Workfare and infant health: evidence from India's public works program

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Workfare and Infant Health: Evidence from India's Public Works Program

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Abstract

This paper examines the relationship between maternal employment and infant health in the context of a large workfare program that explicitly prioritizes female participation, India's National Rural Employment Guarantee Scheme. Using multiple estimation strategies including a triple difference strategy that exploits a discontinuity in program eligibility at age 18, we find that the program had negative impacts on newborn infant survival, increasing newborn mortality among women who were eligible to participate in the program.

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1 Introduction

The objective of improving work opportunities for women in poor countries is increasingly being folded into large-scale, publicly funded programs that directly provide employment. A new generation of social safety net programs that generate employment for participants (also known as workfare programs) is being implemented in a number of countries including Argentina, Ethiopia, India, and South Africa (Subbarao et al., 2013). Female participation is a strong emphasis in these programs, and is targeted with a combination of (relatively) high wages, work quotas and workplace rules. By providing employment opportunities for women, public works programs can reduce gender inequalities, while also improving investments in children.

An important countervailing concern in the context of such programs, and one that remains to be carefully evaluated, is that time spent by a mother working and meeting work-related demands, often means less time spent on child care. The existing evidence indicates that the early years of a child's life constitute a critical period during which maternal employment can result in worse child nutrition, health and cognitive outcomes, presumably because of the time-intensiveness of the care required and lack of high-quality substitutes for maternal time; these negative effects are only partially offset by the income effects arising from maternal employment in the later years of the child's life (see, e.g., Desai et al 1989, Blau and Grossberg 1992, Waldfogel, Han and Brooks-Gunn 2002, Ruhm 2004 in the context of the United States; Engle and Pederesen 1989, Popkin 1983, Glick and Sahn 1998, in the context of developing countries). These findings are complemented by recent studies that examine the effect of macroeconomic shocks on infant health to argue that an increase in maternal labor supply results in worse newborn survival rates (Miller and Urdinola 2008, Bhalotra 2010). An additional consideration suggested by research from the fields of medicine and epidemiology is that exertion and stress associated with work can negatively impact fetal

health leading to worse newborn outcomes (e.g. Rao et al 2003, Dwarkanath et al 2007; see Takito 2009 for a survey of the evidence).¹

In theory, the effects of maternal employment on child health in the workfare context may be quite distinct from those identified in the previous literature. In poor countries, increases in female labor supply can often represent distress responses to economic downturns (as in Bhalotra 2010), whereas a workfare program increases labor supply by boosting the earning power of women. In addition, workfare programs combine high wages with rationing (by means of a cap on the total number of work days for a given household (or individual) in a given period), which may tilt the relative balance of income and substitution effects from maternal employment. Differently from the labor market shocks commonly exploited in the literature, a workfare program also represents a permanent increase in the opportunity cost of time, a distinction that is likely to become more important in the medium to longer-term as households adjust their desired fertility to trade-off child quality and quantity (Becker 1965). Last, but not least, the work provided under such programs (typically in local infrastructure-related projects) may be very different from the kinds of work activities that women would otherwise engage in, with resulting implications for fetal and newborn health.

This paper presents the first rigorous examination of the effect of maternal work on infant health in the context of a public works program, India's Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS), which is the largest direct employment program in the world.² NREGS explicitly prioritizes female labor force participation, and nearly 50% of the allocated work-days have gone to women (Dasgupta and Sudarshan 2011). We identify program effects on infant health by taking

¹Rao et al (2003) in a study of women in rural areas find that even daily household activities such as drawing water from a well increase the risk of low birth-weight and premature birth. Dwarkanath et al (2007) find analogous results in a study of women in urban India, and show that even physical activity in the first trimester of pregnancy has adverse effects on birth-weight and gestation.

²To date NREGS has provided employment to nearly 50 million households in India (about 36 percent of the rural labor force).

advantage of the staggered nature of program rollout across districts. To account for the potentially non-random nature of program rollout, we employ a set of progressively restrictive empirical strategies: (i) a difference-in-differences approach, which relies on the assumption of similar trends in treated and not-yet treated districts, (ii) a difference-in-difference-in difference (or triple difference) design, which relaxes this assumption, and (iii) a triple difference design combined with a parametric regression discontinuity approach that exploits a sharp discontinuity in program participation at age 18 (only individuals 18 or older were eligible to participate).

Applying these techniques to an all-India dataset that includes data on more than 200,000 live births over a five-year period covering program rollout, we find that NREGS led to a significant increase in newborn deaths. The different estimators, which rely on different identification assumptions, generate the same finding. The result is remarkably robust, and survives a range of robustness checks and placebo tests. To shed light on the causal mechanism, we examine two key pathways suggested by the literature: Crowd-out of child health inputs, and stress-related effects of work. Interestingly, while we do find evidence of crowding-out of health inputs during pregnancy, there is also significant evidence of the negative effects of work on fetal (and maternal) health, as seen in an increase in the probability of premature births and stillbirths; in addition, newborn deaths are concentrated within the first 24 hours after birth, suggesting the influence of pre-natal factors (Lawn et al. 2009).

These results have immediate policy relevance, not only for NREGS but potentially for other public works programs as well. They underscore not only the importance of enforcing existing workplace regulations – as a case in point, although NREGS work-sites were supposed to be equipped with creches to accommodate women with children, this was often not the case in practice (Nair et al. 2014) – they also argue for implementing additional strategies to help minimize adverse effects on young infants by providing better support to pregnant women and women with young children par-

ticipating in the program. We take up the policy implications in greater detail in the concluding section of the paper.

The paper proceeds as follows. Section 2 provides a description of the National Rural Employment Guarantee Scheme; Section 3 describes the data; Section 4 explains the empirical strategy, and Section 5 presents the results. We conclude with a discussion of policy implications in Section 6.

2 The National Rural Employment Guarantee Scheme

2.1 Program description

NREGS is the largest direct employment program in the world, providing employment to nearly 50 million rural households in India. During the period under study (2004-2008), each enrolled household was guaranteed a maximum of 100 days of paid labor per year, to be allocated across the adult household members in any fashion throughout the year, with any rural household being eligible to participate. Only individuals 18 and older were eligible to participate, a rule we exploit later. The scale of the program is impressive: in 2007/2008, NREGS provided employment to nearly four million people each day (Dreze and Oldiges 2009). Employed individuals work on public works projects including land development, rural connectivity, and irrigation. NREGS emphasizes manual work, both by restricting the use of machinery on worksites and by limiting the ratio of material to labor cost to be no greater than 2/5. Program participants describe the work as “moderately difficult” or “very difficult” (Reddy et al 2010).

NREGS prioritizes female employment: An explicit requirement is that at least one-third of NREGS jobs should go to women. A second requirement is that women should be paid the same wage as men. In addition, the NREGS Act mandates a number of re-

quirements that are designed to accommodate the needs of working women. These include provision of mobile creches at worksites (if there are at least six women with children under the age of 5), female representation on local councils (which are responsible for deciding on and planning the public works projects), and a requirement that program work should be within a reasonably small radius (5 km) of each village.³

2.2 Rollout

The program was introduced in 2006, and rolled out in three phases. Phase 1 was implemented in 2006 in 200 districts; Phase 2 was implemented in 2007 extending the program to an additional 130 districts; and Phase 3 was implemented in the remaining 285 districts in 2008. Figure 1 presents a color-coded map that shows the assignment of districts to the three phases of implementation. Although the exact allocation rule has not been made public, it is known that the assignment of districts into phases was purposeful, and was intended to prioritize the most backward districts.

2.3 Effect on female labor supply

The program appears to have exceeded its goals in terms of female participation. In 2011, the average share of NREGS workdays actually allocated to women was 49.5 percent (Dasgupta and Sudarshan 2011). Because the NREGS minimum wage is quite high compared to the average female wage prior to the program, the program has substantially boosted the earning power of women. Not surprisingly, Narayanan and Das (2014) find that in a number of states, it is common for households to only send women to work in the program. While accurate estimates of participation in NREGS are hard

³Despite coming under criticism on a number of grounds (including mounting wage arrears, and significant leakages of program funds due to corruption), NREGS has retained its core features. Consecutive droughts in recent years have increased its importance in terms of providing economic security, and the program continues to receive prominent budgetary outlays (*Economic Times*, Feb 2017).

to come by for the period under study (2004-2008),⁴ data from the 2009-10 round of the India National Sample Survey data indicate an all-India average household participation rate of 24% in the program (Dutta et al 2012; Liu and Barrett 2012). The India Human Development Survey (IHDS) in 2011-12 elicited information on participation at the individual level, and indicates an all-India average rural female program participation rate of approximately 8.8% (Joshi, Desai and Tiwari 2015).

From the perspective of our study, interest centers on what kinds of work-related activities the program displaced, since the effect of NREGS on infant health is likely to depend on the level of exertion as well as the time-intensiveness of program work relative to the activities that were displaced. There is both anecdotal as well as formal evidence that a significant proportion of female NREGS participants were either new to the labor force, or at least engaging in paid activity for the first time. Khera and Nayak (2009) find, based on interviews with women in six states, that fewer than 30 percent of women working in NREGS reported being engaged in, or looking for, wage work prior to the program. The local availability of the work, its regular working hours, non-exploitative nature, and social acceptability were cited as important factors that induced participation among women who had not considered working outside the household before (see also Pankaj and Tankha 2010). Estimates from other studies suggest an important, albeit smaller, effect on female labor force participation as a whole: Azam (2012) estimates that over the period 2004-08, NREGS increased the female labor force participation rate by 2.3 percentage points, while Afridi et al (2012) report a 6.5 percentage point increase in female casual labor participation in the state of Andhra Pradesh over the period 2004-09.

