Contraceptive Usage and Fertility: What Happens When Doorstep Access Comes at a Price?*

Somdeep Chatterjee [†1,4], Prashant Poddar [‡2], and Shreya Mishra [§3]

[1]Indian Institute of Management Calcutta
[2]Indian Institute of Management Amritsar
[3]Indian Institute of Management Lucknow
[4]Centre for Development Economics and Sustainability (CDES)

August 12, 2022

Keywords: door-to-door delivery; contraceptive usage; fertility; family planning
JEL: I12; J13; I15; I18

Abstract

Contraceptive usage usually increases with easier access but evidently decreases as prices rise. We study a unique policy from India where home delivery of minimally priced contraceptives replaced the practice of acquiring contraceptives free of cost from village centers. Using a quasi-experimental estimation framework, we find that this intervention led to higher usage of contraceptives and lower fertility, potentially attributable to easier access. However, households substitute away from the priced modern contraception methods towards traditional or permanent forms of contraception, for which prices remained unchanged, reflecting a revealed preference towards costless contraception or high fixed-cost but low variable-cost based methods. From the perspective of health care policy, while door-to-door delivery is a disruptive innovation in the market for health care which should ideally improve convenience for consumers; the actual welfare consequences remain ambiguous due to the potentially inefficient substitution patterns resulting from a highly elastic demand for these products at very low levels of price.

*We thank Sourav Bhattacharya, Tanika Chakraborty, Chirantan Chatterjee, Shreyasee Das and Joseph Flavian Gomes for useful comments and suggestions. Any remaining errors are our own.

†Economics Group; Indian Institute of Management Calcutta, Diamond Harbor Road, Joka, Kolkata 700104, India (Email: somdeep@iimcal.ac.in), *Corresponding Author*.

‡Polytechnic Road, PO: Chheharta, G.T. Road Amritsar - 143105, India. (Email: prashantp@iimamritsar.ac.in)

§Prabandh Nagar, IIM Road, Lucknow, Uttar Pradesh 226013, India.(Email: phd21003@iiml.ac.in)

1 Introduction

According to UN estimates, around 190 million women in the age group of 15-49 have an unmet need for family planning.[1] The incidence rates for such an unmet need are significantly higher in low income - developing countries when compared to the high income - developed nations.[2] In India, the rate of modern contraception usage is a meagre 56% (DHS 2019-20) despite an increase of 9 percentage points over the previous DHS round (2015-16).[3] Clearly, there exists a massive policy space to address the issue of unmet family planning needs of the households especially in developing country contexts, such as, India (Cronin, Guilkey and Speizer 2018).

In general, policies encouraging households to adopt family planning practices can either focus on the supply side of birth control measures, such as removing barriers in access to contraception (Bailey 2006; Bailey 2010; Bailey 2012; Ashraf, Field and Lee 2014; Kelly, Lindo and Packham 2020; Mulligan 2015; Herrera-Almanzaa and Rosales-Rueda 2020; Ananat and Hungerman 2012), or on stimulating demand for contraception by subsidizing the cost of adopting contraceptive measures (Levin, Caldwell and Barkat-e Khuda 1999; Rau, Sarzosa and Urzua 2021; Molyneaux and Gertler 2000; Kearney and Levine 2009; Packham 2017). Either-way, this creates a very large financial burden on the governments, especially in developing countries. Considering the already low proportion of spending on public health in these countries, the problem of unmet demands often accentuates further. For instance, India spends only around 4% of its budget allocated to the flagship National Health Mission on family planning.[4]

In this paper, we study a unique policy experiment from India that improved the access to contraceptives through doorstep delivery while also making them marginally costlier in terms of price. The *home delivery of contraceptives* initiative was launched as a pilot by

[1] See here: https://www.un.org/development/desa/pd/sites/www.un.org.development.desa.pd/files/files/documents/2020/Jan/un_2019_contraceptiveusebymethod_databooklet.pdf

[2] See:

[3] Link to news report available here: https://indianexpress.com/article/india/up-bihar-numbers-encouraging-8-7-point-rise-in-contraception-use-in-5-yrs-nfhs-data-says-7645096/

[4] See report from 2019 here: https://www.business-standard.com/article/current-affairs/family-planning-can-boost-india-s-per-capita-gdp-13-by-2031-study-119022800114_1.html

the Government of India in 233 districts across 17 states of the country, in the year 2011. It sought to improve accessibility of contraceptives by providing home delivery of condoms, oral contraceptive pills (OCPs) and emergency contraceptive pills (ECPs) at a 'nominal' cost. The program was launched in the backdrop of largely stagnant contraceptive usage rates, especially in the socioeconomically backward states of the country.[5] [6]

The program had two competing features that makes its analysis particularly interesting to a policymaker. First, the program focused on improving the accessibility to contraceptives by providing doorstep delivery to the households. This should help as existing literature suggests that improved accessibility is linked with higher usage of contraceptives (Herrera-Almanzaa and Rosales-Rueda 2020; Luca et al. 2021), which can then result in fertility reduction. Second, while the policy improved on distance based accessibility, it also introduced a price for the home delivery of contraceptives. Furthermore, these contraceptives were no longer available, free-of-cost, from the nearest Primary Health Centre and the Sub-Centre in these 233 districts, as was the case prior to the introduction of the program. Consequently, the cost of procuring contraceptives increased for the households residing in the districts where the program was introduced. Existing literature on pricing contraceptives suggests that higher prices lead to reduced usage, which eventually result in higher fertility rates (Kearney and Levine 2009; Bailey 2012; Packham 2017; Rau et al. 2021).

Due to these two competing aspects, it is not obvious ex-ante whether the program would have increased or decreased the probability of child birth and this would likely depend on which of the two aspects discussed above dominates. The policy experiment from India, therefore, allows us to study impacts on contraception usage and fertility when a combination of these interventions is rolled out at the same time, which to the best of our knowledge has not been studied in the health economics literature. Studying the impacts of such a cocktail intervention informs the healthcare policymaker in a developing country greatly - one could potentially reap the large fertility benefits from the supply side while also gather early insights into the tradeoffs of introducing a price on access which in the longer-run may be necessary

[5] nhm.gov.in/images/pdf/guidelines/nrhm-guidelines/family-planning/home-delivery-of-condems.pdf
[6] www.main.mohfw.gov.in/sites/default/files/56324455632156323214.pdf

to help ease some of the fiscal burden.

As the *home delivery of contraceptives* program was introduced as a pilot, it serves as an interesting source of a natural experiment setting where the access to contraceptives was made easier in certain districts, but this improved access only came on a payment of a price assigned to these products. In the other districts, the free contraceptives was still available but from local health centres, as before. We use nationally representative household survey data from India, to study the impact of this pilot policy on usage of contraceptives among Indian households as well as reproductive health outcomes. Our identification strategy relies on plausibly exogenous district-time variation in rollout of the program. We are able to track the same households in a panel dataset over two rounds of the survey, one prior to the announcement of this intervention and the other post-announcement. Essentially, our empirical exercise boils down to a standard 2X2 difference-in-difference specification with respect to survey rounds. However, since the surveys were conducted over 2 years in each round and we are able to track the exact dates of the surveys, we have finer layers of temporal variation which allows us to study more meaningful effects, particularly with respect to trends in the outcome prior to the policy intervention. Specifically, we use the India Human Development Survey (IHDS) datasets from 2004-05 as well as 2011-12. The annexure to the official government letter of August 2011 provides the list of 233 chosen districts for the intervention providing us the source of cross-sectional identifying variation.

We find that in response to the policy, at the extensive margin, households are more likely to use contraception. Interestingly however, we find that households are less likely to use condoms or OCPs but are more likely to adopt alternative methods of contraception. This potentially suggests a substitution effect at the intensive margin, possibly owing to the marginally higher prices of the home delivered contraceptives, towards alternative methods of birth control not under the ambit of the intervention. On average, we do find a lesser likelihood of women being pregnant at the time of the survey as an impact of the policy, relative to the comparison group. Overall, this suggests positive welfare consequences as a reduced form general equilibrium effect of the policy, although the intervention works by way

of an indirect nudge towards adopting family planning while not directly inducing take-up of the home delivered contraceptives.

