Financial Incentives in Health: New Evidence from India's Janani Suraksha Yojana

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This paper studies the health effects of one of the world's largest demand-side financial incentive programmes – India's Janani Suraksha Yojana. Our difference-in-difference estimates exploit heterogeneity in the timing of the introduction of the financial incentive programme across districts. We find that cash incentives to women increased access to maternity services but failed to improve neonatal or early neonatal mortality, even in districts with relatively high quality of care. The positive effects on utilisation are larger for less educated, poorer, and ethnically marginalised women. We also find evidence of unintended consequences. The financial incentive programme was associated with a substitution away from private health providers, an increase in fertility and a positive improvement in breastfeeding behaviour. These findings demonstrate the potential for financial incentives to have unanticipated health effects, which may, in the case of fertility, directly undermine the programme's own objective of reducing mortality.

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I. Introduction

One of the main challenges for global health is to identify policies and strategies that improve the health of women and children (United Nations, 2010). The traditional focus of much of the medical literature has been on intervention research resulting in unprecedented knowledge on what health technologies work (Bhutta *et al.*, 2008; Campbell and Graham, 2006; Jones *et al.*, 2003). Never before have policymakers in developing countries had such a wealth of evidence at their disposal. Indeed, countries that achieved universal coverage of life-saving interventions have seen rapid reductions in mortality. For example, over the past two decades Thailand, Vietnam and Sri Lanka have developed a comprehensive primary health care system. All these countries between 1990 and 2006 witnessed average yearly reductions in under five mortality of over 5 percent (Rohde *et al.*, 2008). Yet across the developing world more broadly there are large gaps in coverage, particularly amongst the poorest (Bhutta *et al.*, 2010). A key question then is whether there are policies that can be introduced within health systems – termed here health system interventions – which can be shown to improve access to priority health services.

In an effort to improve population coverage of health interventions and narrow the differences between income groups, policymakers in developing countries are becoming increasingly bold in their reforms. One promising strategy is to provide financial incentives to individuals who exhibit certain behaviours that improve health.[2] This is the key feature of various programmes that have become popular in recent years. Whether the incentive takes the form of conditional cash transfers, vouchers or one-off cash payments, the central idea of providing monetary rewards conditional on measurable actions is the same. Financial incentives have courted considerable controversial, with views ranging from "as close as you can come to a magic bullet" to a "form of bribery" (Dugger, 2004; Marteau et al., 2009). Critics point to the theoretical possibility of unintended adverse consequences as well as moral concerns over their use, particularly in a health setting.

This paper studies the effects of one of the largest cash incentive programmes for health in the world. With an annual expenditure of 8.8 billion rupees or $207 million, and an estimated 7.1 million individual beneficiaries,[3] India's national Janani Suraksha Yojana (JSY) provides cash to women who give birth in a health facility. The JSY provides an ideal testing ground to examine the effects of financial incentives on health.[4] Although

[2] In this paper we are interested in demand-side financial incentives, rather than provider payment mechanisms such as pay-for-performance. These reward physicians for improvements in quality of care and other measures, and are popular in the US and UK. For brevity, we will use the term financial or cash incentives in health to refer to schemes that target the users of health care.

[3] These figures refer to 2007/08, the financial year closest to our study period.

[4] There have been a number of studies on the JSY, some of which have collected primary household data (Devadasan *et al.*, 2008; Malini *et al.*, 2008; Verma *et al.*, 2010). For the most part these have been descriptive, documenting progress in the implementation of the programme. By contrast, Lim and colleagues (2010) make claims as to the causal effect of the JSY. The headline results are based on two specifications that fail to convincingly control for unobserved heterogeneity. The first performs individual matching on a cross-section of women who did and did not receive the JSY cash, interpreting the difference between the two groups as the causal effect. Not only is there reverse causality (women receive the cash when they give birth in a health facility), but conditional independence is a strong assumption and unobservsables are likely to bias

officially launched in 2005, the rollout of the JSY across districts was incremental, providing variation in its placement. At the same time, much of the health policy environment in India is common within states, which gives us more confidence that district placement of the JSY is not acting as a proxy for other policy initiatives. A second advantage of this setting is the narrow focus of the JSY on women at childbirth. This provides greater scope for examining unintended consequences of the financial incentives on closely related but non-incentivised behaviours. A third advantage is the scale at which the JSY was implemented. This differentiates our study from carefully controlled small scale (incentive) experiments, whose external validity has at times been questioned (Deaton, 2010).

We identify the effect of the JSY on health care seeking behaviour and health status by exploiting variation in the timing of the introduction of the JSY to districts. From 2005 onwards, the JSY was introduced to 424 of the 587 sample districts, staggered over a three-year period. Using data on women who gave birth between 2001 and 2008 from the two most recent rounds of India's District Level Health Survey (DLHS), our empirical approach examines whether the JSY can account for cross-district patterns in health care utilisation and health status over time. In estimating the effect of the JSY, this difference-in-difference strategy allows us to control for time invariant unobservables at the district level that influence study outcomes and are correlated with the placement of the JSY.

Our results show that the JSY increases the proportion of women who give birth in a public health facility. The magnitude of this effect is quite large – when implemented at full coverage, the JSY leads to a 19 percentage point increase or a doubling in the rate of utilisation. Incomplete implementation, however, means that the increase in utilisation due to the JSY is in practice substantially smaller. The positive impact on women giving birth in a public health facility is driven almost entirely by increases in the use of primary health centres and community health centres, providers that operate below the district hospital. This may explain why we see no impact of the JSY on the rate of caesarean section. In addition, we present evidence on the effect of the JSY on health outcomes, finding no statistically significant effect on either neonatal mortality (deaths within 28 days of birth) or early neonatal mortality (deaths within 24 hours of birth). Our preferred point estimate is able to rule out negative effects on early neonatal mortality larger than 8.7 deaths per 1,000 live births. An important limitation of the study is that, for lack of suitable data, we were unable to assess the effect of the JSY on maternal mortality.

Since the JSY was not randomly assigned, the central empirical concern is whether the introduction of the JSY is correlated with unobserved changes in the determinants of women's behaviour at childbirth. In support of our identifying assumption we find that, first, the JSY has little or no effect on antenatal care, a placebo outcome that was not explicitly incentivised by the programme; second, study outcomes are not correlated with future introduction of the JSY – ie. changes in the outcomes of interest do not anticipate JSY placement; and third, the within-district variation over time in JSY coverage that

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the results (Imbens and Wooldridge, 2009). The second approach is similar to a before and after design except that the treatment group comprises only JSY beneficiaries in the second time period.

identifies the effect of the programme is not correlated with a broad range of individual characteristics that strongly influence care seeking behaviour.

We also provide evidence on a number of unintended consequences. First, failure to implement the JSY much beyond the public sector means that the financial incentives result in women substituting away from giving birth in the private sector. Point estimates suggest that this substitution effect accounts for approximately one-third of the positive impact of the JSY on public health facility births. Second, results show that the JSY has a positive, statistically significant effect on fertility. Third, we find evidence of indirect benefits. Women in JSY districts are more likely to start early breastfeeding within one hour of childbirth. Our final set of results concerns heterogeneity in the effect of the JSY. We examine a number of socioeconomic characteristics – mother's education, ethnicity and wealth – that may modify the effect on utilisation, with results showing a greater behavioural response amongst women who are more disadvantaged. When we use data from a survey of health providers to generate a proxy measure of quality of care, we find that there is no differential effect of the JSY on mortality with respect to structural quality of care.

This paper contributes to the existing literature by estimating robust causal effects that lead to conclusions which are qualitatively different from previous studies on the JSY. Our main results are consistent with much of the evidence emerging from conditional cash transfer programmes and small scale incentive experiments.[5] However, we go beyond the typical study of financial incentives in several ways. First, we examine unintended consequences of financial incentives. Similar to the findings from studies in Brazil (Morris *et al.*, 2004b) and Honduras (Morris *et al.*, 2004a), we document evidence of adverse effects, which highlight how important it is for policymakers to anticipate these downside risks in the design of financial incentive schemes. Second, our empirical strategy allows us to identify what we refer to as an implementation gap – that is, the difference between the (intent-to-treat) effect of introducing the JSY programme in a district and the effect of the financial incentives as the mechanism within the JSY.

We also connect to a second literature evaluating the impact of health system interventions and policies. This is a wide ranging and challenging area of research (Mills et al., 2008), and one in which much of the existing econometric evidence focuses on the impact of health financing initiatives, in particular health insurance (eg. Babiarz *et al.*, 2010; Finkelstein *et al.*, 2011; King *et al.*, 2009; Manning *et al.*, 1987; Thornton *et al.*, 2010; Wagstaff *et al.*, 2009). Other areas of health system policy that have been addressed in the econometric literature include the removal of user fees (eg. Ansah *et al.*, 2009), pay-for-performance (eg. Basinga *et al.*, 2011; Farrar *et al.*, 2009), competition (eg. Cooper *et al.*, 2011; Kessler and McClellan, 2000; Propper *et al.*, 2008), pay regulation (eg. Propper and Van Reenen, 2010), and targets (eg. Propper *et al.*, 2010).

[5] The systematic literature review on conditional cash transfers provides a detailed summary of much of this evidence (Lagarde *et al.*, 2007).

Given that the JSY remains a high-profile federal health programme in India, the findings are of immediate relevance to policy. First, they argue for much better administration of the programme. If disbursement of the JSY cash were improved, the effect on use of formal health care would be substantially greater than at present. Second, the findings reinforce the growing sentiment that demand-side intervention by government can be effective in improving access to health services but alone may be insufficient to improve health outcomes. Strengthening the quality of primary health care and the referral system in India is thus a critical complementary strategy, as is staggering supply- and demand-side investments over time such that individuals are encouraged to use services once quality has improved. Third, the findings suggest that financial incentives may be a powerful but imprecise tool for changing health-related behaviours. They can have unintended health effects, on fertility for example, which may undermine the programme's own objectives. Financial incentives must therefore be used with caution.[6]

The paper is structured as follows. Section II describes the JSY and sets out theoretical predictions of its impact on health-related behaviours. Section III describes the data. Section IV presents the empirical strategy and reduced-form estimates. Section V presents the main econometric results and includes a discussion of robustness checks. Section VI examines heterogeneity in the impact of the JSY, and Section VII offers concluding comments.

[6] Of immediate relevance is the Government of India has plan to introduce a new cash transfer scheme for improving child nutrition outcomes.

II. Background

In the early 1990s, maternal and child health in India gained greater policy recognition with the launch of the Child Survival and Safe Motherhood programme. In line with international policy at the time, articulated in the International Conference on Population and Development in Cairo, this represented a paradigm shift from an historical focus on family planning to broader issues around reproductive health and safe motherhood. One particularly relevant initiative in the 1990s was the National Maternal Benefit Scheme, an unconditional cash transfer targeted at pregnant women living in households below the poverty line. This scheme laid the foundation of what was later to become the JSY.

Despite the long history of well-intentioned family welfare policies and some recent progress, maternal and child mortality in India remains high. With 68,000 maternal deaths and 1.5 million deaths among children under five every year, no other country accounts for a larger proportion of global mortality (Paul *et al.*, 2011). Maternal mortality has fallen by 47 percent from 398 deaths per 100,000 live births in 1997-98 to 212 deaths per 100,000 live births in 2007-09 (Registrar General of India, 2006; Registrar General of India, 2011). Meanwhile, under-five mortality has also improved, from 109 deaths per 1,000 live births in 1992-93 to 74 deaths per 1,000 live births in 2005-06 (International Institute for Population Sciences and Macro International, 2007) and neonatal mortality currently stands at 35 deaths per 1,000 live births (Registrar General of India, 2009). Nonetheless, these trends are not sufficient for India to meet its international health targets in 2015. In addition, the national picture masks enormous differences across states. For example, Kerala's maternal mortality rate is almost five times lower than some of the worst performing northern Indian states (Registrar General of India, 2011).

The emerging consensus around strategies to address maternal mortality prioritises one based on delivery in primary health care institutions, backed up by access to referral-level facilities (Campbell and Graham, 2006). In India, national surveys show that institutional deliveries have increased modestly, from 26 percent in 1993 to 39 percent in 2006 (International Institute for Population Sciences, 1995; International Institute for Population Sciences and Macro International, 2007), but a large proportion of women continue to give birth at home. Even when women do reach a health facility to give birth, the quality of care they receive is unlikely to be adequate (Das and Hammer, 2006; Das and Hammer, 2007; Das et al., 2008). For example, only 53 percent of primary health centres in India function 24 hours a day and 45 percent do not have referral services for complicated deliveries (International Institute for Population Sciences, 2010). Absent health workers is also a common phenomenon (Banerjee *et al.*, 2004).

It is against this background that the federal government launched in 2005 the National Rural Health Mission (NRHM), a centrally funded programme to integrate what had previously been a fragmented set of mostly disease focused initiatives. While broad in its scope, the central focus of the NRHM is maternal and child health. Key elements of the mission include large investments in health infrastructure, the deployment of three quarters of a million newly created accredited social health activists as frontline health

workers in the community, strategies to stimulate demand for health services, and decentralisation of the health system (Ministry of Health and Family Welfare, 2005).

