Insuring Girls' Lives Against Drought*

Joshua D. Merfeld†

Jagori Saha‡
Job Market Paper

October 31, 2018

Abstract

This paper revisits the relationship between agricultural productivity shocks and excess female infant mortality in India and investigates how this relationship changes when households have access to employment opportunities outside of agriculture. When a household's preference for sons coincides with adverse agricultural productivity shocks, households tend to disproportionately reduce care (prenatal or postnatal) for their female children. This leads to a relatively more balanced sex-ratio in good rainfall years and a more skewed sex-ratio (in favor of boys) in bad rainfall years. We show that a rural workfare program in India, which decouples both wages and consumption from rainfall, attenuates the relationship between rain and the sex-ratio of infants. Using a back-of-the-envelope calculation, we find that the program could have saved around 550 girls per district per year if the government had implemented it in the years 2001 to 2005. Lastly, we show that the program also attenuates (a) the effect of birth-year rainfall on long-run health outcomes of the surviving girls; and (b) the effect of rainfall on older women's mortality outcomes such as dowry death.

Keywords: sex ratio, health investments, gender, dowry deaths, rain, consumption smoothing, workfare program, National Rural Employment Guarantee, India
JEL Codes: E20, H53, I15, O12

*We are grateful to Rachel M. Heath for her encouragement and guidance since ideation. We thank Chris Anderson, James D. Long, Xu Tan, and the participants at the Pacific Northwest Labor Day Workshop for helpful comments and suggestions. We also thank the University of Washington's South Asia Library for access to the National Sample Surveys. Saha thanks the University of Washington for dissertation support through the Grover and Creta Ensley Graduate Fellowship for Economic Policy. An earlier version of this working paper was titled "Missing Girls, Income Shocks, and Consumption Smoothing."
†Robert F. Wagner Graduate School of Public Service, New York University; merfeld@nyu.edu
‡Department of Economics, Rhodes College; sahaj@rhodes.edu

1 Introduction

Extensive literature shows that girls and boys are treated differently in countries where households exhibit a strong preference for sons. In extreme cases, this discrimination leads to sex-selection of children at early ages[1] through postnatal neglect or prenatal sex-selective abortions[2] (Bhalotra and Cochrane, 2010; Chen et al., 2013). In countries where families provide equal care for both daughters and sons, the sex-ratio is about 1050 females to 1000 males (Sen, 1992). One would expect the sex-ratio to improve with greater economic development over time. Figure 1A shows that, in India's case, the child sex ratio has only worsened over the last half-century – a time which India experienced rapid economic growth, with an average annual GDP growth rate of about 5 percent[3]. The continued use of sex selection in the face of sustained economic growth suggests we need a better understanding of the determinants of sex selection.

Even for fetuses carried to term, there is a gender-gap in prenatal investments: For example, women who are pregnant with a boy are more likely to visit antenatal clinics (Bharadwaj and Lakdawala, 2013). Throughout childhood, unequal human capital investments continue through differences in breastfeeding (Jayachandran and Kuziemko, 2011), food allocation (Chen et al., 1981; Das Gupta, 1987), parental time allocation (Barcellos et al., 2014), vaccination (Borooah, 2004; Ganatra and Hirve, 2001), other health-care practices (Ganatra and Hirve, 1994), and education (Song et al., 2006). In this paper, we add to this literature and show that negative income shocks in rural India continue to affect boys and girls differentially. Also, we show that a large workfare program – which may help households smooth consumption – attenuates the relationship between these

[1]Previous work has argued this as one of the leading causes of unbalanced sex ratios in South and Southeast Asian countries. In his seminal work, Sen (1990) estimated that more than 100 million women were "missing" worldwide. More recent estimates suggest that this number has been steadily increasing over time (reaching 126 million in 2010) and that India and China account for most of this deficit (Bongaarts and Guilmoto, 2015).

[2]Specifically, since the introduction of reliable ultrasound technology in the 1980s.

[3]Jayachandran (2017) finds that sex-selection of children in India is increasing with declining fertility as households still prefer to have at least one son.

negative income shocks and adverse outcomes for girls and women.

Income shocks in developing countries tend to exacerbate these gender gaps. Notably, Rose (1999) finds that in rural India, a primarily agrarian society with a strong son preference, the probability that a child born during a given year is a girl increases with rainfall in that year. In other words, when income is higher, a randomly selected newborn is more likely to be female. When son preference coincides with the lack of formal mechanisms to insure against bad agricultural shocks and the resulting fluctuations in income, households may reduce investments in their female children to help smooth consumption. This underinvestment may lead to adverse outcomes for female children and, in extreme cases, can result in excess female child mortality.

Moreover, increased female mortality in the face of adverse income shocks is not restricted only to the young. Using witch killings in Tanzania (Miguel, 2005) and dowry deaths in India (Sekhri and Storeygard, 2014), recent research shows that adult women in developing countries are also less likely to survive during bad agricultural years. Figure 1B shows that similar to the worsening sex ratio, dowry deaths are also increasing over time, suggesting similar dynamics make survival less likely for both girls and women.

Much of the literature examining female mortality and adverse income shocks rightly points out that the developing world lacks formal insurance mechanisms. These mechanisms could enable consumption-smoothing during bad times and potentially attenuate these gender-differentiated mortality effects. Therefore, a common conclusion in this body of literature is that an important policy measure to improve women's lives moving forward is the provision of consumption-smoothing mechanisms. However, despite the growing number of risk-coping programs implemented in developing countries today, there remains a gap in the literature that empirically tests whether these policies truly reduce female mortality during adverse income shocks. To our knowledge, this is the first paper that formally examines the effects of these policies on female-specific health outcomes. Specifically, in the context of infant mortality and dowry deaths in India, we provide the first evidence of how the relationship between agricultural productivity shocks and female mortality attenuates when a national workfare program enables households to smooth consumption during bad years.

In this paper, we develop an inter-temporal consumption-maximization problem in which a household decides how much to invest in their male and female children during the first period as well as how much time to spend in dowry appropriation of daughters-in-law in the second period. This model yields three main testable predictions. First, if having a girl entails a substantial future cost – which is likely in a society that typically practices dowry, such as India – then a positive agricultural shock leads to more investment in female children and, consequently, a higher likelihood of their survival. Second, a positive agricultural shock decreases the marginal product of labor hours spent in dowry appropriation relative to agricultural work. Therefore, households spend less time on appropriation behavior and, consequently, it is less likely that they abuse (in extreme cases, kill) their daughter-in-law due to dowry demands. Third, the introduction of a non-agricultural labor market with guaranteed minimum wages (to which labor can move freely during bad agricultural years) attenuates the relationship between agricultural productivity shocks and excess female mortality through consumption-smoothing.

To empirically test these predictions, we use (a) deviation of district-level rainfall from its long-run average as a proxy for agricultural productivity shocks; and (b) the interaction of (a) with the spatial and temporal variation in the roll-out of the national workfare program. There is a growing body of literature on the effects of India's workfare program, the Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS)[4], on various development outcomes such as wages (Imbert and Papp, 2015; Merfeld, 2018b), consumption (Jha et al., 2011; Ravi and Engler, 2015), risk (Foster and Gehrke, 2017; Gehrke, 2017; Fetzer, 2014; Merfeld, 2018a), and time allocation decisions (Shah and Steinberg, 2015). The most relevant to this paper is the work by Santangelo (2016), which shows that NREGS

[4]NREGS guarantees up to 100 days of wage employment at the state-level minimum wage in a financial year to every household in India whose adult members volunteer to do unskilled manual work.

attenuates the pro-cyclical response of local wages, income, and consumption to agricultural productivity shocks in rural India[5] Therefore, NREGS is an ideal risk-coping policy to study how a disruption in the positive relationship between agricultural productivity shocks and household consumption can affect excess female infant mortality and dowry deaths.

Consistent with previous literature and the model predictions, we find that rainfall continues to be a significant predictor of the gender of an infant and dowry deaths in India in the early 2000s. Before the implementation of NREGS, an increase in annual rainfall by one standard deviation (from the ten-year mean) increases the probability that an infant born during that year is a girl by two percentage points. However, in contrast to the findings of Rose (1999), who uses data from the 1970s, we do not find that concurrent rainfall is a significant determinant of the gender of older children in the early 2000s. This result is consistent with more recent findings that a large part of the sex-selection of children is at early stages since the advent of reliable ultrasound technology. The effect of rainfall on female mortality is also evident later in life: we find that an increase in annual precipitation by one standard deviation decreases dowry deaths by approximately 1.5 percent.

We then present evidence that the introduction of NREGS attenuates these relationships. A one standard deviation increase in rainfall increases the probability that a child born in a non-NREGS district is 5.4 percentage points more likely to be a girl. Following the introduction of the program, this effect is 4.6 percentage points lower for NREGS districts. This result suggests that there is almost no relationship between agricultural productivity shocks and the sex of an infant following the implementation of NREGS. Similarly, the negative relation between dowry deaths and rainfall attenuates almost entirely following the rollout of the program.

[5]Santangelo (2016) shows these results using the National Sample Surveys. We replicate the effects on consumption using the last wave of the Rural Economic and Demographic Surveys for further support.

We next examine the effect of agricultural productivity shocks at the time of birth on the long-run health of surviving children. If parents are underinvesting in girls relative to boys, we would expect to see evidence of this in non-mortality outcomes, as well. In particular, we explore the relationship between rainfall during the year of birth and child anthropometrics.[6] We first confirm that rainfall during the year of birth is a significant predictor of height-for-age for both boys and girls in India, similar to recent results from Indonesia (Maccini and Yang, 2009). Previous literature suggests that those girls who manage to survive sex-selection at birth still receive gender-biased early-life investments. This discrimination in care during the year of birth and subsequent years is likely to have long-run gender-gaps in the health of the surviving children. Consistent with this, we find that before the implementation of NREGS, an increase in annual rainfall by one standard deviation increases the height-for-age of female children by 0.06 standard deviations compared to male children. Post NREGS, this differential relationship is significantly attenuated.

While is not possible to provide direct evidence of mechanisms, we present suggestive evidence that an improved ability to smooth consumption is responsible for these findings. To add support to the National Sample Survey results in Santangelo (2016), we use the most recent Rural Economic Demographic Survey to show that NREGS attenuates the positive relationship between rainfall and consumption. Additionally, we show that NREGS does not affect household alcohol consumption, tobacco consumption, clothing expenditures for girls, or education expenditures for girls, which have been used to analyze gender-specific bargaining power within a household (see, for example, Quisumbing and Maluccio (2003)). Therefore, the overall evidence suggests that consumption smoothing is indeed the primary mechanism for the effects of NREGS on the infant sex ratio, anthropometrics, and dowry deaths.

We also show that heterogeneity by birth order is consistent with previous literature

[6]We have a single anthropometric measurement for children up to nine years of age.

on sex selection. We show that there is no effect of rainfall on the gender of first-born children, consistent with sex selection being less pronounced for these children (Bhalotra and Cochrane, 2010). As expected, there is similarly no effect of NREGS on this relationship. Rainfall is strongly associated with gender for non-first-born children, however, and NREGS appears to attenuate this relationship, though the estimates are imprecisely estimated. Moreover, further results suggest households may prefer just one girl but many boys, similar to the findings in Bhalotra and Cochrane (2010). NREGS helps attenuate the relationship between rainfall and sex selection based on gender sex composition, though it remains to be seen whether the program affects gender and fertility preferences more broadly.

We calculate the number of girls that NREGS might have saved if implemented in the years 2001 to 2005. We assume that the number of boys is unaffected by rainfall and that the number of boys and girls is equal when rain is two standard deviations above its ten-year mean: empirical evidence presented in this paper supports both assumptions. Using a back-of-the-envelope calculation, we estimate that if NREGS had been implemented in the years 2001 to 2005, approximately 550 additional girls per district per year would have survived to one year of age.

This paper primarily contributes to three strands of existing literature: First, this work fits into the research on how sex-selection is affected by changing economic conditions (Rose, 1999; Bhalotra et al., 2016; Qian, 2008). Second, the study contributes to the literature on the effectiveness of different policies for the well-being of girls and women, such as greater political participation of women (Kalsi, 2017) and financial incentives offered for having daughters (Anukriti, forthcoming; Balakrishnan, 2017). Finally, these results add the growing literature on the risk-mitigation effects of rural workfare programs and the subsequent impact on development outcomes (Fetzer, 2014; Foster and Gehrke, 2017).

2 Conceptual Framework

Building on the framework developed in Eswaran (2002), Rosenblum (2013) and Balakrishnan (2017), and the intuition described in Sekhri and Storeygard (2014), we present a simple theoretical model to demonstrate the following: In a primarily agrarian society, where girls' represent a net future cost relative to boys[7], a favorable agricultural productivity shock can increase the survival of daughters relative to sons and decrease dowry-motivated killing of daughters-in-law. Using this model, we then show that access to employment opportunities outside the agricultural sector can alleviate these effects.

2.1 Set Up

In this model, the household lives for two periods. The household's instantaneous utility is given by u, which follows $u'(.) > 0$ and $u''(.) < 0$. In the first period, the household derives utility from consumption (c_1) and chooses the health investments in their male children (k_b) and female children (k_g). The probability of a child's survival into the second period is linearly increasing in the investments made during the first period and is given by k_j (where $j = b, g$). The natural probability that a child born is a boy and the probability that a child born is a girl is the same and equal to 0.5. N is the number of children that the household has in the first period. Therefore, the surviving number of male children is $0.5Nk_b$ and female children is $0.5Nk_g$ in the second period. All surviving children get married before the second period.

In the second period, the discount factor is β. During this period, the household derives utility from consumption (c_2) and the total number of surviving children. In the second period, the household derives a net benefit ($B \times A$) from each of its alive male children and incurs a net cost (G) from each of its alive female children. The net benefit

[7]Miller (1981) argues that the practice of dowry, a financial transfer from the bride's household to the groom's household at the time of marriage, is one of the major determinants of the gender bias observed in India. Using the prices of gold, which is an important part of dowries in India, Bhalotra et al. (2016) empirically formalize this idea and show that higher gold prices leads to higher mortality of fetal and newborn girls.

from alive sons can be thought of as the dowry receipts from their spouses and the net cost of alive daughters can be thought of as dowry payments upon their marriage. B is the marginal dowry receipts for each labor hour spent in dowry appropriation from each daughter-in-law. Net benefit from each son is linearly increasing in the total labor hours spent in dowry appropriation behavior, which the household chooses in the second period, A[8]. The household has no intrinsic preferences over its male and female children. Instead, preference for sons stems from the future benefits they bring and the future costs associated with daughters.

