

Conditional Cash Transfers, Women's Empowerment and Reproductive Choices

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Conditional Cash Transfers, Women's Empowerment and Reproductive Choices

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Abstract:

We investigate whether Peru's Conditional Cash Transfer (Juntos) program influence beneficiaries' reproductive choices. We find that contraceptive use increased among women exposed to the program for two years or more. Furthermore, Juntos increased modern methods use by 4 percentage points relative to traditional methods. An event study reveals persistent effects over time. Exploring potential mechanisms, we consider spousal discordance in ideal family size. While we find no differential effect on contraceptive use across discordance status, women preferring fewer children than their spouse are more likely to conceal contraceptive use, highlighting limits of cash transfers in empowering women in this domain.

Keywords: Cash Transfers; Fertility; Intra-household Allocation; Bargaining Power; Peru

JEL classification nos: O12; I12; J13; J16

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1 Introduction

Given its central role in promoting economic development, women's economic empowerment lies squarely at the heart of the global policy agenda (Duflo, 2012; United Nations, 2014; Global Affairs Canada, 2017; UN Women, 2018). Women's ability to meaningfully engage in the labour market. access education, and participate in political processes will largely depend on how empowered they are in individual, household and community decision-making and the degree to which they bear the disproportionate burden of care. Self-determination in reproductive outcomes is central to this calculus and empowerment in the reproductive domain is intrinsically linked to empowerment in other domains (Pritchett, 1994; Upadhyay et al., 2014; Prata et al., 2017; Doepke and Tertilt, 2018). Globally, governments and development agencies are increasingly funding programs that target women's rights, empowerment, and reproductive health with the goal of increased equality. Yet despite these trends and increased policy focus, many women (especially in low income settings) continue to experience considerable inequalities in the household decision-making process. And despite rapid progress on the technology and social norms around contraception and family planning, many women find themselves in situations of excess fertility. This is still the case for many women in Latin America, who continue to face unwanted pregnancies despite the availability of modern contraception (Palomino et al., 2011; Bearak et al., 2018) and to face spousal discordance in fertility preferences (McNamee, 2009).¹ Unwanted pregnancies and excess fertility contribute to maternal deaths and adverse child health outcomes and can contribute to a vicious intergenerational cycle of poverty. Increasing women's empowerment can thus lead to improvements in fertility outcomes, in maternal and child health, and in human development.

Of the many economic determinants of fertility behaviours, income plays central – albeit confounding – roles (Jones et al., 2008). Becker's (1960) seminal work shows that if expenditures on children are regarded as a normal good, increased incomes will imply that households will dedicate more resources towards their children. This pure income effect will lead to households either having more children (if the household values quantity over quality) or devoting more resources to fewer children (if the household values quality over quantity). Meanwhile, an increase in the

¹ Even in the US, more than 50% of pregnancies are unplanned, a trend which has worsened in the 2000s (Finer and Zolna, 2014) and leading to the National Institutes of Health declaring contraceptive use a research priority.

woman's income relative to her spouse's will lead to an increase in her bargaining power within the household (Hodinott and Haddad, 1995; Basu et al., 2002; and De Brauw et al., 2014). In a nonunitary household model, a woman's bargaining power, in turn, will affect her fertility behaviour (Doepke and Kindermann, 2018) by putting more weight on her preferences within the household utility function. To the extent that she prefers more/less children, a higher income will lead to increased/decreased fertility. The secular decline observed in fertility rates might thus be partially explained by increased women's empowerment, although numerous confounding effects of income on fertility obscure this causal effect. Most importantly, if women's increased income is generated from labour activities, then fertility effects will be driven by the opportunity cost of her time.

Conditional cash transfers (CCTs) provide a unique opportunity to investigate the relationship between a woman's income and her fertility behaviour. Because CCTs typically target households with school-aged children conditional on their attendance, the cash transfer is not contingent upon the parent's labour supply, effectively shutting off the opportunity cost channel. In a Beckerian sense, we can consider the increase in income from a CCT as a pure income effect.² This could lead to an increase or a decrease in fertility depending on whether children are normal goods and the relative preference for quality over quantity of children. Furthermore, as cash transfers are typically given to the mother, and not the father, they raise the woman's income share in the household budget giving her more bargaining power in household decision-making.

Not much is known about the effects of exogenous income shocks on household fertility decisions. There is evidence that adverse shocks can affect the timing of births, but it is unclear whether the cause is households' intentional planning (say through contraceptive use) or unintended consequences of the shocks (Lindström and Berhanu, 1999; Evans, Hu, and Zhao, 2010; Alam and Pörtner, 2018). Meanwhile, evidence from positive policy driven income shocks – such as cash transfers – is sparse. The large literature on conditional cash transfers has disproportionately focused on effects on families' investments in their children's human capital rather than on their decisions around how many children to have. Conditional cash transfer programs have been linked to increased school attainment rates and improved long term labour market outcomes of child beneficiaries (Behrman et al., 2011). A systematic review by Khan et al. (2016) points to the scarcity of rigorous evidence

 $^{^{2}}$ A CCT also leads to a reduction in the price of investing in children's human capital, which would have similar quantity-quality trade-off effects on fertility as a positive income effect.

of CCT effects on fertility outcomes (timing and spacing of births) or family planning (contraceptive use and composition). The metanalysis of Mexico's CCT, Progresa, by Parker and Todd (2017) identifies only one study that looked at the effect of conditional cash transfers on adult contracepetive use. Indeed, Lamadrid-Figueroa et al. (2010) find that the increase in contraceptive use among Progresa beneficiaries is concentrated among the poorest. Meanwhile, most studies linking CCTs to fertility outcomes focus on the adolescent girls in recipient households.³ Results provide overwhelming evidence of reduced adolescent pregnancy, in part owing to the school attendance conditionality (Baird et al., 2011). And while Molyneux and Thompson (2011) and others do study the effect of CCTs on women's economic empowerment, evidence of the direction of causality is ambiguous. Our reading of the literature suggests a dearth of rigorous evidence on the mechanisms through which cash transfers can influence fertility behaviours of adult beneficiaries. Specifically, do cash transfers influence mothers' intentional family planning by changing the use and type of contraceptive methods used?

In a bid to shed light on the causal effects of income shocks on reproductive choices, we investigate the effect of Peru's Juntos program on mothers' contraceptive use.⁴ Launched in 2005, this social assistance program aims to improve maternal and child health and raise children's educational outcomes by providing bi-monthly cash transfers to poor and mostly rural mothers conditional on their children attending school and regular health checks. While this program was not rolled out on an experimental basis, we exploit rich administrative data with spatial (district level) and time variation in the implementation of Juntos over the decade between 2005 and 2017 to identify the effects of the cash transfer. In addition to a static difference-in-differences (DID) approach using Peru's Demographic Health Surveys (DHS), we conduct an event study analysis (Autor, 2003; Almond et al, 2011; Chetty et al., 2013; Greenstone and Hanna, 2014; Hoynes et al., 2015; Hoynes et al, 2016; Wing et al., 2018; and Cornelius et al., forthcoming) to identify dynamic effects of the program.

We first focus on the use of birth control (extensive margin) and the choice of modern relative to traditional methods (intensive margin) to explore the effect of the cash transfer on a household's

 $^{^{3}}$ One study by Handa et al. (2018) reviews the fertility effects of four different cash transfer schemes in Kenya, Malawi and Zambia, and fail to find any positive effect on fertility.