We find evidence of substitution towards two broadly different groups of alternate forms of contraception. First, a noticeable substitution towards permanent methods of birth-control such as male and female sterilization, hysterectomy etc. Second, a substitution away from modern contraception methods and towards traditional and early methods of birth-control such as abstinence and withdrawal. Although this might seem counter-intuitive at first glance, given the very little price associated with the use of condoms or OCPs under the policy, the counter-factual of paying this nominal price would be getting the contraceptive free of cost. A body of research in behavioral economics establishes the fact that individuals feel larger disutility from a marginal increase in price from zero to 1 (quite literally the case here) compared to the same marginal increase from a non-zero positive value (Shampanier, Mazar and Ariely 2007). As a result, our results are consistent with the idea that paying a marginally higher price can create a large disutility which leads to substitution towards either an alternate free method or a method with high fixed but, zero variable cost.[7]

The rest of the paper is organized as follows. Section 2 presents the background of the policy. Section 3 discusses the basic empirical framework. The data used for the analysis is discussed followed by the regression specification and identification strategy. Section 4 presents the results. Section 5 presents a bunch of robustness checks including a falsification exercise, correction for sample selection bias (panel vs repeated cross-section), controlling for other confounders such as social taboos and marriage age and duration and also accounting for geographic heterogeneity by eliminating a bunch of regional states that were not considered in the program and may have very different characteristics compared to the treated districts. Section 6 presents the conclusions.

[7]A survey-based evaluation commissioned by the Ministry of Health and Family Welfare of the Government of India was conducted by the organization, FHI 360, and in their research brief on progress in family planning they point out that the training provided to the network of accredited social health activists who would be responsible for on-the-ground implementation of the program, often focussed on the counselling component. This implies that these workers would have focussed more on the information aspect of the intervention and played an active role in creating awareness about the various methods. FHI 360 also note in their report that the supply chain for the pilot intervention had certain constraints - this helps motivate our findings on the potential substitution towards other forms of contraception, particularly in the absence of a well functioning supply chain and with an increase in information provision and awareness campaigns. The report is accessible here: `https://www.fhi360.org/sites/default/files/media/documents/contraceptives-doorsteps-india-brief.pdf`

2 Background

As part of the National Rural Health Mission (NRHM) launched in 2005, the government of India offered free supply of contraceptives such as condoms and OCPs to rural households from the primary health care centers or PHCs. This was envisaged under the broader purview of the social marketing strategies of the NRHM.[8]. On 4th August 2011, a letter was sent out to the secretaries or principal secretaries of the health and family welfare departments of 17 states in India by the national deputy commissioner of the family planning division of the government of India under the ministry of health and family welfare. [9] This letter outlined a plan for introducing *home delivery of contraceptives* including condoms, OCPs and ECPs to eligible couples in 233 chosen districts from these 17 states on a pilot basis.[10]

Prior to this intimation, these contraceptives were available free of cost from state-owned rural health care facilities known as Primary Health Centers (PHCs) and this practice would be discontinued with the introduction of the new policy of home delivery. As a fee for the home delivery, a nominal charge of INR 1 (<1.5 cents) for a 3-pack of condoms and the same for a cycle of OCPs would be recovered while INR 2 per ECP would be charged for emergency contraception demand. The structure of the initiative relied on using as well as incentivizing the network of ASHA (Accredited Social Health Activist) workers to promote the usage of contraceptives through home delivery.[11]. This was essentially a supply-side innovation where the distribution and supply chain of these contraceptives was significantly changed. Prior to the policy, the role of the state administration was much more salient in the supply chain but with the pilot intervention, manufacturers would directly send the supply to the districts where it would be received in the district stores and managed by the authorized district pharmacists. The ASHAs would then collect the supplies from the block

[8]The details on the mission can be found here: `https://nhm.gov.in/WriteReadData/l892s/nrhm-framework-latest.pdf`

[9]The pdf of the letter that was issued can be found here: `http://nhm.gov.in/images/pdf/guidelines/nrhm-guidelines/family-planning/home-delivery-of-condems.pdf`

[10]The central government of India (Ministry of Health and Family Welfare) piloted the programme in 233 districts spread across 17 states. However, the ministry has not declared any particular reasons for piloting the programme in these states. The basis for selection of the districts in which the program is being piloted has also not been declared officially. However, on average, most of the states where the program was piloted have lower educational levels and higher fertility rates (number of children per woman) compared to the states where the program was not piloted.

[11]Details about ASHAs can be found here: `https://nhm.gov.in/index1.php?lang=1&level=1&sublinkid=150&lid=226`

level health centers and supply it to the doorsteps of households based on the registers that they maintain where the list of eligible couples have been recorded in every village.

The scheme was universally introduced in all the districts of the country following the government order dated 17 December 2012. This decision was made based on reports submitted by 3 independent agencies which carried out an evaluation of the pilot project and suggested a high level of satisfaction among agents affected by the program. [12] The evaluations, however, also highlighted two potential implementation challenges that lead to some reluctance among community members to pay for the supplies. First, in some of the districts where the policy was rolled out, ASHA still reported carrying some free supply of contraceptives. Second, the packaging of contraceptives under the initiative was not very different from that of free supplies provided by the PHCs. Despite both these challenges that increased the resistance to pay under the initiative, the scheme was still considered as largely acceptable to both ASHA workers and the beneficiaries.[13]

3 Empirical Framework

In this section, we describe our empirical framework by introducing our main dataset and outlining the identification strategy used for estimating the causal effects of the doorstep delivery of contraceptive initiative on outcomes on usage of contraceptives as well as fertility. The basic premise of the study rests on the hypothesis that doorstep delivery of contraceptives enables easier access to contraception and therefore is likelier to induce more contraceptive adoption. This should also have potential effects on fertility, although higher prices compared to the counterfactual may potentially lead to reverse effects.

3.1 Data

We use the panel data version of the India Human Development Survey (IHDS) which combines two rounds of the survey, conducted in 2004-05 and 2011-12.We required a dataset

[12]This news report outlines the details of these studies: `https://www.thehindu.com/news/national/contraceptives-to-be-delivered-at-home/article4210475.ece`

[13]https://www.fhi360.org/sites/default/files/media/documents/contraceptives-doorsteps-india-brief.pdf

comprising demographic and contraceptive details of individuals. Furthermore, we needed the details of fertility, contraceptive choice, contraceptive preferences, details of STDs, and other sexual health indicators. This restricted our choice of dataset. Among the datasets, which provided these details, the datasets with more recent waves of the survey did not have sufficient overlap between the districts in which the pilot programme was implemented and on which panel data was available. Hence, we chose the IHDS panel dataset, as it covered approximately 60% of the treatment districts while also being relevant. Furthermore, there are 233 districts in which the home delivery of contraceptives were piloted, out of which the data was available for 137 districts in the IHDS panel dataset (2004-05 and 2011-12). At the time of the launch of this pilot programme (2011) there were 640 districts in India so the proportion of treated districts to total number of districts is 233/640 which is approximately 36%. Similarly, there are 384 districts, which are covered in the IHDS survey, out of which 137 districts are treated, which gives us the proportion of treated districts to total number of districts is 137/384 which is also approximately 36%.

The survey is nationally representative and reaches over 41,000 households in urban and rural neighborhoods of the country. Each round of the survey was conducted over 2 years and we are able to track the date of interview for every household. Based on these responses, we are able to identify households who were interviewed post-August 2011 in the IHDS-2 and track them back from IHDS-1 providing us with a panel dataset of households with responses before and after the program. Cross-sectional variation is generated by access to the home-delivery program which was only piloted in 233 districts as marked in Figure A1 in the appendix.

