# The Effect of a Large-Scale Workfare Program on Child Marriage in India

Magda Tsaneva[*] Ashley O'Donoghue[†]

Version 2 – June 15, 2019

Abstract

This paper examines the impact of a large-scale workfare program, the National Rural Employment Guarantee Scheme (NREGS), on child marriage in India. We use two rounds of data from the District Level Household & Facility Surveys and estimate a difference-in-differences model by comparing changes in child marriage rates between a cohort of young women and a cohort of older women before and after the program was implemented and across districts with different program intensity. Our results are also robust to using a hazard model. Overall, we find that NREGS is associated with an increase in the probability of marriage before 18 and our findings are similar when using different definitions of child marriage. Heterogeneity analysis shows that the effects are largest in districts with higher prevalence of arranged marriages and dowry deaths at baseline, which is consistent with dowry practices affecting marriage decisions when resource constraints are relaxed.

Keywords: child marriage, workfare, NREGS, India
JEL codes: J12, I38, J68

[*] Department of Economics, Clark University, Worcester, MA. Email: mtsaneva@clarku.edu
[†] Department of Economics, Clark University, Worcester, MA. Email: aodonoghue@clarku.edu

## 1. Introduction

Despite the fact that 18 is the legal age of marriage for girls in India, 44.5 percent of women aged 20 to 24 in 2006 were married by the age of 18 (WDI 2006). Early marriage could have important negative consequences for young women. For example, Jensen and Thornton (2003) show that young brides are often subject to domestic violence. Field and Ambrus (2008) find that delaying marriage by one year in rural Bangladesh could increase schooling, literacy and use of preventive health services. Early marriage is also associated with early pregnancy, which may have adverse effects not only for the young mothers but also for their children. In India, Chari et al. (2017b) find that delaying marriage is associated with better child health and educational outcomes. Understanding the factors that affect early marriage, and subsequently childbearing, is thus of great policy concern.

This paper explores the effects of the National Rural Employment Guarantee Scheme (NREGS) on child marriage among girls in India. NREGS is the largest public works program in the world, accounting for 1 per cent of India's GDP (Subbarao, Del Ninno, Andrews, & Rodriquez-Alas, 2012). While its focus has been on improving rural livelihoods and labor market outcomes, it is important to quantify any spillover effects it may have had on other socio-economic and demographic outcomes. Analysis of the effects of NREGS on child marriage could also have broader implications for the impacts of other income-generating policies or economic growth.

Identifying the effect of the program is challenging for several reasons. Cross-sectional surveys sometimes lack information on age of marriage. Even when the information is available, analysis of determinants of child marriage don't take into account changing trends in age of marriage and direct indicators of household program participation may be subject to selection bias. On the other hand, panel surveys often fail to follow women after marriage, creating a selected sample. Even when household members are followed to their new household, the timing

of their marriage is often unknown. To account for these challenges, we use two rounds of a repeated cross-sectional survey with information on the marital status of every household member and detailed marriage history of all ever-married women between 15 and 49 years of age. Specifically, we use data from the second round of the District Level Household and Facility Survey (DLHS), which took place in 2002-2004, before NREGS was implemented, and the fourth round of the surveys from 2013-2014, after the program was rolled out. We take advantage of time and cohort variation to examine the effect of the program on changes in the probability of marriage before 18. This difference-in-differences specification uses differences between pre and post period for older, unaffected, cohorts to serve as a counterfactual for the difference between pre and post period for younger, affected, cohorts, thus controlling for pre-existing trends in child marriage rates. Our main sample includes both married and unmarried women aged 18 years and above. We also present robustness results for different definitions of child marriage, including marriage before 15, 16, and 17 years of age. While our main analysis identifies the effect of NREGS on changes in the probability of a discrete outcome, the results are also robust to using a duration model to estimate the continuous effect on age of marriage (where observations are right-censored at the age of 17 for unmarried women or women married at 18 or later). To alleviate concerns about other potential nation-wide policy changes or events occurring during the study period that may be affecting our estimates, we also present an augmented model which uses a measure of program intensity across districts.

Overall, we find that the decreasing trend in child marriage rates is slowed after 2006, when NREGS is first introduced, and districts with higher program intensity experience an increase in child marriage rates relative to districts with lower program intensity. We find that the effects are more pronounced in districts that have higher incidence of dowry-related deaths and higher proportion of arranged marriages, suggesting that one potential explanation for the

effect of NREGS on child marriage is that the additional income from the program allows households to pay dowries early on. The proportion of Indian households paying dowry has increased over the last few decades from about 40% of households paying dowries in 1940 to 88% in 1970, and dowries are now largely universal (Chiplunkar and Weaver 2017). Anukriti et al. (2018) estimate that the average expected dowry in 2007 was about 14% of annual household income. They further show that families of girls finance dowries largely through savings and increased parental labor supply. A positive relationship between dowry size and bride age has been documented in India as young brides may be seen as more desirable and thus need to pay lower groom price (Chowdhury 2010). In addition, child marriage in India is considered socially acceptable and even desirable.[1] Households thus have an incentive to marry their daughters young and NREGS may have allowed poorer households to do so by providing an additional source of income.

Next, we provide background on NREGS. In section 3, we discuss the potential channels through which NREGS may affect child marriage. Then, we present the data and empirical strategy. In section 6, we discuss our findings. Section 7 concludes.