II.A India's Janani Suraksha Yojana

One of the more high profile components of the NRHM is the Janani Suraksha Yojana (translated as "Safe Motherhood Scheme"). It was launched officially in April 2005, with the objective of improving maternal and neonatal health through the promotion of institutional deliveries.[7] It provides a cash incentive to women who give birth in a public health facility or an accredited private health provider (Ministry of Health and Family Welfare, 2006).

The JSY programme designates Indian states as low performing or high performing, varying the cash amount to provide greater incentives in the area of higher priority. Specifically, women in low-performing states are offered 1,400 Rs ($31) in rural areas and 1,000 Rs ($22) in urban areas, and those in high-performing states are given 700 Rs ($16) in rural areas and 600 Rs ($13) in urban areas.[8] To put these amounts in perspective, Gross National Income per capita was $1000 in 2007. The cash payment is available to all women in the low-performing states; by contrast, it is offered in high-performing states only to women living in households below the poverty line, belonging to scheduled castes and tribes, or those who have had two or fewer live births. The policy stipulates that the cash is to be disbursed to the mother immediately at the institution itself and within a week of delivery.

To provide incentives for health workers who encourage women to give birth in a formal care provider, accredited social health workers are offered a cash payment of between 200 Rs ($4) and 600 Rs ($13) for each delivery attended. The JSY also pays 500 Rs ($11) to women who give birth at home, conditional on less than two living children and a below the poverty line card. Since this is a direct continuation of the cash assistance provided under the National Maternity Benefit Scheme, the JSY introduces no additional incentive for eligible women to stay at home.

The JSY is one of a number of different types of financial incentive programmes that have become popular in developing countries during the 2000s. Perhaps the most widely adopted are conditional cash transfer programmes, designed to act as a social safety net for the poor while at the same time inducing greater investment in human capital

[7] Ethnographic research in the Indian state of Uttar Pradesh casts doubt on the government strategy to encourage institutional deliveries as a means to improve the health of women. Jeffrey and Jeffrey (2010) argue that the context surrounding the government provision of health care presents challenges that neither the NRHM nor the JSY were intended to address. Decades of mistrust of government health services and controversial family planning programmes have left a credibility gap not easily filled by offering financial incentives and investing in new infrastructure. In line with a report by Human Rights Watch (2009), they contend that accountability of government health providers to the population they serve is key and nothing less than "a dismantling of a long-standing political economy of health care provision" will help to remedy the situation.

[8] The low-performing states consist of Bihar, Chattisgarh, Jharkhand, Orissa, Uttar Pradesh, Uttaranchal, Rajasthan, Madhya Pradesh, Assam and Jammu and Kashmir.

(Fiszbein *et al.*, 2008).[9] Financial incentive schemes that focus on specific health problems or health services have also emerged. These tend to provide one-off cash payments, vouchers, or in-kind transfers linked to specific services, such as maternity services (Nepal), insecticide treated nets (Tanzania), HIV testing (Malawi), immunization (Indian state of Rajasthan), and reproductive health and family planning services (Cambodia, Bangladesh, Kenya).

II.B Theoretical Considerations

Consider a financial incentive programme that rewards families in which the woman gives birth in a health facility. If households lack the financial resources, heavily discount the future or lack information on the benefits of health care to make optimal care seeking choices, short-term financial incentives will increase demand for maternal health care. For simplicity, suppose that there are three health seeking choices at delivery – public health providers, private health providers and giving birth at home. Financial incentives provided to women seeking care in the public sector only will then lead to a substitution away from private health providers and home births (Gertler and Van der Gaag, 1990).

To the extent that health providers can meet this increase in demand, financial incentives will increase utilisation of health services. If instead health providers are functioning at full capacity or are unable to increase supply in the short-term, financial incentives will have little impact. Theoretically, negative outcomes may be generated when incentives crowd out intrinsic motivation. However, we consider such a mechanism unlikely in the context of an incentive scheme targeting care seeking behaviour. Whether an increase in utilisation of public health services improves health outcomes is not clear-cut, and will depend on differences in the clinical quality of care between the three health care seeking choices. We would expect the narrowest difference in quality to be between public and private health providers, particularly in terms of clinical as opposed to interpersonal dimensions quality.

While the financial rewards provide explicit incentives to use maternal health services, implicitly they also serve to incentivise pregnancy. This effect may manifest itself in terms of a reduction in birth spacing or an increase in total lifetime children for women who otherwise would not have become pregnant. We also anticipate indirect effects as financial incentives increase women's exposure to health information. Greater contact with health staff exposes women to more information on healthy behaviours concerning the mother and her neonate. Behaviors shown to have an impact on health outcomes include wrapping the baby within 30 minutes of childbirth, initiating breastfeeding within one hour, and dressing the cord with antiseptic (Darmstadt *et al.*, 2005).

[9] Some of better known CCT programmes *Opportunidades* in Mexico, *Red De Proteccion Social* in Nicaragua, the *Bolsa Familia* in Brazil, *Familias en Acción* in Colombia, *Programa de Asignacion Familiar* (PRAF) in Honduras, Jamaica's *Programme Advancement Through Health and Education* (PATH), and Paraguay's *Tekopora*

III. Measures and Data

III.A Study Outcomes

Data on the study outcomes come from the household component of the District Level Health Survey (DLHS), a repeated cross-section survey carried out by the International Institute for Population Sciences in Mumbai and designed to provide estimates on maternal and child health and service utilisation at the district level in India (International Institute for Population Sciences, 2010). The household survey used a multi-stage stratified random sampling design. We use data from the two most recent rounds of the household survey. The DLHS-2, conducted over the period 2002-04, sampled 620,107 households in 593 districts. The DLHS-3, the most recent round, was carried out in 2007-08 and sampled 720,320 households in 611 districts. Each married woman aged 15-49 years in the sample households was interviewed and it is these data we use to construct our outcome measures (the sample size is 507,622 currently married women in DLHS-2 and 643,944 currently married women in DLHS-3).

The married woman questionnaire, modelled closely on India's established National Family and Health Survey, contains measures of health care utilisation and health status that the JSY would be expected to improve. Our main utilisation outcome is births in a health facility, measured using information on the place of delivery of the woman's most recent birth. The analysis also considers variants on this outcome, such as the type of health provider chosen, whether a health worker was in attendance and the type of procedure performed at delivery.

Our main measure of health status is neonatal mortality, defined as the death of a baby within 28 days after being born alive and measured using information on the birth history of women.[10] The financial year of the most recent delivery and each live birth is established using information on the year and month reported by women.[11] The DLHS-3 limits the recall period of birth histories to 1st January 2004, while those in DLHS-2 are not truncated. However, to ensure recall periods are approximately the same in the two survey rounds, we drop all observations prior to 1st April 2001. Thus, when we stack the data from the two survey rounds, we have observations in every financial year from 2001/02 to 2007/08.

An important contribution of this paper is to consider the effect of the JSY on a second set of outcomes that we refer to as *unintended consequences* of the programme. These include births in a private health facility, fertility, and breastfeeding behaviour. Private health facilities refer to both for-profit and not-for-profit nonstate providers. Fertility is measured using birth histories to establish whether women were pregnant in a given year. Finally, to measure breastfeeding, women were asked if and when they started breastfeeding the child of their most recent delivery. We focus on breastfeeding within

[10] Unless truncated, a birth history documents every pregnancy a woman has had during her lifetime. It typically includes the pregnancy outcome, sex of the child, birth order, month and year of childbirth / abortion, age of woman at childbirth and, if the child died, age at death.

[11] We work in financial years (1st April to 31st March) throughout because the government's annual budgetary cycle is likely to correspond more closely to the introduction of the JSY than calendar years.

the first hour, when information from health providers on the benefits of timely breastfeeding is most likely to take effect. All outcomes in this study are comparable across the two survey rounds, both in terms of how they are defined and the interview questions used to elicit the required information.

Summary statistics on the outcome measures are shown in Panel A of Table 1. Across the two datasets, neonatal mortality is estimated to be 30 deaths per 1,000 live births. Around two-fifths of women give birth in a health facility (the remainder give birth at home), with the public and private sectors catering for approximately equal shares. Almost 8 percent of women give birth by caesarean section and a further 2 percent have an assisted delivery.[12] Two fifths of women have the three antenatal care visits recommended by the Government of India and approximately the same proportion start breastfeeding within one hour of giving birth. The proportion of women who report being pregnant in any given year is 8 percent. In addition to information on study outcomes, we exploit data on a broad range of socio-demographic characteristics. We again include in the analysis only those variables whose measurement is comparable across survey rounds. Panel B in Table 1 provides summary statistics for these demographic variables.

The data contain a district identifier which we use to estimate specifications with district fixed effects. However, because the administrative boundaries of some districts changed in the period between the two survey rounds, we sought to map new districts in the DLHS-3 onto their old counterparts in the DLHS-2 data. In most cases this was possible, leaving 587districts that are consistently defined across the two datasets.[13] In estimating the effect of the JSY on care seeking behaviour and health status, we must assume that the district in which women are residing at the time of interview is the same as the one when she gave birth.

III.B Placement of the JSY

Our estimation strategy rests on there being variation in the timing of the introduction of the JSY. We exploit such variation at the district level, the administrative unit directly below the Indian state which has responsibility for planning and implementation of federal and state policies. Conceptually we wish to make the distinction between the introduction of the JSY and its coverage or penetration. The former reflects a decision on the part of the government health authorities to make the JSY available while the latter implies something about the quality of implementation. As we argue below, using data on both to evaluate the JSY provides for a richer interpretation of the estimates of effect.

To construct our dummy for whether a district introduced the JSY in a given year, we use household data from the DLHS-3 in which women who gave birth are asked to respond to the following question: "Did you receive any government financial assistance for delivery care under the Janani Suraksha Yojana or state-specific scheme." One way to proceed

[12] An assisted delivery is defined as one which involves the use of forceps or a ventouse.

[13] In cases where the geographical boundaries of newly created districts cut across two or more old districts, we were unable to map the new districts onto their old counterparts. We therefore drop observations in which it is not possible to generate a consistent district identifier.

would be to define the year the JSY was introduced in a district as the first (birth) year in which a positive response was given to this question. But, due to imprecise wording, this question picks up responses that refer to the National Maternity Benefit Scheme, an initiative that preceded the JSY (see Section II for more detail). This explains why 7.4 percent of women giving birth in a health facility report receiving a cash payment in 2004/05, before the JSY was even official government policy. Instead we define the year the JSY was introduced in a given district to be the first year in which the proportion of eligible women receiving a facility cash payment is 10 percentage points greater than the 2004/05 level.[14]

We assess the reliability of this measure against an alternative that is based on data from an independent source. Conducted in parallel to the DLHS-3, a survey of health providers in the public sector sampled 8,619 primary health centres, who were asked if they had provided cash to JSY beneficiaries in the month preceding the interview. Using these data, we define a district as having started the JSY if at least one primary health centre reports disbursement of the cash payment to one or more JSY beneficiaries in the previous month. Because the reference periods of the two measures do not precisely match we confine the comparison to the set of districts that, according to our primary measure, had introduced the JSY by 2007/08.[15] We find that there is agreement between the two measures in 97.5 percent of these districts.

Figure 1 depicts on a map of India the year in which each district introduced the JSY, showing variation over time and between districts. According to our measure, 157 districts first introduced the JSY in financial year 2005/06, a further 156 districts had introduced the JSY by 2006/07, and 111 more districts had started the JSY by 2007/08. In total, 424 started the programme and 163 districts did not start the programme during this study period. In anticipation of the empirical analysis, we recognise that variation in the introduction of the JSY across districts is unlikely to be random. Thus, understanding the determinants that are correlated with both the introduction of the JSY and the study outcomes is key to making a causal interpretation of the impact estimates.

Discussions with policymakers and other stakeholders engaged with the JSY suggest that the introduction of the programme was prioritised in socioeconomically disadvantaged places. At the national level, the JSY was explicitly prioritised according to high-focus and low-focus states. More importantly, however, interviews indicated that the JSY was prioritised within states at the district level. For example, in the state of West Bengal, health sector reforms including the JSY gave particular attention to six focal districts, identified on the basis of health indicators, poverty and socially marginalised population groups.[16]

[14] We base our measure of JSY placement on beneficiaries rather than, say, budget releases or district expenditure because a district with JSY beneficiaries implies that the government has taken the necessary steps to start the programme.

[15] This alternative measure corresponds to around the start of the financial year 2008/09 when these health providers were interviewed. In contrast, our main measure applies to the financial year 2007/08 and it is highly likely some districts introduced the JSY in the intervening period.

[16] Scheduled tribes are historically disadvantaged people in India, given explicit recognition in India's Constitution.

