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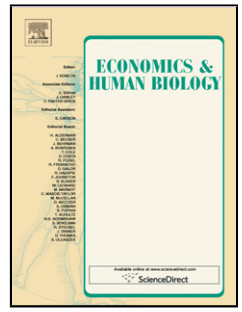

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How Much Do Conditional Cash Transfers Increase the Utilization of Maternal and Child Health Care Services? New Evidence from Janani Suraksha Yojana in India

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Abstract

Janani Suraksha Yojana (safe motherhood scheme, or JSY) provides cash incentives to marginal pregnant women in India conditional on having mainly institutional delivery. Using the fourth round of district level household survey (DLHS-4), we have estimated its effects on both intended and unintended outcomes. Our estimates of average treatment effect on the treated (ATT) from propensity score matching are remarkably higher than those found in previous prominent studies using the second and third rounds of the survey (DLHS-2 and DLHS-3). When we apply fuzzy regression discontinuity design exploiting the second birth order, our estimates of local average treatment effect (LATE) are much higher than that of ATT. For example, due to JSY, institutional delivery increases by around 16 percentage points according to ATT estimate but about 23 percentage points according to LATE estimate.

Keywords: Janani Suraksha Yojana, Demand-side financing, Propensity score matching, India
JEL: I12, I15, I18

1. Introduction

One of the crucial challenges for policymakers of public health is to increase the utilization of maternal and child health care (MCHC) services (Ki-Moon, 2010). For many years, they have tried to increase that solely by increasing the supply of hospitals, doctors, nurses and machines. However, increasing their supply is necessary but not sufficient under circumstances where many destitute women are financially unable to access MCHC services (Bhatia et al., 2006; Bhatia and Gorter, 2007; Ensor and Cooper, 2004; Koblinsky et al., 2006; Thaddeus and Maine, 1994). Over the last three decades, policy focus has shifted to increase the demand for MCHC services by increasing the purchasing power of poor people. Since the 1990s, several developing countries have started conditional cash transfer programs, traditionally called demand-side financing (DSF) programs, to increase the purchasing power of disadvantaged women (Ensor et al., 2017; Yang et al., 2016; Kuwawenaruwa et al., 2016; Kingkaew et al., 2016; Engineer et al., 2016; Skiles et al., 2015).

Policymakers are interested to know to what extent a DSF program plays a role in increasing the utilization of MCHC services, as limited research studies, carried out in several developing countries, have produced mixed findings (e.g., from no effect to high effect). For example, using regression discontinuity design, De Brauw et al. (2011) found that El Salvador's DSF program, Comunidades Solidarias Rurales, increased skilled attendance at birth and institutional delivery by 12.5-17.8 and 15.3-22.8 percentage points respectively, but had no effect on the uptake of antenatal and postnatal care services. A randomized

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controlled trial (RCT) in examining the impact of conditional cash payments to Honduran women, found that such payments increased the coverage of antenatal care and well-child check-ups by 15-20 percentage points, but had no effect on Measles and tetanus toxoid immunization (Morris et al., 2004). In another experimental study, Barber and Gertler (2008) claimed that the Mexican conditional cash transfer program, Oportunidades, increased the uptake of antenatal care service among rural women by 12.2 percentage points. Propensity score matching (PSM) produced 10.7-17.8 percentage points increase in the utilization of the facility birth in Nepal due to the Safe Delivery Incentive Programme (Powell-Jackson and Hanson, 2012). The Maternal Health Voucher Scheme in Bangladesh increased the utilization of at least three antenatal care services by 13-13.7 percentage points and institutional delivery by 10.3-11 percentage points while simple logit regressions were run (Ahmed and Khan, 2011).

In this study, we evaluate one of the world's largest DSF programs, called Janani Suraksha Yojana (safe motherhood scheme or JSY), which was launched in 2005 in India and currently covers more than 10 million women annually (MoHFW, 2017). Its main objective is to reduce maternal and child mortality as, despite steady progress, India still has high maternal and child mortality rates. It mainly works to increase the number of institutional deliveries as a way to acheive the objective, as maternal and child mortality rates are higher among mothers delivering at home than among those delivering at health institutions. MCHC services are insufficient in the case of deliveries at home, especially as emergency obstetric care services are absent there. Its financial incentives also cover at least three antenatal care (ANC) services including tetanus (TT) injection and iron folic acid (IFA), and at least one postnatal care (PNC) service for a mother and her newborn. The program has drawn the attention of researchers globally and has been central to gathering evidence on the effectiveness of a DSF program. Previous studies have examined its effects on the directly intended outcomes such as institutional delivery, ANC and PNC (Powell-Jackson et al., 2015; Carvalho et al., 2014; Gopalan and Durairaj, 2012; Gupta et al., 2011; Gupta et al., 2012; Lim et al., 2010; Modugu et al., 2012), the indirectly or ultimate intended outcome – child mortality (Sengupta and Sinha, 2018; Lim et al., 2010), and also a few unintended outcomes such as breastfeeding and childbirth or pregnancy (Nandi and Laxminarayan, 2016; Powell-Jackson et al., 2015) and immunization (Carvalho et al., 2014).[1]

Although there are several studies evaluating JSY, to the best of our knowledge, only five studies (Nandi and Laxminarayan, 2016; Powell-Jackson et al., 2015; Lim et al., 2010; Carvalho et al., 2014; Sengupta and Sinha, 2018) dealt with the causality issue, by using PSM, inverse propensity score weighting, instrumental variables (IV) regressions, district level Differences-in-Differences (DID) and the nationally representative data. The first four studies used data from the repeated cross-section surveys – second and third rounds of District Level Household Survey (DLHS-2 and DLHS-3) surveyed in 2002-2004 and 2007-2008 respectively, and Sengupta and Sinha (2018) used only DLHS-3. Both surveys were conducted in each state of India, but DLHS-2 happened before the implementation of JSY, and DLHS-3 collected data when JSY was in the initial stages of implementation and had not yet fully matured. Although JSY was launched in 2005, its budget was released in 2006 and proper implementation was started in 2007 (Das et al., 2011). Many women in DLHS-3, who gave birth before 2007, were therefore classified as JSY beneficiaries when they were not (Das et al., 2011). They were probably beneficiaries of other small-scale programs which were run at state level (Das et al., 2011). As the treatment group included many untreated women, those previous studies produced intent-to-treat (ITT) estimates, which have downward biases. As

[1]Institutional delivery, ANC and PNC can be considered as directly intended outcomes as the program, JSY, directly works to increase the utilization of institutional delivery, ANC and PNC services. As the program's ultimate goal is to reduce maternal and child mortality by increasing the utilization of institutional delivery, ANC and PNC services, child mortality can be considered as the ultimate or indirectly intended outcome. As the program does not work to increase child birth and immunization, they can be considered as unintended outcomes. The program has spillover effects on them.

only the treatment group suffered from measurement errors in the treatment dummy, misclassifications were not random. In this situation, even instrumental variables regression will not be able to remove such biases. There were also selection biases, as many women did not know about the program at that time. PSM or district level DID cannot remove endogeneity/selection biases. DID can only remove district level heterogeneity. It cannot eliminate selection biases due to time-variant unobserved factors.

In this study, we have made several contributions to the existing literature. Firstly, we use DLHS-4, surveyed in 2013-2014, when JSY had matured and had been rolled out in all parts of India. Moreover, DLHS-4 collected data on women who gave birth from 2008, and all deliveries fell into the time of JSY's proper implementation; hence there is less chance of systematic misclassifications of untreated women as treated. Thus, we expect that unlike DLHS-2 and DLHS-3, DLHS-4 does not produce much deflated causal effects. However, DLHS-4 also has a downside. Unlike DLHS-2 and DLHS-3, DLHS-4 is not a nationally representative survey. It collected data from only low focus states (also called high performing states).[2] Hence our analysis is neither nationally representative nor externally valid for non-DLHS-4 states. It should be noted that around two-thirds of Indian states are low focus states where JSY's coverage was higher in DLHS-4 than that in DLHS-3. Our results might be considered almost nationally representative. Besides, some high performing states such as Nagaland and Tripura are very similar in socio-economic conditions and the utilization of MCHC services to low performing states. We also produce state wise results, and our results for Nagaland and Tripura can be very close to results for low performing states. We think that severe misclassifications of untreated women as treated can be a bigger problem than not having nationally representative data. For example, the true treatment effect of JSY on an outcome is a, and the nationally representative data, DLHS-3, has produced the treatment effect as b, but DLHS-4 has produced it as c. As misclassifications of untreated women as treated produce downward biases of treatment effects, $a - b$ will be positive. JSY is the universal program in low performing or high focus states, and it has a greater effect on outcomes in these states than that in low focus states (Lim et al., 2010). As a is the weighted average of treatment effects for two types of states, $a - c$ will also be positive. Our point is that $a - b$ might be greater than $a - c$.

Secondly, we have used the PSM method rigorously. In addition to several other robustness checks (e.g., randomization, overlapping and sensitivity analyses), we have checked endogeneity of JSY dummy using Mantel and Haenszel (Mantel and Haenszel, 1959) non-parametric test statistics. We have found no evidence of endogeneity. However, such tests and the national roll-out of the program do not guarantee the absence of endogeneity of the JSY dummy, as there can be self-selection biases or other selection biases. There are always difficulties in finding suitable instruments. We tried fuzzy regression discontinuity (FRD) designs (e.g., IV regressions) exploiting the changes of eligibility for JSY at mother's age 19 and birth order 2. The discontinuity in the probability of treatment is found statistically insignificant when we consider the age cutoff. Also, around the cutoff, there are few observations. We, therefore, ignore this cutoff for FRD design. On the other hand, around the birth order cutoff, all observations are available, and the cutoff produces discontinuity dummy and its interaction with the assignment variable as strong instruments. We apply RFD design exploiting the birth order cutoff as a further robustness check, which is our third contribution.

Fourthly, we have estimated the effects of JSY on all relevant intended outcomes such as any ANC service, institutional delivery, any PNC service for mother and any PNC service for baby, and a long list of unintended outcomes under the continuum of MCHC services such as individual ANC services (e.g., blood pressure check, hemoglobin test, urine test etc.), individual PNC services (e.g., abdomen

[2]If the rate of institutional delivery is less than 25% in a state, that is called low-performing state; otherwise, high-forming state.

examination, breastfeeding advice, advice on infant diarrhoea etc.) and individual immunizations for baby (e.g., BCG, Polio, DPT etc.). It should be noted that we consider individual ANC and PNC services as unintended outcomes, as unlike TT injection and IFA, they are not directly targeted by JSY.

Fifthly, we have also estimated heterogeneous effects of JSY by time, state, wealth and health institution. Because of economic growth, over time the utilization of MCHC services may increase among the control women, and therefore JSY's effects may reduce over time. Compared to advanced states, JSY may have higher effects on backward states where the utilization of MCHC services is low. JSY should have higher effects on the lower wealth group than the higher wealth group. JSY recipients may prefer to go to the public health institutes rather than private ones because of lower costs of MCHC services, and therefore JSY should have higher effects on women taking MCHC services from the public health institutes. No other studies have estimated heterogeneous effects in such extensive forms.

After this introductory section, we discuss the program in Section 2, and Section 3 briefly explains data. Section 4 describes the methodology. Section 5 discusses key findings, and robust analyses are done in Section 6. Section 7 discusses heterogeneous effects of the program, and Section 8 concludes the study.