## 2. Background on the National Rural Employment Guarantee Scheme

The National Rural Employment Guarantee Scheme (NREGS) was first introduced in February 2006 to 200 of the poorest rural districts in India. The second phase of the program expanded access to 130 additional districts in April 2007. A year later, the program was made available to all remaining districts. It is now the largest public works program in the world, accounting for 1% of India's GDP (Subbarao et al. 2012). Given the susceptibility of rural households in India to periodic weather shocks and seasonal variations, NREGS has been tailored

[1] Studies show that Indian families have traditionally felt guilt and shame over the presence of a menstruating daughter in the household (Chari et al. 2017a).

to meet the objective of livelihood security by reducing the dependence on agricultural wages. The program provides households living in rural areas with 100 days a year of paid low-skilled work in projects including construction of roads and improving irrigation and sanitation at a specified minimum wage. It differs from previous schemes in that it promises employment as an entitlement and there are no eligibility requirements. The act also stipulates that one-third of all beneficiaries should be women. While there has been criticism of the program for its poor implementation, demand rationing, and delayed wage payments (Murgai et al. 2016; Narayanan et al. 2016), several studies have documented the short-run benefits of the program.

Bose (2017) finds that households in the early implementer districts had 6.5% to 10% higher monthly per capita consumption within a year of program implementation. Imbert and Papp (2015) show that employment and wages in the private sector increased after the program was introduced, especially during the agricultural off-season, although Zimmermann (2012) finds that private sector wages increased only for women and the effects for women were concentrated in the agricultural season. Deininger et al. (2016) further show that the program had a positive impact on agricultural wages, non-farm employment and on-farm self-employment. Again, there is some evidence that the wage and employment effects were larger for women (Azam 2012). Using a longer time frame, Berg et al. (2018) find that, on average, NREGS introduction was associated with a wage increase of 4.3% per year, and the effects did not diminish over time.

## 3. Conceptual framework

The effect of NREGS on child marriage is a priori ambiguous. On the one hand, previous work has shown that increasing economic opportunities for women delays marriage. For example, in Bangladesh, Heath and Mobarak (2015) study the effects of growth in the garment sector and find that girls who live closer to garment factories and thus have better access to garment employment delay marriage and childbirth compared to girls who live further away.

Similarly, Jensen (2012) finds that increasing (awareness of and access to) women's employment opportunities in India reduces marriage and child bearing of young women. Sivasankaran (2014) also finds that working longer (because of a fixed-term contract) is associated with delayed marriage for young Indian women. Yet, these studies examine programs of long duration that are specifically targeted at young women and where employment is provided outside the village. Under NREGS, the whole household is provided with 100 days of local low-skilled work. This suggests that unlike other employment programs, NREGS may have limited impact on the employment (and thus empowerment) of young (unmarried) women.[2]

Even if NREGS does not increase employment of young women significantly, it could increase their education as parents are able to better afford schooling expenses, or the opportunity cost of child labor. Various studies have found that increasing women's education is associated with better marriage and fertility outcomes.[3] On the other hand, NREGS could also lead to lower schooling if children begin work earlier to substitute for parental labor (market or domestic) as parents shift to workfare employment.[4] Research on the effect of NREGS on child schooling has generally shown negative or no effect of the program. For example, Shah and Steinberg (2015) show that each year of exposure to NREGS decreases school enrollment by two percentage points for children aged 13-16, while Li and Sekhri (2013) find significant negative effect of NREGS introduction on enrollment of primary-school aged children. Das and Singh (2014) and Adukia (2018) find no significant effect of the program on schooling although the

[2] In households with an NREGS job card in the India Human Development Survey of 2011-2012 (https://www.ihds.umd.edu/IHDS-II), about 38% of female household members report having worked under NREGS (compared to 47% of men). Yet, among 15 to 25 year old unmarried women, only 4.5% have worked under NREGS (authors' calculations).

[3] For example, Breierova and Duflo (2004) find that increasing women's education in Indonesia through building more schools also increases age at marriage. Similarly, providing girls with school stipends in Bangladesh increases education as well as age at marriage (Hahn et al. 2015).

[4] Indeed, Sivasankaran (2014) finds that when NREGA work is introduced to a district, children ages 15 to 17 experience an 18 percent increase in time spent working outside the household.

signs of the effects are generally consistently negative. This suggests that while NREGS could potentially have affected the timing of marriage by decreasing education, the effects of the education channel would appear in practice to be muted.

Finally, studies have shown that Indian parents may delay marriage of their girls when they are resource constrained because they struggle to pay dowries, and hasten marriage when resources are available. For example, Corno et al. (2017) study the effect of droughts on marriage in India and find that negative income shocks reduce child marriage in India while Saha (2017) finds that bank expansion in India was associated with increase in marriage for girls as households have improved access to credit. Dowries in India are largely universal (Chiplunkar and Weaver 2017). Anukriti et al. (2018) estimate that the average expected dowry in 2007 was about 14% of annual household income and financed largely through savings and increased parental labor supply. This suggests that the additional income from the program may allow parents to pay dowries and marry their daughters earlier in a setting where early marriages are socially acceptable and often preferable. Indeed, there is some evidence that NREGS could have affected dowry payments. Amaral et al. (2015) find that NREGS is associated with a decrease in dowry deaths.[5] Dowry-related violence is a way for husbands to extract more resources from the girl's parents. A decrease in dowry deaths after the introduction of NREGS is consistent with parents being able to pay the promised dowries because of the additional income from the program.

