# Conditional Cash Transfers and Women's Reproductive Choices*

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**Abstract**: We study potential unintended effects of a large-scale national conditional cash transfer program – Peru's Juntos – on the fertility and reproductive decisions of adult beneficiaries. We use an event study design, exploiting time and geographic variation in the rollout of the program, to identify the causal effects of the program. We find that Juntos increases adult beneficiaries' take-up of modern contraception and decreases the average number of children they have – effects that persist over time. We explore various mechanisms and find that Juntos empowers women to avoid unwanted births most likely due to increased exposure to information regarding and access to family planning and modern birth control.

**Keywords**: Cash Transfers; Fertility; Fertility Preferences; Intrahousehold Bargaining; Peru

*JEL* **classification nos:** H31; I15; J13; J16; O12

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# 1 Introduction

Conditional cash transfers (CCT) are among the most widely adopted social protection programs because of their promise of improved household economic welfare and greater investments in children's human capital.[1] These promises have largely been met thanks to both the cash transfer and the conditions that households must meet in order to receive the income transfer, most often linked to child schooling and health (Bastagli et al. (2016)). At the same time, beyond these outcomes (i.e., household economic welfare and children's human capital) that are directly incentivized by the programs, policy makers also care about potential unintended effects on a broader set of outcomes.

For instance, it is theoretically possible that a CCT influences the fertility behaviour of the recipients, although fertility is not directly targeted by the program. In fact, one long-standing view among sceptics of anti-poverty income maintenance programs is the worry that poor households will use the cash transfer to finance greater fertility, a view dating back to Malthus' discussion of England's Poor Laws (Huzel (1980)). In contrast, there are at least three channels through which a CCT might decrease fertility. First, Becker's quality-quantity tradeoff model predicts that an increase in income leads parents to invest more in the quality of children decreasing desired fertility (Becker (1960)). Second, because they tend to target mothers, the transfers can empower women (Das, Do, and Özler (2005); Fiszbein and Schady (2009); Alcázar, Balarin, and Espinoza Iglesias (2016); Almås et al. (2018)), and there is considerable evidence that women's empowerment is linked to a decline in fertility (Upadhyay et al. (2014)). Finally, the health conditionalities often associated with CCT programs can increase women's exposure to information on family planning and access to birth control.

In this paper, we study the effects of Peru's national CCT program "Juntos" on the fertility and fertility decisions of adult beneficiaries.[2] Juntos was introduced in 2005 and provides a cash transfer to (i) mothers with children under the age of 14 and (ii) pregnant women, on

1. Interest in cash transfers has also increased globally heightened by the economic fallout of COVID-19 (Gentilini (2022)).

2. We look at the effects among beneficiary mothers not among adolescent children in beneficiary households (as in, for example, Baird, McIntosh, and Özler (2011)).

the condition that children attend school, that children aged 5 or less attend well-baby checks, and that pregnant women attend pre-natal care. One important feature of the program is that fertility decisions of beneficiaries are not directly incentivized since Juntos' transfer does not increase with the number of children a woman has. More specifically, we answer the following three questions: 1) what are the effects of Juntos on adult beneficiaries' birth control use and number of children? 2) do the effects last over time? 3) what mechanisms explain these effects?

We answer these questions by combining annual Demographic and Health Survey (DHS) data for Peru from 2004 to 2017 with administrative data on the Juntos rollout. Using these data we determine potential recipients of Juntos using data on individual and household characteristics. We identify the causal effects of Juntos on these potential recipients by exploiting the district-level staggered implementation of the program in an event study approach. Given recent econometric advances in the differences-in-differences literature (e.g., Goodman-Bacon (2021); Chaisemartin and D'Haultfœuille (2020)), we consider both traditional two-way fixed effects models and more robust estimation techniques that are especially appropriate when we may anticipate heterogeneous treatment effects across groups, such as the one suggested by Sun and Abraham (2021). We explore potential mechanisms by looking at the impact of the program on preferences, intra-household bargaining, and facilitating access to reproductive health information or care.

We find long-lasting effects of Juntos on fertility outcomes: the program increased beneficiaries' use of modern forms of birth control and decreased the average number of children a woman has at a given point in time – effects that persist at least 6 years after the introduction of the program. These results dispel concerns that anti-poverty policies create undesired incentives for families to have more children in the case of Peru. They also provide new evidence of long-term and potentially transformative effects of these programs.

Exploring potential mechanisms, we find no effect of Juntos on fertility preferences as measured by the respondents' ideal number of children or by discordance between the respondents and their spouses. Instead, we show that Juntos led to a reduction in excess fertility and a corresponding increase in the probability that women exactly meet their desired family size.

Our results suggest that this alignment between preferences and actual family size is not driven by a change in intra-household bargaining power – we find little effect of Juntos on measures of women's autonomy in the fertility domain – but by increased utilization of reproductive health services. In summary, our analysis shows that Juntos empowers women to avoid unwanted births by increasing uptake of modern contraceptives most likely due to increases exposure to information on family planning and access to birth control.

We conduct several robustness checks of our identification strategy. First, we check and confirm absence of significant pre-trends, both graphically and with parametric tests on pre-rollout coefficients. Second, we test our event study identification strategy against newer methods which are more robust to treatment effect heterogeneity (i.e., Sun and Abraham (2021)). Our results remain robust to these considerations. Third, we conduct a falsification test by restricting the sample to women who should *a priori* not be affected by the policy: non-poor women. Indeed, we find no effect of Juntos on fertility outcomes and reproductive choices for this group.

Our study makes several contributions to the economics of social protection and fertility literatures. First, our study offers new evidence to the literature on the effects of CCTs on fertility. Despite a large literature on the effects of cash transfers on the fertility behaviour of adolescents in beneficiary households (e.g. Baird, McIntosh, and Özler (2011)), less is understood on the effects of CCTs on the fertility behaviour of adult mothers who are beneficiaries. Past studies provide mixed results which can be explained, in part, by differences in program design. On the one hand, studies of CCT programs with features that directly incentivize fertility outcomes (such as varying the transfer amount with the number of children or incentivizing births to take place in health facilities) find that these programs increase fertility (Morris et al. (2004); Stecklov et al. (2007); Nandi and Laxminarayan (2016); Garganta et al. (2017)). On the other hand, studies of CCT programs that don't vary transfer amounts with the number of children in beneficiary households, as it is the case for Peru's Juntos, find null effects on fertility (Stecklov et al. (2007); Feldman et al. (2009)).[3] In contrast, our study shows that CCT

3. Despite not showing or finding effects on actual fertility, four papers report promising effects on birth control use or birth spacing: Stecklov et al. (2007), Feldman et al. (2009), Todd, Winters, and Stecklov (2012), and Perova and Vakis (2012).

programs, with a transfer independent of the number of children, can lead to a strong reduction in family size. The difference between our results and the null effects found by other studies might be explained by our longer analysis window (i.e., up to 6 years after the introduction of the program), and the fact that the effects are not completely immediate, as we show.