 $^{^4}$ We focus on reproductive choices in terms of family planning or contraceptive use and not on lower frequency outcomes such as number of births.

choice of reproductive technology. To explore the role that intra-household dynamics may play in mediating this choice, we conduct sub-analyses utilizing data from the DHS on spousal preferences over fertility to classify families according to spousal discordance (namely, men wanting more children than women). We do so to exploit strategic behaviour in household decision-making in environments where women might not be fully empowered (Baland and Ziparo, 2017). Utilizing a unique question in the DHS which asks if women hide the use of birth control from their partners, we follow Ashraf et al. (2014) who explore the role that moral hazard plays in contraceptive use among families in Zambia. Specifically, women in discordant couples (where their spouses prefer larger families) who desire to delay child birth may compensate for lower bargaining power by concealing birth control use. These data allow us to highlight whether CCTs affect contraceptive use (intensive and extensive margins) controlling for differential preferences over fertility.

Our results show that Juntos influenced reproductive choices among recipient households. Specifically, we find that beneficiary women are more likely to switch from traditional to modern methods. These effects are evident in static DID analyses as well as in the event study in which we estimate persistent effects years after the initial roll out. For women who have more prolonged exposure to Juntos, we estimate a positive effect even on the extensive margin: being exposed to the program for over two years is associated with a 2.25 percentage point increase in using any form of birth control. Our estimates point to strategic intra-household decision-making effects when the beneficiary prefers a smaller family than her spouse. The cash transfer is associated with a higher uptake in concealable forms of contraception (injections). Furthermore, women are more likely to report hiding their use from their spouse, adding new evidence to this phenomenon first described in Ashraf et al. (2014). This result points to the possibility that Juntos allowed some women the ability to take control - albeit clandestinely - over fertility. In contrast, the finding of clandestine use among spousal discordant couples underlines that these women are still not fully empowered given their need to conceal their actions, thus pointing to the limits of cash transfers as a method to fully empower women at least in this domain. This is an especially important finding when we consider that CCTs were in part motivated to help solve market failures arising from intra-household bargaining power imbalances (Das et al. 2005).

2 Peru's Juntos Conditional Cash Transfer Program

Peru's Juntos program – *el Programa Nacional de Apoyo Directo a los más Pobres* – began in 2005 as an anti-poverty program and is currently operated by Peru's Ministry of Development and Social Inclusion (MIDIS) in a bid to reduce rural poverty on the heels of a devastating economic crisis and brutal 20 year civil conflict. Using a two stage targeting system, first targeting districts and second targeting households within eligible districts, households who meet the conditionalities of the program would receive 100 soles every month (eventually 200 soles every 2 months).⁵ This cash transfer is independent of the number of children eliminating a direct incentive to affect fertility for families who already have at least one child.⁶ Figure 1 shows the evolution of the program roll out over time in terms of the number of districts and the number of beneficiaries. By 2017, the program had been rolled out to 1305 out of a total of 1896 districts in Peru. Considering that only rural districts are eligible under the targeting rules, this shows the scope of the regional coverage.⁷

Alácazar (2009) and Linares Garcia (2009) spell out the conditionalities of the cash transfer. While the precise details and thresholds for the conditionalities have changed over the course of the program, the key conditions can be generally summarized as follows. First, children aged 6 and above 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. Finally, pregnant women must receive routine pre- and post-natal health checks and attend reproductive health talks.

3 Data

3.1 Peru's Demographic and Health Surveys

Because of its emphasis on health and especially reproductive health, Peru's Demographic and Health Survey (DHS) is uniquely placed to allow us to investigate the impact that Juntos has had

 $^{^5}$ This is approximately equivalent to US\$30 per month. In comparison, the 2017 National Rural Extreme Poverty Line is 150 soles per month, roughly 50% above the Cash Transfer amount (INEI 2017).

⁶ For more formal details on the Juntos program, see Linares Garcia (2009), Molyneux and Thompson (2011), Escobal and Benites (2012), Dias and Saldarriaga (2014), MIDIS (2014), Carpio et al. (2018).

⁷ In addition, a few predominantly remote departments in the Amazonia have only recently begun to receive any Juntos funding, such as Lambayeque, Madre de Dios and Ucayalí.

⁸ Schooling is compulsory in Peru until age 16.

on matters around fertility, such as reproductive choices and contraceptive use. In addition, since we are concerned about empowerment or intra-household bargaining as a potential mediating factor, the DHS is especially well suited as it allows us to ascertain women's reports of spousal discordance over reproductive matters (preferred family size), use and type of birth control (traditional, condoms, pill, injections...), and strategic decision-making (whether she hides contraceptive use).⁹ The DHS also contains a vast array of socio-economic variables that we will require as controls. The DHS (or ENDES, *Encuesta Nacional Demográfica y de Salud*) was run every four years until 2004 when the Statistical Agency for Peru began running the survey yearly.

In contrast to the DHS surveys for most countries with five year intervals between waves, Peru's DHS is a continuous survey in which data is collected and reported on annually by the Peruvian National Statistics and Information Institute (INEI). This allows us to get a rare relatively high frequency of nationally representative repeated cross sections for a single country, allowing us to exploit greater degree of annual variations in roll out of Juntos and to conduct dynamic effects of a CCT, which may not be feasible in other contexts with more limited data. For our analysis we utilize the yearly DHS waves from the year prior to the introduction of Juntos (2004) to that latest round in 2017.

3.2 Administrative data on Juntos district-level roll out

Juntos was rolled out in numerous phases beginning in 2005. We use administrative data on the roll out using the first stage of targeting: the geographical targeting, as described in Figure 1. As discussed above, the program originally aimed to target rural districts who disproportionately suffered from the civil conflict. Carpio et al. (2018) document the different phases of the roll out of the program and provide a reconciliation of the administrative data (used here) and the eligibility criteria. 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 in the district who were severely

⁹ The continuous DHS for Peru did not collect data from the spouses/partners, so we rely exclusively on the respondents' self-reports.

affected by violence. We utilize this administrative data to identify districts who are targeted for receipt of Juntos at any given point in time. We merge the DHS with the administrative data on the Juntos roll out at the district year level. We generate our main variable of interest, the exposure to Juntos, as a dummy variable equal to 1 if the respondent is in a targeted district at the time of the DHS interview. To be precise, treatment here refers to district groupings: during each phase of the roll out, numerous districts where rolled in simultaneously, so the identification is made off groups of districts, rather than individual districts.

3.3 Sample and Variables of Interest

We impose a number of sample restrictions for the purposes of our empirical analysis. We restrict the analysis to fecund women who are married or in a co-habitation relationship. We exclude from the sample women who report never having had sex, are infertile or in a relationship with an infertile spouse. Furthermore, given the eligibility criteria used by Juntos, we restrict the sample to (1) poor women in (2) rural areas. To do so, we exploit the wealth quintiles from the DHS and we restrict the analysis to the two lowest quintiles. Indeed, respondents in the top three wealth quintiles reported little to no receipt of Juntos.¹⁰ These restrictions yield a sample of 52,619 women (aged 15-49). Finally, we focus our analysis on the districts that enter the Juntos program between 2005 and 2017 (1306 districts out of 1838 districts in Peru). After removing observations with missing values on key variables (e.g. contraceptive use, discordance status, etc.) we end up with an analytic sample of 41,221 women.¹¹

Our primary outcome variables of interest include the use of family planning methods (extensive margin) and type (intensive margin). We use the DHS question on whether the respondent is using some form of birth control at the time of the interview for the extensive margin. Just under 80% of our sample is using birth control at the time of the interview. Those not using birth control at the time of the interview. Those not using birth control at the time of the interview a need for preventing pregnancy. For the intensive margin, we use the DHS question on the method used to avoid pregnancy and follow

¹⁰ Results available upon request

¹¹ We start with a sample of 292,279 observations. After excluding the districts that do not get rolled in to Juntos during our reference period (2005-2017), we obtain a sample of 135,645 women. Restricting the sample to poor women, in rural areas, who are married or co-habitating and excluding women who report never having had sex, are infertile or in a relationship with an infertile spouse, yields a sample of 45,888. After removing observations with missing values on key variables, we obtain 41,221 observations.

the DHS definition for "traditional method" to include periodic abstinence and withdrawal.¹²

Our main mediating factors include spousal discordance in fertility preferences and hidden use of contraception. Our motivation draws from Ashraf et al. (2014) who conducted a randomized controlled experiment in which vouchers for free birth control were offered to families, randomizing whether the offer was made in the presence of the husband or not. They found that women who were given the voucher in the presence of their husband were considerably less likely to seek family planning services and also less likely to choose concealable forms of birth control. This points to important moral hazard effects in intra-household decision making, prompting us to investigate whether such behaviours might be a concern in the setting of our study. Spousal discordance is defined here using respondents' reports on their ideal family size and their self-report on their spouse's ideal family size. We categorize households into three types: both the respondent and her spouse have the same preferences over family size (no discordance), respondent wanting more children than her spouse, and respondent wanting fewer children than her spouse. The latter type will be of specific interest in our analyses. The DHS also asks whether respondents who use birth control conceal their birth control use from their spouse. From this survey question, we create a binary variable for "hidden use".