## 4. Data

The dataset used for this analysis is the District Level Household & Facility Survey (DLHS). It was collected by the International Institute for Population Sciences (IIPS) Mumbai for

[5] This is consistent with research by Sekhri and Storeygard (2014) which shows that droughts (negative income shocks) increase dowry deaths.

the study of maternal and child health. The repeated cross-sectional data contain information on the age, gender, education and marital status for every household member of the surveyed households. For ever-married women between the ages of 15 and 49, the survey also contains detailed information on women's marriages and fertility. Our main outcome of interest is child marriage, defined as marriage under the age of 18, although we also provide results for alternative definitions using marriage under the age of 15, 16, and 17.[6]

The first round of the survey took place in 1998-1999, followed by a second round in 2002-2004. The third round was administered largely in 2008, while the fourth round took place in 2013 and 2014. Our main sample of analysis includes data from DLHS rounds 2 and 4 of both married and unmarried rural women who are between the ages of 18 and 32 in the respective wave (more on sample construction in the methodology section below). We have data from 222 districts, common to both survey waves. We further restrict the data to rural districts which received NREGS and that leaves us with 209 districts from 19 states across India.[7]

## 5. Methodology

### *5.1 Basic specification*

We use data from 2002-2004, before the program was implemented, and data from 2013-2014, after the program had been made available to all rural districts. We compare changes in outcomes over time for cohorts that could have been affected by the program and cohorts who were too old to be affected by the program. In other words, we estimate a difference-in-differences model that uses age and time as the two sources of variation. In our main

[6] Age of marriage in the survey is defined as the age when the woman begins living with her husband, which is usually the time when the marriage ceremonies are held and dowries are paid.

[7] States include Adaman and Nicobar, Andhra Pradesh, Arunachal Pradesh, Daman and Diu, Goa, Haryana, Himachal Pradesh, Kerala, Maharashtra, Manipur, Meghalaya, Mizoram, Puducherry, Punjab, Sikkim, Tamil Nadu, Telangana, Tripura, and West Bengal. In the 2011 Census, there were 640 districts across 35 states. The 209 districts included in our sample are representative of 65% of the Indian population.

specification, we restrict the data in both survey waves only to women who are 18 or older, so that they are past the age of child marriage. We define the cohort of women who were younger than 18 in 2006, at the time of first implementation of the program, to be the affected cohort.[8] This corresponds to women who are 24 or younger during the DLHS-4 survey. Thus, we have seven cohorts of affected girls in DLHS-4 – those between 18 and 24. Then, similarly, we use seven cohorts of older, unaffected, girls in DLHS-4 – those aged between 26 and 32. We exclude girls aged 25 because there is overlap in affected and unaffected girls at this age. Overall, our sample includes women between the ages of 18 and 32.[9] All women interviewed in DLHS-2, before the program started, are considered unaffected by the program.

Table 1 presents summary statistics for our analysis sample by cohort and survey round. The table shows that there are strong cohort effects – about 25% of all girls aged 18 to 24 in the baseline year reported being married before 18, while 39% of girls aged 26 to 32 had a child marriage at baseline. About ten years later, at endline, child marriages are seen to be on the decline – 15% of girls in the younger cohort and 26% of the older cohort report a child marriage. While both cohorts experienced a decrease in the probability of child marriage over time, the decrease is about 3 percentage points smaller for the younger cohort (significant at the 5% level). In other words, the decreasing trend in child marriage appears to slow down, or, child marriage rates are higher than what they would have been if the earlier trends had continued to hold.

[8] This specification doesn't take into account differences in the timing of the program rollout across districts. This could potentially lead to downward bias as all districts are said to be "treated" in 2006. Below we present another specification where we use program intensity in a triple difference model, which accounts to some extent for differences in program implementation. The hazard model we discuss below also uses actual date of program implementation based on the first year with non-zero program person-days in the district.

[9] We re-define our sample and affected/non-affected cohorts when later examining different age cutoffs for child marriage. For example, for child marriage before 15, the affected cohort is girls between 15 and 21, while the unaffected cohort is girls between 23 and 29.

Using a regression framework, we quantify the change in the trend by estimating the following model for girl $i$ living in rural district $j$:

$$M_{ijt} = \alpha + \beta_1 YC_{ijt} * Time_t + \beta_2 YC_{ijt} + \beta_3 Time_t + \eta_j + \eta_a + \epsilon_{ijt} \quad (1)$$

where $YC_{ijt}$ is an indicator for girls who are in the young cohort, $Time_t$ is an indicator that equals 1 in DLHS-4, and $\eta_j$ is a district fixed effect. The district fixed effects control for time-invariant district characteristics, such as cultural norms and demographic composition, which may be correlated with the program as well as with early marriage. Controls for cohorts and age fixed effects, $\eta_a$, additionally account for differences in the probability of early marriage by birth cohort, while time fixed effects control for common time trends. The coefficient of interest which identifies the effect of the changing trend in child marriage is the coefficient $\beta_1$ associated with the interaction between the young cohort and time (i.e., the coefficient for the affected cohort). The outcome, $M_{ijt}$, is an indicator variable, defined as 1 for women who were married before age 18 and 0 for women who were unmarried at the time of the survey or were married at age 18 or later.