Second, our study contributes to our understanding of the cumulative effects of cash transfers by estimating the dynamic effects of Juntos rather than the 'static' canonical effects. Estimating these dynamic effects allows us to understand whether the program has long-lasting and transformative effects. As Cahyadi et al. (2020) discuss, achieving intergenerational poverty reduction is a cumulative process and temporary investments may be of little benefit. Measuring the cumulative effects of cash transfers is challenging. It requires not only that we study longer term effects but do so in a setting where cash transfers have been offered regularly over time and where a control group exists for long enough to identify the cumulative effects. Like Cahyadi et al. (2020), we are able to investigate cumulative effects of a national, government-run, CCT program. While their study of Indonesia's CCT program finds strong cumulative effects on child health and education outcomes but limited long-term economic effects for households, we find cumulative effects on fertility. Given the role that lower fertility is believed to have in reducing poverty (Birdsall and Griffin (1988); Sinding (2009)), this result provides new evidence of potentially transformative and long-term effects of anti-poverty programs.

Third, we add to a scant literature showing that cash transfers can increase the use of modern contraceptives among adult recipients. A recent systematic review and meta-analysis by the World Bank (Neelsen et al. (2021)) studies programs that offer financial incentives to increase the utilization of modern contraceptives (and other reproductive and child health indicators). Based on their standards for study quality, they did not include any CCT study that reported effects on modern contraceptive use. Nonetheless, they report that performance-based financing initiatives led to a statistically significant effect size (2.4 p.p.) which is about half our the effect we find in our study (roughly 5 p.p.). Meanwhile, most studies of UCTs either don't find or don't report effects on modern contraceptive use (Rosenberg et al. (2015); Palermo et al. (2016); Amarante et al. (2016); Handa et al. (2018); Carneiro et al. (2022)). There are

only few studies of CCTs where the transfer is independent of the number of children and also look at modern contraceptive use. Stecklov et al. (2007) find no effect in Nicaragua, and a positive effect only among unmarried women in Mexico. In contrast, Feldman et al. (2009) find a positive effect on the use of modern methods in Mexico, but these dissipate over time, underscoring the importance of considering longer term and dynamic effects.[4]

Fourth, we add to our understanding of the channels through which CCTs operates. Our study is the first to explore potential mechanisms explaining the effects of a CCT on birth control use and fertility by looking at the impact of the program on fertility preferences, intra-household bargaining, and access to reproductive health information or care. We show that a likely channel is an increase in access to and utilization of family planning services. This is especially noteworthy as it shows that conditionalities operate in indirect ways. Programs that incentivize school attendance and child health can still influence women's well-being through improved sexual and reproductive health simply as a by-product of the conditionalities. Our results suggest that requiring mothers to attend health clinics for pre-natal and infant health likely led to increased access and utilization of family planning services at health clinics in districts where Juntos was rolled out, leading to a reduction in fertility. This is also consistent with studies of UCTs that typically don't find any evidence of reduced fertility (e.g. Rosenberg et al. (2015); Palermo et al. (2016); Amarante et al. (2016); Handa et al. (2018); Carneiro et al. (2022)).

Finally, we contribute to the literature on the empowerment effects of cash transfer programs by showing that Juntos can empower women to avoid unwanted births and reduce excess

4. Both Perova and Vakis (2012) and Alencastre Medrano and Del Pozo Loayza (2017) find that Juntos increased contraceptive use in general, without distinguishing between modern and traditional forms). Our study differs from theirs in several meaningful ways. Perova and Vakis (2012) do not focus on fertility and neither study fertility effects or explore mechanisms. For fertility outcomes Alencastre Medrano and Del Pozo Loayza (2017) only consider pregnancy at the time of the survey, which is both a low frequency event and insufficient to speak to overall fertility. They also study the effects over a shorter horizon (Perova and Vakis (2012) only the initial expansion between 2005 and 2009 and Alencastre Medrano and Del Pozo Loayza (2017) only until 2014). We consider a longer time horizon and investigate dynamic effects to up to 6 years after the introduction of the program. Finally, both papers rely on canonical difference-in-difference estimation ignoring the staggered treatment design, which the recent literature on difference-in-difference has identified as leading to considerable bias (e.g. Goodman-Bacon (2021)). In contrast, we apply event study (TWFE) methods which better account for staggered implementation and test for robustness using more recent estimators (e.g. Sun and Abraham (2021)).

fertility. Despite an increasing focus on the role that intra-household decision-making play in explaining CCTs results, we show that these programs can empower women through another channel: indirectly, via better access to family planning.[5]

# 2 Juntos

Juntos (or *el Programa Nacional de Apoyo Directo a los más Pobres*) began in 2005 and is currently operated by Peru's Ministry of Development and Social Inclusion (MIDIS) in a bid to reduce rural poverty. The program offers eligible households who meet the conditionalities 100 soles every month (US$56 PPP), which represents 15% of eligible households average spending (Perova and Vakis (2012)). The amount received is independent of the number of children eliminating a direct incentive to affect fertility for families who already have at least one child. The transfer is given to the female household head if she is present in the household, which can possibly increase her bargaining power within the household (Das, Do, and Özler (2005); Fiszbein and Schady (2009); Alcázar, Balarin, and Espinoza Iglesias (2016)).

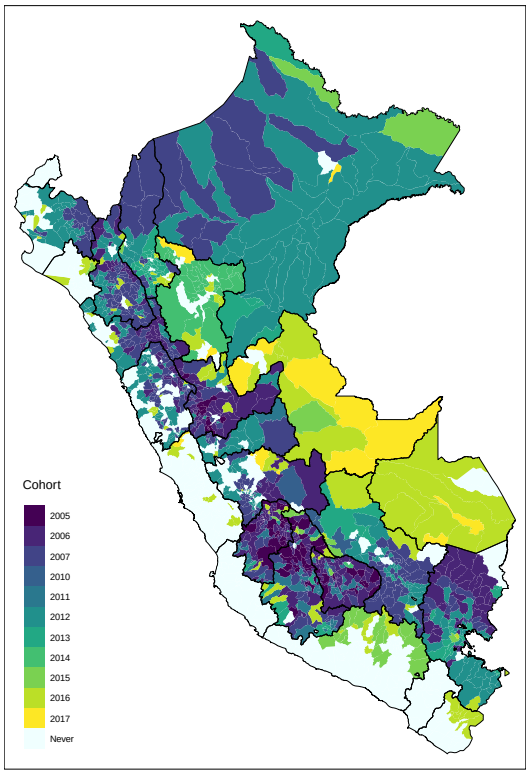
**Targeting** Eligibility is defined using a two-stage targeting system: first targeting districts and second targeting households within eligible districts. Districts were selected to participate in the program in several phases beginning in 2005. Figure 1 shows the evolution of the geographical targeting over time.[6] By 2017, the program had been rolled out to 1,305 out of a total of 1,896 districts in Peru. Considering that mostly rural districts are eligible under the targeting rules, this shows the scope of the regional coverage. Within these eligible districts, poor households with pregnant women or children under 14 were eligible for the transfer. According to Robles, Rubio, and Stampini (2019), using IDB data from 2013, Juntos covered 34.3% of all

5. See, for instance, Das, Do, and Özler (2005); Fiszbein and Schady (2009); Alcázar, Balarin, and Espinoza Iglesias (2016); Almås et al. (2018) for a discussion of the effects of CCTs on women's empowerment.