In each period, the household has one unit of total labor hours and is engaged in the agricultural enterprise. In the first period, the household's labor supply to the agricultural sector (L_1) is inelastic and equal to one. In the second period, the household chooses how much to allocate to agricultural work (L_2) and dowry appropriation (A). The household's income from the agricultural sector (y_t) is the sum of labor income income ($w_t L_t$) and the profits from the agricultural enterprise (π_t), $y_t = \pi_t + w_t L_t$. The agricultural production function is given by $\alpha_t F(L_t)$, where α_t is the agricultural productivity parameter, $F'(.) > 0$, $F''(.) < 0$, and $F'''(.) = 0$.

Therefore, the household's optimization problem is given by:

$$\begin{aligned}
\underset{k_b, k_g, A}{\text{maximize}} \quad & U = u_1(c_1) + \beta u_2(c_2) + \beta u_c(0.5k_b N + 0.5k_g N) \\
\text{subject to} \quad & c_1 + 0.5k_b N + 0.5k_g N = \pi_1 + w_1 L_1 \\
& c_2 = \pi_2 + w_2 L_2 + 0.5k_b NBA - 0.5k_g NG \\
& \pi_t = \alpha_t F(L_t) - w_t L_t \\
& L_1 = 1 \\
& L_2 + A = 1
\end{aligned} \tag{1}$$

[8]The model can assume that the net benefit from the male children includes labor income from sons and their spouses but that will not affect the main predictions of the model.

Introducing NREGS

The Mahatma Gandhi Rural Employment Guarantee Act entitles every rural household in India to 100 days of employment in public works at the state-level minimum wage. Therefore, we assume that the introduction of NREGS introduces a non-agricultural sector in the economy where the household can supply labor hours, E_t, for a fixed wage, s. This changes the household's optimization problem in equation **??** to:

$$\begin{aligned}
\underset{E_1,E_2,k_b,k_g,A}{\text{maximize}} \quad & U = u_1(c_1) + \beta u_2(c_2) + \beta u_c(0.5k_bN + 0.5k_gN) \\
\text{subject to} \quad & c_1 + 0.5k_bN + 0.5k_gN = \pi_1 + w_1L_1 + sE_1 \\
& c_2 = \pi_2 + w_2L_2 + sE_2 + 0.5k_bNBA - 0.5k_gNG \\
& \pi_t = \alpha_t F(L_t) - w_tL_t \\
& L_1 + E_1 = 1 \\
& L_2 + E_2 + A = 1
\end{aligned} \tag{2}$$

2.2 Testable Predictions

The above model gives the following predictions that guide our empirical work. The proofs for the predictions are described in Appendix A.

Prediction 1 *A positive agricultural shock leads to more investment in female children but no change in investment for male children. Consequently, girls are more likely to survive relative to boys in response to a positive agricultural shock.*

The optimal health investment in female children is chosen such that the marginal utility from consumption in the first period is equal to net discounted marginal utility derived from the surviving female children in the second period. This is described by the Euler equation in 11. An increase in the agricultural productivity parameter increases profits from the agricultural sector, and therefore, diminishes the positive marginal utility from consumption in the first period. Following this, the household can operate at the Euler equation by increasing in the health expenditure on the female children.

Prediction 2 *A positive agricultural shock in a year leads to less time spent on dowry appropriation behavior in that year and consequently we will observe lower number of dowry-related deaths.*
As the agricultural productivity parameter increases for a period, the marginal product of labor in agriculture increases in that period. Therefore, the optimal amount of labor hours spent in agricultural work increases and the optimal amount of labor hours spent in dowry appropriation decreases.

Prediction 3 *If the household can supply labor to a non-agricultural sector (NREGS), then: (a) agricultural productivity shocks during the year of birth affects infant sex-ratio less; (b) agricultural productivity shocks during a year affects labor hours spent in dowry appropriation that year (and consequently, dowry deaths) less.*
In the model, negative agricultural productivity shocks decrease the farm profits in the first period and the marginal product of labor in agriculture in the second period. As NREGS provides guaranteed non-farm employment at the state-level minimum wage, labor can move from the agricultural sector to the non-farm sector during the bad agricultural years, keeping household income higher than it would be without a non-farm employment option. Therefore, households do not have to reduce care for their female children in the first period or increase dowry appropriation hours in the second period.

3 Empirical Strategy

3.1 Child Sex-ratio

First, we use the following specification to test **Prediction 1**; that is, to show the effect of agricultural productivity shocks on the gender of a child (Rose, 1999):

$$Girl_{idt} = \alpha + \beta RainZscore_{dt} + \delta_d District'_d + \tau_t BirthYear'_t + \epsilon_{idt} \tag{3}$$

where, $Girl_{idt}$ is an indicator variable that is equal to one if a surviving child i born in district d and year t is a girl and is equal to zero otherwise. Similar to previous studies (Rose, 1999; Kalsi, 2017), we use the sample of surviving children due to misreporting of dead children, which is systematically more for female children and biases the sample of all children. $BirthYear_t$ is a vector of year of birth dummies and captures year-specific shocks common to all districts. $District_d$ is a vector of district of birth dummies and controls for time-in-varying differences in child sex-ratio across districts. In additional specifications, we add a vector of interactions of 2001 Census variables at the district level[9] with the year of birth dummies to in part capture district-specific changes in child sex-ratio over time. In further specifications, we add controls for household characteristics.

$RainZscore_{dt}$ is our proxy for agricultural productivity shocks (Jayachandran, 2006). It is measured as the deviation of district d's rainfall in year t from its 10-year mean and scaled by its 10-year standard deviation. Therefore, in specification 3, our primary coefficient of interest is β. If **Prediction 1** is true, then β is positive. That is, a positive rainfall shock will increase the probability that a child born is a girl. Our main results restrict estimation to only rainfall in the child's year of birth, though additional analyses expand this to include rainfall in the year prior to and following birth. The identification of β relies on the assumption that, after controlling for district effects, the deviation of a

[9]These variables are described in Section 4

district's annual rainfall from its long-run mean is random and plausibly exogenous to unobserved determinants of child sex-ratio.

Apart from a linear rainfall variable, we also estimate specification 3 with different definitions of the rainfall shock to explore the possible non-linearity in the relationship between agricultural productivity shocks and child sex-ratio. These extensions include rainfall shocks defined as follows: (a) six indicator variables that caputure bins of deviation of annual rainfall from its long-run mean; (b) a single dummy that indicates good rainfall and is equal to one for rainfall above +1 SD; and (c) an ordinal rainfall variable with two cut points at -1 and +1 standard deviation, as in Jayachandran (2006). We cluster standard errors for all specifications at the district level.

Second, we use the following specification to find NREGS's effect on the relationship between agricultural productivity shocks in a year and the gender of a child born in that year:

$$\begin{aligned} Girl_{idt} =& \alpha + \beta_1 RainZscore_{dt} \times NREGS_{dt} + \beta_2 RainZscore_{dt} + \beta_3 NREGS_{dt} \\ &+ \delta_d District'_d + \tau_t BirthYear'_t + \epsilon_{idt}, \end{aligned} \tag{4}$$

where, $NREGS_{dt}$ is an indicator that is equal to one if district d in year t received the guaranteed workfare program. The National Rural Employment Guarantee Scheme was implemented in 200 districts starting in April 2006 (Phase 1), 130 districts starting in June 2007 (Phase 2), and the remaining districts received the program beginning in July 2008 (Phase 3)[10]. This enables us to exploit the temporal and spatial variation in the implementation of the program to identify the effects of NREGS.

β_1 is our primary coefficient of interest in specification 4 and identifies the relationship between rainfall shocks and child sex-ratio in districts that receive NREGS compared to districts that do not. If **Prediction 3 (a)** is true in conjunction with **Prediction 1** then β_1 is

[10]Some urban districts such as Hyderabad, Kolkata, Chennai and others never received NREGS. Therefore, 16 such 2001 Census districts are excluded from the analysis.

negative; that is, the positive effect of rainfall on the probability that a surviving child is a girl is reduced after the implementation of NREGS.

A possible threat to the identification of β_1 can stem from differential trends over time in the relationship between rainfall and child sex-ratio across the different phases of NREGS's implementation. This is particularly relevant as NREGS was implemented in the most backward districts first and in more developed districts later. This development ranking was published in a 2003 report of the Planning Commission of India and was based on each district's agricultural wages, agricultural productivity and the population of low-caste individuals (Scheduled Castes and Scheduled Tribes)[11]. To partially address this concern, we include NREGS's phase-specific linear time trends in a stricter specification of 4 and all other specifications where we are interested in NREGS's effect on excess female mortality outcomes. We also test for parallel trends in the relationship between the child sex-ratio and rainfall shocks prior to the implementation of NREGS using the sample of observations before NREGS's implementation in a district and the following specification:

$$\begin{aligned} Girl_{idt} =& \alpha + \beta_1 RainZscore_{dt} \times proxyNREGS_{dt} + \beta_2 RainZscore_{dt} + \beta_3 proxyNREGS_{dt} \\ &+ \delta_d District'_d + \tau_t BirthYear'_t + \epsilon_{idt}, \end{aligned} \tag{5}$$

where $proxyNREGS_{dt}$ is a dummy equal to one for the year prior to actual NREGS implementation and all subsequent years, and other variables are defined as in Equation 4. By assigning NREGS to districts one year prior to actual implementation, we are implicitly testing whether districts were trending in a similar way to our main results in the year prior to implementation.

[11] Zimmermann (2017) and Khanna and Zimmermann (2017) discuss the phased implementation of NREGS in detail.

3.2 Long-run Effects

Next, we investigate the effect of early-life agricultural productivity shocks on the surviving children's health by gender with the following specification:

$$
\begin{aligned}
HAZ_{idt} =& \alpha + \beta_1 Rainzscore_{dt} \times Girl_{idt} + \beta_2 Rainzscore_{dt} + \beta_3 Girl_{idt} \\
& + \delta_d District_d + \tau_t Birthyear_t + \mu_{idt},
\end{aligned} \tag{6}
$$

where, HAZ_{idt} is height for age of child i born in district d and year t and $Girl_{idt}$ is an indicator for whether this child is a girl. β_1 in specification 6 will be positive if early life investments have long-lasting effects and girls received differentially more investment compared to boys during good agricultural years, that is, **Prediction 1** is true.

To explore how the guaranteed workfare program changed the relationship between early-life agricultural productivity shocks and children's health by gender we use the following specification:

$$
\begin{aligned}
HAZ_{idt} =& \alpha + \beta_1 NREGS_{dt} \times Rainzscore_{dt} \times Girl_{idt} + \beta_2 Girl_{idt} \times Rainzscore_{dt} \\
& + \beta_3 NREGS_{dt} \times Girl_{idt} + \beta_4 Rainzscore_{dt} \times NREGS_{dt} + \beta_6 NREGS_{dt} \\
& + \beta_7 Girl_{idt} + \beta_8 Rainzscore_{dt} + \delta_d District_d + \tau_t Birthyear_t + \mu_{idt}.
\end{aligned} \tag{7}
$$

In specification 7, β_1 is our coefficient of interest and if **Prediction 3(a)** is true in conjunction with **Prediction 1** then it will be negative. That is, the positive effect of good agricultural shocks will be reduced after access to the the guaranteed workfare program outside of agriculture.

3.3 Dowry Deaths

Lastly, we look at the effect of NREGS on the number of dowry deaths using the following specification:

$$DowryDeaths_{dt} = \alpha + \beta Rainzscore_{dt} + \delta_d District_d + \tau_t Year_t + \mu_{idt}, \tag{8}$$

where, $DowryDeaths_{dt}$ is the number of dowry-related deaths in district d and year t and $Year_t$ is a vector of fixed effects for the year of the dowry-related death.

$$\begin{aligned} DowryDeaths_{dt} = &\alpha + \beta_1 NREGS_{dt} \times Rainzscore_{dt} + \beta_2 Rainzscore_{dt} + \beta_3 NREGS_{dt} \\ &+ \delta_d District_d + \tau_t Year_t + \mu_{idt} \end{aligned} \tag{9}$$

4 Data

We combine data from six different sources for this paper. First, we use the 0.5 degree by 0.5 degree grid monthly precipitation data from the University of East Anglia's Climate Research Unit (CRU) to construct agricultural productivity shocks. We aggregate the CRU data to annual precipitation and then match the district centroids to the closest grid in the CRU data to construct district-level annual rainfall. Our primary measure of rainfall shock is the deviation of annual district-level rainfall from its long-run mean (using the previous ten years) and scaled by the long-run standard deviation.

Second, the data on the gender of infants is from the National Sample Surveys (NSS). We use their nationally representative labor survey, the Employment and Unemployment rounds of the NSS, collected by the Government of India's Ministry of Statistics and Programme Implementation. We use the 2004-05, 2007-08, and 2011-12 thick waves of the NSS. This data records the age of every resident member of the interviewed households at the time of the survey. We use this to create a panel of the sex and year of birth of alive children born between 2001 and 2011. We take the data on the children born between these waves from the immediately succeeding wave. Our final sample includes 89,264 newborns, 98,980 one-year olds, and 107,764 two-year olds.

Third, the data on children's anthropometrics come from the 2011-12 wave of the India Human Development Survey (IHDS II). The IHDS II collects height and age of all children between 0 and 9 years of age of interviewed households.[12] Using this information, we construct gender-specific height-for-age measures using the Center for Disease Control's (CDC) growth charts. Our final sample is 18,141 children between 0 and 9 years of age in 2011-2012.

Fourth, we use National Crime Records Bureau of India's data on reported annual dowry deaths at the district-level to test the hypotheses on dowry appropriation behavior. A dowry death is defined as the unnatural death of a woman (including suicide) following harassment or cruelty by her husband or his relatives in connection with a demand for dowry. This data is available since 2001 and we use the data up to 2012.

Fifth, we use publicly available information on the roll-out of the NREGS at the district-level to create an indicator variable that is equal to one if a district received the program during a year and zero otherwise.