This difference-in-differences specification uses differences between pre and post period for the older, unaffected, cohorts to serve as a counterfactual for the difference between pre and post period for the younger cohort, thus controlling for the pre-existing trend in child marriage rates. To test our identification strategy, we perform several checks. In Appendix Table 1, we use the following regression specification:

$$M_{ijt} = \alpha + \sum_k \gamma_{1k} Age_{kijt} * Time_t + \gamma_3 Time_t + \eta_j + \eta_a + u_{ijt} \quad (2)$$

and estimate the treatment effect for each age cohort, $\gamma_{1k}$, where $k = 18, \ldots 24, 27, \ldots 32$. We see that the effects are significant for the treatment cohorts of girls aged 18 to 24 (the omitted category is girls aged 26 – the first cohort which is part of the comparison group). The effects are

also decreasing in age, consistent with 18 year olds in DLHS-4 (who were 11 at program implementation in 2006) being exposed to the program before their 18th birthday for longer compared to 24 year olds (who were 17 in 2006). As expected, the effects for the comparison group are generally non-significant. Further, in Appendix Table 2, we perform a couple of placebo tests. In column 1, we use the comparison group of girls from our main model (girls aged 26 to 32) as a fake treatment group and a cohort of girls aged 33 to 39 as the new comparison group. We find a small and statistically insignificant difference in changes in child marriage probability over time between the two groups (estimate for the interaction term is -0.0076 with a standard error of 0.0056). Column 2 assigns the year 2000 as the fake treatment year (thus defining the pseudo treated group as women ages 25 to 30 and the comparison group as women ages 32 to 37). Again, we find no significant changes in trends at that point in time. These analyses provide support for our choice of empirical strategy to identify changing trends after 2006.

*5.2 Augmented specification with program intensity*

One threat to identification is the existence of any national policies or events occurring after 2006 that could be confounding factors in our estimate of the program effect. For example, the Prohibition of Child Marriage Act (PCMA) was issued in 2006 and set to take effect in November of 2007, thus coinciding with the timing of NREGS. The Act stipulated that child marriages are voidable and punishable by fine and imprisonment. Child marriages, however, were not declared illegal and a child had to petition to void her marriage. The Act is widely seen as having been ineffective (HAQ 2010).[10] Even so, with strong preference for early marriages

[10] Recently, India's Supreme Court considered a case on the Act's lack of implementation (Gupta 2018a) and a new law has been proposed to address the Act's shortcomings, requiring all marriages to be registered and making all child marriages void (Gupta 2018b).

because of social norms or dowries, parents may have hurried to marry their underage daughters if they expected the law to be effectively implemented soon after 2006. This could explain the increase in child marriages after 2006. If that was the case, however, we would expect to see the largest increases in child marriages for girls who were just under marriageable age in 2006, and smaller increases (or even decreases) for younger girls either because expectations adapt over time or because the law is put into practice. Instead, in Appendix Table 1, we see that the biggest effects are for the youngest girls – those aged 18, 19 and 20 in round 4 (or, 11, 12 and 13 in 2006). In addition, we find consistent results using different age cutoffs for child marriage (marriage under 15, 16, and 17 years). The results are also robust to estimating a continuous effect (rather than the effect of changing the probability of a discrete outcome) in the duration model (more details below).

Nevertheless, to address the potential concern of the PCMA (or any other national policies or events) significantly affecting child marriage trends after 2006, we directly estimate the effect of NREGS program intensity by estimating an augmented version of equation 1:

$$M_{ijt} = \alpha + \delta_1 Intensity_j * YC_{ijt} * Time_t + \delta_2 YC_{ijt} * Time_t + \\ + \delta_3 YC_{ijt} + \delta_4 Time_t + \eta_j + \eta_a + \epsilon_{ijt} , \quad (3)$$

where the coefficient $\delta_1$ measures the change in the time trend in areas with higher program intensity vs areas with lower program intensity.[11] As measures of intensity, we use the total

[11] One concern with this analysis is that intensity is measured in the current district and not the women's district of birth. Yet, while most Indian women are likely to migrate for marriage, the average travel time to their home village is about 3 hours with few moving more than 5 hours away (Fulford 2015). Thus, for most women, the district of birth is likely to be the same as the current district.

number of district-level person-days per 100,000 people in 2012, the last year before the endline survey, as well as the number of female person-days per 100,000 females in 2012.[12,13]

*5.3 Alternative specification using a duration model*

We test the robustness of our empirical strategy using a Cox Proportional Hazards regression model. The advantage of this model is that it estimates the continuous effect of change in program availability on age of marriage as opposed to the discrete jump in child marriage probability at age 18. It also allows us to more precisely assign different treatments to different years.

We create a panel dataset for each year of the woman's life between 10 (the earliest age of marriage observed in the data) and 17.[14] The duration model is estimated using this person-year data for each woman $i$ where the dependent variable, $M_{i,j,t}$, is a binary indicator for whether the woman is married in a given year and the duration is the time between age 10, when they are first at risk of getting married, $t_0$, and the age when they get married or age 17 if they are currently unmarried or get married when they are older than 17, $t_m$. Thus, each woman contributes $t_m - t_0 + 1$ number of observations to the data. The model controls for district fixed effects, $\eta_j$, age fixed effects, $\eta_a$, and interview year fixed effects, $\eta_t$, and estimates the change in probability of marriage associated with program availability (or program intensity):

$$M_{i,j,t} = \beta Availability_{t,j} + \eta_j + \eta_a + \eta_t + \epsilon_{i,j,t} \ , \qquad (4)$$

[12] Data on NREGS intensity is from DMU reports available at <http://mnregaweb4.nic.in>. Data on sex ratios in the district is from <https://www.census2011.co.in/district.php>. Data on rural population in the district is from <https://data.gov.in/catalog/normal-households-household-size-india-and-states>.
[13] The results are similar using person-days in 2008, the first year in which all rural districts in our sample had access to the program, or average number of person-days between 2008 and 2012 as our intensity measures.
[14] In other words, the data is left-censored at age 10 and right-censored at age 17.

where $Availability$ (*Intensity*) varies across time and across districts. Program availability in year *t* takes the value of 0 for panel years before the program was implemented and the value of 1 for all years after the program was implemented in the specific district. Date of actual implementation is defined based on the time when the district first had non-zero person-days of program use. The main coefficient of interest, $\beta$, shows the effect of program availability (intensity) on the log of survival time (i.e. age of marriage).

