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Essays in Development Economics

Dale B D Pereira

A dissertation submitted to the University of Bristol in accordance with the requirements for award of the degree of Doctor of Philosophy in the Faculty of Social Sciences and Law, School of Economics.

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Abstract

This thesis consists of three chapters which explore research questions pertaining to the welfare of children and women in India. In the first chapter, I study the effect of prenatal exposure to the low-intensity Naxalite conflict in India on neonatal mortality. Using a mother fixed-effects approach, I find that exposure to conflict in the second trimester of pregnancy increases the probability that a child will die in the first month of life by 0.09 percentage points, relative to his/her non-exposed siblings. In addition, I find suggestive evidence that an overall decline in the fetal health distribution ('scarring') and limited access to healthcare services at the time of delivery could explain the observed effect. The results are robust to a range of specifications but appear to be driven by districts with relatively higher levels of conflict activity.

In the second chapter, I analyse panel data on a sample of Indian children using a mother fixed-effects approach and explore the relationship between birth order and height (height-for-age z-scores) as children age. The main estimates confirm that birth order is negatively associated with initial height-for-age z-scores. Further, I find that although second-born children exhibit some catch-up growth relative to their firstborn siblings as they age, the negative birth order gradient in height-for-age z-scores is persistent in nature. Heterogeneity analysis suggests that the main results are largely unaffected by differences in family size, son-preference and maternal education levels.

In the third chapter, I examine the impact of India's large rural workfare programme, the National Rural Employment Guarantee Scheme (NREGS), on women's empowerment. Using village-level availability of an active NREGS as an instrument, I find that household participation in the scheme is associated with a significant increase in the empowerment index that I construct. The effect appears to be driven by improvements in indicators capturing women's access to resources and freedom of mobility. I do not, however, find evidence of an improvement in indicators reflecting women's autonomy in the intra-household decision-making process.

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Author's declaration

I declare that the work in this dissertation was carried out in accordance with the requirements of the University's Regulations and Code of Practice for Research Degree Programmes and that it has not been submitted for any other academic award. Except where indicated by specific reference in the text, the work is the candidate's own work. Work done in collaboration with, or with the assistance of, others, is indicated as such. Any views expressed in the dissertation are those of the author.

SIGNED: DATE:

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Introduction

This thesis contains three chapters that broadly contribute to the field of development economics. The first two chapters explore research questions related to child health in India, whereas the third chapter looks at the welfare of women in rural India.

Chapter 1 is concerned with the well-being of children in conflict affected regions. In this chapter, I examine the effect of in utero exposure to the Naxalite conflict on neonatal mortality. The Naxalite conflict is an ongoing low-intensity armed insurgency active in several central and eastern Indian states. To identify the effect of interest, I employ a mother fixed-effects approach which exploits the variation in utero conflict exposure across siblings born to the same mother. The estimated effect is therefore robust to any selection on unobserved time-invariant maternal characteristics.

The main findings suggest that in utero exposure to conflict in the second-trimester of pregnancy increases the probability that a child will die in the first month of life by 0.09 percentage points. Further, I find suggestive evidence that the observed effect could potentially be explained by an overall decline in the fetal health distribution ('scarring') and a reduction in the use of health services at the the time of delivery. While the results are robust to several potential threats to validity, they appear to be driven by districts characterised with relatively higher levels of conflict activity.

Chapter 2 explores the relationship between birth order and child health. Specifically, I use a panel sample of Indian children to examine how birth order effects on child height evolve as children in the sample age. The identification strategy uses a mother fixed-effects approach which estimates the effect of interest using sibling comparisons, holding constant unobserved total fertility.

In line with the existing literature on birth order effects in developing countries, I find a strong later-born disadvantage in initial height for children aged 0 to 11 years in the first survey wave. Further, while the results indicate that second-born children catch-up relative to their first-born siblings, the estimated effect is not sufficient to offset the birth order height gap. The main result is robust to using i) an alternative measure of birth order and ii) a sample of children born to mothers who can be assumed to have completed childbearing. Moreover, the observed pattern of birth order effects largely hold across family sizes, families with different levels of son preference, various stages of a child's growth development, and maternal education levels.

Chapter 3 shifts the focus of the thesis to the welfare of women in rural India. In this chapter, I use panel data on a sample of rural women to study the effect of a large public workfare programme in India known as the National Rural Employment Guarantee Scheme (NREGS), on women's empowerment. NREGS provides rural households with up to 100 days of wage employment on demand. Moreover, the scheme contains specific provisions that seek to encourage the participation of women, and therefore may have indirect implications for the welfare of women in rural areas.

This chapter focuses on the effect of household participation in NREGS on women's empowerment. To quantify the scheme's impact on empowerment, I construct a women's empowerment index using sub-indices which measure women's self-assessed status with reference to access to resources, freedom of mobility, and autonomy in the intra-household decision-making process. Using village-level availability of an active NREGS as an instrument, I find that household participation in the scheme is associated with an increase in the women's empowerment index. The effect appears to be driven by improvements in sub-indices which capture women's access to resources and freedom of mobility. I do not find evidence of an improvement in women's say in the intra-household decision making process. Further, I find that the scheme is associated with improvements in empowerment regardless of which household member participates in the scheme, and irrespective of the woman's labour force participation history.

Chapter 1

Armed conflict and neonatal mortality: Evidence from the Naxalite insurgency in India

1.1. Introduction

Armed conflicts often have detrimental consequences which extend beyond the immediate destruction of a country's human and physical capital. They can impose significant costs on the health and well-being of local populations who face an increased risk of morbidity and mortality on account of direct exposure to violence, psychological distress, disruption of key services, and forced migration. In many cases, these costs tend to be disproportionately borne by women and children living in conflict affected regions (Bendavid et al., 2021). Moreover, the damaging effects of conflict may not be restricted to the duration of conflict, and might have long-term consequences for the welfare of the affected populations.

In this chapter, I investigate the effect of in utero exposure to conflict on neonatal mortality defined as death in the first month of life. Conflict can negatively impact infant health both directly and indirectly. Exposure to conflict settings can induce physical and psychological stress in pregnant women, which may lead to declines in fetal health, and in turn result in poor health at birth. Conflict may also limit access to health care services due to the destruction of infrastructure or lack of investment in equipment and human resources. This can lead to the deterioration of infant health through its impact on conditions surrounding delivery and on access to antenatal and postnatal care. Lastly, areas affected by conflict may experience disruptions in vital services including food supply, water, sanitation and social welfare schemes which can reinforce the negative effects of conflict on maternal and infant health.

This study focuses on the Naxalite conflict which is an ongoing low-intensity armed insurgency active in several central and eastern Indian states. In the last two decades alone, the Naxalite conflict has claimed the lives of over 12,000 civilians, insurgents, and security personnel.[1] In addition to the deaths, Naxalites are involved in the destruction of public infrastructure (government buildings, railway lines, telephone exchanges, and public transport), imposing both economic and social costs on the vulnerable populations living in areas with active Naxal conflict.

In this study, I use survey data to estimate the effect of in utero exposure to Naxal related conflict deaths in the mother's district of residence, in each trimester of pregnancy, on neonatal mortality. To identify the effect of interest, I use a mother fixed-effects approach which exploits the presence of substantial variation in prenatal conflict exposure across siblings born to the same mother. The resulting estimates are robust to any selection on unobserved time-invariant maternal characteristics correlated with both conflict exposure and the outcome of interest.

This analysis makes the following contributions to literature. First, it is one of the few empirical studies which analyses the effect of the Naxalite conflict, specifically, on child health outcomes. The only related existing empirical work uses data on a single cohort of children in Andhra Pradesh to examine the effect of Naxalite conflict exposure on child nutritional status (Tranchant et al., 2014). The authors employ an instrumental variables strategy and find that exposure to the Naxalite conflict does not have an independent effect on a child's height-for-age z-score.[2] Adding to this strand of literature, I examine the effect of the Naxalite conflict on neonatal mortality as an indicator of infant health. Relative to Tranchant et al. (2014), this study extends the geographic area under analysis to cover several states with persistent Naxalite activity. Further, this study exploits the availability of data on siblings to employ a mother fixed-effects approach which is able to account for the potential selection on time-invariant unobservables of women into

[1] Source: Data from Ministry of Home Affairs, Government of India.

[2] The authors find that violence has adverse effects on child nutrition only when it reduces a household's ability to cope with drought.

pregnancy during periods of conflict.

Second, this chapter adds to the limited empirical literature on the effect of conflict on child outcomes in India. There are three main active conflicts in India: the Kashmir insurgency, insurgencies and ethnic conflicts in multiple states in Northeast India, and the Naxalite conflict. These insurgencies differ in terms of their core objectives, frequency and type of violence involved, and targets, which could lead to differences in whether and how these insurgencies impact child outcomes. Some of the existing empirical research uses a difference-in-differences approach, exploiting the temporal variation in insurgent activity in Kashmir to identify its effect on child human capital (Parlow, 2011, 2012). The author finds that children severely affected by the insurgency are shorter and less likely to complete primary schooling relative to less affected children. Other research finds no effect of the Naxalite conflict on children's height-for-age z-scores in Andhra Pradesh (Tranchant et al., 2014). Adding to this strand of literature, this study explores the potential impact of the Naxalite conflict on early child health as measured by neonatal mortality. Neonatal mortality is a relatively under-studied outcome in the existing conflict literature on India, and the findings could have implications for understanding the effect of conflict on the health of surviving children.

Third, this chapter contributes more generally to the large body of literature on the impact of armed conflict on child health outcomes. Several studies show that in utero and early life exposure to armed conflict largely has a negative impact on child nutrition (Bundervoet et al., 2005, 2009; Akresh et al., 2011; Serdan, 2010; Minoiu and Shemyakina, 2012). Further, existing research examining the relationship between armed conflict and infant mortality consistently finds that conflict-ridden areas have higher child mortality rates (Guha-Sapir and van Panhuis, 2004; Guha-Sapir et al., 2005; De Walque, 2005; Guha-Sapir and D'Aoust, 2011).

Closer in scope to this study is the related strand of literature examining the effect of intrauterine conflict exposure on early child health. Camacho (2008) examines the impact of prenatal exposure to landmine explosions in Colombia on birth weight. She finds that exposure to landmine explosions in early pregnancy is associated with a reduction of 8.7 grams in birth weight. Similarly, Brown (2020) shows that in utero exposure to the 9/11 terrorist attacks, particularly in the first trimester of pregnancy, increases the probability that a child will be born premature and have low birth weight.

Dagnelie et al. (2018) document the effect of exposure to civil war in utero and in the first year of life on infant mortality in the Democratic Republic of Congo. Using an instrumental variables strategy, the authors find that exposure to conflict events is associated with significantly increases in infant mortality only among girls. This chapter departs from these studies by exploring the impact of a persistent low-intensity conflict setting rather than civil wars or one-off terrorist events, on a relatively under-studied outcome of early child health. Further, as noted by earlier studies, landmine explosions were not reflective of the conflict intensity of the Colombian civil war (Mansour and Rees, 2012; Quintana-Domeque and Ródenas-Serrano, 2017).

A related paper by Mansour and Rees (2012) uses data from the Palestinian Demographic and Health Survey to study the effect of intrauterine exposure to fatalities from the al-Aqsa Intifada on birth weight. The authors find that exposure to an additional fatality in the first trimester of pregnancy is associated with a modest increase in the probability of a child being born with low birth weight. While this chapter is methodologically similar to Mansour and Rees (2012), it benefits from a larger sample size to estimate the effect of exposure to armed conflict on child mortality in India.

A more recent related study examines the effect of a sustained terrorist conflict in Spain on health outcomes at birth (Quintana-Domeque and Ródenas-Serrano, 2017). The authors find that exposure to terrorism as measured by bombing casualties, in the first trimester of pregnancy, leads to a reduction in birth weight and the fraction of 'normal' babies, and an increase in the probability of low birth weight children. Although similar in scope, this chapter extends the understanding of the effect of low-intensity conflict exposure on child mortality outcomes to a developing country context. Further, this study identifies the effect of conflict exposure using a mother fixed-effects approach, and is therefore able to account for selection on time-invariant unobserved maternal characteristics that may be correlated with conflict exposure as well as the outcomes of interest.

The main results suggest that in utero exposure to conflict deaths in the second trimester of pregnancy increases neonatal mortality by 0.09 percentage points, holding constant unobserved maternal heterogeneity. Further, I investigate potential mechanisms for the observed effect and identify two plausible explanations. First, I find that in utero conflict exposure is associated with increases in stillbirth. In addition, there is some evidence to indicate that in utero conflict exposure results in increased neonatal mortality for boys relative to girls. Taken together, these results suggest that worse early childhood health could be the result of a decline in the overall fetal health distribution caused by a dominant in utero scarring mechanism. This finding adds to the limited understanding on how in utero selection and scarring mechanisms impact fetal health in conflict settings (Valente, 2015; Quintana-Domeque and Ródenas-Serrano, 2017; Dagnelie et al., 2018). Second, I find suggestive evidence that exposure to conflict could increase neonatal mortality by restricting access to health care, and thereby affecting conditions surrounding delivery and postnatal care.

The remainder of this chapter is organised as follows. Section 1.2 gives some background on the Naxalite conflict. Section 1.3 describes the main data used in the analysis, and provides descriptive statistics. Section 1.4 presents the empirical strategy, and Section 1.5 reports the main results. Section 1.6 investigates potential mechanisms. Section 1.7 presents robustness checks, and Section 1.8 concludes.

1.2. The Naxalite Conflict

The Naxalite movement is an ongoing low-intensity armed conflict between insurgent groups known as Naxalites and the Government of India.[3] The movement, based on communist ideology, originated in 1967 in a remote village (Naxalbari) in West Bengal in response to a land dispute between tribal villagers and local landlords. Over the next three decades, the movement spread to other parts of the country but eventually saw a decline in activity due to retaliation by state forces as well as ideological differences among its many sub-factions.

The movement resurfaced in 2004 when two major Naxalite groups; People's War Group (PWG) and Maoist Communist Centre (MCC) merged to form the movement's largest operating faction known as the Communist Party of India-Maoist (CPI-M). The merger also resulted in the formation of an armed wing; People's Liberation Guerilla Army, leading to an upsurge in violence between Naxalites and the police forces. In 2009 and 2010, the conflict further intensified in response to the launch of an anti-Naxal task force (Left Wing Extremism Division) set up by the government.[4]

[3] The Naxalite conflict is classified as a low-intensity armed conflict based on the number of reported conflict fatalities. However, the intensity of the Naxalite conflict varies substantially over time and across geographic area.

[4] This period saw the highest recorded casualties in the history of the Naxal movement.

The geographic coverage of the Naxalite movement has fluctuated greatly over the years. In 2008, Naxals were active in 223 districts across 20 states whereas, in 2015, they were active in 106 districts across 10 states. As of 2017, the Naxals were active in 126 districts across 11 states.[5] Despite the changing coverage of the movement, the CPI-M and other Naxal sub-factions have a strong presence in the country's 'Red Corridor' which includes the following central and eastern states: Andhra Pradesh, Telangana, Odisha, Jharkhand, Bihar, Chhattisgarh, Uttar Pradesh, Maharashtra, Karnataka, Madhya Pradesh, and West Bengal. Within these states, the insurgency is believed to be stronger in remote districts characterised by low levels of human development and urbanisation as well as limited access to infrastructure (Ghatak and Eynde, 2017). This often includes dense forest areas which lack access to paved roads, electricity, and health care facilities. Figure 1.1 displays the variation in district-wise conflict fatalities across the Red Corridor states for the period between 2009 and 2016.

The intensity of Naxal related violence, like its geographic coverage, has varied substantially over the years. Recent work suggests that conflict intensity in districts is strongly correlated with factors like rainfall shocks and mining activity (Gawande et al., 2017; Vanden Eynde, 2018; Shapiro and Eynde, 2020). Figure 1.2 shows the monthly variation in the total number of fatal incidents and resulting fatalities in Naxal affected districts between 2009 and 2016.[6] It is estimated that between 2005 and 2018, Naxalite related incidents have resulted in at least 8000 fatalities (civilian, security forces, and insurgents), and the displacement of hundreds of thousands of people.[7] While government data indicate a steady decline in Naxal related incidents and fatalities since 2011, more recent reports show that the Naxalite movement remains active with changes in its intensity and geographic coverage.[8]

[5]The statistics vary between sources and across years, and do not follow an obvious trend.

[6]Descriptive figures on the monthly variation in conflict fatalities at the district level are provided in Section 1.A.2 in the Appendix.

[7]The Internal Displacement Monitoring Centre suggests that roughly 120,000 people were internally displaced in 2009 and 2010 due to the Naxalite conflict.

[8]Source: Ministry of Home Affairs, Government of India.

1.3. Data & Descriptive Statistics

1.3.1. Data

The data on infant health outcomes is taken from the fourth round of the Demographic and Health Surveys (DHS) conducted in India between 2015 and 2016. The DHS Program has been carried out in several developing countries with the aim of providing detailed data on demographic topics including marriage, fertility, reproductive health, family planning, and maternal and child nutrition.

The DHS includes a fertility module which collects a retrospective history of reproductive events for women aged 15 to 49 for a period of between 5 and 7 years preceding the survey. The interviewed women are asked to provide the dates of all their pregnancies and resulting live births, as well as the dates of any terminations. Further, for each live birth, the survey records the incidence and dates of any child deaths, and collects information on prenatal care, postnatal care, and anthropometric indicators. The retrospective nature of this data may lead to some measurement error in the outcomes of interest due to possible recall bias. To address this, I restrict the analysis in this study to children born not more than 6 years prior to the date of the interview. Moreover, it has been shown that the calendar method used by the DHS in retrospective surveys to collect fertility histories improves the quality of data collected relative to traditional methods of data collection (Goldman et al., 1989; Becker and Sosa, 1992; Becker and Diop-Sidibé, 2003).[9]

For the main analysis in this chapter, I use the interviewed women's fertility history and the available data on live births to construct a panel dataset in which mothers represent the cross sectional dimension and live births represent the time dimension. I then impose several sampling restrictions. First, I restrict the analysis to states with active Naxalite conflict between 2009 and 2016 as all pregnancies recorded in the data occur during this period.[10] The resulting sample consists of all live births in Naxalite affected states conceived no more than 6 years prior to the month of interview. Second, I drop all

[9] In traditional surveys collecting retrospective data, respondents are asked distinct questions about specific events. Whereas, in the calendar method, retrospective data is gathered on a month by month basis for a specific duration using the inherent hierarchy of respondents' memory. Existing research suggests that calendar methods are more effective at collecting retrospective data relative to traditional methods when the recall task is more complex (Van der Vaart, 2004; Van Der Vaart and Glasner, 2007).

[10] Uttar Pradesh is an active but low-intensity conflict zone between 2009 and 2016. The state is dropped from the sample because the sample of mothers from Uttar Pradesh reside in districts not affected by conflict.

pregnancies starting less than 9 months before the date of the interview because their in utero history is incomplete.[11] Third, I restrict the sample to singleton births as multiple births are known to be associated with a higher mortality risk (Bhalotra and Van Soest, 2008).[12] Fourth, for the analysis of neonatal mortality, I drop live births that did not occur at least a full month before the date of the interview. This allows every live birth in the sample to have full exposure to neonatal mortality risk. The resulting sample contains 106,512 children born to 78,800 mothers.

The main outcome of interest is a binary indicator for neonatal mortality. Neonatal mortality refers to death in the first 28 days of life. A potential issue with retrospective data on neonatal mortality is that a child's completed age at death may be heaped on certain ages (Beckett et al., 2001; Pullum et al., 2013). For instance, respondents may report a child's age at death as one month for deaths occurring within the first month. To account for this, I follow Bhalotra and Van Soest (2008) to extend the definition of neonatal mortality to include deaths which occur up to the first month of life. Consequently, the neonatal mortality indicator equals 1 if the child's age is recorded to be up to 1 month at the time of death; and 0 otherwise.

To construct explanatory variables which reflect a child's in utero exposure to the Naxalite conflict, I use conflict data obtained from the South Asia Terrorism Portal (SATP). The SATP records information on Naxalite-related incidents from local and national English language newspapers reports. This information includes the date and location (district) for each incident as well as the number and type of fatalities.[13] However, the SATP data is likely to under-represent the total number of conflict events for two main reasons. First, the SATP uses only English language newspaper reports which tend to be urban in nature and are, therefore limited in the range of their coverage. Second, the level of media coverage varies by geographic areas. While some areas receive poor coverage on account of being remote or severely affected by conflict, other areas may receive higher coverage for political or economic reasons. Despite these limitations, the SATP has been found to consistently record more Naxalite-related incidents relative to other publicly available conflict databases like the Global Terrorism Database (GTD) or the Uppsala Conflict Data Program (UCDP) (Wischnath and Buhaug, 2014; Behlendorf et al., 2016; Gawande

[11]This includes ongoing pregnancies as well as pregnancies that are aborted or end in miscarriage, stillbirth or live birth.

[12]Multiple births account for 1.7% of all live births in the sample.

[13]Security forces, insurgents, and civilians.

et al., 2017). Moreover, the SATP data is assumed to reflect the conflict trends between districts and is widely used in articles, reports, and academic literature related to the Naxalite conflict.

I use the SATP data to construct a monthly conflict events dataset at the district level. I restrict the data to include Naxalite conflict events occurring during the period 2009 to 2016 in Naxalite affected states. I merge the resulting dataset with the cleaned DHS sample using the reported start date (month and year) of pregnancy to obtain the final data for analysis. Next, I construct variables which reflect an interviewed woman's exposure to conflict in each trimester of pregnancy. $Conflict^{T_1}$ is the total number of deaths in the *first trimester* or the 3 months following the reported start of a given pregnancy in the mother's district of residence.[14] Similarly, $Conflict^{T_2}$ and $Conflict^{T_3}$ are the total number of deaths occurring between months 4 and 6 and months 7 and 9, respectively, following the reported start of pregnancy.