Even among women already in the labor force, some may have undertaken NREGS work in addition to their existing work, thus increasing their total labor supply, while

⁴Administrative data on program take-up covering this period are of doubtful quality (Dutta et al 2012), and a lack of sufficiently specific and high-frequency labor force data has made it difficult to obtain reliable independent estimates.

for other women NREGS work was substituted for non-program work. *A priori*, one would not expect NREGS to have significantly crowded out other kinds of paid or unpaid work activities, given that the program was (in principle) intended to provide employment during the lean agricultural season when other employment opportunities tend to be quite limited. Existing studies paint a mixed picture: Imbert and Papp (2015) find evidence of significant crowding-out, whereas Afridi et al (2012) observe a sharp increase in public works participation that is not matched by any corresponding decline in private sector work. Overall, our weak conclusion is that NREGS is likely to have partially, but not completely, displaced private sector work, at least as evidenced by the consistent finding across studies that rural wages appear to have increased following the introduction of NREGS (Azam 2012, Berg et al 2012, Imbert and Papp 2015, Zimmerman 2012). In turn, the program's general equilibrium effect on wages also implies that some women may have supplied more labor as a result of program introduction, even if they did not participate in the program - this is another distinct group of women whose pregnancy outcomes may have been affected.

To summarize the discussion above, the literature suggests that there may have been a number of different margins of labor supply response to the program. Each of these margins may in turn be associated with a distinct effect on child health outcomes: In our data, although we are able to test (in a limited way, as we explain later in Section 5.2) whether health effects are restricted to labor force participants, the data do not however allow any further disaggregation to answer the question of how program effects differ across the various sub-groups of labor market participants. This is an important caveat to bear in mind when interpreting the findings of our study, and in formulating specific policy prescriptions.

3 Data

3.1 Survey description and timeline

Our analysis uses data from the 2007-2008 round of India's District Level Household and Facility Survey (DLHS). The DLHS is a nationally representative survey of urban and rural households spread across 601 districts and 30,028 villages and urban wards. The survey has national coverage, and is designed to produce representative estimates at district level. All districts were surveyed, and within each district 50 Primary Sampling Units (PSUs) were chosen for survey, based on a stratified sampling design. PSUs are either villages or urban wards, and the allocation between them is based on 2001 Census population figures. Within each PSU, the number of households surveyed ranges from 22 to 33. At district level, 1000-1500 households are surveyed, again based on a stratified sampling approach.⁵

All ever-married women aged 15-49 in sampled households were interviewed, and information on all pregnancies since January 1, 2004 was collected, as well as a limited amount of socio-economic information pertaining to the mother and her household. Data collection started in November 2007 and continued through January 2009, giving us retrospective birth data covering the pre-rollout period (prior to February 2006) and extending through Phase 3 (the final phase of rollout) in 2008, although nearly 99% of the births in the sample occurred prior to April 2008. Figure 2 presents the timeline of the data collection and the retrospective birth period vis-à-vis program rollout.

3.2 Sample

The unit of observation in the main analysis is a live birth. Information on the district, month and year of birth is used to determine whether a birth was exposed

⁵Further details of the sample design can be found in the DLHS-3 manual (http://rchiips.org/pdf/rch3/Manual/1_House_Listing_Manual_DLHS3.pdf).

to the program. The mother's age at pregnancy is imputed from month and year of birth assuming an average nine-month pregnancy duration. Births are assigned to the mother's district of residence at the time of the survey. While in theory, this might be different from the district in which a particular birth occurred, in practice, because rates of internal migration in India are among the lowest in the world (Bell et al 2015) and migration tends to be predominantly within-district (Kone et al 2016; see also Topalova 2005 and Atkin 2016), this is unlikely to be an important distinction. The short time-span of the retrospective period (less than 5 years) also limits the role of internal migration.⁶

Ideally, given the potential importance of pre-natal factors, one might want to restrict the sample to exclude births that were partially exposed to the program (in the sense that the program was introduced while the mother was pregnant). This would imply dropping all births that occurred within nine months of program implementation in a district (assuming an average nine month pregnancy), but as this results in a large loss of observations, we compromise by excluding only births that occur within the first 6 months of program introduction (i.e. pregnancies that were exposed for less than two trimesters). Later, we investigate the sensitivity of the estimates to varying this restriction.

3.3 Key variables

The primary outcome of interest in this study is neonatal mortality. Although neonatal mortality technically denotes death within the first 28 days after birth, subjective reports of mortality tend to exhibit age-heaping. Accordingly, we define neonatal mortality for our study as death within the first month of birth. To ensure that all

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⁶Kone et al's (2016) estimates imply that over the five year period preceding the year 1999, only 1% of women migrated out of their district for reasons other than to take up residence with their husband following marriage

infants in the sample are exposed to the full neonatal period, we exclude births occurring less than one month prior to the interview. Supplementary outcomes include 1-day mortality, premature delivery, and a stillbirth. For the most recent live/stillbirth, women were also asked whether they experienced any of a set of specified health problems during pregnancy – because most of the problems are arguably general symptoms of pregnancy, such as swelling, fatigue, nausea and vomiting, we focus on whether the mother reported experiencing little or no movement of the fetus, as this may be indicative of fetal health.

To examine causal pathways, we study program effects on various health inputs including utilization of prenatal care, receipt of tetanus toxoid (TT) vaccine during pregnancy, and place of delivery (these are all elicited for the most recent live/stillbirth); and child immunization, which is recorded for the two most recent surviving births. We examine receipt of the following vaccines: BCG (for tuberculosis), DPT (for diphtheria, pertussis and tetanus), polio and measles.⁷ We also examine the duration of exclusive breastfeeding, which is elicited for the most recent surviving child. To minimize censoring while retaining a sufficient sample size for analysis, the analysis sample for the breastfeeding and immunization variables is restricted to births that occurred at least six months prior to interview.

We also make use of information on socio-economic characteristics pertaining to time of survey, including data on household religion, caste, and wealth (constructed as an asset index); literacy and education levels of the woman and her spouse; and the woman's work-force participation (including paid and unpaid work) in the seven days preceding the interview.

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⁷The DPT and polio immunization courses are normally completed within the first 14 weeks after birth, while the BCG vaccine is normally given at birth but may also be given at any point in the first 12 months.

3.4 DLHS-2 data

We also use data from the previous survey round, DLHS-2, which occurred over the period 2002-04. While there are some important differences in sample and survey design between DLHS-2 and later survey rounds, we use the DLHS-2 primarily to conduct an auxiliary test. Differently from the DLHS-3, the DLHS-2 elicited information on each mother's entire birth history. For our analysis, we restrict the sample to births that occurred since 1998 - this results in a sample with a comparable number of births to that of the DLHS-2. The main outcome variable of interest in these data is neonatal mortality, defined as before.

3.5 Summary statistics

Table 1 compares the districts in the three NREGS phases of rollout, in terms of average characteristics and health outcomes (the sample is restricted to rural births and households). Reflective of the way in which the program was rolled out, there is a clear gradient in socio-economic indicators across the three program phases. Compared to mothers in Phases 2 and 3, mothers residing in Phase 1 districts are in poorer households, are more likely to belong to the backward castes, and are less likely to be literate. A similar conclusion emerges in the comparison of Phase 2 and Phase 3. The gradient in health outcomes is less evident: While Phase 1 districts have higher rates of newborn mortality than Phase 3 districts, the rates of newborn mortality are in fact highest in Phase 2 districts. This weak correlation between aggregate socio-economic indicators and health outcomes has been noted in a number of studies (see for example, Singh et al 2011), and underlines the complexity of the relationship between these variables.

Because of its relevance to our study, Table 1 also summarizes rates of female labor force participation, separately for women who are (currently) pregnant (approximately

12% of the ever-married women), and those who are not.⁸ Female labor force participation is negatively correlated with socio-economic status,⁹ reflective of the fact that female labor is generally an economic necessity in rural areas.¹⁰ It is also particularly noteworthy that pregnant women are no less likely to be participating in the labor force than women who are not pregnant.

4 Empirical strategy

NREGS was targeted toward adults residing in rural areas. Accordingly, we estimate the Intent-To-Treat (ITT) effect of the program on rural women who were at least 18 years old at the start of pregnancy. The non-random assignment of districts to the phases of program implementation implies that simple comparisons between treated and not-yet-treated districts may not capture the causal effect of the program. We address this issue by implementing a series of estimators that rely on progressively weaker identifying assumptions. First, we use the variation in the timing of program rollout across districts to estimate a difference-in-differences model; next, we incorporate a comparison group of rural women in a triple difference estimation strategy; in an additional set of triple difference specifications, we incorporate a comparison group of births to women who were not yet 18 at the start of pregnancy. These strategies are described in greater detail below.

⁸These categories are admittedly based on self-reports. Women who are aware of being pregnant (and are willing to reveal this information) may be systematically different from the population of pregnant women.

⁹Not surprisingly, the opposite correlation is observed in the urban sample, i.e. labor force participation is greater in richer districts (results available on request).

¹⁰See Bhalotra (2010) for evidence that female labor force participation increases when the household faces a negative income shock.

4.1 Differences-in-differences

We start by estimating the following difference-in-differences (DID) regression specification:

$$y_{jdt} = \alpha + \beta Treat_{dt} + \gamma \mathbf{X}_{jdt} + \eta_d + \eta_t + \varepsilon_{jdt} \quad (1)$$

where y_{jdt} denotes an indicator for neonatal mortality for an infant born at time t , to woman j residing in district d ; $Treat_{dt}$ is an indicator for whether the birth occurred after district d was enrolled in the program; η_d denotes district fixed effects (included to control for unobservable time-invariant differences between districts that may be correlated with program receipt), η_t denotes birth year x birth month fixed effects (i.e. a dummy for each of the 60 months in the retrospective period), which control for time-varying unobservables that are common to treated and untreated groups; and ε_{jdt} denotes an unobserved error term. $\mathbf{X}_{jdt}$ denotes a vector of controls that are plausibly exogenous to the program: A child gender indicator, an indicator for multiple birth (e.g. twins), an indicator for maternal literacy, and a dummy for Hindu households. We are careful not to control for any variables that may be affected by the program, such as birth order, maternal education or household wealth. The standard errors are clustered at the district level to adjust for serial correlation in district outcomes over time.