2. Janani Suraksha Yojana

Although India has achieved the Millennium Development Goal (MDG) on infant mortality by reducing infant deaths by two-thirds between 1990 and 2015, it still accounts for 22 percent of 6.3 million annual under-five deaths globally (Tandon, 2016). In spite of the steady decline in the maternal mortality rate (e.g., 212 per 100,000 live births in 2007-2009 to 178 per 100,000 live births in 2010-2012), it remains higher than that in some of its close competitors (Registrar General of India, 2013). On the other hand, national figures show convincing improvements, but state wise India has high variations in maternal and child mortality rates. Northern states have higher rates than southern states. For example, in 2010-2012, the maternal mortality rate was 328 per 100,000 live births in Assam whilst it was 66 per 100,000 live births in Kerala (Registrar General of India, 2013).

In the above circumstances, the prime minister of India launched JSY ("safe motherhood scheme" in English) under the National Rural Health Mission (NRHM), on 12^{th} April 2005, with the primary goal of reducing maternal and neonatal mortality. To achieve this primary goal, JSY aims mainly to increase the number of institutional deliveries. However, it provides cash to the eligible woman for not only the institutional delivery but also for three ANC services including TT injection and IFA, and at least one PNC service for her and her newborn.

There are 10 low performing states (LPS) – Uttar Pradesh, Uttaranchal, Bihar, Jharkhand, Madhya Pradesh, Chhattisgarh, Assam, Rajasthan, Orissa, and Jammu and Kashmir – where the rates of institutional delivery are low and thereby all pregnant women, who want to deliver their babies in public or accredited private institutions, are eligible for JSY. In high performing states (HPS), the program covers only socio-economically disadvantaged women up to two births whose households have the "below poverty line card", or/and, scheduled caste affiliation, or/and, tribal affiliation. Also, they have to be 19 years or older and deliver in public or accredited private health institutions.[3]

There are field workers, called Accredited Social Health Activists (ASHAs), who identify eligible women. However, their duties go beyond that (see Yojana, 2006). An ASHA also helps an eligible pregnant woman, to register for three ANCs, to obtain the necessary certificates of the eligibility for JSY, to identify a functional government health center or an accredited private health institution for referral

[3]The government withdrew birth order and age restrictions recently. However, such withdrawal does not affect our analysis, because it happened after our data collection period.

and delivery, to arrange the immunization for the newborn until the age of 14 weeks, to inform about the birth or death of the child or mother to the auxiliary nurse midwife (ANM), and to arrange a postnatal visit within 7 days of delivery. They also counsel them on having an institutional delivery, the initiation of breastfeeding the newborn within one hour of birth and its continuance till 3-6 months and family planning. They escort them to the pre-determined health center and stay there until the discharge.

In addition to regular salaries, ASHAs receive cash incentives to identify and assist eligible pregnant women. Table (1) shows cash payments to both an ASHA and a qualified pregnant woman, by state type (HPS and LPS) and location (rural and urban). In both kinds of states, both an ASHA and an eligible pregnant woman in rural areas receive higher cash payments than those in urban areas. Cash payments to an ASHA do not vary by state type, but cash payments to an eligible pregnant woman are higher in LPS than in HPS. Differences in cash payments indicate that the government wants to encourage more disadvantaged women to take up institutional delivery services.

Table 1: Scale of cash assistance under JSY (in Indian rupee (INR), and 64 INR=1 USD)

State Type	Rural Area			Urban Area		
	Mother	ASHA	Total	Mother	ASHA	Total
LPS	1,400	600	2,000	1,000	400	1,400
HPS	700	600	1,300	600	400	1,000

Source: Ministry of Health and Family Welfare Annual Report 2016-2017 (MoHFW, 2017).

Both an ASHA and an eligible pregnant woman receive their entitled cash from the health institution where the woman wants to deliver her baby. A public health institution pays the total entitled cash to the eligible woman when she visits for the first time (before delivery) to be registered for three ANC services and the institutional delivery. If she wants to deliver in an accredited private health institution, she receives at least three-fourths (3/4) of the cash entitlement in the first visit for those registrations, and the rest at the time of visit for the delivery. It should be noted that she will not receive any cash without registration. An eligible woman also receives transportation costs of at least 250 rupees, which also varies with the location and state. For caesarian section or obstetric complications, she receives additional cash up to 1,500 rupees. If she wants to deliver at home, she receives only 500 rupees. An ASHA receives her cash entitlement in two installments – first, when she visits the health institution with the pregnant woman for the first time, and second, after the PNC service taken up by the pregnant woman.

Table 2: JSY's contribution to mean total delivery cost (in INR) estimated from the self reported delivery costs of the pregnant women

Health Institution Type	Rural Area: Delivery Cost (mean)	Rural Area: JSY Contribution (%)	Rural Area: Delivered by JSY recipients (%)	Urban Area: Delivery Cost (mean)	Urban Area: JSY Contribution (%)	Urban Area: Delivered by JSY recipients (%)
Public health institutions						
Sub-health Center (SHC)	1388	50.42	3.05	1614	37.17	0.88
Primary Health Center (PHC)	2308	30.33	23.14	2104	28.52	11.19
Community Health Clinic (CHC)	2663	26.29	8.44	2460	24.39	8.03
United Health Care (UHC)	1453	48.18	1.62	3171	18.92	3.10
Sub-district/District Hospital (SDH/DH)	3161	22.15	47.43	2783	21.56	60.49
AYUSH Hospital/Clinic	3000	23.33	0.09	2000	30.00	0.06
Private health institutions						
Dispensary/Clinic	6850	10.22	0.39	5000	12.00	0.25
Private Dispensary/Clinic	10008	6.99	0.51	9938	6.04	0.63
Private Hospital	8138	8.60	7.81	7738	7.75	12.77
Private AYUSH Hospital/Clinic	3000	23.33	0.06	4500	13.33	
Non Government Organisation (NGO)	2017	34.71	0.18			0.13

Source: DLHS-4. It should be noted that DLHS-4 collected data from only HPS.
Note: To get JSY's contributions (in percents) in the rural area, we have divided 700 (JSY's cash transfer to the rural woman in HPS) by averages of total delivery costs and then multiplied by 100. Similarly, we have divided 600 (JSY's cash transfer to the urban woman in HPS) by averages of total delivery costs in the urban area and then multiplied by 100, to get JSY's contributions (in percents) in the urban area.

It should be noted that JSY does not pay the cost of delivery, it just gives the entitled cash to a pregnant woman. As a part of out-of-pocket (OOP) expenditures, she has to expend that money on her own for the delivery purpose. In general, her total OOP expenditures for the delivery are higher than the cash she received from JSY. One can be interested to know how much JSY contributes to the overall delivery cost (e.g., OOP expenditures) faced by a pregnant woman. In Table (2), we report the average total delivery cost (in INR) estimated from the self-reported total delivery costs (e.g., out of pocket expenditures) of pregnant women, by health institution type and location (rural-urban). In general, mean delivery costs in the public health institutions are lower than those in private health institutions, and therefore, JSY's contribution to the mean delivery costs and delivery rates of JSY women are higher in government facilities than in private ones. This scenario is the same in both rural and urban areas. The mean delivery costs are least in the Sub-health Center, which is the lowest public health institution where health facilities are inadequate. On the other hand, the delivery rates among JSY recipients are the highest in sub-district or district hospitals (e.g., 47.43% in rural areas and 60.49% in urban areas), as they are the top public health institutions, located in sub-district or district towns. However, compared to other public health institutions, contributions of JSY to the mean delivery costs in these hospitals are low (e.g., 22.15% in rural areas and 21.56% in urban areas). These numbers indicate that JSY may not only increase the demand for institutional delivery services but also increase the demand for the quality of care.

3. Data

We have used data from the fourth (latest) round of a repeated cross-section survey named as district level household survey (DLHS-4). However, it was surveyed in 2013-2014, on only eighteen high-performing states, Andhra Pradesh, Arunachal Pradesh, Goa, Haryana, Himachal Pradesh, Karnataka, Kerala, Maharashtra, Manipur, Meghalaya, Mizoram, Nagaland, Punjab, Sikkim, Tamil Nadu, Telangana, Tripura, West Bengal, and three high-performing union territories, the Andaman and Nicobar Islands, Chandigarh, and Puducherry, while the previous rounds of that survey collected data from all parts of India. As we mentioned in the introduction, we have not used data from those rounds.

The DLHS-4 collected socioeconomic data from 378,487 households and their members, but for collecting data on the utilization of MCHC services it surveyed 76,847 women (sample units of this study) in the age group of 15-49 years whose last births happened in 2008 and onward. As proper implementation of JSY started in 2007, all of those women should be included in our analyses. However, all women did not respond to all survey questions used in this study. For example, in some MCHC outcomes, around 42,370 women responded. We also exclude women, who were recipients of other schemes, because those schemes might have different eligibility criteria and different benefit packages. The number of excluded women varies from 3,000 to 3,764 in different variables, but results of treatment effects differ only after third or fourth decimal points.

Table (A.1) in Appendix A shows means of covariates, for JSY women (column (1)) and nonJSY women (column (3)). These covariates are used as explanatory variables in the logit regressions of JSY dummy, which are used to do PSM. The first three covariates on poverty status, scheduled caste status and tribal status are dummy variables, which are the key selection/eligibility criteria set by the JSY administrators. Mother's (e.g, mother means a woman, who is our data unit) age and parity are continuous variables, which are both eligibility criteria and self-selection criteria. Wealth index of a woman's household is also a continuous variable, which is mainly a self-selection criterion.[4] As the poverty status has a high correlation with wealth index, it can also be an eligibility criterion. Mother's education and husband's education are also continuous variables and self-selection criteria. Hindu religion and rural location are dummies, which are also self-selection criteria. Column (5) of the table shows differences in those means between two groups of women, and column (6) shows p values of those differences. As p values are less than 0.0001 in all cases except tribal dummy, mean differences of covariates except tribal dummy are statistically and significantly different from 0 at 1% significance level. Statistically significant mean differences imply that treated women are poorer than control women. We hope that after PSM, those differences will be statistically insignificant. It should be noted that we have also used dummies for years of last deliveries and state dummies as explanatory variables in the logit regressions of JSY dummy, to control time effects and state heterogeneity respectively. We have not reported their summary statistics here.

Table (A.1) in Appendix A also shows observations of the treatment group (in column (2)) and the control group (in column (4)). Although both treated and control observations vary with covariates, treated women remain at around 21% of total observations. Sample sizes largely fall in the cases of mother's education and then her husband's education. As any regression takes common samples of dependent and independent variables, if we ignore these two covariates in logit regressions, sample sizes largely increase, and results of treatment effects also largely increase. We therefore divide samples into two types such as:

Sample 1: It takes all covariates in Table (A.1) in Appendix A, and birth year dummies and state dummies.

Sample 2: It ignores mother's education and her husband's education from Sample 1.

We compare results between these two sample groups. A substantial fall of control observations (e.g., more than 3,000) is seen in the case of scheduled caste affiliation. That fall makes changes in results of treatment effects after third or fourth decimal points. Besides, scheduled caste affiliation is one of the

[4]Wealth index is constructed by applying principal component analysis over a list of wealth of household – cooking fuel, house type, number of dwelling rooms, electricity, house ownership, landholding, radio, television, computer, internet, telephone, mobile phone, washing machine, refrigerator, sewing machine, watch, bicycle, motorcycle, car, tractor, tube well, cart and air cooler.

main eligibility criteria, so we include it in logit regressions. Slight variations of observations in other covariates do not make any changes in results of treatment effects.