1.3.2. Descriptive statistics

Table 1.1 displays summary statistics for the main variables used in the analysis. For the purpose of this table, I divide the main sample into two sub-samples based on the presence of conflict in a district. A district is classified as a 'non-conflict district' if there is no record of any Naxal-related fatalities in that district between 2009 and 2016. On the other hand, a district is classified as a 'conflict district', if at least one Naxal-related fatality occurred in that district between 2009 and 2016. Observations in the 'conflict district' sample are further divided based on exposure to prenatal conflict fatalities.

Within conflict districts, there appears to be no significant difference in neonatal and infant death rates, and birth weight between children based on prenatal exposure to conflict. However, non-exposed children in conflict districts experience a 1 percentage point higher probability of being born with low birth weight relative to exposed children. Further, children with prenatal conflict exposure are more likely to be born in rural households to less educated, lower caste (Scheduled Tribe) mothers relative to their non-

[14] In the absence of gestational length, existing literature identifies the trimesters of pregnancy by counting backwards from the date of birth of the child (Camacho, 2008; Mansour and Rees, 2012; Le and Nguyen, 2020*a*). However, for the main analysis, I define trimester-based exposure to conflict by counting forward from the reported start date of pregnancy assuming a 9 month gestation period. This is likely to be subject to some measurement error but is done to ensure consistency when analysing the effect of in utero conflict exposure on both fetal loss and outcomes related to live births. Moreover, a child's date of birth is likely to be endogenously determined as a function of conflict intensity.

exposed counterparts. Lastly, mothers are marginally older and the total number of siblings are marginally higher for children with prenatal conflict exposure.

1.4. Empirical Strategy

The baseline specification is:

$$Y_{imdt} = \alpha + \sum_{j=1}^{3} \beta_j conflict_{dt}^{T_j} + \gamma X_{imdt} + \theta_t + \lambda_d + \epsilon_{imdt} \tag{1.1}$$

where Y_{imdt} is the outcome of interest for child i conceived to mother m residing in district d in month t. $Conflict_{dt}^{T_j}$ is the total number of conflict deaths that a mother is exposed to in her district of residence d during trimester of pregnancy T_j. This captures child i's prenatal exposure to conflict. X_{imdt} is a vector of maternal characteristics and child specific controls. This includes birth order, sex, mother's age at conception, mother's education (in years), and a dummy for rural households. θ_t is a set of year-month of conception fixed-effects. λ_d represents district fixed-effects which capture time-invariant unobserved heterogeneity across districts. ϵ_{imdt} is the error term, clustered at the district level.

Equation (1.1) uses within-district variation in in utero exposure to conflict to identify the coefficients of interest, namely, β_1, β_2, and β_3. A comparison of these coefficients indicates the stage of pregnancy in which exposure to conflict likely has an impact on the outcomes of interest. However, the estimates obtained from equation (1.1) may be biased if there are differences in the composition of women becoming pregnant or giving birth during periods of conflict.

Women who become pregnant during periods of conflict are likely to differ from women who do not become pregnant during periods of conflict. Some of these potential differences like maternal age and maternal education are observable, and are included as controls in the specification. Other possible sources of maternal heterogeneity are unobservable such as those arising from biological mechanisms related to reproductive ability, sex-specific reporting of outcomes of interest, or fertility responses to (predicted) conflict. Not accounting for unobserved maternal differences may lead to biased estimates if these differences simultaneously affect the composition of women becoming pregnant during times of conflict as well as the outcomes of interest. For instance, if mothers who prioritise health are more likely to postpone pregnancy during periods of conflict, and if these mothers are also more likely to have healthier children owing to relative improvements in pregnancy conditions, then exposure to conflict could be positively correlated with worse child health outcomes due to unobserved maternal heterogeneity.

To address the bias that may result from the selection on unobservable characteristics of women into pregnancy during conflict, I restrict the analysis to a subsample of siblings to estimate the following mother fixed-effects specification:

$$Y_{imdt} = \alpha + \sum_{j=1}^{3} \beta_j conflict_{dt}^{T_j} + \gamma X_{imdt} + \theta_t + \lambda_m + \epsilon_{imdt} \quad (1.2)$$

where λ_m represents mother fixed-effects which captures time-invariant maternal heterogeneity. The availability of data on two or more children born to the same mother for a subsample of mothers allows for the inclusion of mother fixed-effects.[15] Equation (1.2) exploits the variation in prenatal exposure to conflict across siblings born to the same mother to estimate the effect of in utero exposure to conflict on the outcomes of interest. This is the preferred specification as the resulting estimates will be robust to the selection of women into pregnancy during conflict based on time-invariant unobserved maternal characteristics.

1.5. Main Results

1.5.1. Neonatal mortality

Table 1.2 presents the estimated impact of exposure to conflict in each trimester of pregnancy on neonatal mortality. Column (1) reports regression results from estimating equation (1) on the full sample but includes only year-month of conception fixed-effects. The estimates suggest that an additional conflict death in the mother's district of residence in the third trimester of pregnancy reduces the probability that a child will die in the first month of life by 0.09 percentage points (ppt). The results remain largely unchanged when I add controls for maternal and child characteristics to the specification in column (2).

[15] Here, the analysis will be necessarily restricted to a subset of siblings, where a mother fixed-effect is identified. This will be referred to as the sibling sample. Summary statistics for the sibling sample are provided in Table 1.A.1 in the Appendix.

A reduction in neonatal mortality in response to in utero shocks can be driven by a positive selection of fetuses into live births. In utero shocks can increase fetal mortality either by reducing the mean of fetal health ("scarring"), or by raising the threshold of survival to birth ("culling"). The relative strength of these two mechanisms determines the health endowment at birth. The observed within-district reduction in neonatal mortality in columns (1) and (2) indicates an improvement in health outcomes at birth, and might therefore be indicative of a dominant selection effect in utero in response to conflict exposure.

Alternatively, the observed within-district estimates could be driven by unobserved heterogeneity in the composition of women who give birth during periods of conflict. To address this potential source of bias, I run a mother fixed-effects specification which produces impact estimates by comparing siblings who were exposed to prenatal conflict with those who were not. In addition, in column (3) of Table 1.2, I report within-district estimates for the sibling sample to assess whether children who have at least one sibling in the sample are differently affected by conflict exposure relative to the full sample. While the results for the sibling sample are qualitatively similar when compared to the results in columns (1) and (2), the coefficient for exposure to conflict in the third trimester of pregnancy is no longer significant.

The results from the mother fixed-effects specification are reported in column (4). These estimates suggest that exposure to an additional conflict fatality in the district in the second trimester of pregnancy leads to a 0.09 ppt increase in the probability that a child will die in the first month of life relative to his sibling. The coefficient for conflict exposure in the third trimester of pregnancy suggests a larger negative relationship between exposure to conflict deaths in utero and neonatal mortality, relative to the within-district estimates. However, the estimate is not statistically significant. The direction of change in the mother fixed-effects estimates relative to the within-district estimates is consistent with women self-selecting into pregnancy during conflict based on unobserved characteristics. For instance, it could be that women who are more reactive to stress are also less likely to conceive as conflict intensity increases. Alternatively, there could be systematic differences in health behaviours of women who self-select into pregnancy during periods of intense conflict. Therefore, the results in Table 1.2 highlight the need to control for time-invariant unobserved maternal heterogeneity to more accurately identify the effect of in utero conflict exposure on child outcomes.

The main finding from Table 1.2 is that once unobserved maternal heterogeneity is accounted for, exposure to conflict in the second trimester of pregnancy appears to have a detrimental impact on neonatal mortality relative to the other trimesters. This result is in line with the findings of Savitz et al. (1993) and Hernández-Julián et al. (2014) who report increases in neonatal mortality in response to conflict in Vietnam and in utero famine shocks in Bangladesh respectively. Additionally, it is consistent with existing literature which links armed-conflict to increased infant mortality (De Walque, 2005; Guha-Sapir et al., 2005; Dagnelie et al., 2018).

The magnitude of the estimated effect implies that on average, children in the sample conceived during periods of conflict experience a 0.68 ppt increase in the probability of dying in the first month of life relative to their non-exposed siblings.[16] This represents an approximately 22 percent increase in the probability of neonatal mortality relative to non-conflict districts. While a straightforward comparison of the size of the effect to previous literature is not possible due to limited studies on the effect of armed conflict on neonatal mortality, the coefficient found here is similar to that found in studies examining the effect of long duration low-intensity conflict on child health (Mansour and Rees, 2012; Quintana-Domeque and Ródenas-Serrano, 2017). Another aspect of the result is that I find that second trimester exposure to conflict matters for child outcomes. This is consistent with Torche and Shwed (2015) and Hernández-Julián et al. (2014) who report that exposure to shocks in the second trimester of pregnancy has negative effects on child health. However, other empirical studies examining the conflict-child health relationship find exposure in the first trimester of pregnancy to be particularly detrimental for early child health (Camacho, 2008; Mansour and Rees, 2012; Quintana-Domeque and Ródenas-Serrano, 2017).

1.5.2. Additional outcomes

Next, I analyse the effect of prenatal exposure to conflict on post-neonatal mortality. Here, the dependent variable is a binary indicator which equals 1 if a child dies after the

[16] A child among the exposed population in the sample experiences on average 7.5 deaths. Assuming linearity, I multiply this with the estimated coefficient to obtain the average effect.

first month of life but within the first year of life; and 0 otherwise.[17] For the analysis, I drop live births from the sample that did not occur at least 12 months before the date of the interview as these observations were not fully exposed to post-neonatal mortality risk. Moreover, to account for post-natal differences in exposure to conflict, I control for the total number of conflict fatalities occurring in the district of residence in the first year of life.

The specifications in Table 1.3 correspond to that in column (4) of Table 1.2 but with different outcome variables. The mother fixed-effects estimates reported in Column (1) indicate that exposure to conflict fatalities during pregnancy does not have a significant impact on the likelihood of post-neonatal mortality. This finding is in line with Savitz et al. (1993) who find larger increases in neonatal mortality relative to post-neonatal mortality in high-intensity conflict provinces in Vietnam. In contrast, Lindskog (2016) finds that conflict in the Democratic Republic of Congo (DRC) is associated with an increase in post-neonatal mortality relative to neonatal mortality. However, she concludes that her results may be linked to the pre-conflict conditions in the DRC and the specific nature of the Congolese conflict.

Secondly, I examine the relationship between conflict exposure during pregnancy and health at birth using birth weight (in grams) as the outcome of interest. The mother fixed-effects estimates reported in column (2) of Table 1.3 suggest that prenatal exposure to conflict fatalities in any trimester of pregnancy is not associated with changes in birth weight. In column (3), I replace birth weight with a binary indicator for low birth weight which equals 1 if the child is below 2500 grams; and 0 otherwise.[18] The estimates indicate that exposure to an additional conflict fatality in the second trimester of pregnancy is associated with 0.13 ppt increase in the probability of a child having low birth weight. However, the result is only weakly significant (ρ=0.078).

The results on birth weight, while not statistically significant, are similar to existing studies which largely show that exposure to conflict in utero, particularly in early pregnancy, is associated with decreases in birth weight or increases in number of children with low birth weight (Camacho, 2008; Mansour and Rees, 2012; Quintana-Domeque and Ródenas-Serrano, 2017; Le and Nguyen, 2020*a*). However, it should be noted that the

[17]Formally, post-neonatal death is defined as death between 28 and 364 days of age. The definition used in the analysis is adjusted for potential age heaping in retrospective child mortality data.

[18]The World Health Organisation (WHO) defines low birth weight as weighing less than 2,500 grams at birth.

birth weight estimates in Table 1.3 are likely to be imprecisely estimated as the DHS data on birth weight is prone to measurement error.[19]

1.6. Potential Pathways

1.6.1. Conflict and fetal loss

Exposure to adverse events during pregnancy may increase fetal mortality. This can occur via a scarring mechanism which refers to the deterioration of fetal health or via a culling mechanism which refers to an increase in the threshold of fetal health needed to survive till birth (Catalano and Bruckner, 2006). While both these mechanisms would increase fetal loss, worse health outcomes at birth would be observed under a dominant scarring mechanism and improved health outcomes at birth under a dominant culling mechanism.

In the previous section, I found that holding constant unobserved maternal heterogeneity, prenatal conflict exposure in the second trimester of pregnancy is associated with an increase in neonatal mortality. Neonatal mortality is closely related to conditions surrounding pregnancy and health at birth. Therefore, a dominant scarring mechanism could offer a potential explanation for my findings. To explore this further, I use the mother fixed-effects specification to first estimate the effect of in utero exposure to conflict on fetal loss for the sample of mothers with data on the mortality of live births.

The reproductive events history collected by the DHS includes data on the incidence and dates of pregnancy terminations (if any) for the interviewed women. The terminations are further classified into abortions, miscarriages and stillbirths. Following the existing literature, I examine the effect of prenatal conflict exposure separately on miscarriages and stillbirths. This is due to a possible difference in risk associated with miscarriages that occur during pregnancy term and stillbirths that largely occur at the time of delivery.[20] The outcome of interest is a binary indicator for miscarriage (stillbirth) which equals 1 if a pregnancy ends in a miscarriage (stillbirth), and 0 if it ends in a live birth.

I report within-mother estimates of in utero exposure to conflict on miscarriage and stillbirth in columns (1) and (2), repectively, of Table 1.4. The results suggest no sig-

[19]In the main sample, data on birth weight is missing for approximately 32 percent of the observations. Further, among the non-missing observations, 42 percent are from mother's recall.

[20]Some of the terminations in the data are not classified. I do not include these as well as abortions as outcomes in the analysis because these observations are likely to be subject to more severe measurement error.

nificant impact of prenatal exposure to conflict deaths on the probability of miscarriage. However, exposure to an additional conflict death in the district in the second trimester of pregnancy appears to increase the probability of stillbirth by 0.04 ppt. This, together with the findings on neonatal mortality, provides support for the scarring hypothesis and implies that while some babies are stillborn as a result of exposure to conflict in the second trimester of pregnancy, others survive but die in the first month of life.

Another implication of the scarring mechanism is that male fetuses would not experience an improvement in health outcomes relative to females. Existing literature suggests that there are a larger number of males in the lower tail of the fetal health distribution (Byrne et al., 1987; Møller, 1996). Therefore, both scarring and culling mechanisms will result in the spontaneous abortion of a larger number of male fetuses. However, if culling is the dominant mechanism, an improvement in health outcomes would be observed for surviving male fetuses because they would now require a higher mean health endowment to meet the new survival threshold. On the other hand, such an improvement would be absent under a dominant scarring mechanism.

To test this implication, I run the mother fixed-effects specification to estimate the effect of prenatal conflict exposure on neonatal mortality separately for boys and girls. The results are reported in Table 1.5 and suggest that exposure to conflict in the second trimester of pregnancy significantly increases the probability that a surviving male fetus will die in the first month of life. I do not find a significant effect of conflict exposure on the probability of neonatal mortality for girls. This further corroborates the scarring mechanism.

Overall, the results in this section provide suggestive evidence of an in utero scarring mechanism potentially driving the results on neonatal mortality. This is comparable to findings from earlier studies which show evidence of worse health outcomes at birth in conflict affected regions in Colombia, Palestine and Spain (Camacho, 2008; Mansour and Rees, 2012; Quintana-Domeque and Ródenas-Serrano, 2017). By contrast, Dagnelie et al. (2018) provide evidence of a dominant selection mechanism in the Democratic Republic of Congo by showing that exposure to civil war is associated with worse infant mortality outcomes for girls relative to boys. Valente (2015), on the other hand, finds that while exposure to conflict in Nepal skews the sex ratio in favour of girls, it does not appear to be associated with worse health outcomes at birth. She concludes that her results point to the existence of both scarring and selection mechanisms. The heterogeneity in results across settings potentially highlights the role of context in determining the effect of conflict exposure on outcomes of interest. However, given the potential for measurement error in self-reported fetal loss outcomes, the results of this literature need to be interpreted with caution.

1.6.2. Conflict and the use of health services

Restricted access to health care offers another potential explanation for the positive relationship between in utero conflict exposure and neonatal mortality. Conflict regions are often characterised by limited access to health services which likely affects antenatal and postnatal care as well as conditions surrounding delivery. To explore the potential role of this mechanism, I estimate the effect of conflict exposure in utero on indicators reflecting the use of health services during delivery.

Table 1.6 presents mother fixed-effects estimates for the relationship between conflict deaths in utero and two measures which reflect health service usage during delivery: i) whether a child was born in a health care facility and ii) whether a health care professional was present during the delivery of the child.[21,22] The estimates in column (1) indicate that exposure to conflict deaths in the second trimester of pregnancy reduces the probability that a child will be born in a health facility by 0.17 ppt, relative to his non-exposed sibling. A similar effect is found for second trimester exposure to conflict and the attendance of a health care professional at the time of delivery (column (2)). A plausible explanation for this finding is that an increase in conflict intensity towards the later half of pregnancy may trigger an atmosphere of fear among people, restricting their local mobility and in turn their ability to access necessary healthcare. It could also be that healthcare facilities close in response to an increase in conflict intensity. News reports and anecdotal evidence document that fear and security reasons prevent people living in high intensity conflict districts from accessing healthcare from nearby health facilities (Solberg, 2008). The reports further indicate that health care professionals are unwilling to work in Naxal

[21] Health care facilities include both public and private hospitals and clinics, as well as government managed health sub-centres. Health care professionals include doctors, nurses, midwives, and other health personnel.

[22] The DHS collects data on the number of prenatal visits as well as postnatal care. However, this data is collected only for the youngest child born in the 5 years prior to the interview, and as a result does not allow for the use of a mother fixed-effects specification. The district fixed-effects estimates indicate no significant difference in prenatal and postnatal care as a result of in utero exposure to conflict.

affected districts due to safety concerns.

1.7. Robustness Checks

This section discusses the robustness of the main mother fixed-effects estimates. In Tables 1.7 and 1.8, I report results from various robustness checks for the following outcomes: i) neonatal mortality ii) miscarriage iii) stillbirth iv) delivery at a health facility and v) assistance from a medical professional during delivery.

1.7.1. Excluding controls

In the main analysis, I include controls to account for time varying maternal and child characteristics which are widely known to be correlated with the outcomes of interest. However, some of these controls like maternal age at birth and birth (pregnancy) order could be potentially endogenous. In Panel A of Table 1.7, I check whether the mother fixed-effects estimates are robust to the exclusion of these controls. The point estimates are largely similar to main results suggesting that the inclusion of time-varying controls only increases the precision of the estimated effect.

1.7.2. District specific linear year trends

The main specification includes year-month of conception fixed-effects to capture any specific calendar month or seasonal effects. In addition, to check the robustness of the main estimates to any underlying trends, I include a district-specific linear year trend to the specification. This should control for differences in factors like economic development and health care investments across districts that can be assumed to change linearly over time and that may potentially impact the outcomes of interest. The estimates presented in Panel B of Table 1.7 show that the main results remain essentially unchanged.

1.7.3. Migration

Families may migrate in response to increases in conflict activity. This can introduce measurement error in the variable capturing prenatal conflict exposure for children who were not conceived in their current district of residence. The DHS does not collect detailed migration data for the interviewed women. It only records the total number of years that interviewed women have (continuously) lived in their current place of residence. Using this information, I drop all children who were not conceived in their mother's current place of residence.[23] This also excludes the children of women visiting the household. The resulting estimates presented in Panel C of Table 1.7 are similar to the main results but the magnitude of the estimated effect increases for three out of the five outcomes. This suggests that the main findings likely underestimate the effect of in utero conflict exposure on child mortality due to the presence of measurement error.

1.7.4. Placebo check: Cumulative conflict deaths

The analysis in this chapter implicitly assumes that only conflict deaths occurring during pregnancy matter for neonatal mortality. However, it is possible that in utero exposure to conflict deaths in a district may be correlated with underlying trends in conflict intensity which in turn may be relevant for neonatal mortality. In Panel A of Table 1.8, I include in the main specification a placebo treatment variable which captures the total number of conflict deaths occurring up to the month of conception.[24] The results indicate that district level conflict trends prior to conception do not have an effect on neonatal mortality or the probability of stillbirth. The coefficients on second trimester conflict exposure for the outcomes reflecting health service usage although negative, are now smaller in magnitude and not statistically significant.

1.7.5. High-intensity conflict districts

As discussed in Section 1.2, the Naxalite conflict varies substantially in intensity across districts. In the main sample, approximately 90 percent of the districts affected by Naxalite activity between 2009 and 2016 recorded fewer than 100 conflict fatalities. To check whether the main results are driven by districts which recorded periods of higher than average conflict activity, I re-estimate the main specification on the sample excluding

[23]Approximately, 13.8% of the children in the sibling sample were not conceived in their current place of residence.

[24]This measure of cumulative conflict deaths is incomplete because the earliest available conflict data from SATP starts from 2005. However, the existing measure can be assumed to be reflective of conflict trends at the district level.

districts which recorded 100 or more Naxal related fatalities between 2009 and 2016.[25]

The results are reported in Panel B of Table 1.8 and show that the estimates for stillbirth and outcomes reflecting health service usage remain significant and increase in magnitude. However, the estimates in column (1) reflect a loss in both magnitude and significance for the coefficient on second trimester conflict exposure for neonatal mortality. This suggests that the main effect is likely driven by districts with higher levels of conflict activity. Lastly, the estimates in column (2) suggest that the effect of conflict exposure on miscarriage differs in districts with relatively lower levels of conflict activity. Exposure to conflict in the first trimester is now positively associated with the probability of miscarriage, though this effect is only weakly significant (ρ=0.088). Moreover, second trimester conflict exposure appears to be associated with a decline in the probability of miscarriage. While the reason for this effect is not clear-cut, the increase in the probability of stillbirth due to second trimester conflict exposure could account for part of the observed decline in the probability of miscarriage.