Empirically we can examine the relationship between JSY placement and the latter two variables. The data support the qualitative evidence in showing the role of these district characteristics in influencing the decision on where to introduce the JSY. In districts with the JSY, poverty is 46 percent higher and the tribal population share is 26 percent larger. Although income was not mentioned, we also find that average wealth is 40 percent lower than non JSY districts. When we run a district-level regression of JSY placement on poverty incidence, the tribal population share and average household wealth, the bivariate relationships show the same pattern. Table A1 in the Appendix presents the results, showing that the three variables of interest are strong predictors of JSY placement and broadly this remains true when we focus on the introduction of the JSY in 2005 only.

These results do not rule out the presence of other important factors that drive both the introduction of the JSY and changes in the study outcomes. To address this concern, in Section V we will show evidence in support of the identifying assumption that introduction of the JSY is orthogonal to the error term. First, there is little evidence of differential pre-existing trends in our outcomes – that is, changes in the outcomes of interest do not anticipate the introduction of the JSY. Second, once we control for district poverty, wealth and tribal population, we show that the remaining variation in the timing of the JSY introduction is not systematically related to observables at the individual level. These pieces of evidence are only suggestive and it is by definition impossible to test the identifying assumption. We urge the reader to interpret our findings with this important caveat in mind.

III.C JSY Coverage

We will also use data on the extent to which the JSY was implemented within a district such that our impact estimates capture the magnitude of the behavioural response to the incentives. If the financial incentives of the JSY are to bite, households should be exposed to information about the programme[17] and financial incentives should reach eligible women. Data on the latter provide the basis for our measure of JSY penetration. Specifically, we will use the term JSY *coverage* to refer to the proportion of women giving birth in a public health facility who received the financial incentive.[18] Full coverage thus implies every woman giving birth in a public health facility receives the financial incentive.

The data show considerable variation in JSY coverage across districts. Figure 2 maps by district the average coverage of the JSY over the period 2005/06-2007/08. Of the 587 districts for which we have data, JSY coverage is less than 10 percent in 114 districts and more than 40 percent in 188 districts. As expected, JSY coverage at the end of the study

[17] A study carried out in 2008 in the high-focus states of Bihar, Orissa, Uttar Pradesh, Madhya Pradesh and Rajasthan found that four-fifths of women were aware of the scheme and almost half of women giving birth in a health facility received the JSY cash (UNFPA, 2009).

[18] While the JSY is not limited to the public sector, our measure of coverage considers only public sector recipients of the financial incentive because only some nonstate health providers – in contrast to all health providers in the public sector – were accredited and able to participate in the JSY.

period is higher in those districts which started the programme earlier. For example, coverage in 2007/08 is 55 percent in districts that first introduced the JSY in 2005/06, compared with 38 percent in districts that started in 2007/08.

IV. Empirical Strategy

IV.A Identification of Impacts

We use a difference-in-difference approach to identify the impact of the JSY on our study outcomes. A simple way to implement this strategy would be to compare changes over time in health care utilisation and health status for districts that introduce the JSY relative to those districts that do not. More precisely, we would run a regression of each outcome on a dummy for whether the district introduced the JSY, controlling for year and district fixed effects. The fixed effects would absorb variation due to common temporal shocks and time-invariant district factors. The remaining cross-district variation in the evolution of the outcome would identify the parameter on the JSY treatment indicator.

In one sense, interpretation of this coefficient is straightforward and one that would appeal to policymakers – for it reveals the (intent-to-treat) effect of introducing the JSY on the study outcomes.[19] It is useful as an historical record or audit of the success of the JSY during the period 2005-2008. On the other hand, interpretation is obscured by the fact that a given effect of introducing the JSY could reflect a large behavioural response to the financial incentives if within-district programme coverage was low or a smaller effect if implementation was more complete. The interpretation thus depends critically on the extent of implementation within districts.

To make the magnitudes of the impact parameters more directly interpretable, we use information on the coverage of the intervention and an instrumental variable (IV) approach to scale our estimates.[20] More precisely, we include JSY coverage in the right hand side and instrument for it using our indicator for when the JSY was first introduced. In this way, the model is identified by variation in the timing of the introduction of the JSY but the impact parameter is interpreted as the effect of the JSY at *full coverage*. If instead the model were identified purely by variation in JSY coverage, there would be legitimate concerns as to whether the regression estimates are consistent. Districts with higher JSY coverage are also those that are likely to implement effectively other health programmes. District-level unobservables, such as management and the capacity of district health authorities, may therefore generate a spurious relationship between the study outcomes and coverage of the JSY. Pursuing an IV strategy not only improves interpretation but also addresses the potential problem of measurement error in JSY coverage that arises due to the imprecise wording of the particular question in the DLHS-3 questionnaire.

Formally, let y_{idt} denote our outcome, a binary measure of service utilisation or health status for observation i in district d in year t. Let JSY_{dt} denote our measure of programme coverage in district d in year t. Consider the equation:

[19] For this reason we do report these results as the reduced-form second-stage estimates of the effect of our instrument, the introduction of the JSY, on the main study outcomes.

[20] Gentzkow and Shapiro (2008) use a similar approach to scale their estimates of effect of television on adolescent test scores.

$$y_{idt} = \beta_0 + \beta_1 \text{JSY}_{dt} + \vartheta_t Z_d \beta_2 + X_{idt}\beta_3 + \omega_d + \tau_t + \varepsilon_{idt}, \quad (1)$$

where ω_d and τ_t are district and year fixed effects respectively; X_{idt} is a vector of individual demographic characteristics including education of the mother, education of the husband, maternal age, a household wealth asset score and dummies for (categories of) urban residence, religion, ethnicity, and parity; and Z_d is a vector of district-level characteristics which we discuss in more detail below. We cluster our standard errors by district to deal with concerns of serial correlation (Bertrand *et al.*, 2004).

To address several sources of endogeneity in the timing of the introduction of the JSY in a district, we include interactions between the year of birth and the share of the district population below the poverty line, the tribal population share, and the district mean of the household wealth asset score, represented by the term $\vartheta_t Z_d$. Discussions with stakeholders and our reading of the policy literature on the JSY suggest these district characteristics were influential in the decision on where to first introduce the JSY. Data used to generate these district-level variables come from the DLHS-3,[21] which means we are controlling for differential trends based on 2008 values rather than actual trends.

We estimate equation (1) by two-stage least squares (2SLS) and instrument for JSY_{dt} with the dummy for whether the district had introduced the JSY in year t. The first-stage equation takes the form:

$$\text{JSY}_{dt} = \delta_0 + \delta_1 \text{INTRO}_{dt} + \vartheta_t Z_d \delta_2 + X_{idt}\delta_3 + \omega_d + \tau_t + u_{idt}, \quad (2)$$

where INTRO_{dt} indicates whether district d had introduced the JSY in year t. It is variation in the instrument that identifies the effect of JSY coverage on the outcomes of interest. The identifying assumption underlying the analysis is that health care utilisation and health status in districts that introduced the JSY would not in the programme's absence have changed differently from those that did not introduce the JSY. We provide evidence on the plausibility of this identifying assumption later on in Section V. When INTRO_{dt} is orthogonal to the error term in equation (1), then the impact parameter β_1 is the causal effect of the JSY *when implemented at full coverage* on study outcomes. We interpret the 2SLS estimates as a local average treatment effect or LATE (Imbens and Angrist, 1994). They are, in other words, local to the women whose exposure to the financial incentives was influenced by the introduction of the JSY. In essence, this impact parameter "filters out" implementation, isolating the effect of the financial incentives as the mechanism within the JSY.

As is clear from equation (1), we run regressions of each outcome using individual level data to make the most of the rich micro dataset at our disposal. This allows us to include controls for a range of individual demographic characteristics that might affect health

[21] Our measure of poverty is constructed using information relating to the government system of identifying poor households. Specifically, it is based on responses to the question: "Does this household have a below the poverty line (BPL) card?" Because we are interested in controlling for sources of endogeneity that arise from government decision making processes, this poverty measure – rather than one measured perhaps more reliably in terms of household consumption – is particularly appropriate for our purposes.

care utilisation and health status. The model could be estimated using data aggregated at the district level and indeed when we do this the analysis generates estimates of impact that are almost identical to those reported in the paper, albeit with larger standard errors (results available from authors). In using individual level data, we note that the unit of observation differs according to the outcome. Each observation is a delivery (the most recent only) in the utilisation equations, and a live birth in the mortality equations.

IV.B First Stage and Reduced-Form Results

Table 2 presents in column (1) the first stage results in which we regress JSY coverage on the dummy for whether the district introduced the JSY. As expected, the results show that introducing the JSY has a strong positive effect of 26 percentage points on the coverage of the JSY within a district. In other words, introducing the JSY in a district increases the proportion of births in a public health facility for which women receive the cash incentive. The coefficient estimate is statistically significant at the 1 percent level and the *F*-statistic on the instrument is sufficiently strong to avoid bias due to weak instruments (Stock et al., 2002). Note also that the mean of JSY coverage before the programme was introduced in districts is greater than zero, capturing cash payments made under the preceding National Maternity Benefit Scheme and to women seeking care in JSY districts outside of where they live.

In columns (2) to (5) we present the reduced-form estimates of the effect of introducing the JSY on our main study outcomes from the second-stage equation. The coefficients in these models are interpreted as the effect of introducing the JSY into a district on the outcomes of interest, useful as a record of the programme's impact during the period 2005-2008. The result in column (2) shows a positive, significant effect of the JSY on births in a health facility. The estimate of 2.8 percentage points is small both in absolute terms and relative to the baseline mean of 39 percent. When we include demographic controls in column (3), the model is estimated with more precision and the point estimate increases slightly to 3 percentage points.

Despite the increase in utilisation, columns (4) and (5) show that introducing the JSY had no effect on neonatal mortality. The direction of the effect is negative, as expected, but the coefficient estimates in both models are not statistically significant at conventional levels. Our preferred estimate in column (5) implies that we are able to reject reductions in neonatal mortality due to the JSY of greater than 2.7 deaths per 1,000 live births. These preliminary findings cast doubt on the extent to which the JSY has been able to improve health outcomes.

V. Main Results

V.A Use of Health Care and Mortality

Table 3 presents IV estimates of the effect of the JSY on the various measures of utilisation. We follow equation (1) in controlling for district and year fixed effects, district characteristics interacted with the year dummies and individual demographic characteristics. For each outcome we also present a baseline estimate from a model without individual demographic controls.

Column (1) shows the results for health workers in attendance at delivery. The JSY at full coverage is associated with an 8 percentage point increase (from a baseline mean of 46 percent) in the proportion of women who give birth with a health worker in attendance. Columns (2) and (3) show the effect of the JSY on health facility births. The point estimates indicate that the JSY at full coverage leads to a 12 percentage point increase in facility births and a 19 percentage point increase in public facility births. We discuss the difference between these two estimates below in relation to unintended consequences of the programme. Columns (4) to (6) present the effect of the JSY on utilisation by each type of public health facility. These results imply that the impact on public health facility births is driven largely by increases in births at community health centres and primary health centres. For example, the JSY at full coverage is associated with a 9 percentage point increase in births at primary health centres, relative to a baseline mean of 2.6 percent. In contrast, district hospitals account for only a small proportion of the treatment effect. These findings suggest a large expansion in access to public health providers below the district hospital.

Column (7) shows that there is no evidence the JSY had a substantial effect on utilisation of antenatal care services. The point estimate on the outcome indicating whether a woman received three or more antenatal care visits is small and statistically insignificant in the specification without demographic controls. This result holds irrespective of how we define the antenatal care outcome (result not shown).[22] Inclusion of the demographics increases the point estimate and its significance marginally. This finding of little or no impact comes as no surprise given that the financial incentive in the JSY is not explicitly tied to the use of antenatal care. Indeed, antenatal care may be considered a placebo outcome with the result suggesting that the JSY treatment indicator is not simply acting as a proxy for other government policies aimed at strengthening maternal health services. In the Appendix, Table A2 shows that the JSY had no significant effect on the rate of caesarean sections and a small, positive effect on assisted deliveries, significantly different from zero at the 10 percent level.

Taken together, the results on utilisation paint a consistent picture. The JSY had a positive effect on utilisation of maternity services – but little or no effect on antenatal care, a behaviour that was not explicitly incentivised. The impact of maternity services is greatest at lower levels of health provider, where only basic health services are available.

[22] Alternative measures of antenatal care utilisation include the total number of antenatal care visits and the proportion of women who have at least one antenatal care visit in a health facility.

For example, at the first level of referral, only 18 percent of community health centres offer caesarean sections and less than 10 percent have blood storage facilities (International Institute for Population Sciences, 2010). This may explain why the results show no impact on the caesarean section rate, a procedure that requires a high level of technical ability and resources.