The analysis sample includes only births to rural women who were at least 18 years old at the start of pregnancy. The coefficient β in the specification above then represents the DID estimate of the Intent-To-Treat (ITT) effect of the program.

4.2 Difference-in-difference-in-difference (DDD)

The principal threat to identification in the difference-in-differences estimation arises from any time-varying unobservables that are trending differently in treated relative

to control districts - for instance, districts that were experiencing large increases in mortality could have been the first to receive the program; or, the timing of program implementation may have co-incided with the introduction of other policy changes at the district-level. To account for such confounders, we turn to triple difference specifications that take advantage of the fact that not all individuals within a district are eligible to participate in NREGS. This approach can control for time-varying differences between treated and not-yet-treated districts. The targeting of NREGS implies two potential control groups:

(a) **Urban births:** NREGS is explicitly a rural employment scheme and women residing in urban areas were not eligible to participate in the program. Data from the India Human Development Survey of 2011-12 confirm that program participation is limited to rural areas, where the average female participation rate is 10.3%, compared to a rate of 0.5% in urban areas.

Since there are rural and urban areas within a district, we can use urban women as a third comparison (in addition to the comparison across districts and time) to sweep out within-district changes that may confound our estimates of the program. To do so, we expand the sample to include births in urban areas, and estimate the following triple difference regression specification:

$$y_{jldt} = \alpha + \beta Treat_{dt} \times Rural_{ldt} + \gamma X_{jdt} + \eta_{ld} + \eta_{lt} + \eta_{dt} + \varepsilon_{jdt} \quad (2)$$

where l indexes location of residence (i.e. rural or urban), and $Rural_{ldt}$ is an indicator that takes the value 1 if the mother resides in a rural area in the district, and zero otherwise. As before, $Treat_{dt}$ denotes the district-level program exposure variable. In this specification, the district and time fixed effects are allowed to vary by location of residence (these are now denoted by η_{ld} and η_{lt} respectively); because districts contain rural as well as urban areas, we can also control for unobservable time-varying fac-

tors at the district level by means of the district-time fixed effects η_{dt} (these absorb the level (uninteracted) effect of $Treat_{dt}$). The coefficient β represents the triple differences estimate of the ITT effect of the program.

(b) **Births to younger women:** At the individual level, eligibility for work in NREGS is based on age: Only adults 18 and older can obtain employment through NREGS. Figure 3 (upper panel) graphs the rate of NREGS participation among rural ever-married women (the sample is chosen to match the DLHS sample), using program participation data from the India Human Development Survey and confirms that participation is indeed virtually zero until the age of 18, at which point participation rates increase sharply. Marriage and child-bearing at early ages are not uncommon in India (Raj et al. 2009) and our sample includes approximately 26,000 births to women younger than 18 at time of pregnancy, which allows us to examine how program eligibility is related to newborn mortality. The lower panel of Figure 3 graphs the rate of neonatal mortality against maternal age, separately for the sample of treated births (i.e. births that occurred after NREGS was introduced in a district) and the sample of untreated births (for better comparability, the figure excludes births in Phase 3 districts since these were never treated during the sample period). We observe that mortality rates for treated and untreated births are not systematically different below the eligibility threshold, but deviate thereafter, as a result of an increase in the rate of mortality for treated births at the eligibility threshold. These figures provide descriptive evidence linking program eligibility to increased mortality.

To formally incorporate this age-based variation in program eligibility into the estimation, we estimate the following triple difference specification:

$$y_{jdt} = \alpha + \beta Treat_{dt} \times Adult_{jdt} + \gamma \mathbf{X}_{jdt} + \eta_{ad} + \eta_{at} + \eta_{dt} + \varepsilon_{jdt} \quad (3)$$

where $Adult_{jdt}$ is an indicator for whether the mother was at least 18 years old at the

time of pregnancy. In this specification, the district and time fixed effects are allowed to vary with adult status (these are now denoted by η_{ad} and η_{at} respectively); we can also control for unobservable time-varying factors at the district level by means of the district-time fixed effects η_{dt} (these absorb the level (uninteracted) effect of $Treat_{dt}$). The coefficient β represents the triple differences estimate of the ITT effect of the program.

Equation (3) represents our preferred specification. A potential concern in the context of this identification strategy is that the program may have had spillover effects onto non-adult women, via, for instance, a general equilibrium increase in female wages, which would then have increased the labor supply of all women. Ineligible women may also have been directly affected if the age criterion was insufficiently enforced, so that some ineligible women were able to avail of program work. In both cases, the triple difference strategy outlined above would tend to understate the effect of NREGS. A related conjecture is that there may have been intra-household substitution of domestic work duties between ineligible and eligible women. We assess this conjecture in Section 5.2.

The remaining threat to identification arises from unobservable confounders that vary across time, space and within-district eligibility. In Appendix A, we outline two further refinements of the triple difference specifications that can potentially address this issue, including a quadruple difference specification and a regression discontinuity specification that exploits the fact that program eligibility changes discontinuously at age 18.

5 Results

5.1 Main results

Table 2 reports the results from estimating the difference-in-differences and triple differences regressions. Appendix Table 1 presents the results for the quadruple-difference specification and the triple difference-RD specification. Appendix Table 2 presents the regression results without the inclusion of any controls. The dependent variable in all regressions is neonatal mortality.

The results indicate that NREGS increased neonatal mortality, a finding that is robust across the range of identifying strategies (the null hypothesis is rejected at least the 10% level of significance in all the specifications, except in the case of the quadruple-difference regression). The DID point estimates are evidently smaller than those in the remaining specifications, but it is not clear whether this reflects a bias in the DD specification, because the standard errors are also large enough that we cannot reject the hypothesis that the effect sizes are the same in the two sets of specifications.¹¹ It is also correspondingly difficult to make a precise statement about effect sizes, given the width of the confidence intervals. A lower bound on the Treatment-on-Treated (ToT) effect can be calculated as follows: Using the estimate from our preferred specification (Column 3), and assuming an 8.8% rate of program participation (as reported in Joshi, Desai and Tiwari 2015), the smallest ToT effect that fails to be rejected at the 5% level of significance is an additional 2 deaths per 1000 live births. This represents an approximately 7% increase over the baseline rate of 27 deaths per 1000 live births. It should be emphasized that calculating the ToT effect with respect to program participants is convenient, but may not accurately capture the ToT effect: As the discussion in Sec-

¹¹Essentially, we appear to be trading off bias versus variance in the DD-DDD comparison: The DDD specifications control for many more confounding factors, but the effects are not as well-measured because the comparison groups (urban and ineligible women) are small.

tion 2.3 suggested, the group of program participants may represent either an over- or under-estimate of the proportion of women whose pregnancy outcomes were affected as a result of program exposure.

As an additional guide to interpreting the effect sizes, we benchmark against estimates in the literature. A close point of comparison to our paper is Miller and Urdinola's (2010) study of the effect of coffee price shocks in Colombia on female labor supply and hence cohort size (and child mortality). The underlying "experiment" is similar to the one represented by NREGS, namely a large change in the opportunity cost of time that affects all adults but has a particularly strong effect on women's labor supply. Under reasonable assumptions, their point estimates imply that a 1 percent increase in the wage rate would result in an ITT effect of an additional 0.41 neonatal deaths per 1000 live births.¹² Imbert and Papp (2015) find that NREGS has increased rural wages by 5.5%, which would then imply an ITT effect of an additional 2.25 deaths per 1000 live births, which is similar to our point estimate using the difference-in-differences estimator (Column 1 of Table 2).

5.2 Validity and robustness checks

We now undertake a range of tests to back up the credibility of the research design, as described below:

Heterogeneous effects by workforce participation: A critical feature that can help distinguish the impact of NREGS from the impact of other programs or policies is that the former is expected to be limited to working mothers. As the first of our valid-

¹²They estimate that a 500 peso increase in the price of coffee (an approximately 25% price increase) would reduce cohort size by 2.16% for children under 2 (Table 2 of their paper), amounting to an additional 21.6 deaths per 1000 births. Assuming that half of these deaths occur within the neonatal period, this corresponds to an additional 10.6 neonatal deaths per 1000 births. Assuming additionally that the entire price increase passes through to wages, the implied effect of a 1% increase in wages is an additional 0.41 deaths per 1000 births.

ity checks, therefore, we examine program effects separately for working and non-working women (Appendix Table 3). We acknowledge that the test is not definitive, because work participation is measured at time of survey, rather than at birth.¹³ With that caveat, it is striking that the increase in mortality is only observed among working women, with smaller and statistically insignificant effects in the non-working sample. This finding is robust to the choice of estimation strategy, and lends support to the hypothesis that we are indeed capturing the effects of NREGS.

Testing the parallel trends assumptions: The DID analysis rests on the assumption of parallel trends between the three program phase groups. Figure 4 graphs the rate of neonatal mortality for each of the years in the sample period, separately for the three program phase groups. Mortality trends are indeed parallel in the years leading up to 2006 (the first year of implementation), after which the Phase 1 districts diverge from Phases 2 and 3, with Phase 3 districts experiencing an increase in mortality starting in 2007 (the second year of implementation). In Figure 5, we show mortality trends for births to program-ineligible women (i.e. urban and non-adult women), with the caveat that these figures are somewhat noisier due to the sample sizes being significantly smaller.