3.4 Descriptive statistics

Table 1 presents birth control use (at time of interview) and in the affirmative case whether the respondent is concealing (hiding) this from her spouse. The use of birth control is fairly stable around 78% to 80% for our sample and very few respondents hide this use from their spouse. However, women in discordant couples, who want fewer children than their spouse (19.0% of the sample), are 5 times more likely to conceal birth control use. This is a first indication that there is potentially interesting strategic intra-household dynamics in the space of reproductive choices. For respondents who are not using any form of birth control, Table 2 presents the principal reasons. Close to one third of the sample of women not using birth control report a current pregnancy as

¹² Options for methods include female sterilization, male sterilization, pill, DIU, injection, implants or Norplant, condom, foams and jellies, amenorrhea, periodic abstinence, withdrawal, other. While some scholars would include lactational amenorrhea as non-modern (Hubacher and Trussell, 2015), we follow the DHS and the World Health Organization and consider this as a modern method (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).

the reason. The main difference across discordance status comes from women in relationships with spouses who they report wanting to have more children. These women are five times more likely to cite their husband's opposition for not using any birth control.

Table 3 provides the distribution of contraceptive methods chosen by women who report using birth control, broken down by whether she reports to conceal use or not. Close to 40% of the total sample uses a traditional form of birth control (periodic abstinence or withdrawal). Injections followed by birth control pills account for the majority of modern birth control use. Comparing across Panels A and B reveals interesting patterns between the choice of modern methods. While most respondents select the pill or injections when they report hiding birth control use from their spouse, the proportion who use injections is considerably higher for women whose spouses want more children.¹³

In addition to these main outcome and mediating variables, we include a large array of control variables from the DHS which will allow us to control for socio-economic and demographic influences, namely age and education of the respondent and spouse, respondent's occupation, marital status (married versus co-habitating) and a wealth index (quintile) calculated by the DHS. The descriptive statistics for these variables are reported in Table 4. The average woman in our sample is 31 years old, has just over 6 years of schooling, and is most likely engaged in agricultural self-employment (or not working). Her husband is 35 years of age on average, has just over one more year of education. Two thirds of respondents are co-habitating but not married. These characteristics do not vary by discordance status.

The DHS asks respondents whether they have received the Juntos cash transfer. However, we do not utilize this variable for our main analysis for two reasons. First, it is only collected between 2009 and 2012 and only for women with a child aged 5 or less, which would lead to a considerable reduction in sample size. Second, we have reasons to believe that the self-reports contain considerable error. Indeed, more than half of those who claimed to have received the program were unable to produce their Juntos beneficiary card. We will appeal to the administrative roll out for data on Juntos which we describe next, and explain our empirical approach in the subsequent section. However, in our robustness checks we will re-examine the relationship using the restricted sample (corresponding to

 $^{^{13}}$ While both the pill and injections could be viewed as concealable, it can be argued that there is more risk that the pill can be discovered by the spouse than injection (Ashraf et al., 2014)

the DHS waves which collected self-reported receipt of Juntos).

4 Identification Strategy

4.1 Difference-in-Differences

We begin by conducting a standard DID analysis using individual level observations. We estimate the following equation:

$$y_{itq} = \alpha + \beta Juntos_{tq} + Year_t + DistrictGroup_q + X_{itq} + \epsilon_{itq}$$
(1)

where y_{itg} is the outcome of interest for woman *i*, in year *t*, in district group *g*. Juntos_{tg} is equal to 1 when the woman lives in a district group *g* which is targeted in year *t*, Year_t is a set of dummies to control for year specific trends, DistrictGroup_g is a set of dummies capturing district-group specific characteristics. A group of districts is formed by all the districts that entered the program the same year. The treatment variable is whether the respondent resides in a treated district, and not whether she herself received the transfer. In this sense, our results should be interpreted only as intent-to-treat (ITT) effects. X_{itg} are individual covariates (woman age and age squared, occupation and education, partner's age, age squared and education, wealth dummies, married or cohabiting). We cluster the standard errors at the district level.

4.2 Event Study Analysis

Alternatively, we may be interested in exploring whether Juntos had any persistent effects by considering the length of exposure to the program. Indeed, the effects of Juntos may not be immediate and there may be lagged effects. Furthermore, a more careful exploration of both leads and lags of program introduction will allow us to identify any pre-trends that may act as threats to the identification of causal effects.

To explore this, we estimate an event study model (Generalized DID) as in Hoynes et al. (2016), Chetty et al. (2013), and Greenstone and Hanna (2014) and Cornelius et al. (forthcoming). This analysis entails estimating a DID type model with leads and lags. In particular, the event study design allow us to explore non-parametrically the relationship between the time at which we first observe Juntos in our data and the effects on reproductive outcomes of interest. We thus estimate the following equation:

$$y_{itg} = \alpha + \sum_{\tau} \sigma_{\tau} \mathbf{Juntos}_{\tau, \mathbf{tg}} + Year_t + DistrictGroup_g + X_{itg} + \epsilon_{itg}$$
(2)

where $\mathbf{Juntos}_{\tau, \mathbf{tg}}$ is a vector with separate indicators for the years before and after Juntos is introduced in district group g. The year index τ is normalized at 0 for the year the district group entered the program. A test of the differences-in-differences assumption is that the coefficients on all leads of the treatment should be zero ($\sigma_{\tau} = 0, \forall \tau < 0$). Moreover, $\sigma_{\tau}, \tau >= 0$ may not be identical. For example, the effect of the treatment could accumulate over time, so that the effect of Juntos increases over time. The fact that Juntos assignment is permanent at the district level gives us a useful opportunity to investigate how quickly the program leads to behavioural changes and how these effects evolve over time.

5 Results

5.1 Difference-in-Difference

Table 5 provides the results from a difference-in-difference estimation of the ITT effect of the Juntos cash-transfer program on three outcome variables of interest: whether the respondent uses birth control (column 1), and for those who do use birth control whether she is using a traditional method (column 2) and whether she hides her birth control use from her spouse (column 3). While we do not find any effect on the extensive margin for our basic DID (whether the respondent uses birth control at all), we find a large and statistically statistically significant effect on the intensive margin: beneficiary respondents are about 4.3 percentage points more likely to switch away from traditional forms of birth control. Meanwhile, there is no indication that the average beneficiary is more or less likely to conceal the use of birth control from her spouse. While these results are interesting, they do not say anything about the possible role that intra-household bargaining might play as a mechanism through which the cash transfer influences reproductive choices. Indeed, these results would be entirely consistent with two alternative stories: (1) that the cash transfer relaxes the affordability constraint and (2) that the cash transfer includes some reproductive health advice as

part of the program.