We next turn to the mortality results presented in Table 4. IV estimates in column (1) show that the JSY had a negative, though statistically insignificant, effect on neonatal mortality. Our preferred point estimate indicates that we are able to rule out with 95 percent confidence a negative effect of the JSY at full coverage on neonatal mortality larger than 10.4 deaths per 1,000 live births. In columns (2) to (4) we separate out neonatal mortality into its constituent parts. These definitions of mortality are far from standard. However, we anticipate that if the JSY were to reduce mortality, this effect would be strongest within the first 24 hours of childbirth when maternity care is provided. In column (2) we test for this possibility, with the results showing a stronger negative but still statistically insignificant effect of the JSY on mortality.[23] Columns (3) and (4) confirm that there was no effect of the JSY on later neonatal mortality, which provides some confidence that the findings in column (2) are not spurious for we would not anticipate maternity care to have a direct effect on the mortality of the baby after the mother is discharged to go home.

These findings give rise to the question why the increase in use of maternity services has not translated into improved health outcomes. One explanation points to limitations of quality of care. While we examine this hypothesis in more detail in Section VI and are reluctant to draw firm conclusions here, we note that the JSY increased access to maternity services at health facilities below the district hospital, which are less able to manage emergency complications at childbirth. It is also important to note that our mortality findings of no effect contradict those from the closest antecedent to this paper, which concluded that the financial incentives of the JSY were effective in reducing neonatal mortality (Lim et al., 2010).

V.B Magnitudes and Simple Cost-Effectiveness

The magnitudes of effects in our study can be gauged by using our reduced-form coefficient estimate from column (3) in Table 2 to examine what utilisation of maternity services would have been in a number of states with and without the JSY. The vast majority of districts in the state of Madhya Pradesh introduced the JSY early. If instead they had not started the programme, facility births would have been 7 percent lower than the level in 2007/08. No districts in the state of Punjab introduced the JSY during the study period but if they all had, facility births would have been 105 percent higher than the 2007/08 level. These magnitudes are clearly modest.

[23] We can statistically rule out a reduction in mortality within the first 24 hours due to the JSY of larger than 8.7 deaths per 1,000 live births.

According to our reduced-form estimates, the JSY encouraged an additional 580 thousand women in India to give birth in a health facility in 2007/08. In a back-of-the-envelope calculation using programme expenditure data, we estimate that the government spent $357 of JSY money for each additional facility birth.[24] Because the financial incentive is given irrespective of whether the individual would have given birth in the health facility in the absence of the JSY, the cost per marginal visit is clearly high. Using data on the cost of delivery from Bonu et al (2009), we calculate a total cost of $486 for each additional facility birth.[25] However, while a cost to the government, one could argue that the financial incentives should not be considered a cost at all since they represent a transfer of resources. The cost to society then is only the deadweight loss associated with taxation, the administrative cost of running the JSY and the cost of providing delivery care services.

In contrast, our IV results suggest that the programme when implemented at full coverage would have incentivised around 2.1 million more women to give birth in a health facility. This comparison between the reduced form and the IV results – the difference between the effect of JSY placement and the effect of the financial incentives as the mechanism within the JSY – is revealing. It highlights what might be called an "implementation gap" and suggests an important opportunity to improve the effectiveness of the programme on utilisation.

There is a growing literature on demand-side incentives in health against which to compare the magnitudes of our estimated effects, although few are specific to maternal health. Experimental evidence comes from studies of conditional cash transfers in Mexico (Fernald et al., 2008; Gertler, 2000; Gertler, 2004), Nicaragua (Maluccio and Flores, 2005), Brazil (Morris et al., 2004b), Ecuador (Paxson and Schady, 2008) and Honduras (Morris et al., 2004a), one-off financial incentives in Malawi (Thornton, 2008), and non-financial incentives in India (Banerjee et al., 2010). The interventions in these studies are targeted towards poor families and most provide some evidence of positive effects on utilisation of health services and immunization coverage.[26]

In Malawi, a small-scale project was found to increase the percentage of individuals who collected their HIV test results by 44 percentage points (Thornton, 2008). Perhaps the most well-known CCT programme is Mexico's *Oportunidades*, which was shown to increase health clinic consultations by 2.1 visits per day (Gertler, 2000). The CCT programme in Honduras increased utilisation of prenatal care by women, routine

[24] This figure is likely to represent a minimum cost since we have not factored in administration of the JSY, whose economic cost is not captured by programme expenditures. If we assume conservatively administration costs represent 10 percent of programme spending, expenditure per additional facility birth is $393.

[25] Bonu and colleagues (2009) report estimates of household expenditure on delivery care from India's National Sample Survey in 2004. We use household expenditure on a private facility birth on the basis that this better reflects the full economic cost of giving birth. Because the public sector is subsidised, expenditure on a public facility birth is likely to be a gross underestimate. While crude, our cost estimate gives a sense of the order of magnitude. Note that the financial data are adjusted for inflation.

[26] We report below effects that were shown to be significant but note that the studies also show evidence of no effect on numerous other utilization outcomes, which are summarized in a systematic review of CCT programmes by Lagarde and colleagues (2007). Robust causal estimates from nonexperimental studies include CCT programmes in Columbia (Attanasio *et al.*, 2005), Turkey (Ahmed *et al.*, 2007), and Chile (Galasso, 2007).

paediatric examinations and child growth monitoring by 19 percentage points, 20 percentage points and 16 percentage points respectively (Morris et al., 2004a). A similar programme in Nicaragua increased utilisation of child preventive health visits by 11 percentage points (Maluccio and Flores, 2005). No significant impact of CCTs on health visits was found in Ecuador (Paxson and Schady, 2008). Finally, in the Indian state of Rajasthan, lentils were offered alongside immunization camps, raising full immunization rates to 39 percent compared with 6 percent in control and 18 percent in immunization camp only villages (Banerjee et al., 2010). The evidence on financial incentives in health is limited almost exclusively to the use of simple health technologies. More complex health services, whose quality of care is more difficult for patients to assess, have rarely been targeted with demand-side financial incentives.

V.C Unintended Consequences

Our results thus far have focused on outcomes the JSY was intended to improve. However, high powered incentives have the potential to influence a broad range of behaviours, which in turn may have both positive and negative implications for welfare. Here we study three possible effects of such incentives.

First, economic theory predicts that reducing the price of the health services in the public sector using financial incentives will increase demand through a substitution effect. In other words, we expect the JSY to increase demand for public maternity services, in part, through a substitution away from private health providers. Second, some have argued that cash payments for delivery or child health care provide an incentive to become pregnant. Experimental findings from a study of conditional cash transfers in Honduras suggest that the intervention increased fertility (Morris *et al.*, 2004a; Stecklov *et al.*, 2006). Third, financial incentives for delivery care may have positive benefits through changes in health-related behaviours subsequent to childbirth, such as breastfeeding. The idea is that women who give birth in a health facility are more likely to be exposed to information on the benefits of timely breastfeeding.[27]

We investigate the causal link between the JSY and two of the outcomes – private health care seeking and breastfeeding behaviour –using the same econometric specification as previous. In the analysis of fertility, we modify the approach to take advantage of the opportunity to include individual- rather than district-level fixed effects. To do so, we use data on the birth histories from the DLHS-3 only. More formally, we run panel regressions of the form,

$$y_{idt} = \beta_0 + \beta_1 \mathrm{JSY}_{dt} + \vartheta_t Z_d \beta_2 + \vartheta_t X_{id} \beta_3 + \alpha_i + \tau_t + \varepsilon_{idt}, \qquad (3)$$

[27] We also considered other health-related behaviours potentially influenced by exposure to information during childbirth, including postnatal care seeking, whether the baby was immediately wiped dry and wrapped, and whether a sterilized blade was used to cut the umbilical cord. The DLHS, however, provides no scope for measuring these outcomes consistently between the two survey rounds. Child immunization was not regarded as a plausible indirect outcome given the long time lag between childbirth and vaccinations.

where y_{idt} is the probability of being pregnant, α_i is an individual fixed effect, and the term $\vartheta_t X_{id}$ represents interactions between year dummies and a vector of individual demographic characteristics, measured at the time of interview. Inclusion of the individual fixed effects allows us to control for unobserved, time-invariant factors that affect fertility at the individual level. We continue to instrument for JSY_{dt} with the indicator for whether the district introduced the programme. The unit of observation is a woman-year and because we are using only the DLHS-3 we have one year instead of four years of pre-JSY data.

Before turning to estimation, we note that the policymakers anticipated some of these unintended consequences in the design of the JSY. By limiting the cash payment to women with two or fewer children in the low focus states, partial safeguards were in place to mitigate the risk of incentivising pregnancy. The intention was also to expand implementation of the JSY into the private sector through an accreditation process, thereby limiting incentives for substitution between health providers. Despite good intentions, these policy measures were either inadequate or not implemented, as we discuss below.

Table 5 presents the results on unintended consequences of the JSY. Column (1) shows that the JSY at full coverage reduced utilisation of the maternity services in the private sector by 7.2 percentage points. For reference, we reproduce in column (2) previous findings on utilisation of services in the public sector. Substitution away from the private sector thus accounts for more than one-third of the effect of the JSY on public facility births. Descriptive statistics from the DLHS data lend support to these findings by showing that the JSY has been predominantly a public sector programme despite the stated policy to involve private health providers. Only 6 percent of women giving birth in a private health facility in JSY districts received the financial incentive, compared with 34 percent in the public sector.[28] Moreover, only 10 percent of JSY beneficiaries nationwide gave birth in a private health facility. The mortality results suggest that this substitution between providers is unlikely to have had any detrimental effect on mortality but it may have led to a reduction in household expenditure on health care and, in turn, an improvement in living standards.[29]

We next look at the fertility results. For women who report being pregnant at the time of interview, we have no information on when they became pregnant to assign the pregnancy to a specific year. We therefore report two sets of results, based on alternative assumptions to construct the outcome measure. In column (3), we assume that women are six months pregnant if pregnant at the time of interview and find that the JSY increases the likelihood of pregnancy by 1.1 percentage points. A second, more appealing, approach is to use a random number generator, constrained between three and nine, to determine the number of months a woman is pregnant, if pregnant at the time of

[28] This comes as no surprise given the need for private health providers to go through a process of accreditation if they are to participate in the JSY programme.

[29] The literature on the household cost of health care at childbirth provides consistent evidence that expenditures on maternity services in the private sector far exceed those in the public sector (Bonu *et al.*, 2009; Borghi *et al.*, 2006; Powell-Jackson and Hoque, 2011).

interview. The result, in column (4), shows a stronger effect on pregnancy with a point estimate of 1.7 percentage points. [30] This, our preferred estimate, is large given the baseline mean of 8.8 percent.[31]

From these results it is not possible to say whether the effect of financial incentives on pregnancy reflects an increase in the total lifetime number of children a woman has or a reduction in birth spacing. Either way, it represents an unintended consequence and raises several concerns. First, the JSY appears to be acting against government population policy. Second, the JSY may be counterproductive to its own objectives – birth spacing and total fertility are important underlying causes of maternal and neonatal mortality (Zhu et al., 1999).

This finding may take on less meaning if the increase in the likelihood of pregnancy reflects a short-lived, transitory effect of introducing the JSY. We test for this possibility in the reduced form equation by including interactions between JSY introduction and two dummies measuring the number of years the JSY has been active in a district at time t (we omit the first year in which the JSY is introduced). The pattern of the coefficient estimates show that, if anything, the positive effect of the JSY on fertility *increased* as the programme matured.[32] The effect on fertility therefore cannot be written off as a temporary phenomenon; the evidence suggests it is a more permanent feature of the programme.

The risk of increased fertility was partly anticipated by policymakers in the design of the JSY and these safeguards provide some motivation to scrutinise the validity of the fertility results. If women with more than two children were unable to receive the JSY cash, why would they be incentivised to become pregnant? However, the policy of limiting the cash payment to women with two or fewer children applied only to the low focus states and was difficult to implement. DLHS-3 data show that the probability of a woman receiving the cash incentive after giving birth in a public health facility is statistically the same across parity groups, a pattern which suggests policy attempts to mitigate this unintended consequence provide no reason for questioning the validity of the fertility results. [33]

[30] In a bounds check we estimate the effect of the JSY on fertility, taking extreme values for the number of months a woman is pregnant, if pregnant at the time of interview. When we assume nine months, we find that the JSY led to a 1.0 percentage point (standard error 0.0089) increase in the likelihood of becoming pregnant, our most conservative estimate. If we assume three months, the fixed effect regression gives a point estimate of 2.7 percentage points (standard error 0.0065).

[31] A third approach might seek to model seasonality in pregnancy. The data, however, show that the probability of pregnancy differs little across months of the year.

[32] The reduced form coefficient estimates indicate that the JSY had an additional effect on fertility of 0.2 percentage points (standard error 0.001) in the second year since introduction and 0.9 percentage points (standard error (0.003) in the third year introduction. While these results may be driven by greater coverage of the JSY in the second and third year of implementation, they provide little support for the argument that the effect on fertility is transitory.

[33] The percentage of women in JSY districts who received the cash incentive conditional on giving birth in a public health facility is as follows: first birth (33.0 percent); second birth (32.5 percent), third birth (29.1 percent); fourth birth (33.4 percent); and fifth or higher birth (35.5 percent). While these data are not perfect – the number of times a woman has given birth does not necessarily equal the number of living children – they are highly suggestive of the policy not being effective in practice.