In Table (A.2) in Appendix A, we have presented summary statistics of MCHC services, outcome variables, in a similar way to that in Table (A.1) in Appendix A. All variables are dummy variables, except Days of first breast feeding, which is after how many days of birth a mother started breastfeeding her child. The first four variables are intended outcomes included in the JSY benefit package, and JSY should have direct positive effects on them. We see statistically significant differences in means of them between JSY and nonJSY recipients, as p values are less than 0.0001. These imply that JSY might have positive effects on them. Similarly, we expect that JSY has positive effects on all individual ANC services, PNC services for mother, PNC services for baby except Days of first breast feeding and immunizations for baby. We expect negative mean difference in the case of Days of first breast feeding, as a JSY recipient is expected to start breastfeeding the child earlier than a nonJSY recipient. We also see negative mean differences in advice on infant diarrhoea and pneumonia, which are unexpected. We should not fully rely on the results presented here, as mean differences cannot be considered as causal effects.

Table (A.2) in Appendix A also shows treatment and control observations, which are not the same in each outcome. However, in the case of five immunization dummies (BCG, POLIO, first POLIO, DPT, and Measles), observations are remarkably lower than that of all other outcomes. We also divide outcomes into two types, such as:

Outcome type 1: Excluding BCG, POLIO, first POLIO, DPT and Measles, it consists of all other outcomes in Table (A.2) in Appendix A.

Outcome type 2: It consists of BCG, POLIO, first POLIO, DPT and Measles.

Although to estimate treatment effects we run separate logit regressions for each outcome, logit regressions' results will remain almost the same in an outcome type, as sample sizes are similar. The above divisions of outcomes help us to show logit regressions' results, covariates balancing and overlap conditions in generalized ways. Otherwise, we are supposed to show them for each outcome, and such shows would make the paper clumsy as we have a long list of outcomes.

4. Methodology

4.1. Identification of Average Treatment Effect (ATE) and Average Treatment Effect on the Treated (ATT)

Let y_1 be the outcome of a woman (e.g., a binary variable of having institutional delivery) with a treatment status (e.g., with JSY) and y_0 the outcome with a control status (e.g., without JSY). It should be noted that a woman cannot be in both statuses in a single point of time, and we assume that the treatment of woman i affects only the outcome of woman i (e.g., Stable Unit Treatment Value Addition (SUTVA) assumption). However, for the time being, we consider that both y_1 and y_0 are available for randomly selected woman i in a single point of time. Then, one way of estimating the treatment/causal effect is the average treatment effect (ATE) defined as,

$$\tau_{ate} \equiv E(y_1 - y_0), \tag{1}$$

which averages across all women including those who did not receive JSY benefits.

Now, let w be a binary treatment indicator, where $w = 1$ denotes treatment status (e.g., with JSY) and $w = 0$ the control status (e.g., without JSY). Then, another way of estimating causal effect is the average treatment effect on the treated (ATT) defined as,

$$\tau_{att} \equiv E(y_1 - y_0 | w = 1), \tag{2}$$

which is the mean effect for those who actually participated in the program. Although ATE and ATT are generally different, they are equivalent in some special cases.

Now, we assume that treatment depends on observables. Let X denote a vector of observed covariates. To identify ATE, the following assumption should be satisfied,

Assumption 1 (ignorability): *Conditional on X, w and (y_0, y_1) are independent.*

The above assumption is called ignorability or unconfoundedness or simply conditional independence. It implies that a woman given her socioeconomic status does not tell a lie about her outcomes with and without treatment statuses, to get a selection from the program. It has a better chance of holding when the set of control variables, X, is richer. From this point, the violation of the assumption implies that X did not include all possible control variables, and there is a chance of an endogeneity problem (e.g., endogeneity of w as some variables are left unobserved in the error term). However, another source of violation occurs when one includes variables in X that can themselves be affected by treatment. The ignorability assumption implies the following ignorability in mean assumption,

Assumption 1′ (ignorability in mean): *(a) $E(y_0|X, w) = E(y_0|X)$; (b) $E(y_1|X, w) = E(y_1|X)$.*

If assumption 1′ is satisfied, ATE and ATT conditional on X are identical. More precisely, define

$$\tau_{ate}(X) = E(y_1 - y_0|X) \tag{3}$$

$$\tau_{att}(X) = E(y_1 - y_0|X, w = 1) = E(y_1 - y_0|X). \tag{4}$$

And, from the iterated expectation, we can write unconditional ATE and ATT as

$$\tau_{ate} = E[\tau_{ate}(X)] = \tau_{att}. \tag{5}$$

If assumptions 1 and 1′ are satisfied, we can identify ATE and ATT in equations (3) and (4). To identify them in equation (5), in addition to assumptions 1 and 1′, we will require being able to observe both control and treated women at every value of X. In other words, the following overlap assumption needs to be held,

Assumption 2 (overlap): *For all X, $0 < p(w = 1|X) < 1$,*

where $p(w = 1|X)$ is called the propensity score, and the assumption implies that the propensity score can never be zero or one.

4.2. Estimation of ATE and ATT using Propensity Score Matching (PSM)

If the above identification conditions are satisfied, we can estimate consistent estimators of τ_{ate} and τ_{att} in several ways, such as simple regression adjustment, propensity score methods and matching methods. Although there is no clear superiority of one method over others, we use here a popular matching method named as propensity score matching (PSM).

For each woman i, we can impute values for the counterfactuals, y_{i0} and y_{i1}, using matching. Let $\hat{y}_{i0}$ and $\hat{y}_{i1}$ denote the imputed values, and $\hat{y}_{i0} = y_i$ when $w_i = 0$ and $\hat{y}_{i1} = y_i$ when $w_i = 1$. But when $w_i = 0$, $\hat{y}_{i1}$ should be imputed by taking the closest match(es) from the treated units, and vice versa. After having imputed values, we can estimate τ_{ate} and τ_{att} by simply using the mean difference method as follows,

$$\hat{\tau}_{ate} = N^{-1}\sum_{i=1}^{N}(\hat{y}_{i1} - \hat{y}_{i0}) \tag{6}$$

$$\hat{\tau}_{att} = N_1^{-1} \sum_{i=1}^{N} w_i (y_i - \hat{y}_{i0}), \tag{7}$$

where N is the number of both treated and control women and N_1 is the number of only treated women.

Abadie and Imbens (2006) suggested to find the closest match(es) based on covariates, X, but Rosenbaum and Rubin (1983) proposed to find the closest match(es) based on propensity scores estimated from running a logit model. However, matching based on propensity scores complicates the estimation of the bootstrapped standard errors. A different function of the propensity score, such as the log-odds ratio, reduces the complication. We use the log-odds ratio to do matching and estimate bootstrapped standard errors. A matched counterfactual is chosen based on the nearest neighbor or the M number of the nearest neighbors of the log-odds ratio. We choose the third nearest neighbors option. However, in the robust analysis section, we check the sensitivity of our main results for different matching methods and different sets of covariates in X. It should be noted that we only estimate ATTs in all cases, not ATEs.

Matching is motivated by the thought experiment. It is just the sample analog of the thought experiment. For example, we draw a value X from the distribution of covariates in the population. Then, for the given X, we randomly draw a treated woman and a control woman, and thus we estimate $\hat{y}_{i1}$ and $\hat{y}_{i0}$. In this way, for all randomly selected X values, we estimate $\hat{\tau}_{ate}$ and $\hat{\tau}_{att}$. Thus, given the above identification conditions, a matching method can give us estimates of $\hat{\tau}_{ate}$ and $\hat{\tau}_{att}$ close to that found in experimental studies.

Although the above identification conditions are difficult to test, we test them to some extent by using the conventional ways. For example, we test the ignorability condition by using Mantel and Haenszel non-parametric test statistics (Mantel and Haenszel, 1959), which suggest that the condition is satisfied. However, we do not rule out the chance of endogeneity of w_i. To control the endogeneity problem, we use the fuzzy regression discontinuity design (FRDD) as a further robustness check. We also test the overlap condition. Simple density plots of the propensity scores of treatment and control women imply that both groups of women do not sufficiently overlap in each value of X. To keep those women who overlap, we drop some observations outside of $0.10 < p(w_i = 1|X) < 0.90$, as suggested by Crump et al. (2009). We also check whether biases of covariates between the treated and control units become insignificant after matching.

5. Results

For each outcome, we run two separate logit regressions of JSY dummy (1 if a woman participated in JSY program, 0 if not) on covariates under samples 1 and 2 and then use the third nearest neighbors of log-odds ratios, estimated from two logit regressions, to impute counterfactual outcomes and estimate two ATTs utilizing the formula in equation (7). An estimation of ATT takes the common observations of the outcome variable and all variables used in the logit regression. Therefore, an estimation of the logit regression also considers those common observations. If the number of common observations varies, results of the logit model with the same specification may differ. As some of our outcomes have different sample sizes, different logit regressions for them may produce different results. We do not show pairs of logit regressions (using two sets of covariates in samples 1 and 2) for all outcomes as they are too numerous. However, as different outcomes in an outcome group have the same or close sample sizes, separate logit regressions using the same covariates produce identical or similar results. We choose institutional delivery from outcome type 1 and BCG from outcome type 2 as representative outcomes of two outcome groups so that we can show four representative logit models for two outcome groups. In Appendix Table (A.3), we report four logit regressions with changes in log-odds ratios due to changes in covariates excluding state dummies and birth year dummies. We see that, in each logit regression, two main selection criteria (poverty and scheduled caste) have the highest effects on the log-odds ratios, as expected. In the cases of all other reported covariates, changes in log-odds ratios also have expected signs and magnitudes. Although most of the reported covariates' parameters do not show clear patterns among four logit regressions, poverty effects become stronger with the increase in sample size in those regressions. In the following subsections, the effects of JSY on outcomes listed in Table (A.2) are discussed.

5.1. *Main Outcomes Under the JSY Benefit Package*

Table (3) shows results of ATTs for four main outcomes, with bootstrapped standard errors, by samples 1 and 2. All results are statistically significant, and estimates of ATTs are higher in sample 2 than in sample 1. With the increase in sample sizes, estimates of ATTs become closer to raw estimates (e.g., mean differences) shown in Table (A.2) in Appendix A. The main reason for increasing ATTs with the increase in sample sizes is that the proportion of poorer women rises in the control group and thereby the means of outcomes of the control group fall. As expected, JSY has the highest positive effects on institutional delivery. JSY increases institutional delivery by 12.5 percentage points with sample 1 and 15.7 percentage points with sample 2. The second highest effects are estimated on at least one PNC for mother, which are 10.1 percentage points with sample 1 and 12.2 percentage points with sample 2, as the values of ATTs are 0.101 and 0.122 respectively. The third and fourth highest effects are estimated on at least one ANC and at least one PNC for baby respectively. It should be noted that Powell-Jackson et al. (2015) got a statistically insignificant effect on at least three ANC services, but we get statistically significant effects on at least one ANC. We also got statistically significant effects on at least three ANC services, which are 0.100 with sample 1 and 0.115 with sample 2 (not shown here). These results are clearly higher than those found by Powell-Jackson et al. (2015). Our other results are also remarkably higher than those found by Powell-Jackson et al. (2015). These imply that the previous study got downward biased results, as we mentioned earlier.