6. While the precise criteria for geographic targeting and data sets employed by the program implementation changed across the expansion periods, they generally all include the following components with some minor variation: the district poverty or extreme poverty rate (total poverty gap, proportion of households with unmet basic needs, percentage of households with chronic malnutrition) and the proportion of population centres ('centros poblados') in the district who were severely affected by violence. See Carpio et al. (2019) for more details on the different phases of the rollout of the program.

poor in Peru (56.2% in rural areas, 11.1% in urban areas).[7]

Figure 1: District level rollout by year 2005-2017




Source: authors' calculations from data on Juntos portal http://www.Juntos.gob.pe/infoJuntos/indexe.html, last accessed October 22 2018.

**Conditionalities** While the precise details and thresholds for the conditionalities have changed over the course of the program, the key conditions to receive the transfer can be summarized as follows. First, children between ages 6 and 14 must register and regularly attend school.[8] Second, children under the age of 5 must attend routine well-baby checks and must be up-to-date with child vaccination, and pregnant women must receive monthly pre-natal health checks

7. They calculate poverty using the national poverty line.

8. Schooling is compulsory in Peru until age 16. According to the national statistical agency, primary enrolment rates were quite high prior to the introduction of the program (over 90%, even in rural areas) but secondary school enrolment rates were only 57% in rural areas (INEI (2019)).

(Silva Huerta and Stampini (2018); Díaz and Saldarriaga (2019)). Finally, pregnant women and mothers must attend nutritional and reproductive health discussions (World Bank (2019)). Note that these conditionalities might not always have been met: according to an early evaluation, despite low take-up of some of the secondary health conditions such as full immunization and attendance at health discussions, only 5% of beneficiaries were rejected for failure to meet the conditions (Presidencia del Consejo de Ministros (2010)).

For more formal details on the Juntos program, see Alcázar (2009), Linares Garcia (2009), Molyneux and Thomson (2011), Escobal and Benites (2012), Díaz and Saldarriaga (2014), MIDIS (2016), Silva Huerta and Stampini (2018), Díaz and Saldarriaga (2019), World Bank (2019) and Carpio et al. (2019).

# 3 Data and Sample

## 3.1 Data

We use data from Peru's DHS (or ENDES: *Encuesta Demográfica y de Salud Familiar*) linked to district-level administrative data on Juntos' geographic rollout. Because of its emphasis on women's health and especially women's reproductive health, Peru's DHS is uniquely placed to allow us to investigate the impact that Juntos has had on matters around contraception (use and type) and fertility outcomes (number of children). In addition, the DHS allows us investigate some dimensions of fertility preferences (ideal family size, spousal discordance in fertility) and intra-household bargaining in this domain (who in the household is the main decision-maker in the use of contraception), which may shed light on mechanisms, as well as a large number of socio-economic variables we use as controls.[9] Contrary to other countries where the DHS was run every four years, the Statistical Agency for Peru (INEI) began running the DHS as a continuous annual survey in 2004. This yields a relatively rare high frequency of nationally representative repeated cross-sections for a single country, allowing us to exploit annual variation in the program rollout and to estimate the dynamic effects of the CCT. For

9. The continuous DHS for Peru did not collect data from the spouses/partners.

our analysis, we utilize the yearly cross-section DHS waves, pooled from 2004, the year prior to the introduction of Juntos, to 2017.

One limitation of the DHS data is that the self-reported Juntos receipt is only collected between 2009 and 2012 and only for women with a child aged 5 or less, which would lead to a small and restrictive sample.[10] Instead, to measure exposure to Juntos, we merge the DHS with administrative data on the Juntos rollout at the district-year level to identify districts that are targeted at any given point in time.[11] The main variable of interest, exposure to Juntos, is constructed as a binary variable equal to 1 if the respondent is in a targeted district at the time of the DHS interview and 0 otherwise. As we discuss in section 4, this allows us to estimate intent-to-treat effects rather than average treatment effects.

## 3.2 Sample Selection

**Selection of districts** We restrict our sample to women living in rural districts, excluding 57% of women in the DHS.[12] We impose this restriction since Juntos targets rural districts, and urban districts are not appropriate counterfactuals for targeted districts.[13] In fact, urban districts are typically wealthier with better access to family planning services and lower fertility rates than targeted districts, which can lead to a failure of the parallel trend assumption, a critical assumption of our empirical strategy.

Our sample is composed of rural districts regardless of whether they ever received Juntos: 78% of the districts in our sample were ever targeted by Juntos, 22% were not. For more details on the distribution of treated and untreated districts by urban/rural status, see Appendix Table A1.

10. In addition, we believe that this variable is measured with error, since more than half of those who claimed to have received the program were unable to produce their Juntos beneficiary card.

11. We obtained the administrative data on rollout from Juntos official website (last accessed October 22 2018): www.Juntos.gob.pe/infoJuntos/indexe.html.

12. We define urban districts as districts in which part of the population lives in a city and rural districts as the inverse.

13. 96% of the districts targeted by Juntos in 2017 are rural.

**Selection of women within districts** Since we are interested in the effects of Juntos on reproductive behaviours of *recipients* we make two further sample restrictions to mimic the eligiblity criteria. We restrict our sample to women with at least a child and to poor women. We define being poor as living in an household with a wealth index in the bottom 40%. Note that this measure of poverty is not a perfect indicator of eligibility but is rather use as a proxy for eligibility. We estimate that roughly 50% of women in our sample living in a targeted district actually receive Juntos. In addition, because we are interested in the relationship between Juntos, women's reproductive behaviours, and intra-household decision making, we restrict our attention to fecund and married/cohabitating women. See Appendix Table A2 for details on the number of women kept at each stage of the selection process.

Selecting our sample according to poverty, martial status or the presence of children may be problematic if Juntos affects these characteristics. To dispel concerns this might cause a sample selection bias, we show in the Appendix Table A3 that Juntos assignment to the respondent's district does not affect the probability that the respondent has at least one child, whether she is married or cohabitating with her partner, or whether she is poor or non-poor.

## 3.3 Variables of Interest & Descriptive Statistics

After restrictions we end up with a sample of 47,900 women. Appendix Tables A4, A5, A6 present the descriptive statistics for the main control and outcome variables, by treatment status. Socio-economically, the average woman in the analytic sample is just over 32 years old (with a partner 4 years older) and has 6.2 years of schooling (just one year less than her partner). Seventy five percent of respondents are employed, with agricultural self-employment being the most frequently reported occupation (50%) followed by sales (11%). About 57% of respondents have wealth positions placing them in the poorest quintile, and roughly 1/3 report formalizing their relationships into legal unions.

For main outcomes of interest, we consider the respondent's number of children to capture overall fertility at the intensive margin, and whether she is currently using any form of birth control (which we split between modern and traditional). The average respondent has approxi-

mately 3.4 children and 77% of women in our main sample use some form of birth control, with 40% using modern forms. We follow the DHS and WHO classification for 'modern method'.[14]

Finally, we also investigate potential mechanisms (Table A6). First, to capture fertility intentions (which we call fertility preferences) as main drivers of decision making in this domain (Pritchett (1994)) (Panel A), we consider the ideal number of children. The average respondent has an ideal family size of approximately 2.7 children, pointing to an average excess fertility of approximately 0.7 children. In fact, Panel D shows that 43% of respondents have excess fertility and 24% have exactly met their fertility intentions. We also consider spousal discordance in fertility preferences (inspired by Ashraf, Field, and Lee (2014)). We define spousal discordance as the case when the respondent reports a different ideal family size than her partner. We categorize households into three types: both the respondent and her spouse have the same preferences over family size (no discordance, accounting for 64% of the sample), respondent wanting more children than her spouse (12%), and respondent wanting fewer children than her spouse (18%).

