008	0.007	0.010	-0.017	-0.032
	(0.009)	(0.009)	(0.009)	(0.009)	(0.099)	(0.107)

[a] Pre-trend *beta* is the coefficient for treatment in the period prior to the intervention. Specifically, the change in outcome is regressed on the treatment and any control variables included in the specification.

Robust standard errors clustered at the province level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In all the columns, we control for child's characteristics (ranking among siblings, gender, quarter of birth), mother's characteristics (age, age squared, years of education, whether she is indigenous, whether she is married and whether she is currently working), household's characteristics (wealth and location) and region fixed effects.

TABLE 5 Children's Breastfeeding and Vaccination

	(1)	(2)	(3)	(4)	(5)	(6)
			Vaccines			
	Breastfeeding		at birth		by 1st year	
treat*post	0.185***	0.253***	-0.027	-0.050	0.021	0.006
	(0.060)	(0.075)	(0.062)	(0.077)	(0.063)	(0.075)
Treat	-0.122***	-0.153***	0.082*	0.059	0.044	0.052
	(0.038)	(0.045)	(0.046)	(0.061)	(0.056)	(0.065)
Post	-0.088*	-0.093	0.032	0.064	0.040	0.036
	(0.051)	(0.056)	(0.063)	(0.077)	(0.071)	(0.074)
Indigenous	-0.018	-0.040	-0.007	-0.027	-0.003	-0.013
	(0.032)	(0.080)	(0.039)	(0.098)	(0.045)	(0.088)
treat*indigenous		0.087		0.052		-0.013
		(0.082)		(0.109)		(0.103)
post*indigenous		0.019		-0.121		0.013
		(0.098)		(0.123)		(0.107)
treat*post*indigenous		-0.131		0.101		0.021
		(0.114)		(0.133)		(0.127)
Observations	1567	1567	2477	2477	1511	1511
Mean in control	0.791	0.791	0.342	0.342	0.246	0.246
Pre-trend *beta*[a]	-0.188	-0.275	-0.044	-0.052	-0.042	-0.046
	(0.224)	(0.210)	(0.067)	(0.064)	(0.069)	(0.068)

[a] Pre-trend *beta* is the coefficient for treatment in the period prior to the intervention. Specifically, the change in outcome is regressed on the treatment and any control variables included in the specification.

Robust standard errors clustered at the province level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In all the columns, we control for child's characteristics (ranking among siblings, gender, quarter of birth), mother's characteristics (age, age squared, years of education, whether she is indigenous, whether she is married and whether she is currently working), household's characteristics (wealth and location) and region fixed effects.

TABLE 6 Mothers' Contraceptive Methods and Children's Health Care

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mothers' contraceptive methods			Mortality				Vaccines	
	any	temporary	permanent	neonatal	infant	Home delivery	Breastfeeding	at birth	by 1st year
a. Categorical (the category omitted corresponds to provinces treated later – control group in the main analysis)									
treat*post	0.013	0.030	-0.017	-0.040***	-0.044**	0.028	0.096*	-0.051	0.005
	(0.042)	(0.028)	(0.035)	(0.010)	(0.013)	(0.031)	(0.053)	(0.061)	(0.063)
not included *post	0.028	-0.013	0.041	-0.000	0.000	0.004	0.052	-0.031	-0.032
	(0.043)	(0.028)	(0.035)	(0.008)	(0.011)	(0.028)	(0.047)	(0.057)	(0.056)
treat	-0.028	-0.030	0.002	0.021**	0.020*	0.002	-0.065*	0.072	0.029
	(0.034)	(0.030)	(0.019)	(0.008)	(0.011)	(0.037)	(0.033)	(0.051)	(0.053)
not included	-0.010	0.003	-0.013	-0.001	-0.005	0.016	-0.064*	0.082*	0.042
	(0.034)	(0.031)	(0.019)	(0.007)	(0.010)	(0.041)	(0.034)	(0.048)	(0.050)
post	-0.008	-0.158***	0.150***	-0.003	-0.005	-0.017	-0.065	0.071	0.066
	(0.038)	(0.026)	(0.033)	(0.008)	(0.010)	(0.026)	(0.045)	(0.057)	(0.061)
indigenous	-0.072***	-0.075***	0.004	0.014***	0.018***	0.118***	-0.009	-0.037	-0.031
	(0.022)	(0.020)	(0.008)	(0.004)	(0.006)	(0.021)	(0.022)	(0.027)	(0.030)
Observations	12930	12930	12930	27652	23420	7586	5694	8596	5014
Mean in control	0.363	0.311	0.053	0.033	0.058	0.588	0.791	0.342	0.246
b. Provinces treated later or not included									
treat*post	0.026	0.050***	-0.024	-0.040***	-0.044***	0.024	0.049	-0.023	0.034
	(0.029)	(0.016)	(0.023)	(0.006)	(0.008)	(0.019)	(0.036)	(0.028)	(0.037)
treat	-0.036	-0.053**	0.017	0.022***	0.023***	-0.007	-0.020	0.018	-0.000
	(0.024)	(0.022)	(0.011)	(0.006)	(0.009)	(0.028)	(0.029)	(0.034)	(0.039)
post	0.008	-0.171***	0.179***	-0.003	-0.005	-0.013	-0.017	0.042**	0.037