Lastly, as additional controls, we use a number of variables from the 2001 Census, including population, sex-ratio, literacy rate, percentage of scheduled caste and scheduled tribe, employment rate, and percentage of rural population. We interact these census variables with year-of-birth (or year of police report in the case of dowry deaths) dummies to allow for time-varying trends in the district characteristics.

[12]Who were present at the time of survey.

4.1 Summary Statistics

We present summary statistics for the main variables used in the rainfall analyses in Table 1. The top panel presents NSS data. Across all three NREGS phases, girls make up less than half of newborns, one-year olds, and two-year olds. Consistent with the phased rollout of NREGS – in which the poorest districts were the first to receive the program – the household head is slightly younger and has less education, on average, in phase-one districts.

The second panel presents the anthropometric measures of height-for-age using the IHDS data. Phase three districts appear to have somewhat "healthier" children, with the highest height-for-age z-scores for both girls and boys. However, it is important to note that the z-score is still well below the international standard (mean), even in phase-three districts. Interestingly, both boys and girls appear to be slightly shorter in phase-two districts than in phase-one districts, despite phase-one districts generally being poorer, on average.

The third panel presents summary statistics for the crime data used in this paper. The crime data is reported as counts per year (per district). Our analyses focus only on dowry deaths, which are highest in phase-one districts. This is consistent with the poorest families spending large proportions of their income on dowry, sometimes more than seven times their annual income (Rao, 1999), and the omnipresence of dowry demands in poor areas (Kaur, 2004). However, it does not appear that dowry deaths are simply another crime. In fact, other crimes against girls and all other crimes are increasing from phase-one to phase-three districts. Dowry deaths, on the other hand, are deceasing. This suggests there are mechanisms specific to dowry deaths that are not shared by all crimes.

Finally, Panel D presents census statistics across all three NREGS phases. Phase-one districts – the poorest districts – have the highest percentage of scheduled caste and scheduled tribes. Similarly, these districts also have the fewest literate residents, while phase-three districts have the most. Phase-one districts are also the most rural, again consistent with the phased rollout of the program. However, phase-one districts have the highest labor force participation rate and, surprisingly, the highest sex ratio (of females per 1,000 males). The dynamics of sex-selection are clearly complicated, as previous statistics may have led us to conclude that sex-selection was most acute in phase-one districts, which does not seem to be the case.

5 Results

We begin with a simple graphical representation of our key motivation in Figure 2. It shows kernel-weighted polynomial regressions of infant gender (Panel A), child's height-for-age (Panel B), and dowry deaths (Panel C) on rainfall during the year of birth (for Panels A and B) or the year of the police report (for Panel C). These figures clearly show that agricultural productivity shocks are positively related to the survival and health of women. In other words, female mortality is higher and female health is worse during bad agricultural years, on average, than during good agricultural years. A newborn is more likely to be female in good rainfall years and it appears that the effect of rainfall on height-for-age is stronger for girls than for boys. Taken together, these results suggest that there is a still a lack of formal consumption-smoothing mechanisms in India and support that households may decrease investments in girls and women – relative to boys and men – as a way to help smooth consumption.

We move to a more robust empirical examination of this relationship in Table 2. In all columns, the dependent variable is a dummy variable indicating whether a child is female. Since data are nationally representative, this is effectively equivalent to analyzing the gender of a randomly selected child. In the first five columns, we restrict estimation to children under the age of one. In columns one through three, we examine the relationship between between rainfall and child gender prior to implementation of NREGS. Column one presents the most basic specification. The coefficient on rainfall indicates that a one-standard-deviation increase in rainfall increases the probability that a randomly chosen child is female by 1.4 percentage points. Adding more control variables for district characteristics and household characteristics in columns two and three increases the estimated effect size slightly; the coefficient in both columns is 0.019, or 1.9 percentage points. In all three cases, the coefficient is significantly different from zero ($p<0.01$).

To put these numbers in context, the interquartile range for rainfall is approximately 2.08 standard deviations. This indicates that the difference in the probability a randomly-selected child is a girl could increase by almost four percentage points when moving from the 25th percentile of rainfall to the 75th percentile. If accurate, this suggests negative rainfall shocks could be a major contributor to "missing" women in south Asia. We return to this point below.

Column four removes the sample restriction of pre-NREGS years and estimates the relationship for the years 2001-2011.[13] Comparing the same specifications in columns three and four, removing the year restriction decreases the coefficient by more than 40 percent, from 0.019 to 0.011. This is suggestive evidence that something happened between 2006 and 2011 to attenuate this relationship. Below, we present additional evidence that NREGS may be responsible.

In the fifth column, we add the previous year's rainfall and the following year's rainfall to the regression. Both coefficients are small and statistically insignificant. This evidence is consistent with rainfall right around birth being the most important component of (female) child survival. We explore this possibility further in columns six and seven. Rose (1999) found that rainfall during the ages of one and two also had a significant impact on the sex ratio. While the results in column five suggest this is unlikely to be the case, we now test this explicitly. In neither column six nor seven is the coefficient on rainfall significant. In

[13]Recall that NREGS was implemented in 2006 for phase one districts, 2007 for phase two districts, and 2008 for phase three districts.

fact, the coefficient in column six is just 0.003 and the coefficient in column seven is actually negative. These results again suggest that only rainfall right around birth is an important predictor of child gender in modern India. This also supports the argument in Bharadwaj and Lakdawala (2013) that families are now more likely to sex-select during pregnancy, relative to previous decades, and may partially explain why our results diverge from Rose (1999) on this point.

Table A1 presents several robustness checks, mostly related to specification choices. We first show that the inclusion of state-by-wave fixed effects does not affect the conclusions. In fact, their inclusion increases the coefficient to 0.026. We also explore different definitions of our rainfall variables, including bins, a simple dummy for good ($Z \geq 1$) rainfall, and an ordinal variable as in Jayachandran (2006). In all cases, substantive conclusions are unchanged by these specification changes. In other words, the empirical evidence supports the contention that, in the early 2000s, girls were more likely to die than boys when households faced anything other than a positive income shock.

We next move to an analysis of the effects of NREGS on the relationship between rainfall and newborn gender in Table 3. The model elaborated in the previous section predicted that NREGS could attenuate the relationship between rainfall and child gender if the program helped households smooth consumption in the face of negative rainfall shocks. In columns one through four, we include the continuous rainfall variable and the years 2001-2011. The coefficient on "Year of birth rainfall (Z)" is positive in all four columns, suggesting the effect of rainfall on the probability of being female is positive prior to NREGS's implementation, consistent with the estimates in Table 2. Though the coefficient on NREGS is negative, it is never close to significant.[14] There is no ex ante reason to expect NREGS to decrease the number of girls, and the insignificant results in column one are consistent with this.

The coefficient of interest is the interaction term between rainfall and NREGS, in the first

[14]The largest t-statistic is in column one and is just -0.79.

row, which represents the change in the effect of rainfall on the sex ratio following implementation of the program. The interaction term is negative, suggesting this relationship decreases markedly following program roll-out. In all four columns, the interaction term is more than 80 percent as large as the coefficient on rainfall and the linear combination is never significant (results not shown), suggesting NREGS almost completely reverses the relationship between rainfall and the sex ratio. Additionally, the coefficients are very stable across specifications. Column two adds year-of-birth fixed effects, column three adds household variables, and column four adds phase linear trends to insulate the estimates from possible differences in trends by phase prior to program implementation. Reassuringly, the results remain surprisingly consistent across these columns.

Columns one through four utilize the entire panel we have constructed, from 2001-2011. While we include district and year-of-birth fixed effects, there may still remain concerns that we are isolating variation in years far removed from NREGS implementation. To test this possibility, in column five we restrict estimation only to the years 2005-2009, one year prior to NREGS to one year following the final phase of NREGS. Though the results are slightly more imprecise, conclusions are unchanged. In fact, the interaction term is now slightly larger than the rainfall coefficient, though the linear combination is not significantly different from zero, similar to columns one through four. If the change in the effect of rainfall is indeed due to the implementation of NREGS, we would expect to see these changes manifest themselves in the years NREGS is actually implemented; this is exactly what we see in column five.

The previous results focused on the sex ratio. However, does the effect of rainfall at the time of birth also extend to long-run indicators of human capital investments for *surviving* girls, like anthropometrics? Figure 2 has already presented suggestive evidence that this is indeed the case. Table 4 presents a number of different specifications exploring this possibility more formally. The dependent variable in all columns is height-for-age, defined using CDC growth charts.[15] Column one estimates the effect of year-of-birth rainfall on height-for-age in 2012. The coefficient on rainfall is positive but small and insignificant. The coefficient on the female dummy is negative and significant, suggesting that there is differential investment in boys and girls in India.

The coefficient on female in conjunction with our previous results of the effect of rainfall on newborn gender raises the possibility that rainfall may differentially affect height-for-age for boys and girls. To explore this possibility, the specification in column two adds an interaction between female and rainfall. The coefficient on rainfall – which now represents the effect of rainfall on boys' height-for-age – decreases to almost exactly zero. Moreover, the coefficient on the interaction term between rainfall and female is positive and significant, and the linear combination of this coefficient with the coefficient on rainfall is also significant (results not shown; p=0.049), suggesting rainfall is significantly correlated with girls' height-for-age. Putting these two results together, rainfall during year of birth apparently affects height-for-age for girls but not for boys. It is also worth noting that this relationship is only identified off of surviving children. It seems plausible that poorer households may be more affected by rainfall shocks, such that children who do not survive would come from the lower end of the height-for-age distribution. If so, then the true results are actually much stronger than the results in Table 4 would indicate (Barcellos et al., 2014).

Column three again removes the pre-NREGS restriction and estimates the relationship over the years 1998-2012. Similar to Table 2, the result is no longer significant and, in fact, actually reverses, though the coefficients are small in magnitude and neither rainfall coefficient is significant. This again supports the contention that something changed between 2006 and 2012.

Column four explores the effects of NREGS on the relationship between rainfall and

[15] We translate height and age into the height-for-age z-score using the user-written *zanthro* command in Stata (Vidmar et al., 2004).

height-for-age, restricting the effect to be the same for both boys and girls. Consistent with the pooled results in column one, it does not appear that NREGS affects the average relationship between rainfall and height-for-age. Nonetheless, the coefficients are in the expected direction and the coefficient on rainfall – which now represents the effect of rainfall on height-for-age prior to NREGS implementation – is marginally significant.

Column five allows the effects of NREGS to vary by gender, which is predicted by the model and is suggested by the results in the previous tables. We find further evidence that NREGS impacts human capital investments differently for girls and boys. In particular, the triple interaction of *NREGS* × *Female* × *Rainfall* is negative and significant, suggesting NREGS attenuated the relationship between rainfall and height-for-age more for girls than for boys. Since these results use the IHDS, we are also able to control for village fixed effects, which we do in column five. Many health outcomes are determined at levels below the district – due to differences in medical care, nutrient availability, etc. – so the inclusion of village fixed effects might be expected to affect the results. However, it appears that the inclusion of village fixed effects has no effect on our substantive conclusions and increases precision (consistent with the argument that many healthcare-related outcomes are determined at a more local level), providing further evidence that NREGS improves girls' human capital outcomes in poor agricultural years, relative to boys'.

The previous results explore the effects of NREGS on two childhood outcomes: the overall sex ratio and child height-for-age. However, our model also predicts similar effects on a specific adult outcome: dowry deaths. Table 5 shows the effect of rainfall on the number of dowry deaths and how it changes after the implementation of NREGS. Column one shows that, prior to NREGS, a one-standard deviation increase in rainfall leads to a decrease in the number of dowry deaths by approximately 0.17 deaths per district per year (at more than 600 districts in the country, this is the equivalent of more than 100 women per year across the entire country). The mean number of deaths in the sample is 11.28, suggesting a one-standard deviation increase in rainfall is associated with a fall in dowry deaths of approximately 1.5 percent.

Adding the number of other reported crimes to the specification has no effect, increasing the coefficient only slightly. This is consistent with the argument advanced earlier that dowry deaths may be caused by different mechanisms than other other crimes. In other words, it does not appear that dowry deaths are simply “another crime” that goes up when overall crime goes up and goes down when overall crime goes down. Column three removes the pre-NREGS restriction on the sample and re-estimates the effect of rainfall on dowry deaths for all years between 2001 and 2012. Like in previous tables, the coefficient attenuates substantially.

Columns four and five investigate the effect of rainfall on dowry deaths by the availability of the NREGS program in a district during a year. We find that the coefficient on rainfall before the implementation of NREGS is negative, but also smaller and more imprecise. This suggests the control variables affect dowry deaths slightly differently prior to and following NREGS implementation, since their effect across time is constrained to be the same in columns four and five. The coefficient on the interaction between rainfall and NREGS is positive and statistically significant. This implies that, after the implementation of NREGS, the negative relation between rainfall and dowry deaths is attenuated. The results are robust to the inclusion of NREGS phase-specific linear trends, which has almost no effect on the magnitude of the interaction term. Overall, it appears that NREGS has effects on both childhood and adult outcomes for women.

5.1 Possible Explanations

In this section, we explore several possible explanations for our findings: failure of the identifying assumptions, changes in women’s bargaining power, and consumption smoothing.

5.1.1 Failure of the Identifying Assumptions

The first possible explanation for our findings is that the difference-in-differences assumption of parallel trends does not hold. While this assumption is not explicitly testable, we explore pre-program trends by implementing placebo analyses in Table 6. We do this by reassigning NREGS to districts three years prior to its true implementation date and drop years with NREGS implementation so that no districts are treated at all during the proxy test. In other words, phase one districts "receive" the program in 2003, phase two districts receive the program in 2004, and phase three districts receive the program in 2005. This is the exact same specification as our main results in Table 3 – but with implementation moved up three years – so the coefficients are directly comparable. If pre-program trends our responsible for our results, we would expect to see similar results in Table 6 and Table 3.

Columns one and two of Table 6 present the corresponding results for the NSS sample, with phase-year trends added in column two. The first coefficients in each column are the coefficients of interest. The coefficients are small in magnitude and relatively precisely estimated. Both coefficients are less than 20 percent as large as our main effects in Table 2, suggesting pre-trends are not responsible for our results. Columns three and four present the corresponding estimates for the IHDS sample and height-for-age. The triple-interaction specification is again identical to that in Table 4, but with implementation moved up three years. The triple interaction is actually positive – the opposite result we found in Table 4 – suggesting our results may actually be underestimating the true effect of NREGS. It appears that differential trends are not responsible for the NREGS results in the NSS or IHDS sample.