The IHDS dataset provides information on contraceptive usage at the extensive margin as well as composition of the types of contraception used, including multiple preferred methods. It also provides data on fertility outcomes. The IHDS has a host of variables which are used as demographic controls in our regressions such as household size, age, education levels etc. On average, around 68% households in the sample seem to be using contraceptives with an average of 61% reported in IHDS 1 as compared to an increase to about 74% in IHDS 2. In

the pre-policy wave of the data, the average contraception usage for households in these 233 districts appears to be around 51% which increased to 68% post-policy. This also provides a rationale for our difference-in-difference identification design as a simple means comparison between the treated and non-treated districts would likely run into selection issues. The incidence of currently pregnant women was around 5% in both rounds of the survey.

3.2 Identification Strategy

To identify the causal effects of the home-delivery initiative on contraceptive usage and fertility outcomes, we run a difference-in-difference specification as follows where the 233 pilot districts are considered the treated districts and the second wave of the survey (2011-12, post-August 2011 only) is considered the post-policy period. For household h in village v of district d and time t we estimate the policy impact on outcome Y_{hdt}:

$$Y_{hvdt} = \alpha_v + \delta \cdot Post_t + \beta \cdot HomeDeliv_{dt} + \gamma \cdot X_{hdt} + \epsilon_{hvdt} \tag{1}$$

In this equation, α_v captures village fixed-effects and $Post_t$ is a dummy variable indicating that the observation is from 2011-12 (post-August 2011). The vector X_{hdt} contains a host of household level demographic controls such as average education levels of males, females and household head, age of the household head or respondent, number of married men and women in the household, income, household size, number of adults, caste and religion identifiers. The variable $HomeDeliv_{dt}$ is an interaction dummy which takes the value 1 if district d is one of the 233 districts in the post-policy period. Consequently, β is our coefficient of interest which gives us the causal effect of the home delivery intervention on outcomes Y under the identifying assumption that in the absence of this pilot project, the differences in the mean outcomes in the 233 districts over time would be identical to these differences in the rest of the districts over time. The term ϵ captures the residuals of the regression.

There may be potential concerns about our identifying assumption and therefore to convince the readers, we performed a set of checks and balances on our identification design. The foremost consideration that we had was to check if the contraceptive usage in the pilot

districts were significantly different compared to the other districts, even without the intervention. Such an event would actually invalidate the basic foundation of our identifying assumption and give rise to concerns of endgoenous program placement. In the appendix, we present a figure where we plot the district-wise mean contraceptive usage from both rounds of the data, by year of completion of the survey and find that the treated and non-treated districts have very similar pre-intervention trends in the outcome. Additionally, we perform tests of exact randomization following Bharadwaj et al. (2014) and Chatterjee and Poddar (2021) and present the findings in a later section below.

4 Results and Discussion

We present the main results from our analysis in this section. Table 1 reports the results from the regressions of equation 1 using outcomes such as the indicator variables for contraceptive usage and current pregnancy. In column 1, we present the findings from the baseline 2X2 difference-in-difference regressions without including any controls of fixed-effects. We find that the policy seems to have a positive impact on contraceptive usage with an increase of 5.5 percentage points whereas the probability of a woman being currently pregnant is lower by 1.1 percentage points. We include demographic controls and fixed-effects in columns 2 and 3 and the results are very similar. In our most preferred specification reported in column 4 based on equation 1 we include the fixed-effects as well as demographic controls and find that the policy leads to a 6.9 percentage point increase in contraceptive usage whereas led to a 1.6 percentage point decrease in the probability of being pregnant at the time of the survey. [14]

[14]These estimates capture the "Intent-to-Treat (ITT)" effects and not necessarily the Local Average Treatment Effect (LATE). While it is true that the policy is implemented at the district level and the residents of treated districts can no longer avail of free contraception at the Primary Healthcare Centres because the service was discontinued after the pilot programme's introduction, there is no guarantee of compliance that we are able to ensure through the policy or estimate the compliance rate in our econometric specification. In other words, we cannot rule out the possibility that some people did collect the contraceptives but did not use them and many who would have wanted to use them were unable to secure it. Even as ASHA workers deliver contraceptives at the doorstep in the treated districts, it is important to observe that no home delivery of contraception is available in the control districts, and in these districts free-of-cost contraception is available at the Primary Healthcare Centres, following government guidelines. Hence there is no way to eliminate the possibility of non-compliance in the treatment and control districts.

Table 1: ***Main Results: Contraceptive Usage and Fertility***

	Dependent Variables			
	(1)	(2)	(3)	(4)
Panel A: Contraceptive Usage (=1 if Yes)				
$HomeDeliv_{dt}$	0.055 ** (0.026)	0.064 ** (0.026)	0.060** (0.026)	0.069*** (0.026)
Demographic Controls	No	Yes	No	Yes
Region Fixed effects	No	No	Yes	Yes
Observations	59,434	48,460	59,434	48,460
Mean of Dependent variable	— 0.686 —			
Panel B: Currently Pregnant (=1 if Yes)				
$HomeDeliv_{dt}$	-0.011^{**} (0.005)	-0.015^{***} (0.006)	-0.012^{**} (0.005)	-0.016^{***} (0.006)
Demographic Controls	No	Yes	No	Yes
Region Fixed effects	No	No	Yes	Yes
Observations	62,755	51,006	62,755	51,006
Mean of Dependent variable	— 0.048 —			

Notes: Each column reports a separate regression from the IHDS panel-dataset from 2004-05 and 2011-12. The estimated coefficient $HomeDeliv_{dt}$ is the effect of policy of doorstep delivery of contraceptives on outcomes of interest and is essentially the estimated coefficient of β from equation 1. Regressions using control variables include family size, age of respondent, education of male and female as well as household head, number of adult members in household, number of married individuals, household income, identifiers for rural/urban locations, identifiers for caste as well as religion. Robust standard errors clustered at the district level are reported in parentheses. *** p $<$ 0.01 **p $<$ 0.05 *p$<$ 0.1.

These results suggest that the policy on average had a positive impact at the extensive margin and doorstep delivery seemed to benefit households by way of increased adoption of family planning and lowering of fertility rates. At face value, this implies that the channel of easier access appears to be dominant compared to the nominal increase in prices, at least in terms of nudging households towards using contraception methods with exposure to the policy. However, on closer inspection, we find that the composition of contraceptive usage changed and there are very interesting substitution patterns between types of contraception that can be documented as a result of this intervention, as presented in Table A1 and Table A2. Results from Table A2 are summarized in Figure 1 where we plot the coefficients and

95% confidence intervals from regressions using dummy variables for chosen methods of contraception.