Second, we capture intra-household decision-making (Panel B) by considering the respondents' answers to a question about who makes the decision about contraceptive use and create the following three variables (conditional on using any birth control): whether the respondent makes the decision (herself at 13% or jointly with her partner at 78%), someone else (her partner at 6%), and whether she conceals the use of contraceptives from her partner (2%). We also consider their answers to the question whether husbands' oppose the use of birth control (a mere 0.5%).

Third, to capture information about access to family planning services, we consider their answer to whether they discussed family planning at a health care facility during the last 12 months (only 23% did), and whether they do not use birth control because of lack of knowledge or access (3%).

14. This includes female sterilization, male sterilization, pill, DIU, injection, implants or Norplant, condom, foams and jellies, and amenorrhea. (http://www.who.int/news-room/fact-sheets/detail/family-planning-contraception and https://dhsprogram.com/data/Guide-to-DHS-Statistics/Current_Use_of_Contraceptive_Methods.htm). Meanwhile, traditional methods include periodic abstinence and withdrawal.

# 4 Empirical Strategy

**Econometric Model** To estimate the effect of Juntos on women's reproductive health choices and outcomes, we conduct an event-study analysis that allows for time varying treatment effects (as in, for instance, Hoynes, Schanzenbach, and Almond (2016), Chetty, Friedman, and Saez (2013), Greenstone and Hanna (2014), Christian, Hensel, and Roth (2019)). Specifically, we estimate the following semi-dynamic model:

$$y_{idt} = \lambda_t + \delta_d + \sum_{\tau=0}^{5} \mu_{d\tau} \mathbf{1}\{\tau = t - E_d\} + \mu_{6+} \mathbf{1}\{t - E_d \geq 6\} + \beta X_{idt} + \epsilon_{idt}, \quad (1)$$

where $y_{idt}$ is the outcome of interest for respondent $i$ in district $d$ at time $t$, $E_d$ is equal to the first year Juntos was rolled out in the district where woman $i$ resides, $\lambda_t$ is a set of dummies to control for year specific effects, $\delta_d$ are dummies for district fixed effects, and $X_{idt}$ are respondent $i$'s socio-economic characteristics in district $d$ at time $t$ (age, age squared, years of schooling, occupation, marital status, husband/partner years of schooling and education, wealth index). Distant lags are binned from 6 to 12 to increase the sample size. And $\mu_\tau$ capture the dynamic treatment effects of Juntos. For sake of simplicity, we also report – and we sometimes only report – the average effect computed from the estimated coefficients from lags t1-t6.[15] It is important to note that we are identifying treatment at the district-level, and not at the respondent-level, such that we are capturing an intent-to-treat effect rather than an average treatment effect.[16]

**Identification** The identification of the treatment effects $\mu_\tau$ comes from the comparison of the districts after the introduction of Juntos with the districts prior to the program and with the districts that never received the program (pure control).

The estimated $\mu_\tau$ are unbiased under three assumptions (Sun and Abraham (2021)): (1) parallel trends in baseline outcomes, (2) no anticipatory behaviour prior to treatment, and (3) treatment effect homogeneity across cohorts.

15. An alternative specification is to run a canonical Difference-in-Difference model, which is now known to be biased when the treatment effect varies over the analytic period (Goodman-Bacon (2021)). Taking the average over the linear effects estimated using the semi-dynamic model avoids these concerns.

16. See section 3.1.

We investigate assumptions (1) and (2) using the following fully dynamic specification with leads and lags:

$$y_{idt} = \lambda_t + \delta_d + \sum_{\substack{\tau \geq -5 \\ \tau \neq -1}}^{5} + \mu_{d\tau} \mathbf{1}\{t - E_d = \tau\} + \mu_{6-} \mathbf{1}\{t - E_d \leq -6\} \mu_{6+} \mathbf{1}\{t - E_d \geq 6\} + \beta X_{idt} + \epsilon_{idt}, \quad (2)$$

where $\mu_\tau$ with $\tau < 0$ are pre-trend coefficients and $\mu_\tau$ with $\tau \geq 0$ capture the dynamic treatment effects estimated above. We normalize $\tau = -1$ to follow the common practice in the literature, and bin distant leads from -13 to -6 to increase the sample size. Using the fully-dynamic specification, we investigate the plausibility of the parallel trend assumption (and the no anticipatory behaviour assumption) by visually inspecting whether the lead coefficients ($\mu_\tau$ with $\tau < 0$) follow a trend prior to Juntos. We also formally test the absence of pre-trend by testing the joint significance of all lead coefficients ($F$-test), and by testing the significance of the average lead effect. Both the visual inspection and the formal tests suggest that the fertility behaviours and outcomes of women in treated districts evolve, in the absence of Juntos, similarly to the ones of women in non-treated districts.

With regards to Assumption (3), we have no a priori reason to believe that women treated in later years would respond differently to women treated in previous years. Nonetheless, we conduct a robustness check by estimating the event study model using the methodology developed by Sun and Abraham (2021) that is robust to treatment effect heterogeneity.[17] Our results are qualitatively and quantitatively similar, suggesting that heterogeneity in treatment effects across cohorts is likely to be minimal.

We further assess the validity of our methodology by conducting falsification tests on non-poor women who are not eligible for the transfer. We find no effect of Juntos on these women.

17. We use the Stata package *eventstudyinteract* by Sun and Abraham (2021).

# 5 Results

## 5.1 Impact on Reproductive Behaviour and Outcomes

Table 1 reports the effects of Juntos on the number of children women have (column 1) and on modern birth control use (column 2). In addition to the estimated effects from the semi-dynamic model, Table 1 also reports the average effect computed from the estimated coefficients from lags t1-t6.

Two main results emerge from the table. First, we find that Juntos led to a decline in the number of children women have. Since we did not find an effect of Juntos on "has at least one child" (Appendix Table A3) and restrict the sample to women with at least one child, the effect is purely driven by a reduction in fertility at the intensive margin. Second, we find that Juntos increased modern birth control use by an average of 5 percentage points above the mean of 40%. In Appendix Table A7, we show that this effect is driven by a corresponding decrease in reliance on traditional methods of birth control, as opposed to an increase in overall birth control use. This result is important given the relative importance of traditional forms of birth control in Peru compared to other countries in the region (Ponce de Leon et al. (2019)).

The dynamics of treatment effects over time are noteworthy for two reasons. First, the two effects only appear a year or two after Juntos is implemented in a district. This is consistent with the notion that changing women's reproductive behaviour takes time (given the time from conception to birth, the time to understand the implications of the program in terms of both the benefits and conditionalities). In this regard, evaluations of CCTs that only look at immediate effects may miss changes in fertility and contraceptive use that may be most visible over time. Second, we find that the effects are persistent and possibly stronger over time, speaking to the long-term effects of conditional cash transfers. As Cahyadi et al. (2020) note, whether CCT programs continue to be effective beyond the 'static' effect of increasing compliance with incentivized behaviours on those entering the program is unclear.[18] Our results adds useful

18. There are a number of reasons to expect that the 'static' effects of the program may change over time. Interventions may become less effective when implemented by the government at scale than in a smaller pilot stage (e.g., Bold et al. (2018)). Treatment effects could weaken over time as people's initial excitement of being in the program fades, or once beneficiaries learn that the conditions of the CCT were not always perfectly enforced

evidence because we show long-term dynamic effects on behaviours not explicitly incentivized by the CCT, suggesting that the program has had deeper, more structural, impacts than would be implied by intended effect on conditioned outcomes (schooling and child health).