Columns five and six perform the same test for the dowry death results. The coefficient of interest is again the simple interaction between Proxy NREGS and rainfall. Though the coefficient is insignificant, it is somewhat large in magnitude, at roughly 60 percent the size of the estimates presented above. Thus, though the point estimates suggest differential trends do not explain the entirety of our results, the imprecision and magnitude leave some doubts about the dowry death estimates.

Table A6 presents an alternative pre-trends check for the NSS and IHDS samples. The table explicitly compares phase trends up to the year just prior to NREGS. The regressions interact the coefficients of interest with dummies for phase two and three. Since the main results are *negative*, any trends driving results would be, on average, more negative in phase one districts than phase two and three districts and more negative in phase two districts than in phase three districts. In fact, we see no differential trends in the NSS sample and the opposite pattern for the IHDS results: phase two and three districts were trending downward relative to phase one districts just before implementation and phase three districts were trending only slightly more positive than phase two districts just prior to implementation. As such, it appears that trends do not explain our main results for gender or height-for-age in either of the parallel trends tests we conducted.

5.1.2 Women's Bargaining Power

Women were originally intended to make up a large proportion of NREGS beneficiaries. The original legislation mandated that: 1) women make up at least one-third of beneficiaries; 2) worksites provide a crèche for the care of children; and 3) men and women are paid equal wages (Ministry of Law and Justice, 2005). In addition, according to one government advisor, the mandated minimum program wage often double the prevailing wage rate for women at the time.[16] While enforcement of these requirements varied by state – and even district – this suggests another mechanism through which NREGS could affect the outcomes studied in this paper: women's bargaining power.

Research suggests increasing women's wages or access to employment improves their bargaining power and decision-making authority (Anderson and Eswaran, 2009; Jensen, 2012), in turn improving their children's health and/or education (Atkin, 2009; Luke and

[16] http://www.levyinstitute.org/pubs/EFFE/Mehrotra_Rio_May9_08.pdf

Munshi, 2011). Although there is litle evidence that NREGS increased women's bargaining power, given NREGS' wages and focus on female beneficiaries, it may be that the effects found in this paper are driven by women's bargaining power. Thus, a second possible mechanism is through an increase in the earning power of women. If the future (expected) earnings of girls increase, parents may respond by increasing investments in girls' human capital – e.g. education and nutrition (Balakrishnan, 2017; Heath and Mobarak, 2015) – and may even affect the sex ratio (Balakrishnan, 2017; Qian, 2008). If rural households believe the program will persist into the future, then they may adjust their investments in girls.

Previous results in this paper suggested that NREGS was not associated with changes in height-for-age for children; the coefficient was actually negative, though insignificant. This is *prima facie* evidence that NREGS did not increase the bargaining power of women. We explore this possibility empirically in Appendix Table A2 and Table A3. Both tables explore the effects of NREGS on expenditures that are normally associated with higher levels of male bargaining power (Lundberg et al., 1997; Browning et al., 2013). If female bargaining power increases, we might expect to see a decrease in the amount of money being spent on tobacco and alcohol and an increase in the amount of money being spent on girsl' clothing and education.

Table A2 looks at the effects of NREGS on alochol and tobacco expenditures, while Table A3 explores the effects of the program on girls' clothing and girls' education. In no column is the effect ever significant, though the effects for alcohol are highly imprecise. On the whole, these results do not suggest that NREGS significantly increased women's bargaining power, which is consistent with previous literature (Sukhtankar, 2016).

5.1.3 Consumption Smoothing

The apparent channel through which NREGS affects these gendered outcomes is through its ability to help households smooth consumption. Santangelo (2016) shows that NREGS decouples wages and consumption from rainfall. We replicate these results in Table A4 using the same dataset used in the previous section. Importantly, the triple interaction between rainfall, NREGS, and post is negative and strongly significant, suggesting the effect of rainfall on consumption is more negative following implementation of NREGS.

5.2 Birth Order Heterogeneity

The results above suggest that NREGS attenuates the relationship between rainfall and the gender of children born in India. In this section, we explore heterogeneity by birth order and identity of the parents.

The estimates above include all children, regardless of birth order. Previous results have found that sex selection is increasing in birth order, with relatively little sex selection of first-born children (Bhalotra and Cochrane, 2010). If this is the case, we would expect to see insignificant effects of rainfall on the gender of first-born children and, correspondingly, no effect of NREGS on this relationship. Table 7 tests these arguments. First, column one explores the effects of rainfall on gender of children of the household head. This is to validate how we identify birth order and confirm that the effect remains when examining children of the head only. The magnitude on rainfall is positive and strongly significant and of a similar magnitude to our main results, suggesting that our results also hold when restricting attention to just children of the head.

We create a variable equal to one for the oldest child of the household head still living in the household, and zero otherwise. We present the rainfall results using only first-born children in column two. Consistent with previous literature, the coefficient is small and insignificant (and actually negative).[17] This suggests rainfall is uncorrelated with the gender of first-born children in India. In column three, we repeat the analysis using the subsample of non-first-born children. The coefficient is now large and significantly

[17]Note that our variable for "first-born" child is not constructed using the actual identity of the first-born child. Rather, we use the oldest child still in the household, which ignores children who have left the household prior to the survey.

positive. The magnitude is also quite a bit larger than our main results, consistent with the idea that there is no effect on first-born children; in other words, the magnitude must be larger for non-first-born children to compensate for the null effect among first-born children.

Columns four through six examine whether the effects of NREGS are similar across these three subsamples of children. In all three columns, we lose substantial precision due to the interaction term and the decidedly smaller samples. Nonetheless, we believe the coefficients can help shed some light on the sex-selection process, rainfall, and the effects of NREGS. Reassuringly, conclusions regarding the rainfall term and interaction in column four are similar to the main results reported above; both terms are also marginally significant ($p<0.15$). Consistent with the null effect on first-born children in column two, column five finds no effect of NREGS on the relationship between rainfall and gender of the first-born child. Column six repeats the analysis using non-first-born children. Again consistent with previous results, we find larger (relative to all children of the head) effects of rainfall in non-NREGS districts and a larger effect of NREGS on this relationship. The results in columns five and six arguably strengthen the identification assumptions, as the results suggest any spurious trends would have to be specific to non-first born children only.

Child gender preferences may lead to differential effects of rainfall based on the current gender composition of siblings. Table 8 explores heterogeneity along this dimension. The first three columns present results using the gender of the first-born child, which is arguably exogenous given the results in Table 7. Comparing the coefficients in columns one and two, it appears that the effect of rainfall on the gender of a child is stronger for subsequent children of first-born girls than first-born boys, though the difference is not statistically significant at traditional levels. Taking the point estimates at face value, the pattern is unsurprising if we assume Indian parents desire at least one boy: if the first-born is a boy, they have less incentive to sex select in subsequent births, as they already have their boy. If the first-born is a girl, on the other hand, the parents may sex select in order to increase the probability of having at least one boy. Column three presents the effects of NREGS on the relationship between rainfall and gender of non-first-born children for whom the first-born sibling was a girl.[18] Though we again lost substantial precision, the coefficients are broadly similar to previous results.

Columns four through seven present results using more general measures of sibling gender composition: simple dummies for whether a given child has an older brother and/or sister. These results must be interpreted with caution, however, as they are endogenous in the presence of sex selection. Nonetheless, we believe the results can inform the context in which these decisions take place. Columns four and five begin with the effect of an older brother. The effect of rainfall on the gender of children with an older brother is broadly similar to the effects of rainfall on the gender of children without an older brother. We see a different pattern for girls, though: the effect of rainfall on the gender of children with an older sister is substantially stronger than the effect on the gender of children without an older sister.

Taken together, these four columns are consistent with a number of explanations. One possible explanation is that parents want exactly one girl and otherwise want more boys. If parents wanted no girls, we would expect similarly large effects regardless of the presence of an older sister. However, since the effect is greater for children with an older sister, there is likely a relatively large subset of households that want at least one girl. In other words, if households did not want any girls, they would sex select regardless of the presence of a girl in the household. For older boys, on the other hand, families sex select less than when there is an older sister, but similarly regardless of the presence of a boy. In other words, parents are always more likely to want another boy than another girl, regardless of whether there is a boy in the household or not.

Column seven presents the NREGS results using the subsample of children with an

[18] We present only the results for first-born girls due to the null effect in column one.

older sister, for whom we find the largest and most consistent evidence of sex selection. As before, the results are very imprecise, but the magnitude and direction of the coefficients is broadly consistent with both our previous results and how we would expect these patterns to manifest themselves given the patterns of sex selection previously documented in India.

5.3 "Missing" Girls

All the results presented in this paper suggest that negative rainfall shocks had profound impacts on girls and women in the early 2000s. Importantly, it appears that negative income shocks, as proxied by rainfall, may be responsible for a substantial number of "missing" women (Sen, 1990, 1992; Bongaarts and Guilmoto, 2015). Additionally, if this is the case, NREGS may have "saved" a significant portion of these girls. In this section, we estimate both the number of missing girls caused by rainfall and the girls plausibly saved by NREGS, since the program appears to have attenuated the relationship between rainfall the probability of having a girl.

We estimate these numbers by assuming that the birth of boys is unaffected by rainfall. We analyze the plausibility of this assumption in Table A5. We collapse the data to the district/year level, summing the number of boys and the number of girls born in each cell, weighted using the survey weights provided by the NSS. We then regress the (log) number of girls at the district/year level on district-level rainfall. We restrict estimation to pre-NREGS years (back to 2001) and include district fixed effects, year fixed effects, and the census variables interacted with year dummies.

The results in Table A5 are, reassuringly for our methodology, as we would expect for the number of girls born. A one-standard deviation increase in rainfall at the district centroid is associated with an increase in the number of girls by approximately 4.5 percent. This is consistent with the results presented above. However, it appears that rainfall is not correlated with the number of *boys* born each year. In fact, the coefficient in column two is only slightly more than 0.01, suggesting the assumption that rainfall does not affect the number of newborn boys is plausible. This is important, as it suggests that households are not decreasing concurrent fertility in response to rainfall shocks. If this were the case, we would expect to see a significantly positive coefficient on rainfall for boys, as well.

We next assume that the number of boys and the number of girls in a district is equated when rainfall is approximately two standard deviations above its ten-year mean. This is the rainfall level at which the probability of being a girl is highest and, to ease computation, we assume that a girl will never be more likely than a boy. We next take the effect of rainfall on the probability of being a girl from the coefficient in column three of Table 2: 0.019. Using this, we predict the number of girls in each district, relying on the assumption that boys and girls are equated at $Z = 2$. Finally, we estimate the effect of rainfall on the probability of being a girl after implementation of NREGS. This coefficient is now shown but is equal to 0.08. Taking this point estimate at face value, we construct predicted number of girls, again for the years 2001 to 2005, prior to implementation of NREGS.

Over the years 2001 to 2005, the difference in the two predicted values is approximately 1.4 million girls. This equates to around 288,000 girls per year in all of India, or around 550 girls per district per year. In other words, if NREGS had been available in the years 2001 to 2005, we estimate that about 1.4 million more girls would have been alive in 2006. This does not take into account the effect of NREGS on dowry deaths, which appears to be a relatively small number relative to the estimate here. Additionally, this number does not consider the improvement in life for girls who receive more investments around birth and are thus healthier. The effect of rainfall on girls' height-for-age decreased by approximately 0.1 standard deviations – relative to boys – following NREGS implementation. Since this is the average effect across all girls, the overall effect is likely quite large.

6 Conclusion

In this paper, we explore the effects of risk-mitigation through workfare programs in rural India on the relationship between agricultural productivity shocks and sex-selection of infants. First, using more recent data, we re-establish that a positive agricultural shock reduces female child mortality. Second, we show that the introduction of Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS) reduces consumption volatility. Third, as a consequence, the introduction of NREGS mitigates the effect of income shocks on the sex-selection of infants. Fourth, we find that prior to the advent of NREGS, a positive agricultural shock also more positively related to the health of surviving female children compared to male children. Lastly, this relationship between income shocks and health of girls is mitigated after the introduction of NREGS.

This paper establishes that policies that are successful in providing tools for consumption-smoothing to rural households in India can also successfully reduce sex-selection of infants and decrease differential child health investments by gender. Though the paper uses one such policy, a rural workfare program, to show that a program which provides households with insurance during lean agricultural years reduces sex-selection among children, the channels explored in this paper more broadly establish that policies that help risk-mitigation can decrease sex-selection when son-preference prevails. This is especially important since the most common policy directed at reducing female child mortality is providing households with financial incentives for having daughters. However, recent literature shows that the success of such policies are very sensitive to the design of these policies (Anukriti, 2018; Balakrishnan, 2017). Therefore, risk-mitigation and similar policies that help households smooth consumption may be an attractive development intervention, with favorable consequences for the sex-ratio and female health investments, as well.

Our results suggest that NREGS would have saved approximately 550 girls per district per year from the years 2001 to 2005. Mechanically, this suggests that NREGS would have increased the total number of children in those years, since rainfall does not appear to be correlated with the number of boys born each year. However, a lingering question from this analysis is whether lifetime fertility increases. In other words, would surviving girls take the place of an additional child, or would women have the same number of future children as they would have prior to implementation of the program? Unfortunately, we are not able to answer this question, which remains an important question mark for future research.

Several questions remain from our analyses. First, while we show that a girl is more likely to survive a poor rainfall year following implementation of NREGS, it is not clear whether this finding also suggests that household fertility will increase. In other words, does the surviving girl get added to the counterfactual number of children, or does the surviving girl "replace" one of them? Given that NREGS was rolled out over just three years, we are not able explore long-term fertility changes using our data and empirical methodology. Second, this paper explores sex selection in response to a negative income shock. We show that NREGS decreases sex selection *due to fluctuations in income*. However, the results do not suggest that son preference diminishes following NREGS or that sex selection does not take place through other channels. Given that one form of sex selection apparently decreases following implementation of the program, it seems reasonable to assume that sex selection, on average, must have also decreased. However, there are many other mechanisms apart from income shocks that may lead to sex selection, and our paper does not address these other possibilities.