Standard errors in all models are clustered at the district level.

## 6. Results and discussion

### *6.1 Changes in child marriage trends*

Table 2 presents the results from estimating equation 1. In column 1, we show that the younger cohort has a 2.66 percentage point higher probability of child marriage relative to its counterfactual. Given a baseline rate of 25% of women in the young cohort reporting child marriages, our analysis suggests that child marriage rates increased by 10.64% after 2006. While marriage before 18 is our preferred definition of child marriage given the legal age of marriage is 18, in Table 2 we also examine the sensitivity of the results to using different definitions of child marriage. We see similarly strong positive effects on child marriage for marriage before 15, 16, and 17.[15]

### *6.2 The effect of program intensity*

Next, we examine the effect of program intensity on the probability of marriage before 18. The results, presented in Table 3, show that in all specifications, the coefficient associated with the interaction between young cohort and time is small and statistically insignificant, suggesting

[15] We see no effect on marriage before 19 or 20 and a decrease in the probability of marriage between 18 and 24, consistent with substitution from marriage after 18 to marriage before 18.

that in districts where program intensity is zero, there are no differences in the change in child marriage probability over time between the young and old cohorts. As program intensity increases, however, the difference between the two cohorts increases so that higher intensity is associated with higher child marriage probability. In column 1, an increase of one person-day per 100,000 people is associated with a 0.89 percentage points higher probability of child marriage, or 2.9 (1.6) percentage points at the mean (median) number of total person-days of 3.3 (1.8). This translates to 11.6% (6.4%) higher probability relative to the baseline rate. The estimated effects are very similar to the effects using the base specification in column 1 of Table 2, suggesting limited possibility for confounding events occurring around the time of NREGS implementation. The effects using number of female person-days per 100,000 females in column 2 are slightly lower (2.0 percentage points at the mean and 1.1 percentage points at the median) but are still statistically significant.

Interestingly, controlling for both total person-days and female person-days in column 3, we find that while total person-days increase child marriage, female person-days, holding total person-days constant, have a negative effect. This suggests that in areas where women are better able to take advantage of the increase in economic opportunities, some of the deleterious effect of the program on child marriages is mitigated.

*6.3 Robustness analysis using a duration model*

In Table 4 and Appendix Table 3, we further examine the robustness of the results using a hazard model. In Table 4, we show that program availability increases the risk of child marriage. Girls living in districts where the program was available for at least one year have 1.0687 times the hazard that girls living in districts without program availability have of child marriage. In other words, program availability is associated with 6.87% higher risk of child marriage. In addition, the longer the program is available, the stronger its effect, which is consistent with NREGS allowing parents to accumulate savings and pay dowries. In Appendix Table 3, we further show results

using intensity of program use at the district level. Both the size and direction of the estimates from the hazard model are consistent with the findings in Table 3: an additional person-day in the previous year increases the risk of child marriage by 3.88%, or 12.80% (6.21%) at the mean (median) number of person-days. Overall, this analysis confirms that program intensity is associated with higher risk of child marriage. As shown above, higher female use has a mitigating effect but given that young unmarried women are unlikely to participate in the program, there is limited possibility for a direct effect of the program on child marriage through female employment. Instead, this effect is likely explained by the fact that districts with higher female labor force participation may be less conservative and also have cultural norms that make child marriage less desirable. Below we further present analyses for heterogeneity in the treatment effect by district characteristics.

*6.4 Heterogeneity in the treatment effect*

First, we present heterogeneity analysis by the prevalence of dowry deaths at the district level. While there is lack of recent information on prevalence or size of dowries at the district level, dowry deaths (totaling about 7,000 across all of India in 2001) could partly reflect the prevalence of dowry customs (Banerjee et al. 2013). Thus, we use available data on number of dowry deaths in 2001, divided by the total female population in the district.[16] We calculate the 75$^{th}$ percentile for the incidence of dowry deaths (about 1 per 100,000 women) and define the district as being in the top quartile of dowry deaths if its dowry deaths are equal to or greater than the value for the 75$^{th}$ percentile.

Columns 1 and 2 in Table 5 present the results by district categories for the 204 districts in our sample with available crime information. Overall, we find that while higher intensity of the

[16] Crime data is available at: https://data.gov.in/catalog/district-wise-crimes-under-various-sections-indian-penal-code-ipc-crimes. Population data is from the 2001 Indian Census.

program is associated with a significant increase in child marriages across all districts, the effect in districts with higher number of dowry deaths at baseline is about 2.5 times greater compared to the effect in districts with lower number of dowry deaths and this difference is statistically significant (the F-test comparing the two coefficients yields a p-value of 0.0016). If dowry deaths are a good proxy for dowry prevalence, then these results support the hypothesis that one mechanism through which NREGS may increase child marriage is through providing additional resources for dowry payments.