1.8. Conclusion

This chapter contributes to a growing body of empirical work on the effects of in utero exposure to violent conflict on early childhood health. The conflict under consideration is the Naxalite insurgency which is a relatively understudied low-intensity civil conflict in India. This chapter specifically focuses on the effect of trimester-based in utero exposure to conflict fatalities on neonatal mortality across Indian states with active Naxal activity.

I rely on a mother fixed-effects approach to identify the effect of interest. This approach accounts for time-invariant unobserved behavioural and biological factors which may influence both the composition of women who give birth during periods of conflict as well the outcome of interest. The main finding indicates that exposure to conflict in the second trimester of pregnancy has a detrimental impact on neonatal mortality, holding constant unobserved maternal heterogeneity. This result aligns with other empirical studies which show that prenatal exposure to shocks is associated with worse child health outcomes in terms of declines in health at birth or increases in infant mortality rates (Camacho, 2008; Mansour and Rees, 2012; Hernández-Julián et al., 2014; Quintana-Domeque

[25] This results in the exclusion of 10 districts across Chhattisgarh, Jharkhand, Odisha, Maharashtra, and West Bengal from the main sample.

and Ródenas-Serrano, 2017; Dagnelie et al., 2018).

Next, I explore two potential explanations for the observed effect. First, I present some evidence that in utero scarring rather than selection potentially drives some of the negative effect of conflict on neonatal mortality. This points to maternal stress as a plausible explanation for the main findings but establishing a definite relationship would require more detailed and reliable data. Second, I show that the exposure to conflict in utero reduces the probability that a child will be delivered in a health facility as well as the probability of a medical professional being present at the time of delivery. This suggests that apart from maternal stress, limited access to health care services at the time of delivery offers another potential channel through which in utero conflict exposure can impact neonatal mortality.

The main findings are robust to several potential threats to validity. They appear to be driven by districts which experience relatively intense periods of conflict during the study period. Here, the identification of the main effect relies on the accuracy with which the SATP data captures conflict intensity across districts. Further investigation using a fully comprehensive conflict database could potentially shed more light on the role of intensity in determining the effect of prenatal exposure to the Naxal conflict on outcomes of interest.[26] Lastly, the mother fixed-effects estimates inform an understanding of the main effect for families with two or more closely-spaced children, and so, the generalizability of these results may be limited. However, as explained earlier, the use of mother fixed-effects is relevant for the identification of the main effect as it controls for unobserved maternal confounders which may be correlated with the outcome of interest.

Notwithstanding the data limitations, this study points to the Naxal conflict being detrimental for infant health, at the very least in districts characterised by high-intensity conflict activity. A potential implication is the need to test for the presence of survival bias in future research concerned with the effect of in utero exposure to the Naxalite conflict on later-life health outcomes. Another potential implication is the need for public policy in India to extend protective and remediation services to vulnerable populations affected by the conflict in addition to its current focus on reducing Naxal violence through armed forces. There appears to be a specific need to support pregnant women in conflict

[26] In recent years, terrorism databases like ACLED have started reporting data on the Naxalite conflict. Two recent papers combine data from multiple terrorism databases and/or manually code data from local language newspapers to construct a more accurate and complete database of Naxal related conflict events and fatalities (Gomes, 2015; Dasgupta et al., 2017).

situations through improved health care access. The persistent nature and recent increase in conflict activity towards the second-half of 2020 emphasizes the need to continuously document and analyse the effects of the Naxal conflict on women and child welfare.

1.9. Tables

Table 1.1: Summary statistics

	Non-conflict district		Conflict district					
			prenatal exp=0		prenatal exp>=1		difference	
	mean	sd	mean	sd	mean	sd	diff	se
Live births sample: Outcomes								
Neonatal death	0.03	(0.16)	0.03	(0.17)	0.03	(0.17)	-0.00	(0.00)
Post-neonatal death	0.01	(0.10)	0.01	(0.09)	0.01	(0.10)	-0.00	(0.00)
Infant death	0.04	(0.19)	0.04	(0.19)	0.04	(0.20)	-0.00	(0.00)
Birth weight (in gms)	2795	(605)	2824	(565)	2816	(547)	7	(5.71)
Low birth weight (=1 if <2500 gms)	0.18	(0.39)	0.16	(0.36)	0.15	(0.36)	0.01**	(0.00)
Child characteristics								
Birth order	2.19	(1.38)	2.19	(1.38)	2.27	(1.40)	-0.08***	(0.01)
Age (in months)	30.53	(17.01)	28.56	(16.92)	33.53	(16.99)	-4.97***	(0.15)
Girl	0.48	(0.50)	0.48	(0.50)	0.49	(0.50)	-0.01	(0.00)
Mother characteristics								
Mother's age at conception (in yrs)	23.73	(4.70)	23.89	(4.90)	24.10	(5.07)	-0.21***	(0.04)
Mother's education (in completed yrs)	5.65	(4.78)	5.72	(4.94)	4.95	(4.87)	0.77***	(0.04)
Household characteristics								
Rural	0.76	(0.43)	0.80	(0.40)	0.84	(0.37)	-0.04***	(0.00)
Hindu	0.83	(0.37)	0.84	(0.37)	0.83	(0.38)	0.01***	(0.00)
Muslim	0.14	(0.35)	0.12	(0.33)	0.08	(0.28)	0.04***	(0.00)
Scheduled caste	0.20	(0.40)	0.21	(0.41)	0.17	(0.38)	0.04***	(0.00)
Scheduled tribe	0.18	(0.39)	0.15	(0.36)	0.31	(0.46)	-0.16***	(0.00)
Others	0.02	(0.15)	0.04	(0.19)	0.09	(0.28)	-0.05***	(0.00)
Fertility sample: Outcomes								
Miscarriage	0.04	(0.20)	0.05	(0.23)	0.05	(0.21)	0.01***	(0.00)
Stillbirth	0.01	(0.10)	0.01	(0.11)	0.01	(0.11)	-0.00	(0.00)

Notes: The 'Live births' sample excludes twin births. Both the 'Live births' and 'Fertility' samples exclude all observations from the state of Uttar Pradesh. ***p< 0.01, **p< 0.05, *p<0.10.

Table 1.2: Impact of in utero exposure to conflict deaths on neonatal mortality

	Neonatal mortality			
	(1)	(2)	(3)	(4)
Trimester 1	-0.0001	-0.0001	-0.0002	0.0003
	(0.0001)	(0.0001)	(0.0002)	(0.0003)
Trimester 2	0.0002	0.0002	0.0005	0.0009***
	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Trimester 3	-0.0008**	-0.0007**	-0.0008	-0.0016
	(0.0004)	(0.0004)	(0.0006)	(0.0010)
Mean (non-conflict districts)	0.03	0.03	0.03	0.03
Month × Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
District FE	Yes	Yes	Yes	No
Mother FE	No	No	No	Yes
Observations	106,512	106,512	52,865	52,865

Notes: Columns (1) to (3) report within-district estimates and column (4) reports within-mother estimates. Column (3) reports within-district estimates on the mother fixed-effects sample. All regressions include year-month of conception fixed-effects. The specification for neonatal mortality includes conflict deaths occurring in the first month of life. Additional controls include infant's sex, birth order, mother's age at conception, mother's education in completed years, a binary indicator for rural households, and three indicators for religion. Standard errors clustered at the district level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 1.3: Impact of in utero exposure to conflict deaths on other outcomes

	(1) Post neonatal	(2) Birthweight	(3) Low birthweight
Trimester 1	-0.0001	-1.6695	0.0011
	(0.0002)	(1.1214)	(0.0011)
Trimester 2	0.0002	-1.5606	0.0013*
	(0.0003)	(1.1081)	(0.0007)
Trimester 3	0.0003	2.8416	-0.0012
	(0.0007)	(2.7708)	(0.0018)
Mean (non-conflict districts)	0.01	2796	0.18
Month × Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
District FE	No	No	No
Mother FE	Yes	Yes	Yes
Observations	33,895	36,249	36,249

Notes: Columns (1) to (3) report reports within-mother estimates on the sibling sample. All regressions include year-month of conception fixed-effects. The specification for post-neonatal mortality includes conflict deaths occurring in the first year of life. Additional controls include infant's sex, birth order, mother's age at conception, mother's education in completed years, a binary indicator for rural households, and three indicators for religion. Standard errors clustered at the district level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 1.4: Impact of in utero exposure to conflict deaths on fetal loss

	(1) Miscarriage	(2) Stillbirth
Trimester 1	0.0002	-0.0001
	(0.0004)	(0.0002)
Trimester 2	0.0004	0.0004***
	(0.0004)	(0.0001)
Trimester 3	-0.0006	0.0001
	(0.0010)	(0.0003)
Mean (non-conflict districts)	0.04	0.01
Month × Year FE	Yes	Yes
Controls	Yes	Yes
Mother FE	Yes	Yes
Observations	67,162	62,379

Notes: All regressions include year-month of conception fixed-effects and mother fixed-effects. Additional controls include pregnancy order, mother's age at conception, mother's education in completed years, a binary indicator for rural households, and three indicators for religion. Standard errors clustered at the district level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 1.5: Impact of in utero exposure to conflict deaths on neonatal mortality by gender

	(1) Boys	(2) Girls
Trimester 1	-0.0001	-0.0002
	(0.0004)	(0.0006)
Trimester 2	0.0014**	0.0004
	(0.0006)	(0.0006)
Trimester 3	-0.0021	-0.0017
	(0.0018)	(0.0015)
Mean (non-conflict districts)	0.03	0.03
Month × Year FE	Yes	Yes
Controls	Yes	Yes
Mother FE	Yes	Yes
Observations	13,350	14,350

Notes: All regressions include year-month of conception fixed-effects and mother fixed-effects. Additional controls include conflict deaths in the first month of life, birth order, mother's age at conception, mother's education in completed years, a binary indicator for rural households, and three indicators for religion. Standard errors clustered at the district level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 1.6: Impact of in utero exposure to conflict on health service usage during delivery

	(1) Delivery: health facility	(2) Delivery: assistance
Trimester 1	0.0003	0.0007
	(0.0006)	(0.0006)
Trimester 2	-0.0017**	-0.0016**
	(0.0007)	(0.0007)
Trimester 3	0.0020	0.0007
	(0.0017)	(0.0014)
Mean (non-conflict districts)	0.79	0.80
Month × Year FE	Yes	Yes
Controls	Yes	Yes
Mother FE	Yes	Yes
Observations	53,614	53,614

Notes: All regressions include year-month of conception fixed-effects and mother fixed-effects. Additional controls include birth order, mother's age at conception, mother's education in completed years, a binary indicator for rural households, and three indicators for religion. Standard errors clustered at the district level are in parentheses.***p< 0.01, **p< 0.05, *p<0.10.

Table 1.7: Robustness check: Controls, district-specific linear trend and migration

	(1) Neonatal	(2) Miscarriage	(3) Stillbirth	(4) Health facilty	(5) Assisstance
Panel A: No controls					
Trimester 1	-0.0000	-0.0002	-0.0001	0.0003	0.0007
	(0.0003)	(0.0004)	(0.0002)	(0.0007)	(0.0006)
Trimester 2	0.0008**	0.0002	0.0003***	-0.0018**	-0.0017**
	(0.0003)	(0.0004)	(0.0001)	(0.0008)	(0.0008)
Trimester 3	-0.0014	-0.0005	0.0001	0.0021	0.0007
	(0.0010)	(0.0010)	(0.0004)	(0.0018)	(0.0014)
Observations	52,865	67,162	62,379	53,614	53,614
Panel B: District-specific linear year trend					
Trimester 1	0.0003	0.0001	0.0001	0.0006	0.0009
	(0.0003)	(0.0005)	(0.0002)	(0.0008)	(0.0008)
Trimester 2	0.0010**	0.0002	0.0005***	-0.0020***	-0.0018**
	(0.0004)	(0.0004)	(0.0001)	(0.0008)	(0.0008)
Trimester 3	-0.0014	-0.0003	0.0001	0.0009	-0.0009
	(0.0011)	(0.0011)	(0.0004)	(0.0014)	(0.0012)
Observations	52,865	67,162	62,379	53,614	53,614
Panel C: Children conceived in current district of residence					
Trimester 1	0.0002	-0.0003	-0.0003	0.0004	0.0008
	(0.0004)	(0.0007)	(0.0002)	(0.0010)	(0.0011)
Trimester 2	0.0011**	0.0006	0.0002*	-0.0021***	-0.0020***
	(0.0005)	(0.0005)	(0.0001)	(0.0006)	(0.0006)
Trimester 3	-0.0015	-0.0004	-0.0000	0.0028	0.0011
	(0.0009)	(0.0011)	(0.0002)	(0.0019)	(0.0015)
Observations	43,196	54,862	51,096	43,762	43,762

Notes: All regressions include year-month of conception fixed-effects and mother fixed-effects. Additional controls (in Panel B and Panel C) include pregnancy/birth order, mother's age at conception, mother's education in completed years, a binary indicator for rural households, and three indicators for religion. Standard errors clustered at the district level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 1.8: Robustness check: cumulative fatalities and high-intensity conflict districts

	(1) Neonatal	(2) Miscarriage	(3) Stillbirth	(4) Health facilty	(5) Assisstance
Panel A: Controlling for cumulated conflict deaths prior to conception					
Trimester 1	0.0002	0.0003	-0.0001	0.0013	0.0013
	(0.0003)	(0.0004)	(0.0002)	(0.0010)	(0.0009)
Trimester 2	0.0008**	0.0004	0.0004***	-0.0007	-0.0010
	(0.0004)	(0.0004)	(0.0001)	(0.0008)	(0.0007)
Trimester 3	-0.0016	-0.0005	0.0001	0.0027	0.0011
	(0.0010)	(0.0010)	(0.0003)	(0.0019)	(0.0015)
Cumulative deaths	-0.0001	0.0001	0.0000	0.0008**	0.0006*
	(0.0001)	(0.0001)	(0.0000)	(0.0004)	(0.0003)
Observations	52,865	67,162	62,379	53,614	53,614
Panel B: Excluding high-intensity conflict districts					
Trimester 1	0.0006	0.0014*	0.0001	0.0002	0.0014
	(0.0010)	(0.0008)	(0.0006)	(0.0018)	(0.0017)
Trimester 2	0.0006	-0.0029***	0.0011**	-0.0049**	-0.0060**
	(0.0015)	(0.0011)	(0.0005)	(0.0023)	(0.0023)
Trimester 3	-0.0021	0.0019	0.0004	-0.0013	-0.0012
	(0.0019)	(0.0015)	(0.0006)	(0.0019)	(0.0020)
Observations	51,023	64,637	60,065	51,726	51,726

Notes: All regressions include year-month of conception fixed-effects and mother fixed-effects. Additional controls include pregnancy/birth order, mother's age at conception, mother's education in completed years, a binary indicator for rural households, and three indicators for religion. Standard errors clustered at the district level are in parentheses. ***$p< 0.01$, **$p< 0.05$, *$p<0.10$.

1.10. Figures

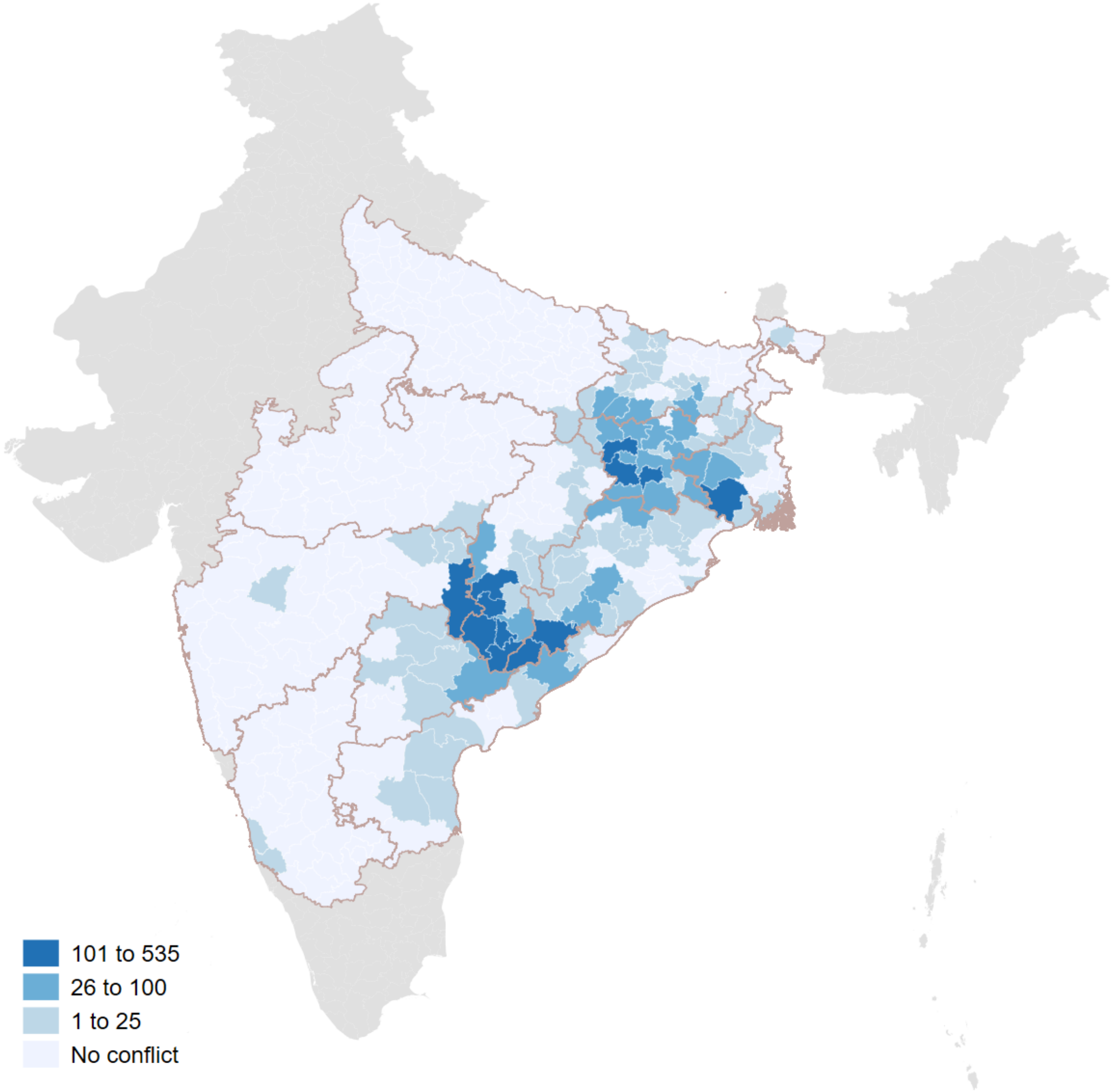


Figure 1.1: District-wise distribution of Naxal related conflict fatalities between 2009 and 2016.
Source: SATP data

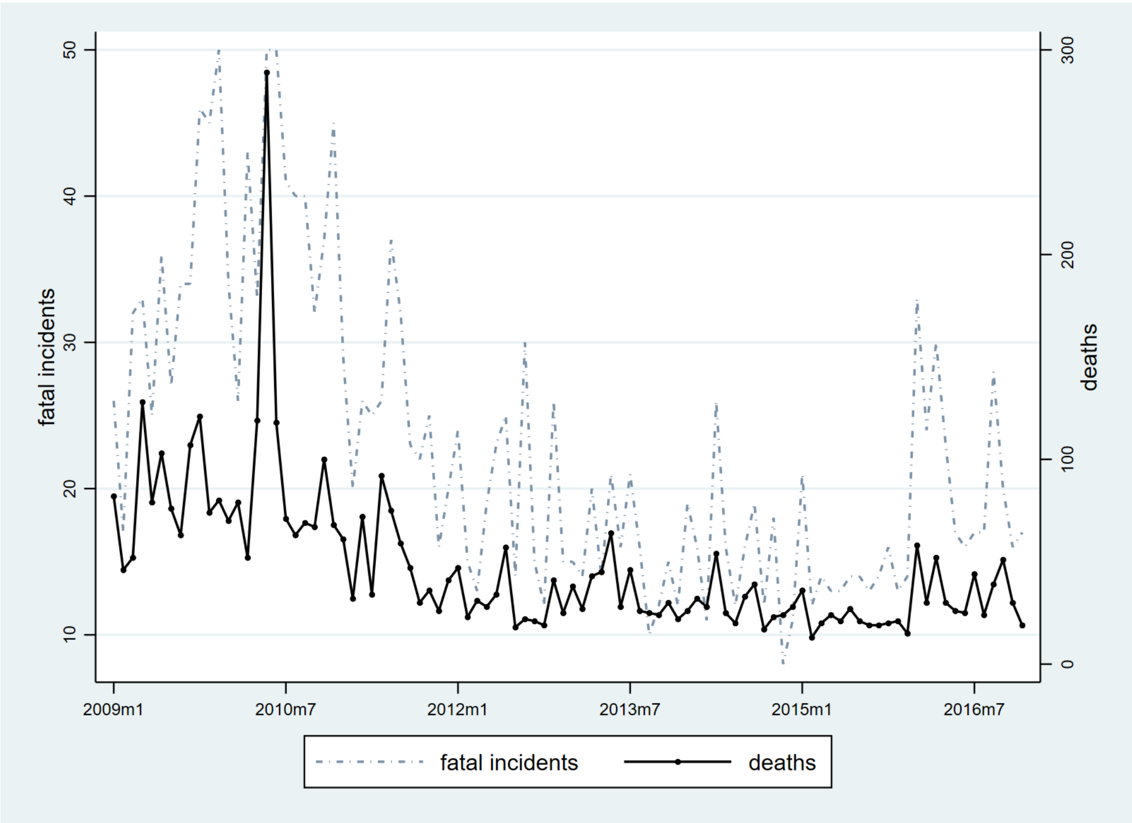


Figure 1.2: Monthly variation in Naxal related fatal incidents and deaths between 2009 and 2016

1.A. Appendix

1.A.1. The sibling sample

The preferred specification in this chapter uses mother fixed-effects to identify the effect of in utero conflict exposure on the outcomes of interest. This necessarily restricts the analysis to a subsample of siblings (also referred to as the sibling sample), conceived no more than 6 years prior to the interview date. The siblings in this sample are likely to be more closely spaced than siblings in families where some of the children are older than 5 at the time of the survey.