To better get at whether the cash transfer increases women's empowerment and that intrahousehold bargaining may play a role in the decision over reproductive choices, we investigate heterogeneous effects according to spousal discordance status. Before doing so, we show in Table 6 that the cash transfer program is not associated with spousal discordance status. Table 7 presents the results from interacting the treatment variable with spousal discordance (with no discordance as the reference category). The result from Table 5 of no ITT effect on the extensive margin is maintained, as is the effect on the shift away from traditional forms of birth control. The negative linear effect of husbands wanting more children than the respondent on the use of traditional forms of birth control, conditional on using, is consistent with a story in which intra-household dynamics plays a role in reproductive choices. Indeed, in column (3), we find a statistically significant positive effect of the interaction between the treatment dummy and husbands wanting more children dummy on the likelihood the respondent will conceal the use of birth control.¹⁴ This result suggests that strategic behaviour does matter for women's reproductive choices and that the cash transfer program may facilitate women's ability to assert – albeit clandestinely – their preferences around childbearing.¹⁵

5.2 Event Study Analysis

Figures 2 to 4 present the graphical representation of the event study analysis from equation (2). For all districts, we consider 6 years before it enters the program and 6 years after it enters the program.¹⁶ These diagrams allow us to flesh out a few patterns that are obscured by a simple

¹⁴ We considered only the concealment of injections because of the argument that they are more easily concealable than the pill (Ashraf et al, 2014). Running this analysis on a dependent variable which denotes concealing either injections or the pill leads to similar though weaker results. Results available upon request.

¹⁵ To corroborate the possibility that these results are driven by an empowering effect of the cash transfer, we considered the autonomy questions in the DHS. These are known to be problematic in Latin America as women tend to already greatly, and disproportionately, participate in household decisions. Described as *Supermadres* (Chaney, 1979; Folbre, 1994; Martinez-Restrepo, 2017), Latin American women, compared to their counterparts in other regions in the developing world, score quite highly in terms of their autonomy over the household budget and whether they can freely visit friends and family. We re-ran our analysis (not shown here but available upon request) on the autonomy measures most directly related to the domain in question: who has the final say on the respondent's health care and who makes the decision over birth control. We find similar results using the former, and statistically insignificant results in the latter.

 $^{^{16}}$ Our event study diagrams are sensitive to the number of leads and lags – the coefficients do move around and the standard errors are affected by the choice of pre- and post- time periods, but the overall patterns are similar. To our knowledge, there is no explicit econometric guidelines to select the number of leads and lags.

difference-in-difference. In Figure 2, we see evidence of a positive effect of the Juntos program on birth control use after the district enters the program and no effect before the treatment district enters the program. Meanwhile, the corresponding difference-in-difference estimate in column 1 from Table (7) showed no effect. Indeed, the closer inspection afforded by the event study shows an increasingly positive estimated ITT coefficient though its confidence interval never quite clears the null. This is suggestive evidence that Juntos may have lead to increase in birth control on the extensive margin over time as well.

Figure 3 studies dynamic effects on use of traditional versus modern contraceptive method. These results show an increasingly negative ITT effect after the introduction of Juntos and no clear pattern before its introduction. The confidence intervals on the ITT effect tend to clear the null hypothesis after the introduction of the program. This is consistent with the point estimate from the simple difference-in-difference result in Table (7) which indicated a strongly negative effect of the program on the dependent variable. The fact that the point estimates increase over time suggests that the effects of Juntos accumulate over time. The comparison before and after treatment is less clear in Figure 4, for the imputed ITT interaction effect with husbands wanting more children than the respondent. However, all post-treatment coefficients lie above zero while there is no discernible pattern pre-treatment and our estimates are not precisely estimated. Given that the sample of women actually hiding use of birth control in discordant couples is small (340 observations), our analysis may not be powered enough to identify subtle effects for this measure.

The comparison between pre- and post-treatment in these event studies allow us to speak to the common trend analysis. Our results in these figures fail to show clear pre-trends for the first two outcomes (using birth control and, conditional on using birth control, choosing a traditional method). The interaction effect with husbands wanting more children than the respondent is less clear.

To further understand the dynamic/longer term effects Juntos, we estimate a difference-indifference model in which we break the treatment into two components: whether the respondent lives in a district which has been targeted within the last two years or whether the district has been targeted by Juntos for more than two years. Table (8) provides the results from this analysis.

The static difference-in-difference results in Table (7) that the cash transfer has no effect on

birth control use is partially reversed when we allow for a more dynamic functional form. Longer exposure to Juntos is associated with a positive and significant ITT effect of the conditional cash transfer on the use of any form of birth control, consistent with the graphical evidence from Figure 2. A similar pattern emerges for use of traditional methods – the effect is stronger and more precisely estimated for respondents living in districts treated for more than two years. Our results on women hiding birth control from their male partners are maintained in column (3) of Table (8). We find persistence in this effect over time, though the magnitude of the coefficient is smaller for those targeted for 2 years or more.

6 Robustness Checks

6.1 Sample Inclusion Criteria

We check for the robustness of our results to allow for stricter sample inclusion criteria. First, we exclude from our sample any pregnant women, for whom birth control is irrelevant. Panel A of Table (9) re-runs the analysis from Table (7) excluding pregnant women. We confirm that this does not affect our results. The program had no effect on whether respondents used birth control, and since none of the women responding using birth control were pregnant we could not anticipate any differences in columns (2) and (3).

Second, our analysis has so far included women who might not have had a child when Juntos entered their district. Meanwhile the eligibility criteria for Juntos essentially requires that the recipient has at least one child under the age of 15. To better approximate the individual eligibility criteria for receiving Juntos, it may be useful to restict the sample to women who had at least one child at the time that Juntos was rolled in their district. We present the results in Panel B of Table (9). We find similar results for using birth control (no significant ITT effect) and on concealing the use of birth control for discordant couples (positive effect), and a negative and statistically significant result on contraceptive use.

6.2 Specification Tests

Table (10) compares our base specification in column (2) (this corresponds to our results in Table (7)) to removing all socio-economic controls (column 1), adding province fixed effects (column 3), adding household demographic composition (column 4), and adding group-specific trends composition (column 5). Our estimates are largely unaffected by these tests, reassuring us that our main results are not driven by specification along geographic or time dimensions, or by our selection of individual or household control variables. Similarly, we re-ran the results from Table (7) with different methods of estimating robust standard errors (clustering at the province instead of district level, unclustered robust standard errors, and simple unclustered non-robust standard errors). Our results are unaffected.¹⁷

6.3 Parallel Trends

The group-specific trends (in column 5) provide another useful check on the common trend analysis. Following Autor (2003) and Angrist and Pischke (2009), as long as the inclusion of group specific trends does not affect the coefficients of interest and are jointly statistically insignificant, then we fail to reject the common trends assumption. For the first two outcome variables (using birth control, and using traditional methods), the group-specific trends do very little to move the estimated treatment coefficients and the F-test fails to reject that they are jointly insignificant. For the third outcome, the F-test rejects that the group-specific trends are jointly insignificant and their inclusion does change the sign and order of magnitude of the linear treatment variable. However, the result on the interaction terms, our main coefficient of interest in this regression, is robust to this specification.

6.4 Placebo Analysis

To alleviate any remaining concerns that our results using district level targeting are driven by spurious results, we conduct a placebo test on the non-poor. Our analysis in section 5 restricted the sample to married or co-habitating fecund women in the lowest two wealth quintiles living in rural

 $^{^{17}}$ Results not shown here but available upon request.

areas. The restriction on wealth was to recover one of the main eligibility criteria (poverty). To make sure our treatment variable is indeed relevant for individual targeting, we run the same analysis as in Table 7 using respondents in the upper three wealth quintiles instead (still restricting the sample to married or co-habitating fecund women in both rural and urban areas).¹⁸ The results in Table 11 show us that spousal discordance does play a role in predicting a woman's choice of reproductive technology: in households where the husband wants more children than the wife, she is more likely to shift from using traditional technologies to concealing the use of injections. Similarly, if women report a larger ideal family size than their spouses, they are more likely to report not using any form of birth control. These patterns are consistent with the intra-household bargaining dynamics underlying the mechanisms studied in this paper. However, there is no statistically significant ITT effect of Juntos, neither directly nor through heterogeneous effects with spousal discordance for non-poor women, as expected.