Table 1: Main Results: Effect of Juntos on Total Number of Children and Birth Control Use, Semi-Dynamic Model (OLS)

| | (1) | (2) |
|---|---|---|
| VARIABLES | Total number of children | Use of modern contraceptives |
| *Mean of the dep. var.* | 3.40 | 0.40 |
| Year Juntos implemented | -0.033 | 0.014 |
| | (0.058) | (0.019) |
| 1 year later | -0.083 | 0.019 |
| | (0.065) | (0.019) |
| 2 years later | -0.149** | 0.048** |
| | (0.063) | (0.020) |
| 3 years later | -0.089 | 0.066*** |
| | (0.063) | (0.020) |
| 4 years later | -0.125* | 0.072*** |
| | (0.066) | (0.022) |
| 5 years later | -0.141* | 0.063*** |
| | (0.076) | (0.023) |
| 6 or more years later | -0.201** | 0.040 |
| | (0.086) | (0.026) |
| Observations | 47,900 | 47,900 |
| R-squared | 0.566 | 0.135 |
| Average effect | -0.131** | 0.051*** |
| | (0.057) | (0.018) |

*Notes:* Sample is all poor, married, and fecund women with at least one child in rural districts. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

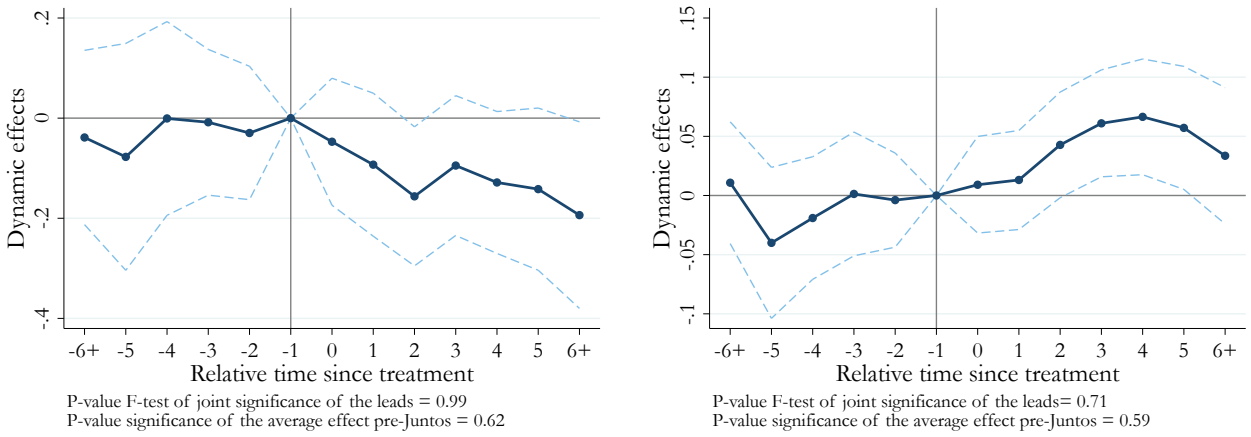
by the government. Furthermore, inflation and improvements in economic conditions could make nominal level of benefit payments less effective in changing household decision making.

## 5.2 Robustness Checks

**Common Trends** Our identification strategy relies on the assumption that, in the absence of Juntos, women in treated districts would evolve similarly than women in non-treated districts. We test the plausibility of this assumption by looking at the differential evolution of women in treated and non-treated districts prior to Juntos implementation, using the fully dynamic specification in equation (2). Results are presented in Figure 2 - Panel (a) for number of children and Panel (b) for the use of modern form of birth control.

Both the visual inspection of the two figures and the formal tests of the existence of parallel trends prior to Juntos confirm the plausibility of the common trend assumption. Indeed, the visual inspection indicate that the difference in outcomes between treated and non-treated districts is stable over time as we can see from the flat line pre-Juntos. Moreover, the joint test of significance of the leads ($\mu_\tau$ with $\tau < 0$) and the test of significance of the average effect pre-Juntos fail to reject the null of statistical insignificance for both outcomes.

Figure 2: Fully Dynamic Event Study Specification




(a) Fertility: Number of children

(b) Birth control use: Modern methods

*Notes:* Effects from fully-dynamic models. Point estimates together with the associated 95% confidence interval are reported. Sample is all poor, married, and fecund women with at least one child in rural districts (N=47,900). All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

**Sun and Abraham Estimator** We also check for the robustness of our results to the new methodologies which allow for heterogeneous treatment effects across treated cohorts. In particular, we use the estimator developed by Sun and Abraham (2021). The estimates which are presented in Table 2 are both quantitatively and qualitatively similar to our main estimates reported in Table 1.

Table 2: Robustness: TWFE versus Sun and Abraham (OLS)

| VARIABLES | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Total number of children | | Use of modern contraceptives | |
| | TWFE | SA | TWFE | SA |
| Year Juntos implemented | -0.033 | -0.046 | 0.014 | 0.013 |
| | (0.058) | (0.049) | (0.019) | (0.015) |
| 1 year later | -0.083 | -0.127*** | 0.019 | 0.014 |
| | (0.065) | (0.049) | (0.019) | (0.015) |
| 2 years later | -0.149** | -0.162*** | 0.048** | 0.031* |
| | (0.063) | (0.055) | (0.020) | (0.017) |
| 3 years later | -0.089 | -0.089 | 0.066*** | 0.057*** |
| | (0.063) | (0.055) | (0.020) | (0.017) |
| 4 years later | -0.125* | -0.117** | 0.072*** | 0.069*** |
| | (0.066) | (0.058) | (0.022) | (0.018) |
| 5 years later | -0.141* | -0.135** | 0.063*** | 0.054*** |
| | (0.076) | (0.064) | (0.023) | (0.020) |
| 6 or more years later | -0.201** | -0.195** | 0.040 | 0.033 |
| | (0.086) | (0.082) | (0.026) | (0.026) |
| Observations | 47,900 | 47,889 | 47,900 | 47,889 |
| R-squared | 0.566 | 0.557 | 0.135 | 0.136 |

*Notes:* Sample is all poor, married, and fecund women with at least one child in rural districts. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

**Falsification test** We finally test our identification strategy by running a falsification test on non-poor women (those in the top three wealth quintiles). Since non-poor women are unlikely to meet the eligibility criteria for Juntos, we should not find statistically significant results.

Instead, statistically significant results would indicate a possible failure of the common trend assumption. Note that DHS wealth index may not correspond one to one with wealth and income data used to determine eligibility so we cannot exclude that a small fraction of the non-poor women used for the falsification test receive Juntos.[19] Nonetheless, we expect to see muted effects on these women relative to our main sample composed of the poorest households. Table 3 shows the estimated effect of Juntos on the main outcomes (number of children and modern contraceptive use) for the non-poor women. With one exception which we take as spurious (1 year later for column (2)), all the estimates among the non-poor sample are statistically insignificant at the 10% level and generally smaller in magnitude.

## 5.3 Mechanisms

We now explore potential mechanisms through which Juntos may lead to a reduction in fertility (as measured by the number of children) and the uptake in modern forms of birth control. We consider, in turn, three potential mechanisms: household preferences over the number of children, intra-household bargaining, and improved access to family planning services.