References

Anderson, S. and Eswaran, M. (2009). What determines female autonomy? Evidence from Bangladesh. *Journal of Development Economics*, 90(2):179–191.

Anukriti, S. (2018). Financial Incentives and the Fertility-Sex Ratio Trade-off. *American Economic Journal: Applied Economics*.

Atkin, D. (2009). Working for the future: Female factory work and child health in Mexico. *Working Paper*.

Balakrishnan, U. (2017). Incentives for Girls and Gender Bias in India. *Working Paper*.

Barcellos, S., Carvalho, L., and Lleras-Muney, A. (2014). Child Gender and Parental Investments In India: Are Boys and Girls Treated Differently? *American Economic Journal: Applied Economics*, 6(1):157–189.

Bhalotra, S., Chakravarty, A., and Gulesci, A. (2016). The Price of Gold: Dowry and Death in India. *IZA Discussion Paper 9679*.

Bhalotra, S. and Cochrane, T. (2010). Where Have All the Young Girls Gone? Identifying Sex-Selective Abortion in India. *IZA Discussion Paper*, 5381.

Bharadwaj, P. and Lakdawala, L. (2013). Discrimination Begins in the Womb: Evidence of Sex-Selective Prenatal Investments. *Journal of Human Resources*, 48(1):71–113.

Bongaarts, J. and Guilmoto, C. Z. (2015). How many more missing women? Excess female mortality and prenatal sex selection 1970âĂŞ2050. *Ithaca: Cornell University Press*, 42(2):241–269.

Borooah, V. (2004). Gender Bias Among Children in India in their Diet and Immunisation Against Disease. *MPRA Paper from University Library of Munich, Germany*.

Browning, M., Chiappori, P.-A., and Lewbel, A. (2013). Estimating consumption economies of scale, adult equivalence scales, and household bargaining power. *Review of Economic Studies*, 80(4):1267–1303.

Chen, L., Huq, E., and D'Souza, S. (1981). Sex Bias in the Family Allocation of Food and Health Care in Rural Bangladesh. *Population and Development Review*, 7(1):55–70.

Chen, Y., Li, H., and Meng, L. (2013). Prenatal Sex Selection and Missing Girls in China: Evidence from the Diffusion of Diagnostic Ultrasound. *Journal of Human Resources*, 48(1):36–70.

Das Gupta, M. (1987). Sex Bias in the Family Allocation of Food and Health Care in Rural Bangladesh. *Population and Development Review*, 13(1):77–100.

Eswaran, M. (2002). The empowerment of women, fertility, and child mortality: Towards a theoretical analysis. *Journal of Population Economics*, 15(3):433–454.

Fetzer, T. (2014). Social Insurance and Conflict: Evidence from India. *Working paper*.

Foster, A. D. and Gehrke, E. (2017). Consumption Risk and Human Capital Accumulation in India. *NBER Working Paper No. 24041*.

Ganatra, B. and Hirve, S. (1994). Male bias in health care utilization for under-fives in a rural community in Western India. *Bulletin of the World Health Organization*, 72(1):101–104.

Ganatra, B. and Hirve, S. (2001). Does increased access increase equality? Gender and child health investments in India. *Journal of Development Economics*, 89(1):62–76.

Gehrke, E. (2017). An employment guarantee as risk insurance? assessing the effects of the nregs on agricultural production decisions. *The World Bank Economic Review*, 1:23.

Heath, R. and Mobarak, A. M. (2015). Manufacturing growth and the lives of Bangladeshi women. *Journal of Development Economics*, 115:1–15.

Imbert, C. and Papp, J. (2015). Labor Market Effects of Social Programs: Evidence from India's Employment Guarantee. *American Economic Journal: Applied Economics*, 7(2):233–263.

Jayachandran, S. (2006). Selling labor low: Wage responses to productivity shocks in developing countries. *Journal of Political Economy*, 114(3):538–575.

Jayachandran, S. (2017). Fertility Decline and Missing Women. *American Economic Journal: Applied Economics*, 9(1):118–1392.

Jayachandran, S. and Kuziemko, I. (2011). Why Do Mothers Breastfeed Girls Less than Boys? Evidence and Implications for Child Health in India. *The Quarterly Journal of Economics*, 126(3):1485–1538.

Jensen, R. (2012). Do labor market opportunities affect young women's work and family decisions? Experimental evidence from India. *The Quarterly Journal of Economics*, 127(2):753–792.

Jha, R., Bhattacharyya, S., and Gaiha, R. (2011). Social safety nets and nutrient deprivation: An analysis of the National Rural Employment Guarantee Program and the Public Distribution System in India. *Journal of Asian Economics*, 22(2):189–201.

Kalsi, P. (2017). Seeing is Believing - Can Increasing the Number of Female Leaders Reduce Sex Selection in Rural India? *Journal of Development Economics*, 126(1).

Kaur, R. (2004). Across-region marriages: Poverty, female migration and the sex ratio. *Economic and political weekly*, pages 2595–2603.

Khanna, G. and Zimmermann, L. (2017). Guns and butter? Fighting violence with the promise of development. *Journal of Development Economics*, 124:120–141.

Luke, N. and Munshi, K. (2011). Women as agents of change: Female income and mobility in India. *Journal of Development Economics*, 94(1):1–17.

Lundberg, S. J., Pollak, R. A., and Wales, T. J. (1997). Do husbands and wives pool their resources? Evidence from the United Kingdom child benefit. *Journal of Human Resources*, pages 463–480.

Maccini, S. and Yang, D. (2009). Under the weather: Health, schooling, and economic consequences of early-life rainfall. *American Economic Review*, 99(3):1006–26.

Merfeld, J. D. (2018a). Moving up or Just Surviving? Non-Farm Self-Employment in India. *Working Paper*.

Merfeld, J. D. (2018b). Spatially Heterogeneous Effects of a Public Works Program. *Working paper*.

Miguel, E. (2005). Poverty and witch killing. *The Review of Economic Studies*, 72(4):1153–1172.

Miller, B. (1981). The Endangered Sex: Neglect of Female Children In Rural North India. *Population and Development Review*.

Ministry of Law and Justice (2005). The National Rural Employment Guarantee Act. shttp://nrega.nic.in/rajaswa.pdf.

Qian, N. (2008). Missing women and the price of tea in China: The effect of sex-specific earnings on sex imbalance. *The Quarterly Journal of Economics*, 123(3):1251–1285.

Quisumbing, A. R. and Maluccio, J. A. (2003). Resources at marriage and intrahousehold allocation: Evidence from Bangladesh, Ethiopia, Indonesia, and South Africa. *Oxford Bulletin of Economics and Statistics*, 65(3):283–327.

Rao, V. (1999). *Poverty and public celebrations in rural India*. The World Bank.

Ravi, S. and Engler, M. (2015). Workfare as an effective way to fight poverty: The case of India's NREGS. *World Development*, 67:57–71.

Rose, E. (1999). Consumption Smoothing and Excess Female Mortality in Rural India. *Review of Economics and Statistics*, 81(1):41–49.

Rosenblum, D. (2013). The effect of fertility decisions on excess female mortality in India. *Journal of Population Economics*, 26(1):147–180.

Santangelo, G. (2016). Firms and Farms: The Impact of Agricultural Productivity on the Local Indian Economy. *Working paper*.

Sekhri, S. and Storeygard, A. (2014). Dowry deaths: Response to weather variability in India. *Journal of Development Economics*, 111:212–223.

Sen, A. (1990). More Than 100 Million Women Are Missing. *The New York Review of Books*, 20:61–66.

Sen, A. (1992). How many more missing women? Excess female mortality and prenatal sex selection 1970-2050. *BMJ: British Medical Journal*, 304(6827):587.

Shah, M. and Steinberg, B. M. (2015). Workfare and human capital investment: Evidence from India. *National Bureau of Economic Research Working Paper*.

Song, L., Appleton, S., and Knight, J. (2006). Does increased access increase equality? Gender and child health investments in India. *World Development*, 34(9):1639–1653.

Sukhtankar, S. (2016). India's National Rural Employment Guarantee Scheme: What Do We Really Know about the World's Largest Workfare Program? In *India Policy Forum*, volume 13, pages 2009–10.

Vidmar, S., Carlin, J., Hesketh, K., Cole, T., et al. (2004). Standardizing anthropometric measures in children and adolescents with new functions for egen. *Stata Journal*, 4(1):50–55.

Zimmermann, L. (2017). Why Guarantee Employment? Evidence from a Large Indian Public-Works Program. *Working Paper*.

Figures & Tables

Figure 1: Rainfall in Year of Birth and Child Outcomes

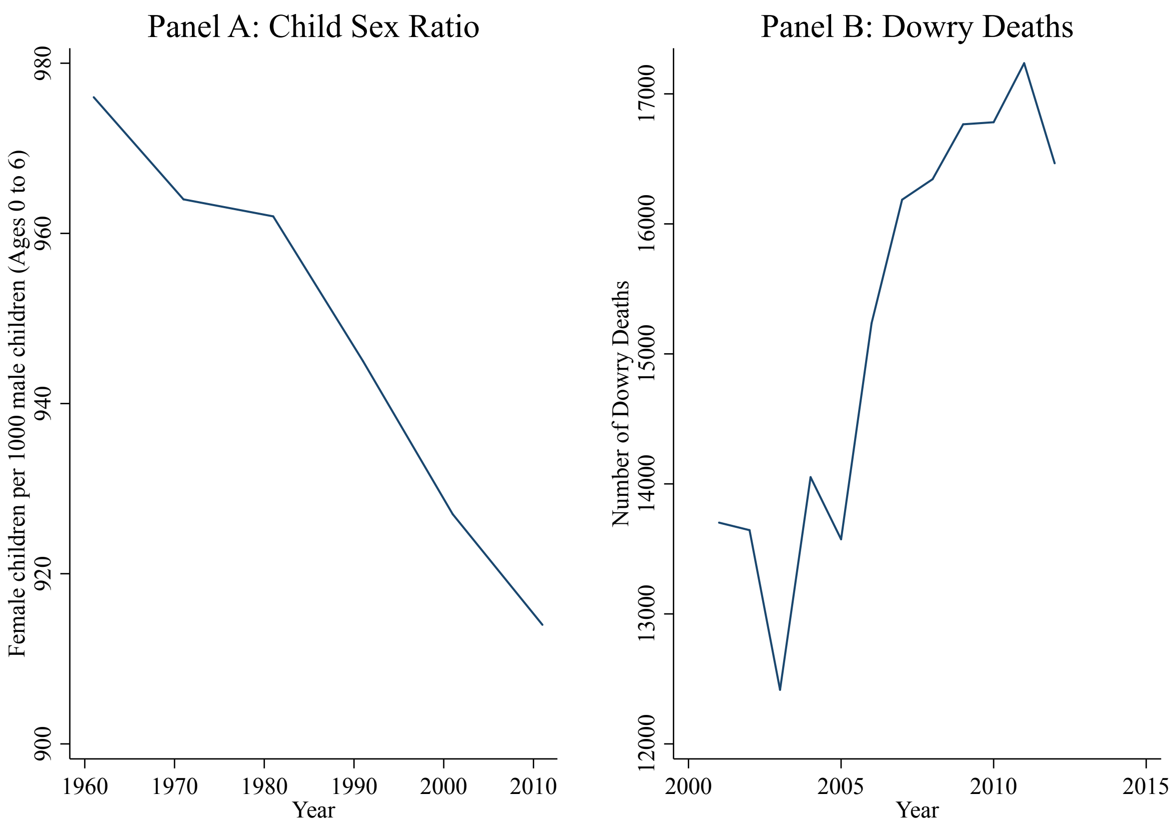
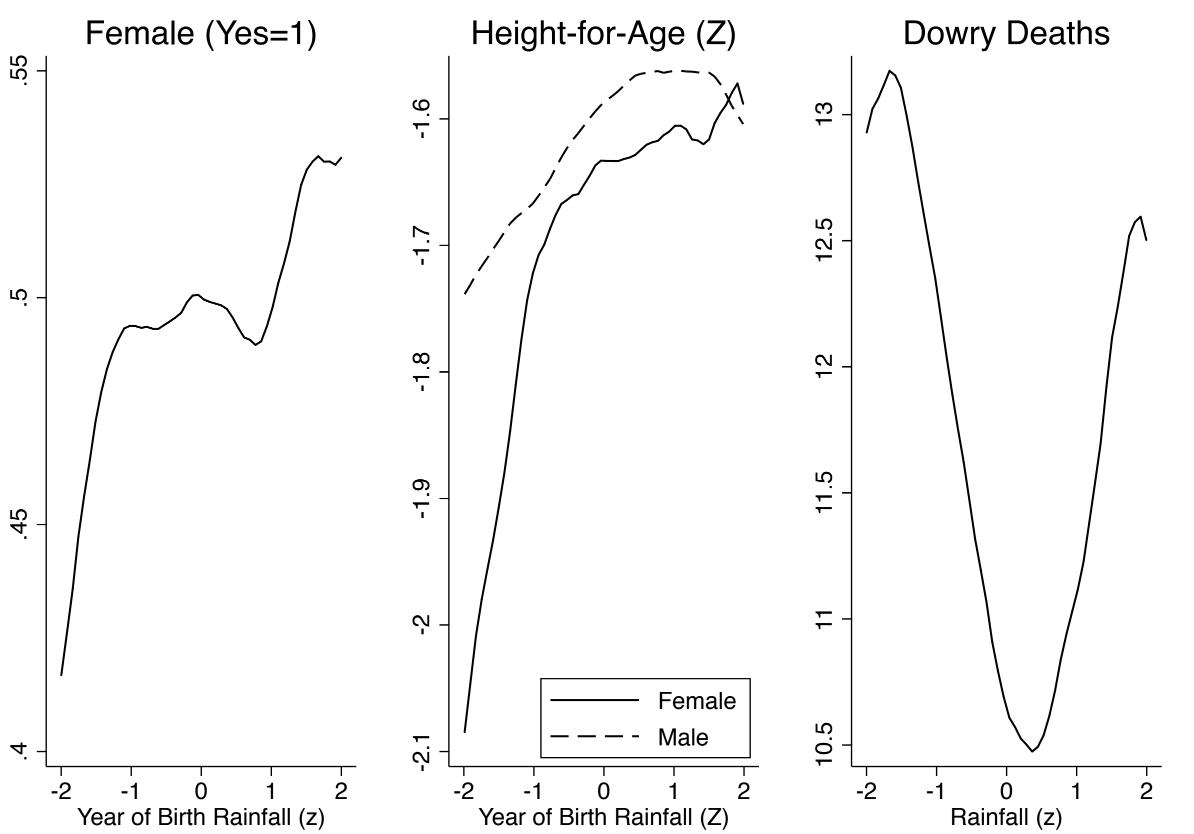