In Table 1.A.1, I compare summary statistics for the sibling sample and the full sample. The sibling sample appears to exhibit higher levels of child mortality relative to the full sample but no significant differences are observed for birth weight outcomes. The children in the sibling sample are also more likely to born to younger, lesser educated mothers in rural, lower caste households than children in the full sample.

The sibling sample is relevant for the main analysis as it allows for the main effect to be identified holding constant time-invariant unobserved biological and behavioural factors which may influence both the composition of women who give birth during periods of conflict as well as the outcomes of interest. The summary statistics in Table 1.A.1 suggest that the sibling sample significantly differs from the full sample on observable child, mother, and household characteristics, though these differences are not substantively large in most cases. Therefore, while the main results inform an understanding of the conflict-neonatal mortality relationship for families with two or more relatively closely-spaced children in Naxal affected states in India, the genralizability of the results to the general population of children in these states may be limited.

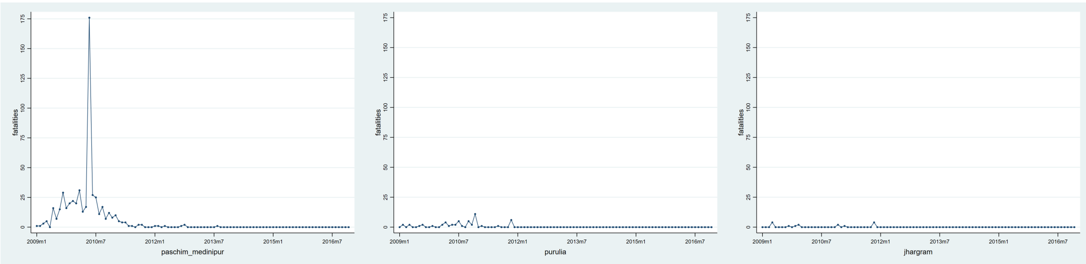
Table 1.A.1: Summary statistics (full sample versus sibling sample)

	Full sample		Sibling sample		Difference	
	mean	sd	mean	sd	diff	se
Live birth sample: Outcomes						
Neonatal death	0.03	(0.17)	0.04	(0.20)	-0.01***	(0.00)
Post-neonatal death	0.01	(0.10)	0.01	(0.12)	-0.00***	(0.00)
Infant death	0.04	(0.19)	0.06	(0.23)	-0.02***	(0.00)
Birth weight (in gms)	2808	(583)	2810	(591)	-2.01	(3.55)
Low birth weight (=1 if <2500 gms)	0.17	(0.37)	0.17	(0.37)	0.00	(0.00)
Child characteristics						
Birth order	2.20	(1.39)	2.36	(1.36)	-0.15***	(0.01)
Age (in months)	30.49	(17.06)	30.63	(17.20)	-0.14	(0.09)
Girl	0.48	(0.50)	0.51	(0.50)	-0.03***	(0.00)
Mother characteristics						
Mother's age at conception (in yrs)	23.85	(4.84)	23.39	(4.36)	0.45***	(0.02)
Mother's education (in completed yrs)	5.54	(4.85)	4.73	(4.61)	0.81***	(0.02)
Household characteristics						
Rural	0.79	(0.41)	0.82	(0.38)	-0.04***	(0.00)
Hindu	0.84	(0.37)	0.83	(0.37)	0.00*	(0.00)
Muslim	0.13	(0.33)	0.13	(0.34)	-0.01***	(0.00)
Scheduled caste	0.20	(0.40)	0.20	(0.40)	-0.01***	(0.00)
Scheduled tribe	0.20	(0.40)	0.20	(0.40)	-0.01***	(0.00)
Others	0.04	(0.19)	0.03	(0.18)	0.01***	(0.00)

Notes: The sample excludes twin births and all observations from the state of Uttar Pradesh. ***p< 0.01, **p< 0.05, *p<0.10.

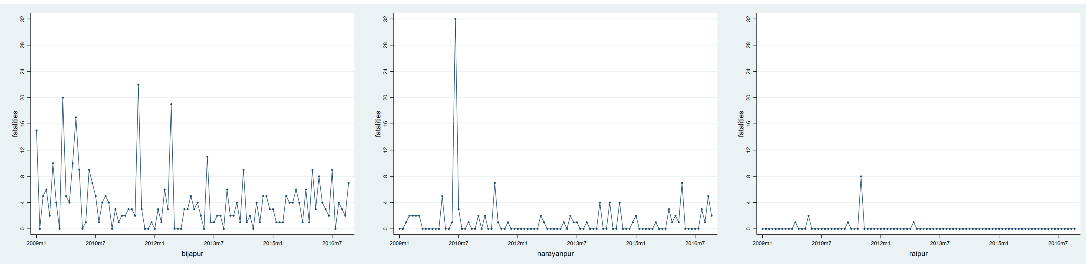
1.A.2. Spatial and temporal variation in conflict

Panels (a), (b), and (c) in Figures 1.A.1 to 1.A.8 display the monthly variation in conflict fatalities in a high-, medium-, and low-intensity district, respectively, in each of the Naxal affected states included in the analysis. The selected high-intensity (low-intensity) district in each state has the highest (lowest) number of fatalities relative to other affected districts in that state between 2009 and 2016. The selected medium-intensity district in each state has a fatality count which lies somewhere between the total number of fatalities in the selected high- and low-intensity districts in that state.



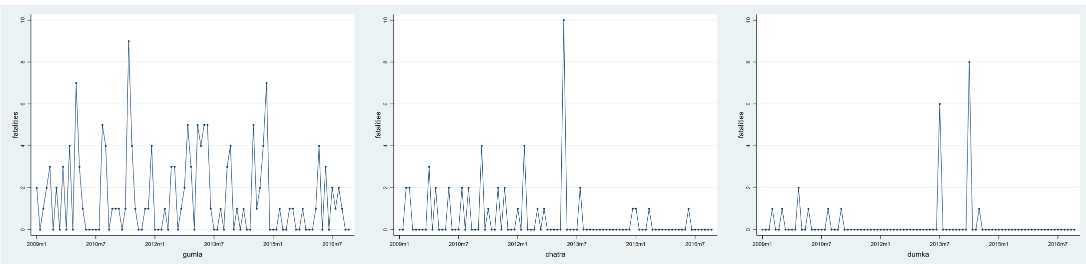
(a) (b) (c)

Figure 1.A.1: Conflict deaths between 2009 and 2016 in West Bengal



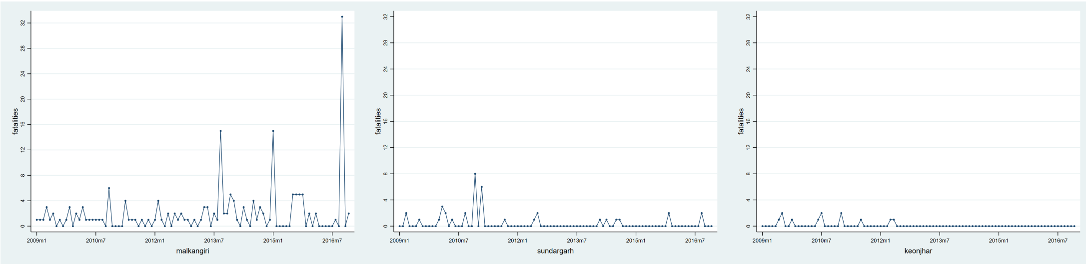
(a) (b) (c)

Figure 1.A.2: Conflict deaths between 2009 and 2016 in Chhattisgarh



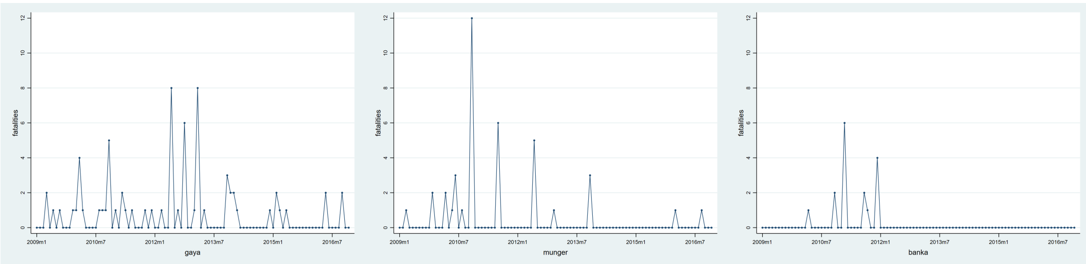
(a) (b) (c)

Figure 1.A.3: Conflict deaths between 2009 and 2016 in Jharkhand



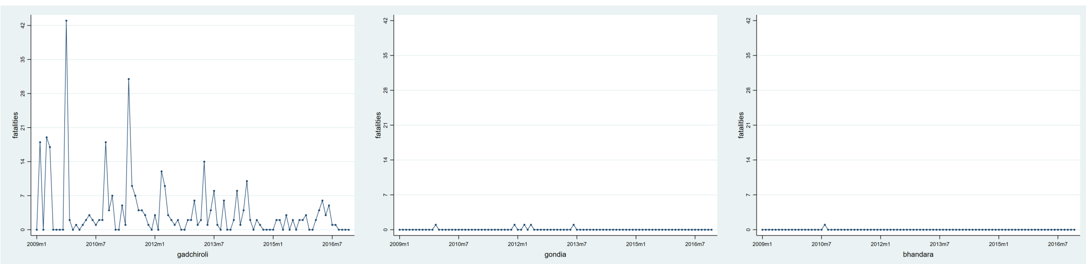
(a) (b) (c)

Figure 1.A.4: Conflict deaths between 2009 and 2016 in Odisha



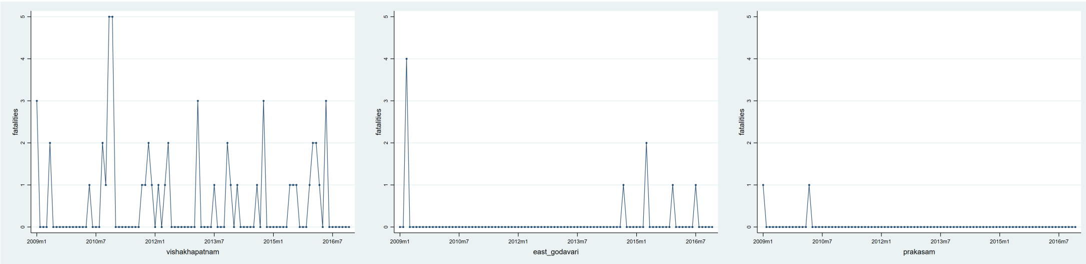
(a) (b) (c)

Figure 1.A.5: Conflict deaths between 2009 and 2016 in Bihar



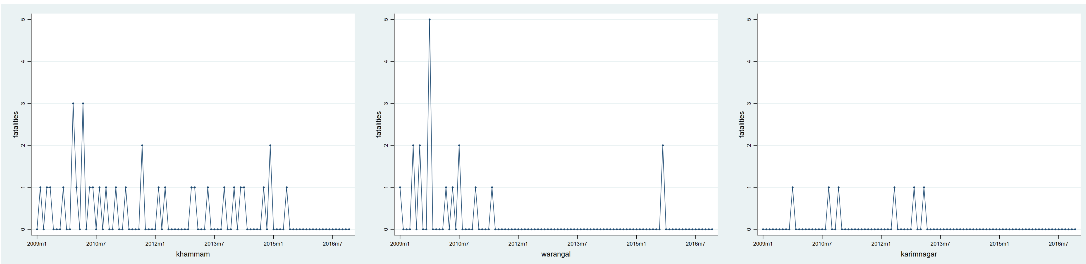
(a) (b) (c)

Figure 1.A.6: Conflict deaths between 2009 and 2016 in Maharashtra



(a) (b) (c)

Figure 1.A.7: Conflict deaths between 2009 and 2016 in Andhra Pradesh



(a) (b) (c)

Figure 1.A.8: Conflict deaths between 2009 and 2016 in Telangana

Chapter 2

Do birth order effects on height persist? A panel study of Indian children

2.1. Introduction

Children in India are shorter, on average, than children in other developing countries, despite relatively higher levels of economic growth (Deaton, 2007). According to the Global Nutrition Report (2018), roughly 46.6 million or a third of the world's stunted (low height-for-age) children are found in India. Sustained height deficits in childhood, also referred to as chronic undernutrition, have been linked to later-life health and cognitive deficits, and therefore, can impede the efficient accumulation of human capital in a country.

Height deficits are often the result of long-term inadequate nutrition and/or chronic exposure to an infectious disease environment. In addition, there are several underlying factors which indirectly contribute to height deficits in children. These include but are not limited to adverse household socioeconomic conditions, limited access to health services, lower levels of maternal nutritional status and educational attainment, and poor childcare and feeding practices. Recent empirical evidence suggests that a combination of environmental factors and parental preferences is responsible for the high prevalence of height deficits in Indian children. Spears (2020) finds that poor sanitation facilities and exposure to open defecation are among the main causes of height deficits in the country. Jayachandran and Pande (2017) demonstrate that a significant fraction of childhood stunting in the country can be attributed to the negative birth order gradient in height driven by a parental preference for the eldest son.

In this chapter, I explore the relationship between birth order and height deficits in Indian children. Birth order effects on child nutritional status have been widely documented in both developed and developing countries. The evidence generally finds that birth order is negatively associated with anthropometric indicators of child nutritional status. However, in developing countries, most empirical studies examining birth order effects are primarily concerned with outcomes in early childhood. There is scant and mixed evidence on how birth order affects health and nutrition as children transition into middle childhood and adolescence. This is an important gap in existing literature, particularly given the ongoing debate around the potential for later-life catch-up growth.

Economic theories of birth order effects mostly assume that these effects are linear and monotonic in nature. This is in line with the long-standing view that growth retardation is largely irreversible after the first two years of a child's life.[1] However, recent evidence disputes this view, suggesting that the potential for catch-up growth which can reduce or even erase a child's early-life height deficit is present beyond infancy (Prentice et al., 2013; Georgiadis et al., 2017). Here, the extent of catch-up growth would depend in part on parents' ability to identify weaker children and follow a successful compensatory strategy in health and nutritional investments. Parental behaviour with respect to resource allocation would then contribute to whether birth order differences attenuate, persist or worsen as children age. Figure 2.1 plots the relationship between height-for-age z-scores, age and birth order for Indian children aged 0 to 11 years using the data employed in this chapter. Visually, the birth order disadvantage in height can be observed most clearly at ages 3 and 4. However, the pattern does not appear to hold for older children.

In this chapter, I use a panel sample of Indian children to study how birth order effects on height evolve with age. To address concerns surrounding the endogeneity of family size, I employ a mother fixed-effects specification which identifies the birth order effect using sibling comparisons. In line with the existing literature on birth order effects in developing countries, I find a strong later-born disadvantage in initial height. Furthermore, the results show that the negative birth order effect on height persists as children age into middle-childhood and adolescence.

[1] The first 1000 days or 24 months of a child's life is commonly referred to as the "critical period" to address nutritional deficits.

This chapter contributes to the existing literature in the follwing ways. First, it adds to the broad literature on birth order effects on markers of child nutritional status in developing countries. Moreover, it is one of the few studies to analyse birth order effects in children up to the age of 18 years, as compared to most of the previous empirical work which examines the effect of birth order on height for children under the age of 5. The use of a mother fixed-effects specification in these studies to identify birth order effects raises selection concerns as the siblings in the sample will likely have shorter than average birth spacing. This study overcomes this issue by extending the analysis to include siblings in a wider age range.

Second, it contributes to the limited literature that documents changes in the association between birth order and nutritional status as children age. The two existing related empirical studies reach conflicting conclusions. Collin (2006) examines the relationship between (relative) birth order, age and anthropometric indicators in rural Ethiopia and finds that the later-born height disadvantage reduces but persists with age. However, he relies on a measure of birth order constructed by assigning a birth order number to all children observed in a household based on their age ranking within the household. This leads to inaccuracies in the measure of birth order as it does not represent the exact order in which children move through a household. It excludes children who may have left the household, and therefore is unable to fully capture the interactions between birth order, sibling history and related household environment. This would particularly matter for outcomes like height which represent accumulated investments. This chapter accounts for children who do not reside in the household by using a more accurate measure of (absolute) birth order constructed using a mother's fertility history.

Collin (2013) finds that while birth order differences in height are present in early life for a cohort of Filipino children, they are largely transitory in nature and mostly disappear as the cohort approaches adulthood. However, he focuses on a single cohort of children and is therefore not able to fully account for cohort effects driving the results. This study, on the other hand, identifies birth order effects using a panel of children born in different years. Further, the analysis exploits the availability of data on more than one child born to the same mother to identify birth order effects using sibling comparisons. Moreover, this chapter differs from the previous studies in that it is the first study to examine whether birth order effects are permanent or transitory in the Indian context.

This is of interest in its own right given that children in India tend to have worse height outcomes than children in most other developing countries.

Third, this chapter also contributes to the debate on the potential for later-life catch-up growth. The main findings indicate that first-born children retain their advantage in initial height across family sizes, stages of growth development, maternal education levels, and groups that exhibit different levels of son preference. However, I find some evidence that second-born children exhibit catch-up growth relative to their first-born siblings, though the magnitude is not sufficient to suggest complete catch-up. Further, this pattern of results is significant for children aged between 2 and 5 years at baseline, providing suggestive evidence that potential for growth recovery may exist beyond infancy.

The rest of this chapter is organised as follows: Section 2.2 reviews the existing evidence on birth order effects on child health. Section 2.3 briefly discusses the potential mechanisms through which birth order influences child nutritional status. Section 2.4 describes the data and provides summary statistics. Section 2.5 discusses the empirical strategy. Section 2.6 presents the main estimation results and Section 2.7 reports results from heterogeneity analysis. Section 2.8 checks the robustness of the main results. Section 2.9 concludes.

2.2. Literature review

Birth order effects on nutrition and health have been extensively documented in social science and medical literature. Within economics, empirical evidence largely points to the existence of a later-born disadvantage in health outcomes. There is, however, some variation in this result based on the context and outcome being investigated.

Studies examining birth order effects in developed countries find that later-born children are more likely to be shorter relative to earlier-born children (Savage et al., 2013; Myrskylä et al., 2013). Higher birth order is also known to be associated with worse self-reported physical and mental health, risky health behaviors and increased mortality risk (Modin, 2002; Argys et al., 2006; Jelenkovic et al., 2013; Barclay and Kolk, 2015; Black et al., 2016). On the other hand, some studies show the existence of a later-born advantage in health outcomes like health at birth, body mass index (BMI) and blood pressure (Brenøe and Molitor, 2018; Black et al., 2016; Lundberg and Svaleryd, 2017).

Existing empirical work on birth order differences in health in developing countries also finds a later-born disadvantage in anthropometric outcomes like height and weight. Horton (1988) examines birth order effects on the nutritional status of siblings in the Bicol region in the Philippines. She finds that birth order is negatively correlated with a child's long-run and short-run nutritional status as measured by height-for-age and weight-for-age, respectively. She also finds that while the negative effect of birth order is larger on height-for-age indicating a stronger later-born disadvantage in long-run nutritional status, the effects on short-run nutritional status are not as prominent. She argues that her findings are consistent with the idea that birth order effects are driven by parents' inability to allocate resources equitably across time.

Kebede (2005) employs an instrumental variables estimation strategy on data from the Ethiopian Rural Household Surveys (ERHS) to examine the determinants of child height in rural Ethiopia. He finds that birth order is negatively associated with child height but that the effect is significant only up to birth order 3. Like Horton (1988), his findings suggest that the negative birth order effects on child height are likely driven by declining resources per capita in a household. Bishwakarma and Villa (2019) find similar results in South Africa using a mother fixed-effects specification on a nationally representative sample of children between 1 and 18 years of age. Their findings point to a later-born disadvantage in child height which increases in poorer, rural households as well as with family size, suggesting that the effect is largely driven by resource dilution in households faced with financial constraints.

Some studies find that the negative birth order effect on child health in developing countries is likely driven by parental discrimination in resource allocation across siblings resulting from a preference for a specific birth order or gender. Behrman (1988) uses data on households in rural south India to examine how parental preferences and seasonality interact to determine nutrient allocation among siblings based on their birth order. He finds that parental preference for older children coupled with lower levels of inequality aversion in the lean season exacerbates the later-born disadvantage in a child's short-run nutritional status.[2] He also shows that the effect is particularly strong for children in low-caste, landless households where the household heads have little to no education.

Jayachandran and Pande (2017) use Demographic and Health Survey (DHS) data on

[2]Behrman (1988) uses three measures of short-run nutritional status: weight-for-height, arm circumference, and tricep skinfold thickness.

over 168,000 children under 5 years of age to show that Indian children exhibit a steeper negative birth order gradient in height relative to African children. Further, they find that the steeper birth order gradient in India holds for other indicators of child health like weight-for-age and haemoglobin levels as well as for some prenatal and postnatal inputs. They demonstrate that parental preference for the eldest son in India influences fertility decisions as well as resource allocation across siblings resulting in a steeper height drop-off with birth order in India, than in Africa.

The studies on birth order effects in developing countries discussed so far mostly use cross-sectional data to estimate birth order effects in early childhood and as such, are unable to discern whether the observed effects are transitory or permanent. Few studies examine how the relationship between birth order and child nutritional status evolves as children age. Collin (2006) employs a household fixed-effects specification on data from four rounds of the ERHS to investigate how the relationship between birth order and child nutritional status in Ethiopia develops as children age. He finds that while later-born children have worse height outcomes relative to their earlier-born siblings, they fare better in terms of short-run nutritional status. He argues that the later-born advantage in short-run nutritional status indicates that parents promote equity to allow later-born children to catch-up to their earlier-born siblings. Moreover, he finds that even with parents compensating later-born children who have worse health outcomes, birth order differences persist as children transition into adulthood.