**Preferences** According to textbook microeconomic analysis of fertility, a increase in non-labour income associated with the cash transfer would lead to an increase or decrease in desired fertility depending on whether the decision-maker views children as normal or inferior goods, respectively. Alternatively, Becker's quality-quantity tradeoff would imply decreased desired fertility with an increase in non-labour income (Becker (1960)). In other words, since the direction of the net effect of the cash transfer on desired fertility is ambiguous, it is worth investigating whether desired fertility (measured by the ideal number of children) is driving the results found in Table 1. Table 4 suggests that Juntos neither affects respondents' ideal number of children nor their partner's. There is thus little evidence that desired fertility has reacted to the introduction of Juntos. In addition to shedding light on potential mechanisms, this

19. To get an idea of how large this fraction can be, we estimate the fraction of women receving Juntos by wealth quintile using the 2005-2007 DHS waves in which women with children under 5 were asked whether or not they receive Juntos. Among these women in the three richest wealth quintiles, about 10% declared receiving the transfer. Meanwhile, 55% of the lowest wealth quintile and 38% of the second poorest reported receiving it.

Table 3: Placebo: Effects on Non-Poor Women (OLS)

| VARIABLES | (1) Total number of children | (2) Use of modern contraceptives |
|---|---|---|
| *Mean of the dep. var.* | 1.78 | 0.37 |
| Year Juntos implemented | -0.083 | -0.013 |
| | (0.065) | (0.027) |
| 1 year later | -0.079 | -0.053** |
| | (0.072) | (0.026) |
| 2 years later | -0.038 | -0.009 |
| | (0.063) | (0.027) |
| 3 years later | -0.028 | 0.019 |
| | (0.060) | (0.029) |
| 4 years later | -0.015 | -0.011 |
| | (0.077) | (0.027) |
| 5 years later | -0.023 | 0.030 |
| | (0.071) | (0.034) |
| 6 or more years later | 0.053 | 0.035 |
| | (0.089) | (0.034) |
| Observations | 29,086 | 29,086 |
| R-squared | 0.649 | 0.233 |
| Average effect | -0.021 | 0.002 |
| | (0.047) | (0.020) |

*Notes:* Sample is all non-poor women in rural districts. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

result should also alleviate concerns that the conditional cash transfer has unintended effects by incentivizing increased child bearing among recipients.

Rather than changing preferences, Table 5 shows that Juntos led to a reduction in excess fertility, measured as the difference between actual and ideal number of children. We see that the program decreased the fraction of women who report a greater number of children than their ideal (column (1)) and increased the fraction of women who report having exactly here

Table 4: Mechanisms: Preferences (OLS)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | | Husband's preferences | | |
| VARIABLES | Ideal number of children | Wants fewer | Wants same | Wants more |
| *Mean of the dep. var.* | 2.66 | 0.12 | 0.64 | 0.18 |
| Average effect | -0.034 | -0.009 | 0.005 | 0.006 |
| | (0.055) | (0.011) | (0.017) | (0.013) |
| Observations | 47,700 | 47,900 | 47,900 | 47,900 |
| R-squared | 0.153 | 0.053 | 0.053 | 0.051 |

*Notes:* Sample is all poor, married, and fecund women with at least one child in rural districts. Average effects are obtained from semi-dynamic models. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

desired number of children (column (2)).

**Intra-Household Decision-Making** What are the drivers of the alignment between women's preferences and actual fertility? One possible explanation is that, since the transfer is given to the female household head, it can increase her bargaining power within the household. If women excess fertility is caused by husbands wanting more children and having more bargaining power, we would expect, following Juntos, a decrease in fertility and an alignment between women's preferences and actual fertility.

We investigate this hypothesis in Table 6. Columns (1) to (3) report the effect of Juntos on whether the respondent, her spouse or both jointly is the main decision-maker in the use of birth control, a common measure of women's autonomy and empowerment in the fertility domain. Similarly, column (4) considers whether the respondent is concealing contraceptive use from her partner, which can be considered a substitute for empowerment.[20] Columns (1) to (4)

20. Concealed use of birth control suggests prevalence of moral hazard related concerns in intra-household decision making (Ashraf, Field, and Lee (2014)). There exists evidence of women concealing their actions from their spouse in the context of intrahousehold bargaining (e.g. Fiala and He (2017) and Chang et al. (2020)).

Table 5: Mechanisms: Excess Fertility (OLS)

| | (1) | (2) | (3) |
|---|---|---|---|
| | Number of children | | |
| VARIABLES | $>$ ideal number | $=$ ideal number | $<$ ideal number |
| *Mean of the dep. var.* | 0.43 | 0.24 | 0.33 |
| Average effect | -0.030* | 0.030** | 0.000 |
| | (0.016) | (0.014) | (0.015) |
| Observations | 47,700 | 47,700 | 47,700 |
| R-squared | 0.294 | 0.051 | 0.257 |

*Notes:* Sample is all poor, married, and fecund women with at least one child in rural districts. Average effects are obtained from semi-dynamic models. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

present the results run only on the subsample reporting contraceptive use. We show in Table A7 that the fraction of women using birth control – whether a traditional or modern form – is not affected by Juntos, such that the sample restriction should not lead to a selection bias. Finally, we consider in column (5) whether women report not using birth control because of their partners' opposition to using contraception. We find no evidence that the program led to a change in intra-household decision making in the fertility domain.[21] In fact, the effects on all five variables are not significant and close to zero.

Table 6: Mechanisms: Intra-household decision making (OLS)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Who is the decision-maker for using birth control? | | | | Not using birth control |
| VARIABLES | Husband/ partner | Woman | Joint decision | Hidden use | because husband/ partner opposed |
| *Mean of the dep. var.* | 0.06 | 0.13 | 0.79 | 0.02 | 0.01 |
| Average effect | 0.011 | -0.010 | -0.006 | 0.007 | 0.000 |
| | (0.011) | (0.014) | (0.017) | (0.006) | (0.002) |
| Observations | 36,014 | 36,014 | 36,014 | 36,014 | 43,842 |
| R-squared | 0.078 | 0.087 | 0.097 | 0.056 | 0.035 |

*Notes:* Sample is all poor, married, and fecund women with at least one child in rural districts. Columns (1)–(4) are restricted to women using birth control. Column (5) is unconditional on using. Average effects are obtained from semi-dynamic models. All regressions include district and year fixed effects, individual characteristics, and DHS weights. The questions on decision-making and reason why not using birth control were not asked in 2004 and 2009, respectively. Standard errors are clustered at the district level. ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

**Birth control knowledge and access** Finally, we consider whether the health conditionalities offer a mechanism through which Juntos may affect fertility decisions. Since Juntos

See also OlaOlorun, Anglewicz, and Moreau (2020) for the relationship between concealing birth control and women's empowerment.