Table 1: Summary Statistics

	Phase 1	Phase 2	Phase 3
Panel A: NSS Individuals			
Girl (if < 1 year old)	0.49	0.48	0.48
	(0.50)	(0.50)	(0.50)
Girl (if one year old)	0.49	0.48	0.49
	(0.50)	(0.50)	(0.50)
Girl (if two years old)	0.48	0.47	0.48
	(0.50)	(0.50)	(0.50)
Household size	6.55	6.51	6.59
	(2.92)	(2.93)	(2.95)
Head is male	0.93	0.93	0.93
	(0.25)	(0.26)	(0.26)
Head age	41.62	42.00	42.63
	(13.64)	(13.80)	(14.37)
Head education	1.90	2.00	2.26
	(1.36)	(1.43)	(1.51)
Observations	51493	38055	73759
Panel B: IHDS Individuals			
Girls' height for age (Z)	−1.65	−1.73	−1.57
	(1.56)	(1.52)	(1.53)
Boys' height for age (Z)	−1.53	−1.80	−1.33
	(1.45)	(1.45)	(1.46)
Observations	2385	1793	4260
Panel C: NCIB Districts			
Dowry deaths	12.75	10.97	10.14
	(16.05)	(11.85)	(12.29)
Other crimes against girls	220.80	228.83	255.94
	(207.16)	(221.38)	(256.25)
All other crimes	2028.20	2243.32	3205.94
	(1721.05)	(1829.61)	(3668.59)
Observations	181	112	248
Panel D: Census Districts (NSS Sample			
Percent SC/ST	0.38	0.31	0.27
	(0.20)	(0.21)	(0.22)
Percent literate	0.47	0.53	0.58
	(0.11)	(0.13)	(0.10)
Labor force participation	0.42	0.40	0.40
	(0.07)	(0.07)	(0.07)
Population (log)	14.06	14.11	13.98
	(0.87)	(0.89)	(1.09)
Percent rural	0.86	0.82	0.72
	(0.09)	(0.13)	(0.17)
Sex ratio	945.83	940.27	926.76
	(45.99)	(46.97)	(64.60)
Observations	171	112	236

Statistics are means. All individual statistics are nationally representative and are estimated using survey weights. The individual statistics for the NSS are for children less than two years old, for the years 2001-2005. The NSS Districts data are from the 2000 census. The IHDS anthropometrics are constructed using CDC charts and the *zanthro* command in Stata (Vidmar et al., 2004).

Figure 2: Rainfall in Year of Birth and Child Outcomes



Graphs are kernel-weighted local polynomial regressions. All observations are before the implementation of NREGS in a district. The top and bottom one percent of rainfall values are trimmed for ease of presentation.

Table 2: Rainfall and Child Gender

	Newborns					One-Year Olds	Two-Year Olds
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Current rainfall (Z)	0.014***	0.019***	0.019***	0.011*	0.019***	0.003	−0.006
	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)
Previous rainfall (Z)					0.001		
					(0.006)		
Next rainfall (Z)					0.006		
					(0.006)		
Pre NREGS	Yes	Yes	Yes	No	No	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Vars	No	Yes	Yes	Yes	Yes	Yes	Yes
Household Vars	No	No	Yes	Yes	Yes	Yes	Yes
Observations	66312	65810	65791	88547	65791	72315	78593

Standard errors are in parentheses and are clustered at the district level. Columns one through three and five through seven use the years 2001-2007; column four uses the years 2001-2011. All data are from NSS waves 61, 64, and 68. Newborns are defined as chilren less than one year of age. Current rainfall is standardized using the mean and standard deviation of the previous 10 years. * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 3: NREGS, Rainfall, and Child Gender

	Years 2001-2011				Years 2005-2009
	Model 1	Model 2	Model 3	Model 4	Model 5
Rainfall (z) times NREGS	−0.038**	−0.044**	−0.045**	−0.046**	−0.054**
	(0.018)	(0.020)	(0.020)	(0.020)	(0.025)
Year of birth rainfall (Z)	0.047***	0.053***	0.053***	0.054***	0.052*
	(0.018)	(0.020)	(0.020)	(0.020)	(0.027)
NREGS	−0.038	−0.025	−0.024	0.017	0.006
	(0.027)	(0.037)	(0.037)	(0.042)	(0.043)
District FE	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes
District Vars	No	Yes	Yes	Yes	Yes
Household Vars	No	No	Yes	Yes	Yes
Phase Linear Trend	No	No	No	Yes	No
Observations	89264	88570	88547	88547	38451

Standard errors are in parentheses and are clustered at the district level. The dependent variable in all columns is whether a newborn (defined as less than one year of age) is a girl. Columns one through four use the years 2001-2011, while column five restricts estimation to just one year prior to NREGS to one year following implementation of the final phase. Current rainfall is standardized using the mean and standard deviation of the previous 10 years. * p<0.1 ** p<0.05 *** p<0.01

Table 4: NREGS, Rainfall, and Child Height-for-Age

	District FE					Village FE
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Year of birth rainfall (Z)	0.026	0.002	0.025	0.035*	0.014	0.057***
	(0.019)	(0.023)	(0.018)	(0.018)	(0.022)	(0.021)
Female	−0.087**	−0.091**	−0.047	−0.047	−0.084**	−0.080*
	(0.036)	(0.036)	(0.029)	(0.029)	(0.036)	(0.042)
Female times Rainfall		0.051*	−0.003		0.044	0.023
		(0.030)	(0.024)		(0.030)	(0.019)
NREGS				0.003	−0.048	−0.037
				(0.088)	(0.090)	(0.080)
Rainfall (z) times NREGS				−0.028	0.028	0.017
				(0.031)	(0.037)	(0.028)
Female times NREGS					0.116*	0.094**
					(0.062)	(0.036)
NREGS times Female times Rainfall					−0.118*	−0.098**
					(0.060)	(0.040)
Years	Pre NREGS	Pre NREGS	1998-2012	1998-2012	1998-2012	1998-2012
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
District Vars	Yes	Yes	Yes	Yes	Yes	Yes
Household Vars	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13309	13309	19373	19373	19373	19373

Standard errors are in parentheses and are clustered at the district level (columns one through five) or village level (column six). Columns one and two include children born between the years 1998 and 2005, though only 3.38 percent of observations come from prior to 2002. Columns three through six includes children born during the years 1998-2011 (only 1.20 percent of observations are from prior to 2002). The dependent variable in all columns is height-for-age, standardized using the CDC charts and the *zanthro* command in Stata (Vidmar et al., 2004). Rainfall is always defined as rainfall during year of birth. * p<0.1 ** p<0.05 *** p<0.01

Table 5: Rainfall, NREGS, and Dowry Deaths

	(1)	(2)	(3)	(4)	(5)
Rainfall (z)	-0.17*	-0.19*	0.02	-0.12	-0.11
	(0.10)	(0.10)	(0.07)	(0.11)	(0.11)
NREGS				-0.05	-0.11
				(0.53)	(0.53)
Rainfall (z) times NREGS				0.26*	0.25*
				(0.14)	(0.14)
Years	Pre-NREGS	Pre-NREGS	2001-2012	2001-2012	2001-12
Year Effects	Yes	Yes	Yes	Yes	Yes
District Vars	Yes	Yes	Yes	Yes	Yes
Other Crime Variables	No	Yes	Yes	Yes	Yes
Phase Trends	No	No	No	No	Yes
Observations	3316	3316	6499	6499	6499

All specifications include district fixed effects. Standard errors are in parentheses and are clustered at the district level.
* p<0.1 ** p<0.05 *** p<0.01

Table 6: Testing the Parallel Trends Assumption

	Female		HAZ		Dowry Deaths	
	(1)	(2)	(3)	(4)	(5)	(6)
Proxy NREGS times Female times Rainfall			0.095*	0.096*		
			(0.054)	(0.054)		
Proxy NREGS times Rainfall	−0.004	−0.003	−0.089**	−0.087**	0.318	0.314
	(0.007)	(0.007)	(0.041)	(0.041)	(0.204)	(0.197)
Female times Rainfall			−0.010	−0.010		
			(0.029)	(0.029)		
Proxy NREGS times Female			−0.015	−0.017		
			(0.066)	(0.066)		
Proxy NREGS	0.006	0.013	0.085	0.090	0.633	0.343
	(0.016)	(0.017)	(0.071)	(0.077)	(0.488)	(0.499)
Female			−0.041	−0.041		
			(0.031)	(0.031)		
Rainfall (Z)	0.007*	0.007*	0.032	0.033	−0.295**	−0.279**
	(0.004)	(0.004)	(0.022)	(0.023)	(0.139)	(0.136)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
District Vars	Yes	Yes	Yes	Yes	Yes	Yes
Household Vars	Yes	Yes	Yes	Yes	Yes	Yes
Phase-Year Trends	No	Yes	No	Yes	No	Yes
Observations	57529	57529	28216	28216	3248	3248

Standard errors are in parentheses and are clustered at the district level. Columns one and two use the National Sample Survey and the dependent variable is whether a newborn is female. Columns three and four use the IHDS and the dependent variable is height-for-age Z score. Columns five and six use the National Crime Records and the dependent variable is number of dowry deaths. The Proxy NREGS variable is defined similarly to the NREGS variables in the prior tables, but with implementation date moved up one year (e.g. assuming phase one districts received the program in 2005 instead of 2006).
* $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 7: First Born and Sex Selection

	Pre-NREGS			Effects of NREGS		
DV in all columns: Female	Child of Head	First Child	Not First Child	Child of Head	First Child	Not First Child
Rainfall (z) times NREGS				−0.034	0.000	−0.048
				(0.027)	(0.052)	(0.033)
Year of birth rainfall (Z)	0.020***	−0.004	0.032***	0.037	−0.006	0.051
	(0.006)	(0.013)	(0.008)	(0.026)	(0.051)	(0.033)
NREGS				−0.002	0.006	0.000
				(0.048)	(0.071)	(0.052)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
District Vars	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Household Vars	Yes	Yes	Yes	Yes	Yes	Yes
Observations	36130	8732	27398	48341	12139	36202

Standard errors are in parentheses and are clustered at the district level. The first three columns use observations prior to NREGS. The last three columns use the years 2001-2011. First born is defined as the oldest child currently in the household.
* p<0.1 ** p<0.05 *** p<0.01

Table 8: First Born and Sex Selection II

	First-Born Gender			Older Brother		Older Sister		
DV in all columns: Female	Boy	Girl	Girl	Yes	No	Yes	No	Yes
Year of birth rainfall (Z)	0.018	0.032***	0.039	0.019*	0.022*	0.033***	0.014	0.036
	(0.012)	(0.010)	(0.040)	(0.010)	(0.012)	(0.009)	(0.014)	(0.035)
Rainfall (z) times NREGS			−0.023					−0.031
			(0.043)					(0.038)
NREGS			0.027					0.026
			(0.072)					(0.061)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Vars	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Vars	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13898	13500	17920	19091	8307	18882	8516	24797

Standard errors are in parentheses and are clustered at the district level. The first three columns use observations prior to NREGS. The last three columns use the years 2001-2011. First born is defined as the oldest child currently in the household.
* $p<0.1$ ** $p<0.05$ *** $p<0.01$

Appendix: Conceptual Framework

After substituting the budget constraints in the utility maximization problem, equation 1 becomes:

$$\begin{aligned}\underset{k_b,k_g,A}{\text{maximize}}\; U =& u_1[\alpha_1 F(1) - 0.5k_b N - 0.5k_g N] \\ &+ \beta u_2[\alpha_2 F(1-A) + 0.5k_b NBA - 0.5k_g NG] \\ &+ \beta u_c[0.5k_b N + 0.5k_g N].\end{aligned} \tag{10}$$

For given optimal health made investments in male and female children during the first period, the optimal labor hours in dowry appropriation in the second period is given by:

$$\frac{\partial U(k_b, k_g, A)}{\partial A} = \beta u_2'[-\alpha_2 F'(1-A) + 0.5k_b NB] = 0. \tag{11}$$

As $\beta > 0$ and $u_2' > 0$, solving equation 11 yields:

$$A^* = 1 - F'^{-1}\left[\frac{0.5k_b NB}{\alpha_2}\right]. \tag{12}$$

As $F'' < 0$, from equation 12, we get,

$$\frac{\partial A^*}{\partial \alpha_2} < 0. \tag{13}$$

This proves **Prediction 2**.