We now formally test the parallel trends assumption underlying each of the identification strategies by estimating leads and lags of program effects for each specification:

$$y_{jdt} = \sum_{c=-4, c \neq -1}^{c=2} \beta_{T+c} \text{Treatment}(T+c)_{jdt} + \Gamma_{dt} + \varepsilon_{jdt} \quad (4)$$

where $\text{Treatment}(T+c)_{jdt}$ is a treatment indicator: In the case of the DID specification, Treatment takes the value 1 if the birth occurred c years after the program was

¹³A related limitation is that we cannot disaggregate program effects according to the margins of labor supply response identified in Section 3.3, i.e. we cannot test, for instance, whether health effects are different for women who switch from not working to working in NREGS, as compared to women who switch from non-NREGS work to NREGS work.

implemented in the district, while in the case of the triple difference specifications, *Treatment* takes the value 1 if the birth occurred to an eligible woman k years after the program was implemented in her district.¹⁴ Γ_{dt} denotes a vector of fixed effects that varies according to the specification being tested (e.g. in the DID specification, Γ_{dt} consists of district fixed effects and time fixed effects, and so on). The coefficients β_{T+c} represent differential effects relative to the omitted lag of $T - 1$, and can be directly examined to test for any significant changes in outcomes that anticipate the implementation of the program.

The results of these regressions are reported in Appendix Table 4. Across all specifications, the coefficients β_{T-4} , β_{T-3} and β_{T-2} are generally small and statistically insignificant, confirming that there are no significant anticipatory effects or differential pre-program trends that would undermine identification.

Placebo program implementation: As a second test of the validity of the parallel trend assumptions, we turn to data from the previous survey round (DLHS-2), and consider a placebo program implementation that started in Feb 2000 (instead of Feb 2006). We maintain the original assignment of districts to program phase groups and the spacing between the implementation phases (i.e. the second placebo phase is implemented in April 2001 and the third phase in April 2002). We then re-estimate each of the specifications on this sample. Appendix Table 5 reports the results. The estimated placebo effects are small and statistically insignificant across all four specifications, as one would expect if there were no confounding differential trends in the pre-program period.

Testing for compositional changes: Next, we test whether the estimated effects may be

¹⁴In this specification, analysis years are defined to be fiscal years (from April to March the following year), as these coincide with the timing of program implementation, with a small adjustment for the fact that the first round of implementation actually started in February 2006 (instead of April 2006). Thus the analysis years are defined as Jan-March 2004, April 2004-March 2005, April 2005-Jan 2006, Feb 2006-March 2007, and April 2007-March 2008.

confounded by changes in the composition of births. Changes in migration triggered by the introduction of NREGS are an obvious source of compositional change in this context.¹⁵ Two additional sources of compositional bias that are relevant in the context of newborn mortality are (i) sample attrition due to maternal mortality, and (ii) fetal loss (Harris 1982), both of which can change the risk composition of the sample of live births. To test for such changes, we implement each of the estimating regressions using each of a set of observable birth, mother and household characteristics as dependent variables.

Appendix Table 6 reports the results. We test a wide range of observable characteristics, including some that may be endogenous to the program, such as birth order and household wealth. For each characteristic, the table displays the estimated treatment effect from each of the four regression specifications that were reported in Table 2. We do not observe any systematic changes in composition for any of the characteristics.

Fertility and marriage market responses are two additional sources of change in the composition of births that may induce bias in our estimates. We investigate fertility responses directly in Section 5.4, but note for now that the compositional tests in Appendix Table 6 do not indicate any significant changes in parity or maternal age at birth, which would normally be indicative of fertility responses. As an additional test of the fertility and marriage market hypotheses, we first estimate the difference-in-differences specification while restricting the sample to mothers who were married prior to 2004 - this sample restriction directly rules out compositional changes due to marriage market responses. Next we estimate the regression on the same sample, but this time including maternal fixed effects, in order to further test whether sample selection due to fertility responses could be affecting the results. The results are reported in Appendix Table 7. The program effect is robust to both these specifications (the points

¹⁵See Imbert and Papp (2014) for some evidence that NREGS has reduced seasonal migration out of rural areas.

estimates are now somewhat larger), providing reassurance that the mortality effects are not driven by compositional changes.

Permutation test: We implement a permutation test to provide an alternative assessment of the statistical significance of the results.¹⁶ We first randomly assign districts to each of the three groups (or phases) of program implementation, while holding fixed the proportion of districts enrolled in each phase of rollout to match the actual proportions. We then estimate the adult vs non-adult triple difference specification in Equation (3) for this placebo treatment assignment, and repeat this procedure 100 times. Figure 6 graphs the empirical cumulative distribution function (CDF) of the estimated treatment effects; the red vertical line marks the “actual” treatment effect that was obtained using the actual assignment. The actual treatment effect lies in the far right tail of the distribution, with only 3% of the estimated placebo treatment effects being larger (only 5% of the estimated placebo treatment effects are larger in absolute terms).

Placebo eligibility thresholds: To verify that the adult vs non-adult triple difference specifications are not picking up a spurious result, we consider a set of four alternative, equally spaced, placebo eligibility thresholds at the ages 21, 24, 27, and 30, and re-estimate the specifications at each of the alternative thresholds.¹⁷ If the true eligibility threshold is at age 18, we should not expect to pick up any significant program effects using these placebo thresholds. Appendix Table 8 reports the results from estimating the triple difference specifications using the placebo eligibility thresholds. In the simple triple difference specification, the estimated program effect shrinks as we move away from the actual eligibility threshold, and is statistically insignificant throughout. In the triple difference-discontinuity specification which looks for sharp changes at the eligibility threshold, the treatment effects at the placebo thresholds are also statistically

¹⁶See Chetty et al (2009) and Martin et al (2017) for analogous tests.

¹⁷Fewer than 5% of births occur to mothers above the age of 33.

insignificant and do not exhibit any systematic pattern. These results are in line with intuition, and provide reassurance that the estimated effect using the actual eligibility threshold is indeed a true program effect, rather than an artefact.

Manipulation tests: A potential concern with the adult vs non-adult triple difference specifications is that NREGS exposure might have induced some women to either defer or to bring forward child-bearing in order to take advantage of program eligibility. We investigate this hypothesis further in the data by testing for changes in the maternal age distribution (analogous to the density test proposed by McCrary 2008 in the context of the regression discontinuity design).

Figure 7 graphs the empirical CDF of maternal age at pregnancy, separately for treated and untreated births (right and left panels respectively), where treated births are defined to be those that occurred after the program was introduced in a district. The empirical CDFs are nearly identical, and do not suggest any significant changes in the distribution of births at the threshold. To assess this concern more formally, we test whether program exposure has changed the proportion of births to women above the age of 18. For each district-month combination, we calculate the empirical cumulative distribution function (CDF) of maternal age, i.e. the proportion of births in all ages up to and including each age category. We then use the empirical CDF as the dependent variable in the adult vs non-adult triple difference specifications, where the unit of observation is now a district-month-age cell. The results, reported in Appendix Table 9, confirm that program exposure has not significantly altered the distribution of births vis-a-vis the eligibility threshold.

Testing the domestic labor substitution hypothesis: As we observed in Section 4.2, the adult vs non-adult triple difference strategy may be undermined by program spillover effects on ineligible women. A particular concern in this regard is that ineligible women may take up a greater share of domestic work in order to substitute for eligible women

in the household (who take up NREGS work). Indeed, there is some evidence that the introduction of NREGS increases domestic work take-up by adolescent girls in the household (Shah and Steinberg 2015), although it should be noted that the ineligible mothers in our sample are married women, and therefore likely to have a distinct status in the spousal household (as compared to adolescent daughters). Nevertheless, we investigate this channel indirectly by estimating program effects while restricting the sample to mothers in households in which there is no other female above the age of 14, as the domestic labor substitution effect should not obtain in this sample. A limitation of this analysis is that household composition is measured at time of survey, which may imperfectly reflect composition at time of birth. The results are reported in Appendix Table 10, for each of the two adult vs non-adult triple difference specifications. The sample restriction results in a significant reduction in sample size (approximately 93,000 births), and the standard errors are accordingly much larger. It is notable however that the point estimates remain stable, suggesting that intra-household substitution is not a significant source of bias in these specifications.

Sensitivity to sample restrictions: Lastly, we assess the sensitivity of the results to changes in sample definitions. First, we examine the sensitivity of the results to the inclusion of partially exposed pregnancies (i.e. pregnancies that overlapped with program implementation). We re-estimate the adult vs non-adult triple difference specification for three alternative samples, including in turn, births that were exposed for (i) At least 1 trimester, (ii) At least 2 trimesters and (iii) At least 3 trimesters (i.e. fully exposed pregnancies only). Appendix Table 11 reports the results. While the estimated effects remain significant in all three samples, the point estimates increase, consistent with increasing intensity of exposure, as one moves from the least restrictive (Column 1) to the most restrictive sample (Column 3).

Second, given that the household survey overlapped with the program implemen-

tation, there may be a concern that the sequencing of the data collection may induce a selection bias in our estimates of the program effect. Accordingly, we conduct a sensitivity analysis that drops all births occurring after the start of the survey, i.e. by truncating birth histories at the first month of data collection.¹⁸ Appendix Table 12 confirms that truncating the birth histories at the first month of the survey implementation does not affect the results, with the estimates retaining their size and statistical significance.

5.3 Mechanisms

In this section, we attempt to understand why NREGS increased newborn deaths. Our investigation is guided by two broad hypotheses: First, the epidemiological literature discussed in Section 1 raises the possibility that program effects on infant health may reflect the fact that work participation during pregnancy can be detrimental to the health of the mother and the child as a result of work-related exertion and stress. While this conjecture is supported to some extent by qualitative evidence regarding the arduous nature of NREGS work, and the lack of availability of basic facilities such as drinking water at NREGS worksites (Reddy et al 2010),¹⁹ it is important to emphasize that the physical exertion hypothesis does not require that NREGS work should be any more strenuous than other kinds of work - it may be sufficient for this hypothesis that women are simply working more hours as a result of the program.