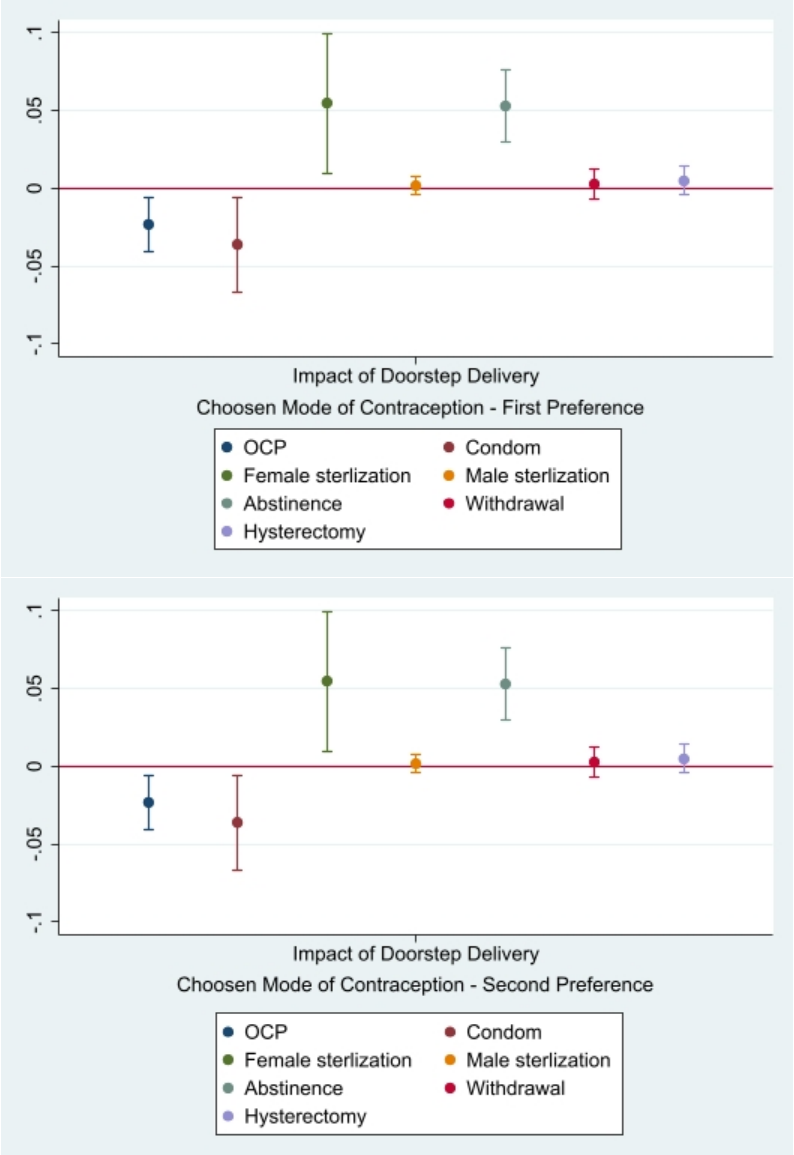

Figure 1: ***Composition of Contraceptive Choice***

Notes: The figures depict the level of usage of different types of contraceptive methods. The first panel represents the usage of the various contraceptives as the preferred method of contraception of the individual. The second panel represents the usage of the same contraceptive methods as the secondary choice of contraception of the individual.

We find that as an impact of the pilot intervention, households are less likely to choose Condoms and OCPs as their first or second preference contraception methods and are more likely to choose permanent methods such as sterilization or traditional methods which do

not have a price tag attached to them such as abstinence or withdrawal. This is suggestive evidence that the price effect of the intervention also had an impact on contraceptive choice and operationalized through these substitutions.

The general summary coming out this analysis is that of a revealed preference among households for free of cost methods of contraception and an overall sensitivity to prices. Our findings suggest that in the counterfactual with freely distributed condoms and OCPs, the usage of these contraceptives would have been higher. In the appendix, we also report results from separate regressions using the first and second preference methods of contraception as different outcome variables and results are largely similar. However, the intervention seems to have nudged individuals towards safer reproductive health practices, which is reflected by the increased overall adoption of contraceptives. This increased usage however comes from alternate forms, including permanent birth-control techniques such as sterilization.

This is consistent with the idea that households may perceive the price shock as a potential permanent increase to their variable costs of using contraception and therefore may prefer incurring a high fixed-cost instead and avoid paying variable costs in future. The detailed regression results for these substitution patterns are available in the attached appendix.

The ASHA workers who perform the doorstep delivery of contraceptives also provide counselling to the couples in their area regarding various contraceptive choices. They also get the couples in their area screened by the Master of Obstetrics/ Auxiliary Nurse Midwife before providing them with OCPs. The counselling provided by the ASHA workers seems to have impacted the fertility preferences of couples, as seen in Table 2. We observe a shift in the fertility preference of couples; the ideal number of children they wish to procreate has gone down for both male and female children.

5 Robustness

In this section, we report results from a couple of simple exercises performed to test the robustness of our empirical specification. First, we report results from simulated regressions

Table 2: *Ideal number of Children*

	Ideal number of Children (1)	Ideal number of Sons (2)	Ideal number of Daughters (3)
$HomeDeliv_{dt}$	−0.08180*	−0.03997	−0.00950
	(0.04309)	(0.03171)	(0.02220)
Demographic Controls	Yes	Yes	Yes
Region Fixed effects	Yes	Yes	Yes
R^2	0.3244	0.3214	0.1848
No. of Observations	49,259	45,303	44,892
Mean Dep Var	2.4170	1.3720	1.1169

Notes: Each column reports a separate regression from the IHDS panel-dataset from 2004-05 and 2011-12. The estimated coefficient $HomeDeliv_{dt}$ is the effect of policy of doorstep delivery of contraceptives on outcomes of interest and is essentially the estimated coefficient of β from equation 1. Regressions using control variables include family size, age of respondent, education of male and female as well as household head, number of adult members in household, number of married individuals, household income, identifiers for rural/urban locations, identifiers for caste as well as religion. Robust standard errors clustered at the district level are reported in parentheses. *** p < 0.01 **p < 0.05 *p< 0.1.

in tests of exact randomization as part of a falsification exercise. Second, we remove some of the states and union territories of India from our analysis which do not have any districts that were part of the pilot project, to see if our results are sensitive to choice of control districts. Third, we re-run our main specification with additional controls (namely, age and years of marriage), which may impact individuals' contraceptive usage and fertility choices.

5.1 Falsification Exercise

To make sure that the coefficient β in equation 1 is picking up the true effect of the intervention and not a spurious relation, we run 1000 simulated regressions by randomly assigning treatment status to the districts while preserving the original distribution and re-run our model above. Figure 2 plots the distribution of the coefficients from these simulated regressions for our main outcome variables. Re-assuringly, we find that our empirical specification passes this test of exact randomization because the coefficients from these *fake* regressions are distributed around zero, suggesting that any random assignment of treatment status does

not yield the spurious effects that may mask the main results. Also, the main point estimates from Table 1, marked by the red vertical lines, are positioned beyond the extreme tail of the distribution of these simulated coefficients. This is because the main point estimates for both our outcomes do not fall in the entire range of simulated coefficients generated from these 1000 fake regressions.

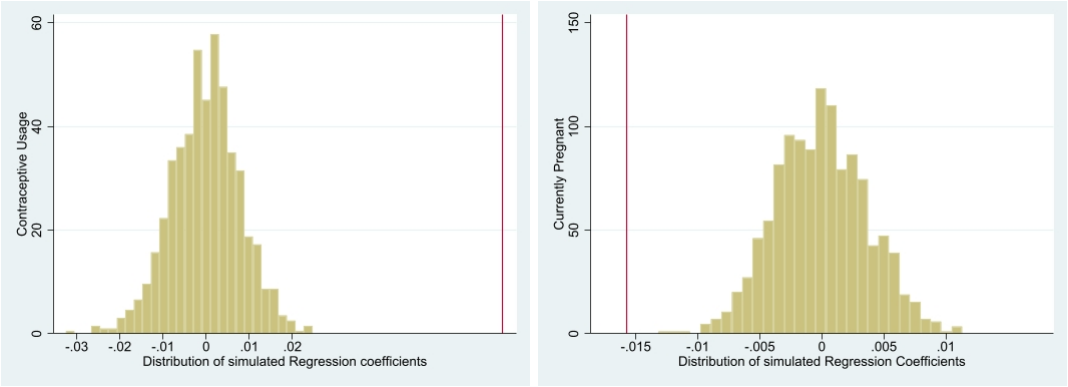

Figure 2: ***Test of Exact Randomization***

Notes:The figures present the distribution of simulated regression coefficients produced from the test of exact randomization. The panels present the tests of exact randomization for contraceptive usage and pregnancy, respectively. The vertical red lines are the regression coefficients estimated through our analysis.