We denote the optimal dowry appropriation hours using $A^* = A^*(k_b)$ and re-wirte the household's problem in equation 1 as:

$$\begin{aligned}\underset{k_b,k_g}{\text{maximize}}\; U =& u_1[\alpha_1 F(1) - 0.5k_b N - 0.5k_g N] \\ &+ \beta u_2[\alpha_2 F(1-A^*(k_b)) + 0.5k_b NBA^*(k_b) - 0.5k_g NG] \\ &+ \beta u_c[0.5k_b N + 0.5k_g N].\end{aligned} \tag{14}$$

The household's optimal health investment in its male children is given by:

$$
\begin{aligned}
\frac{\partial U(k_b, k_g)}{\partial k_b} =& -0.5Nu_1' \\
& + \beta u_2'[-\alpha_2 F'\{1 - A^*(k_b)\}A^{*\prime}(k_b) + 0.5NBA^* k_b + 0.5k_b NBA^{*\prime}(k_b)] \\
& + \beta 0.5Nu_c' \\
=& 0,
\end{aligned} \tag{15}
$$

and optimal health investment in its female children is given by:

$$
\frac{\partial U(k_b, k_g)}{\partial k_g} = -0.5Nu_1' - 0.5\beta NGu_2' + 0.5\beta Nu_c' = 0. \tag{16}
$$

Setting equations 15 and 16 equal to each other, we have:

$$
-\alpha_2 F'\{1 - A^*(k_b)\}A^{*\prime}(k_b) + 0.5NBA^*(k_b) + 0.5k_b NBA^{*\prime}(k_b) = -0.5\beta NG. \tag{17}
$$

After the total differentiation of equations 15 and 16 with respect to α_1, we get:

$$
\begin{aligned}
\frac{\partial^2 U(k_b, k_g)}{\partial k_b^2}\frac{\partial k_b^*}{\partial \alpha_1} + \frac{\partial^2 U(k_b, k_g)}{\partial k_b \partial k_g}\frac{\partial k_g^*}{\partial \alpha_1} + \frac{\partial^2 U(k_b, k_g)}{\partial k_b \partial \alpha_1} = 0, \\
\frac{\partial^2 U(k_b, k_g)}{\partial k_g \partial k_b}\frac{\partial k_b^*}{\partial \alpha_1} + \frac{\partial^2 U(k_b, k_g)}{\partial k_g^2}\frac{\partial k_g^*}{\partial \alpha_1} + \frac{\partial^2 U(k_b, k_g)}{\partial k_g \partial \alpha_1} = 0.
\end{aligned} \tag{18}
$$

The second partial derivatives in 18 are as follows:

$$
\frac{\partial^2 U(k_b, k_g)}{\partial k_g^2} =(-0.5N)^2 u_1'' + \beta(-0.5NG)^2 u_2'' + \beta(-0.5N)^2 u_c'' < 0, \tag{19}
$$

as N, β and G are positive, and $u'' < 0$.

$$\begin{aligned}\frac{\partial^2 U(k_b, k_g)}{\partial k_b^2} =& (-0.5N)^2 u_1'' \\ &+ \beta u_2''[-\alpha_2 F'\{1 - A^*(k_b)\}A^{*\prime}(k_b) + 0.5NBA^*(k_b) + 0.5k_b NBA^{*\prime}(k_b)]^2 \\ &+ \beta u_2'[\alpha_2 F''(1 - A^*(k_b))A^{*\prime}(k_b)^2 + NBA^{*\prime}(k_b)] \\ &+ \beta(-0.5N)^2 u_c'',\end{aligned} \tag{20}$$

$$\begin{aligned}\frac{\partial^2 U(k_b, k_g)}{\partial k_b \partial k_g} =& (-0.5N)^2 u_1'' \\ &+ \beta u_2''[-\alpha_2 F'\{1 - A^*(k_b)\}A^{*\prime}(k_b) + 0.5NBA^*(k_b) + 0.5k_b NBA^{*\prime}(k_b)][-0.5NG] \\ &+ \beta(0.5N)^2 u_c'',\end{aligned} \tag{21}$$

$$\frac{\partial^2 U(k_b, k_g)}{\partial k_b \partial \alpha_1} = -0.5N u_1'' F(1), \tag{22}$$

$$\frac{\partial^2 U(k_b, k_g)}{\partial k_g \partial \alpha_1} = -0.5N u_1'' F(1). \tag{23}$$

Substituting equations 17 and 19 in equation 21, we get:

$$\frac{\partial^2 U(k_b, k_g)}{\partial k_b \partial k_g} = \frac{\partial^2 U(k_b, k_g)}{\partial k_g^2}. \tag{24}$$

Substituting equations 17 and 19 in equation 20, we get:

$$\frac{\partial^2 U(k_b, k_g)}{\partial k_b^2} = \frac{\partial^2 U(k_b, k_g)}{\partial k_g^2} + \beta u_2'[\alpha_2 F''(1 - A^*(k_b))A^{*\prime}(k_b)^2 + NBA^{*\prime}(k_b)]. \tag{25}$$

Using Cramer's rule, we solve the system of equations in 18 to find:

$$\frac{\partial k_b^*}{\partial \alpha_1} = \frac{\begin{vmatrix} -\frac{\partial^2 U(k_b,k_g)}{\partial k_b \partial \alpha_1} & \frac{\partial^2 U(k_b,k_g)}{\partial k_b \partial k_g} \\ -\frac{\partial^2 U(k_b,k_g)}{\partial k_g \partial \alpha_1} & \frac{\partial^2 U(k_b,k_g)}{\partial k_g^2} \end{vmatrix}}{\begin{vmatrix} \frac{\partial^2 U(k_b,k_g)}{\partial k_b^2} & \frac{\partial^2 U(k_b,k_g)}{\partial k_b \partial k_g} \\ \frac{\partial^2 U(k_b,k_g)}{\partial k_g \partial k_b} & \frac{\partial^2 U(k_b,k_g)}{\partial k_g^2} \end{vmatrix}} \tag{26}$$

After substituting equations 22, 23 and 24 in equation 26, the numerator in equation 26 becomes 0, and therefore:

$$\frac{\partial k_b^*}{\partial \alpha_1} = 0 \tag{27}$$

Using Cramer's rule again, we solve the system of equations in 18 to find:

$$\frac{\partial k_g^*}{\partial \alpha_1} = \frac{\begin{vmatrix} \frac{\partial^2 U(k_b,k_g)}{\partial k_b^2} & -\frac{\partial^2 U(k_b,k_g)}{\partial k_b \partial \alpha_1} \\ \frac{\partial^2 U(k_b,k_g)}{\partial k_g \partial k_b} & -\frac{\partial^2 U(k_b,k_g)}{\partial k_g \partial \alpha_1} \end{vmatrix}}{\begin{vmatrix} \frac{\partial^2 U(k_b,k_g)}{\partial k_b^2} & \frac{\partial^2 U(k_b,k_g)}{\partial k_b \partial k_g} \\ \frac{\partial^2 U(k_b,k_g)}{\partial k_g \partial k_b} & \frac{\partial^2 U(k_b,k_g)}{\partial k_g^2} \end{vmatrix}} \tag{28}$$

After substituting equations 22 through 25 in equation 28, we can rewrite equation 28 as:

$$\frac{\partial k_g^*}{\partial \alpha_1} = \frac{\begin{vmatrix} \frac{\partial^2 U(k_b,k_g)}{\partial k_g^2} + \beta u_2'[\alpha_2 F''(1 - A^*(k_b))A^{*\prime}(k_b)^2 + NBA^{*\prime}(k_b)] & 0.5Nu_1''F(1) \\ \frac{\partial^2 U(k_b,k_g)}{\partial k_g^2} & 0.5Nu_1''F(1) \end{vmatrix}}{\begin{vmatrix} \frac{\partial^2 U(k_b,k_g)}{\partial k_g^2} + \beta u_2'[\alpha_2 F''(1 - A^*(k_b))A^{*\prime}(k_b)^2 + NBA^{*\prime}(k_b)] & \frac{\partial^2 U(k_b,k_g)}{\partial k_g^2} \\ \frac{\partial^2 U(k_b,k_g)}{\partial k_g^2} & \frac{\partial^2 U(k_b,k_g)}{\partial k_g^2} \end{vmatrix}} \tag{29}$$

As $u'' < 0$, $N > 0$, and $F(1) > 0$, using equation 19 in equation 29 we get:

$$\frac{\partial k_g^*}{\partial \alpha_1} = \frac{0.5Nu_1''F(1)}{\frac{\partial^2 U(k_b,k_g)}{\partial k_g^2}} > 0. \tag{30}$$

This proves **Prediction 1**, that is,

$$\frac{\partial k_g^*}{\partial \alpha_1} > \frac{\partial k_g^*}{\partial \alpha_1} = 0. \tag{31}$$

Introducing NREGS

After substituting the budget constraints in the utility maximization problem, equation **??** becomes:

$$\begin{aligned} \underset{E_1,E_2,k_b,k_g,A}{\text{maximize}}\ U =& u_1[\alpha_1 F(1-E_1) + sE_1 - 0.5k_bN - 0.5k_gN] \\ &+ \beta u_2[\alpha_2 F(1-E_2-A) + sE_2 + 0.5Nk_bBA - 0.5Nk_gG] \\ &+ \beta u_c[0.5k_bN + 0.5k_gN] \end{aligned} \tag{32}$$

For given optimal health investments made in male and female children during the first period, the optimal labor hours in dowry appropriation in the second period is given by

$$\frac{\partial U(k_b, k_g, E_1, E_2, A)}{\partial A} = \beta u_2'[-\alpha_2 F'(1-E_2-A) + 0.5Nk_bB] = 0, \tag{33}$$

and the optimal labor hours in the non-farm NREGS sector in the second period is given by

$$\frac{\partial U(k_b, k_g, E_1, E_2, A)}{\partial E_2} = \beta u_2'[-\alpha_2 F'(1-E_2-A) + s] = 0. \tag{34}$$

If $s < 0.5Nk_bB$, then $E_2^*\big|_{NREGS} = 0$ and $A^*\big|_{NREGS} = A^*$. That is, if the minimum wage in the non-farm sector is less than the marginal return from dowry appropriation, then the household's optimal labor hours spent in dowry appropriation is the same as when the non-farm sector was not present.

If $s > 0.5Nk_bB$, then $E_2^*\big|_{NREGS} = A^*$ and $A^*\big|_{NREGS} = 0$. That is, if the minimum wage in the non-farm sector is greater than the marginal return from dowry appropriation, then the household's optimal labor hours spent in the non-farm sector is the same as the hours it would have spent in dowry appropriation when the non-farm sector was not present. In this case, the household does not spend any labor hours in dowry appropriation when the non-farm sector is present.

If $s = 0.5Nk_bB$, then $E_2^*\big|_{NREGS} + A^*\big|_{NREGS} = A^*$. That is, if the minimum wage in the non-farm sector is equal to the marginal return from dowry appropriation, then the total labor hours spent in the non-farm sector and in dowry appropriation together when the non-farm sector is present is equal to the labor hours the household would have spent in dowry appropriation in the absence of the non-farm sector.

After the total differentiation of equations 33 and 34 with respect to α_1, we get:

$$\begin{aligned}
\frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial A^2}\frac{\partial A^*}{\partial \alpha_2} + \frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial A \partial E_2}\frac{\partial E_2^*}{\partial \alpha_2} + \frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial A \partial \alpha_2} &= 0 \\
\frac{\partial^2 U(k_b,k_g,,E_1,E_2,A)}{\partial E_2 \partial A}\frac{\partial A^*}{\partial \alpha_2} + \frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial E_2^2}\frac{\partial E_2^*}{\partial \alpha_2} + \frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial E_2 \partial \alpha_2} &= 0
\end{aligned} \tag{35}$$

The second partial derivatives in equation 35 are as follows:

$$\begin{aligned}
\frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial A^2} =& \beta u_2''[-\alpha_2 F'(1-E_2-A) + 0.5Nk_bB]^2 + \beta u_2'[\alpha_2 F''(1-E_2-A)] \\
=& \beta u_2'[\alpha_2 F''(1-E_2-A)],
\end{aligned} \tag{36}$$

using equation 33.

$$\begin{aligned}\frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial E_2^2} =&\beta u_2''[-\alpha_2 F'(1-E_2-A)+s]^2+\beta u_2'[\alpha_2 F''(1-E_2-A)] \\ =&\beta u_2'[\alpha_2 F''(1-E_2-A)],\end{aligned} \tag{37}$$

using equation 34.

$$\begin{aligned}\frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial E_2 \partial A} =&\beta u_2''[-\alpha_2 F'(1-E_2-A)+s][-\alpha_2 F'(1-E_2-A)+0.5Nk_bB] \\ &+\beta u_2'[\alpha_2 F''(1-E_2-A)] \\ =&\beta u_2'[\alpha_2 F''(1-E_2-A)],\end{aligned} \tag{38}$$

using either equation 33 or 34.

$$\begin{aligned}\frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial E_2 \partial \alpha_2} =&\beta u_2''[-\alpha_2 F'(1-E_2-A)+s][F(1-E_2-A)]+\beta u_2'[-F'(1-E_2-A)] \\ =&\beta u_2'[-F'(1-E_2-A)],\end{aligned} \tag{39}$$

using equation 33.

$$\begin{aligned}\frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial A \partial \alpha_2} =&\beta u_2''[-\alpha_2 F'(1-E_2-A)+\theta N p(k_b)B][F(1-E_2-A)]+\beta u_2'[F'(1-E_2-A)] \\ =&\beta u_2'[-F'(1-E_2-A)],\end{aligned} \tag{40}$$

using equation 34.

Using Cramer's rule, we solve the system of equations in 35 to find:

$$\left.\frac{\partial A^*}{\partial \alpha_2}\right|_{NREGS} = \frac{\begin{vmatrix} -\frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial A \partial \alpha_2} & \frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial A \partial E_2} \\ -\frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial E_2 \partial \alpha_2} & \frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial E_2^2} \end{vmatrix}}{\begin{vmatrix} \frac{\partial^2 U(N,k_b,k_g,E_1,E_2,A)}{\partial A^2} & \frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial E_2 \partial A} \\ \frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial E_2 \partial A} & \frac{\partial^2 U(k_b,k_g,E_1,E_2,A)}{\partial E_2^2} \end{vmatrix}} \tag{41}$$

Using equations 37 through 40, we find that the numerator in equation 41 is equal to 0. Therefore,

$$\left.\frac{\partial A^*}{\partial \alpha_2}\right|_{NREGS} = 0. \tag{42}$$

This proves **part (b) of Prediction 3**.