Columns (5) and (6) of Table 5 report the results on breastfeeding within the first hour and the first 24 hours of birth respectively. The point estimate in column (5) is a statistically significant 7.4 percentage points, suggesting that the JSY increased breastfeeding in the first hour. The result in column (6) shows that when we lengthen the time period to cover breastfeeding within the first 24 hours of birth, the effect of the JSY dissipates. We therefore conjecture that the positive effect on breastfeeding reflects increased exposure to information from health workers around the time of childbirth. While these findings on breastfeeding concern just one behaviour, they highlight the potential for financial incentives to have positive benefits on other, non-incentivised health-related behaviours.

V.D Robustness

Our estimates of effect have a causal interpretation in so far as our identifying assumption holds that placement of the JSY is orthogonal to the error term after controlling for district level determinants. While it is by definition impossible to formally test this assumption, we can mitigate concerns of bias due to non-random placement of the JSY by pursuing several robustness checks. Before we do so, we reiterate the result that shows the JSY had little or no effect on utilisation of antenatal care – which we interpret as evidence supporting the notion that JSY placement is not simply acting as a proxy for other closely-related health policies.

First, we examine whether introduction of the JSY next year is associated with the study outcomes. If unobservables are driving both the introduction of the JSY and changes in outcomes, we may expect evidence of pre-existing trends (so long as these unobservables change slowly over time). A visual inspection of trends offers one approach to assessing pre-existing trends. Figure A3 in the Appendix shows little sign of divergent trends between the intervention and comparison districts before the JSY was introduced for any of the study outcomes.[34] To conduct a more formal test for whether trends in outcomes anticipate the introduction of the JSY, we include in the reduced form equations an additional dummy which indicates if the district introduced the JSY next year.[35]

Table 6 presents the results of this specification check for each of the study outcomes. With the exception of breastfeeding, the coefficient on the dummy indicating the introduction of the JSY next year is small, insignificant and, where expected, statistically different from the effect of the JSY this year. Introducing the JSY next year does not predict changes in these study outcomes, suggesting that the JSY dummy is not merely capturing the continuation of pre-existing trends. There is, however, evidence that changes in breastfeeding anticipate the introduction of the JSY and we must be cautious in interpreting the breastfeeding results as causal.

[34] While these graphs are informative with respect to the possibility of confounding trends (before the JSY was introduced), they are less helpful in providing visual evidence of impacts. Our reduced-form estimates of effect are typically too small to make meaningful conclusions from an inspection of trends.

[35] Because we have no information on whether the 163 non JSY districts introduced the JSY in 2008/09, the year following the end of our study period, we drop all 2007/08 observations in this robustness check.

Second, we examine whether the within-district variation over time in the coverage of the JSY that identifies the effect of the programme is correlated with the demographic characteristics of individuals. Following Gentkzow and Shapiro (2008), we generate a predicted value of JSY coverage for each individual using the first stage model, equation (1) but without controls for individuals characteristics, and then regress this variable on the full set of individual-level demographic controls.

Table 7 presents in column (1) the results of this robustness check. Column (2) shows the results when we do not control for district-level poverty, tribal population and wealth, and column (3) shows the relationship between the demographic characteristics and our main utilisation outcome, births in a health facility. The results in column (1) show that the demographic controls are not strongly correlated with predicted JSY coverage. The coefficient estimates in all cases are small and statistically insignificant at the 5 percent level. They suggest that once we control for district level characteristics, the remaining variation is largely idiosyncratic. In contrast, when we fail to include district poverty, tribal population and wealth, some of the coefficients on the demographic characteristics become highly significant (column 2). These results are all the more convincing in view of the fact that each of the demographic characteristics is highly correlated with facility births (column 3).[36]

Third, we examine the extent to which the results are sensitive to our definition of JSY introduction. Recall that we defined the year the JSY was introduced in a given district to be the first year in which the proportion of women giving birth in a public health facility who received a cash payment is 10 percentage points greater than the 2004/05 level. We experiment with a 20 percentage point and 5 percentage point increment. Table A3 in the Appendix reports IV estimates using our preferred model that controls for individual demographics. As shown in columns (2) and (3), the results are qualitatively similar for each outcome. The magnitudes and statistical significance of the estimated effects are broadly consistent across different definitions of JSY introduction.

[36] The fertility results raise the possibility of systematic differences emerging between JSY and non JSY districts in the characteristics of women who give birth. These differences could lead to bias in estimated treatment effects for our utilisation and mortality outcomes. Note that this potential problem would arise even if placement of the JSY were randomised. The specification checks in Table 7 account for this possibility.

VI. Heterogeneity in Impacts

In this section we go beyond average effects to investigate the extent to which the impact of the JSY varies along relevant dimensions. Understanding heterogeneity in the effect of the JSY is likely to be of interest to policymakers concerned with equity. It can also shed light on how financial incentives work to improve care seeking behaviour and health status.

We begin by examining how the effect of the JSY is distributed along several standard dimensions of socioeconomic status. These can be considered demand-side factors that may modify the effect of the JSY on health care seeking behaviour. To study heterogeneity in the effect of the JSY with respect to socioeconomic status, we use our same instrumental variable approach on subsamples of women that are divided according to categories of mother's education, ethnicity and household wealth. All categories are mutually exclusive and collectively exhaustive. Education is split into women with no education and those with some education. Women are separated into two quantiles according to the household wealth asset score. Ethnicity is divided into two categories: women belonging to scheduled castes, scheduled tribes or other "backward ethnic groups" and women who belong to none of these groups.[37]

Table 8 presents the JSY treatment effects across various subsamples with public facility births as the dependent variable. The first two columns show that the effect of the JSY at full coverage on utilisation is greater amongst women with no education (21 percentage points) than women with some education (16 percentage points). In terms of relative impacts, this difference is considerable. The next two columns compare the treatment effect between the two wealth groups, with point estimates showing a similar pattern to the education results. Poorer women are more likely to give birth in a public health facility in response to the JSY than richer women. The results in columns (5) and (6) again document a similar pattern with respect to ethnicity of the household. The JSY increases utilisation amongst women belonging to marginalised ethnicities by 19 percentage points, while the treatment effect is 15 percentage points for nonmarginalised women. There is a consistent pattern across the results demonstrating that the behavioural response to the financial incentives of the JSY is greater amongst women who are more disadvantaged. The findings imply that the JSY contributed to greater equity in access to maternity services.

In the final two columns of Table 8 we run separate regressions for districts considered by the JSY programme as high-priority and those considered low-priority districts.[38] The effect of the JSY is therefore identified using district variation in the introduction of the programme within each subsample of districts. Results show that the JSY increased public facility births in the high focus districts by 21 percentage points. In contrast, the

[37] Here we have used the exact language of the various ethnic categories given in the DLHS questionnaires.
[38] The high focus states are also called low-performing states in JSY policy documents in reference to the fact that a low proportion of women give birth in health facilities. They include Bihar, Chattisgarh, Jharkhand, Orissa, Utta Pradesh, Uttaranchal, Rajasthan, Madhya Pradesh, Assam and Jammu and Kashmir).

point estimate is statistically insignificant in the low priority districts, indicating no strong behavioural response to the financial incentives in these areas of India. While it is difficult to unpack with certainty the reasons behind this finding, we note that the financial package was more generous in the high priority states.

These findings also speak to the robustness of the our main results for the effect of the JSY in the high-priority districts is remarkably close to that reported in column (3) of Table 3. They imply that when we restrict the analysis to a subsample of districts whose characteristics are much more similar, the magnitude of the coefficient estimates remains almost unchanged. In other words, we can be more confident that the comparison districts in the analysis of the full sample provide an appropriate counterfactual.

We next examine heterogeneity in the effect of the JSY on mortality with respect to the structural quality of care of health care providers (Donabedian, 1966). As discussed previously, whether greater access to maternity services translates into health improvements depends on the supply-side. It is this hypothesis we wish to test for in the following analysis. Using data from a survey of public health providers that was conducted in parallel with the DLHS-3, we develop a quality of care index for the district hospital, community health care centres and primary health centres. Our measure of structural quality of care is made up of six components: 24 hour service availability; staffing; training of staff; basic infrastructure; equipment; and drugs. Within each category there are a number of binary indicators that relate to specific questions on the availability of inputs. We compute the share of inputs that are available in each category and then take an unweighted average across all the categories. The quality of care index is normalised to have mean zero and standard deviation one, then aggregated at the district level because we are unable to link specific health facilities to administrative areas within a district.

The econometric analysis proceeds on the basis of the following model:

$$y_{idt} = \beta_1 \text{JSY}_{dt} + (\text{JSY}_{dt} \times Q_d)\beta_2 + \vartheta_t Z_d \beta_3 + X_{idt}\beta_4 + \omega_d + \tau_t + \varepsilon_{idt}, \quad (4)$$

where $Q_d$is an index of quality of care for one of three types of public health provider. We interact the JSY introduction dummy with the demographic controls and use this vector to instrument for both JSY_{dt} and the interaction term $(\text{JSY}_{dt} \times Q_d)$. If the JSY leads to a greater improvement in mortality in districts where quality of care is higher, we would expect the coefficient on the term $(\text{JSY}_{dt} \times Q_d)$ to be negative.

Table 9 reports the results from estimations of equation (4). The left hand side variable in column (1) is mortality within the first 24 hours of birth and the quality of care index in the interaction term refers to primary health centres. The coefficient on the interaction between JSY coverage and quality of care is negative, in keeping with expectations. Its estimate implies that a one standard deviation increase in the quality of care index reduces the marginal effect of the JSY at full coverage on mortality by 3 deaths per 1,000 live births. The point estimate, however, is not statistically significant.

Column (2) repeats the specification in column (1), replacing quality of care at primary health centres with quality of care at community health centres in the interaction term. Again the coefficient on quality of care interacted with JSY coverage is negative and insignificant. Finally, we see a similar pattern in column (3) when we use the district hospital index as our measure of structural quality of care.

To summarize, we find no evidence to suggest that the JSY led to greater improvements in mortality in districts with better quality of care. In other words, the average treatment effect on mortality does not appear to mask important differences with respect to quality of care. The absence of a differential effect of the JSY according to our proxy for quality of care is surprising and we do not have a definitive answer. One potential explanation is that our measures of mortality and quality of care are simply not appropriate in this particular context. One could argue that maternal mortality should be the outcome of interest. While there is some merit to this argument, we might reasonably expect an effect on early neonatal deaths. Almost 50 percent of neonatal deaths in our sample occur within the first 24 hours when medical intervention at childbirth has the potential to be effective. Similarly, quality of care could be better measured in terms of the process of care.[39]

A second and, in our view, more persuasive explanation is that the JSY incentivises women predominantly to health facilities whose purpose is not to manage life-threatening complications. However good the quality of care in health institutions below the district hospital, it may remain inadequate to save the lives of women and their baby, particularly when obstetric emergencies require intensive rather than obstetric care (Costello *et al.*, 2006). Having a fully functional referral system is thus critical for the success of any intervention which seeks to increase access to institutional delivery care, as argued by Campbell and Graham (2006).

[39] With some notable exceptions (Das and Hammer, 2006), it is not uncommon for quality of care to be measured using information on the availability of inputs. This remains an important area for further research.

VII. Conclusions

In this paper, we have examined the causal link between one of the world's largest demand-side financial incentive programmes and health-related outcomes in India. Consistent with much of the literature, we find that the financial incentives in the JSY are effective in increasing use of formal health services, particularly at lower levels of the public health system. However, there is an implementation gap. Because of incomplete coverage of the JSY, the increase in use of formal maternal health care due to the programme has been modest. Our findings on neonatal mortality show no effect, suggesting that recent evidence on the JSY may require reassessment.

We have argued that high powered incentives have the potential to influence a broad range of behaviours, intended or otherwise. Any evaluation of financial incentives should go beyond the narrow objectives of the programme to examine potential unintended consequences. Our fertility results are particularly striking because they suggest a pathway through which the programme's own objective of reducing maternal and neonatal mortality may be undermined. It also serves to demonstrate the importance of anticipating such risks in the programme design and, in turn, ensuring appropriate measures are put into practice.

A further point of discussion relates to the generalisability of our findings to an expanded JSY programme, say five years down the line. One possibility is that the effects in this paper may be larger than those observed when the JSY finally reaches all districts in India. Early introduction of the JSY was understandably prioritised in districts that contain poorer and predominantly tribal populations and evidence on impact heterogeneity suggests that these districts were the ones where the greatest benefits from the programme could be realised. Thus, extending our estimates of effect to districts that since 2008 have introduced the JSY may not provide a good approximation to the true impact of the financial incentives in these districts.

The collective evidence in this paper, on both intended and unintended effects, points towards the need for policymakers to be cautious in the use of financial incentives. For example, even though it is self-evident that the supply-side must be in place if demand-side financial incentives are to work, there is a proliferation of schemes in countries where the quality and even availability of care is vastly inadequate. Future research on this topic should broaden its scope to address questions around the ethics and acceptability of financial incentives, their long-term effects, and the potential harms caused by them (Lagarde et al., 2007; Marteau et al., 2009).