Table 3: Effects of JSY on the utilization of main MCHC services

	Sample 1			Sample 2		
	ATT	Bootstrap S.E.	*N*	ATT	Bootstrap S.E.	*N*
At least one ANC	0.079***	(0.004)	54,659	0.101***	(0.005)	68,531
Institutional delivery	0.125***	(0.005)	54,656	0.157***	(0.005)	68,527
At least one PNC for mother	0.101***	(0.006)	54,655	0.122***	(0.006)	68,527
At least one PNC for baby	0.070***	(0.006)	54,588	0.086***	(0.006)	68,429

Note: We impute values of the above four outcomes of the counterfactual groups using third nearest neighbors of log-odds ratios estimated from the logit regressions of JSY dummy on covariates under sample 1 and sample 2. We then estimate ATTs for these outcomes applying the formula in equation (7). Bootstrapped standard errors are in parentheses. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

5.2. *ANC Services*

Table (4) shows values of ATTs for individual MCHC services by samples 1 and 2. We estimate them with bootstrapped standard errors in a similar way to the previous four outcomes in Table (3). All ATTs are statistically significant, and ATTs with sample 2 are higher than those with sample 1. If we look at individual ANC services, we see that JSY has the highest effects on IFA and TT injection (e.g., their values of ATTs are 0.104 and 0.097 with sample 1, and 0.125 and 0.117 with sample 2). They are not surprising results, as IFA and TT injection are included in the JSY benefit package. They are intended outcomes, but all other individual MCHC services are unintended outcomes (e.g., not directly targeted by JSY). Large increases are also seen in the uptake of weight measure, blood pressure check, haemoglobin test, blood group test, abdomen examination and urine test due to JSY, as their values of ATTs are reasonably high. We see the relatively lower increases in the uptake of height measure, breast examination and ultrasound. One can say that women have a high demand for those ANC services, which include crucial medical check-ups at the time of pregnancy.

5.3. PNC Services for Mother

The second panel of Table (4) shows results of ATTs for individual PNC services for mother by samples 1 and 2. In each sample group, ATTs are close to each other and have moderate sizes. However, as we see in Table (A.2), the utilization of PNC services for mother is very low without a program like JSY. If we consider this fact, the role of JSY in uplifting their usage cannot be regarded as great. As we have seen in ANC services, their utilization has already been very high with no program, and JSY further improves their usage at rates, which can be appreciated. Abdominal examination has the same values of ATTs in both PNC and ANC. Advice on breastfeeding and baby care may seem to be PNC services for baby, but they can also be considered as a part of PNC services for mother. The most important point is that JSY increases advice on family planning, which may increase the actual utilization of family planning services. This finding is opposite to the findings from Powell-Jackson et al. (2015) and Nandi and Laxminarayan (2016) who found that JSY increases the childbirth or pregnancy rate. It may be that childbirth increases in the short term, but it may ultimately decrease in the long term.

5.4. PNC Services for Baby

By samples 1 and 2, the results of ATTs for PNC services for baby, with bootstrapped standard errors, are shown in the third panel of Table (4). Due to JSY, measurement of baby's weight at the time of birth increases by 10.6 and 13.6 percentage points (under samples 1 and 2 respectively), which can be regarded as high. As expected earlier, JSY has a negative effect on days of first breastfeeding. JSY recipients start breastfeeding their newborns 0.088 and 0.086 days (under sample1 and sample 2 respectively) earlier than the control women. Advice on infant diarrhoea and pneumonia now show the expected positive signs, as opposed to negative raw differentials in Table (A.2) in Appendix A.

5.5. Immunizations for Baby

Child immunizations are entirely outside of JSY's benefits package. JSY is supposed not to have any effect on them. However, field workers or ASHAs encourage JSY recipients to take immunizations for their babies. Further, women may be educated about vaccinations when they come to health facilities for institutional deliveries. We can then expect positive effects of JSY on immunizations, but such results will be low. By samples 1 and 2, estimates of ATTs for immunizations for baby, with bootstrapped standard errors, are shown in the fourth panel of Table (4). JSY has relatively higher effects on the usage of Hepatitis-B (7.6 and 9.4 percentage points under samples 1 and 2 respectively), Vitamin-A (7.2 and 8.0 percentage points under samples 1 and 2 respectively) and first polio within two weeks of birth (4.7 and 6.0 percentage points under samples 1 and 2 respectively), but low effects on the usage of BCG, Measles, DPT and polio.

Table 4: Effects of JSY on the utilization of individual MCHC services

	Sample 1			Sample 2		
	ATT	Bootstrap S.E.	*N*	ATT	Bootstrap S.E.	*N*
ANC services						
Weight measured	0.089***	(0.005)	54,622	0.110***	(0.005)	68,491
Height measured	0.062***	(0.008)	54,622	0.069***	(0.006)	68,491
Blood pressure checked	0.093***	(0.006)	54,622	0.114***	(0.005)	68,491
Blood tested (haemoglobin)	0.088***	(0.007)	54,622	0.108***	(0.006)	68,491
Blood tested (blood group)	0.088***	(0.006)	54,622	0.099***	(0.006)	68,491
Urine tested	0.090***	(0.006)	54,622	0.107***	(0.005)	68,491
Abdomen examined	0.083***	(0.008)	54,622	0.091***	(0.008)	68,491
Breast examined	0.044***	(0.005)	54,622	0.048***	(0.006)	68,491
Ultrasound done	0.058***	(0.007)	54,622	0.072***	(0.007)	68,491
Iron Folic Acid tablet/syrup	0.104***	(0.008)	54,659	0.125***	(0.006)	68,531
At least one tetanus injection	0.097***	(0.005)	54,650	0.117***	(0.005)	68,521
PNC services for mother						
Abdomen examined	0.083***	(0.006)	54,650	0.090***	(0.007)	68,517
Advice on breastfeeding	0.085***	(0.006)	54,650	0.089***	(0.007)	68,517
Advice on baby care	0.078***	(0.005)	54,650	0.085***	(0.007)	68,517
Advice on Family Planning	0.076***	(0.007)	54,650	0.081***	(0.006)	68,517
PNC services for baby						
Weight taken at birth	0.106***	(0.004)	54,586	0.136***	(0.004)	68,427
Days of first breastfeeding	-0.088***	(0.012)	54,579	-0.086***	(0.011)	68,418
Advice on infant diarrhoea	0.038***	(0.007)	54,648	0.041***	(0.007)	68,517
Advice on infant pneumonia	0.034***	(0.005)	54,654	0.034***	(0.005)	68,526
Immunizations for baby						
BCG	0.024***	(0.004)	30,366	0.026***	(0.003)	38,326
Polio	0.020***	(0.004)	30,368	0.016***	(0.003)	38,327
First Polio in two weeks of birth	0.047***	(0.008)	30,371	0.060***	(0.007)	38,330
DPT	0.037***	(0.007)	30,366	0.043***	(0.007)	38,326
Measles	0.037***	(0.007)	30,365	0.045***	(0.006)	38,325
Hepatitis-B	0.076***	(0.006)	54,326	0.094***	(0.005)	68,091
Vitamin-A	0.072***	(0.007)	54,332	0.080***	(0.006)	68,096

Note: We impute values of the above outcomes of the counterfactual groups using third nearest neighbors of log-odds ratios estimated from the logit regressions of JSY dummy on covariates under sample 1 and sample 2. We then estimate ATTs for these outcomes applying the formula in equation (7). Bootstrapped standard errors are in parentheses. * p<0.05, ** p<0.01, *** p<0.001.

6. Robust Analysis

6.1. Whether Covariates are Balanced

If covariates are not randomly distributed between treatment and control groups, estimates of ATTs in the previous section are not valid. Randomization means that covariates are balanced between two groups. Covariates balancing usually means that means of covariates in treatment and control groups are statistically the same. However, a good way of checking covariates balancing is to check whether percentage biases in covariates between two groups are near zero or between -10 and +10 (see Thomas, 2003). In Figure (A.1) in Appendix A, we plot percentage biases in all covariates including state dummies and birth year dummies, between two groups. After running each representative logit model in Table (A.3) in Appendix A, we estimate biases in covariates used in that logit model, with and without matching. Thus, we plot eight sets of biases in the cases of four logit models. Biases usually vary between matched and unmatched cases. They may also be different in four logit models because of large differences in sample sizes. In each case/subfigure, the horizontal axis represents biases and the vertical axis represents covariates but they are not seen because of the long list, and black filled dots present percentage biases in the unmatched case, and crosses indicate percentage biases after matching. In each subfigure, we see that percentage biases drastically reduce after matching, and they are now close to zero, and also fall between -10 and +10, so we can say that covariates are balanced between the treatment and control groups, and our results in the previous section are not biased due to a covariates balancing issue.

6.2. *Whether the Overlap Condition is Satisfied*

The validity of estimates of ATTs can be questioned again if the overlap condition is violated. When an estimated density of the propensity score (pscore), $Pr(JSY = 1|X)$, estimated from the logit regression of JSY dummy on covariates (X), has too much mass around 0 or 1, then there can be an indication of the violation of the condition (see Busso et al., 2014). After running each logit model in Table (A.3) in Appendix A, we estimate propensity scores for treatment and control groups. In Figure (A.2) in Appendix A, we plot their densities in the cases of four logit models. The long dash-dot lines represent the treatment group, and the solid lines represent the control group. In the case of the control group, the solid lines have too much probability mass near 0. We can say that there is evidence of violating the overlap assumption.

Table 5: Effects of JSY on the utilization of main MCHC services with reduced samples (the propensity score$\geq$ 0.10)

	Sample 1			Sample 2		
	ATT	Bootstrap S.E.	N	ATT	Bootstrap S.E.	N
At least one ANC	0.076***	(0.004)	43,260	0.097***	(0.004)	54,352
Institutional delivery	0.127***	(0.005)	43,257	0.147***	(0.005)	54,349
At least one PNC for mother	0.103***	(0.006)	43,258	0.121***	(0.007)	54,350
At least one PNC for baby	0.067***	(0.007)	43,202	0.082***	(0.006)	54,271

Note: We impute values of the above four outcomes of the counterfactual groups using third nearest neighbors of log-odds ratios estimated from the logit regressions of JSY dummy on covariates under sample 1 and sample 2. We then estimate ATTs for these outcomes applying the formula in equation (7). Bootstrapped standard errors are in parentheses. * p<0.05, ** p<0.01, *** p<0.001.

To satisfy the overlap assumption, Crump et al. (2009) have suggested dropping observations if the propensity score is below 0.10 and above 0.90 and to estimate treatment effects again with reduced samples. If we look at Figure (A.2), almost all propensity scores are less than 0.80. So, by samples 1 and 2, we re-estimate ATTs for only main outcomes if the propensity scores, estimated from two representative logit models for outcome type 1, are greater than or equal to 0.10. Table (5) shows re-estimated values of ATTs for main outcomes, with reduced samples. Results are very close to those shown in Table (3). These imply that the overlap condition is not severely failed in the cases of samples 1 and 2.

6.3. *Whether the Unconfoundedness/Ignorability Condition is Satisfied*

Violation of the unconfoundedness/ignorability assumption means the existence of an endogeneity problem and thus implies that our results in the previous section are biased. There is a chance of an endogeneity problem, as some women were selected for JSY based on unobserved factors. For example, some women probably had a good connection with ASHAs or field workers, and therefore, they were well aware of the program and its selection procedures.