We denote the optimal dowry appropriation hours using $A^*\big|_{NREGS} = A^*\big|_N(k_b)$ and the optimal non-farm NREGS labor hours using $E_2^*\big|_{NREGS} = E_2^*\big|_N(k_b)$ to re-wirte the household's problem in equation 32 as:

$$\begin{aligned} \underset{E_1,k_b,k_g}{\text{maximize}}\ U =& u_1[\alpha_1 F(1-E_1) + sE_1 - 0.5k_bN - 0.5k_gN] \\ &+ \beta u_2[\alpha_2 F(1 - E_2^*\big|_N(kb) - A^*\big|_N(k_b)) + sE_2^*\big|_N + 0.5Nk_bBA^*\big|_N(k_b) - 0.5Nk_gG] \\ &+ \beta u_c[0.5k_bN + 0.5k_gN] \end{aligned} \tag{43}$$

The household's optimal health investment in its male children is given by:

$$\begin{aligned} \frac{\partial U(E_1,k_b,k_g)}{\partial k_b} &= -0.5Nu_1' + 0.5\beta N u_c' \\ +\beta u_2'[-\alpha_2 F'\{1 - E_2^*\big|_N(k_b) - A^*\big|_N(k_b)\}(E_2^*\big|_N'(k_b) + A^*\big|_N'(k_b)) &+ sE_2^*\big|_N'(k_b) + 0.5NBA^*\big|_N'(k_b)] = 0, \end{aligned} \tag{44}$$

and optimal health investment in its female children is given by:

$$\frac{\partial U(E_1, k_b, k_g)}{\partial k_g} = -0.5Nu_1' - 0.5\beta NGu_2' + 0.5\beta Nu_c' = 0. \tag{45}$$

Setting equations 44 and 45 equal to each other, we have:

$$-0.5NG = -\alpha_2 F'\{1 - E_2^*\big|_N(k_b) - A^*\big|_N(k_b)\}(E_2^*\big|_N'(k_b) + A^*\big|_N'(k_b)) + sE_2^*\big|_N'(k_b) + 0.5NBA^*\big|_N'(k_b) \tag{46}$$

The household's optimal labor hours in the non-farm NREGS sector is given by:

$$\frac{\partial U(E_1, k_b, k_g)}{\partial E_1} = u_1'[-\alpha_1 F'(1 - E_1) + s] = 0. \tag{47}$$

After the total differentiation of equations 44, 45, and 46 with respect to α_1, we get:

$$\begin{aligned}
&\frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_b^2}\frac{\partial k_b^*}{\partial \alpha_1} + \frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_b \partial k_g}\frac{\partial k_g^*}{\partial \alpha_1} + \frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_b \partial E_1}\frac{\partial E_1^*}{\partial \alpha_1} + \frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_b \partial \alpha_1} = 0,\\
&\frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_g \partial k_b}\frac{\partial k_b^*}{\partial \alpha_1} + \frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_g^2}\frac{\partial k_g^*}{\partial \alpha_1} + \frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_g \partial E_1}\frac{\partial E_1^*}{\partial \alpha_1} + \frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_g \partial \alpha_1} = 0,\\
&\frac{\partial^2 U(E_1, k_b, k_g)}{\partial E_1 \partial k_b}\frac{\partial k_b^*}{\partial \alpha_1} + \frac{\partial^2 U(E_1, k_b, k_g)}{\partial E_1 \partial k_g}\frac{\partial k_g^*}{\partial \alpha_1} + \frac{\partial^2 U(E_1, k_b, k_g)}{\partial E_1^2}\frac{\partial E_1^*}{\partial \alpha_1} + \frac{\partial^2 U(E_1, k_b, k_g)}{\partial E_1 \partial \alpha_1} = 0.
\end{aligned} \tag{48}$$

The second partial derivatives in equation 48 are as follows:

$$\frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_g^2} = (-0.5N)^2 u_1'' + \beta(-0.5NG)^2 u_2'' + \beta(0.5N)^2 u_c'' < 0, \tag{49}$$

as N, β and G are positive, and $u'' < 0$.

$$\frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_b^2} = (-0.5N)^2 u_1'' + (0.5N)^2 \beta u_c''$$
$$+\beta u_2''[-\alpha_2 F'\{1 - E_2^*\big|_N(k_b) - A^*\big|_N(k_b)\}(E_2^*\big|_N'(k_b) + A^*\big|_N'(k_b)) + sE_2^*\big|_N'(k_b) + 0.5NBA^*\big|_N'(k_b)]^2$$
$$+\beta u_2'[\alpha_2 F''\{1 - E_2^*\big|_N(k_b) - A^*\big|_N(k_b)\}](E_2^*\big|_N'(k_b) + A^*\big|_N'(k_b))^2], \quad (50)$$

as $F'''(.) = 0$.

$$\frac{\partial^2 U(E_1, k_b, k_g)}{\partial E_1^2} = u_1'[\alpha_1 F''(1 - E_1)] \quad (51)$$

$$\frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_b \partial k_g} = (-0.5N)^2 u_1'' + (0.5N)^2 \beta u_c''$$
$$+\beta u_2''[-\alpha_2 F'\{1 - E_2^*\big|_N(k_b) - A^*\big|_N(k_b)\}(E_2^*\big|_N'(k_b) + A^*\big|_N'(k_b)) + sE_2^*\big|_N'(k_b) + 0.5NBA^*\big|_N'(k_b)][-0.5NG] \quad (52)$$

$$\frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_b \partial E_1} = 0, \quad (53)$$

using equation 47.

$$\frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_g \partial E_1} = 0, \quad (54)$$

using equation 47.

$$\frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_b \partial \alpha_1} = -0.5N u_1'' F(1 - E_1), \quad (55)$$

$$\frac{\partial^2 U(E_1, k_b, k_g)}{\partial k_g \partial \alpha_1} = -0.5N u_1'' F(1 - E_1). \quad (56)$$

$$\frac{\partial^2 U(E_1,k_b,k_g)}{\partial E_1 \partial \alpha_1} = -u_1' F'(1-E_1), \tag{57}$$

using equation 47.

Substituting equation 46 in equation 50 and using equation 49 , we have:

$$\frac{\partial^2 U(E_1,k_b,k_g)}{\partial k_b^2} = \frac{\partial^2 U(E_1,k_b,k_g)}{\partial k_g^2} + \beta u_2' [\alpha_2 F'' \{1 - E_2^*\big|_N(k_b) - A^*\big|_N(k_b)\}](E_2^*\big|_N'(k_b) + A^*\big|_N'(k_b))^2]. \tag{58}$$

Substituting equation 46 in equation 51 and using equation 49 , we have:

$$\frac{\partial^2 U(E_1,k_b,k_g)}{\partial k_b^2} = \frac{\partial^2 U(E_1,k_b,k_g)}{\partial k_b \partial k_g}. \tag{59}$$

From equations 55 and 56, we have:

$$\frac{\partial^2 U(E_1,k_b,k_g)}{\partial k_g \partial \alpha_1} = \frac{\partial^2 U(E_1,k_b,k_g)}{\partial k_b \partial \alpha_1}. \tag{60}$$

Using Cramer's rule, we solve the system of equations in 48 to find:

$$\frac{\partial k_g^*}{\partial \alpha_1}\bigg|_{NREGS} = \frac{\begin{vmatrix} \frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_b^2} & -\frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_b \partial \alpha_1} & \frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_b \partial E_1} \\ \frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_g \partial k_b} & -\frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_g \partial \alpha_1} & \frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_g \partial E_1} \\ \frac{\partial^2 U(k_b,k_g,E_1)}{\partial E_1 \partial k_b} & -\frac{\partial^2 U(k_b,k_g,E_1)}{\partial E_1 \partial \alpha_1} & \frac{\partial^2 U(k_b,k_g,E_1)}{\partial E_1^2} \end{vmatrix}}{\begin{vmatrix} \frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_b^2} & \frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_b \partial k_g} & \frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_b \partial E_1} \\ \frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_g \partial k_b} & \frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_g^2} & \frac{\partial^2 U(k_b,k_g,E_1)}{\partial k_g \partial E_1} \\ \frac{\partial^2 U(k_b,k_g,E_1)}{\partial E_1 \partial k_b} & \frac{\partial^2 U(k_b,k_g,E_1)}{\partial E_1 \partial k_g} & \frac{\partial^2 U(k_b,k_g,E_1)}{\partial E_1^2} \end{vmatrix}} \tag{61}$$

Using equations 53 through 60, we solve equation 61, to get:

$$\left.\frac{\partial k_g^*}{\partial \alpha_1}\right|_{NREGS} = \frac{-\frac{\partial^2 U(E_1,k_b,k_g)}{\partial k_b \partial \alpha_1}}{\frac{\partial^2 U(E_1,k_b,k_g)}{\partial k_g^2}} = \frac{0.5Nu''F(1-E_1)}{\frac{\partial^2 U(E_1,k_b,k_g)}{\partial k_g^2}} > 0, \tag{62}$$

as $u'' < 0$, $N > 0$, $F(1) > 0$, and the denominator is negative from equation 49.

We then compare equations 6 and 62. The denominator for both are the same. If $s > 0$, then $E_1 > 0$ and the numerator in 62 is smaller than in 6. Therefore,

$$\frac{\partial k_g^*}{\partial \alpha_1} > \left.\frac{\partial k_g^*}{\partial \alpha_1}\right|_{NREGS} \tag{63}$$

This proves **part (a) of Prediction 3**.

Appendix: Results

Table A1: Rainfall Robustness

	Rainfall Robustness				NREGS	
	Model 1	Model 2	Model 3	Model 4	All Years	2005-2009
Year of birth rainfall (Z)	0.026***					
	(0.007)					
Rain <-2		−0.085***				
		(0.029)				
Rain between -1 and -2		−0.050**				
		(0.024)				
Rain between 0 and -1		−0.045**				
		(0.019)				
Rain between 0 and 1		−0.024				
		(0.019)				
Rain between 1 and 2		−0.010				
		(0.020)				
Good year (Z>1)			0.027*			
			(0.015)			
Ordinal rainfall (cuts -1 and 1)				0.021**	0.060*	0.073
				(0.010)	(0.035)	(0.046)
NREGS times Ordinal rainfall					−0.037	−0.051
					(0.037)	(0.045)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Household Vars	Yes	Yes	Yes	Yes	Yes	Yes
State/Year of Birth FE	Yes	No	No	No	No	No
Observations	65791	65791	65791	65791	88547	38451

Standard errors are in parentheses and are clustered at the district level. The first column repeats results from Table 2 but adds state by wave fixed effects. Column two creates "bins" of rainfall. Column three uses a simple dummy variable equal to one if rainfall is greater than $Z = 1$. Column four defines an ordinal variable, similar to Jayachandran (2006).
* p<0.1 ** p<0.05 *** p<0.01

Table A2: NREGS and Bargaining Power I

	Alcohol		Cigarettes	
	Any	Log (R's + 1)	Any	Log (R's + 1)
NREGS times Post	−0.048	−0.144	−0.010	0.099
	(0.065)	(0.622)	(0.066)	(0.480)
District FE	Yes	Yes	Yes	Yes
District Vars	Yes	Yes	Yes	Yes
Household Vars	Yes	Yes	Yes	Yes
Observations	8296	8296	8296	8296

Standard errors are in parentheses and are clustered at the district level. All regressions use the ARIS/REDS, collected in 1999 and 2008. The dependent variable (DV) in column one is a dummy variable for whether a household purchased any alcohol. In column two, the DV is amount spent on alcohol (log of rupees plus one). In column three, the DV is a dummy variable for cigarette purchases, while the DV in column four is total spent on cigarettes (log of rupees plus one).
* p<0.1 ** p<0.05 *** p<0.01

Table A3: NREGS and Bargaining Power II

	Girl Clothing Exp. Percent (log R's + 1)		Girl Education Exp. Percent	
	All	Both	Both	All
NREGS times Post	0.080**	−0.011	0.015	−0.035
	(0.039)	(0.025)	(0.046)	(0.037)
District FE	Yes	Yes	Yes	Yes
District Vars	Yes	Yes	Yes	Yes
Household Vars	Yes	Yes	Yes	Yes
Observations	5634	2928	4734	2027

Standard errors are in parentheses and are clustered at the district level. All regressions use the ARIS/REDS, collected in 1999 and 2008. The dependent variable (DV) in column one is total spent on girls' clothing as a percentage of total children's clothing purchases. In column two, the DV restricts the sample to only households that purchased both girls' and boys' clothing. In columns three and four, the DV is similary defined but for education expenditures instead of clothing.
* p<0.1 ** p<0.05 *** p<0.01

Table A4: Rainfall, Household Consumption, and NREGS

	Total Consumption	Food Consumption
Rainfall times NREGS times Post	−0.243***	−0.194**
	(0.081)	(0.080)
District FE	Yes	Yes
District Vars	Yes	Yes
Household Vars	Yes	Yes
Observations	7996	7992

Standard errors are in parentheses and are clustered at the district level. All regressions use the National Sample Survey and are at the district/year level. The dependent variable in the first column is (log of) number of newborn girls, defined as girls under one year of age. The dependent variable in the second column is (log of) number of newborn boys.
* p<0.1 ** p<0.05 *** p<0.01

Table A5: Rainfall and Number of Newborns at District/Year Level

	Girls	Boys
Current rainfall (Z)	0.044**	0.012
	(0.020)	(0.019)
District FE	Yes	Yes
Year FE	Yes	Yes
District Vars	Yes	Yes

Standard errors are in parentheses and are clustered at the district level. All regressions use the National Sample Survey and are at the district/year level. The dependent variable in the first column is (log of) number of newborn girls, defined as girls under one year of age. The dependent variable in the second column is (log of) number of newborn boys.
* p<0.1 ** p<0.05 *** p<0.01

Table A6: Testing the Parallel Trends Assumption

	Female	HAZ		
	All	Female	Male	All
Phase=2 times Rainfall times Years to NREGS	0.003	−0.035	−0.095**	0.015
	(0.005)	(0.044)	(0.044)	(0.038)
Phase=3 times Rainfall times Years to NREGS	0.000	−0.018	−0.062*	0.019
	(0.004)	(0.038)	(0.034)	(0.026)
Phase=2 times Rainfall times Years to NREGS times female				−0.062
				(0.042)
Phase=3 times Rainfall times Years to NREGS times female				−0.043
				(0.034)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District Vars	Yes	Yes	Yes	Yes
Household Vars	Yes	Yes	Yes	Yes
Observations	57529	9607	6975	19939

Standard errors are in parentheses and are clustered at the district level. Columns one and two use the National Sample Survey and the dependent variable is whether a newborn is female. Columns three and four use the IHDS and the dependent variable is height-for-age Z score. Columns five and six use the National Crime Records and the dependent variable is number of dowry deaths. The Proxy NREGS variable is defined similarly to the NREGS variables in the prior tables, but with implementation date moved up one year (e.g. assuming phase one districts received the program in 2005 instead of 2006).
* p<0.1 ** p<0.05 *** p<0.01