Table 1
Summary Statistics

	Mean	Standard deviation	Number of observations
Panel A. Study outcomes			
Neonatal mortality	0.0300	0.170	429,514
Health worker in attendance at delivery	0.476	0.499	344,925
Delivery in a health facility	0.412	0.492	344,925
Public health provider	0.228	0.419	344,925
Private health provider	0.185	0.388	344,925
Caesarean section	0.0768	0.266	344,904
Assisted delivery	0.0222	0.147	344,904
Three or more antenatal care visits	0.452	0.498	342,301
Breastfeeding within one hour	0.356	0.479	336,288
Pregnant this year	0.0827	0.275	2,549,485
Panel B. Demographic variables			
Urban	0.221	0.415	344,925
Hindu	0.761	0.426	344,925
Scheduled caste	0.187	0.390	344,925
Scheduled tribe	0.172	0.377	344,925
Other backward caste / tribe	0.403	0.490	344,925
Maternal age (years)	24.79	5.300	344,925
Number of live births	2.589	1.404	344,925
Woman's education (grades completed)	4.410	4.893	344,925
Husband's education (grades completed)	6.648	5.037	344,925
Household wealth asset score	-0.0380	1.970	344,925

Notes: Summary statistics are based on data from the DLHS-2 and DLHS-3, including only observations over the period 2001/02 – 2007/08. The unit of observation is a woman's most recent delivery, except in the case of neonatal mortality (live birth) and pregnant this year (woman-year). Assisted delivery includes the use of forceps or a ventouse. The household asset wealth score is generated by applying principal component analysis to a set of household asset ownership variables.

Table 2

Reduced-Form Estimates of the Effect of JSY on Health Care Use and Neonatal Mortality

Dependent variable:	JSY coverage	Delivery in a health facility		Neonatal mortality	
	(1)	(2)	(3)	(4)	(5)
JSY introduced in district	0.26***	0.028***	0.030***	-0.00028	-0.00038
	(0.010)	(0.0057)	(0.0051)	(0.0012)	(0.0012)
District and year fixed effects	YES	YES	YES	YES	YES
District demographics × year	YES	YES	YES	YES	YES
Demographics	NO	NO	YES	NO	YES
Mean of dependent variable at baseline	0.048	0.394	0.394	0.0307	0.0307
	[0.107]	[0.489]	[0.489]	[0.173]	[0.173]
$F(1, 586)$ stat on instrument	635.91	24.01	33.88	0.06	0.11
p value	(<0.0001)	(<0.0001)	(<0.0001)	(0.8103)	(0.7416)
Number of observations	344,923	344,923	344,923	429,445	429,445
Number of districts	587	587	587	587	587

Notes: Data are from the DLHS-2 and the DLHS-3. *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors, corrected for clustering at the district level, are reported in parentheses. Standard deviation of the dependent variable mean is in square brackets. JSY coverage is the proportion of women giving birth in a public health facility who received the cash incentive. District demographics include controls for share of population below the poverty line, the tribal population share, and the mean wealth asset score. Demographics include controls for mother's education, husband's education, mother's age at birth, wealth asset score, and dummies for categories of urban dwelling, religion, ethnicity and number of live births. The unit of observation in columns (1) – (3) is a delivery (most recent only). The unit of observation in columns (4) and (5) is a live birth (based on the four-year recall birth history of a woman).

Table 3

2SLS Estimates of the Effect of JSY on Use of Maternal Health Care Services

Dependent variable:	Health worker in attendance at delivery	Delivery in a health facility	Delivery in public health facility	Delivery by type of public health facility			Three or more antenatal care visits
				Hospital	Community health centre	Primary health centre	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect of JSY at full coverage							
Baseline model	0.074***	0.11***	0.19***	0.032**	0.088***	0.087***	0.033
	(0.021)	(0.021)	(0.020)	(0.016)	(0.0097)	(0.0088)	(0.022)
Baseline model + demographics	0.081***	0.12***	0.19***	0.033**	0.088***	0.087***	0.039*
	(0.018)	(0.019)	(0.019)	(0.015)	(0.0096)	(0.0087)	(0.020)
Mean of dep. variable at baseline	0.463	0.394	0.197	0.135	0.0214	0.0262	0.445
	[0.499]	[0.489]	[0.398]	[0.342]	[0.145]	[0.160]	[0.470]
Number of observations	344,903	344,923	344,923	344,923	344,923	344,923	342,229
Number of districts	587	587	587	587	587	587	587

Notes: Data are from the DLHS-2 and the DLHS-3. Estimates are from instrumental variable models in which the introduction of the JSY is used to instrument for JSY coverage. *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors, corrected for clustering at the district level, are reported in parentheses. Standard deviation of the dependent variable mean is in square brackets. Health worker is in attendance if the birth is in a health facility or at home with a doctor, nurse, midwife, or lady health volunteer. Baseline model includes fixed effects for district and year of birth, and interactions between year of birth and district share of the population below the poverty line, tribal population share, and mean wealth asset score. Demographics include controls for mother's education, husband's education, mother's age at birth, wealth asset score, and dummies for categories of urban dwelling, religion, ethnicity and number of live births. The unit of observation is a delivery (most recent only). Deviations in sample size are due to missing data.

Table 4

2SLS Estimates of the Effect of JSY on Neonatal Mortality

Dependent variable:	Neonatal mortality	Disaggregated measures of mortality		
		Death within 24 hours	Death between 2 and 28 days	Death between 8 and 28 days
	(1)	(2)	(3)	(4)
Effect of JSY at full coverage				
Baseline model	-0.0011	-0.0025	0.0014	0.00055
	(0.0046)	(0.0031)	(0.0030)	(0.0018)
Baseline model + demographics	-0.0015	-0.0027	0.0012	0.00051
	(0.0046)	(0.0031)	(0.0030)	(0.0018)
Mean of dep. variable at baseline	0.0307	0.0151	0.0156	0.0059
	[0.173]	[0.122]	[0.124]	[0.076]
Number of observations	429,445	429,445	429,445	429,445
Number of districts	587	587	587	587

Notes: Data are from the DLHS-2 and the DLHS-3. Estimates are from instrumental variable models in which the introduction of the JSY is used to instrument for JSY coverage. *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors, corrected for clustering at the district level, are reported in parentheses. Standard deviation of the dependent variable mean is in square brackets. Baseline model includes fixed effects for district and year of birth and interactions between year of birth and district share of the population below the poverty line, tribal population share, and mean wealth asset score. Demographics include controls for mother's education, husband's education, mother's age at birth, wealth asset score, and dummies for categories of urban dwelling, religion, ethnicity, number of live births, and a twin birth. The unit of observation is a live birth (based on the four-year recall birth history of a woman).

Table 5

2SLS Estimates of the Effect of JSY on Unintended Outcomes

Dependent variable	Place of delivery		Pregnant (2004 – 2008)	Pregnant (2004 – 2008)	Breastfeeding	
	Private health facility	Public health facility			Within 1 hour	Within 24 hours
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of JSY at full coverage	-0.072***	0.19***	0.011*	0.017***	0.074**	0.0063
	(0.013)	(0.019)	(0.0062)	(0.0057)	(0.030)	(0.037)
Individual fixed effects	NO	NO	YES	YES	NO	NO
District fixed effects	YES	YES	NO	NO	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
District demographics × year	YES	YES	YES	YES	YES	YES
Demographics	YES	YES	NO	NO	YES	YES
Demographics × year	NO	NO	YES	YES	NO	NO
Mean of dep. variable at baseline	0.197	0.197	0.087	0.088	0.321	0.553
	[0.398]	[0.398]	[0.282]	[0.283]	[0.468]	[0.497]
Number of observations	344,923	344,923	2,528,498	2,528,498	336,286	336,286
Number of districts	587	587	587	587	587	587

Notes: Data are from the DLHS-2 and the DLHS-3, except columns (3) and (4) which use pregnancy data constructed from the birth history of women in the DLHS-3 only. Estimates are from instrumental variable models in which the district introduction of the JSY is used to instrument for JSY coverage. *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors, corrected for clustering at the district level, are reported in parentheses. Standard deviation of the dependent variable mean is in square brackets. Column (3) assumes that women reporting being pregnant at the time of interview are six months pregnant. Column (4) assumes that the number of months a woman has been pregnant, if pregnant at the time of interview, is as good as random (constrained to be between three and nine months). District demographics include the share of district population below the poverty line, the tribal population share, and district wealth asset score. Demographics include controls for mother's education, husband's education, mother's age at birth, wealth asset score, and dummies for categories of urban dwelling, religion, ethnicity and number of live births. The unit of observation is a delivery (most recent only), except in columns (3) and (4) where it is a woman-year.

Table 6

Reduced-Form Estimates of the Effect of Future JSY on Study Outcomes

Dependent variable:	Health worker in attendance at delivery	Delivery in a health facility	Neonatal mortality	Pregnant (2004 – 2008)	Breastfeeding within 1 hour
	(1)	(2)	(3)	(4)	(5)
Effect of JSY introduction					
JSY introduced in district	0.018***	0.023***	-0.00059	0.0064***	0.014*
	(0.0048)	(0.0048)	(0.0013)	(0.0012)	(0.0079)
JSY introduced next year	-0.0040	0.0015	0.00039	-0.0017	0.018**
	(0.0043)	(0.0041)	(0.0011)	(0.0012)	(0.0080)
Number of observations	277,541	277,556	361,984	1,896,387	270,378
Number of districts	587	587	587	587	587

Notes: Data are from the DLHS-2 and the DLHS-3. *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors, corrected for clustering at the district level, are reported in parentheses. Standard deviation of the dependent variable mean is in square brackets. Column (4) assumes that the number of months a woman has been pregnant, if pregnant at the time of interview, is as good as random (constrained to be between three and nine months). Regressions include fixed effects for district and year of birth, interactions between year of birth and district share of the population below the poverty line, tribal population share, and mean wealth asset score, and a set of demographics that control for mother's education, husband's education, mother's age at birth, wealth asset score, and dummies for categories of urban dwelling, religion, ethnicity and number of live births. The unit of observation is a delivery (most recent only) in columns (1), (2) and (5), a live birth in column (3), and a woman-year in column (4). Observations in financial year 2007/08 are dropped for lack of data on JSY introduction next year.

Table 7

Correlation between Predicted JSY Coverage and Demographics

	Predicted JSY coverage	JSY coverage	Delivery in a health facility
	(1)	(2)	(3)
Urban	0.00079	0.0016	0.11***
	(0.00054)	(0.0012)	(0.0046)
Hindu	-0.00039	0.00034	0.022***
	(0.00065)	(0.0013)	(0.0045)
Scheduled caste	0.00035	-0.0012	-0.044***
	(0.00065)	(0.0011)	(0.0040)
Scheduled tribe	0.0012	0.0045**	-0.10***
	(0.0010)	(0.0020)	(0.0062)
"Other backward" ethnicity	0.000076	-0.00038	-0.031***
	(0.00073)	(0.0012)	(0.0036)
Woman's education (grades completed)	-0.000085*	-0.00027***	0.014***
	(0.000046)	(0.000090)	(0.00030)
Husband's education (grades completed)	-0.000031	0.000060	0.0052***
	(0.000036)	(0.000073)	(0.00022)
Two live births	0.00032	0.0010	-0.11***
	(0.00037)	(0.00071)	(0.0027)
Three live births	-0.00033	-0.00018	-0.16***
	(0.00047)	(0.00099)	(0.0034)
Four live births	0.00044	-0.00036	-0.19***
	(0.00057)	(0.0012)	(0.0040)
Five or more live births	0.00080	-0.00048	-0.23***
	(0.00083)	(0.0017)	(0.0045)
Mother's age at childbirth (years)	-0.000096*	-0.00011	0.0065***
	(0.000053)	(0.00011)	(0.00024)
Wealth asset score	0.00017	-0.00026	0.038***
	(0.00013)	(0.00025)	(0.00088)
F (13, 586)	1.57	2.59	1045.68
p-value	(0.0881)	(0.0017)	(<0.0001)
Number of observations	344,923	344,923	344,923
Number of districts	587	587	587
R-squared	0.853	0.598	0.199

Notes: Data are from the DLHS-2 and the DLHS-3. *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors, corrected for clustering at the district level, are reported in parentheses. Standard deviation of the dependent variable mean is in square brackets. Columns (1) and (3) includes fixed effects for district and year of birth, and interactions between year of birth and district share of the population below the poverty line, tribal population share, and mean wealth asset score. The model in column (2) is the same as column (1) except that it does not control for district demographics.