Although unconfoundedness is untestable from the statistical ground, there are some ways to get a flavor of whether it exists in data. Rosenbaum et al. (1987) suggested dividing the control group into two groups. One control group is the group of eligible non-participants, and another control group includes ineligibles. If average values of outcomes are statistically and significantly different between these two groups, then this assumption might be violated. In our case, we consider those women in the control group as the eligible non-participants whose households have a below poverty line card or scheduled caste affiliation or tribal affiliation, and who have had less than three births. All other women in the control group are considered as ineligibles. We make a dummy variable by giving 1 to the eligible non-participants, and 0 to the ineligibles. If we run separate OLS regressions of all outcomes on this dummy, we get the coefficients of the dummy, which are differences in means of outcomes between two control groups (e.g., raw differentials). In Table (A.4) in Appendix A, we are supposed to present raw differentials. However, eligible non-participants and ineligibles are likely to have different socio-economic characteristics, and there is a chance that differences in mean outcomes between the two control groups are due to differences in mean covariates between two groups.

We run separate OLS regressions of outcomes on that dummy of two control groups plus all of our covariates in samples 1 and 2. Columns (1) and (4) of Table (A.4) in Appendix A show the coefficients of the dummy, and p values estimated from robust standard errors are in columns (2) and (5) of the table. We see that, after controlling covariates, means of most of the outcomes are statistically and significantly different between two control groups. Thus, it can be said that there is a chance of violating unconfoundedness assumption in the cases of both samples.

Aakvik (2001) and Rosenbaum (2002) proposed the bounding approach to check unonconfoundedness. For binary outcomes, they suggested the Mantel and Haenszel non-parametric test statistics (Mantel and Haenszel, 1959). In Table (A.5) in Appendix A, for four main intended outcomes, by samples 1 and 2, we present the upper bounds, Q_{mh+}, with the assumption of the overestimation of the treatment effects due to unobserved factors and the lower bounds, Q_{mh-}, with the assumption of the underestimation of the treatment effects due to unobserved factors, with their associated p values of statistical significance, p_{mh+} and p_{mh-}, at different levels of γ. p values imply whether we can reject the assumptions of overestimation and underestimation. In most of the cases, both p_{mh+} and p_{mh-} have zero values, which imply that we can reject the null hypotheses of both overestimation and underestimation due to unobserved factors. The bounding approach says that there is no strong evidence of violating the unconfoundedness/ignorability assumption. This is also true for higher values of γ, which are not shown.

6.4. Whether Results are Sensitive to the Different Specifications of the Logit Model

Now, we check whether ATTs for four main outcomes vary to the different specifications of the logit model (Dehejia and Wahba, 1999). We consider five sets of covariates, named as covariates1, covariates2, covariates3, covariates4 and covariates5, and we run five separate logit regressions of JSY dummy using these five sets of covariates. Covariates1 includes the main selection criteria – whether a household has a below poverty line card or schedule caste affiliation or tribal affiliation, and birth order – which were set by the program administrators. Covariates2 includes covariates1 plus some self-selection criteria listed in Table (A.1), such as, current age of mother, wealth index of mother's household, whether woman's religion is hindu, and whether woman lives in a rural area. In covariates3, state dummies are added with covariates2, and birth year dummies are added with covariates3 to make covariates4, which is basically sample 2. Covariates5 includes covariates4 plus district dummies. It should be noted that Powell-Jackson et al. (2015) and Nandi and Laxminarayan (2016) considered district level heterogeneity in their analyses.

We also check here whether our results change due to the district level fixed effect. None of the five sets of covariates includes mother's education or her husband's education, as they drastically drop observations. Here we are interested in showing how much results are changed due to different specifications of the logit model, given closer sample sizes in those specifications.

In Table (6), for main outcomes, estimates of ATTs under the different choices of covariates in the logit regression are presented. ATTs are estimated here in the same way as in the previous section, but for the different specifications of the logit model. Results here are comparable with Table (3)'s results under sample 2, because of closer sample sizes. For the first set of covariates in covariates1, results are very different from those in Table (3) (also shown in column (4) of Table (6)). In the case of the second set of covariates in covariates2, results are slightly higher than we found before. In the case of the last three sets of covariates, the results are similar (see columns (3), (4) and (5) of the table). They imply that district-level heterogeneity or time effects do not affect our results much. In other words, our previous results are robust.

Table 6: Effects of JSY on the utilization of main MCHC services, under different specifications of the logit regression

	Covariates1 (1)	Covariates2 (2)	Covariates3 (3)	Covariates4 (4)	Covariates5 (5)
At least one ANC	0.024	0.127***	0.099***	0.101***	0.082***
	(0.051)	(0.004)	(0.004)	(0.004)	(0.004)
	[N=68,567]	[N=68,543]	[N=68,543]	[N=68,531]	[N=68,522]
Institutional delivery	0.382***	0.177***	0.156***	0.157***	0.147***
	(0.058)	(0.005)	(0.005)	(0.005)	(0.006)
	[N=68,563]	[N=68,539]	[N=68,539]	[N=68,527]	[N=68,518]
At least one PNC for mother	-0.002	0.127***	0.120***	0.122***	0.115***
	(0.065)	(0.006)	(0.005)	(0.005)	(0.006)
	[N=68,563]	[N=68,539]	[N=68,539]	[N=68,527]	[N=68,518]
At least one PNC for baby	0.141**	0.091***	0.087***	0.086***	0.074***
	(0.059)	(0.005)	(0.005)	(0.005)	(0.005)
	[N=68,465]	[N=68,441]	[N=68,441]	[N=68,429]	[N=68,420]

Note: Estimates of ATTs are shown with bootstrapped standard errors in parentheses and numbers of observations in brackets. * p <0.10, **p <0.05, ***p <0.01. Covariates1 includes program administrators' selection criteria – poor, schedule caste, tribe, birth order. Covariates2 includes covariates1 plus self-selection criteria excluding woman's education and her husband's education. In covariates3, state dummies are added with covariates2, and birth year dummies are added with covariates3 to make covariates4. Covariates5 includes covariates4 plus district dummies.

6.5. Whether Results are Sensitive to the Different Choices of Matching Methods

We check here whether estimates of ATTs are sensitive to the different choices of matching techniques, such as one-to-one matching (first nearest neighbor), radius matching, kernel matching, local linear regression matching and spline matching (Nandi and Laxminarayan, 2016). We keep the specification of the logit model the same as we specified using covariates in sample 2 in the previous section or covariates4 in the previous subsection. In Table (7), for main outcomes, we see that the results are not very sensitive to the different matching techniques.

Table 7: Effects of JSY on the utilization of main MCHC services, under different matching techniques

	One-to-One (1)	Radius (2)	Kernel (3)	Local linear regression (4)	Spline (5)
At least one ANC [N=68,531]	0.100***	0.125***	0.105***	0.101***	0.101***
	(0.004)	(0.002)	(0.003)	(0.003)	(0.003)
Institutional delivery [N=68,527]	0.154***	0.165***	0.154***	0.155***	0.155***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
At least one PNC for mother [N=68,527]	0.116***	0.116***	0.119***	0.121***	0.121***
	(0.006)	(0.004)	(0.004)	(0.005)	(0.005)
At least one PNC for baby [N=68,429]	0.084***	0.086***	0.085***	0.086***	0.086***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)

Note: Estimates of ATTs are shown with bootstrapped standard errors in parentheses. * p <0.10, **p <0.05, ***p <0.01. To estimate above ATTs, we use covariates in sample 2.

6.6. *Fuzzy Regression Discontinuity (FRD) Design – Instrumental Variables (IV) Regressions*

Although the Mantel and Haenszel non-parametric test statistics (Mantel and Haenszel, 1959) ensure that the unconfoundedness/ignorability assumption is not violated, there is always doubt about the endogeneity of the treatment dummy as there can always be selection bias. We apply here FRD design exploiting the policy change of JSY at birth order 2 (e.g., a woman is eligible for JSY up to her second birth). The IV technique exists when the probability of treatment is discontinuous and/or kinked at the cutoff, but the discontinuity size is less than 1 and greater than 0. It estimates the local average treatment effect (LATE) for the compliers under the local randomization and monotonicity assumptions, and therefore, estimates of LATEs can be very different from estimates of ATEs and ATTs (Imbens and Angrist, 1994).

Before going to further analysis, Table (8) shows the observations we have at each birth order. We see that most of the women belong up to the second birth order, and observations drastically fall from 27,256 at birth order 2 to 11,352 at birth order 3, implying the violation of the local randomization assumption (McCrary, 2008). The zero birth order should not exist here because the survey was conducted on those women who gave birth before the survey. This implies that there are measurement errors in birth order. The FRD design corrects biases of treatment effects raised from these errors. We produce results considering the birth order from 1 to 7, and in this way, some observations are lost. Although this range is chosen on an ad-hoc basis, our results do not vary significantly with different bandwidths. For discrete forcing variable, the methods for choosing the optimal bandwidth, such as cross-validation method of Ludwig and Miller (2007) and squared error method of Imbens and Kalyanaraman (2012), do not work.

In Figure (1), we plot the probability of participating in JSY against the birth order. Scatter points are means of JSY dummy at different birth orders. The line is estimated from the local linear regression allowing different slopes at either side of the cutoff, 2. The line and scatter points imply that there is a little discontinuity but a clear kink in the probability of participating in JSY at the cutoff. Similarly, we plot four main outcomes in Figure (2). They do not show any remarkable discontinuities, but they have similar kinks at the cutoff as we found kinks in the probability of participating in JSY. Causal relationships between JSY and those outcomes exist mainly through kinks, not through jumps. We should not rule out that kinks in outcomes may happen due to kinks in covariates (excluding birth order), used in the previous analyses, not kinks in JSY. We have found that there are kinks in most of the covariates (not shown here), which again imply the violation of the local randomization assumption. In this situation, we have to control these covariates to get unbiased results.

Table 8: Number of observations in each birth order

Birth order	Freq.	Percent	Cum.
0	12	0.02	0.02
1	26,201	36.09	36.11
2	27,256	37.55	73.66
3	11,352	15.64	89.30
4	4,399	6.06	95.36
5	1,863	2.57	97.92
6	862	1.19	99.11
7	334	0.46	99.57
8	170	0.23	99.80
9	78	0.11	99.91
10	45	0.06	99.97
11	8	0.01	99.98
12	6	0.01	99.99
13	2	0	100
14	2	0	100
17	1	0	100
Total	72,591	100	

Note: We show discontinuity and kink in the probability of participating in the program, JSY, at birth order 2. Scatter point in each birth order is the mean of JSY dummy. From birth order 1 to 7, line is estimated from the local linear regression allowing different slopes in either side of the cutoff. We clearly see that JSY has both (small) discontinuous and kinked relationship with birth order.

Figure 1: Jump and kink in the probability of participating in the program, JSY, at birth order 2

Note: We show discontinuities and kinks in the probabilities of having at least one ANC service, institutional delivery service, at least one PNC service for mother and at least one PNC service for baby, at birth order 2. Scatter points in each birth order are means of those respective variables. From birth order 1 to 7, lines are estimated from local linear regressions allowing different slopes in either side of the cutoff. We clearly see that those four variables have kinked relationship (rather than discontinuous) with birth order.