5.2 Doorstep Delivery, Social Barriers and Taboos

One concern with the partial interpretation of our results as the impact of ease of access to contraception is the fact that doorstep delivery of contraceptives might be confounded with social factors which would not have been the case with access to contraception from a designated center. For instance, the doorstep delivery may compromise the secrecy of purchase or use of the product which in many societies in India is associated with taboos and constraining barriers. The fact that contraception is associated with some social barriers can be correlated with the implementation of the policy as well as our outcome variables and therefore our estimates could be confounded by these factors. To alleviate such concerns, we take advantage of the IHDS dataset which provides information on 4 interesting variables. First, if the interview being conducted for the IHDS survey was being observed by some other family member, therefore compromising privacy.

Table 3: ***Impact of Social Constructs on Contraceptive Usage and Fertility***

	(1)	(2)	(3)	(4)
Panel A: Impact on Contraceptive Usage				
Policy effect	0.057^{**}	0.069^{***}	0.070^{***}	0.074^{***}
	(0.026)	(0.026)	(0.026)	(0.026)
Demographic Controls	Yes	Yes	Yes	Yes
Region Fixed effects	Yes	Yes	Yes	Yes
Social Norm/ Cue	Observed Interview	Joint Family	Eye Contact	Confidence
No. of Observations	46,498	48,460	48,329	47,359
Panel B: Impact on Current Pregnancy				
$HomeDeliv_{dt}$	-0.017^{***}	-0.016^{***}	-0.015^{***}	-0.014^{**}
	(0.006)	(0.006)	(0.006)	(0.006)
Demographic Controls	Yes	Yes	Yes	Yes
Region Fixed effects	Yes	Yes	Yes	Yes
Social Norm/ Cue	Observed Interview	Joint Family	Eye Contact	Confidence
No. of Observations	48,852	51,006	50,864	49,817

Notes: Each column reports a separate regression from the IHDS panel-dataset from 2004-05 and 2011-12. The estimated coefficient $HomeDeliv_{dt}$ is the effect of policy of doorstep delivery of contraceptives on outcomes of interest and is essentially the estimated coefficient of β from equation 1. Regressions using control variables include family size, age of respondent, education of male and female as well as household head, number of adult members in household, number of married individuals, household income, identifiers for rural/urban locations, identifiers for caste as well as religion. Robust standard errors clustered at the district level are reported in parentheses. *** p < 0.01 **p < 0.05 *p< 0.1.

Second, if the family was a joint family where the woman resides with her husband's family in the same premises as opposed to nuclear families where the woman resides only with the spouse and kids. Third, whether the interviewee was making eye contact with the interviewer. This was a proxy for whether the interviewee was feeling shy or uncomfortable discussing these matters. Fourth, the perceived confidence level of the interviewee with her responses as evaluated by the interviewer. We include each of these variables as controls in our regressions and re-run our model.

The results are reported in Table 3. Re-assuringly, we find that our original estimates are largely unchanged and retain their levels of precision. We therefore conclude that these social constructs do not necessarily confound our main estimates of the impact of the policy on contraceptive usage and fertility rates.

5.3 Concerns about Geographic Heterogeneity

As was evident from Figure A1, many of the southern states of India of India are not part of the pilot intervention, even though our control group for regressions consider all these districts. This could be problematic particularly given the diversity of Indian states in terms of culture, demographics, religion etc. If the southern states are systematically different from the northern states and these differences change over time, implying that region fixed-effects would not be able to control for this issue, our estimates may be confounded by this generic differences and therefore we would not be picking up the true causal effect of the pilot intervention. To alleviate these concerns, we re-run our main model by dropping these states to confirm that our results and central findings are not sensitive to choice of the control group. Results are reported in Table A3.

We find from Table A3 that despite some loss of precision, the general conclusion from the analyses remains unchanged. The magnitudes are marginally lower suggesting that increase in contraception usage is roughly 5 percentage points higher due to the policy and fertility is about 1.3 percentage points lower.

We conduct another check by creating a dummy variable for the southern states $South_s$

and created an interaction variable $South_s * Post_t$; where $Post_t$ is a dummy variable indicating that the observation is from 2011-2012 (post- August 2011).[15] We re-run the main regression while controlling for the interaction variable $South_s * Post_t$. As evident from results presented in Table A4, there is a loss of precision but the results are close to our previous estimates. There is an increase of 4.8 percentage points in contraception usage and a decrease of 1.3 percentage points in fertility.

5.4 Concerns about Sample Selection Bias

As discussed in our Identification Strategy, we use the panel data version of the IHDS for our analysis. The re-interview rate between the two surveys is 85%, which means that a significant number of households were not included in the second round of the survey. The reasons for attrition are not obvious but this leads to an empirical concern regarding potential selective attrition which may result in a sample selection bias for our model. If this is the case, our point estimates could be biased and consequently the inference may not be correct. To circumvent this issue, we perform our main analysis on the repeated cross-sectional data from IHDS I and II. The results are presented in Table A5. We find that the outcomes are similar to our main findings, suggesting an increase in contraceptive usage and a decrease in fertility.

5.5 Duration and Age of Marriage and Self-Selection into Contraception

The age at which an individual gets married may have some impact on their contraceptive usage and fertility preferences (an eighteen-year-old and a thirty-year-old married woman may have significantly different fertility and contraceptive preferences). Hence, we take the age at which one got married as additional control and re-run our main specifications. Similarly, the years an individual has been married may also impact their contraceptive and fertility preferences; therefore, we take years of marriage as additional control and re-run our main specification. The estimates of these regressions are presented in Table A6; as

[15]For definition of what constitutes a "southern state", please refer to: http://interstatecouncil.nic.in/wp-content/uploads/2020/02/COMPOSITION-OF-SOUTHERN-ZONAL-COUNCIL.pdf

observed, these estimates are close to our initial estimates without losing their significance.

6 Concluding Remarks

The issue of unmet demand for family planning can be met by policymakers either through supply side interventions such as providing easy access to contraceptives through ensuring an unrestrained supply or demand side stimulations such as providing subsidies on the prices of contraceptives. Either way, these policies impose a large financial burden on governments, particularly in developing countries, who are also the most adversely affected by these issues including unwarranted rises in fertility rates. If governments were to potentially explore offsetting some of the costs of providing access to contraceptives, charging a nominal price for these contraceptives seem to be an attractive option. However, stand alone studies on price shocks to the contraceptive market suggest that adoption of these methods would adversely be impacted by increases in price.

The government of India, in 2011, introduced a unique policy which brought access to contraceptives at the doorstep of households compared to earlier centralized supply but at a price instead of providing them free of cost as before. We estimate the impacts of this policy to try and understand whether the easier access to contraception actually leads to increased adoption of contraception or whether the price hike nullifies this effect. Interestingly, we find that the policy can successfully nudge adoption of contraceptives among households although the households do respond to the price shock by substituting away towards alternate forms of contraception. Some of these alternate forms are more traditional methods which are costless in terms of monetary expenses while others are permanent methods which have high one-time expenses but zero variable costs for future.

This revealed preference of households towards freely available contraception can potentially have its roots on the institutional setting where contraceptives used to be available free of cost for long periods of time and hence provides a sense of entitlement to the households. At the same time, policy makers may find it useful that despite the introduction of nominal

prices which can offset some of the costs of providing easier access (such as transportation due to home-delivery etc), in general the adoption of contraception does not go down and is able to operate as a behavioral nudge.

On the other hand, policymakers would be concerned about the high elasticity of demand for these products at such low levels of prices. The demand appears to be highly sensitive to a marginal price change which results in substitution away from these methods towards traditional forms of contraception, which in general would be considered inefficient due to a shift from modern forms of contraception which are considered highly effective for birth control and sexual health. In general, the health policy literature seems to favor the idea of door-to-door delivery as a welfare enhancing innovation in the healthcare market, from the point of view of consumer satisfaction and convenience, despite this being a disruption in the business model of standard health care delivery in the private sector (or sometimes public sector), which usually operate through centralized outlets such as clinics or hospitals (Hwang and Christensen 2008; Ba et al 2018). Our results suggest, that despite the policy of the Indian government regarding doorstep delivery of contraception being a classic example of such innovative disruption in the business model for health care markets, the welfare consequences of such policies are not obvious, given the highly elastic demand which can lead to potentially inefficient substitution patterns.