We find similar results when using a measure of prevalence of arranged marriages in the district. Vogl (2013) shows that female children in India are married by birth order, increasing the risk of child marriage for the oldest girls. He further finds that the effects of birth order are especially strong in areas with higher prevalence of arranged marriages. Thus, we use prevalence of arranged marriages as a proxy for conservative social norms and preference for child marriage. We use data from the first wave of the India Human Development Survey (IHDS) (2004-2005) on women's marital history and calculate the proportion of ever married women in the district who report having chosen their husband or having participated in the decision.[17] For the 102 districts in our sample that we are able to match to the districts in IHDS, we calculate the 25$^{th}$ percentile for the proportion of women having participated in their marriage decisions (about 16 percent). We define the district as being in the bottom quartile of marriage decisions if its participation rates are equal to or less than the value for the 25$^{th}$ percentile. Again, we find that the effect of NREGS in conservative districts that are more likely to engage in traditional arranged marriages (which

[17] IHDS data are available at: https://www.ihds.umd.edu/IHDS-I .

pay dowries) is stronger (about 3.5 times higher compared to the effect in less conservative districts).[18]

## 6 Conclusion

Child marriage has significant negative consequences for child brides as well as for their children. Yet, it is prevalent and socially acceptable across many countries, including in India, where as recently as 2016, a quarter of women between 20 and 24 were married before age 18. In this paper, we examine trends in child marriage after the introduction of the National Rural Employment Guarantee Scheme, the largest workfare program in the world. We use time and age variation, as well as variation in district program intensity, and show that NREGS implementation is associated with higher probability of child marriage. One possible explanation for these findings is that parents use the additional income from the program to pay dowries and marry their daughters early. This is supported by heterogeneity analysis that finds that the effects of the program are largest in traditionally conservative districts with high prevalence of arranged marriages and dowry deaths. Overall, the results show that while the program was aimed at improving rural livelihoods and labor market opportunities, it may also have had some negative spillover effects on child marriage. Our research also has broader implications for understanding the context-specific effects of economic growth or other income-generating policies where cultural norms may interact with economic growth to produce unintended consequences.

[18] The p-value for the difference in the two coefficients is 0.11 which may be due to the smaller number of districts in each category, accounting for larger standard errors.

## References


Adukia A (2018) Spillover Impacts on Education from Employment Guarantees

Amaral S, Bandyopadhyay S, Sensarma R (2015) Employment programmes for the poor and female empowerment: The effect of NREGS on gender-based violence in India. J Interdiscip Econ 27:199–218. doi: 10.1177/0260107915582295

Anukriti S, Kwon S, Prakash N (2018) Dowry: Household Responses to Expected Marriage Payments

Azam M (2012) The Impact of Indian Job Guarantee Scheme on Labor Market Outcomes : Evidence from a Natural Experiment

Banerjee BA, Duflo E, Ghatak M, Lafortune J (2013) Marry for What? Caste and Mate Selection in Modern India. Am Econ J Microeconomics 5:33–72

Berg E, Bhattacharyya S, Rajasekhar D, Manjula R (2018) Can public works increase equilibrium wages ? Evidence from India' s National Rural Employment Guarantee. World Dev 103:239–254. doi: 10.1016/j.worlddev.2017.10.027

Bose N (2017) Raising Consumption Through India's National Rural Employment Guarantee Scheme. World Dev 96:245–263. doi: 10.1016/j.worlddev.2017.03.010

Breierova L, Duflo E (2004) The Impact of Education on Fertility and Child Mortality : Do Fathers Really Matter Less Than Mothers ?

Chari A V., Maertens A, Srinivasan S (2017a) Does Rising Inequality Delay Marriage? Evidence? In: Ray AM and T (ed) Markets, Governance, and Institutions in the Process of Economic Development

Chari A V, Heath R, Maertens A, Fatima F (2017b) The causal effect of maternal age at marriage on child wellbeing: Evidence from India. J Dev Econ 127:42–55. doi: 10.1016/j.jdeveco.2017.02.002

Chiplunkar G, Weaver J (2017) Marriage Markets and the Rise of Dowry in India

Chowdhury AR (2010) Money and Marriage : The Practice of Dowry and Brideprice in Rural India

Corno L, Hildebrandt N, Voena A (2017) Age of Marriage , Weather Shocks , and the Direction of Marriage Payments

Das S, Singh A (2014) The Impact of Temporary Work Guarantee Programs on Children ' s Education : Evidence from the Mahatma Gandhi National Rural Employment Guarantee Act from India

Deininger K, Nagarajan HK, Singh SK (2016) Short-Term Effects of India's Employment Guarantee Program on Labor Markets and Agricultural Productivity

Field E, Ambrus A (2008) Early Marriage, Age of Menarche, and Female Schooling Attainment in Bangladesh. J Polit Econ 116:881–930. doi: 10.1086/593333

Fulford SL (2015) Marriage migration in India : Vast , varied , and misunderstood

Gupta D (2018a) Plea on non-implementation of anti-child marriage law ; Supreme Court seeks Centre' s reply. In: New Indian Express. http://www.newindianexpress.com/nation/2018/apr/13/plea-on-non-implementation-of-anti-child-marriage-law-supreme-court-seeks-centres-reply-1801154.html

Gupta M Das (2018b) From ' voidable ', child marriage to turn illegal. In: Hindustan Times. https://www.hindustantimes.com/india-news/from-voidable-child-marriage-to-turn-illegal/story-UrZ5S1hOvE9c5fq2O7CnZL.html

Hahn Y, Islam A, Nuzhat K, et al (2015) Education, Marriage and Fertility: Long-Term Evidence from a Female Stipend Program in Bangladesh

HAQ (2010) Child Marriage in India : Achievements , Gaps and Challenges

Heath R, Mobarak M (2015) Manufacturing growth and the lives of Bangladeshi women. J Dev

Econ 115:1–15. doi: 10.1016/j.jdeveco.2015.01.006

Imbert C, Papp J (2015) Labor Market Effects of Social Programs: Evidence from India's Employment Guarantee. Am Econ J Appl Econ 7:233–263. doi: 10.1257/app.20130401

Jensen R (2012) Do Labor Market Opportunities Affect Young Women's Work and Family Decisions? Experimental Evidence from India. Q J Econ 127:753–792. doi: 10.1093/qje/qjs002