A second hypothesis is that the increase in female labor supply due to the introduction of NREGS may have crowded out health-producing inputs that are time-intensive to procure. Antenatal care is an important example of a time-intensive (and time-sensitive) input that can have a significant impact on pregnancy outcomes: The WHO

¹⁸By doing so, we ensure that all birth histories span the same period of time, Jan 2004-Oct 2007.

¹⁹Worksites are required by law to provide for drinking water, shade, and first aid kits, but in practice these facilities appear to be rarely provided (Reddy et al 2010).

now recommends a minimum of eight antenatal visits, in which the mother should receive advice on diet (including counseling against drinking and smoking), iron and folic acid supplements, prenatal vaccination against tetanus (which is a leading cause of neonatal mortality), and an early ultra-sound scan to detect any fetal anomalies or multiple pregnancies. Skilled care during childbirth is another time-sensitive health input that may be compromised if the mother is at work when she goes into labor (it may also happen that physical exertion associated with work triggers premature labor).

These two hypothesized channels are not mutually exclusive, and may even act to reinforce one another - for instance, pregnancy complications arising from physical exertion may not be attended in a timely manner if the mother does not have the time to avail of proper medical care. To shed light on these mechanisms in the data, we estimate program effects on two sets of outcomes. First, we examine program effects on indicators of fetal health, including premature births, and stillbirths. We also examine whether program impacts on neonatal mortality are concentrated in the very early neonatal period (24 hours after birth). Increases in the probabilities of these outcomes would generally be indicative of in-utero factors that adversely affect the fetus. Second, we test for program impacts on a set of pregnancy care inputs, including antenatal visits, receipt of tetanus vaccination, and hospital/clinic delivery. For completeness, we also look at program effects on breastfeeding duration and child immunization, although these are unlikely to explain neonatal mortality.

Given the multiplicity of hypotheses being tested, we undertake two corrective measures against the threat of false discovery. First, we estimate program effects on summary indices that efficiently aggregate the underlying outcome variables. Specifically, we construct an index of fetal health, which aggregates the outcomes of abnormal fetal movement, prematurity and stillbirth; an index of pregnancy care that aggregates the outcomes of any prenatal care, tetanus toxoid vaccination and hospital/clinic delivery; and an index of immunization that aggregates the vaccination outcomes. The

construction of the indices follows the GLS weighting procedure described in O'Brien (1984) and Anderson (2008). Second, we estimate program effects on the individual outcome variables while reporting uncorrected as well as False Discovery Rate corrected marginal "q-values" as in Anderson (2008).

Table 3 reports the estimated program effects on (i) the fetal health index, (ii) 24-hour mortality, and (iii) neonatal mortality conditional on surviving the first 24-hours. Appendix Table 13 reports the program effects on the individual components of the fetal health index. The estimations utilize the adult vs non-adult triple difference in Equation (3). We observe a significant worsening of fetal health, with statistically significant increases in premature births and probability of weak fetal movement. We find, as well, that the increase in newborn mortality appears to be concentrated within the first 24 hours after birth, while mortality effects in the post-24 hour period are small and statistically insignificant. These results strongly suggest that program exposure has adversely affected maternal and/or fetal health during pregnancy.

Table 4 reports estimated program effects on the pregnancy care index, breastfeeding duration and the immunization index. Appendix Table 14 reports program effects on the individual components of the indices. The program effects on the health inputs, although never statistically significant at conventional levels of significance, are uniformly negative, consistent with the findings of Miller and Urdinola (2010) and Bhallowra (2010), who also report negative effects of maternal employment on the demand for antenatal as well as post-natal health inputs.

The timing of pregnancy care inputs, in conjunction with the seasonality of rural work patterns, suggests a test of the crowding-out hypothesis. Seasonality in Indian agriculture is due to the timing of the monsoons, which usually arrive in the second half of the year. The first six months of the year are therefore typically a period during which the agricultural labor market is slack and work opportunities are few, and this is when NREGS would be expected to have the largest impact on work (Imbert and Papp

(2015) have exploited this variation in seasonality to examine program impacts on employment). In terms of mechanisms, one might conjecture therefore that children who were in utero during the slack season would be less likely to receive adequate antenatal care. This implies that children born in the peak season would have (on average) received less antenatal care than children born in the slack season. If maternal work also reduces the possibility of hospital/clinic delivery, children born in the slack season would be more likely to be delivered at home than children born in the peak season. We test these hypotheses in Appendix Table 15, where we estimate program effects on any antenatal care, tetanus vaccination and hospital/clinic delivery, separately for slack- and peak-season births. We find, as conjectured, that slack season births are indeed more likely to occur at home, whereas peak season births are less likely to receive antenatal care.

Overall, the evidence is supportive of the hypothesis of program exposure crowding-out time-intensive inputs, while also suggesting the effect of work-related stresses on fetal health. A natural question is to what extent these factors can explain the observed increase in neonatal mortality. Our results indicated that women exposed to NREGS had higher rates of preterm deliveries, and lower rates of antenatal care, tetanus vaccination and hospital/clinic delivery. Prematurity is a leading cause of neonatal mortality and is directly responsible for about 28% of neonatal deaths (Lawn 2009). Together with its consequences such as infections, it is indirectly responsible for up to half of all neonatal deaths (Simmons et al. 2010). Neonatal tetanus is yet another important cause of neonatal death. Lack of tetanus toxoid vaccination during pregnancy has been estimated to account for up to 21% of neonatal deaths in India (Singh et al 2012). Reduction in skilled delivery care is also thought to be an important cause of neonatal mortality (Powell-Jackson et al 2015 show that an 8 percentage point increase in facility births was associated with a reduction in newborn mortality of about 3 per 1000 live births). Based on these three factors alone, we would expect an additional 2.5 neonatal

deaths per 1000 births, which is within the range of estimates obtained in our study.²⁰

5.4 Program effects on longer-term child health, and fertility

So far the analysis has focused on short run effects. In this section we explore longer-term effects. First, we examine whether NREGS has had any effect on mortality for children beyond the neonatal period; and second, we test for fertility responses.

a. Post-neonatal mortality

One may conjecture that the increase in mortality observed for newborn infants may be reversed for older children, for whom the increase in household income due to the program may play a more dominant role in determining health outcomes. As we will see, our ability to draw conclusions about post-neonatal mortality is limited. To avoid bias due to censoring when looking at infant mortality (for example), we must restrict the sample to births that occurred at least 12 months prior to the survey. Doing so reduces power not only because of the sample size reduction, but also because of the loss of some of the identifying variation from the second phase of program implementation, since the latter occurred in April 2007 and the survey data collection started in November 2007.

In Appendix Table 16, we estimate program effects on 6-month, 9-month and 12-month (infant) mortality. While the point estimates suggest a reduction in mortality rates in the post-neonatal period, the effects are poorly estimated and preclude a

²⁰We estimated an increase of about 2.3 percentage points (6% relative to the mean) in preterm deliveries due to NREGS. Using the average rate of prematurity in the data, this suggests that the increase in preterm births would result in approximately 1 additional neonatal death per 1000 births.

Similarly, the observed 1.6 percentage point reduction in tetanus vaccination rates implies an additional 0.25 neonatal deaths per 1000 births.

Finally, the observed 3.3 percentage point reduction in rates of hospital/clinic delivery, in conjunction with the estimates of Powell-Jackson et al (2015), implies an additional 1.25 neonatal deaths per 1000 births.

definitive inference.

b. Fertility responses

Program effects on fertility constitute an interesting, and potentially important, mechanism by which health outcomes may be affected in the long run. In the developed world, improvements in work opportunities for women have reduced fertility. A similar quantity-quality tradeoff may be activated by NREGS, which might then improve child health outcomes in the long run. The close spacing of program phase-in again limits our ability to rigorously test for such longer-term effects – by April 2008 all districts are treated, and program effects cannot thereafter be easily distinguished from secular trends. Nevertheless, we directly test for short-run program effects on fertility by constructing a balanced panel of ever-married women who were married prior to 2004 (and were therefore “present” throughout the sample period in the sense of being able to have children). Each woman is observed once in each of the analysis years (as defined previously in Section 5.2). The resulting balanced panel has more than 2 million observations. We estimate program effects on fertility in this sample, looking at the probability of pregnancy and the probability of live birth. The results are reported in Table 5. We do not find a statistically significant program impact on either pregnancy or live birth, although it should be noted that the point estimates are negative (which is reassuring in the sense that our main results are unlikely to be driven by a worsening in the composition of births arising from an increase in fertility). The lack of significant fertility response may appear surprising, but it should be emphasized that these are short-run effects. The lack of immediate response on these dimensions may also reflect an initial uncertainty regarding the permanency of NREGS. Whether NREGS affected fertility choices in the longer-term remains an interesting, but unanswered question.

6 Concluding Remarks

This paper has examined the effect of maternal employment on infant health in the context of a large workfare program targeting women, India's National Rural Employment Guarantee Scheme. We find that NREGS rollout resulted in a significant increase in newborn deaths. This conclusion is robust to a range of identification strategies, including a triple difference strategy that takes advantage of a discontinuity at age 18 in program eligibility. Mortality effects are also concentrated among working women, consistent with the nature of the program, and the results exhibit a dose-response pattern as exposure to the program increases.