Collin's (2006) estimates rely on an inaccurate measure of birth order constructed by assigning a birth order number to all children observed in a household based on their age-ranking. This does not reflect the true order in which children are born in a household, and can lead to inaccuracies in the estimates particularly for outcomes like height which represent cumulative investment. In this chapter, birth order is computed using the fertility histories of ever-married women, and therefore it also accounts for children who no longer reside in the household. This measure of birth order more accurately captures the true order in which children enter and transition through a household.

Conversely, Collin (2013) finds that birth order effects on child outcomes in the Philippines are largely transitory in nature. He uses data from a longitudinal survey administered in the Filipino island of Cebu, which follows a single cohort of children from birth until their early twenties. His findings demonstrate the existence of a negative birth order effect on health outcomes and educational attainment in early childhood which attenuates as children approach adulthood. Moreover, he finds some evidence of a later-born advantage relative to middle-borns, indicating that birth order effects may at times be non-monotonic. However, Collin (2013) uses data on a single cohort of children and therefore, cannot fully rule out cohort effects influencing his main results. This chapter, instead, estimates birth order effects across siblings using panel data on children of different ages, and a mother fixed-effects approach to control for unobserved time-invariant sources of maternal heterogeneity across children.

2.3. How does birth order affect child health?

Existing literature offers multiple theories on the potential mechanisms through which birth order can have an impact on child health. While these hypothesized mechanisms are often specific to the context under consideration, they can be broadly classified into three categories: biological factors, intrahousehold dynamics, and parental incentives. In this section, I consider the extent to which the empirical findings align with, or go against, each of these mechanisms.

2.3.1. Biological mechanisms

Theories on biological mechanisms point to maternal depletion as a potential explanation for the existence of a negative birth order effect on child health. Later-born children are more likely to be born to older mothers who are more at risk of congenital anomalies and preterm births (Cleary-Goldman et al., 2005). This in turn would lead to a later-born disadvantage in early health outcomes. However, existing empirical evidence shows that firstborns have worse health at birth relative to later-born children (Brenøe and Molitor, 2018; Julihn et al., 2020). These results are perhaps better aligned with alternative theories on biological mechanisms which suggest that a woman's womb becomes more effective at nurturing a fetus with each subsequent pregnancy (Khong et al., 2003). More generally, there is little support in empirical literature for biological mechanisms driving birth order differences in child health outcomes (Barclay, 2015; Lundberg and Svaleryd, 2017).

2.3.2. Intrahousehold dynamics

The household environment during childhood is hypothesised to be an important driver of birth order differences in child outcomes. The most commonly discussed theory in literature is the resource dilution hypothesis which argues that as family size increases, parental resources (time, money) per capita decrease, resulting in an unequal allocation of resources between siblings across time (Becker, 1991). This predicts a negative birth order effect as higher birth order children are often born into larger families and therefore, spend a larger proportion of their childhood competing for parental resources relative to their older siblings

The negative birth order effect predicted by the resource dilution hypothesis is stronger when the constraint in question is i) binding and ii) a significant predictor of child outcomes (Ejrnæs and Pörtner, 2004). For instance, if parental time is an important determinant of child development, then the inability to transfer parental time intertemporally could lead to negative birth order effects (Birdsall, 1991).[3] However, unlike time, parents concerned with equity might be able to allocate financial resources across time to potentially mitigate or possibly reverse the resource dilution effect.[4]

Conversely, certain factors associated with intrahousehold dynamics may systematically favour later-born children. For instance, Parish and Willis (1993) find that an increase in household wealth or earnings over time due to life cycle effects would favour later-born children relative to their earlier-born siblings. Similarly, Hatton and Martin (2010) find that later-born children are born to older, more experienced parents resulting in improved health investments.

2.3.3. Parental incentives

Birth order effects can also be the result of a parental incentive to discriminate among children, affecting resource allocation across siblings. In some cases, this incentive can stem from a parental preference for older children. For example, parents may favour first-born children as they are expected to serve as old-age security (Medina, 1991). Alternatively, in some contexts, the last-born child may be favoured if parents stop having

[3]Parental time refers to time spent by either one or both of the parents with their children on daily caregiving and other developmental activities.

[4]This assumes the existence of a functioning credit system or access to savings/credit.

children once they have achieved their desired family size or desired child gender (Basu and De Jong, 2010).

In certain countries, cultural factors may also result in parents favouring one gender over another which in turn may lead to birth order differences in child outcomes. For instance, in India, where sons plays an important role in funeral rites, Jayachandran and Pande (2017) find that parental preference for an eldest son is the main driver of the negative birth order gradient in child height.

2.4. Data

This chapter uses data from two waves of the India Human Development Survey (IHDS) series, conducted jointly by the University of Maryland and the National Council of Applied Economic Research (NCAER). The first wave of the survey (IHDS-I) was carried out between 2004 and 2005, and collected data on 215,754 individuals from 41,554 households (Desai et al., 2008). The second wave of the survey (IHDS-II) re-interviewed 83 percent of the original sample between 2011 and 2012 (Desai, Vanneman et al., 2015). In areas where more than 5 households were lost to attrition, replacement households were randomly selected and added to the sample. The final sample interviewed by the IHDS-II includes 204,659 individuals from 42,152 households.

The IHDS provides data on a wide variety of socioeconomic topics for households and individuals from both rural and urban neighbourhoods across the country. The survey typically interviews the household head to collect information on the employment status, income, and health of the household members, as well as on other aspects related to the socioeconomic condition of the household. The survey also records basic anthropometric information (height and weight) for household members available at the time of the interview. In addition, the IHDS includes a module on 'eligible women' which collects data on birth histories, education, health, family planning, and gender relations for at least one ever-married woman per household aged 15 to 49 years.

The explanatory variable of interest in the main analysis is a child's absolute birth order. Using the available birth histories of ever-married women, I define a child's absolute birth order as birth order based on all children ever born to a mother, including children who are deceased or who currently do not reside in the household. I assign the same birth order to twins and higher-order multiple births.[5] Further, for the main analysis, I top-code the birth order variable by assigning the same birth order to third and higher-order births.[6]

The outcome of interest is a child's height-for-age (HFA) z-score. An HFA z-score is a measure of linear growth progression and is widely considered to be a good indicator of a child's long-term health and nutritional status. For the analysis, I convert a child's recorded height (in centimetres) into an HFA z-score using the World Health Organisation (WHO) growth reference curves.[7] The resulting HFA z-scores account for natural gender and age differences in height. A z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. Following the WHO guidelines, z-scores less than -6 or greater than 6 are excluded from the sample as these are biologically implausible and likely to be the result of erroneous data.

For the analysis, I restrict the main sample to children with at least one sibling who are observed in both waves of the IHDS to construct a panel of siblings observed on average 7 years apart. Next, to avoid any bias stemming from unusually large families, I exclude families with 7 or more children.[8] Lastly, I restrict the sample to children with non-missing anthropometric information in both survey waves.[9] This results in a final working sample of 5,523 children aged 0 to 11 years and 7 to 18 years in the first and second survey rounds, respectively.

Figure 2.2 shows the distribution of HFA z-scores for the sample in the first survey round. Based on the WHO recommended cutoff for stunting, around 44% of the children in the sample can be classified as stunted.[10] Figure 2.3 plots the relationship between HFA z-scores and birth order (top-coded at 3) for the panel of children in each survey wave. While the variables appear to exhibit a linear negative relationship in the first survey

[5]For a child born after a multiple birth, birth order is increased by the size of the multiple birth. For example, a child born after second-born twins will be assigned a birth order of 4.

[6]Roughly 16% of the children in the sample have a birth order of 4 or higher.

[7]HFA z-scores are computed separately for children under 5 years of age and children aged 5 or older. For children under 5 years of age, the HFA z-scores are computed using the 2006 WHO growth reference curves. For children aged 5 and older, the HFA z-scores are computed using the 2007 WHO growth reference curves.

[8]The total fertility in the second wave ranges from 2 to 15. Approximately 91% of the sample children belong to families with 2 to 6 children.

[9]This results in the loss of approximately 55% of the original sample of interest due to children with missing anthropometric information in either or both survey waves. In Table 2.A.1 in the Appendix, I report summary statistics for the original sample of interest and the sample used in the analysis. The samples significantly differ in terms age and the proportion of Hindu and Muslim households.

[10]The WHO recommended z-score cut off for stunting is -2.

wave, second-born children display a relative height advantage in the second survey wave. Also, HFA z-scores increase for all birth orders between the two survey waves indicating some growth recovery from initial height deficits.

Table 2.1 reports summary statistics for the main outcomes used in the analysis as well as for child- and mother-level characteristics. The height-for-age (HFA) and body mass index (BMI) z-scores for the average sample child in the first survey wave are around 1.67 and 0.61 standard deviations (SD), respectively, below the median of the reference population. The mean birth order is 2.3 and roughly 49 percent of the sample is female. The average mother in the sample is 21 years old at the time of her first child birth, has 5 years of completed education, and has 3.5 children in the second survey wave. Roughly 68 percent of the children come from rural households with a per capita monthly income of 620 rupees at baseline. Approximately 83 percent of the households are Hindus, 11 percent are Muslim, and 6 percent belong to other religious minorities.

2.5. Empirical strategy

To examine the association between a child's birth order and nutritional status over time, I estimate the following specification:

$$
\begin{aligned}
Y_{it} =& \alpha + \beta_1 second_i + \beta_2 thirdplus_i + \theta_1 swave_t + \theta_2 (second * swave)_{it} + \\
& \theta_3 (thirdplus * swave)_{it} + \gamma X_{it} + \epsilon_{imt}
\end{aligned}
\tag{2.1}
$$

where Y_{it} is the height-for-age z-score for child i observed at time t. $Second_i$ and $thirdplus_i$ are binary indicators of birth order which equal 1 for second-born children and third- or later-born children respectively; and 0 otherwise. Here, first-born children serve as the reference group. $Swave_t$ is a dummy variable for survey wave, and equals 1 for observations in the second wave of the IHDS. X_{imt} is a vector of controls which includes child's gender and age, mother's age at first birth and age at birth, mother's education, and observed total fertility.[11] In addition, to control for differences in economic and environmental conditions which may have an impact on the outcome of interest, district

[11] The controls include dummies for child's age (in months) at first observation to address any bias that may arise due to the correlation between birth order and age within a family.

fixed-effects are included in the specification. The error term, ϵ_{imt}, is clustered at the mother level.

β_1 and β_2 reflect the initial difference in height-for-age z-scores for second-born and third- and later-born children respectively, relative to firstborns. The main coefficients of interest, θ_2 and θ_3, capture changes in these birth order effects as children age. More specifically, the coefficients capture how relative differences in height associated with birth order change as children in the sample transition into middle childhood and adolescence.

The coefficients estimated using specification (2.1) can be interpreted as the unbiased effect of birth order on height only if birth order is orthogonal to other potential determinants of the outcome of interest. However, birth order, by default, is correlated to the total number of siblings born in a family, and family size can have an independent effect on child height through its likely association with household income or its effect on the resources available per child.[12] Further, family size is endogenously determined by parents and thus, may be related to other unobservable parental characteristics that influence child height. While specification (2.1) controls for observed total fertility, the estimates may still be subject to bias if actual total fertility differs from observed total fertility on account of mothers in the sample who have not completed childbearing. To address this potential source of bias, I exploit the availability of data on more than one child born to the same mother to estimate the following mother fixed-effects specification:

$$\begin{aligned} Y_{imt} = &\alpha + \beta_1 second_{im} + \beta_2 thirdplus_{im} + \theta_1 swave_t + \theta_2 (second * swave)_{imt} + \\ &\theta_3 (thirdplus * swave)_{imt} + \gamma X_{imt} + \lambda_m + \epsilon_{imt} \end{aligned} \tag{2.2}$$

where Y_{imt} is now the height-for-age z-score for child i born to mother m observed at time t. λ_m are mother-level fixed-effects which capture all unobserved time-invariant family characteristics such as genetics and fertility preferences, which do not vary across siblings. The vector of controls, X_{imt}, now only includes child's age and gender, as well as mother's age at birth as the other controls drop out due to collinearity with mother fixed-effects. Otherwise, the remainder of the specification is identical to equation (2.1). The mother fixed-effects specification (2.2) is the preferred specification in this analysis as it uses sibling comparisons to identify birth order effects on child height over time, while holding

[12]In India and many other low-income countries, larger families tend to be poorer or have lower incomes.

eventual total family size fixed.[13]

2.6. Results

2.6.1. Height-for-age z-scores

Table 2.2 presents regression estimates for the relationship between a child's birth order and height-for-age (HFA) z-score. In column (1), I report coefficients estimated using specification (2.1) excluding the vector of controls. The results point to a negative relationship between birth order and HFA z-scores in the first survey wave, though the coefficients are not statistically significant. Further, while the estimates indicate that HFA z-scores increase across all birth orders as children age, the magnitude of the effect appears larger for second-born children relative to firstborns.

Once I add controls for potential confounders in column (2), the later-born disadvantage in initial HFA z-scores increases in magnitude and is significant. The estimates suggest that on average, second- and third- or later-born children are 0.41 SD and 0.51 SD shorter, respectively, relative to firstborns. As the children in the sample transition into middle-childhood and adolescence in the second survey wave, HFA z-scores increase by 0.16 SD across all birth orders, and the relative height gap between first- and second-born children appears to reduce by approximately 0.13 SD.

In column (3), I add mother fixed-effects to the main specification (equation 2.2) to control for time-invariant unobserved maternal heterogeneity which may be correlated with the outcome of interest. The resulting estimates account for potentially endogenous total fertility which is assumed to be constant within mothers. The birth order gradient in initial HFA z-scores is now steeper relative to column (2) with second- and third- or later-born children being 0.55 SD and 0.84 SD shorter on average, respectively, relative to their first-born sibling. The estimates for the change in HFA z-scores as siblings age, are identical to those in columns (1) and (2) for each birth order. As a further check, I report child fixed-effects estimates in column (4). The main estimates appear robust to

[13] The main sample is a balanced child-level panel and thus, allows for the inclusion of child fixed-effects in the specification. This is likely to be a stronger control for sources of time-invariant unobserved heterogeneity across children such as differences in genetic composition across siblings born to the same mother. However, the use of child fixed-effects does not allow for the identification of birth order effects on initial HFA z-scores as the required variables drop from the specification due to collinearity. Since the main analysis aims to first demonstrate the existence of an initial birth order gradient in height, the child fixed-effects estimations are provided only as a robustness check.

controlling for time-invariant unobservable child characteristics.

The main finding first demonstrates a later-born disadvantage in initial HFA z-scores for children aged 0 to 11 years in the first survey wave. This is consistent with previous literature which establishes a negative relationship between birth order and height in developing countries (Collin, 2006; Jayachandran and Pande, 2017; Bishwakarma and Villa, 2019). Another aspect of the main finding is that second-born children exhibit an improvement in HFA z-scores relative to their first-born siblings. However, the estimated increase in height is not sufficient to suggest complete catch-up growth indicating that firstborns continue to experience an advantage in height outcomes as they age, relative to their later-born siblings. This effectively implies that height deficits driven by birth order are largely persistent in nature over a child's development period.

Therefore, concerning the question of whether birth order driven height gaps are transitory or permanent, my findings corroborate those of Collin (2006) who finds that birth order continues to matter for child outcomes at the end of a child's growth period in rural Ethiopia. On the other hand, my findings are in contrast to another closely related study by Collin (2013) which finds that birth order differences in health outcomes in the Philippines are largely transitory in nature. The variation in findings could arise on account of different methodologies or alternatively, could highlight the importance of context in determining birth order differences in child outcomes.[14] Moreoever, in the debate over childhood growth faltering and the potential for later-life recovery, my findings appear to lend support to the view that catch-up growth which erases early childhood height deficits is unlikely to occur (Martorell et al., 1994; Monyeki et al., 2000). However, the present data does not permit a further exploration of potential factors contributing to the persistence of the birth order driven height gap observed in the sample.

2.6.2. Body Mass Index

Next, I will examine the relationship between birth order and a child's short-term nutritional status. Weight-for-age and weight-for-height are commonly used indicators to assess a child's short-term nutritional status. While children with low weight-for-age (WFA) scores are categorised as "underweight", WFA is a composite indicator which

[14] Context-specific factors which may influence birth order differences in child outcomes could refer to the level of initial height deficits which are shown to be severe in India. It could also refer to parental preferences which are likely to differ across countries and may lead to differences in birth order effects.

does not distinguish between stunted and wasted children, and therefore, may be difficult to interpret. Weight-for-height (WFH), on the other hand, is used to identify children who are too thin for their given height or "wasted". A low WFH score indicates that a child suffers from acute undernutrition and is often the result of insufficient food intake or frequent exposure to infectious diseases. Thus, weight-for-height is a better measure of a child's short-term nutritional status relative to weight-for-age.

For the analysis, I use Body Mass Index (BMI) as an alternative to weight-for-height as the WHO growth standards computes WFH z-scores only for children up to 5 years of age. BMI is often used as an indicator for adult malnutrition and is almost identical in meaning to WFH. I compute BMI-for-age z-scores for the children in the sample using the WHO growth reference curves. The resulting z-scores account for gender- and age-specific differences in BMI. As with HFA z-scores, a BMI-for-age z-score of 0 indicates that the child's BMI is equal to the median of the age- and gender-specific reference population.[15] In line with the WHO guidelines, BMI-for-age z-scores greater than 5 or less than -5 are dropped from the sample due to biological implausibility.

In the absence of data on nutritional investments over childhood, BMI scores serve as a proxy for a child's recent nutritional intake. Behrman's (1988) model of intra-household resource allocation argues that holding constant parental preferences for birth order, parents who are sufficiently inequality averse will identify and compensate children with worse health outcomes.[16] On the other hand, parents concerned with quality will direct resources towards children with higher levels of initial endowment. Therefore, examining birth order differences in BMI scores could offer some insight into parental behaviour with respect to nutritional allocation across children. In this analysis, a positive birth order effect on BMI-for-age z-scores would suggest that parents follow a compensatory strategy, whereas a negative birth order effect would reflect that parents reinforce initial differences in child health.

Table 2.3 is organised identical to Table 2.2, and reports regression results for the effect of birth order on BMI-for-age z-scores. Across the columns, the estimates suggest that birth order is positively associated with BMI in the first survey wave. The preferred specification which includes mother fixed-effects (column (3)) suggests that on average,

[15] As per the WHO recommendations, a child with a BMI-for-age z-score less than -2 is classified as 'thin' or 'wasted'.

[16] A discussion of Behrman's (1988) theoretical model is provided in Section 2.A.2 in the Appendix.

the BMI-for-age z-score for second- and third- or later-born children is 0.13 SD and 0.21 SD higher, respectively, relative to that of their first-born siblings. The estimates also indicate that the BMI-for-age z-score declines by 0.23 SD across all birth orders, but the magnitude of the decline appears larger for third- or later-born children, relative to their first-born siblings.[17] The results do not change when child fixed-effects are added to the specification in column (4).

The later-born advantage in BMI observed in the first survey wave may suggest that parents in the sample exhibit some degree of inequality aversion, and therefore identify and compensate children who are lagging behind their siblings in terms of health outcomes. If a child's short-term nutritional status in linked to changes in growth in subsequent periods, then a compensatory strategy in terms of nutritional inputs could potentially lead to catch-up growth for later-born children (He and Karlberg, 2001). As seen in Table 2.2, only second-born children experience some catch-up growth as they age, but birth order driven height differences remain. However, BMI is reflective of short-term nutritional allocation, and patterns of nutritional allocation across children may change over time between the two survey waves. Therefore, it is unclear whether the lack of catch-up of growth is due to the absence of a sustained compensatory strategy or due to the potentially permanent nature of height deficits that occur in childhood.

The results also suggest that children in the sample become increasingly undernourished as they age. This is in keeping with evidence which documents the wide prevalence of malnutrition in Indian children between the ages of 6 to 17 (Shroff and Shokeen, 2019; Bhargava et al., 2020). The emergence of birth order differences in BMI scores in the second survey wave is harder to interpret. It could reflect shifts in parental behaviour with respect to nutritional allocation across children as they age. For instance, an increase in nutritional investments required by children as they grow older may result in parents favouring healthier (taller) first-born children. Alternatively, earlier-born children who are likely to be older may be allocated more nutrition if they participate in the labour market.

[17] A t-test rejects the equivalence of BMI-for-age z-scores between first- and third- or later-born children in the second survey wave at the 5% level.

2.7. Heterogeneity analysis

2.7.1. Family size

The sample used in the main analysis consists of families which have between 2 and 6 children at the end of the second survey wave. Children with higher birth orders, by default, are more likely to be born into larger families, and larger families may systematically differ in ways which could matter for a child's nutritional status. While the inclusion of mother fixed-effects controls for total family size, larger families may still be more likely to contribute to the estimation of higher birth order effects in the main sample.

To examine whether heterogeneity across family sizes plays a role in determining the relationship between birth order and child nutritional status observed in Table 2.2, I re-estimate the main specification separately for each family size in the sample.[18] The regression results are reported in Table 2.4. The estimated birth order effect across each family size largely mirrors the main results for the full sample. The mother fixed-effects estimates indicate a negative birth order gradient in HFA z-scores in the first survey wave irrespective of family size. The birth order effect on initial HFA z-scores also appears to differ in magnitude across family sizes, but remains consistently higher for later-born siblings.