	(0.017)	(0.010)	(0.013)	(0.002)	(0.003)	(0.010)	(0.021)	(0.018)	(0.031)
indigenous	-0.071***	-0.075***	0.003	0.014***	0.018***	0.118***	-0.008	-0.038	-0.032
	(0.022)	(0.020)	(0.008)	(0.004)	(0.006)	(0.021)	(0.022)	(0.027)	(0.030)
Observations	12930	12930	12930	27652	23420	7586	5694	8596	5014
Mean in control	0.445	0.344	0.101	0.024	0.039	0.464	0.710	0.498	0.392

Robust standard errors clustered at the province level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In columns (1) to (3), we control for mother's characteristics (age, age squared, years of education, whether she is indigenous, whether she is married and whether she is currently working), household's characteristics (wealth and location) and region fixed effects. In columns (4) to (9), we control for child's characteristics (ranking among siblings, gender, year and quarter of birth), mother's characteristics (age, age squared, years of education, whether she is indigenous, whether she is married and whether she is currently working), household's characteristics (wealth and location) and region fixed effects.

TABLE 7 Treated Provinces: Contraceptive Methods on Mortality, Breastfeeding and Vaccination

	(1)	(2)	(3)	(4)	(5)	(6)
	Mortality				Vaccines	
	neonatal	infant	Home delivery	Breastfeeding	at birth	by 1st year
			a. Women aged less than 31			
Indigenous						
temporary	0.046	-0.030	-0.003	0.168***	0.261**	0.143
	(0.057)	(0.046)	(0.073)	(0.043)	(0.098)	(0.092)
Permanent	-0.019*	-0.082***	-0.303**	0.001	-0.055	-0.010
	(0.011)	(0.017)	(0.132)	(0.221)	(0.170)	(0.125)
Observations	618	906	233	399	312	288
Mean in control	0.018	0.074	0.884	0.752	0.330	0.260
Nonindigenous						
Temporary	0.004	-0.070*	-0.176**	0.042	0.168	0.135
	(0.003)	(0.035)	(0.069)	(0.046)	(0.116)	(0.097)
Permanent	0.008	-0.026	-0.239	0.231*	-0.135	0.060
	(0.006)	(0.025)	(0.200)	(0.123)	(0.234)	(0.171)
Observations	445	722	159	300	191	184
Mean in control	0.007	0.048	0.503	0.173	0.387	0.310
			b. All women			
Indigenous						
Temporary	0.036	-0.007	-0.048	0.141***	0.209**	0.097
	(0.046)	(0.057)	(0.064)	(0.044)	(0.102)	(0.076)
Permanent	0.004	-0.033	-0.206**	0.048	0.006	-0.035
	(0.022)	(0.024)	(0.090)	(0.100)	(0.123)	(0.090)
Observations	959	1443	362	621	477	455
Mean in control	0.017	0.070	0.895	0.776	0.308	0.237
Nonindigenous						

Temporary	-0.001	-0.011	-0.193***	0.066	0.172*	0.113
	(0.006)	(0.037)	(0.058)	(0.044)	(0.101)	(0.087)
Permanent	-0.010	-0.019	-0.219*	0.199	0.183*	0.149
	(0.008)	(0.017)	(0.123)	(0.167)	(0.094)	(0.125)
Observations	644	1031	248	435	284	268
Mean in control	0.008	0.049	0.480	0.722	0.380	0.310

Robust standard errors clustered at the province level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In all the columns, we control for child's characteristics (ranking among siblings, gender, quarter of birth), mother's characteristics (age, age squared, years of education, whether she is indigenous, whether she is married and whether she is currently working), household's characteristics (wealth and location).