Table 8

Heterogeneity in the Effect of the JSY on Use of Maternal Health Care Services

Dependent variable: public health facility births	Education of mother		Wealth of household		Ethnicity of household		Focal districts	
	No education	Some education	Poorest	Richest	Scheduled caste or tribe	No scheduled caste or tribe	High focus	Low focus
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect of JSY at full coverage								
Baseline model	0.21***	0.16***	0.22***	0.15***	0.19***	0.15***	0.21***	0.060
	(0.021)	(0.025)	(0.023)	(0.026)	(0.023)	(0.027)	(0.023)	(0.048)
Baseline model + demographics	0.22***	0.16***	0.22***	0.15***	0.19***	0.16***	0.22***	0.068
	(0.021)	(0.025)	(0.022)	(0.026)	(0.022)	(0.026)	(0.022)	(0.047)
Mean of dependent variable at baseline	0.116	0.271	0.119	0.273	0.183	0.239	0.154	0.278
	[0.320]	[0.445]	[0.323]	[0.446]	[0.387]	[0.426]	[0.361]	[0.448]
Number of observations	163, 041	181, 879	175,704	169,263	262, 714	82, 202	234,825	110,098
Number of districts	580	587	585	587	587	577	343	244

Notes: Data are from the DLHS-2 and the DLHS-3. Estimates are from instrumental variable models in which the introduction of the JSY is used to instrument for JSY coverage. *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors, corrected for clustering at the district level, are reported in parentheses. Standard deviation of the dependent variable mean is in square brackets. Baseline model includes fixed effects for district and year of birth, and interactions between year of birth and district share of the population below the poverty line, tribal population share, and mean wealth asset score. Demographics include controls for mother's education, husband's education, mother's age at birth, wealth asset score, and dummies for categories of urban dwelling, religion, ethnicity and number of live births. The demographic variable on which the sample is divided is excluded. The unit of observation is a delivery (most recent only).

Table 9

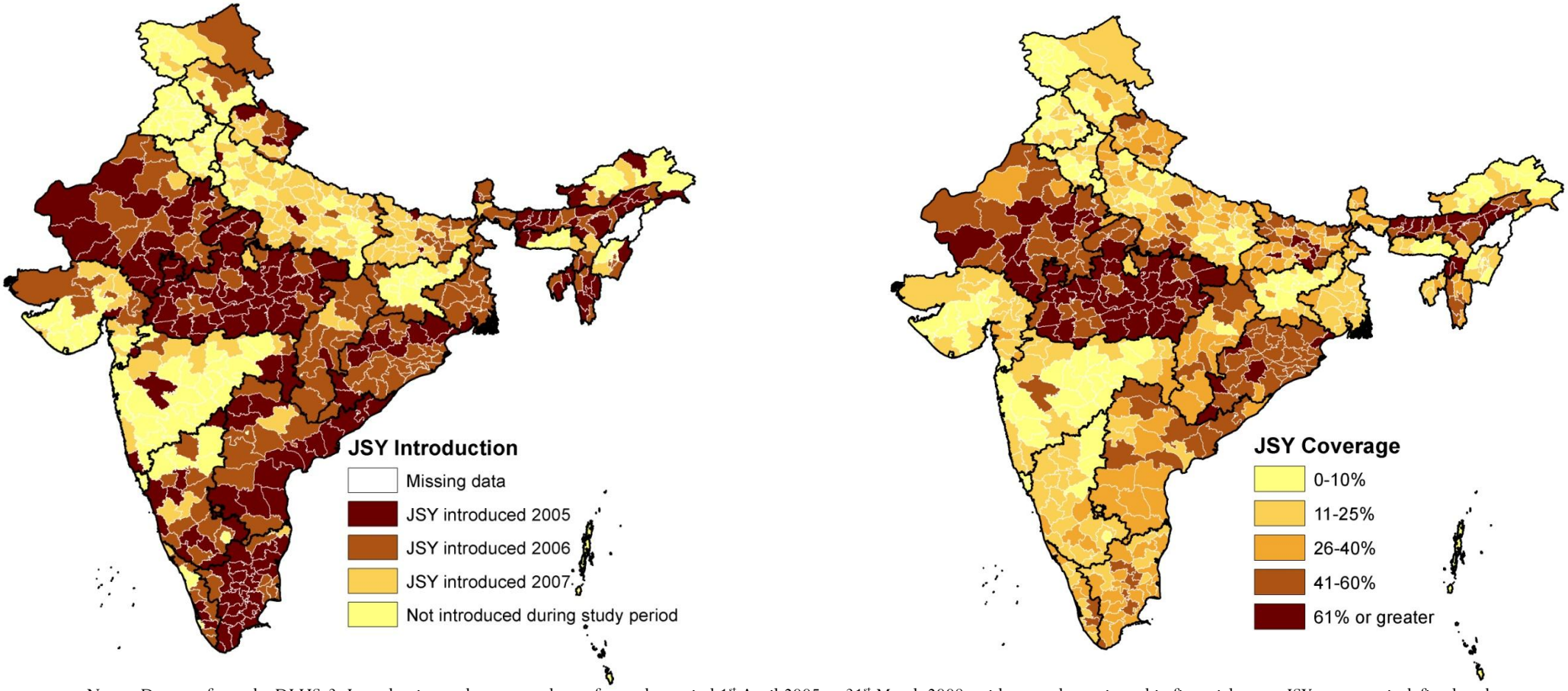
Quality of Care and the Effect of the JSY on Health Outcomes

Dependent variable: death within 24 hours	Primary health centre quality of care index	Community health centre quality of care index	District hospital quality of care index
	(1)	(2)	(3)
JSY coverage	-0.0029	-0.0048	-0.0030
	(0.0031)	(0.0032)	(0.0033)
JSY coverage × Quality of care	-0.0030	-0.0052	-0.0057
	(0.0079)	(0.011)	(0.012)
Mean of dep. variable at baseline	0.0152	0.0154	0.0152
	[0.123]	[0.123]	[0.122]
Number of observations	422,639	394,120	406, 006
Number of districts	572	544	551

Notes: Data are from the DLHS-2 and the DLHS-3. Estimates are from instrumental variable models in which interactions between district introduction of the JSY and demographic characteristics are used to instrument for the term $(\mathrm{JSY}_{dt} + \alpha(\mathrm{JSY}_{dt} \times Q_d))$ in equation (4). *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors, corrected for clustering at the district level, are reported in parentheses. Standard deviation of the dependent variable mean is in square brackets. Regression models includes fixed effects for district and year of birth, interactions between year of birth and district share of the population below the poverty line, tribal population share, and mean wealth asset score, and a set of demographics that control for mother's education, husband's education, mother's age at birth, wealth asset score, and dummies for categories of urban dwelling, religion, ethnicity, number of live births and a twin birth. The unit of observation is a live birth (based on the four-year recall birth history of a woman). Deviations in sample size are due to missing data on quality of care across different types of provider.

Figure 1

Introduction and Coverage of the JSY during the Study Period



Notes: Data are from the DLHS-3. Introduction and coverage data refer to the period 1st April 2005 to 31st March 2008, with years denominated in financial years. JSY coverage is defined as the proportion of women giving birth in a public health facility who received a financial incentive after childbirth. Black (white) lines denote state (district) administrative boundaries. Regions not shown on the map are due to missing data. The maps do not imply the expression of any opinion on the part of the authors on the legal status of any territory.

Appendix Table 1
Correlates of JSY Placement

Dependent variable: JSY introduced	Wealth	Poverty	Tribal population	Year 2005 only
	(1)	(2)	(3)	(4)
Average asset wealth score	-0.099***	-0.088***	-0.068***	-0.00061
	(0.011)	(0.011)	(0.012)	(0.018)
Poor share of population		0.21***	0.19***	0.31***
		(0.067)	(0.066)	(0.10)
Tribal share of population			0.30***	0.43***
			(0.063)	(0.098)
Number of observations	1,761	1,761	1,761	587

Notes: Data are from the DLHS-3. *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors are reported in parentheses. The unit of observation, except in column 4, is a district-year over the period 2005/06 - 2007/08. Independent variables are measured at the time of interview in 2007/08.

Appendix Table 2
Estimates of the Effect of JSY on Medical Procedures at Childbirth

Dependent variable:	Caesarean section	Assisted delivery
	(1)	(2)
Effect of JSY at full coverage		
Baseline model	-0.00079	0.014*
	(0.0086)	(0.0076)
Baseline model + demographics	0.0023	0.015*
	(0.0085)	(0.0076)
Mean of dependent variable at baseline	0.0758	0.0236
	[0.265]	[0.152]
Number of observations	344,902	344,902
Number of districts	587	587

Notes: Data are from the DLHS-2 and the DLHS-3. Estimates are from instrumental variable models in which the introduction of the JSY is used to instrument for JSY coverage. *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors, corrected for clustering at the district level, are reported in parentheses. Standard deviation of the dependent variable mean is in square brackets. Assisted delivery involves the use of forceps or a ventouse. Baseline model includes fixed effects for district and year of birth, and interactions between year of birth and district share of the population below the poverty line, tribal population share, and mean wealth asset score. Demographics include controls for mother's education, husband's education, mother's age at birth, wealth asset score, and dummies for categories of urban dwelling, religion, ethnicity and number of live births. The unit of observation is a delivery (most recent only).

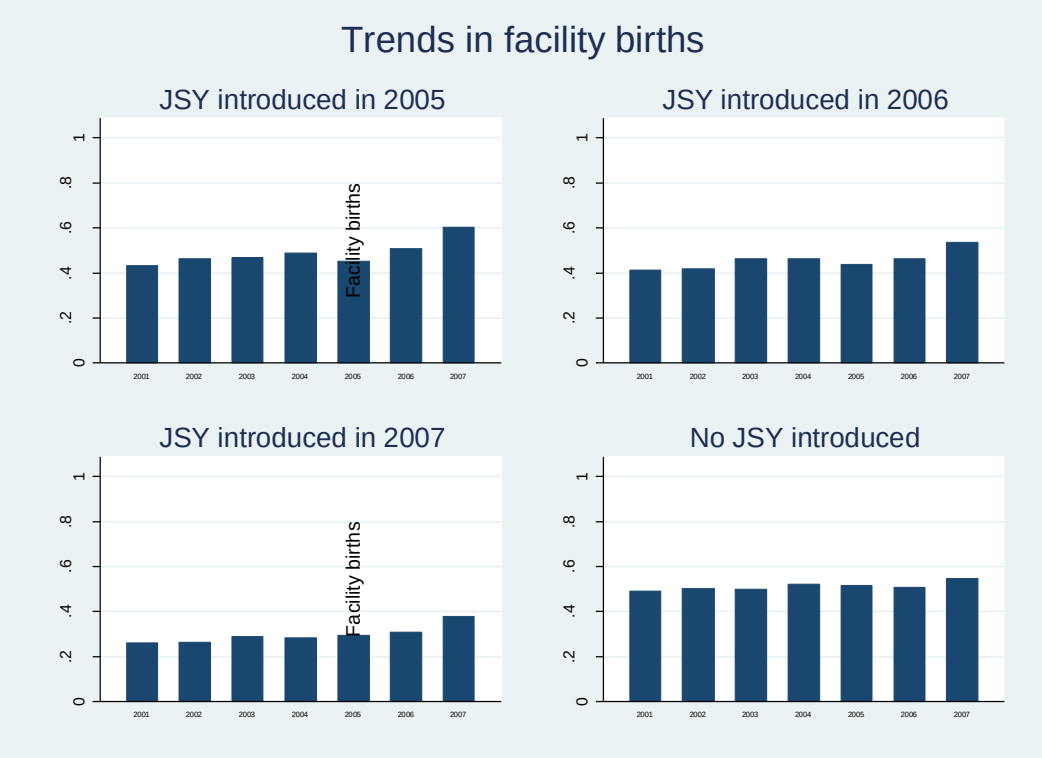
Appendix Table 3
Robustness to Definition of JSY Introduction