Figure 2: Jumps and kinks in the probabilities of having four main MCHS services, at birth order 2

We consider both jumps and kinks when we run regressions. In Table (9), we first run local linear (OLS) regressions of JSY dummy and four outcomes on z=1[Birth order$\leq$2], $z\times$ (Birth order-2) and (Birth order-2) within birth order 1 and 7. The goodness of fit test suggested by Lee and Card (2008) does not allow us to consider any polynomial term of (Birth order-2). The coefficients of z and $z\times$ (Birth order-2) from local linear regressions, which are estimates of discontinuities and kinks respectively, are shown under the 'without covariates' option. Other coefficients from the regressions are not shown. The size of discontinuity in the probability of participating in JSY is 0.069, but apart from institutional delivery and PNC for mother, those outcomes have almost zero and statistically insignificant discontinuities. In the cases of four outcomes, the sizes of kinks (changes in slopes) are much higher than those of jumps, but in the case of JSY, the jump size is slightly higher than the kink size. All kinks are statistically significant. When we add all other covariates under sample 2 in the local linear regression of JSY dummy, the size of discontinuity in the probability of participating in JSY increases from 0.069 to 0.087, but the size of kink remains almost the same. After adding those covariates in the local linear regressions of four outcomes, discontinuities in outcomes remain almost the same in most of the cases, but kinks in outcomes dramatically fall implying that covariates explain most of the kinks in outcomes.

Table 9: Estimated results of discontinuities, kinks and LATEs exploiting birth order 2

	JSY (1)	At least one ANC (2)	Institutional delivery (3)	At least one PNC for mother (4)	At least one PNC for baby (5)
Without covariates					
OLS regression					
z=1[Birth order$\leq$2]	0.069***	0.006	0.029**	0.019**	0.020
	(0.008)	(0.006)	(0.010)	(0.007)	(0.012)
$z\times$ (Birth order-2)	0.051***	0.056***	0.062***	0.060***	0.067***
	(0.004)	(0.004)	(0.006)	(0.004)	(0.007)
N	72,260	72,267	72,263	72,263	72,164
IV regression					
E(JSY=1|Birth order-2)		0.593***	0.814***	0.720***	0.800***
		(0.112)	(0.091)	(0.097)	(0.120)
N		72,260	72,256	72,256	72,157
With covariates					
OLS regression					
z=1[Birth order$\leq$2]	0.087***	-0.002	0.025**	0.019***	0.019*
	(0.012)	(0.005)	(0.007)	(0.003)	(0.008)
$z\times$ (Birth order-2)	0.049***	0.026***	0.008	0.001	0.005
	(0.006)	(0.002)	(0.006)	(0.003)	(0.006)
N	68,220	68,226	68,224	68,222	68,127
IV regression					
E(JSY=1|Birth order-2)		0.196**	0.227***	0.144***	0.173***
		(0.078)	(0.030)	(0.021)	(0.015)
N		68,220	68,216	68,216	68,121

Note: Using FRD design, we estimate discontinuities and kinks in the probabilities of receiving JSY, at least one ANC service, institutional delivery service, at least one PNC service for mother and at least one PNC service for baby, at birth order 2. The coefficients of dummy instrument, z, and the coefficients of its interaction with the forcing variable, $z\times$ (Birth order-2), are estimates of discontinuities and kinks respectively. They come from local linear regressions of reduced form equations of JSY dummy and dummies for four main outcomes, within birth order 1 and 7. The coefficients of E(JSY=1|Birth order-2) are LATE estimates, which come from second stage regressions of those outcomes. We see that when we add all other covariates under sample 2, in addition to (Birth order-2), in regressions, LATE estimates dramatically reduce. Robust standard errors estimated after the adjustment of birth order clusters are in parentheses. * p <0.10, **p <0.05, ***p <0.01.

Now, we consider the local linear (OLS) regression of JSY dummy on z=1[Birth order$\leq$2], $z\times$ (Birth order-2) and (Birth order-2) as the first stage regression where z=1[Birth order$\leq$2] and $z\times$ (Birth order-2) are instruments, and we take the predicted value of JSY dummy, E(JSY=1|Birth order-2), from that regression and use it in the second stage (OLS) regressions where those four outcomes are run on E(JSY=1|Birth order-2) and Birth order-2. We show only the coefficients of E(JSY=1|Birth order-2), which are estimates of LATEs, under the 'without covariates' option. They are very high, as they are biased by kinks in covariates. After adding covariates under sample 2 in both the first and second stage regressions, estimates of LATEs become reasonable, as now they are corrected from the effects of those covariates. Compared to almost all previous estimates of ATTs, they are much higher. For example, in the case of institutional delivery, estimates of ATTs (under sample 2) were around 16 percentage points, but now the estimate of LATE (with covariates option) is 22.7 percentage points. As we said earlier, the FRD design estimates causal effects for a sub-group called compliers, and it can produce higher estimates than that of ATTs. However, we do not exclude results (with covariates) from our FRD design.

7. Heterogeneous Effects

The effects of JSY can be different for different sub-groups of women. Policy makers may need to know which sub-group of women is affected by JSY, and by how much. In general, the effects of JSY vary with wealth of a woman's household (Powell-Jackson et al., 2015; Nandi and Laxminarayan, 2016), health institution type where a woman gave her last birth (Powell-Jackson et al., 2015) and state (Lim et al., 2010). They can also be changed by time because day by day knowledge about the program and health care services increases with economic development, and that can affect estimates of ATTs. In this section, estimates of ATTs for different wealth percentiles, years of last deliveries, states and places of last deliveries are shown and results for four main outcomes are presented. All ATTs are estimated by using the same specification of the logit regression and the same matching technique (e.g., third nearest neighbors) as under sample 2 option in the results section.

7.1. Effects by Wealth Percentile

Households are divided into five groups based on the five ranges of the percentile of wealth index, such as, wealth index $\leq 20^{th}$ percentile, 20^{th} percentile$<$wealth index$\leq 40^{th}$ percentile, 40^{th} percentile$<$wealth index$\leq 60^{th}$ percentile, 60^{th} percentile$<$wealth index$\leq 80^{th}$ percentile, and wealth index$> 80^{th}$ percentile. Households with wealth index $\leq 20^{th}$ percentile are considered as the poorest group, and households with wealth index $> 80^{th}$ percentile are considered as the richest group. In Figure (3), we plot estimates of ATTs for four main outcomes of these five groups of households. We can see that on average JSY's effects on these outcomes decrease as wealth increases. From the lowest wealth group (0-20) to the second lowest wealth group (20-40), results sharply decline in all cases. From the second lowest wealth group (20-40) to the middle wealth group (40-60), there are moderate declines of effects in all cases except at least one PNC for mother, but after that, both declines and increases are seen. The pattern of these results is consistent with that in Powell-Jackson et al. (2015) and Nandi and Laxminarayan (2016).

Figure 3: Effects of JSY on main outcomes by percentile of wealth index

7.2. Effects by Year of Last Birth

In Figure (4), we plot estimates of ATTs for four main outcomes by year of last (live) birth. We do not see any clear relation of ATTs with time. Probably, over time, exposures of both treatment and control women to the health facilities have increased in the same proportion, and therefore, ATTs remain stable.

Figure 4: Effects of JSY on main outcomes by year of last birth

7.3. Effects by State

JSY's effects (e.g., estimates of ATTs) have high variations across states. In Figure (5), four states – Meghalaya, Arunachal Pradesh, Nagaland, and Tripura, show the highest effects of JSY on four main outcomes. These are similar types of state located in the north-east of the country. It may be the case that health workers are highly active in those states. Another reason might be that they are relatively backward states. In advanced states, such as Kerala, Karnataka, Tamilnadu, Puducherry, and Telangana, ATTs are very low. In these states, health facilities are better than those in relatively backward states, and exposure of both treatment and control women to the health facilities is also higher than in relatively backward states. In the figure, ATTs for one state, Goa, and two union territories, Andaman and Nicobar Islands, and Chandigarh are not shown due to their small sample size.

Figure 5: Effects of JSY on main outcomes by state

Table 10: Effects of JSY on main outcomes by institution type

	At least one ANC (1)	Institutional delivery (2)	At least one PNC for mother (3)	At least one PNC for baby (4)
JSY×Sub-health Center (SHC)	0.123***	0.310***	0.152***	0.121***
	(0.016)	(0.009)	(0.022)	(0.019)
JSY×Primary Health Center (PHC)	0.104***	0.270***	0.162***	0.115***
	(0.005)	(0.004)	(0.008)	(0.007)
JSY×Community Health Center (CHC)	0.113***	0.285***	0.161***	0.148***
	(0.007)	(0.005)	(0.013)	(0.010)
JSY×United Health Center (UHC)	0.097***	0.213***	0.195***	0.151***
	(0.010)	(0.008)	(0.017)	(0.012)
JSY×Sub-district/District Hospital (SDH/DH)	0.112***	0.272***	0.155***	0.114***
	(0.004)	(0.003)	(0.006)	(0.005)
JSY×Private Hospital	0.074***	0.186***	0.130***	0.101***
	(0.007)	(0.005)	(0.013)	(0.010)
R^2	0.204	0.325	0.227	0.244
N	55,528	55,528	55,524	55,442

Note: Separate OLS regressions of the above outcomes are run on all covariates in sample 2 plus the above interactions. Robust standard errors are in parentheses. * p <0.10, ** p <0.05, *** p <0.01. Coefficients of interactions between JSY dummy and institution type are treatment effect parameters by institution type. Base categories include home deliveries and institutional deliveries in other private and NGO operated health institutions.

7.4. *Effects by Health Institution*

Here women are categorized by the place of last delivery. Using PSM technique, different estimates of ATTs are not possible for different health facilities, especially in the case of institutional delivery, because this variable does not have any variation (e.g., only 1, not any 0). We run OLS regressions of four outcomes on the interactions of JSY dummy and health institution dummies plus all covariates in sample 2. It should be noted that both OLS and PSM produce almost the same results, as we checked in our original analysis. We take interactions of JSY dummy with all types of main public health facilities such as sub-health center (SHC), primary health center (PHC), community health center (CHC), united health center (UHC) and sub-district/district hospital (SDH/DH), and private hospital. Base categories are other small public and private health facilities such as dispensary and NGO-operated health facility, and home delivery. Table (10) shows regression results of those interactions only (to note, here sample sizes are lower than before because there are missing values in the place of delivery).

If we compare results between public and private health facilities, we see that all public health facilities have higher effects of JSY than private hospitals. These results are consistent with those found by Powell-Jackson et al. (2015). JSY has the highest effect on institutional delivery at SHC (31.0 percentage points) where delivery cost is the lowest as shown in Table (2) because it provides very basic care, with no obstetric care. Although delivery cost is the highest in sub-district/district hospital among all public health facilities, the effect of JSY on institutional delivery at this hospital is not that low (27.2 percentage points). This public hospital, located in a sub-district/district town, can provide obstetric care services. It has qualified doctors and modern machines. Results imply that JSY women not only seek institutional delivery services but also look for quality of care, which can be provided by sub-district/district hospital.

8. Conclusion

JSY is one of the leading conditional cash transfer programs in the world covering more than 11 million women annually. This paper analyses the effects of the program on the utilization of several MCHC services under the continuum of care. We have made significant contributions in the evaluation of JSY. Leading studies on JSY (Nandi and Laxminarayan, 2016; Powell-Jackson et al., 2015; Lim et al., 2010; Carvalho et al., 2014; Sengupta and Sinha, 2018), that explored causal effects, used DLHS-3, surveyed in 2007-2008. Although JSY was launched in 2005, its budget was allocated in 2006, and it started its operation in 2007-2008. Many women in DLHS-3, who gave birth before 2007, responded as JSY beneficiaries when they were not (Das et al., 2011). They were probably beneficiaries of other programs run at state level. So, the treatment statuses of the treated women in DLHS-3 suffer from measurement errors, and thus, the treatment dummy is endogenous there. The control statuses of the control women are less likely to suffer from the measurement errors, so such errors are one-sided, and therefore, they are not random. In this situation, if one considers even instrumental variables regression to remove endogeneity bias, the bias will not go away.