References

1. Ananat, E. O., & Hungerman, D. M. (2012). The Power of the Pill for the Next Generation: Oral Contraception's Effects on Fertility, Abortion, and Maternal and Child Characteristics. *Review of Economics and Statistics*, 94(1), 37-51.
2. Ashraf, N., Field, E., & Lee, J. (2014). Household Bargaining and Excess Fertility: An Experimental study in Zambia. *American Economic Review*, 104(7), 2210-37.
3. Bâ EH, Pitt C, Dial Y, Faye SL, Cairns M, Faye E, Ndiaye M, Gomis JF, Faye B, Ndiaye JL, Sokhna C, Gaye O, Cissé B, Milligan P. (2018). Implementation, coverage and equity of large-scale door-to-door delivery of Seasonal Malaria Chemoprevention (SMC) to children under 10 in Senegal. *Sci Rep*, 8(1):5489.
4. Bailey, M. J. (2006). More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply. *The Quarterly Journal of Economics*, 121(1), 289-320.
5. Bailey, M. J. (2010). "Momma's Got the Pill": How Anthony Comstock and Griswold v. Connecticut shaped US Childbearing. *American Economic Review*, 100(1), 98-129.
6. Bailey, M. J., Hershbein, B., & Miller, A. R. (2012). The Opt-In Revolution? Contraception and the Gender Gap in Wages. *American Economic Journal: Applied Economics*, 4(3), 225-54.
7. Bharadwaj, P., Johnsen, J. V., & Loken, K. V. (2014). Smoking bans, maternal smoking and birth outcomes. *Journal of Public Economics*, 115, 72-93.
8. Chatterjee, S. & Poddar, P. (2021), From Safe Motherhood to Cognitive Ability: Exploring Intrahousehold and Intergenerational Spillovers. *Economica*, 88: 1075-1106.
9. Cronin, C. J., Guilkey, D. K., & Speizer, I. S. (2018). The effects of health facility access and quality on family planning decisions in urban Senegal. *Health Economics*, 27(3), 576-591.
10. Herrera-Almanza, C., & Rosales-Rueda, M. F. (2020). Reducing the Cost of Remoteness: Community-Based Health Interventions and Fertility Choices. *Journal of Health*

Economics, 73, 102365.

11. Hwang, J., & Christensen, C. M. (2008). Disruptive Innovation In Health Care Delivery: A Framework For Business-Model Innovation. Health Affairs, 27(5), 1329-1335.
12. Kearney, M. S., & Levine, P. B. (2009). Subsidized Contraception, Fertility, and Sexual Behavior. *The Review of Economics and Statistics*, 91(1), 137-151.
13. Kelly, A., Lindo, J. M., & Packham, A. (2020). The power of the IUD: Effects of expanding access to contraception through Title X clinics. *Journal of Public Economics*, 192, 104288.
14. Levin, A., Caldwell, B., & Khuda, B. E. (1999). Effect of price and access on contraceptive use. *Social Science & Medicine*, 49(1), 1-16.
15. Molyneaux, J. W., & Gertler, P. J. (2000). The Impact of Targeted Family Planning Programs in Indonesia. *Population and Development Review*, 26, 61-85.
16. Mulligan, K. (2015). Contraception use, abortions, and births: The effect of insurance mandates. *Demography*, 52(4), 1195-1217.
17. Packham, A. (2017). Family planning funding cuts and teen childbearing. *Journal of Health Economics*, 55, 168-185
18. Rau, T., Sarzosa, M., & Urzúa, S. (2021). The children of the missed pill. *Journal of Health Economics*, 79, 102496.
19. Shampanier, K., Mazar, N., & Ariely, D. (2007). Zero as a special price: The true value of free products. *Marketing science*, 26(6), 742-757.

A Appendix

A.1 Map of Treated and Control Districts

In this section, we plot the map of treated and control districts for the pilot intervention. The shaded districts were the ones included in the pilot project where the home delivery was introduced. There seems to be some regional clustering as majority of the southern districts of India seem to have been left out here. We do perform robustness checks in our paper as discussed in the text to make sure our results are not sensitive to the choice of control districts. One reason for this could be that southern states in India usually are documented to have lower rates of fertility and higher sterilization rates compared to national averages.[16]

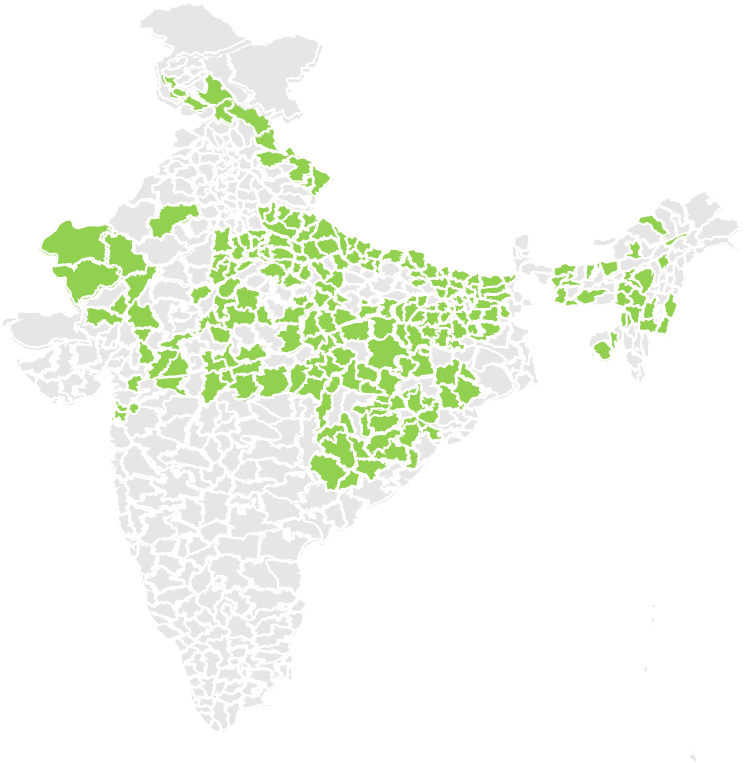

Figure A1: ***Map of India: 233 Pilot Districts Shaded***

Notes: The graph depicts the districts of India. The 233 districts, which were a part of the pilot programme for the home delivery of contraceptives, are highlighted in green, while the districts, which were not a part of the pilot programme, are depicted in grey.

[16]This article lays down the literature behind this claim as well as contributes to this premise `https://journals.plos.org/plosone/article?id=10.1371/journal.pone.0263532`

A.2 Parallel Trends

Our paper runs the following regression for household h in village v of district d and time t to estimate the policy impact on outcome Y_{hdt}:

$$Y_{hvdt} = \alpha_v + \delta \cdot Post_t + \beta \cdot HomeDeliv_{dt} + \gamma \cdot X_{hdt} + \epsilon_{hvdt} \tag{2}$$

α_v captures village fixed-effects

$Post_t$ is a dummy variable indicating that the observation is from 2011-12 (post-August 2011)

X_{hdt} contains controls such as average education levels of males, females and household head, age of the household head or respondent, number of married men and women in the household, income, household size, number of adults, caste and religion identifiers

$HomeDeliv_{dt}$ is an interaction dummy which takes the value 1 if district d is one of the 233 districts in the post-policy period

ϵ captures the error

In Figure A2, we plot the district-wise mean contraceptive usage from both rounds of the data, by year of completion of the survey.