Jensen R, Thornton R (2003) Early Female Marriage in the Developing World. Gend Dev 11:9–19

Li T, Sekhri S (2013) The Unintended Consequences of Employment Based Safety Net Programs

Murgai R, Ravallion M, van de Walle D (2016) Is work fare cost-effective against poverty in a poor labor-surplus economy? World Bank Econ Rev 30:413–445. doi: 10.1093/wber/lhv038

Narayanan S, Das U, Liu Y, Barrett C (2016) The " Discouraged Worker Effect " in Public Works Programs: Evidence from the MGNREGA in India

Sekhri S, Storeygard A (2014) Dowry deaths: Response to weather variability in India. J Dev Econ 111:212–223. doi: 10.1016/j.jdeveco.2014.09.001

Shah M, Steinberg BM (2015) Workfare and Human Capital Investment: Evidence from India. NBER Work Pap. doi: 10.3386/w21543

Sivasankaran A (2014) Essays on Gender, Intra-Household Allocation and Development

Subbarao K, del Ninno C, Andrews C, Rodriquez-Alas C (2012) Public works as a safety net: Design, evidence, and implementation. World Bank Publications.

Vogl TS (2013) Marriage Institutions and Sibling Competition: Evidence from South Asia. Q J Econ 128:1017–1072. doi: 10.1093/qje/qjt011.Advance

WDI (2006) World Development Indicators. https://databank.worldbank.org/data/source/world-

development-indicators

Zimmermann L (2012) Labor market impacts of a large-scale public works program: evidence from the Indian Employment Guarantee Scheme

Tables of Results

Table 1: Descriptive statistics for full sample of women between the ages of 18 to 32

| | Young Cohort | | | Older Cohort | | |
|---|---|---|---|---|---|---|
| | Round 2 [1] | Round 4 [2] | p-value [1] vs [2] | Round 2 [3] | Round 4 [4] | p-value [3] vs [4] |
| Age | 20.7 | 20.85 | 0.000 | 28.92 | 28.92 | 0.931 |
| Married before 18 years of age | 0.25 | 0.15 | 0.000 | 0.39 | 0.26 | 0.000 |
| Observations | 45,247 | 45,175 | | 33,708 | 37,904 | |
| Districts | 209 | 209 | | 209 | 209 | |

Notes:

[1] Young cohort is defined as the cohort of girls ages 18 to 24 in the respective survey round. Older cohort includes girls ages 26 to 32.

[2] Round 2 refers to the second round of the District Level Household Survey, which took place in 2002-2004. Round 4 took place in 2013-2014. The survey is a repeated cross-sectional survey. We restrict the sample to rural districts available in both survey waves.

Table 2: Analysis for different age cutoffs

| | Marriage before 18 | Marriage before 15 | Marriage before 16 | Marriage before 17 |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Young Cohort*Time | 0.0266** | 0.0268** | 0.0339** | 0.0351** |
| | (0.0064) | (0.0036) | (0.0050) | (0.0058) |
| Young Cohort | -0.1135** | -0.0807** | -0.0962** | -0.1132** |
| | (0.0071) | (0.0065) | (0.0062) | (0.0076) |
| Time | -0.1853** | -0.0518** | -0.0957** | -0.1440** |
| | (0.0228) | (0.0091) | (0.0135) | (0.0179) |
| Number of observations | 162,034 | 174,023 | 176,429 | 169,148 |
| Number of districts | 209 | 209 | 209 | 209 |
| Survey Year FE | Yes | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes | Yes |

Notes:
[1] Standard errors, clustered at the district level, in parentheses.
[2] * denotes 10% significance level; ** denotes 5% significance level.

Table 3: Intensity analysis for child marriage

| | Child Marriage | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Young Cohort*Time*Total Person-days | 0.0089** | | 0.0143** |
| | (0.0016) | | (0.0021) |
| Young Cohort*Time*Female Person-days | | 0.0060** | -0.0064** |
| | | (0.0019) | (0.0026) |
| Young Cohort*Time | 0.0007 | 0.009 | 0.0037 |
| | (0.0087) | (0.0088) | (0.0087) |
| Young Cohort | -0.1139** | -0.1137** | -0.1140** |
| | (0.0070) | (0.0070) | (0.0070) |
| Time | -0.1897** | -0.1916** | -0.1858** |
| | (0.0232) | (0.0233) | (0.0230) |
| Number of observations | 162,034 | 162,034 | 162034 |
| Number of districts | 209 | 209 | 209 |
| Survey Year FE | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes |

Notes:
[1] Standard errors, clustered at the district level, in parentheses.
[2] * denotes 10% significance level; ** denotes 5% significance level.
[3] Total (female) person-days defined as total (female) number of district-level person-days per 100,000 people (women) in 2012. Data on NREGS intensity is from DMU reports available at <http://mnregaweb4.nic.in>. Data on sex ratios in the district is from <https://www.census2011.co.in/district.php>.
Data on rural population in the district is from <https://data.gov.in/catalog/normal-households-household-size-india-and-states>.