Because of the above measurement errors, the average outcomes of the treatment group were underestimated, and those previous studies produced downward biased results. In this study, we use DLHS-4, surveyed in 2013-2014. DLHS-4 surveyed women who gave birth from 2008 to the survey time. As JSY was operated at that time, we assume that women responded correctly to the question – whether you were selected by JSY. If any woman responded as a beneficiary of another program, we have excluded her from the data. In this way, our treatment group includes actual JSY women, and thus, there is little chance of downward biased results. Also, at the survey time of DLHS-4, JSY was mature enough, and well known by women, and we envisage there is less selection bias in DLHS-4. Although we have used PSM technique, we have used it rigorously. Several robust analyses, including ignorability check, are carried out which have implied our results are robust. However, there always remains a chance of endogeneity bias, and we, therefore, apply FRD design exploiting birth order cutoff.

While PSM produces reasonable estimates of treatment effects, FRD design gives us relatively higher effects of JSY. Both methods provide us with higher estimates of treatment effects than those found in previous studies. For example, according to PSM's ATT estimates, women in the treatment group are 15.7 percentage points more likely to take up institutional delivery service, 10.1 percentage points more likely to take up at least one ANC service, 12.2 percentage points more likely to take up at least one PNC service and 8.6 percentage points more likely to take up at least one PNC service for newborns. When we apply FRD design, these estimates become 22.7, 19.6, 14.4 and17.3 percentage points respectively. These higher estimates are anticipated, as they are LATE estimates which are estimates of treatment effects for a sub-group called compliers. On the other hand, due to JSY, Powell-Jackson et al. (2015) found 7.5 percentage points increase in the uptake of institutional delivery service, but no statistically significant increase in the uptake of ANC service. However, our LATE estimates are also in line with those found in other countries. For example, using regression discontinuity design, De Brauw et al. (2011) found that El Salvador's DSF program, Comunidades Solidarias Rurales, increased the uptake of institutional delivery service by 15.3-22.8 percentage points.

Using PSM, we also find reasonable effects of JSY on the uptake of individual ANC services (4.8-12.5 percentage points) and PNC services (3.4-13.6 percentage points excluding "Days of first breastfeeding"), but relatively lower effects on the uptakes of immunizations (1.6-9.4 percentage points). The government has several other programs to increase the number of immunized children. Without the JSY program, the uptake of immunizations is already high, and the program has little scope to improve this. Considering this fact, increases in immunizations due to JSY can be considered as high.

We also find that JSY women in the lowest wealth group and poorer states are most likely to utilize

MCHC services. So, if the government can minimize the targeting errors (by reaching more disadvantaged women), the program's impact may increase greatly. As Powell-Jackson et al. (2015) found, JSY also contributes to crowding-out of the private sector. Among public health facilities, the program has the lowest contribution to the delivery cost in sub-district/district hospitals where the JSY women are 27.2 percentage points (third highest compared to other public health facilities) more likely to go for institutional delivery services. JSY also increases the demand for quality of care, which can be provided more easily by sub-district/district hospitals than other lower/primary public health facilities. If the government can improve the quality of care by managing cheaper services, the program can further increase the utilization of MCHC services.

In advanced settings, investments in motherhood and early childhood have been found useful in yielding a high economic and social return in the future. For example, Early Childhood Care and Education from the Head Start program in the USA have improved children's immediate and near future levels of nutrition and health screening, which in turn have improved their productivity and thus economic activities (Belfield and Kelly, 2013). The UK Government has invested in the antenatal and early years through a large-scale pilot program, the Nurse-Family Partnership, which has generated returns to the individuals regarding increased earnings, higher education, improved physical and mental well-being (Doyle et al., 2009). Such findings are not typical in developing countries. No studies have focused on the future returns of JSY and we do not have scope to do that. However, we can argue that since JSY increases the utilization of ANC services, institutional deliveries, PNC services, and immunizations, the productivity of children born to JSY women is likely to grow in the future.

As DLHS-4 has less chance of misclassification of control women as treated, our results have less downward biases. However, this survey also has a limitation. It is not a nationally representative survey, and therefore, our analysis is neither nationally representative nor externally valid for non-DLHS-4 states. Using the Annual Health Survey or National Family Health Survey, an analysis is possible for non-DLHS-4 or high focus states too, but JSY is universal in those states as all women delivering at public or accredited private hospitals are eligible for JSY benefits as if it is a different program in those states. That analysis would require different research designs such as Difference-in-Difference technique, which is beyond the scope of this paper and we leave such analysis for future research. Although DLHS-4 is not a nationally representative survey, our results are closer to true or nationally representative effects of JSY compared to that found in the previous prominent studies using nationally representative data such as DLHS-3, as our results are higher than those found in previous studies. Also, our results for Nagaland and Tripura close correspond to results for low performing states.

Declaration of interests

We declare no competing interests.

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Appendix A .

Table A.1: Descriptive statistics of covariates

Covariates	JSY Mean (1)	JSY Obs. (2)	NonJSY Mean (3)	NonJSY Obs. (4)	Diff. (5)	p value (6)
Household has below poverty line card (1 yes, 0 no)	0.469	15,841	0.318	57,220	0.151	<0.0001
Household has scheduled caste affiliation (1 yes, 0 no)	0.310	15,144	0.221	53,925	0.089	<0.0001
Household has tribal affiliation (1 yes, 0 no)	0.177	15,837	0.176	57,159	0.002	0.596
Current age of woman/mother	23.854	15,844	25.047	57,239	-1.193	<0.0001
Birth order/parity	1.842	15,788	2.163	56,796	-0.320	<0.0001
Wealth Index	-0.654	15,838	-0.016	57,204	-0.639	<00001
Highest years of education taken by woman/mother	8.675	13,665	9.563	47,616	-0.888	<0.0001
Highest years of education taken by husband	8.812	14,032	9.821	50,431	-1.009	<0.0001
Religion: Hindu (1 yes, 0 no)	0.698	15,842	0.653	57,223	0.045	<0.0001
Residence: Rural (1 yes, 0 no)	0.683	15,844	0.593	57,239	0.090	<0.0001

Note: Birth year dummies and state dummies were also used as covariates, but they are not reported here.

Table A.2: Descriptive statistics of outcome variables

Outcome Variables	JSY Mean (1)	JSY Obs. (2)	NonJSY Mean (3)	NonJSY Obs. (4)	Diff. (5)	p value (6)
Main outcomes						
At least one ANC (1 yes, 0 no)	0.949	15,844	0.826	57,239	0.122	<0.0001
Institutional delivery (1 yes, 0 no)	0.935	15,843	0.773	57,236	0.162	<0.0001
At least one PNC for mother (1 yes, 0 no)	0.747	15,844	0.632	57,234	0.115	<0.0001
At least one PNC for baby (1 yes, 0 no)	0.824	15,770	0.741	56,708	0.084	<0.0001
ANC services						
Weight measured (1 yes, 0 no)	0.872	15,835	0.742	57,207	0.130	<0.0001
Height measured (1 yes, 0 no)	0.512	15,835	0.420	57,207	0.092	<0.0001
Blood pressure checked (1 yes, 0 no)	0.806	15,835	0.671	57,207	0.136	<0.0001
Blood tested (haemoglobin) (1 yes, 0 no)	0.717	15,835	0.613	57,207	0.104	<0.0001
Blood tested (blood group) (1 yes, 0 no)	0.648	15,835	0.544	57,207	0.105	<0.0001
Urine tested (1 yes, 0 no)	0.783	15,835	0.667	57,207	0.117	<0.0001
Abdomen examined (1 yes, 0 no)	0.574	15,835	0.485	57,207	0.088	<0.0001
Breast examined (1 yes, 0 no)	0.352	15,835	0.311	57,207	0.041	<0.0001
Ultrasound done (1 yes, 0 no)	0.634	15,835	0.581	57,207	0.053	<0.0001
Iron Folic Acid tablet/syrup (1 yes, 0 no)	0.795	15,844	0.633	57,239	0.162	<0.0001
At least one tetanus injection (1 yes, 0 no)	0.921	15,842	0.788	57,230	0.133	<0.0001
PNC services for mother						
Abdomen examined (1 yes, 0 no)	0.495	15,841	0.387	57,228	0.108	<0.0001
Advice on breastfeeding (1 yes, 0 no)	0.501	15,841	0.386	57,228	0.116	<0.0001
Advice on baby care (1 yes, 0 no)	0.468	15,841	0.373	57,228	0.095	<0.0001
Advice on Family Planning (1 yes, 0 no)	0.341	15,841	0.249	57,228	0.092	<0.0001
PNC services for baby						
Weight taken at birth (1 yes, 0 no)	0.918	15,769	0.754	56,708	0.164	<0.0001
Days of first breastfeeding	1.450	15,769	1.567	56,698	-0.117	<0.0001
Advice on infant diarrhoea (1 yes, 0 no)	0.551	15,842	0.566	57,226	-0.015	0.001
Advice on infant pneumonia (1 yes, 0 no)	0.284	15,843	0.312	57,234	-0.029	<0.0001
Immunizations for baby						
BCG (1 yes, 0 no)	0.971	7,779	0.945	32,573	0.027	<0.0001
Polio (1 yes, 0 no)	0.973	7,782	0.956	32,571	0.017	<0.0001
First Polio in two weeks of birth (1 yes, 0 no)	0.807	7,782	0.738	32,574	0.069	<0.0001
DPT (1 yes, 0 no)	0.906	7,782	0.860	32,570	0.046	<0.0001
Measles (1 yes, 0 no)	0.865	7,781	0.805	32,570	0.060	<0.0001
Hepatitis-B (1 yes, 0 no)	0.773	15,721	0.684	56,488	0.089	<0.0001
Vitamin-A (1 yes, 0 no)	0.665	15,723	0.599	56,490	0.066	<0.0001

Table A.3: Logit regression of JSY dummy on covariates

	Dep var: Log-odds ratio of JSY							
	Outcome type 1				Outcome type 2			
	Sample 1		Sample 2		Sample 1		Sample 2	
	Coeff.	Robust S.E.	Coeff.	Robust S.E.	Coeff.	Robust S.E.	Coeff.	Robust S.E.
Household has below poverty line card	0.465***	(0.026)	0.474***	(0.023)	0.399***	(0.036)	0.407***	(0.032)
Household has scheduled caste affiliation	0.458***	(0.027)	0.429***	(0.024)	0.359***	(0.037)	0.334***	(0.032)
Household has tribal affiliation	0.137***	(0.050)	0.113***	(0.043)	0.154**	(0.070)	0.099	(0.061)
Current age of woman/mother	-0.006**	(0.003)	-0.011***	(0.002)	-0.012***	(0.004)	-0.016***	(0.003)
Birth order/parity	-0.308***	(0.014)	-0.307***	(0.011)	-0.234***	(0.018)	-0.243***	(0.015)
Wealth Index	-0.094***	(0.008)	-0.099***	(0.006)	-0.102***	(0.011)	-0.115***	(0.008)
Highest years of education taken by woman	-0.019***	(0.004)			-0.023***	(0.005)		
Highest years of education taken by husband	-0.029***	(0.004)			-0.022***	(0.005)		
Religion: Hindu	0.146***	(0.030)	0.123***	(0.027)	0.103**	(0.042)	0.097***	(0.037)
Residence: Rural	0.306***	(0.025)	0.335***	(0.023)	0.333***	(0.035)	0.355***	(0.032)
Birth year dummies	Yes		Yes		Yes		Yes	
State dummies	Yes		Yes		Yes		Yes	
Pseudo R^2	0.110		0.104		0.097		0.090	
N	54,656		68,527		30,372		38,332	

Note: In this Table, coefficients imply the changes in log-odds ratio of JSY dummy. * p <0.10, **p <0.05, ***p <0.01. Birth year dummies and state dummies were also used in the above logit regressions, but their coefficients (e.g., changes in log-odds ratio of JSY dummy) are not reported here.