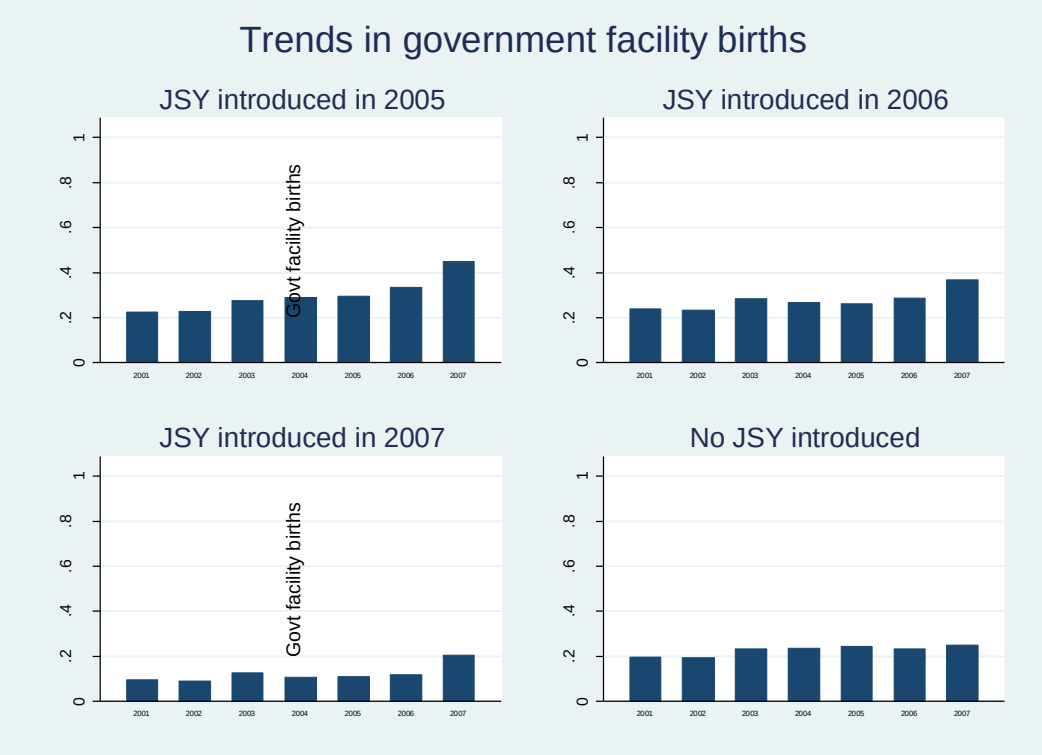
	Percentage point increase in coverage over the 2004 level used to define introduction of the JSY		
	10 %	20%	5%
	(1)	(2)	(3)
Health worker in attendance at delivery	0.081***	0.13***	0.056***
	(0.018)	(0.018)	(0.021)
Delivery in a health facility	0.12***	0.18***	0.096***
	(0.019)	(0.020)	(0.021)
Delivery in a public health facility	0.19***	0.28***	0.17***
	(0.019)	(0.021)	(0.021)
Delivery in a private health facility	-0.072***	-0.092***	-0.070***
	(0.013)	(0.013)	(0.016)
Three or more antenatal care visits	0.039*	0.032*	0.040*
	(0.020)	(0.020)	(0.023)
Neonatal mortality	-0.0015	-0.0013	-0.0056
	(0.0046)	(0.0042)	(0.0051)
Death within 24 hours	-0.0027	-0.0015	-0.0041
	(0.0031)	(0.0028)	(0.0035)
Pregnant (2004-2008)	0.017***	0015**	0.014**
	(0.0057)	(0.0060)	(0.0062)
Breastfeeding within 1 hour	0.074**	0.094***	0.065*
	(0.030)	(0.029)	(0.034)

Notes: This table presents estimates of the effect of the JSY for each of the study outcomes using three alternative measures of JSY introduction. Data are from the DLHS-2 and the DLHS-3, except for pregnancy which uses pregnancy data constructed from the birth history of women in the DLHS-3 only. Estimates are from instrumental variable models in which the district introduction of the JSY is used to instrument for JSY coverage. *** denotes significance at 1%, ** at 5%, and * at 10% level. Standard errors, corrected for clustering at the district level, are reported in parentheses. To construct our measure of pregnancy we assume that the number of months a woman has been pregnant, if pregnant at the time of interview, is as good as random (constrained to be between three and nine months). Regression models includes fixed effects for district and year of birth, interactions between year of birth and district share of the population below the poverty line, tribal population share, and mean wealth asset score, and a set of demographics that control for mother's education, husband's education, mother's age at birth, wealth asset score, and dummies for categories of urban dwelling, religion, ethnicity, and number of live births.

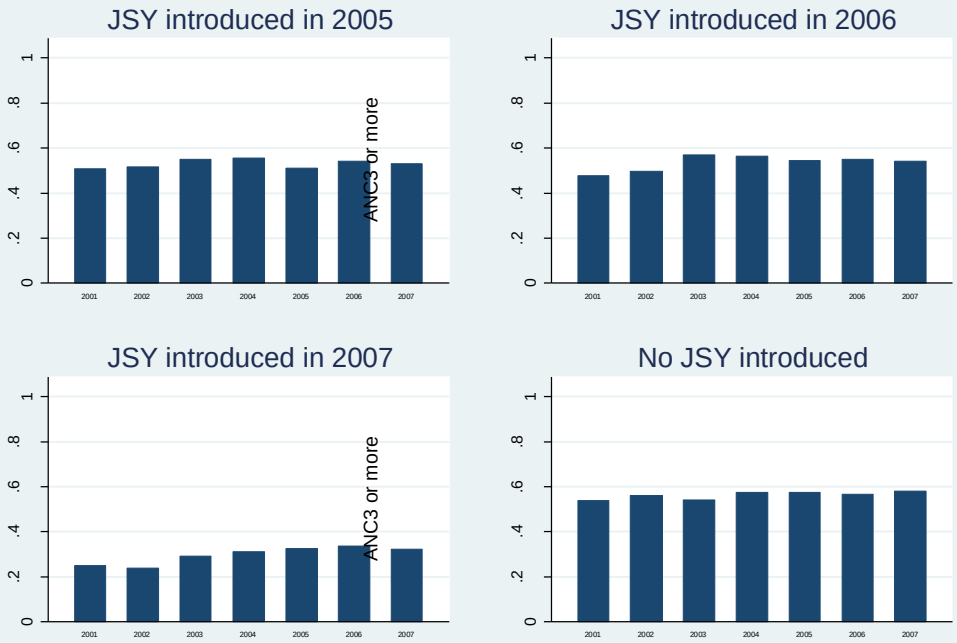
Appendix Figure 1

Evolution in Outcomes over Study Period

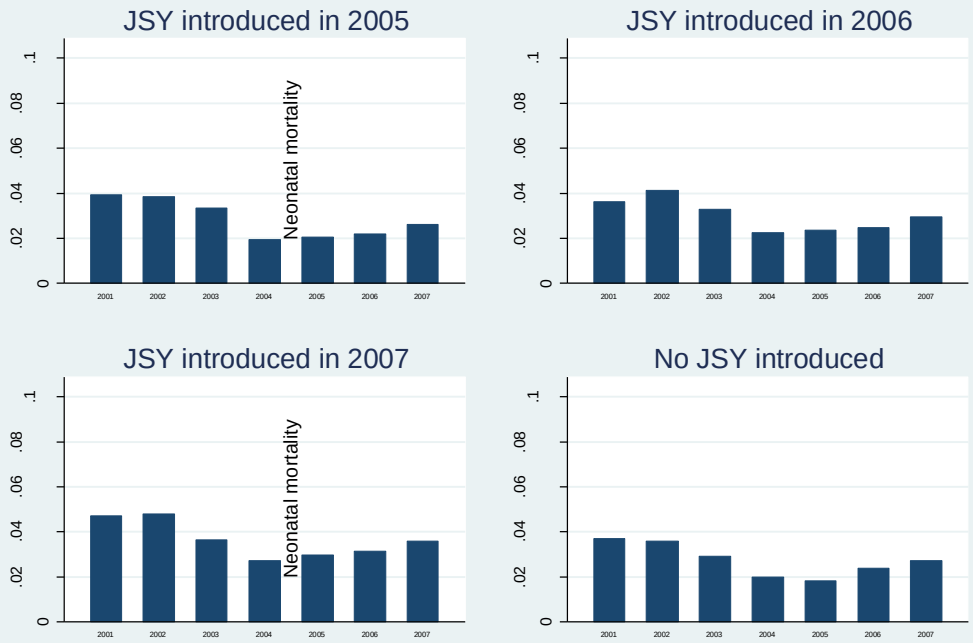


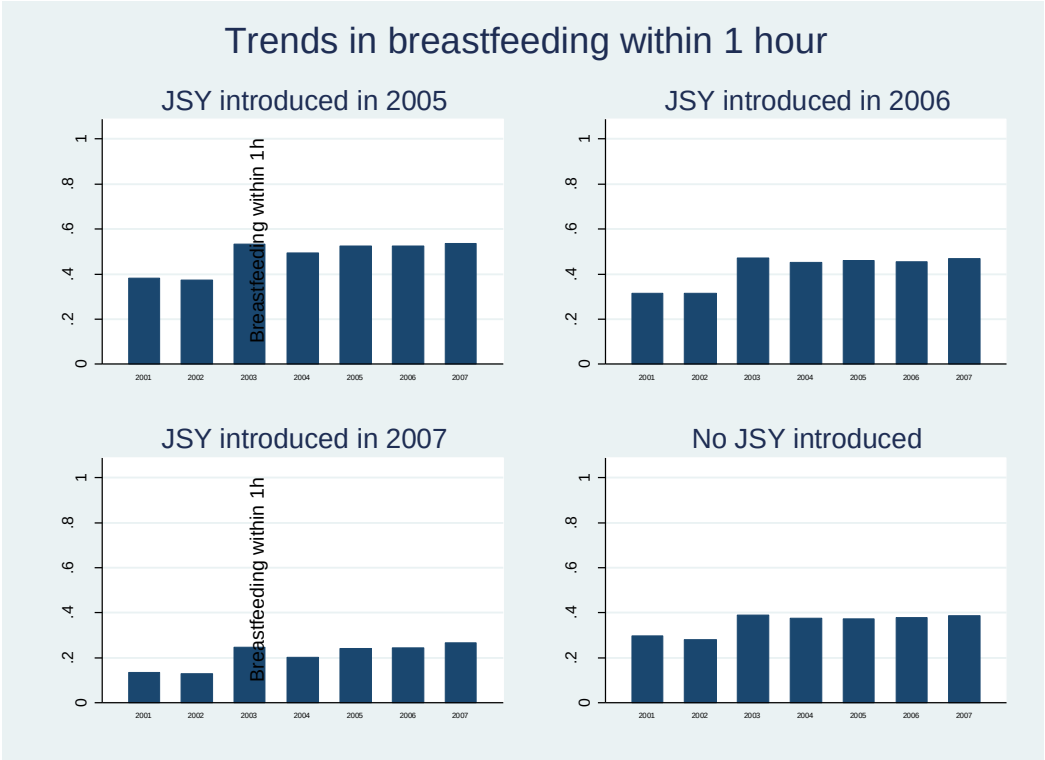


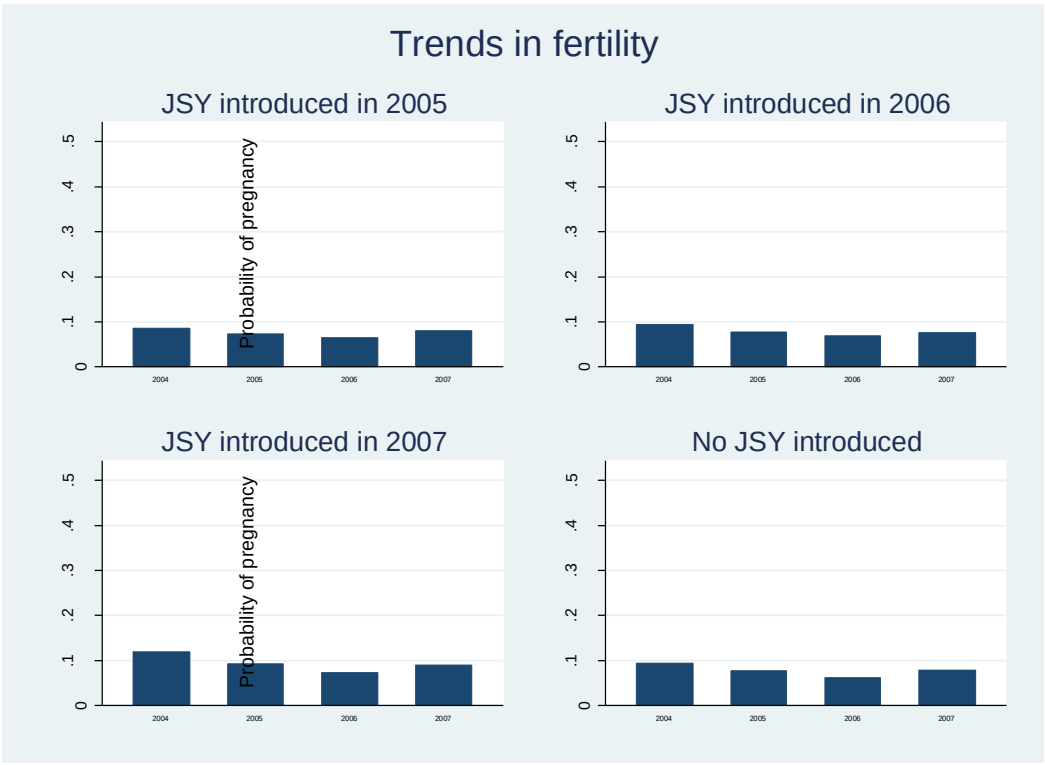
Trends in use of antenatal care



Trends in neonatal mortality




Trends in breastfeeding within 1 hour
JSY introduced in 2005
JSY introduced in 2006
JSY introduced in 2007
No JSY introduced
Breastfeeding within 1h
0
.2
.4
.6
.8
1
2001
2002
2003
2004
2005
2006
2007


Trends in fertility
JSY introduced in 2005
JSY introduced in 2006
JSY introduced in 2007
No JSY introduced
Probability of pregnancy
0
.1
.2
.3
.4
.5
2004
2005
2006
2007

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