6.5 Estimating Effects of Juntos at the Individual Level

Our main difference-in-difference approach relies on estimating ITT effects from district level targeting, and not ATE effects from individual level targeting. To the extent that spillover effects exist within treatment communities, estimating the effect at the district level may in fact be preferred to individual level estimation (Angelucci and De Giorgi, 2009). Alternatively, we can analyse the individual-level receipt of Juntos. This data is only available in 4 DHS waves, in which the question was only posed to women with a child under the age of 5. We propose two robustness checks of our main results here.

We re-run our reduced form analysis using only the four waves of the DHS for which there is selfreported respondent level data on Juntos receipt and compare our ITT effects to ATE effects using individual, rather than district, level data for that more limited sample. Since we are concerned with measurement error in the individual self-reports in the DHS and the fact that individual targeting was not random, we conduct a 2 SLS-IV using the administrative and district level roll out data as an instrument for the individual level data. We believe the instrument should satisfy both the relevance and the exclusion conditions on conceptual grounds. Geographic targeting is the first

¹⁸ We included urban women in this placebo test as the sample size of non-poor women in rural areas is too small.

stage in the two step targeting process so it should be relevant. Also, there is little reason to believe that the instrument (the district level targeting) would have any impact on the outcome variables other than through the individual level targeting.¹⁹

The DHS only asked respondents about whether they received Juntos in the 2009-2012 waves for women with a child under 5 only which makes a total of 7,834 women who answered the question. More than half of those that responded 'yes' were however not able to produce the Juntos subscriber card when asked by the surveyor (Table 12). These two points give us concern around the measurement of this variable. Nevertheless we are reassured by the fact that the vast majority (97%) of those that respond in the DHS that they have received the cash transfer happen to be in districts we identify as being targeted in the administrative roll out data.²⁰

We re-run equation (1) in three variations on the smaller sample. First, we use the individual self-report of Juntos participation instead of the administrative data at the district level. This should give us an ATE of Juntos on the outcomes of interest. Second, since we are working on a very different sample, we re-run our earlier analysis on the restricted sample by considering the district level administrative data. This gives us the ITT which we can compare to the ITT results from the full sample reported in section 5. Finally, we can use the administrative district level roll out variable as an instrumental variable for the self-reported variable. Table (13) presents the first stage of the 2SLS-IV to assess the strength of the instrument in predicting the individual level self-report. This coefficient is strongly statistically significant and positive suggesting the relevance criteria for the instrument is met.

Tables (14) to (16) compare the ATE (column 1), ITT (column 2) and 2SLS-IV (column 3) for each outcome respectively, on this restricted sample for which we observe Juntos participation status. First, comparing the ITT effects in columns (3) to those in Table (7), our results are consistent across identification methods for birth control use and for concealing use from their spouse. The result for traditional methods maintains a similar order of magnitude but is now statistically insignificant. This points to the limitations from a considerably reduced sample size.

¹⁹ One might be concerned about Type II errors in targeting: the inclusion of the non-poor. According to Hanna and Olken (2018) and Robles et al. (2015), the inclusion error in Juntos is relatively low at just over 6%.

²⁰ The fact that we see a large number of self-reported non recipients in the districts identified in the administrative roll out data is however not so informative in this table as the table ignores the timing: a recipient may for instance live in a district that will receive Juntos at some time after she is observed in the DHS wave.

Second, the ATE effects (column 1) show a positive effect of self-reporting Juntos participation on using birth control, in roughly the same magnitude as we saw in the main results in section 5. The ATE effect on using traditional methods, conditional on using, are statistically insignificant but again quite similar in magnitude and sign to the ITT coefficients in column (2). The ATE effects on hidden birth control are also similar to those in Table (7): the linear effect of treatment is negative (though now statistically significantly so), but the interaction effect with husbands wanting more is still statistically significant and positive and of a considerably larger magnitude.

Third, the 2SLS-IV results, reported in columns (3), corroborate the ITT estimates from using the administrative district level targeting variable: no statistically significant effect on using birth control or traditional methods, but a strongly significant effect (of larger magnitude) on the interaction with spousal discordance (consistent with an attenuation bias).

In sum, while the analysis in Tables (13) to (16) substantiate our main results, we feel justified to maintain estimated ITT effects off the administrative roll out data for the three reasons highlighted above: we would have to restrict our analysis to fewer DHS waves; there are an alarming number of missing observations for Juntos participation in these waves; we have concerns about reporting error in these self-reports as respondents were unable to produce the Juntos participation card. Furthermore, to the extent that the program might have had spillover effects on non-eligible poor households in the targeted districts as in Angelucci and De Giorgi (2009), district level analysis is recommended.

7 Conclusion

More than 220 million women in developing countries have unmet need for modern contraceptives (Singh et al. 2012). The post-2015 Copenhagen Consensus targets elimination of unmet need for modern contraception by 2040. Availability of modern contraception can allow parents to control their fertility, allowing for short and long-term positive effects on health, educational attainment and earnings (Goldin and Katz, 2002; Bailey, 2006; Miller, 2009; Joshi and Shultz, 2013; Miller and Babiarz, 2016). Kohler and Behrman (2014) estimate that universal access to sexual and reproductive health services and meeting unmet need for contraceptives will yield a staggering \$120

for every \$1 invested.

This paper studies the impact of Juntos on contraceptive use among poor Peruvian women. On the extensive margin, we find that the program lead to an increase in birth control use, but only after the program was in place for 2 or more years. On the intensive margin, we find that Juntos lead to a 4.34% points increase in modern methods relative to traditional methods, which is a 7% effect as a percentage of the mean. Event study analyses reveal that these effects tend to increase over time with the duration of the program, with no evidence of pre-trends. A placebo analysis further shows that we dont find any effect on wealthier households.

We use data on couples' decision-making and fertility preferences to shed light on plausible mechanisms. In the context of intra-household allocation models, increasing a womans bargaining power places greater weight on her preferences. This ought to matter particularly in cases where there is spousal discordance in preferences over goods. Ashraf et al. (2014) show that by their concealability, modern contraceptives introduce a scope for moral hazard as women may chose to hide the use of such methods from their partners which can lead to distrust and inefficiencies in household allocation of resources. Our paper adds new evidence of such behaviour. Namely, women whose spouses want more children (those with greater scope for moral hazard) are more likely to hide the use of modern contraceptive methods. While this suggests that some beneficiary women in discordant relationships were able to take some control of their fertility, the need to do so clandestinely highlights the limitations of Juntos as an instrument to fully empower women.

Our paper speaks to the complex ways in which economic development may affect the demand for family planning and hence fertility. Even if contraceptives are widely available and affordable, women and couples still bear significant costs should they opt for such modern reproductive technologies. These costs arise not only due to distance to family planning services but also due to intra-household bargaining. Conditional cash transfers like Peru's Juntos may be able to reduce the costs of accessing such services. At the same time, CCTs can also allow women in discordant relationships to take back control over fertility. While our results are generally supportive of a welfare-improving effect of the program on women's reproductive choices, the possibility that CCTs may also exacerbate moral hazard concerns nonetheless suggest further welfare gains could be achieved if women could freely assert their preferences. At the very least, our analysis suggests directions future research to unpack the mechanisms through which programs meant to empower low-income women affect decision-making over fertility and family size and to pay greater attention to moral hazard in intra-household bargaining contexts.