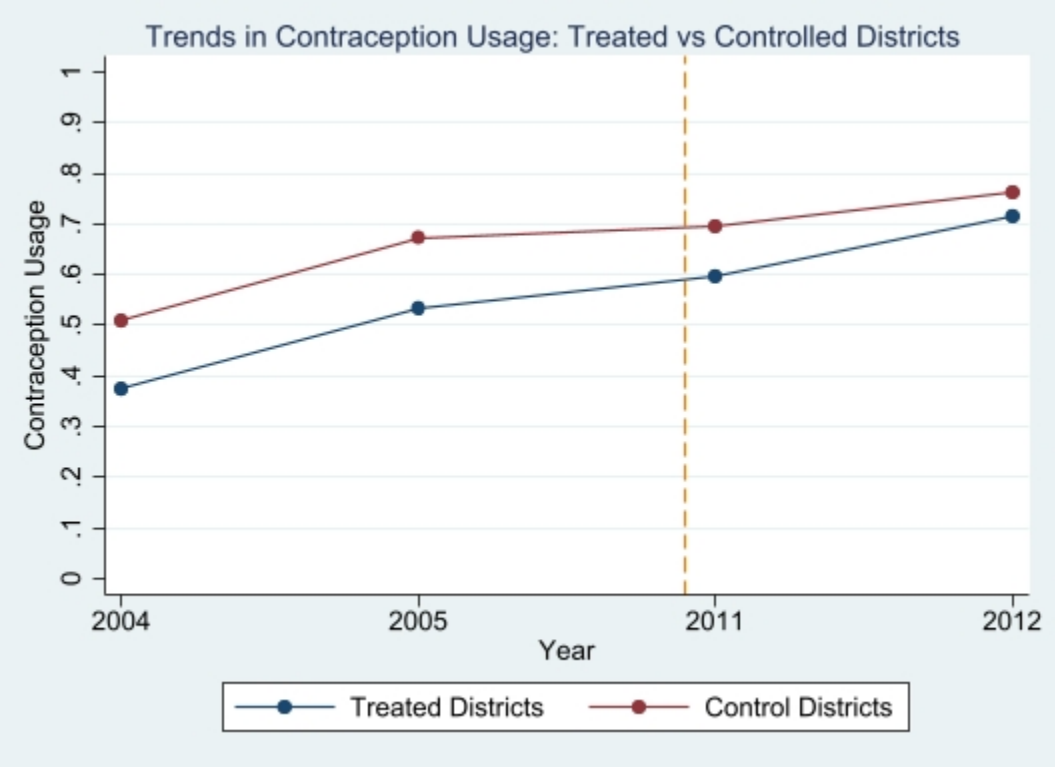

Figure A2: ***Validity of Identifying Assumption***

This figure provides support to our identiying assumption about the counterfactual, i.e., in the absence of the program, the trends in outcome for treated and non-treated districts would be similar. Since we find that from our year-wise plots, from the pre-policy period, these trends are identical, we are reasonably confident about this assumption.

A.3 Additional Results

As observed in Table 1, our main findings suggest a positive policy impact on contraceptive usage, and it has increased by 6.9 percentage points. However, the main results do not explore the impact of the policy on the composition of contraceptive usage. To highlight these patterns, we focus on the contraceptive methods which are being delivered to the doorstep under the policy, i.e., Condoms and Oral Contraceptive Pills (OCPs).

We create a dummy variable for the first preference for contraception of the individual. The dummy variable takes value 1 if the first preference for contraception is either condoms or OCPs, and the dummy variable takes value 0, otherwise. Similarly, we create a dummy

variable for the second preference for contraception of the individual, where the dummy variable takes value 1 if the second preference for contraception is either condoms or OCPs, and the dummy variable takes value of 0, otherwise. We run the regressions for both the first and second preference for contraceptive choice and present the results in Table A1. We observe a decrease of 5.9 percentage points and 1.3 percentage points in the first and second preference for condoms and OCPs. This suggests that the overall impact of the policy has reduced the usage of condoms and OCPs, due to the increased price that they now come at.

Table A1: *Condoms and OCP Usage*

Panel A: Preferred Contraception method 1

	(1)	(2)	(3)	(4)
$HomeDeliv_{dt}$	-0.05571^{***}	-0.06115^{***}	-0.05216^{***}	-0.05941^{***}
	(0.01651)	(0.01837)	(0.01686)	(0.01872)
Demographic Controls	No	Yes	No	Yes
Region Fixed effects	No	No	Yes	Yes
R^2	0.0030	0.0414	0.1335	0.1670
No. of Observations	66,172	51,854	66,172	51,854
Mean of Dependent variable		— 0.01705 —		

Panel B: Preferred Contraception method 2

	(1)	(2)	(3)	(4)
$HomeDeliv_{dt}$	-0.01323^{**}	-0.01292^{**}	-0.01347^{**}	-0.01318^{**}
	(0.00598)	(0.00619)	(0.00623)	(0.00646)
Demographic Controls	No	Yes	No	Yes
Region Fixed effects	No	No	Yes	Yes
R^2	0.0008	0.0057	0.1008	0.1108
No. of Observations	66,172	51,854	66,172	51,854
Mean of Dependent variable		— 0.00240 —		

Notes: Each column reports a separate regression from the IHDS panel-dataset from 2004-05 and 2011-12. The estimated coefficient $HomeDeliv_{dt}$ is the effect of policy of doorstep delivery of contraceptives on outcomes of interest and is essentially the estimated coefficient of β from equation 1. Regressions using control variables include family size, age of respondent, education of male and female as well as household head, number of adult members in household, number of married individuals, household income, identifiers for rural/urban locations, identifiers for caste as well as religion. Robust standard errors clustered at the district level are reported in parentheses. *** p < 0.01 **p < 0.05 *p< 0.1.

As observed in Table A1 the usage of both condoms and OCPs has reduced due to the impact of the policy. However, there is a positive impact of the policy on contraceptive usage, as presented in Table 1. Therefore, there must exist a substitution pattern in the usage of contraceptives, which offsets the reduced usage of condoms and OCPs and results in the overall increase in contraceptive usage. To investigate these substitution patterns, we run regressions for different types of contraceptive methods (namely, Oral Pill, Copper T/ IUD, Diaphragm/Jelly, Injectibles, Condoms, Female Sterility, Male Sterility, Withdrawal, Abstinence and Hysterectomy). We find that there is an increase in permanent methods of contraception, such as sterilization and abstinence, as presented in Table A2.

Table A2: ***Usage of Different Forms of Contraception Methods***

	Oral Pill	Copper T/ IUD	Diaphragm/ Jelly	Injectible	Condoms
	(1)	(2)	(3)	(4)	(5)
$HomeDeliv_{dt}$	−0.02307***	−0.00252	−0.00153	0.00097	−0.03633**
	(0.00883)	(0.00444)	(0.00125)	(0.00419)	(0.01530)
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Region Fixed effects	Yes	Yes	Yes	Yes	Yes
R^2	0.1362	0.0942	0.0567	0.0961	0.1526
No. of Observations	51,854	51,854	51,854	51,854	51,854
Mean Dep Var	0.00749	0.00313	0.00012	0.00124	0.00956

	Female Sterility	Male Sterility	Abstinence	Withdrawal	Hyster-ectomy
	(1)	(2)	(3)	(4)	(5)
$HomeDeliv_{dt}$	0.05476**	0.00161	0.05292***	0.00273	0.00484
	(0.02285)	(0.00307)	(0.01189)	(0.00472)	(0.00474)
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Region Fixed effects	Yes	Yes	Yes	Yes	Yes
R^2	0.3156	0.1705	0.1284	0.1089	0.0897
No. of Observations	51,854	51,854	51,854	51,854	51,854
Mean Dep Var	0.06837	0.00186	0.00772	0.00225	0.00182

Notes: Each column reports a separate regression from the IHDS panel-dataset from 2004-05 and 2011-12. The estimated coefficient $HomeDeliv_{dt}$ is the effect of policy of doorstep delivery of contraceptives on outcomes of interest and is essentially the estimated coefficient of β from equation 1. Regressions using control variables include family size, age of respondent, education of male and female as well as household head, number of adult members in household, number of married individuals, household income, identifiers for rural/urban locations, identifiers for caste as well as religion. Robust standard errors clustered at the district level are reported in parentheses. *** p < 0.01 **p < 0.05 *p< 0.1.