Further, while there appears to be an improvement in HFA z-scores across all birth orders and family sizes as siblings age, the relative birth order gap in height persists. Similar to the results in Table 2.2, the coefficient on second-born siblings in the second survey wave is positive regardless of family size, though it is not statistically significant. The results in Table 2.4 imply that heterogeneity across families of different sizes might explain some of the birth order effects, particularly on the initial HFA z-scores of later-born siblings. However, it cannot fully explain the observed birth order effects as the pattern of results is consistent across all family sizes. The main takeaway from this analysis is that birth order differences in height persist as children age, irrespective of family size.

2.7.2. Heterogeneity across age groups

So far, the main results indicate that birth order deficits in initial height outcomes largely persist as children transition into middle childhood and adolescence. This suggests that

[18]Families with 5 or 6 children are combined with four-child families due to the small sample size.

there is little to no recovery in birth order driven height deficits across all children who are aged 0 to 11 years at first observation. However, the potential for a child's growth recovery may not be uniform across all ages, and may depend instead, on the child's stage of growth development.

There is a lack of consensus in existing literature on the age at which the window for catch-up growth closes. While earlier studies point out that growth deficits in children are largely irreversible after 2 years of age, more recent research indicates that there is potential for growth recovery well beyond infancy even in the absence of interventions (Prentice et al, 2013). Therefore, to examine if the association between birth order and HFA z-scores observed in Table 2.2 differs based on a child's stage of growth development, I re-estimate the effect separately for three distinct periods of child growth: infancy (up to 2 years of age), post-infancy (between 2 and 5 years of age), and middle childhood (between 5 and 11 years of age).[19] The results are reported in Table 2.5.

The estimated birth order effects for each age group reflect a pattern similar to the main results, though the estimates for the effect in the second-survey wave are not consistently statistically significant. In the first survey wave, birth order appears to be negatively associated with HFA z-scores regardless of the child's stage of growth development, but the magnitude of the effect is lower for older children. Further, second-born children aged 2 to 5 years in the first survey wave appear to exhibit some catch-up growth as they age, relative to firstborns. While the corresponding coefficient for second-born children in the infant and middle childhood age group is positive, it is not statistically significant. The results are robust to the inclusion of child fixed-effects.

The results in Table 2.5 indicate that irrespective of the stage of growth development, first-born children retain their initial height advantage as they age. Though limited in scope, the estimates also provide suggestive evidence that there might be potential for growth recovery beyond infancy. However, the observed recovery does not correspond to complete catch-up growth, and does not appear to occur in children older than 5 years at first observation.

[19] The reported specifications in this section examine birth order effects across children and not between siblings.

2.7.3. Son preference

The prevalence of son preference in India and its implications for gender gaps in child health and cognitive outcomes are well-established (Pande, 2003; Pande and Malhotra, 2006; Bose, 2012; Pillai and Ortiz-Rodriguez, 2015). In the context of this study, son preference could lead to birth order differences in health outcomes through its influence on parents' fertility decisions or on how resources are allocated across siblings. This is confirmed in a recent study by Jayachandran and Pande (2017) which demonstrates that a parental preference for an eldest son results in a steeper height drop-off for later-born children in India than in Africa.

First, to examine if a general preference for sons influences the evolution of the observed birth order gradient in height, I allow the effect of birth order to vary by gender. The results reported in Table 2.6 indicate that birth order driven deficits in intial height are not systematically different for daughters relative to sons. Further, there appears to be no difference in how these effects evolve for sons and daughters as they age.

Next, to examine whether sibling sex composition plays a role in the evolution of birth order effects with age, I estimate the main specification separately for families based on the sex of the first-born child. The mother fixed-effects estimates presented in Table 2.7 are largely similar to the main results. However, the initial birth order gradient in height appears to be steeper in families with a first-born son than in families with a first-born daughter. This is in line with the view that a preference for eldest sons in India exacerbates the birth order gradient in height.

The estimates also indicate that HFA z-scores increase as children age in the second survey wave irrespective of the sex of the first-born child, though the effect appears to be more pronounced in families with first-born boys. Moreover, a statistically significant reduction in the height deficit for second-born children relative to their first-born siblings is observed in families with first-born girls. The lack of catch-up growth in second-born children in families with first-born boys may suggest that families with a strong preference for an eldest son may invest more resources in first-born boys relative to later-born children.

Lastly, I examine whether there is any variation in how birth order effects evolve between Hindu and Muslim families as Hinduism is known to place a stronger emphasis on having a male child relative to Islam. To do this, I re-estimate the main specification interacting birth order with an indicator for Muslim families.[20] The results are reported in Table 2.8. The mother fixed-effects estimates suggest that third- or later-born children in Muslim households are taller than their counterparts in Hindu households. The coefficient for second-born Muslim children is also positive, but not statistically significant. These results align with the view that the birth order gradient, at least in initial height, will be steeper in families which exhibit higher levels of son preference.

Further, the estimates in Table 2.8 indicate that HFA z-scores increase across all birth orders as children age, in Hindu and Muslim households. There is also evidence of a reduction in the birth order gradient for second-born Hindu children in the second survey wave, relative to their first-born siblings. This effect does not appear to be significantly different for second-born children in Muslim households. Overall, the results in this section suggest that son preference might play a role in how birth order differences in height evolve as children age. However, similar to the main findings, the firstborn advantage in initial height appears to persist across groups that exhibit different levels of son preference.

2.7.4. Mother's education

Maternal education has been found to be associated with a more efficient allocation of resources across children as well as better child health outcomes (Grossman, 2006; Semba et al., 2008; Le and Nguyen, 2020*b*; Bras and Mandemakers, 2022). Given this, I next examine whether maternal education plays a role in determining how birth order effects on siblings' height evolve with age.

In Table 2.9, I re-estimate the main specification interacting birth order with completed years of maternal education. The results do not indicate a significant difference in the negative birth order gradient in initial HFA z-scores for siblings born to more educated mothers, relative to those born to less educated mothers. Similar to the main results, the coefficient on second-born children in the second survey wave is positive, though it is not statistically significant. The estimates are robust to the inclusion of child fixed-effects.

Mainly, the results in Table 2.9 suggest that the firstborn advantage in height among siblings persists with age, irrespective of maternal education levels.[21] Moreover, unlike

[20] These results are likely to be imprecisely estimated due to the relatively small number of Muslim families in the sample.

[21] An alternative specification which interacts birth order with a binary indicator for whether a mother has completed primary education yields similar results.

Bras and Mandemakers (2022), I do not find evidence that more educated mothers are associated with a shallower birth order gradient in child height compared to less educated mothers.

2.8. Robustness checks

2.8.1. Relative birth order

A concern surrounding the identification of birth order effects on the outcome of interest is the correlation between family size and absolute birth order. In the main analysis, I address this issue using a mother fixed-effects specification which estimates birth order effects holding constant total family size. However, some studies employ an alternative measure of birth order known as relative birth order which by construction reduces the correlation between birth order and family size (Ejrnæs and Pörtner, 2004; Collin, 2006). In this section, I will check the robustness of the main results to an alternative specification where relative birth order is the explanatory variable of interest.

Relative birth order is defined as $\frac{(b-1)}{(n-1)}$, where b is the child's birth order and n is the total sibship size. This transformation ensures that a firstborn child will have a relative birth order of 0 and a last-born child will have a relative birth order of 1. The resulting birth order estimate will capture the effect of being a last-born child relative to firstborns. Moreover, if being a last-born child matters for outcomes in its own right, then the coefficient on relative birth order would be able to better capture this effect in comparison to absolute birth order.[22]

In column (1) of Table 2.10, I report mother fixed-effects estimates for HFA z-scores using relative birth order as the independent variable. The main findings remain largely unchanged. Later-born children have worse HFA z-scores relative to their first-born siblings. Moreover, while there is an increase in HFA z-scores across all birth orders as children in the sample age, the relative birth order height gap between siblings does not appear to change. The estimates are robust to the inclusion of child fixed-effects in column (2).

[22] A last-born child may be favoured by parents simply for being the youngest. In addition, they are the only ones in sibships to not have younger siblings as well as be more likely to have siblings significantly older than them.

2.8.2. Completed fertility sample

In the main analysis, I use mother fixed-effects to address omitted variable bias concerns arising due to unknown total family size for mothers in the sample who have not completed childbearing. In this section, I will check the robustness of the main results using a subsample of mothers who can be considered to have completed fertility by the second survey wave.

To construct the completed fertility sample, I use data on the fertility preferences and outcomes of ever-married women collected in the second survey wave. First, I drop all women who report being pregnant during the second survey wave. Next, I exclude from the sample all women who give birth in the period between the two survey waves. This leaves 3,636 children in the sample who are born to mothers who can be assumed to have completed fertility based on a long interval of no childbearing.[23]

In Table 2.11, I present regression estimates for the effect of birth order on HFA z-scores for children in the completed fertility sample. The results corroborate the main findings suggesting that mother fixed-effects sufficiently control for bias that may arise due to incomplete information on the total fertility of mothers in the sample who have not completed childbearing.

2.9. Conclusion

In this chapter, I use panel data on a sample of Indian children to explore how birth order effects on child height evolve with age. To identify the effect of interest, I exploit the availability of data on siblings and employ a mother fixed-effects approach. The main results indicate that birth order is negatively associated with initial HFA z-scores for children aged 0 to 11 years in the first survey round. This confirms findings from earlier empirical work which shows the existence of a negative birth order gradient in child height in developing countries.

Further, I find that height outcomes improve across all birth orders as children transition into middle childhood and adolescence in the second survey wave. The results also point to a reduction in height deficits between first- and second-born siblings, though the estimated effect is not sufficient to suggest complete catch-up. The observed pattern

[23]The method to define a completed fertility sample is taken from Jayachandran and Pande (2017).

of birth order effects largely holds irrespective of family size, the child's stage of growth development, differences in parental preference for sons, and maternal education levels. The results are also robust to using an alternative measure of birth order.

The main finding of this study is that the first-born children in the sample retain their advantage in initial height with age, suggesting that birth order effects on height are persistent in nature. While my findings lend support to an existing study by Collin (2006) which finds similar effects in rural Ethiopia, they are in contrast to those of Collin (2013) who argues that birth order effects on height are largely transitory in nature in the Philippines. Given that these studies examine birth order effects across different contexts, it is possible that the association between birth order effects and age is sensitive to the context under consideration. This is in keeping with the idea that mechanisms driving birth order effects may vary over time and tend to be context-specific (Modin, 2002).[24]

In addition to the main results, I find that birth order is positively associated with BMI in the first survey wave. Given the negative birth order gradient in height, a later-born advantage in BMI may reflect that inequality averse parents follow a compensatory strategy in child health. However, the effect does not persist as children age in the second survey wave. Further, a heterogeneity analysis of the birth order effect on height based on a child's stage of growth development provides suggestive evidence that the potential for growth recovery may exist beyond infancy. However, data limitations do not permit a further investigation of these effects.

A limitation of this study is that the sample used in the analysis is restricted to include siblings observed in both waves of the survey and for whom anthropometric information is available. While the findings help inform an understanding of birth order effects for a specific population, analysis using nationally representative samples across multiple contexts will help better evaluate the role of context in determining birth order effects on outcomes of interest. Moreover, this study is unable to address whether birth order effects persist due to the lack of potential for later-life recovery or due to absence of sustained investments in worse-off children. Given that this study finds suggestive evidence of growth recovery, further research on key mechanisms that trigger such catch-up growth could help inform policy aimed at addressing birth order differences in child nutritional status.

[24]For instance, India and the Philippines are likely to differ in the levels of initial height deficits as well as parental preferences, both of which may influence the evolution of birth order effects with age.

2.10. Tables

Table 2.1: Summary statistics

	Wave-I (2001-05)		Wave-II (2011-12)	
	mean	sd	mean	sd
Outcomes				
HFA z-score	-1.67	(1.98)	-1.48	(1.31)
BMI z-score	-0.61	(1.60)	-1.08	(1.36)
Child characteristics				
Birth order			2.34	(1.20)
Age (in years)	4.95	(3.25)	12.05	(3.27)
Girl			0.49	(0.50)
Mother's age at birth			24.06	(4.38)
Mother characteristics				
Mother's age at first birth			20.84	(3.35)
Total fertility			3.53	(1.22)
Mother's height			152.02	(7.08)
Mother's completed education (in years)			4.92	(4.57)
Household characteristics				
Rural			0.68	(0.46)
Monthly household income per capita (in 1'000 rupees)	0.62	(0.72)	1.66	(2.15)
Hindu			0.83	(0.37)
Muslim			0.11	(0.31)
Others			0.06	(0.24)
Observations	5523		5523	

Notes: HFA z-score refers to height-for-age z-score and BMI z-score refers to the Body Mass Index-for-age z-score. The z-scores are computed using the WHO growth standards. An HFA (BMI) z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA (BMI) z-score $\leq$-2 is classified as stunted (wasted).

Table 2.2: Birth order and HFA z-scores

	(1)	(2)	(3)	(4)
2^{nd} child	-0.088	-0.413***	-0.553***	-
	(0.055)	(0.062)	(0.070)	(.)
3^{rd}+ child	-0.074	-0.512***	-0.837***	-
	(0.066)	(0.089)	(0.108)	(.)
wave-II (2012)	0.155***	0.155***	0.155***	0.155***
	(0.043)	(0.044)	(0.044)	(0.043)
2^{nd} child × wave-II (2012)	0.127**	0.127**	0.127**	0.127**
	(0.055)	(0.056)	(0.055)	(0.055)
3^{rd}+ child × wave-II (2012)	0.000	0.000	0.000	0.000
	(0.065)	(0.066)	(0.065)	(0.065)
Observations	11,046	11,046	11,046	11,046
Controls	No	Yes	Yes	No
Mother FE	No	No	Yes	No
Child FE	No	No	No	Yes

Notes: HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA z-score $\leq$-2 is classified as stunted. Birth order is top-coded at 3; 2^{nd} child refers to a child with birth order 2 and 3^{rd}+ child refers to a child with birth order 3 or higher. Controls in column (2) include district fixed-effects, mother's age at first birth, mother's age at birth, mother's age at birth squared, mother's education (in years), mother's observed total fertility, and dummies for gender, child's age (in months) in the first survey wave and rural status of the household. Sibling-invariant controls and time-invariant controls drop out in columns (3) and (4), respectively. Standard errors clustered at mother level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 2.3: Birth order and BMI-for-age z-scores

	(1)	(2)	(3)	(4)
2^{nd} child	0.210***	0.091*	0.134**	-
	(0.046)	(0.051)	(0.056)	(.)
3^{rd}+ child	0.273***	0.153**	0.206**	-
	(0.054)	(0.074)	(0.086)	(.)
wave-II (2012)	-0.237***	-0.238***	-0.234***	-0.238***
	(0.046)	(0.047)	(0.046)	(0.046)
2^{nd} child × wave-II (2012)	-0.228***	-0.227***	-0.225***	-0.227***
	(0.054)	(0.055)	(0.054)	(0.054)
3^{rd}+ child × wave-II (2012)	-0.417***	-0.422***	-0.428***	-0.407***
	(0.063)	(0.065)	(0.064)	(0.064)
Observations	10,800	10,800	10,800	10,800
Controls	No	Yes	Yes	No
Mother FE	No	No	Yes	No
Child FE	No	No	No	Yes

Notes: BMI-for-age z-score refers to the Body Mass Index-for-age z-score and is computed using the WHO growth standards. A BMI z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with BMI z-score $\leq$-2 is classified as wasted.The sample size for BMI is smaller relative to Table 2.2 due to missing weight and/or implausible BMI-for-age z-scores. Birth order is top-coded at 3; 2^{nd} child refers to a child with birth order 2 and 3^{rd}+ child refers to a child with birth order 3 or higher. Controls in column (2) include district fixed-effects, mother's age at first birth, mother's age at birth, mother's age at birth squared, mother's education (in years), mother's observed total fertility, and dummies for gender, child's age (in months) in the first survey wave and rural status of the household. Sibling-invariant controls and time-invariant controls drop out in columns (3) and (4), respectively. Standard errors clustered at mother level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 2.4: Heterogeneity analysis: Birth order, HFA z-scores and family size

	Two-child families		Three-child families		$\geq$ Four-child families	
	(1)	(2)	(3)	(4)	(5)	(6)
2^{nd} child	-1.042***	-	-0.651***	-	-0.415***	-
	(0.165)	(.)	(0.132)	(.)	(0.115)	(.)
3^{rd}+ child			-1.044***	-	-0.724***	-
			(0.230)	(.)	(0.150)	(.)
wave-II (2012)	0.117	0.117*	0.189**	0.189**	0.175**	0.175**
	(0.072)	(0.070)	(0.079)	(0.078)	(0.077)	(0.076)
2^{nd} child $\times$ wave-II (2012)	0.131	0.131	0.138	0.138	0.089	0.089
	(0.101)	(0.098)	(0.095)	(0.093)	(0.098)	(0.097)
3^{rd}+ child $\times$ wave-II (2012)			-0.026	-0.026	-0.023	-0.023
			(0.121)	(0.118)	(0.095)	(0.094)
Observations	2,558	2,558	3,484	3,484	5,004	5,004
Controls	Yes	No	Yes	No	Yes	No
Mother FE	Yes	No	Yes	No	Yes	No
Child FE	No	Yes	No	Yes	No	Yes

Notes: HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA z-score $\leq$-2 is classified as stunted. Birth order is top-coded at 3; 2^{nd} child refers to a child with birth order 2 and 3^{rd}+ child refers to a child with birth order 3 or higher. Columns (5) and (6) include families with 4 to 6 children in the second survey wave. Controls in column (1), (3) and (5) include mother's age at birth, mother's age at birth squared, and dummies for gender and child's age (in months) in the first survey wave. Time-invariant controls drop out in columns (2), (4) and (6). Standard errors clustered at mother level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 2.5: Heterogeneity analysis: Birth order, HFA z-scores and age-groups

	0 to 24 months		25 to 60 months		$\geq$61 months	
	(1)	(2)	(3)	(4)	(5)	(6)
2^{nd} child	-0.837**	-	-0.714***	-	-0.228***	-
	(0.415)	(.)	(0.115)	(.)	(0.078)	(.)
3^{rd}+ child	-0.533	-	-1.058***	-	-0.496***	-
	(0.460)	(.)	(0.165)	(.)	(0.107)	(.)
wave-II (2012)	0.005	0.005	0.358***	0.358***	0.054	0.054
	(0.385)	(0.354)	(0.077)	(0.074)	(0.055)	(0.053)
2^{nd} child $\times$ wave-II (2012)	0.128	0.128	0.211**	0.211**	0.087	0.087
	(0.408)	(0.375)	(0.106)	(0.101)	(0.074)	(0.071)
3^{rd}+ child $\times$ wave-II (2012)	-0.227	-0.227	0.164	0.164	0.006	0.006
	(0.409)	(0.376)	(0.109)	(0.104)	(0.084)	(0.081)
Observations	1,934	1,934	3,788	3,788	5,324	5,324
Controls	Yes	No	Yes	No	Yes	No
Child FE	No	Yes	No	Yes	No	Yes

Notes: HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA z-score $\leq$-2 is classified as stunted. Birth order is top-coded at 3; 2^{nd} child refers to a child with birth order 2 and 3^{rd}+ child refers to a child with birth order 3 or higher. Controls in column (1), (3) and (5) include district fixed-effects, mother's age at first birth, mother's age at birth, mother's age at birth squared, mother's education (in years), mother's observed total fertility, and dummies for gender, child's age (in months) in the first survey wave and rural status of the household. Time-invariant controls drop out in columns (2), (4) and (6). Standard errors clustered at mother level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 2.6: Heterogeneity analysis: Birth order, HFA z-scores, and gender

	(1)	(2)
2^{nd} child	-0.530***	-
	(0.096)	(.)
3^{rd}+ child	-0.831***	-
	(0.127)	(.)
wave-II (2012)	0.205***	0.205***
	(0.063)	(0.062)
2^{nd} child × wave-II (2012)	0.111	0.111
	(0.087)	(0.086)
3^{rd}+ child × wave-II (2012)	-0.022	-0.022
	(0.091)	(0.090)
2^{nd} child X female	-0.045	-
	(0.126)	(.)
3^{rd}+ child × female	-0.012	-
	(0.125)	(.)
wave-II (2012) × female	-0.096	-0.096
	(0.087)	(0.086)
2^{nd} child × wave-II (2012) × female	0.026	0.026
	(0.127)	(0.127)
3^{rd}+ child × wave-II (2012) × female	0.035	0.035
	(0.127)	(0.127)
Observations	11,046	11,046
Controls	Yes	No
Mother FE	Yes	No
Child FE	No	Yes

Notes: HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA z-score ≤-2 is classified as stunted. Birth order is top-coded at 3; 2^{nd} child refers to a child with birth order 2 and 3^{rd}+ child refers to a child with birth order 3 or higher. Controls in column (1) include mother's age at birth, mother's age at birth squared, and dummies for child's age (in months) in the first survey wave. Time-invariant controls drop out in column (2). Standard errors clustered at mother level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 2.7: Heterogeneity analysis: Birth order, HFA z-scores, and sex of the first-born child

	First–born: Boy		First-born: Girl	
	(1)	(2)	(3)	(4)
2^{nd} child	-0.635***	-	-0.491***	-
	(0.102)	(.)	(0.099)	(.)
3^{rd}+ child	-0.875***	-	-0.790***	-
	(0.155)	(.)	(0.147)	(.)
wave-II (2012)	0.203***	0.203***	0.110*	0.110*
	(0.063)	(0.062)	(0.061)	(0.060)
2^{nd} child × wave-II (2012)	0.091	0.091	0.159**	0.159**
	(0.080)	(0.079)	(0.078)	(0.077)
3^{rd}+ child × wave-II (2012)	-0.122	-0.122	0.097	0.097
	(0.098)	(0.096)	(0.089)	(0.088)
Observations	5,028	5,028	6,014	6,014
Controls	Yes	No	Yes	No
Mother FE	Yes	No	Yes	No
Child FE	No	Yes	No	Yes