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Figure 1: Number of Affiliated Households 2005-2017

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



Figure 2: Event Study of Juntos' Effect on Birth Control Use



Figure 3: Event Study of Juntos' Effect on Traditional Form of Birth Control

Figure 4: Event Study of Juntos' Effect on Hidden Use of Birth Control



	Total	Husband	Husband	Husband
		wants more	wants the same	wants fewer
	Panel A	I - Using any	birth control?	
% No	21.4	22.2	20.9	22.9
% Yes	78.6	77.8	79.1	77.1
Pan	el B - If	Yes, conceal th	he use from spous	e?
%Non hidden	98.1	94.4	99.0	98.6
% Hidden	1.9	5.6	1.0	1.4
% of sample		19.0	67.8	13.3
N	41,221	7,824	27,929	$5,\!468$

Table 1: Birth control use

Table 2: Reasons why not using any birth control

		Husband	Husband	Husband
	Total	wants more	wants the same	wants fewer
% Pregnant	32.1	30.7	32.5	31.9
% Postpartum, breastfeeding	20.9	16.7	22.5	19.6
% No or infrequent sex	16.8	15.5	16.8	18.8
% Health concerns	8.7	9.2	8.5	9.1
% Wants more children	5.9	7.4	5.8	4.2
% Husband opposed	1.7	5.4	0.7	1.0
% Lack of access/Cost too much	1.9	2.2	1.8	1.9
% Other	12.0	12.9	11.4	13.3
Ν	8,819	1,740	5,827	1,252

			Husband	Husband			
	Total	wants more	wants the same	wants fewer			
Panel A - sample that does not conceal use							
% Injections	37.9	39.2	37.5	38.0			
% Periodic Abstinence	29.4	26.3	30.1	29.5			
% Pill	10.4	11.5	10.3	9.4			
% Withdrawal	10.1	9.9	10.0	10.4			
% Condom	7.3	7.9	7.1	7.3			
% Other	5.0	5.3	4.8	5.4			
N	31,774	5,744	21,873	4,157			
Par	nel B - s	ample that cos	nceals use				
% Injections	76.8	82.6	71.2	64.4			
% Pill	8.9	9.1	8.7	8.5			
% Other	14.3	8.2	20.1	27.1			
N	628	340	229	59			

Table 4: Main covariates

	(1)		(2)	(3	5)	(4)
	Enti	ire	Husb	and Hus		and	Husb	and
	samj	ple	wants	more	wants th	ne same	wants	fewer
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	31.56	8.19	31.97	8.30	31.40	8.19	31.76	7.99
Education (in years)	6.02	3.61	6.13	3.71	6.01	3.59	5.88	3.58
Occ: Not Working	0.22	0.42	0.21	0.41	0.23	0.42	0.19	0.39
Occ: Prof., Tech., Manag.	0.02	0.14	0.02	0.14	0.02	0.14	0.02	0.13
Occ: Clerical	0.00	0.06	0.00	0.06	0.00	0.06	0.00	0.06
Occ: Sales	0.09	0.29	0.10	0.30	0.09	0.29	0.09	0.29
Occ: Agric-self employed	0.57	0.49	0.57	0.49	0.57	0.50	0.60	0.49
Occ: Household & domestic	0.02	0.13	0.02	0.15	0.02	0.13	0.02	0.13
Occ: Services	0.01	0.10	0.01	0.10	0.01	0.09	0.01	0.10
Occ: Skilled manual	0.02	0.15	0.02	0.15	0.02	0.15	0.03	0.17
Occ: Unskilled manual	0.01	0.11	0.01	0.11	0.01	0.10	0.01	0.12
Occ: Missing	0.02	0.15	0.02	0.15	0.02	0.15	0.03	0.16
Husb. Age	35.64	9.23	35.93	9.33	35.52	9.22	35.86	9.08
Husb. education (in years)	7.39	3.36	7.31	3.41	7.41	3.35	7.40	3.38
Cohabiting	0.64	0.48	0.68	0.47	0.64	0.48	0.62	0.49
Wealth index	1.32	0.47	1.34	0.47	1.31	0.46	1.31	0.46
Number of household members	5.24	2.01	5.24	2.04	5.24	2.01	5.26	2.02
Total number of children	3.38	2.30	3.50	2.40	3.33	2.28	3.49	2.27
Number of children under 6	0.98	0.79	0.96	0.78	0.98	0.79	1.02	0.81
Age at first birth	19.17	3.49	19.09	3.53	19.21	3.50	19.11	3.39
N	41,221		7,824		27,929		5,468	

Table 5: Effect of Juntos on Birth Control Use and Type of Birth Control Use. OLS estimates.

	(1)	(2)	(3)		
		Among women using BC			
VARIABLES	Using BC	Using traditional	Using hidden		
		forms	forms		
Targeted district	0.0072	-0.0434***	0.0019		
	(0.009)	(0.014)	(0.003)		
Constant	-0.0354	1.0340^{***}	0.0088		
	(0.047)	(0.058)	(0.014)		
Ν	41,221	32,402	32,402		
R-squared	0.052	0.081	0.006		
Mean of the dep. var.	0.786	0.387	0.019		

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The regressions include year of interview fixed effects, phase group fixed effects, woman and husband socioe-conomic characteristics.

	Husband	Husband	Husband
	wants more	wants the same	wants fewer
VARIABLES	(1)	(2)	(3)
Targeted district	0.0122	-0.0092	-0.0030
	(0.008)	(0.010)	(0.008)
Constant	0.1421^{***}	0.8467^{***}	0.0112
	(0.039)	(0.045)	(0.033)
N	41,221	41,221	41,221
R-squared	0.006	0.005	0.006
Mean of the dep. var.	0.190	0.678	0.133

Table 6: Effect of Juntos on Discordance in Ideal Number of Children. OLS estimates.

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The regressions include year of interview fixed effects, phase group fixed effects, woman and husband so-cioeconomic characteristics.

	(1)	(2)	(3)
		Among womer	using BC
VARIABLES	Using BC	Using traditional	Using hidden
		forms	forms
Targeted district	0.0056	-0.0372**	-0.0021
	(0.009)	(0.014)	(0.003)
Targeted district x Husband wants more	0.0081	-0.0139	0.0184^{***}
	(0.013)	(0.015)	(0.007)
Targeted district x Husband wants fewer	0.0013	-0.0228	-0.0024
	(0.016)	(0.019)	(0.005)
Husband wants more	-0.0234**	-0.0427***	0.0320^{***}
	(0.012)	(0.013)	(0.005)
Husband wants fewer	-0.0258*	0.0115	0.0048
	(0.014)	(0.017)	(0.004)
Constant	-0.0312	1.0337^{***}	0.0090
	(0.047)	(0.058)	(0.014)
Intent-to-treat effects by discordance status			
Husband wants fewer	0.0069	-0.0600***	-0.0045
	(0.016)	(0.022)	(0.005)
Husband wants the same	0.0057	-0.0372**	-0.0021
	(0.009)	(0.015)	(0.003)
Husband wants more	0.0138	-0.0511***	0.0163**
	(0.014)	(0.019)	(0.007)
	()	()	()
N	41,221	32,402	32,402
R-squared	0.053	0.083	0.023
Mean of the dep. var.	0.786	0.387	0.0194

Table 7: Effect of Juntos on Birth Control Use and Type of Birth Control Use. OLS estimates.

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The regressions include year of interview fixed effects, phase group fixed effects, woman and husband socioeconomic characteristics.