2. Figures

FIGURE 1 Sterilizations, 1986-2006

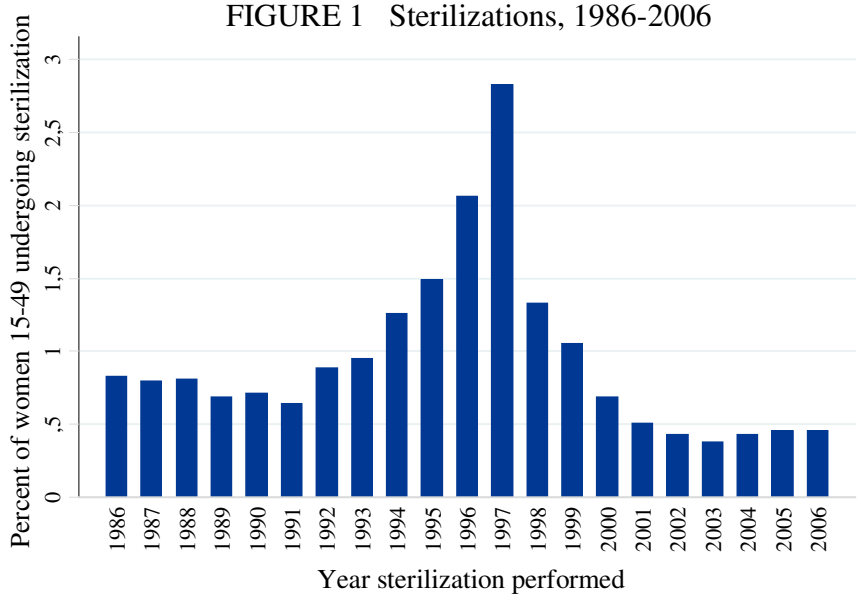


FIGURE 2 Sterilizations, 1986-2006

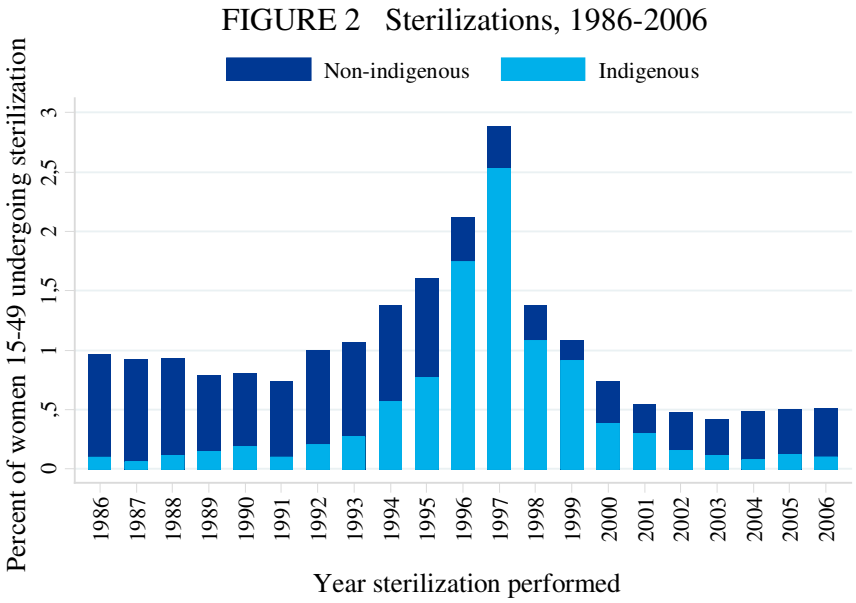
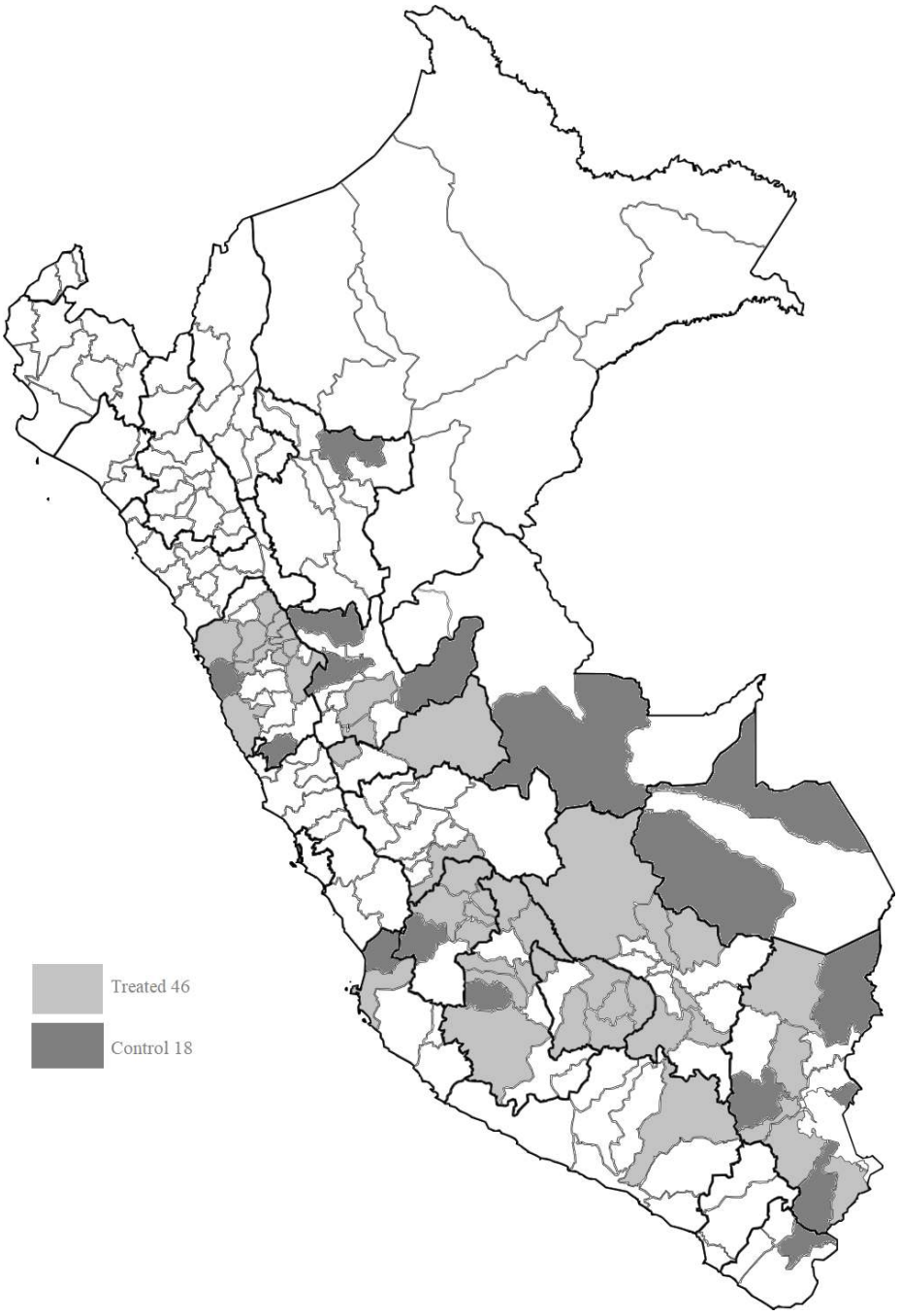