Notes: HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA z-score $\leq$-2 is classified as stunted. Birth order is top-coded at 3; 2^{nd} child refers to a child with birth order 2 and 3^{rd}+ child refers to a child with birth order 3 or higher. Controls in columns (1) and (3) include mother's age at birth, mother's age at birth squared, and dummies for child's age (in months) in the first survey wave. Time-invariant controls drop out in columns (2) and (4). Standard errors clustered at mother level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 2.8: Heterogeneity analysis: Birth order, HFA z-scores, and religion

	(1)	(2)
2^{nd} child	-0.601***	-
	(0.076)	(.)
3^{rd}+ child	-0.930***	-
	(0.115)	(.)
2^{nd} child × muslim	0.220	-
	(0.190)	(.)
3^{rd}+ child × muslim	0.497**	-
	(0.222)	(.)
wave-II (2012)	0.137***	0.137***
	(0.048)	(0.047)
2^{nd} child × wave-II (2012)	0.151**	0.151**
	(0.060)	(0.059)
3^{rd}+ child × wave-II (2012)	0.058	0.058
	(0.071)	(0.070)
wave-II (2012) × muslim	-0.035	-0.035
	(0.151)	(0.149)
2^{nd} child × wave-II (2012) × muslim	-0.177	-0.177
	(0.193)	(0.192)
3^{rd}+ child × wave-II (2012) × muslim	-0.356	-0.356
	(0.221)	(0.220)
Observations	10,390	10,390
Controls	Yes	No
Mother FE	Yes	No
Child FE	No	Yes

Notes: HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA z-score ≤-2 is classified as stunted. Birth order is top-coded at 3; 2^{nd} child refers to a child with birth order 2 and 3^{rd}+ child refers to a child with birth order 3 or higher. Controls in column (1) include mother's age at birth, mother's age at birth squared, and dummies for child's age (in months) in the first survey wave. Time-invariant controls drop out in column (2). Standard errors clustered at mother level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 2.9: Birth order, HFA z-scores, and mother's education

	(1)	(2)
2^{nd} child	-0.478***	-
	(0.101)	(.)
3^{rd}+ child	-0.713***	-
	(0.135)	(.)
wave-II (2012)	0.191***	0.191***
	(0.068)	(0.067)
2^{nd} child × wave-II (2012)	0.070	0.070
	(0.083)	(0.083)
3^{rd}+ child × wave-II (2012)	-0.032	-0.032
	(0.094)	(0.093)
2^{nd} child × mother's education	-0.012	-
	(0.012)	(.)
3^{rd}+ child × mother's education	-0.025	-
	(0.016)	(.)
wave-II (2012) × mother's education	-0.006	-0.006
	(0.010)	(0.009)
2^{nd} child × wave-II (2012) × mother's education	0.010	0.010
	(0.012)	(0.012)
3^{rd}+ child × wave-II (2012) × mother's education	0.005	0.005
	(0.016)	(0.016)
Observations	11,046	11,046
Controls	Yes	No
Mother FE	Yes	No
Child FE	No	Yes

Notes: Mother's education is measured as completed years of education. HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA z-score $\leq$-2 is classified as stunted. Birth order is top-coded at 3; 2^{nd} child refers to a child with birth order 2 and 3^{rd}+ child refers to a child with birth order 3 or higher. Controls in column (1) include mother's age at birth, mother's age at birth squared, and dummies for child's age (in months) in the first survey wave. Time-invariant controls drop out in column (2). Standard errors clustered at mother level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 2.10: Robustness check: Relative birth order and HFA z-scores

	(1)	(2)
Relative birth order	-0.748***	-
	(0.096)	(.)
wave-II (2012)	0.166***	0.166***
	(0.040)	(0.040)
Relative birth order × wave-II (2012)	0.062	0.062
	(0.064)	(0.064)
Observations	11,046	11,046
Controls	Yes	No
Mother FE	Yes	No
Child FE	No	Yes

Notes: HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA z-score $\leq$-2 is classified as stunted. Relative birth order is defined as $\frac{(b-1)}{(n-1)}$, where b is the child's birth order and n is the total sibship size. First-born children will have a relative birth order of 0 and last-born children will have a relative birth order of 1. Controls in column (1) include mother's age at birth, mother's age at birth squared, and dummies for child's age (in months) in the first survey wave. Time-invariant controls drop out in column (2). Standard errors clustered at mother level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 2.11: Robustness check: Birth order and HFA z-scores in the completed fertility sample

	(1)	(2)
2^{nd} child	-0.492***	-
	(0.085)	(.)
3^{rd}+ child	-0.772***	-
	(0.127)	(.)
wave-II (2012)	0.123**	0.123**
	(0.055)	(0.055)
2^{nd} child × wave-II (2012)	0.163**	0.163**
	(0.068)	(0.068)
3^{rd}+ child × wave-II (2012)	0.073	0.073
	(0.078)	(0.078)
Observations	7,272	7,272
Controls	Yes	No
Mother FE	Yes	No
Child FE	No	Yes

Notes: The completed fertility sample includes all mothers who reported not being pregnant in the second survey wave, and who did not give birth in the period between the two survey waves. HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA z-score $\leq$-2 is classified as stunted. Birth order is top-coded at 3; 2^{nd} child refers to a child with birth order 2 and 3^{rd}+ child refers to a child with birth order 3 or higher. Controls in column (1) include mother's age at birth, mother's age at birth squared, and dummies for child's age (in months) in the first survey wave. Time-invariant controls drop out in column (2). Standard errors clustered at mother level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

2.11. Figures

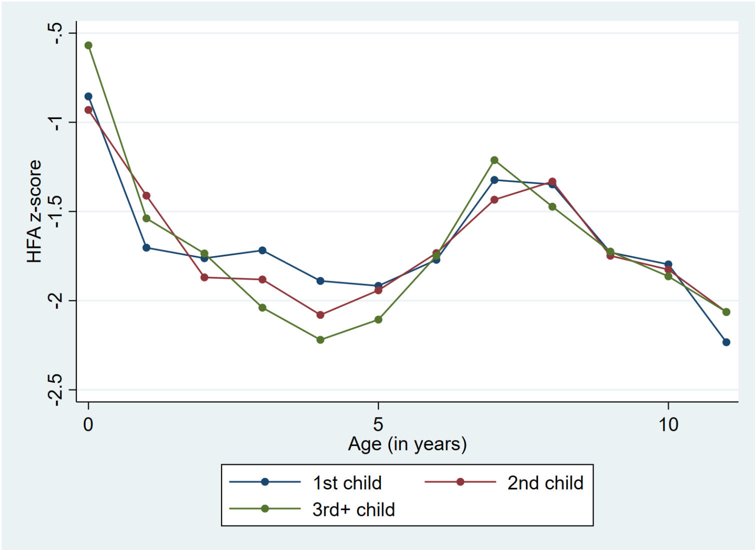


Figure 2.1: HFA z-scores, birth-order, and age in IHDS-I

Notes: HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. Children in the sample are aged 0 to 11 years in IHDS-I. The WHO growth standards computes HFA z-scores with reference to optimal normal child growth. The children in the sample appear to exhibit an upward trajectory in height between the ages of 5 and 7 across all birth orders. While the reason for this trend is unclear, it might to some extent reflect a shift in height velocity at age 5 and subsequently at age 7. A study by Khadilkar et al. (2019) finds that the median height velocity in a sample of 'healthy' Indian children increases between the ages of 8 and 10 for girls, and between the ages of 10 and 13 for boys, and declines thereafter.

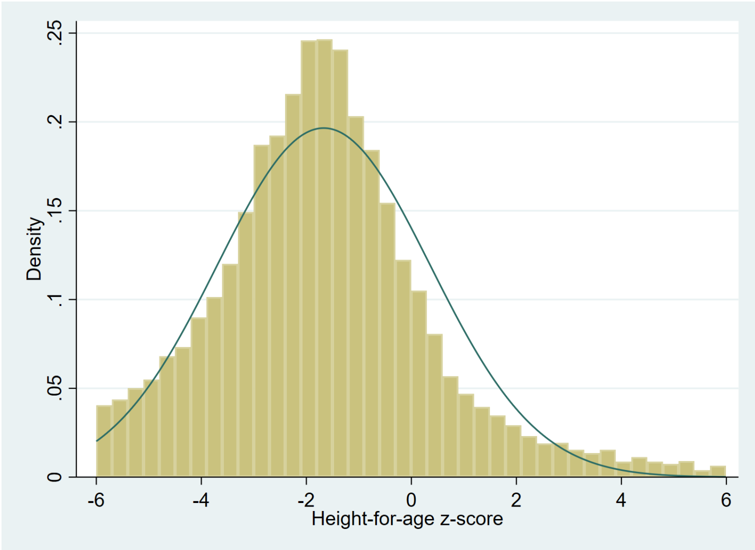


Figure 2.2: HFA z-scores in IHDS-I

Notes: HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. A child with HFA z-score $\leq$-2 is classified as stunted.

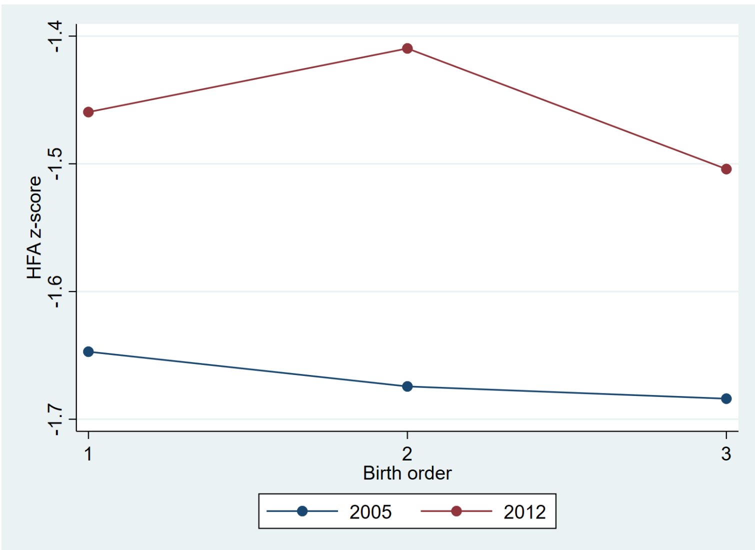


Figure 2.3: HFA z-scores and birth-order in IHDS-I & IHDS-II

Notes: HFA z-score refers to height-for-age z-score and is computed using the WHO growth standards. An HFA z-score of 0 indicates that the child's height is equal to the median of the age- and gender-specific reference population. Birth order is top-coded at 3; 2^{nd} child refers to a child with birth order 2 and 3^{rd}+ child refers to a child with birth order 3 or higher.

2.A. Appendix

2.A.1. Missing anthropometric information

Table 2.A.1: Summary statistics

	Sample of Interest		Analysis sample		Difference	
	mean	sd	mean	sd	diff	se
Child characteristics						
Birth order	2.33	(1.21)	2.34	(1.20)	-0.01	(0.02)
Age in years, Wave-I	5.07	(3.48)	4.95	(3.25)	0.12**	(0.05)
Girl	0.48	(0.50)	0.49	(0.50)	-0.01	(0.01)
Mother's age at birth	24.04	(4.59)	24.06	(4.38)	-0.01	(0.07)
Mother characteristics						
Mother's age at first birth	20.89	(3.50)	20.84	(3.35)	0.05	(0.06)
Total fertility	3.49	(1.23)	3.53	(1.22)	-0.04**	(0.02)
Mother's height	152.11	(7.29)	152.02	(7.08)	0.09	(0.12)
Mother's completed education (in years)	4.84	(4.55)	4.92	(4.57)	-0.07	(0.07)
Household characteristics						
Rural	0.68	(0.47)	0.68	(0.46)	-0.01	(0.01)
Monthly household income per capita (in 1'000 rupees)	0.58	(0.50)	0.57	(0.49)	0.01	(0.01)
Hindu	0.81	(0.39)	0.83	(0.37)	-0.02***	(0.01)
Muslim	0.12	(0.33)	0.11	(0.31)	0.02***	(0.01)
Others	0.06	(0.25)	0.06	(0.24)	0.01	(0.00)
Observations	12308		5523		17831	

Notes: The sample of interest includes children with at least one sibling, where the child and his/her sibling are observed in IHDS-I and IHDS-II. Because IHDS-I collects anthropometric information for children aged between 0 and 5 years and between 8 and 11 years in 2004-05, the sample of interest excludes children aged 6 and 7 years in 2004-05. Approximately 55 percent of the sample of interest is lost due to missing anthropometric information in either or both survey waves as well as implausible HFA z-scores. ***p< 0.01, **p< 0.05, *p<0.10.

2.A.2. Theoretical framework

Behrman's (1988) adaptation of the Behrman, Pollak and Taubman (1982) model of intra-household resource allocation illustrates the interrelations between parental preferences, child health and nutritional investments. In the model, parental preferences for equity or efficiency can influence nutrient allocation in a household to determine relative health inequalities among siblings. It should be noted that the model is a one-period model, and therefore cannot be used to predict how birth order effects change with age. However, it provides a framework to interpret some of the empirical results in this chapter, particularly with reference to the allocation of nutritional inputs across siblings based on birth order.

The model

In a household, parents are assumed to allocate nutrients among their children so as to maximize an objective function subject to constraints. The objective function is assumed to be separable between a utility function which depends on the expected health outcomes of each of the I children in the household, and a utility function which depends on other relevant outcomes. The parents' utility function with respect to their children's expected health outcomes is assumed to be a constant elasticity of substitution (CES) utility function of the form,

$$U = \left[\sum_{i=1}^{I} \alpha_i {H_i}^c \right]^{\frac{1}{c}} \qquad ; (c \leq 1) \tag{2.3}$$

The parameter α_i captures the subjective weight that parents place on child i's expected health outcome. If parents care about all I children equally, then $\alpha_i = \alpha$. If factors like birth order or gender shift parental preferences in favour of one child over another, then α_i depends on the relevant factor for the i^{th} child.

The parameter c measures parents' inequality aversion. As $c \to -\infty$, parents move towards complete inequality aversion, and are only concerned with health improvements in the worst-off child. At $c = 1$, parents are indifferent to inequality, and place equal value on health improvements across all I children. In between these two extremes, parents face a range of productivity-equity tradeoffs which influences how they allocate nutrients among their children.[25]

Child i's expected health outcome; H_i; is assumed to be determined by a Cobb-Douglas health production function of the form;

$$H_i = E_i {X_i}^{\beta} {N_i}^{\alpha} \tag{2.4}$$

E_i is child i's endowment, which depends on genetic and environmental factors including past health investments. Endowments may also capture birth order differences in health outcomes among children if such differences are driven by underlying biological mechanisms or by differences in household environments (refer section 2.3). Expected health also depends on nutritional inputs; N_i, and other non-nutritional investments in health; X_i. N_i and X_i are assumed to exhibit diminishing marginal returns.[26]

Given the total household resources for child related investments (R), parents maximise their utility with reference to the expected health of their I children subject to the following budget constraint,

$$\sum_{i=1}^{I} (P_N N_i + P_X X_i) \leq R \tag{2.5}$$

where prices for nutritional inputs; P_N, and for non-nutritional inputs; P_X are assumed to be fixed for all children. Under the assumption that there exists an interior solution to the maximisation problem, the ratio of the first-order conditions for nutrient allocation between child i and child j is given by:

$$\frac{N_i}{N_j} = \left(\frac{\alpha_i}{\alpha_j}\right)^{\frac{1}{1-(\alpha+\beta)c}} \left(\frac{E_i}{E_j}\right)^{\frac{c}{1-(\alpha+\beta)c}} \tag{2.6}$$

The relative difference in nutrient allocation between the i^{th} and the j^{th} child is a function of their relative parental preference weights and endowments, as well as the measure of their parents' inequality aversion. Ceteris paribus, parents will allocate more nutrients to child i over child j if $\alpha_i > \alpha_j$. As $c \rightarrow -\infty$, parents become more inequality averse, and the effect of relative birth order (or gender) differences on nutrient allocation declines,

[25] The condition $c \leq 1$ is needed for an interior maximum

[26] The model assumes that the health production function exhibits non-increasing returns to scale, and that endowments matter for health outcomes. Therefore, the sum of the elasticities of the health production function with respect to nutritional and non-nutritional investments in health is less than one, i.e., $(\alpha + \beta) \leq 1$.

but remains non-negative.

However, holding all else equal, the effect of relative endowment differences is increasing in c. For $E_i > E_j$ and $c \to -\infty$, child j will receive more nutrients than child i as an increase in inequality aversion causes a shift in parental behaviour towards compensatory investments. For $c = 0$, parents allocate nutrients among their children independent of endowment differentials.

Given the relative nutritional and non-nutritional investments, the expected health outcome of child i relative to child j is,

$$\frac{H_i}{H_j} = \left(\frac{\alpha_i}{\alpha_j}\right)^{\frac{\alpha+\beta}{1-(\alpha+\beta)c}} \left(\frac{E_i}{E_j}\right)^{\frac{1}{1-(\alpha+\beta)c}} \tag{2.7}$$

Ceteris paribus, the effect of endowment differences between the i^{th} and the j^{th} child on health outcomes is increasing in c. Relative health inequalities will attenuate as $c \to -\infty$ and exacerbate as $c \to 1$. For $c = 0$, relative health outcomes are proportional to endowment differentials.

If the expected health outcome under consideration is height, it is relevant to consider that a child's height in subsequent periods is a function of his/her current height. If parents are concerned with efficiency over equity, then endowment driven inequalities in height will increase. If on the other hand, parents' preferences for equity outweigh their preferences for productivity, then parents will direct nutrients towards children who are worse off to allow them to catch up, thus reducing relative height inequalities.[27] Therefore, parental allocation of investments based on c offers a potential channel for birth order effects on height to change with age.

[27]The shift in relative health inequalities in both cases is increasing in $(\alpha + \beta)$.

Chapter 3

Participation in public works and women's empowerment[1]

3.1. Introduction

There are two main arguments favouring the promotion of women's empowerment as an element in development policies. The first argument is that equity is in itself an intrinsic human right (World Bank, 2011). The second argument is that women's empowerment leads to improvements in outcomes associated with child welfare, and is therefore beneficial to society as a whole (Thomas, 1990; Quisumbing and Maluccio, 2003; Duflo, 2012). While gender disparities in education, employment, health and autonomy exist in all societies, these differences are more pronounced in developing countries like India, highlighting the need to encourage policies that promote women's welfare in these regions.

In this chapter, I explore the potential impact of a social safety net programme in India known as the National Rural Employment Guarantee Scheme (NREGS), on women's empowerment. NREGS is one of the largest public workfare programmes in the world and was implemented with the aim of providing rural households with a guaranteed entitlement to 100 days of wage employment. The official data on NREGS participation shows that in 2010-11 alone, the scheme employed nearly 53 million households, generating 2.3 billion person-days of work.[2]

A core feature of NREGS is that it includes provisions to ensure that women have

[1]This chapter builds on previous work I submitted for obtaining a Masters of Science in Economics from the University of Bristol in 2017.

[2]Source: http://nrega.nic.in/.

equitable access to work under the scheme. The provisions mandate that women should constitute at least one-third of the scheme's workers, and that equal wages should be paid to both men and women. Moreover, the scheme requires that workers are allocated to worksites close to their residence, and that creche facilities are provided at worksites where children are present.

Women's participation in NREGS has remained relatively stable since its inception. Administrative data shows that between 2007 and 2011, women consistently accounted for over 43 percent of all person-days of employment generated under the scheme.[2] Moreover, as per the 2011 Census, of the 34.9 percent of rural women active in the labour market, roughly 48 percent report working under the scheme (Narayanan and Das, 2014). NREGS therefore appears to play an important role in providing women with access to paid work in rural areas.

While often considered an indicator of empowerment in itself, women's labour force participation can also reinforce other dimensions of empowerment. Collective bargaining theory based on household behaviour suggests that women can increase their intra-household bargaining power by improving their outside option (Blundell et al., 2007).[3] In keeping with this, existing studies show that participating in paid work leads to an increase in women's agency reflected in improvements in their relative status in decision-making, control over resources and freedom of mobility (Agarwal, 1997; Rahman and Rao, 2004; Anderson and Eswaran, 2009; Kabeer et al., 2013).

The scheme's potential to empower women therefore arises as a result of its implications for their labour force participation rates in rural areas, where opportunities for women to engage in paid work are generally limited (Khera and Nayak, 2009; Mehrotra and Parida, 2017). In addition, the scheme may influence empowerment outcomes through gradual shifts in actual or perceived gender norms. For instance, men working under NREGS may update their beliefs about women's status when they see more women working under the scheme.

In this study, I examine the effect of NREGS participation on women's empowerment using a panel survey of households first carried out in 2004-05 before the implementation of NREGS, and subsequently in 2011-12 when the scheme was operational across the country. Since person-days of work under the scheme are allocated at the household

[3]In the context of this study, the outside option would refer to access to or participation in NREGS.

level, and there is no restriction on how the days are split amongst adult members in a household, this study focuses on the effect of household participation in the scheme. This is also intended to capture any direct impact of NREGS on women's empowerment due to their own participation in the scheme, as well as any indirect effects due to the participation of other household members.

A potential issue associated with identifying the effect of interest is that household participation in NREGS is likely to be endogenously determined. This is because NREGS is supposed to be a demand driven scheme, and so, participant households may differ from non-participant households on unobservable characteristics that may simultaneously influence participation in the scheme and empowerment outcomes. To address this issue, I employ an instrumental variables strategy where I use the village-level availability of an active NREGS as an instrument for household participation in the scheme. This allows me to estimate the effect of NREGS on empowerment using plausibly exogenous variation in households' participation in the scheme generated largely by village-level administrative factors which determine the presence or absence of an active scheme in a village.