Table 4: Hazard model analysis for child marriage

| | Child Marriage | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Availability_t$ | 1.0413 | | | |
| | [1.2941] | | | |
| $Availability_{t-1}$ | | 1.0697** | | |
| | | [2.1064] | | |
| $Availability_{t-2}$ | | | 1.0955** | |
| | | | [2.5495] | |
| $Availability_{t-3}$ | | | | 1.1987** |
| | | | | [4.4921] |
| Number of individuals | 162,034 | 162,034 | 162,034 | 162,034 |
| Number of observations | 1,073,729 | 1,073,729 | 1,073,729 | 1,073,729 |
| Number of districts | 209 | 209 | 209 | 209 |
| Survey Year FE | Yes | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes | Yes |

Notes:

[1] Hazard ratios estimated using cox proportional hazards regression model. Z-statistics in parentheses. Model uses standard errors clustered at the district level.
[2] * denotes 10% significance level; ** denotes 5% significance level.
[3] Program availability in the district is defined based on the time when the district first had non-zero person-days of program use.

Table 5: Heterogeneity analysis by district characteristics

| | Child Marriage | | | |
|---|---|---|---|---|
| | Top quartile of dowry deaths | Not in top quartile of dowry deaths | Bottom quartile for having say in marriage | Not in bottom quartile for having say in marriage |
| | (1) | (2) | (3) | (4) |
| Young Cohort*Time*Total Person-days | 0.0149** | 0.0060** | 0.0253** | 0.0071 |
| | (0.0021) | (0.0018) | (0.0106) | (0.0045) |
| Young Cohort*Time | 0.0093 | 0.0085 | 0.0043 | 0.0104 |
| | (0.0123) | (0.0112) | (0.0203) | (0.0138) |
| Young Cohort | -0.1587** | -0.1006** | -0.1219** | -0.1129** |
| | (0.0143) | (0.0080) | (0.0179) | (0.0112) |
| Time | -0.4193** | -0.1800** | -0.1590** | -0.2059** |
| | (0.0265) | (0.0247) | (0.0341) | (0.0488) |
| Number of observations | 42,808 | 115,535 | 15,006 | 59,970 |
| Number of districts | 51 | 153 | 20 | 82 |
| Survey Year FE | Yes | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes | Yes |

Notes:

[1] Standard errors, clustered at the district level, in parentheses.

[2] * denotes 10% significance level; ** denotes 5% significance level.

[3] Districts in top quartile of dowry deaths are determined based on crime data for 2001 available at: <https://data.gov.in/catalog/district-wise-crimes-under-various-sections-indian-penal-code-ipc-crimes>. Number of dowry deaths is divided by total female population in the district using population data from the Indian census in 2001.

[4] Districts in bottom quartile for having say in marriage are defined based on the proportion of ever married women in the district who report having chosen their husband or having participated in the decision. The proportion is calculated using data from the first wave of the India Human Development Survey: <https://www.ihds.umd.edu/IHDS-I> of 2004-2005.

Appendix Table 1: Results by age cohort

| | Child Marriage |
|---|---|
| Age 18*Time | 0.0604** |
| | (0.0117) |
| Age 19*Time | 0.0327** |
| | (0.0113) |
| Age 20*Time | 0.0426** |
| | (0.0105) |
| Age 21*Time | 0.017 |
| | (0.0111) |
| Age 22*Time | 0.0311** |
| | (0.0109) |
| Age 23*Time | 0.0031 |
| | (0.0112) |
| Age 24*Time | 0.0273** |
| | (0.0102) |
| Age 27*Time | 0.0114 |
| | (0.0111) |
| Age 28*Time | 0.0119 |
| | (0.0108) |
| Age 29*Time | -0.0126 |
| | (0.0131) |
| Age 30*Time | 0.0220* |
| | (0.0116) |
| Age 31*Time | -0.0293** |
| | (0.0118) |
| Age 32*Time | 0.0156 |
| | (0.0119) |
| Number of observations | 162,034 |
| Number of districts | 209 |
| Age FE | Yes |
| District FE | Yes |

Notes:
[1] Standard errors, clustered at the district level, in parentheses.
[2] * denotes 10% significance level; ** denotes 5% significance level.

Appendix Table 2: Placebo Tests

| | Placebo Test 1 | Placebo Test 2 |
|---|---|---|
| | [1] | [2] |
| Young Cohort*Time | -0.0076 | -0.004 |
| | (0.0056) | (0.0060) |
| Young Cohort | -0.0233** | -0.0257** |
| | (0.0081) | (0.0086) |
| Time | -0.2033** | -0.1998** |
| | (0.0319) | (0.0299) |
| Number of observations | 127,322 | 121,789 |
| Number of districts | 209 | 209 |
| Age FE | Yes | Yes |
| District FE | Yes | Yes |

Notes:
[1] The pseudo treated group in Placebo Test 1 includes ages 26 to 32 (the original comparison group in the main specification), and the comparison group includes ages 33 to 39. Placebo test 2 assigns 2000 as the fake treatment year and defines the pseudo treated group as ages 25 to 30 (excluding ages 18 to 24 that are in the original treatment group), and the comparison group includes ages 32 to 37.
[2] Standard errors, clustered at the district level, in parentheses.
[3] * denotes 10% significance level; ** denotes 5% significance level.

Appendix Table 3: Intensity analysis using hazard model for child marriage

| | Child Marriage | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Total Person-days$_{t-1}$ | 1.0388** | | 1.0762** |
| | [7.0657] | | [7.6999] |
| Female Person-days$_{t-1}$ | | 1.0300** | 0.9581** |
| | | [4.4307] | [-3.1217] |
| Number of individuals | 162,034 | 162,034 | 162,034 |
| Number of observations | 1,073,729 | 1,073,729 | 1,073,729 |
| Number of districts | 209 | 209 | 209 |
| Survey Year FE | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes |

Notes:
[1] Hazard ratios estimated using cox proportional hazards regression model. Z-statistics in parentheses. Model uses standard errors clustered at the district level.
[2] * denotes 10% significance level; ** denotes 5% significance level.