Our investigation of possible mechanisms supports a 'crowd-out' hypothesis - women exposed to the program report fewer antenatal visits and are also less likely to utilize hospital delivery care, consistent with the economic literature that finds that maternal work displaces time-intensive health-inputs. At the same time, we also find some evidence suggestive of adverse effects of work, potentially as a result of exertion and stress. We observe a decrease in fetal movements (as reported by the mother), and an attendant increase in the rate of premature birth and stillbirth. Also suggestive of work-related stresses during pregnancy adversely affecting fetal health, we find that the increase in newborn mortality occurs within a 24-hour period immediately surrounding birth.

The results in this paper appear to present a dilemma for policy-makers, given that programs such as NREGS tend to be enormously popular, and have a range of beneficial effects (see for example Zimmerman 2012).²¹ Our results, while highlighting the potential negative effects of such programs, specifically in this case on infant health, do however suggest some ways in which these effects can be mitigated. For instance, a

²¹Li and Sekhri (forthcoming) and Shah and Steinberg (2015) highlight yet another unintended consequence of NREGS, namely its effect on human capital investments. Both studies find negative impacts on school enrolment and learning outcomes.

policy that increases flexibility of work schedules for pregnant women may be an appropriate and reasonable way to reduce or minimize the crowding-out of health-care during pregnancy. In terms of the physical demands of program work, at a minimum, there should be better enforcement of existing program regulations regarding the provision of, and accessibility to, facilities available at worksites for women. In addition, sensible strategies to reduce physical labor and exertion by pregnant women could also be implemented. Practicable strategies may include shifting pregnant women onto lighter tasks, and/or incentivizing households to delay pregnant household members from working until after the delivery.

We end by emphasizing that the effects on child health found in this paper are short-run effects and longer-run overall effects may well be positive, if, for instance, maternal work participation has positive income effects for older children, as has been reported in the existing literature. Furthermore, in the longer-run, the increase in female work opportunities and earning power embodied in NREGS may help to reduce fertility and increase investments in child health, as has happened following an increase in work opportunities for women in other developing countries (Korstad and Rønsen 2014, Van den Broeck et al 2015; Fang et al. 2013). Research into these longer-term program impacts is essential for a more rounded understanding of the effects of such programs.

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Appendix A: Quadruple differences, and Triple difference + Regression discontinuity

In the triple difference specifications outlined in Section 4.2, the remaining threat to identification arises from unobservable confounders that vary across time, space and within-district eligibility. Specifically, in the rural vs. urban triple difference estimation, any remaining confounders would have to vary by time, district and rural/urban location; in the adult vs. non-adult triple difference estimation, confounders would have to vary by time, district and adult status. One way to account for such confounders is to "layer" the two comparison groups to estimate a quadruple- or fourth-difference, as in the following specification:

$$y_{jdt} = \alpha + \beta Treat_{dt} \times Adult_{jdt} \times Rural_{ldt} + \gamma \mathbf{X}_{jdt} + \eta_{adl} + \eta_{adt} + \eta_{alt} + \eta_{dlt} + \varepsilon_{jdt} \quad (5)$$

where we now control for all the potential triple interactions between district, time, rural/urban location and adult/non-adult status by means of the fixed effects η_{adl} , η_{adt} , η_{alt} and η_{dlt} . In this specification, β is the fourth-difference estimate of the ITT effect of NREGS.

As we will see, the fourth difference estimator has insufficient power to estimate program effects with any precision. We therefore consider an alternative estimation strategy that refines the adult vs non-adult triple difference specification by exploiting the fact that program eligibility changes discontinuously at age 18. The intuition behind this strategy is that if one assumes that the unobserved confounders vary smoothly with maternal age, then any discontinuous change in the effect of (district-level) program exposure at the eligibility threshold is likely to represent a true program effect. This approach combines the triple difference strategy with a regression discontinuity design: It is as though we were estimating a triple difference specification in

the vicinity of the eligibility threshold, comparing the effects of program exposure on just-eligible and just-ineligible women.²² Because the sample sizes do not permit such a fine comparison, however, we instead adopt a flexible parametric version of the strategy. Specifically, we modify the adult vs. non-adult triple difference specification in Equation (3) to accommodate an age-based discontinuity in the effect of (district-level) program exposure as follows:

$$\begin{aligned}
 y_{jdt} = & \alpha + \beta Treat_{dt} \times Adult_{jdt} \\
 & + \gamma_1 Treat_{jdt} \times f(Age_{jdt}) \times Adult_{jdt} + \gamma_2 Treat_{jdt} \times f(Age_{jdt}) + \gamma_3 f(Age_{jdt}) \times Adult_{jdt} \\
 & + \gamma_4 f(Age_{jdt}) + \lambda \mathbf{X}_{jdt} + \eta_{ad} + \eta_{at} + \eta_{dt} + \varepsilon_{jdt}
 \end{aligned} \tag{6}$$

where Age is a maternal age variable centered at 18 and f denotes a linear function. In this specification, the effect of district-level NREGS exposure is allowed to vary smoothly with maternal age, and for added flexibility, these trends are allowed to be different on the two sides of the eligibility threshold. Analogous to the case of a simple parametric regression discontinuity design, the centering of the age variable around the eligibility threshold ensures that β will estimate the discontinuity in the effect of program exposure at the threshold - we refer to this as the triple difference-RD estimate of the ITT effect.

²²See Carneiro, Løken and Salvanes (2015) for an estimation strategy that is similar in spirit.

Figure 1. The color-coded map above shows the districts that were enrolled in each of the three phases of NREGS implementation.

Figure 2. The figure shows the timeline of NREGS implementation vis-à-vis survey data collection and the retrospective survey period.

Figure 3: The upper panel graphs the rate of participation in NREGS for rural, married women as a function of age. The lower panel graphs the rate of neonatal mortality against maternal age at pregnancy, separately for births that occurred before and after program introduction in a district ("untreated" and "treated" births, respectively). The sample in the lower panel is restricted to live births in rural areas that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district.

Figure 4. The figure plots the rate of neonatal mortality for each of the sample birth years, separately for each of the three program phase groups. The sample includes all live births to rural adult women, that occurred at least one month prior to interview.

Figure 5. The figure plots the rate of neonatal mortality for each of the sample birth years, separately for each of the three program phase groups. The sample includes all live births to program-ineligible women, i.e. those in urban areas and/or under the age of 18 at time of birth. The sample includes births that occurred at least one month prior to interview.

Figure 6. The figure graphs the empirical cumulative distribution function (CDF) of the treatment effects estimated for the 100 placebo district assignments. The vertical line marks the treatment effect estimated for the actual district assignment.

Figure 7. The figures above graph the empirical cumulative distribution function (CDF) of the age distribution of births, separately for untreated (left panel) and treated births (right panel), respectively, where a birth is considered to be "treated" if it occurred after the program was introduced in the district. The samples are restricted to live births in rural areas, and exclude births that occurred within 6 months of program introduction in a district.

Table 1. Summary statistics

	Phase 1	Phase 2	Phase 3
<i>Household, maternal and birth characteristics</i>			
Household wealth index	2.160 (0.004)	2.456 (0.006)	3.013 (0.004)
Hindu	0.804 (0.001)	0.740 (0.002)	0.731 (0.002)
Scheduled Caste/Scheduled Tribe	0.469 (0.002)	0.337 (0.002)	0.359 (0.002)
Mother is literate	0.381 (0.002)	0.425 (0.002)	0.546 (0.002)
Spouse's education (yrs)	5.351 (0.015)	5.572 (0.019)	6.717 (0.015)
Mother currently working (currently pregnant women)	0.389 (0.006)	0.355 (0.007)	0.293 (0.006)
Mother currently working (not currently pregnant women)	0.367 (0.002)	0.343 (0.003)	0.299 (0.002)
Child gender (Boy==1)	0.517 (0.0017)	0.520 (0.002)	0.526 (0.002)
Multiple birth	0.015 (0.000)	0.015 (0.001)	0.015 (0.000)
<i>Health inputs</i>			
Any antenatal care (last live/still birth)	0.666 (0.002)	0.712 (0.002)	0.729 (0.002)
Facility delivery (last live birth)	0.302 (0.002)	0.342 (0.003)	0.464 (0.002)
<i>Health outcomes</i>			
Premature birth	0.349 (0.002)	0.293 (0.002)	0.276 (0.002)
Still birth	0.010 (0.000)	0.011 (0.000)	0.009 (0.000)
1-day mortality	0.0129 (0.0004)	0.0138 (0.0005)	0.0123 (0.0004)
30-day mortality	0.0267 (0.0006)	0.0285 (0.0007)	0.0216 (0.0005)
Number of districts	192	125	268

Notes: The table presents means and standard errors (in parentheses) for the variables used in the analysis. The samples include all live births in rural areas since January 2004, except in the case of health problems during pregnancy, premature birth, antenatal care and health facility delivery which are only recorded for the most recent live/still birth; and the number of vaccinations which is known for the two most recent surviving children.

	Diff-in-diff	Triple difference: Rural vs Urban comparison	Triple difference: Adult vs non-adult comparison
<i>Dependent variable: Neonatal mortality</i>	(1)	(2)	(3)
NREGS (ITT effect)	0.0033** (0.0014)	0.0082** (0.0034)	0.0080** (0.0039)
Sample	Rural Adults	Rural Adults+All Urban	Rural Adults+All non- adults
Observations	1,57,812	204,703	208,456

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS. The dependent variable is neonatal mortality. *Adult* denotes a mother who was at least 18 years old at the start of pregnancy. All regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. The difference-in-differences regression (Column 1) includes district fixed effects and month (i.e. birth year x birth month) fixed effects, and the sample includes births to rural, adult mothers. The regression in Column 2 includes district x month fixed effects, district x rural fixed effects and rural x month fixed effects. The regression in Column 3 includes district x month fixed effects, adult x month fixed effects, and district x adult fixed effects. In all regressions, the sample is restricted to live births that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district. Standard errors, clustered at district level, are in parentheses.