	(1)	(2)	(3)
		Among womer	n using BC
VARIABLES	Using BC	Using traditional	Using hidden
		forms	forms
Targeted for less than 2 years	-0.0031	-0.0187	-0.0025
	(0.011)	(0.016)	(0.003)
Targeted for 2 years or more	0.0225^{*}	-0.0599***	-0.0044
	(0.012)	(0.018)	(0.004)
Targeted $< 2y x$ Husb. wants more	0.0122	-0.0345	0.0280^{**}
	(0.021)	(0.024)	(0.011)
Targeted $\geq 2y x$ Husb. wants more	0.0074	-0.0100	0.0166^{**}
	(0.013)	(0.016)	(0.007)
Targeted $< 2y x$ Husb. wants fewer	-0.0092	-0.0380	-0.0017
	(0.024)	(0.028)	(0.007)
Targeted $\geq 2y \times Husb.$ wants fewer	0.0029	-0.0198	-0.0025
	(0.016)	(0.019)	(0.005)
Husband wants more	-0.0235***	-0.0425***	0.0320***
	(0.012)	(0.013)	(0.005)
Husband wants fewer	-0.0255*	0.0110	0.0048
	(0.014)	(0.017)	(0.004)
Constant	-0.0374	1.0419***	0.0099
	(0.047)	(0.058)	(0.014)
	× /	× /	× /
N	41,221	32,402	32,402
R-squared	0.053	0.083	0.023
Mean of the dep. var.	0.786	0.387	0.0194

Table 8: Dynamic Effect of Juntos on Birth Control Use and Type of Birth Control Use. OLS estimates.

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The regressions include year of interview fixed effects, phase group fixed effects, woman and husband socioeconomic characteristics.

	(1)	(2)	(3)	
		Among women	n using BC	
VARIABLES	Using BC	Using traditional	Using hidden	
		forms	forms	
Panel A: Excluding pregnant wome	cn			
Targeted district	0.0040	-0.0372**	-0.0021	
	(0.009)	(0.014)	(0.003)	
Targeted district x Husband wants more	0.0066	-0.0139	0.0184***	
	(0.012)	(0.015)	(0.007)	
Targeted district x Husband wants fewer	-0.0065	-0.0228	-0.0024	
N	38 350	32 402	39.409	
B-squared	0.036	0.083	0.023	
Mean of the dep. var.	0.845	0.387	0.0194	

Table 9: Testing Sample Restrictions. Effect of Juntos on Birth Control Use and Type of Birth Control Use. OLS estimates.

Panel B: Restricting to women with at least a child when Juntos starts in their districts

Targeted district	0.0074	-0.0298^{**}	-0.0035
Targeted district x Husband wants more	(0.010) 0.0094	(0.015) -0.0130	(0.003) 0.0253^{***}
	(0.014)	(0.016)	(0.007)
Targeted district x Husband wants fewer	-0.0092	-0.0157	-0.0035
	(0.016)	(0.020)	(0.005)
N	20.071	96.974	06.274
IN	32,871	20,374	20,374
R-squared	0.055	0.091	0.027
Mean of the dep. var.	0.802	0.401	0.0204

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.The regressions include year of interview fixed effects, phase group fixed effects, woman and husband socioeconomic characteristics.

Table 10: Specification Tests. Effects of Juntos on Birth Control Use and Type of Birth Control Use. OLS Estimates.

	(1)	(2)	(3)	(4)	(5)
	No	Socioeco.	Province	HH	Group-specific
	controls	char. ^a	FE	composition	trends
Outcome A: Using BC. N=41,221					
Targeted district	0.0039	0.0056	0.0006	0.0010	-0.0022
5	(0.010)	(0.009)	(0.009)	(0.009)	(0.011)
Targeted district x Husband wants more	0.0094	0.0081	0.0080	0.0084	0.0086
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Targeted district x Husband wants fewer	0.0011	0.0013	0.0037	0.0022	0.0019
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Adjusted R-squared	0.0342	0.0520	0.0621	0.0661	0.0661
F-test (H_0 : Group-specific trends=0)					[0.4258]
Intent-to-treat effects by discordance status					
Husband wants fewer	0.0051	0.0070	0.0042	0.0032	-0.0004
	(0.017)	(0.016)	(0.016)	(0.016)	(0.018)
Husband wants the same	0.0040	0.0057	0.0006	0.0010	-0.0022
	(0.010)	(0.009)	(0.009)	(0.009)	(0.011)
Husband wants more	0.0133	0.0138	0.0083	0.0093	0.0064
	(0.014)	(0.014)	(0.013)	(0.013)	(0.014)

Outcome B: Using traditional forms (among women using). N=32,402

Targeted district	-0.0361**	-0.0372**	-0.0474***	-0.0467***	-0.0438***
	(0.015)	(0.014)	(0.013)	(0.013)	(0.014)
Targeted district x Husband wants more	-0.0125	-0.0139	-0.0150	-0.0130	-0.0124
0	(0.016)	(0.015)	(0.015)	(0.015)	(0.015)
Targeted district x Husband wants fewer	-0.0225	-0.0228	-0.0188	-0.0210	-0.0215
5	(0.020)	(0.019)	(0.018)	(0.018)	(0.018)
Adjusted R-squared	0.0378	0.0813	0.135	0.138	0.138
F-test (H_0 : Group-specific trends=0)					1.01
					[0.4320]
Intent-to-treat effects by discordance status					
Husband wants fewer	-0.0586**	-0.0600***	-0.0662***	-0.0677***	-0.0653***
	(0.0235)	(0.0220)	(0.0203)	(0.0204)	(0.0212)
Husband wants the same	-0.0361**	-0.0372**	-0.0474***	-0.0467***	-0.0438***
	(0.0152)	(0.0145)	(0.0132)	(0.0133)	(0.0145)
Husband wants more	-0.0486**	-0.0511**	-0.0623**	-0.0597**	-0.0563**
	(0.0194)	(0.0190)	(0.0169)	(0.0170)	(0.0177)

Outcome C: Using hidden forms (among women using). N=32,402

Targeted district	-0.0017	-0.0021	-0.0021	-0.0022	0.0010
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Targeted district x Husband wants more	0.0177***	0.0184***	0.0185***	0.0180***	0.0177***
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
Targeted district y Husband wants fewer	0.0027	0.0024	0.0018	0.0012	0.0014
Targeted district x Husband wants lewer	-0.0021	-0.0024	-0.0013	-0.0012 (0.005)	-0.0014
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Adjusted R-squared	0.0378	0.0813	0.135	0.138	0.138
F-test (H_0 : Group-specific trends=0)					3.12
- ···· (····· ········ ······ ······ ······					[0,0006]
Intent-to-treat effects by discordance status					[0.0000]
Husband wants fewer	-0.0044	-0.0045	-0.0039	-0.0034	-0.0004
	(0.005)	(0.005)	(0.005)	(0.0051)	(0.005)
Husband wants the same	-0.0017	-0.0021	-0.0021	-0.0022	0.0010
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Husband wants more	0.0160**	0.0163**	0.0164**	0.0159**	0.0187**
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)
Year of interview fixed effects	YES	YES	YES	YES	YES
Phase group fixed effect	YES	YES	YES	YES	YES
Socioeconomic characteristics ^{b}	NO	YES	YES	YES	YES
Province fixed effects	NO	NO	YES	YES	YES
Household composition ^c	NO	NO	NO	YES	YES
Group-specific trends	NO	NO	NO	NO	YES

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. a. Our main specification. b. Woman's age, age squared, occupation and education, partner's age, age squared and education, wealth dummies, rural/urban, married or cohabiting. c. Number of household members and number of children under 5. P-values for F-stats in square brackets.