(a) Sample 1 for outcome type 1

(b) Sample 2 for outcome type 1

(c) Sample 1 for outcome type 2

(d) Sample 2 for outcome type 2

Note: After running four representative logit models in Table (A.3), we estimate four sets of standardized % biases between treatment and control groups across covariates after matching and four sets of such biases without matching. We show biases in horizontal axes. Vertical axes represent lists of covariates, but they are not seen because of long lists. After matching, all biases fall within two dashed vertical lines at -10 and 10. We can say that covariates are balanced after matching, in all four cases.

Figure A.1: Before and after matching percentage biases in covariates between treatment and control groups

(a) Sample 1 for outcome type 1

(b) Sample 2 for outcome type 1

(c) Sample 1 for outcome type 2

(d) Sample 2 for outcome type 2

Note: After running four representative logit models in Table (A.3), we estimate four sets of propensity scores for the treatment group and four sets of propensity scores for the control group. We show propensity scores in the horizontal axes and their densities in the vertical axes. In each of four subfigures, if the propensity score of the control group tends to zero, the density becomes higher. In no subfigure, the overlap condition is satisfied. In this situation, we drop observations if the propensity score is less than 0.10.

Figure A.2: Density of the propensity score, $Pr(JSY = 1|X)$, for treatment group ($JSY = 1$) and control group ($JSY = 0$)

Table A.4: Comparisons of mean outcomes between two control groups (eligible non-participants and ineligibles) as an unconfoundedness check

	Sample 1			Sample 2		
Outcome Variables	Coeff. (1)	p value (2)	Obs. (3)	Coeff. (4)	p value (5)	Obs. (6)
Main outcomes						
At least one ANC	-0.010	(0.127)	45,335	-0.010	(0.105)	56,992
Institutional delivery	-0.017***	(0.010)	45,332	-0.027***	(0.000)	56,988
At least one PNC for mother	-0.001	(0.941)	45,331	-0.017**	(0.015)	56,988
At least one PNC for baby	-0.018**	(0.016)	45,278	-0.021***	(0.002)	56,907
ANC services						
Weight measured	-0.026***	(0.001)	45,306	-0.031***	(0.000)	56,961
Height measured	-0.015*	(0.073)	45,306	-0.018***	(0.009)	56,961
Blood pressure checked	-0.026***	(0.002)	45,306	-0.028***	(0.000)	56,961
Blood tested (haemoglobin)	-0.009	(0.300)	45,306	-0.019***	(0.007)	56,961
Blood tested (blood group)	-0.020**	(0.015)	45,306	-0.027***	(0.000)	56,961
Urine tested	0.008	(0.305)	45,306	-0.002	(0.792)	56,961
Abdomen examined	-0.024***	(0.006)	45,306	-0.032***	(0.000)	56,961
Breast examined	-0.014*	(0.084)	45,306	-0.024***	(0.000)	56,961
Ultrasound done	-0.007	(0.413)	45,306	-0.022***	(0.002)	56961
Iron Folic Acid tablet/syrup	-0.015*	(0.066)	45,335	-0.022***	(0.002)	56,992
At least one tetanus injection	-0.015**	(0.036)	45,327	-0.017***	(0.009)	56,983
PNC services for mother						
Abdomen examined	-0.013	(0.117)	45,326	-0.022***	(0.001)	56,979
Advice on breastfeeding	-0.015*	(0.066)	45,326	-0.027***	(0.000)	56,979
Advice on baby care	-0.017**	(0.037)	45,326	-0.029***	(0.000)	56,979
Advice on Family Planning	0.000	(0.960))	45,326	-0.008	(0.232)	56,979
PNC services for baby						
Weight taken at birth	-0.003	(0.668)	45,277	-0.020***	(0.002)	56,906
Days of first breastfeeding	0.009	(0.560)	45,269	0.015	(0.253)	56,897
Advice on infant diarrhoea	-0.015*	(0.088)	45,326	-0.018**	(0.020)	56,980
Advice on infant pneumonia	0.003	(0.723)	45,330	-0.002	(0.828)	56,987
Immunizations for baby						
BCG	0.013**	(0.031)	25,908	0.012**	(0.035)	32,755
Polio	0.001	(0.788)	25,907	0.005	(0.308)	32,753
First Polio in two weeks of birth	-0.022**	(0.032)	25,910	-0.017*	(0.062)	32,756
DPT	-0.007	(0.408)	25,905	-0.002	(0.825)	32,752
Measles	0.002	(0.810)	25,905	0.006	(0.487)	32,752
Hepatitis-B	0.007	(0.417)	45,053	0.000	(0.997)	56,618
Vitamin-A	0.004	(0.653)	45,057	0.000	(0.993)	56,621

Note: Coefficients are estimated by running separate OLS regressions of outcomes on the dummy for two control groups (1 if eligible non-participants, 0 ineligibles) and respective covariates under sample 1 and sample 2. In columns (1) and (4), we show the coefficients of the dummy for the control group, for sample 1 and sample 2 respectively. p values from robust standard errors are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.5: Mantel and Haenszel (Mantel and Haenszel, 1959) test statistics for four main outcome variables

	At least one ANC								Institutional Delivery							
	Sample 1				Sample 2				Sample 1				Sample 2			
γ	Q_{mh+}	Q_{mh-}	p_{mh+}	p_{mh-}	Q_{mh+}	Q_{mh-}	p_{mh+}	p_{mh-}	Q_{mh+}	Q_{mh-}	p_{mh+}	p_{mh-}	Q_{mh+}	Q_{mh-}	p_{mh+}	p_{mh-}
1.0	23.7	23.7	0.000	0.000	31.1	31.1	0.000	0.000	31.2	31.2	0.000	0.000	39.6	39.6	0.000	0.000
1.1	21.5	25.9	0.000	0.000	28.4	33.8	0.000	0.000	28.8	33.7	0.000	0.000	36.5	42.6	0.000	0.000
1.2	19.6	28.0	0.000	0.000	26.1	36.3	0.000	0.000	26.6	36.0	0.000	0.000	33.9	45.5	0.000	0.000
1.3	17.8	29.9	0.000	0.000	23.9	38.6	0.000	0.000	24.6	38.2	0.000	0.000	31.4	48.1	0.000	0.000
1.4	16.2	31.7	0.000	0.000	22.0	40.9	0.000	0.000	22.8	40.2	0.000	0.000	29.2	50.6	0.000	0.000
1.5	14.7	33.4	0.000	0.000	20.1	42.9	0.000	0.000	21.2	42.2	0.000	0.000	27.1	53.0	0.000	0.000
1.6	13.3	35.1	0.000	0.000	18.5	44.9	0.000	0.000	19.6	44.0	0.000	0.000	25.2	55.3	0.000	0.000
1.7	12.1	36.6	0.000	0.000	16.9	46.8	0.000	0.000	18.2	45.8	0.000	0.000	23.4	57.4	0.000	0.000
1.8	10.9	38.1	0.000	0.000	15.4	48.7	0.000	0.000	16.9	47.5	0.000	0.000	21.8	59.5	0.000	0.000
1.9	9.7	39.6	0.000	0.000	14.1	50.4	0.000	0.000	15.6	49.1	0.000	0.000	20.2	61.4	0.000	0.000
2.0	8.7	40.9	0.000	0.000	12.8	52.1	0.000	0.000	14.4	50.6	0.000	0.000	18.8	63.3	0.000	0.000
	At least one PNC for mother								At least one PNC for baby							
	Sample 1				Sample 2				Sample 1				Sample 2			
γ	Q_{mh+}	Q_{mh-}	p_{mh+}	p_{mh-}	Q_{mh+}	Q_{mh-}	p_{mh+}	p_{mh-}	Q_{mh+}	Q_{mh-}	p_{mh+}	p_{mh-}	Q_{mh+}	Q_{mh-}	p_{mh+}	p_{mh-}
1.0	16.1	16.1	0.000	0.000	22.7	22.7	0.000	0.000	12.2	12.2	0.000	0.000	18.6	18.6	0.000	0.000
1.1	12.4	19.8	0.000	0.000	18.5	26.9	0.000	0.000	9.0	15.4	0.000	0.000	14.9	22.3	0.000	0.000
1.2	9.0	23.2	0.000	0.000	14.7	30.8	0.000	0.000	6.1	18.4	0.000	0.000	11.5	25.7	0.000	0.000
1.3	5.9	26.3	0.000	0.000	11.2	34.4	0.000	0.000	3.4	21.2	0.000	0.000	8.4	28.9	0.000	0.000
1.4	3.1	29.2	0.001	0.000	8.0	37.7	0.000	0.000	0.9	23.7	0.186	0.000	5.6	31.9	0.000	0.000
1.5	0.4	32.0	0.339	0.000	5.0	40.8	0.000	0.000	1.4	26.1	0.082	0.000	2.9	34.6	0.002	0.000
1.6	2.0	34.5	0.021	0.000	2.2	43.7	0.014	0.000	3.6	28.4	0.000	0.000	0.4	37.3	0.336	0.000
1.7	4.4	37.0	0.000	0.000	0.4	46.5	0.344	0.000	5.6	30.5	0.000	0.000	1.9	39.7	0.030	0.000
1.8	6.6	39.3	0.000	0.000	2.9	49.1	0.002	0.000	7.5	32.5	0.000	0.000	4.1	42.1	0.000	0.000
1.9	8.7	41.4	0.000	0.000	5.2	51.6	0.000	0.000	9.3	34.5	0.000	0.000	6.2	44.3	0.000	0.000
2.0	10.6	43.5	0.000	0.000	7.5	53.9	0.000	0.000	11.1	36.3	0.000	0.000	8.1	46.4	0.000	0.000

γ : Odds of differential assignment due to unobserved factors
Q_{mh+} : Mantel-Haenszel statistic (assumption: overestimation of treatment effect)
Q_{mh-} : Mantel-Haenszel statistic (assumption: underestimation of treatment effect)
p_{mh+} : significance level (assumption: overestimation of treatment effect)
p_{mh-} : significance level (assumption: underestimation of treatment effect)

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Highlights

- We estimate the effects of conditional cash transfer program, Janani Suraksha Yojana, on the utilization of maternal and child health services in India.
- We apply both propensity score matching (PSM) and fuzzy regression discontinuity (FRD) design.
- According to PSM, program beneficiaries are around 16 percentage points more likely to have institutional deliveries.
- When we apply FRD design, that estimate becomes around 23 percentage points.
- Effects of JSY vary by state, wealth, and the place of birth.