21. While Alcázar, Balarin, and Espinoza Iglesias (2016) did find that Juntos led to an increased in women's empowerment, the authors defined empowerment in other domains: decision-making over resources, freedom of movement, gender ideology and gender based violence, and perception of life, self esteem, and agency. They do not consider decision-making in the fertility domain.

requires recipient mothers to regularly attend health centres if they are pregnant or with their young children (under the age of 5), it is possible that beneficiaries were exposed to greater reproductive health information. We test this channel in Table 7 by estimating the effect of Juntos on whether the respondent discussed family planning at a health facility during the last 12 months (column (1)) and on whether women do not use birth control because of lack of knowledge or access, because it costs too much or because of the fear of side effects (column (2)). We find that Juntos is associated with an increase in family planning discussions at health facilities and reduces the probability that women cite lack of knowledge or access, cost, or fear of side-effects as reasons for non-use. Access to reproductive health services, which may be a by-product of the health conditionalities, is therefore a likely channel through which Juntos affects women's reproductive behaviour and outcomes.

Table 7: Mechanisms: Birth Control Knowledge and Access (OLS)

| | (1) | (2) |
|---|---|---|
| VARIABLES | Discussed family planning at a health facility last year | Does not use BC: lack of knowledge or access |
| *Mean of the dep. var.* | 0.23 | 0.03 |
| Average effect | 0.031** | -0.013* |
| | (0.014) | (0.007) |
| Observations | 47,900 | 43,842 |
| R-squared | 0.104 | 0.054 |

*Notes:* Sample is all poor, married, and fecund women with at least one child in rural districts. Sample is smaller in column (2) because the question about the reasons why not using birth control was not asked in 2009. Average effects are obtained from semi-dynamic models. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district level rollout of Juntos.

# 6 Conclusion

Using Peru's Juntos program, we investigate the fertility and reproductive health outcomes effects of cash transfers that are conditional on child school attendance and prenatal and infant health checks. We find that eligible women in targeted districts tend to have fewer children for at least 6 years after the program was rolled in, suggesting strong long-term effects at the intensive margin. The decrease in fertility seems to be driven by an increase in the take-up of modern forms of birth control. This last result is of particular importance for sexual and reproductive health and rights given a disproportionate reliance on traditional methods in Peru (Ponce de Leon et al. (2019)), methods that are neither effective nor necessarily safe.

We test our identification strategy in two ways. We first investigate the plausibility of the parallel trend assumption by inspecting the trends between treated and non-treated districts prior Juntos implementation and by running falsification tests on an alternative sample of non-eligible women. We then check that our results are robust to new techniques for estimating event studies in the presence of heterogeneous treatment effects across cohorts. All robustness checks suggest that the effects we capture are likely to be causal.

The persistence of the effects on fertility and reproductive health outcomes has policy relevant importance for two reasons. First, since these are outcomes that are not explicitly targeted by the program conditionalities, our findings add to the case that evaluating anti-poverty programs ought to consider broader sets of outcomes than those directly targeted by program designers. Second, our results provide encouraging insights against the concern that dynamic effects of social protection programs may wane as beneficiaries' excitement about the program fades, compliance with conditionalities become less strictly enforced, and interventions become less effective when implemented by governments at scale compared to smaller pilot programs run by NGOs (Bold et al. (2018); Cahyadi et al. (2020)). Indeed, the long-term, dynamic, effects of a large scale cash transfer program on women's reproductive outcomes suggests potentially transformative effects on the lives of beneficiary families, given the long term expected benefits of reduced fertility (Birdsall and Griffin (1988); Sinding (2009)).

We also unpack the mechanisms underlying our main results. We test for three possible

channels of causality that might drive the effect of Juntos on women's reproductive decisions: fertility preferences, intra-household decision-making, and access to reproductive health services. We find that fertility preferences remain stable following Juntos and that women are more able to reach and not surpass their ideal family size. We do not find any evidence that this alignment between women's preferences and actual fertility is driven by a change in intra-houshold decision making in the fertility domain. Rather, we find that access to reproductive health services, which may be a by-product of the health conditionalities, did seem to improve after the introduction of Juntos in respondents' districts. This suggests that the conditions in CCTs matter: by encouraging attendance at health clinics for infants and pregnant women, the program may empower women to increase use of modern birth control methods and thus reduce excess fertility and unwanted births. Future research is needed to evaluate the dynamic fertility effects of other CCT programs (where transfer amounts are independent of the number of children) to verify the external validity of our findings beyond Peru.

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Table A1: District-level targeting and urban/rural status

| | # of districts ever treated | # of districts never treated | Total |
|---|---|---|---|
| # of urban districts | 41 | 161 | 202 |
| | (38,181) | (122,251) | (160,432) |
| # of rural districts | 951 | 274 | 1,227 |
| | (91,919) | (27,897) | (119,816) |
| Total | 992 | 435 | 1,427 |
| | (130,100) | (150,148) | (280,248) |

*Notes:* There are 1,838 districts in Peru. Districts that are not in our sample were either not sampled at all between 2004 and 2017, or were affected by a border change during the 2004-2017 period (4 districts). We define urban districts as districts in which part of the population lives in a city and rural districts as the inverse. The 41 urban districts that are targeted by Juntos belongs to the urban areas of Huánuco, Moquegua, Piura, and Pucallpa. The number of women in the 2004–2014 DHS living in each district type is given in parentheses.

Table A2: Sample selection

| | Sample size |
|---|---|
| Initial sample (2004–2014 DHS) | 280,248 |
| *Restrictions to women:* | |
| Living in rural districts | 119,816 |
| + poor (wealth index in the bottom 40%) | 90,730 |
| + with at least one child | 72,052 |
| + not fecund, menopausal or sterilized | 57,062 |
| + married or cohabiting | 47,900 |

*Notes:* The table shows the number of women that are kept in our sample at each step of the selection process.

Table A3: Sample selection: selection bias

| VARIABLES | (1) At least one child | (2) Married / Cohabit. | (3) Poor |
|---|---|---|---|
| *Mean of the dep. var.* | 0.75 | 0.66 | 0.74 |
| Average effect | 0.009 | 0.007 | -0.018 |
| | (0.007) | (0.009) | (0.015) |
| Observations | 119,816 | 119,816 | 119,816 |
| R-squared | 0.517 | 0.324 | 0.504 |

*Notes:* Sample is all women in rural districts. Average effects are obtained from semi-dynamic models. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district