	(1)	(2)	(3)
		Among women	n using BC
VARIABLES	Using BC	Using traditional	Using hidden
	-	forms	forms
Targeted district	-0.0012	0.0063	0.0054
	(0.013)	(0.018)	(0.004)
Targeted district x Husband wants more	0.0065	0.0214	-0.0061
	(0.015)	(0.023)	(0.005)
Targeted district x Husband wants fewer	0.0002	0.0183	-0.0025
	(0.020)	(0.028)	(0.005)
Husband wants more	-0.0119	-0.0367**	0.0205^{***}
	(0.008)	(0.018)	(0.003)
Husband wants fewer	-0.0232*	0.0085	0.0013
	(0.013)	(0.018)	(0.003)
Constant	0.3460***	0.7965^{***}	0.0241
	(0.071)	(0.115)	(0.032)
Intent-to-treat effects by discordance status			
Husband wants fewer	-0.0010	0.0246	0.0029
	(0.020)	(0.031)	(0.005)
Husband wants the same	-0.0012	0.0063	0.0054
	(0.013)	(0.018)	(0.004)
Husband wants more	0.0053	0.0277	-0.0007
	(0.017)	(0.027)	(0.005)
	× /		× /
N	16,354	13,108	13,108
R-squared	0.038	0.081	0.013
Mean of the dep. var.	0.802	0.340	0.0116

Table 11: Effects of Juntos on Birth Control Use and Type of Birth Control Use for Non-Poor Women¹. OLS Estimates.

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The regressions include year of interview fixed effects, phase group fixed effects, woman and husband socioeconomic characteristics. 1. The sample consists of rich women (wealth index greater than 2) in both urban and rural areas.

	Is th	he district	
	targete	d by Juntos?	
	No	Yes	Total
Is the respondent receiving Juntos?			
Yes and shows the Juntos card	16	1,230	$1,\!246$
Yes and does not show the Juntos card	59	$1,\!399$	$1,\!458$
No	2,992	2,128	$5,\!120$
Does not know	4	6	10
N	3,071	4,763	$7,\!834$

Table 12: Self-Declared Juntos Status and District Targeting.

The question about whether the respondent is receiving Juntos or not was only effectively asked during the 2009 to 2012 surveys. Moreover, the question was only asked to women with a child under 5. Accordingly, the following analysis will be performed on the subsample of women with a child under 5 interviewed between 2009 and 2012. In what follows, we classify women who answered "Yes" to that question as receiving Juntos (regardless of whether they show the card or not) and women who answered "No" as not receiving Juntos. Women who don't know are removed from the sample.

VARIABLES	(1) Resp. is receiving Juntos
	~ ~ ~
Targeted district	0.0791^{***}
	(0.026)
Constant	-0.2823***
	(0.094)
N	7,824
R-squared	0.431
F-stat	103.6

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The regressions include year of interview fixed effects, phase group fixed effects, woman and husband socioeconomic characteristics.

	(1)	(2)	(3)
VARIABLES	OLS	Reduced-form	(J) IV
Juntos	0.0290**		-0.0476
	(0.014)		(0.308)
Juntos x Husband wants more	0.0388		0.0592
	(0.024)		(0.048)
Juntos x Husband wants fewer	0.0157		-0.0098
	(0.028)		(0.049)
Targeted district		-0.0080	
		(0.025)	
Targeted district x Husband wants more		0.0305	
-		(0.024)	
Targeted district x Husband wants fewer		-0.0065	
		(0.027)	
Husband wants more	-0.0140	-0.0188	-0.0203
	(0.015)	(0.018)	(0.019)
Husband wants fewer	-0.0270	-0.0159	-0.0149
	(0.017)	(0.020)	(0.022)
Constant	0.8416^{***}	0.8373***	0.8270***
	(0.082)	(0.087)	(0.099)
	. ,		. ,
Observations	7,824	7,824	7,824
R-squared	0.077	0.076	0.072
Mean of the dep. var.	0.805	0.805	0.805

Table 14: Effect of Juntos on Using BC: OLS, Reduced Form and IV Estimates

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The regressions include year of interview fixed effects, phase group fixed effects, woman and husband socioeconomic characteristics. The F-stat for the first-stage is equal to 103.6.

	(1)	(2)	(3)
VARIABLES	OLS	Reduced-form	IV
Juntos	-0.0238		-0.4518
	(0.020)		(0.471)
Juntos x Husband wants more	-0.0252		-0.0811
	(0.032)		(0.065)
Juntos x Husband wants fewer	-0.0155		0.0014
	(0.037)		(0.074)
Targeted district		-0.0281	
		(0.037)	
Targeted district x Husband wants more		-0.0414	
		(0.033)	
Targeted district x Husband wants fewer		-0.0122	
		(0.038)	
Husband wants more	-0.0269	-0.0112	-0.0020
	(0.019)	(0.024)	(0.027)
Husband wants fewer	0.0043	0.0050	0.0115
	(0.023)	(0.030)	(0.032)
Constant	0.2824^{**}	0.3202^{***}	0.2001
	(0.112)	(0.118)	(0.143)
Ν	6,299	6,299	6,299
R-squared	0.044	0.044	
Mean of the dep. var.	0.344	0.344	0.344

Table 15: Effect of Juntos on Using Traditional Forms: OLS, Reduced Form and IV Estimates

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The regressions include year of interview fixed effects, phase group fixed effects, woman and husband socioeconomic characteristics. The F-stat for the first-stage is equal to 103.6.

	(1)	(2)	(3)
VARIABLES	OLS	Reduced form	ĪV
Juntos	-0.0154^{***}		-0.0735
	(0.005)		(0.110)
Juntos x Husband wants more	0.0581^{***}		0.0838^{***}
	(0.018)		(0.029)
Juntos x Husband wants fewer	0.0032		0.0096
	(0.011)		(0.020)
Targeted district		-0.0137	
		(0.009)	
Targeted district x Husband wants more		0.0446^{***}	
		(0.015)	
Targeted district x Husband wants fewer		0.0032	
		(0.011)	
Husband wants more	0.0401^{***}	0.0339^{***}	0.0320^{***}
	(0.008)	(0.011)	(0.011)
Husband wants fewer	0.0104	0.0093	0.0099
	(0.007)	(0.009)	(0.009)
Constant	0.0443	0.0524	0.0369
	(0.037)	(0.038)	(0.042)
Ν	$6,\!299$	$6,\!299$	$6,\!299$
R-squared	0.038	0.036	0.022
Mean of the dep. var.	0.0238	0.0238	0.0238

Table 16: Effect of Juntos on Using Hidden Forms: OLS, Reduced Form and IV Estimates

Robust standard errors clustered at the district level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The regressions include year of interview fixed effects, phase group fixed effects, woman and husband socioeconomic characteristics. The F-stat for the first-stage is equal to 103.6.

8 Appendix

						# of	women	intervie	wed in						
	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	
whose districts entered															
the program in															Total
2005	148	194	69	63	105	187	242	324	318	318	278	430	347	349	3,372
2006	216	328	196	234	278	520	679	865	768	687	743	$1,\!178$	$1,\!114$	1,034	8,840
2007	164	542	208	233	245	584	728	765	895	826	820	1,232	$1,\!104$	1,061	9,407
2010	0	40	0	0	14	27	29	21	34	34	37	40	47	38	361
2011	88	72	40	24	15	81	81	49	58	71	76	229	196	191	1,271
2012	399	464	214	173	199	531	845	769	765	808	739	1,056	933	948	8,843
2013	55	78	66	59	24	123	154	146	171	148	183	221	209	207	1,844
2014	42	76	49	33	92	103	155	138	164	179	145	185	175	184	1,720
2015	0	10	10	0	32	37	29	4	24	55	42	72	59	63	437
2016	121	138	87	74	158	305	295	278	317	299	324	485	464	452	3,797
2017	22	28	8	23	69	72	95	110	85	123	123	218	168	185	1,329
Total	1,255	1,970	947	916	1,231	2,570	3,332	$3,\!469$	$3,\!599$	$3,\!548$	$3,\!510$	5,346	4,816	4,712	41,221

Table 17: Sample Description