As presented in Figure A1, the pilot programme is mostly implemented in the central and northern parts of India. However, in our analysis we have taken all the districts which were not a part of the pilot programme in the control group, irrespective of the region. To make sure that our results are not sensitive to the choice of the control group, we re-run our main regressions while dropping the Southern States from the control group. We find that the results in Table A3 are similar to our previous results in Table 1, despite some loss of precision.

Table A3: ***Dropping Southern States from Control Group***

	Contraception Usage (1)	Currently Pregnant (2)
Policy effect	0.048^{*}	-0.013^{**}
	(0.028)	(0.006)
Demographic Controls	Yes	Yes
Region Fixed effects	Yes	Yes
Subsample Excludes	Southern States	Southern States
R^2	0.249	0.120
No. of Observations	38,043	39,992
Mean of Dependent variable	0.686	0.048

Notes: Each column reports a separate regression from the IHDS panel-dataset from 2004-05 and 2011-12. The estimated coefficient $HomeDeliv_{dt}$ is the effect of policy of doorstep delivery of contraceptives on outcomes of interest and is essentially the estimated coefficient of β from equation 1. Regressions using control variables include family size, age of respondent, education of male and female as well as household head, number of adult members in household, number of married individuals, household income, identifiers for rural/urban locations, identifiers for caste as well as religion. Southern states refer to the states of Andhra Pradesh, Karnataka, Kerala, Tamil Nadu and the Union Territory of Puducherry. Robust standard errors clustered at the district level are reported in parentheses. *** p < 0.01 **p < 0.05 *p< 0.1.

We perform another robustness check to uncover any underlying geographical trends. We created a dummy variable for the southern states of India, $South_s$ and created an interaction variable $South_s$ * $Post_t$; where $Post_t$ indicates that the observation is from 2011-2012 (post-August 2011). We then run our main regression, the interaction variable $South_s$ * $Post_t$ as a control. Even after controlling for geographical trends, our estimates remain roughly the same, but our estimates have a loss of precision. As evident from table A4, there is

an increase of 4.7 percentage points in contraceptive usage and a decrease of 1.3 percentage points in fertility.

Table A4: ***Southern States impact on Contraceptive Usage and Fertility***

	Contraception Usage (1)	Currently Pregnant (2)
$HomeDeliv_{dt}$	0.04749^{*}	-0.01335^{**}
	(0.02801)	(0.00610)
Demographic Controls	Yes	Yes
Region Fixed effects	Yes	Yes
R^2	0.2463	0.1217
No. of Observations	48,460	51,006
Mean Dep Var	0.68648	0.04830

Notes: Each column reports a separate regression from the IHDS panel-dataset from 2004-05 and 2011-12. The estimated coefficient $HomeDeliv_{dt}$ is the effect of policy of doorstep delivery of contraceptives on outcomes of interest and is essentially the estimated coefficient of β from equation 1. Regressions using control variables include family size, age of respondent, education of male and female as well as household head, number of adult members in household, number of married individuals, household income, identifiers for rural/urban locations, identifiers for caste as well as religion. Southern states refer to the states of Andhra Pradesh, Karnataka, Kerala, Tamil Nadu and the Union Territory of Puducherry. Robust standard errors clustered at the district level are reported in parentheses. *** p < 0.01 **p < 0.05 *p< 0.1.

As discussed in our empirical framework, we use the Indian Household Development Survey (IHDS) panel dataset for our analysis. However, the re-interview rate between the two waves of the survey is 85%, which means that a large number of households are not re-interviewed. There must have been specific reasons for which the said households were left out of the reinterview process (possibly due to relocation or non-response of the interviewee). Therefore, we are concerned about a potential sample selection bias where selective attrition may have led to spurious inference using the true panel for the datasets. To overcome this issue, we re-run our main regression on the full sample, which is a repeated cross-sectional data from IHDS I and II (2004-05 and 2011-12). We find that our results from the repeated cross-sectional data largely align with our findings from the panel data. We find an increase of 6.6 percentage points in contraceptive usage and a decrease of 1.7 percentage points in fertility. This is re-assuring that our main results are not necessarily driven by the choice of

sample.

Table A5: ***Main Results: Contraceptive Usage and Fertility***

	Dependent Variables			
	(1)	(2)	(3)	(4)
Panel A: Contraceptive Usage (=1 if Yes)				
$HomeDeliv_{dt}$	0.053 **	0.061 **	0.059**	0.066**
	(0.025)	(0.025)	(0.026)	(0.026)
Demographic Controls	No	Yes	No	Yes
Region Fixed effects	No	No	Yes	Yes
R^2	0.027	0.071	0.199	0.245
Observations	65,655	53,303	65,655	53,303
Mean of Dependent variable	— 0.682 —			
Panel B: Currently Pregnant (=1 if Yes)				
$HomeDeliv_{dt}$	-0.011^{**}	-0.016^{***}	-0.012^{**}	-0.017^{***}
	(0.005)	(0.006)	(0.005)	(0.006)
Demographic Controls	No	Yes	No	Yes
Region Fixed effects	No	No	Yes	Yes
R^2	0.002	0.068	0.048	0.117
Observations	69,280	56,069	69,280	56,069
Mean of Dependent variable	— 0.048 —			

Notes: Each column reports a separate regression from the IHDS repeated cross-sectional dataset from 2004-05 and 2011-12. The estimated coefficient $HomeDeliv_{dt}$ is the effect of policy of doorstep delivery of contraceptives on outcomes of interest and is essentially the estimated coefficient of β from equation 1. Regressions using control variables include family size, age of respondent, education of male and female as well as household head, number of adult members in household, number of married individuals, household income, identifiers for rural/urban locations, identifiers for caste as well as religion. Robust standard errors clustered at the district level are reported in parentheses. *** p < 0.01 **p < 0.05 *p< 0.1.

We also addressed the potential concern of the impact of age of marriage on contraceptive usage and pregnancy. We re-ran our main regression with additional controls of the age of the individual at marriage (as the age of marriage in the Indian context varies a great deal), and it may impact the usage of contraception as well as fertility preferences of couples. We find that the results are close to our previous estimates; as seen in Table A6, there is an increase of 7.0 percentage points in contraceptive usage and a decrease of 1.6 percentage

points in fertility. As there may exist patterns of fertility based on the years the couple has been married, we re-ran our main specification with additional control of years of marriage. The estimates from these regressions are close to our initial estimates, with an increase of 7.0 percentage points in contraceptive usage and a decrease of 1.6 percentage points in fertility.

Table A6: ***Impact of age and years of marriage on Contraceptive Usage and Fertility***

	Contraception Usage (1)	Currently Pregnant (2)
Panel A: Controlling for Age of Marriage		
Policy effect	0.070^{***}	-0.016^{***}
	(0.026)	(0.006)
Demographic Controls	Yes	Yes
Region Fixed effects	Yes	Yes
R^2	0.248	0.123
No. of Observations	48,423	50,968
Mean of Dependent variable	0.686	0.048
Panel A: Controlling for Years of marriage		
$HomeDeliv_{dt}$	0.070^{***}	-0.016^{***}
	(0.026)	(0.006)
Demographic Controls	Yes	Yes
Region Fixed effects	Yes	Yes
R^2	0.248	0.119
No. of Observations	48,409	50,950
Mean Dep Var	0.687	0.048

Notes: Each column reports a separate regression from the IHDS panel-dataset from 2004-05 and 2011-12. The estimated coefficient $HomeDeliv_{dt}$ is the effect of policy of doorstep delivery of contraceptives on outcomes of interest and is essentially the estimated coefficient of β from equation 1. Regressions using control variables include family size, age of respondent, education of male and female as well as household head, number of adult members in household, number of married individuals, household income, identifiers for rural/urban locations, identifiers for caste as well as religion. Robust standard errors clustered at the district level are reported in parentheses. *** $p < 0.01$ **$p < 0.05$ *$p< 0.1$.