	(1)	(2)	(3)
<i>Dependent variable</i>	Index of fetal health	1-day (24-hour) mortality	Neonatal mortality after the first day
NREGS (ITT)	-0.0490*** (0.0187)	0.0087*** (0.0032)	-0.0008 (0.0023)
Observations	154,641	208,456	204,867

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on a range of health outcomes related to pregnancy. The estimation uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations), adult x month fixed effects, and district x adult fixed effects. The outcome variable in Column 1 is an efficient weighted index of the following fetal health indicators: (i) Weak or abnormal fetal movement, (ii) Premature birth, and (iii) Still birth, and is defined so that larger (i.e. more positive) values of the index correspond to better fetal health. With the exception of (iii), the other outcomes are only elicited for the mother's most recent live/still birth. The dependent variable in Column 3 is an indicator for neonatal death conditional on the child surviving the first 24 hours after birth. The regression samples exclude births that occurred within 6 months of program introduction in a district. All regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. Standard errors are clustered at district level.

Table 4. Mechanisms: Health inputs

	(1)	(2)	(3)
<i>Dependent variable</i>	Index of pregnancy care utilization	Breastfeeding	Vaccination index
NREGS (ITT)	-0.0444** (0.0215)	-1.4996 (1.5685)	-0.0242 (0.0216)
Observations	154,641	124,401	156,852

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on measures of health input provision/utilization. The estimation uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations), adult x month fixed effects, and district x adult fixed effects. The outcome variable in Column 1 is an efficient weighted index of the following pregnancy care indicators: (i) Any prenatal care, (ii) Tetanus toxoid vaccination, (iii) Hospital/clinic delivery, and is defined so that larger (i.e. more positive) values of the index correspond to greater care usage. These outcomes are only elicited for the mother's most recent live/still birth. The outcome variable in Column 2 is the duration in days of exclusive breastfeeding, elicited for the most recent surviving birth, with the regression sample being restricted to births that occurred at least six months prior to interview. The outcome variable in Column 3 is an efficient weighted index of the following vaccination indicators: (i) BCG, (ii) DPT, (iii) Polio and (iv) Measles. Information on vaccines was elicited for the two most recent surviving births, and the sample is restricted to children who were born at least 6 months prior to survey. In all regressions, the sample excludes births that occurred within 6 months of program introduction in a district. All regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. Standard errors are clustered at district level.

Table 5. Program effect on fertility

<i>Dependent variable</i>	(1)	(2)
	Pregnancy	Live birth
NREGS (ITT)	-0.0181 (0.0135)	-0.0150 (0.0131)
Mean of dep variable	0.0932	0.0881
Observations	2,174,995	2,174,995

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on the probability of pregnancy and probability of live birth, using a balanced panel of mothers who were married prior to 2004. Each mother is observed for each of the analysis years defined in Section 5.2. The estimation uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations), adult x month fixed effects, and district x adult fixed effects. Standard errors are clustered at district level.

Appendix Table A1. NREGS and neonatal mortality: Quadruple difference and triple difference-RD estimates

	4 th difference estimator	Triple difference + RD
<i>Dependent variable: Neonatal mortality</i>	(1)	(2)
NREGS (ITT effect)	0.0037 (0.0190)	0.0087* (0.0045)
Sample	Rural+Urban, Adult+Non-adult	Rural Adults+All non-adults
Observations	248,722	208,456

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS. The dependent variable is neonatal mortality. The estimation in Column 1 uses the fourth difference specification, which includes district x month x adult fixed effects, district x rural x adult fixed effects, district x month x rural fixed effects, and adult x rural x month fixed effects. The regression sample includes births to rural as well as urban mothers of all ages. In Column 2, the regression includes district x month fixed effects, adult x month fixed effects, and district x adult fixed effects, as well as interactions between the district-level treatment exposure indicator NREGS, *Adult* and a maternal age variable centered at 18. The regression samples are restricted to live births that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district. The regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. Standard errors, clustered at district level, are in parentheses.

Appendix Table 2. NREGS and neonatal mortality (Without controls)

<i>Dependent variable: Neonatal mortality</i>	Diff-in-diff	Triple difference: Rural vs Urban comparison	Triple difference: Adult vs non-adult comparison
	(1)	(2)	(3)
NREGS (ITT effect)	0.0029** (0.0014)	0.0077** (0.0034)	0.0071* (0.0039)
Observations	158,006	204,948	208,710

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS. The dependent variable is neonatal mortality. The difference-in-differences regression (Column 1) includes district fixed effects and month (i.e. birth year x birth month) fixed effects, and the sample includes births to rural, adult mothers. The regression in Column 2 includes district x month fixed effects, district x rural fixed effects and rural x month fixed effects. The regression in Column 3 includes district x month fixed effects, adult x month fixed effects, and district x adult fixed effects. In all regressions, the sample is restricted to live births that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district. Standard errors, clustered at district level, are in parentheses.

Appendix Table 3. Treatment effects in working and non-working samples

<i>Dependent variable: Neonatal mortality</i>	Difference-in-differences	Triple difference: Rural vs urban	Triple difference: Adult vs non-adult
<i>Panel A: Working women</i>			
NREGS (ITT)	0.0061** (0.0026)	0.0185** (0.0078)	0.0151* (0.0090)
Observations	57,621	65,538	72,206
<i>Panel B: Non-working women</i>			
NREGS (ITT)	0.0012 (0.0018)	0.0050* (0.0028)	0.0033 (0.0046)
Observations	57,621	65,538	72,206

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS, separately for working and non-working women (top and bottom panels respectively), where working women refers to women who reported having worked in the week prior to interview. The dependent variable is neonatal mortality. *Adult* is an indicator for whether the mother was at least 18 years old at the time of pregnancy. *Rural* is an indicator for whether the mother resides in a rural area. The difference-in-differences regression (Column 1) includes district fixed effects and month (i.e. birth year x birth month) fixed effects, and the sample includes births to rural, adult mothers. The regression in Column 2 includes district x month fixed effects, district x rural fixed effects and rural x month fixed effects, and the sample includes births to rural as well as urban adult mothers. The regression in Columns 3 includes district x month fixed effects, adult x month fixed effects, and district x adult fixed effects, and the regression samples include births to rural mothers of all ages. In all regressions, the sample is restricted to live births that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district. All regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. Standard errors are clustered at district level.

Appendix Table 4. Leads and lags of program effects

<i>Dependent variable: Neonatal mortality</i>	Difference-in-differences	Triple difference: Rural vs urban	Triple difference: Adult vs non-adult
Treatment (T-4)	0.0017 (0.0026)	0.0017 (0.0026)	0.0017 (0.0026)
Treatment (T-3)	-0.0006 (0.0017)	-0.0006 (0.0017)	-0.0006 (0.0017)
Treatment (T-2)	0.0003 (0.0011)	0.0004 (0.0011)	0.0003 (0.0011)
Treatment (T)	0.0031* (0.0019)	0.0044* (0.0023)	0.0056* (0.0032)
Treatment (T+1)	0.0036 (0.0027)	0.0049* (0.0029)	0.0060 (0.0038)
Observations	157,625	199,951	207,649

Notes: The table reports the estimated leads and lags of the Intent-To-Treat (ITT) effects of NREGS on neonatal mortality. The sample period is divided into the following years that co-incide with program implementation: Jan 04-March 04, Apr 04-Mar 05, Apr 05-Jan 06, Feb 06-Mar 07, Apr 07-Mar 08. All effects are relative to T-1, the year prior to program implementation. In all regressions, the sample is restricted to live births that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district. All regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. Standard errors are clustered at district level.

Appendix Table 5. NREGS and neonatal mortality: Placebo program implementation

<i>Dependent variable: Neonatal mortality</i>	Diff-in-diff	Triple difference: Rural vs Urban comparison	Triple difference: Eligible vs ineligible comparison
	(1)	(2)	(3)
NREGS (ITT effect)	0.0012 (0.0027)	0.0019 (0.0071)	0.0003 (0.0099)
Observations	182,903	277,626	213,330

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of a placebo implementation of NREGS, which is assumed to start in Feb 2000. The assignment of districts to phases and the spacing between the phases is maintained as per the actual assignment and spacing. The analysis uses data from the DLHS round of 2002-04. The dependent variable in the regressions is neonatal mortality. The difference-in-differences regression (Column 1) includes district fixed effects and month (i.e. birth year x birth month) fixed effects. The regression in Column 2 includes district x month fixed effects, district x rural fixed effects and rural x month fixed effects. The regression in Column 3 includes district x month fixed effects, adult x month fixed effects, and district x adult fixed effects. In all regressions, the sample is restricted to live births that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district. All regressions include controls for Hindu household, multiple birth and child gender. Standard errors, clustered at district level, are in parentheses.

Appendix Table 6. Tests for compositional changes

	Diff-in-diff	Triple difference: Rural vs Urban	Triple difference: Adult vs non-adult
	(1)	(2)	(3)
<i>Characteristics (dependent variables)</i>			
Gender (Boy=1)	-0.0049 (0.0049)	-0.0087 (0.0142)	0.0019 (0.0116)
Multiple birth	-0.0012 (0.0016)	-0.0065 (0.0042)	-0.0024 (0.0029)
Parity	-0.0013 (0.0177)	-0.0256 (0.0458)	-0.0265 (0.0250)
Maternal age at birth	0.0258 (0.0462)	0.1034 (0.1158)	-0.0364 (0.0566)
Mother literate	-0.0031 (0.0040)	0.0093 (0.0104)	-0.0054 (0.0101)
Spouse's education (years)	-0.0623 (0.0386)	0.0623 (0.1014)	-0.0873 (0.0924)
Hindu household	-0.0044 (0.0029)	-0.0003 (0.0084)	-0.0075 (0.0077)
Scheduled Caste/Tribe	-0.0004 (0.0038)	-0.0016 (0.0093)	0.0048 (0.0093)
Household asset index	-0.0118* (0.0060)	0.0329 (0.0217)	-0.0071 (0.0140)

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS. The dependent variables (in rows) are observed household, mother and birth characteristics; the ITT effects are reported across the columns, for each of the specifications reported in Table 2. The regression samples include only live births that occurred at least one month prior to interview, and exclude births that occurred within 6 months of program introduction in a district. Standard errors are clustered at district level.