FIGURE 3 Treated and Control Provinces



F

FIGURE 4a Parallel trend assumption

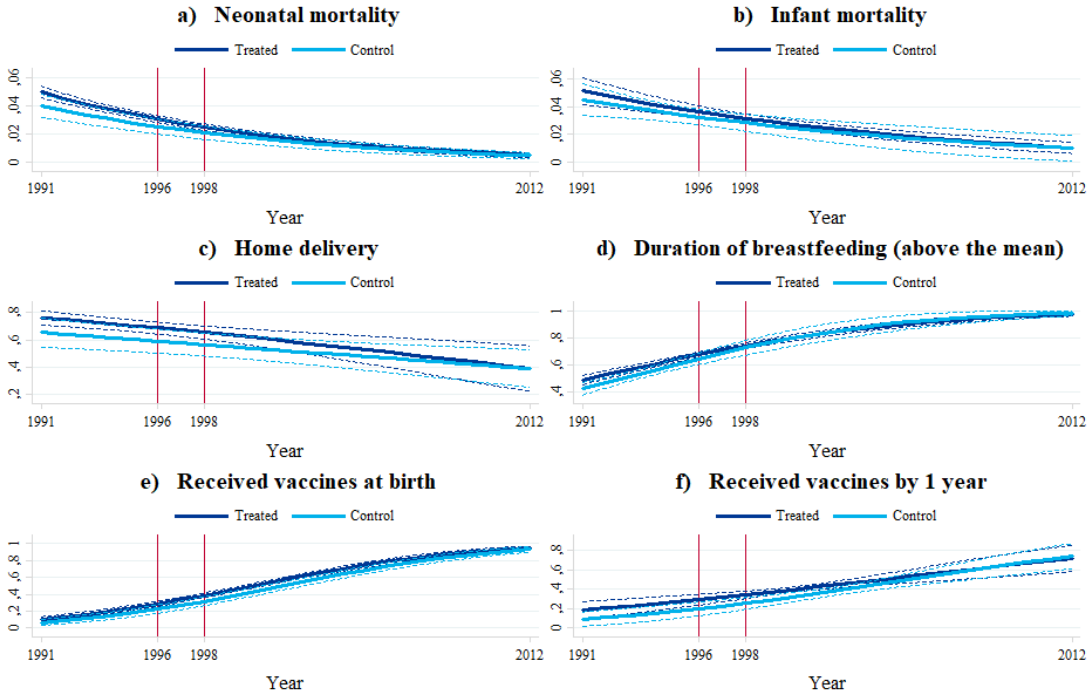


FIGURE 4b Parallel trend assumption