Table A4: Descriptive Statistics – Covariates

| | All sample | Never treated districts | Ever treated districts | | |
|---|---|---|---|---|---|
| | | | All | Pre-Juntos | Post-Juntos |
| A – Age | | | | | |
| Respondent | 32.39 | 32.17 | 32.43 | 32.11 | 32.53 |
| | (8.06) | (7.91) | (8.08) | (8.06) | (8.09) |
| Partner/husband | 36.48 | 36.49 | 36.48 | 36.48 | 36.48 |
| | (9.21) | (9.38) | (9.19) | (9.25) | (9.17) |
| B – Years of education | | | | | |
| Respondent | 6.18 | 7.68 | 5.97 | 5.97 | 5.97 |
| | (3.71) | (3.76) | (3.66) | (3.46) | (3.72) |
| Partner/husband | 7.50 | 8.47 | 7.36 | 7.30 | 7.38 |
| | (3.40) | (3.25) | (3.40) | (3.33) | (3.42) |
| C – Occupation | | | | | |
| Agriculture | 0.50 | 0.37 | 0.52 | 0.54 | 0.52 |
| | (0.50) | (0.48) | (0.50) | (0.50) | (0.50) |
| Not working | 0.25 | 0.32 | 0.24 | 0.22 | 0.25 |
| | (0.44) | (0.46) | (0.43) | (0.41) | (0.43) |
| Sales | 0.11 | 0.13 | 0.11 | 0.10 | 0.11 |
| | (0.31) | (0.34) | (0.31) | (0.30) | (0.31) |
| Manual | 0.04 | 0.05 | 0.04 | 0.04 | 0.04 |
| | (0.19) | (0.22) | (0.19) | (0.20) | (0.19) |
| Services (incl. domestic) | 0.03 | 0.07 | 0.03 | 0.03 | 0.03 |
| | (0.18) | (0.25) | (0.17) | (0.16) | (0.17) |
| Professional, technical, & managerial | 0.02 | 0.02 | 0.02 | 0.01 | 0.02 |
| | (0.14) | (0.14) | (0.14) | (0.12) | (0.14) |
| Unknown | 0.03 | 0.04 | 0.03 | 0.05 | 0.03 |
| | (0.18) | (0.19) | (0.18) | (0.22) | (0.17) |
| D – Marital status | | | | | |
| Married (vs. cohabiting) | 0.35 | 0.31 | 0.36 | 0.36 | 0.36 |
| | (0.48) | (0.46) | (0.48) | (0.48) | (0.48) |
| E – Poverty level | | | | | |
| 1st wealth quintile | 0.57 | 0.27 | 0.62 | 0.48 | 0.66 |
| | (0.49) | (0.44) | (0.49) | (0.50) | (0.47) |
| 2nd wealth quintile | 0.43 | 0.73 | 0.38 | 0.52 | 0.34 |
| | (0.49) | (0.44) | (0.49) | (0.50) | (0.47) |
| Sample size | 47,900 | 6,110 | 41,790 | 10,703 | 31,087 |

*Notes:* Mean and standard deviation (in parentheses) for the covariates. Missing values are imputed with the variable mean.

Table A5: Descriptive Statistics – Main Outcomes

| | All sample | Never treated districts | Ever treated districts | | | Sample size |
|---|---|---|---|---|---|---|
| | | | All | Pre-Juntos | Post-Juntos | |
| Number of children | 3.40 | 2.90 | 3.47 | 3.56 | 3.45 | 47,900 |
| | (2.19) | (1.81) | (2.23) | (2.25) | (2.23) | |
| Uses a modern form of birth control | 0.40 | 0.50 | 0.39 | 0.36 | 0.39 | 47,900 |
| | (0.49) | (0.50) | (0.49) | (0.48) | (0.49) | |
| Uses a traditional form of birth control | 0.37 | 0.29 | 0.38 | 0.38 | 0.38 | 47,900 |
| | (0.48) | (0.46) | (0.48) | (0.49) | (0.48) | |
| Does not use birth control | 0.23 | 0.21 | 0.24 | 0.26 | 0.23 | 47,900 |
| | (0.42) | (0.40) | (0.43) | (0.44) | (0.42) | |

*Notes:* Mean and standard deviation (in parentheses) for the main outcomes of interest.

Table A6: Descriptive Statistics – Mechanisms

| | All sample | Never treated districts | Ever treated districts | | | Sample size |
|---|---|---|---|---|---|---|
| | | | All | Pre-Juntos | Post-Juntos | |
| A – Fertility preferences | | | | | | |
| Ideal family size | 2.66 | 2.56 | 2.68 | 2.68 | 2.67 | 47,700 |
| | (1.37) | (1.28) | (1.38) | (1.41) | (1.37) | |
| Husband wants fewer children | 0.12 | 0.11 | 0.13 | 0.12 | 0.13 | 47,900 |
| | (0.33) | (0.31) | (0.33) | (0.33) | (0.33) | |
| Husband wants the same nbr. | 0.64 | 0.62 | 0.64 | 0.64 | 0.64 | 47,900 |
| | (0.48) | (0.49) | (0.48) | (0.48) | (0.48) | |
| Husband wants more children | 0.18 | 0.23 | 0.18 | 0.18 | 0.17 | 47,900 |
| | (0.39) | (0.42) | (0.38) | (0.39) | (0.38) | |
| B – Decision-maker for using birth control (cond. on using) | | | | | | |
| Husband/partner | 0.06 | 0.06 | 0.06 | 0.07 | 0.05 | 36,014 |
| | (0.23) | (0.24) | (0.23) | (0.25) | (0.23) | |
| Woman | 0.13 | 0.17 | 0.13 | 0.14 | 0.12 | 36,014 |
| | (0.34) | (0.38) | (0.33) | (0.34) | (0.33) | |
| Joint decision | 0.78 | 0.74 | 0.79 | 0.77 | 0.80 | 36,014 |
| | (0.41) | (0.44) | (0.41) | (0.42) | (0.40) | |
| Conceals use | 0.02 | 0.02 | 0.02 | 0.02 | 0.02 | 36,014 |
| | (0.15) | (0.13) | (0.15) | (0.14) | (0.15) | |
| Husband's opposition to use | 0.005 | 0.005 | 0.005 | 0.005 | 0.005 | 43,842 |
| (unconditional) | (0.14) | (0.16) | (0.14) | (0.14) | (0.14) | |
| C – Birth Control Knowledge and Access | | | | | | |
| Discussed family planning | 0.23 | 0.21 | 0.23 | 0.16 | 0.25 | 47,900 |
| | (0.42) | (0.41) | (0.42) | (0.37) | (0.43) | |
| Not using: lack of know. or access | 0.03 | 0.02 | 0.03 | 0.02 | 0.04 | 43,842 |
| | (0.17) | (0.15) | (0.17) | (0.19) | (0.16) | |
| D – Excess fertility | | | | | | |
| Nbr. of children > ideal nbr. | 0.43 | 0.37 | 0.44 | 0.46 | 0.43 | 47,700 |
| | (0.49) | (0.48) | (0.50) | (0.50) | (0.50) | |
| Nbr. of children = ideal nbr. | 0.24 | 0.25 | 0.24 | 0.23 | 0.24 | 47,700 |
| | (0.43) | (0.44) | (0.42) | (0.42) | (0.43) | |
| Nbr. of children < ideal nbr. | 0.33 | 0.38 | 0.33 | 0.31 | 0.33 | 47,700 |
| | (0.47) | (0.49) | (0.47) | (0.46) | (0.47) | |

*Notes:* Mean and standard deviation (in parentheses) for the outcomes of the mechanism section. Variations in sample size arise for three reasons. First, the outcomes on decision-making are conditional on using and not using birth control. Second, 200 women reported non numerical value for ideal family size which explains the slightly smaller sample size for this outcome and the excess fertility outcomes. Finally, the questions on decision-making and reason why not using birth control were not asked in 2004 and 2009, respectively.

Table A7: Not using versus using a traditional form (OLS)

| VARIABLES | (1) Not using any form | (2) Use of a traditional form |
|---|---|---|
| *Mean of the dep. var.* | 0.23 | 0.37 |
| Average effect | -0.017 | -0.035** |
| | (0.014) | (0.017) |
| Observations | 47,900 | 47,900 |
| R-squared | 0.069 | 0.141 |

*Notes:* Sample is all poor, married, and fecund women with at least one child in rural districts. Average effects are obtained from semi-dynamic models. All regressions include district and year fixed effects, individual characteristics, and DHS weights. Standard errors are clustered at the district level. ***, **, * are 1%, 5% and 10% respectively. Data are from the 2004-2017 DHS waves for Peru and the administrative data on the district