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Appendix Table 7. Testing for the effect of compositional changes

<i>Dependent variable: Neonatal mortality</i>	(1)	(2)
NREGS (ITT)	0.0032** (0.0016)	0.0057* (0.0031)
Sample	Mothers married prior to 2004	
Specification	District fixed effects	Mother fixed effects
Observations	134,628	134,628

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on neonatal mortality. The regressions include month (i.e. birth year x birth month) fixed effects, and the samples include births to rural, adult mothers. The samples are restricted to births to mothers who were married prior to 2004. The regression in Column 2 includes mother fixed effects. In all regressions, the sample is restricted to live births that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district. All regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. Standard errors are clustered at district level.

Appendix Table 8. Placebo eligibility thresholds

	Triple difference: Adult vs non- adult (1)	Triple difference - RD (2)
<i>Dependent variable: Neonatal mortality</i>		
<i>Placebo eligibility threshold at:</i>		
Age 21	0.0055* (0.0031)	0.0008 (0.0050)
Age 24	0.0026 (0.0029)	-0.0036 (0.0044)
Age 27	-0.0015 (0.0035)	-0.0083 (0.0053)
Age 30	0.0003 (0.0045)	0.0015 (0.0072)

Notes: The table reports placebo Intent-To-Treat (ITT) effects of NREGS, separately for each of a set of placebo eligibility thresholds. The dependent variable is neonatal mortality. The estimation in Column 1 uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations), adult x month fixed effects, and district x adult fixed effects. The estimation in Column 2 uses the triple difference-RD specification, which also includes interactions between NREGS, Adult and a maternal age variable centered at the placebo threshold. The triple difference-RD specification also includes interactions between NREGS and each of the control variables. In all regressions, the sample is restricted to live births that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district. All regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. Standard errors are clustered at district level.

Appendix Table 9. Testing for manipulation of eligibility

<i>Dependent variable: Empirical CDF</i>	Triple difference: Adult vs non-adult	Triple difference - RD
	(1)	(2)
NREGS (ITT)	0.0008 (0.0018)	-0.0029 (0.0027)
Observations	168,474	168,474

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS, where the dependent variable is the empirical cumulative distribution function (CDF) of maternal age, calculated for each district-month combination. Thus, the regression sample includes one observation for each district x month x maternal age cell. The estimation in Column 1 uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations), adult x month fixed effects, and district x adult fixed effects. The estimation in Column 2 uses the triple difference-RD specification, which also includes interactions between NREGS, Adult and a maternal age variable centered at 18. The sample excludes births that occurred within 6 months of program introduction in a district. Standard errors are clustered at district level.

Appendix Table 10. Treatment effect in single-woman households

<i>Dependent variable: Neonatal mortality</i>	Triple difference: Adult vs non- adult	Triple difference- RD
NREGS (ITT)	0.0093 (0.0064)	0.0073 (0.0073)
Observations	118,583	118,583

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on neonatal mortality, restricting the sample to households in which the mother is the only female above the age of 14. The regressions include district x month fixed effects, adult x month fixed effects, and district x adult fixed effects. The triple difference-RD regression (Column 2) also includes interactions between NREGS, Adult and a maternal age variable centered at 18, as well as interactions between NREGS and each of the control variables. In all regressions, the sample is restricted to live births that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district. All regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. Standard errors are clustered at district level.

Appendix Table 11. Sensitivity to alternative partial birth definitions

<i>Dependent variable: Neonatal mortality</i>	(1)	(2)	(3)
NREGS (ITT)	0.0060 (0.0038)	0.0080** (0.0039)	0.0087* (0.0046)
Sample: Number of trimesters of exposure	≥1	≥2	≥3
Observations	213,109	208,456	198,695

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on neonatal mortality, separately for three different samples: Including only pregnancies that were exposed for at least 1 trimester (Col 1), including only pregnancies exposed for at least 2 trimesters (Col 2), and including only pregnancies that were exposed for all 3 trimesters (Col 3). The estimation uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations), adult x month fixed effects, and district x adult fixed effects. In all regressions, the sample is restricted to live births that occurred at least one month prior to interview. All regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. Standard errors are clustered at district level.

Appendix Table 12. Sensitivity to sample truncation

<i>Dependent variable: Neonatal mortality</i>		(1)
NREGS		0.0071* (0.0042)
Sample	Births prior to Nov 2007	
Observations		199,130

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on neonatal mortality, while truncating the sample to only include births that occurred prior to November 2007. The estimation uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations) , adult x month fixed effects, and district x adult fixed effects. All regressions include controls for Hindu household, maternal literacy, multiple birth and child gender. In all regressions, the sample is restricted to live births that occurred at least one month prior to interview, and excludes births that occurred within 6 months of program introduction in a district. Standard errors are clustered at district level.

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App Table 13. Mechanisms: Fetal health components

	Weak or no fetal		
	movement	Premature birth	Still birth
	(1)	(2)	(3)
NREGS (ITT)	0.0167 (0.0084)	0.0212 (0.0140)	0.0040 (0.0031)
Uncorrected p-value	0.04	0.11	0.18
FDR q-value	0.12	0.17	0.185
Mean of dep variable	0.081	0.308	0.014
Observations	154,641	154,641	154,641

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on the separate components of the fetal health index. The estimation uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations) , adult x month fixed effects, and district x adult fixed effects. The outcome variables in Columns 1 and 2 are only elicited for the mother's most recent live/still birth. The regression sample in Column 3 includes live as well as still births. The regression samples exclude births that occurred within 6 months of program introduction in a district. Standard errors are clustered at district level. The table reports uncorrected p-values for each coefficient, as well as False Discovery Rate corrected marginal "q-values" as in Anderson (2008).

	Received any antenatal care	Received tetanus vaccination	Delivered in health facility	BCG vaccine	DPT vaccine	Polio vaccine	Measles vaccine
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NREGS (ITT)	-0.0150 (0.0124)	-0.0206 (0.0127)	-0.0241 (0.0131)	-0.0132 (0.0096)	-0.0059 (0.0125)	-0.0093 (0.0129)	-0.0094 (0.0117)
Uncorrected p-value	0.23	0.10	0.07	0.17	0.64	0.47	0.42
FDR q-value	0.40	0.36	0.36	0.39	0.64	0.55	0.55
Mean of dep variable	0.699	0.679	0.368	0.839	0.776	0.979	0.655
Observations	154,641	154,641	154,641	167,370	168,394	168,394	157,262

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on the separate components of the pregnancy care index and the vaccination index. The estimation uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations), adult x month fixed effects, and district x adult fixed effects. The outcomes in Columns 1, 2 and 3 are only elicited for the mother's most recent live/still birth. Information on BCG, DPT, polio and measles vaccines was elicited for the two most recent surviving births, and the sample is restricted to children who were born at least 6 months prior to survey. In all regressions, the sample excludes births that occurred within 6 months of program introduction in a district. Standard errors are clustered at district level. The table reports uncorrected p-values for each coefficient, as well as False Discovery Rate corrected marginal "q-values" as in Anderson (2008).

App Table 15. Seasonality and the timing of inputs

	Received any antenatal care (1)	Received tetanus vaccination (2)	Delivered in health facility (3)
<i>Panel A: Slack season births</i>			
NREGS (ITT)	0.0072 (0.0215)	-0.0022 (0.0218)	-0.0411* (0.0226)
Observations	71,174	71,174	71,174
<i>Panel B: Peak season births</i>			
NREGS (ITT)	-0.0334** (0.0160)	-0.0374** (0.0167)	-0.0152 (0.0174)
Observations	83,467	83,467	83,467

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on pregnancy care inputs, separately for slack season and peak season births, which refer to the first and last six months of the year respectively. The estimation uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations), adult x month fixed effects, and district x adult fixed effects. The outcomes in Columns 1, 2 and 3 are only elicited for the mother's most recent live/still birth. In all regressions, the sample excludes births that occurred within 6 months of program introduction in a district. Standard errors are clustered at district level.

Appendix Table 16. Program effects on longer-term mortality

<i>Dependent variable</i>	(1)	(2)	(3)
	6-month mortality	9-month mortality	12-month (infant) mortality
NREGS (ITT)	0.0084 (0.0056)	0.0055 (0.0065)	0.0018 (0.0077)
Observations	188,007	175,658	162,265

Notes: The table reports the estimated Intent-To-Treat (ITT) effects of NREGS on six-month, nine-month and twelve-month (infant) mortality. The estimation uses the adult vs non-adult triple difference specification, which includes district x month fixed effects (where month denotes birth year x birth month combinations), adult x month fixed effects, and district x adult fixed effects. For 6-month mortality, the sample is restricted to live births that occurred at least six months prior to interview. For 9-month mortality, the sample is restricted to live births that occurred at least nine months prior to interview. For 12-month mortality, the sample is restricted to live births that occurred at least twelve months prior to interview. In all regressions, the sample excludes births that occurred within 6 months of program introduction in a district. Standard errors are clustered at district level.

Highlights

- We study the effect a workfare program in India on newborn survival.
- Program exposure appears to have increased the rate of neonatal mortality.
- Underlying these effects are indicative changes in fetal health, as well as changes in pregnancy care usage.