In order to assess any role NREGS might play in the empowerment of women, I construct quantitative measures of empowerment. While existing literature across domains does not agree on a singular definition of empowerment, there is some concensus that central to the concept of empowerment is the ability to make choices along multiple dimensions like resources, agency and achievements (Kabeer, 1999). Taking this into account, I use data on women's self-assessed status on various aspects of gender relations to construct empowerment indices that capture women's access to resources, freedom of mobility, and relative status in the intra-household decision-making process.

This chapter is closely related to the broad literature on the effects of social safety nets such as conditional cash transfers and workfare schemes on women's empowerment (Attanasio and Lechene, 2002; Handa et al., 2009; de Brauw et al., 2014; Bonilla et al., 2017). More specifically, this chapter contributes to the existing empirical literature examining the relationship between NREGS and various aspects of women's empowerment. Amaral et al. (2015) find that the implementation of NREGS led to an increase in police-reported cases of gender-based violence, though it is unclear whether this reflects an actual increase in crimes against women or an increase in reporting rates due to improvements in empowerment. Sarma (2022) shows that NREGS is postively associated with women's welfare mainly due to its role as an effective insurance mechanism in mitigating the effect of an adverse rainfall shock on both police-reported and self-reported cases of domestic violence.

Closely related to this study is existing empirical work examining the impact of NREGS on women's decision-making power. Tagat (2020) uses survey data on intra-household decision-making collected across rural villages in five Indian states to investigate whether NREGS affects the share of women who are major decision-makers in a household. The author exploits the phased implementation of NREGS to employ a difference-in-differences strategy and finds that the implementation of the scheme was associated with an increase in a household's share of female decision-makers in terms of decisions pertaining to female labour supply, children's education and the household's consumption of nutritious food.

A related study by Desai, Vashishtha, Joshi et al. (2015) uses two waves of the India Human Development Survey (IHDS) to analyse the relationship between NREGS and indicators of women's empowerment. They exploit differences in NREGS participation intensity across villages to find that there were substantial improvements in empowerment outcomes for women in households where they themselves participated in the scheme. Using the same data and an individual fixed-effects approach, de Mattos and Dasgupta (2017) examine the effect of women's participation in the scheme on an index of women's empowerment, relative to participation in other forms of paid employment. The authors find that women who participate in NREGS are twice as likely to have control over resources and decision-making within the household relative to non-participant women.

This study explores the effect of household participation in NREGS, departing from existing literature which focuses either on the effect of implementation of the scheme or the effect of women's participation in the scheme. By examining the effect of household participation in NREGS, the estimated effect captures any direct impact of the scheme on empowerment due to women's participation in the scheme as well as any indirect impacts due to the participation of other household members. This is relevant for two reasons. First, if working under NREGS potentially leads to shifts in perceived gender norms, then the participation of men in the scheme may have an impact on women's autonomy. Second, the participation of other women living in the household could shift intra-household dynamics in favour of women more generally, and therefore, influence the surveyed woman's empowerment outcomes.

This study also builds on previous work by Pereira (2017) which uses longitudinal data from the IHDS to examine the effect of household participation in NREGS on women's empowerment. Both studies address the potential endogeneity in household participation using village-level availability of an active scheme as an instrument. This is an innovation relative to existing published literature which largely relies on an individual fixed-effects approach or a difference-in-differences framework.

Moreover, compared to Pereira (2017) and existing published literature, I study the effect of the scheme separately on empowerment indices which capture women's access to resources, freedom of mobility, and relative status in the intra-household decision-making process. This permits an examination of possible variations in the scheme's impact across the different dimensions of empowerment. Further, by systematically grouping together outcomes that reflect a common dimension of empowerment, this study offers an improvement over Pereira (2017) which uses individual responses to a few survey questions as proxies for empowerment. Lastly, relative to Pereira (2017), I extend the analysis to assess heterogeneity in the scheme's impacts based on i) which member of the household participated in the scheme and ii) the surveyed woman's labour force participation history. This allows for a better understanding of the potential factors contributing to the scheme's impact on empowerment.

The main results indicate that household participation in NREGS is associated with better empowerment outcomes. In addition, the results suggest that the improvements in empowerment are largely driven by a woman's access to resources and freedom of mobility. I do not find evidence that the scheme leads to an increase in women's autonomy in the intra-household decision-making process. Further, I find that the effects on empowerment are present irrespective of whether or not the woman herself participated in the scheme, and regardless of her prior engagement in the labour force.

The rest of this chapter is organised as follows. Section 3.2 provides a general background on NREGS. Section 3.3 reviews the existing literature on the impact of the workfare scheme. Section 3.4 describes the data used in the analysis. Section 3.5 outlines the empirical strategy and Section 3.6 presents the results. Section 3.7 concludes.

3.2. Background on NREGS

The National Rural Employment Guarantee Act (NREGA) was passed by the Parliament of India in 2005 with the aim of improving livelihood security in rural areas through the provision of unskilled manual work.[4] Under this legislation, rural households are entitled to demand up to 100 days of wage employment per year at the statutory minimum wage rate. The enactment of the legislation resulted in the implementation of the National Rural Employment Guarantee Scheme (NREGS) in 2006 and today, NREGS constitutes the largest workfare programme in the world.

NREGS was rolled out to the rural districts of the country in three phases. The roll out was based on an allocation mechanism that prioritised poor regions while ensuring inter-state fairness in programme allocation (Zimmerman, 2020).[5] NREGS was first implemented in 200 of India's poorest rural districts in February 2006, and subsequently extended to cover an additional 130 districts in April 2007. The final phase in April 2008 made the scheme available to the remaining rural districts.

NREGS is funded jointly by the central and state governments. The central government covers the entire wage costs but only 75 percent of the material costs. The state governments bear the remainder of the costs, and are also responsible for setting the wage rates. The scheme aimed to generate productive assets in rural areas to provide for long-term employment opportunities and encourage sustainable development in these areas. Projects taken up under the scheme typically include road construction, pond conservation, land development, drought proofing, and flood control.

The type of work provided under NREGS as well as its demand-driven nature is expected to encourage poor households to self-select into the programme. The guidelines of the programme specify that households willing to participating in the scheme must first file an application for a job card at the Gram Panchayat.[6] The Gram Panchayat issues only one job card per household, and is required to do so within 15 days of receipt of an application. The NREGS job card lists the adult members of a household including

[4] In 2009, the Act was renamed the Mahatma Gandhi Rural Employment Guarantee Act (MGNREGA).

[5] The allocation mechanism was designed by the Planning Commission of India. It ensured that there was at least one treatment district in every state in each phase of programme allocation, with poorer states receiving a larger number of treatment districts relative to richer states. The programme was then rolled out to the poorest districts in each state using a ranking of districts based on indicators of low economic development.

[6] A Gram Panchayat refers to a locally elected village council.

details of past employment and wage payments under the scheme. Once in possession of the job card, members of the household can apply for work by submitting a written application to the Gram Panchayat. The provisions of the act call for the payment of an unemployment allowance if work is not allotted to the household within 15 days of application.

A key feature of NREGS is that it contains a number of provisions that specifically seek to encourage the participation of women. First, NREGS mandates that one-third of its participants should be women. Second, the act also stipulates the payment of equal wages to men and women. Given that the average casual wage rate for women is lower than that for men across all Indian states, wage equity under the scheme implies an increase in earning potential for rural women (Khera and Nayak, 2009).[7] Lastly, NREGS incorporates provisions that take into account the gendered needs of women workers. These include the provision of employment within a 5-kilometre radius of the applicant's home, and the provision of childcare facilities at any NREGS worksite where there are more than five children below the age of six years.

The official data on participation at the national level shows that in 2012-13, NREGS generated more than 2.3 billion person days of employment of which 47% were accounted for by women. However, studies indicate that the implementation of the scheme varied widely across states with significant unmet demand for work resulting in large scale rationing of work under the scheme (Dutta et al., 2012; Imbert and Papp, 2015; Das, 2015). The implementation of the programme is some states is met with additional obstacles in terms of leakages and corruption, irregularities and delays in wage payments and inadequate worksite facilities (Bhatia and Dreze, 2006; Chopra, 2019).

3.3. Literature Review

There is a large body of literature documenting the potential impact of NREGS on various aspects of rural welfare. Given that NREGS is a largescale workfare programe, its impact on labour markets has been widely studied. Azam (2012) uses a difference-in-differences approach and nationally representative data from the National Sample Survey (NSS) to show that NREGS has a positive impact on labour force participation rates as well as

[7] As of 2009-10, the average wage rate under NREGS was higher than the average casual wage for women across all states (Ministry of Rural Development, 2012).

the real wages of casual workers. His results indicate that these effects are significantly stronger for women, suggesting that the scheme plays a role in not only increasing employment opportunities for women but also in reducing the prevailing gender wage gaps.

Imbert and Papp (2015) use the NSS data and a difference-in-differences approach comparing early and late phase NREGS districts to show that NREGS employment crowds out private sector work and increases casual wages in the private sector by 4.7 percent. The authors also find that the effect is concentrated in states that were known to implement the programme well. Berg et al. (2018) report broadly similar results employing a difference-in-differences framework and monthly data on agricultural wages. Their findings indicate that the scheme increased the real agricultural wage rate by 4.3 percent per year and that the effect was concentrated in the main agricultural season. Unlike Azam (2012), Berg et al. (2018) find no evidence of a gender differential in the impact of the scheme on wages.

Using the NSS data and a regression discontinuity approach, Zimmerman (2020) reports substantially different results. Her findings indicate that the scheme does not result in a significant increase in either public sector employment or real wages in the private sector. She also finds no evidence of a difference in impact based on gender. The author, however, does find some evidence of an increase in NREGS take-up after a negative rainfall shock, suggesting that the programme acts as an effective safety net.

A section of literature on NREGS examines the impact of the scheme on child wellbeing through its effect on women's labour force participation. Afridi et al. (2016) use a child and household level panel from the Young Lives study in Andhra Pradesh to analyse the impact of mothers' participation in the labour force on their children's educational outcomes. The authors instrument mothers' labour force participation using lagged NREGS funds sanctioned at the mandal level, and show that an increase in mothers' labour-force participation on account of NREGS leads to better child outcomes in terms of grade progression and time spent at school.[8] They also establish that this change is driven mainly by working mothers having greater decision-making authority in the household, as opposed to income and substitution effects.

Das and Singh (2013) use a difference-in-differences strategy and data from two phases of the District Level Household and Facility Survey (DLHS) to study the impact of

[8] A mandal refers to a local government area below the district level.

NREGS on children's educational outcomes. In contrast to Afridi et al. (2016), the authors find no evidence that NREGS leads to an improvement in educational outcomes as measured by completed years of schooling. However, they find suggestive evidence of a decline in educational outcomes for older girls, which they attribute to a reduction in mother's time spent at home due to increased participation in the scheme.

Shah and Steinberg (2021) find that an increase in labour demand due to NREGS raises the opportunity cost of schooling for children, leading to a decline in human capital investment. They use a household survey on school enrolment and test scores, and employ a difference-in-differences approach to show that NREGS reduces enrolment and math test scores among children aged 13 to 16. Similar to Das and Singh (2013), the authors find that adolescent girls have worse outcomes as they substitute school for unpaid domestic work, and conclude that this is likely caused by an increase in mothers' labour force participation due to NREGS.

The existing studies on the scheme's impact on child health through its influence on mother's work force participation also produces mixed results. Dev (2011) uses existing studies on NREGS conducted in various Indian states, and a small focus group in Rajasthan to understand the mechanisms through which NREGS has an impact on household and child outcomes. His findings suggest that an increase in employment opportunities under the scheme has a positive impact on women's nutrition and empowerment outcomes, and could lead to improved infant feeding and increased health expenditure on children. However, Chari et al. (2019) use a triple differences framework and nationally representative data from the DLHS to find that NREGS is associated with worse infant health. The authors find that NREGS led to an increase in neonatal mortality rates among women who were eligible to participate in the scheme.

The existing literature on NREGS includes qualitative studies that document the impact of the scheme on the lives of rural women. Using a four-state survey, Jandu (2008) measures the benefits of NREGS to women over a two-year period following its implementation. The author finds that despite an insufficient supply of work and delays in wage payments under the programme, NREGS has contributed to the empowerment of women. Some of the perceived benefits include the reduced need for migration, lower dependence on money lenders and increased health expenditure. Her findings further suggest that women experience greater confidence in decision-making and enjoy more economic independence as a result of the scheme.

Narayanan (2008) finds similar results using a survey covering 15 NREGS worksites in the Viluppuram district in Tamil Nadu. She concludes that though NREGS has a positive impact on the lives of women in rural areas, the lack of proper child-care facilities prevents the scheme from reaching its full empowerment potential. Similarly, Khera and Nayak (2009) survey 1060 NREGS workers in six North Indian states, and find that the lack of creches at worksites and presence of illegal contractors hinder women's participation in the scheme. Their study however shows that the women who do participate in NREGS benefit in terms of improved food security, better ability to cope with illness, reduced need for migration and protection from exploitative work.

Pankaj and Tankha's (2010) findings from a field survey conducted across four states in North India show that women who collect their NREGS wages themselves experience greater choice over their consumption basket and an increased involvement in the household decision-making process. The authors find no evidence of change in women's participation in community development processes, indicating that the scheme's benefits on empowerment may be limited to the household level. In contrast, Pellissery and Jalan (2011) find no immediate impact of the scheme on the social transformation of women in their case study of a village in the Guntur district in Andhra Pradesh. The authors suggest that while NREGS has the potential to contribute to women's empowerment through improved gender relations, the benefits were limited due to shortcomings associated with programme implementation.

Similar to Pellissery and Jalan (2011), Chopra (2019) notes that the scheme's potential for women's empowerment remains largely unrealised due to the inadequate implementation of its provisions that sought to benefit women. Some of these provisions include the availability of childcare facilities at worksites and the participation of women in the formal planning process of work provision under the scheme. The qualitative literature on the empowerment effects of NREGS reinforces the view that the scheme's immediate potential to empower women largely lies in its ability to reduce their economic dependence on family members (Drèze and Oldiges, 2007).

Complementing the above discussed qualitative literature, is a growing body of quantitative work using secondary data to examine the scheme's impact on various dimensions of women's empowerment. Amaral et al. (2015) use data from the National Crime Record

Bureau (NCRB) to investigate the relationship between NREGS and police-reported cases of crimes against women. The authors employ a difference-in-differences strategy to show that increased access to employment following the implementation of NREGS led to an increase in gender-based violence with the exception of dowry deaths. However, they are unable to ascertain if the higher incidence of gender-based violence reflected an increase in actual crimes or an increase in reporting rates due improvements in empowerment.

Similarly, Sarma (2022) examines the association between NREGS and domestic violence, but focuses on whether NREGS mediates the effect of a negative rainfall shock on domestic violence. The author employs a difference-in-differences approach to analyse the effect of interest at the district and household level, using officially reported cases of crimes against women and self-reported measures of domestic violence respectively. She shows that the implementation of NREGS mitigates the effect of a negative rainfall shock on domestic violence both at the district and household level. Her findings indicate that the mediating effect of NREGS mainly operates through a reduction in economic stress rather than improvements in empowerment.

Tagat (2020) uses household survey data from five Indian states to examine the effect of NREGS on women's intra-household decision-making ability. The author uses a difference-in-differences strategy to show that NREGS implementation is associated with an increase in a household's share of female decision-makers taking decisions pertaining to expenditure on nutritious food, children's education and female labour supply.

Desai, Vashishtha, Joshi et al. (2015) analyse the effect of NREGS on women's empowerment using two waves of the India Human Development Survey (IHDS). They exploit differences in NREGS participation intensity at the village level to show that the scheme had a positive impact on women's empowerment indicators particularly for households where women participated in the scheme. The indicators include the ability to visit a health centre alone, control over spending decisions and say in household-decision making. The authors suggest that the observed effect is likely because NREGS provided most rural women with their first opportunity to participate paid work.

de Mattos and Dasgupta (2017) also use panel data from the IHDS and an individual fixed-effects approach to estimate the impact of women's NREGS participation on an empowerment index, relative to participation in other types of paid employment. The authors find that women who participate in NREGS are twice as likely to have control over resources and decision-making within the household relative to non-participants. However, they find that the scheme is limited in its ability to lead to transformative gender equality as captured by the time the household's older daughter spends in school. This study examines the effect of household participation in the scheme on women's empowerment outcomes, departing from existing literature which focuses on the effect of scheme implementation or the effect of women's participation in the scheme.

This study also builds on previous work by Pereira (2017) which uses panel data from the IHDS to examine the effect of household participation in NREGS on women's empowerment. Both studies address potential endogeneity in household participation using village-level availability of an active scheme as an instrument. This is an innovation relative to existing published literature which largely relies on a difference-in-differences framework or an individual fixed-effects approach. Moreover, compared to Pereira (2017) and existing published literature, I study the effect of the scheme separately on three distinct dimensions of empowerment which capture women's access to resources, freedom of mobility, and relative status in the intra-household decision-making process.

3.4. Data

The analysis in this chapter uses data from two waves of the India Human Development Survey (IHDS) series which covers over 40,000 households across all Indian states and union territories.[9] The IHDS is a multi-topic survey providing data on the employment, health, education, and socioeconomic status of individuals and households in both rural and urban areas. In each household, the survey also administered a separate module to at least one ever-married woman aged 15 to 49 years to collect data on marriage, fertility history, and gender relations. These women are referred to as "eligible women" in the survey documentation.

The first wave of the IHDS (IHDS-I) interviewed 215,754 individuals from 41,554 households during 2004-05, prior to the implementation of NREGS (Desai et al., 2008). The second survey wave (IHDS-II) re-interviewed 83 percent of the original households during 2011-12, by which time the scheme was operational in all rural districts of the country (Desai, Vanneman et al., 2015). The recontact rate for the second survey wave

[9] The IHDS sample excludes Andaman and Nicobar islands, and Lakshadweep.

was higher at approximately 90 percent in rural areas. Randomly selected households were used as replacements in areas where attrition was particularly high. The final sample for IHDS-II included 204,569 individuals from 42,152 households.

While the two survey waves are similar, they are not identical. The second wave interviewed more than one eligible-woman per household where possible, and collected additional information on various aspects of households and individuals. Notwithstanding the discrepancies in the two waves of data collection, the IHDS series offers a panel dataset that benefits from a high re-contact rate and in-depth information on multiple topics, facilitating an examination of the impact of NREGS on outcomes of interest.

In this analysis, the main explanatory variable is a binary indicator for household participation in NREGS. The IHDS-II collects information on NREGS participation in the twelve months preceding the survey for each household. Using this data, I construct a dummy variable which assumes the value 1 for a household if any member in the household; male or female, participated in the scheme in the twelve months preceding the interview date; and 0 otherwise.[10]

For the outcome of interest, I construct a composite index of women's empowerment using data from the IHDS module on eligible women. The questionnaire administered under this module collected information on an ever-married woman's self-assessed status on various aspects of gender relations and decision-making within the household. The composite index is comprised of sub-indices which measure empowerment along three distinct dimensions commonly used in empirical empowerment literature, namely, economic, sociocultural and interpersonal dimensions.

To construct a sub-index for each dimension, I group together the eligible woman's responses to questions that reflect her degree of autonomy with reference to the dimension under consideration. The *economic sub-index* which measures a woman's access to or control over resources includes responses to the following questions, asked in both rounds:

- Do you yourself have cash in hand to spend on household expenditures?
- Is your name on any bank account?

The *sociocultural sub-index* which reflects a woman's degree of mobility is constructed

[10] A household is considered to have participated in NREGS if any member of the household participated in the scheme for at least a day in the 12 months preceding the interview day. The average number of days a household in the sample worked in the scheme is approximately 46 days.

using responses to the following questions:[11]

- Do you have to ask permission of your husband or a senior family member to go to the local health centre?
- Do you practice the ghungat/purdah/pallu?[12]

Lastly, the *interpersonal sub-index* measures a woman's relative bargaining power in the decision-making process within the household. This sub-index considers whether the eligible woman is the major decision-maker across decisions indicated in the following questions:

- Do you have the most say in what to do if your child falls sick?
- Do you have the most say in how many children you have?
- Do you have the most say in whether to buy an expensive item such as TV or fridge?

The questions listed above are dichotomous in nature asking for yes/no responses. I assign a value of 1 to a response if it is indicative of a positive empowerment outcome; and 0 otherwise. For example, I assign a value of 1 if a woman does not practice the purdah or if she has the most say in deciding how many children to have. Next, each individual sub-index is obtained by computing a simple average of the assigned values across its component questions. Finally, I take the average of the economic, sociocultural and interpersonal sub-indices to obtain a composite or overall index of women's empowerment.[13]

To maintain objectivity, I construct the sub-indices and the overall index of empowerment by assigning equal weights to each of their component indicators. The resulting women's empowerment sub-indices and overall index range between 0 and 1, with 0 indicating no empowerment and positive values indicating increasing empowerment. Since this study is concerned with changes in and not levels of empowerment, any positive change in the empowerment index due to programme participation would be considered favourable.

[11] A 'yes' response to these questions counts negatively towards the sociocultural sub-index of empowerment.

[12] Purdah, ghungat, or pallu refers to the veiling of women, specifically in the company of men. The mobility of women practising different forms of veiling is often restricted.

[13] The indices are constructed for one eligible woman per household observed in both waves of the survey.

For the analysis, I restrict the sample to only those households and eligible women interviewed in both waves of the survey. This is done to minimise any bias that may arise due to attrition which could be non-random. Next, I drop all urban households from the sample as NREGS specifically targeted rural households. Lastly, I restrict the sample to eligible women with non-missing responses to the questions used in the construction of the empowerment indices.[14] The final sample includes 14,397 eligible women and rural households observed in both rounds of the survey.

Table 3.1 presents summary statistics for the outcomes, household characteristics and eligible women characteristics of participant and non-participant households in 2004-05, prior to the implementation of NREGS. Non-participant households appear to exhibit higher levels of economic and interpersonal empowerment than participant households. However, there seems to be no difference in the sub-index of sociocultural empowerment based on NREGS participation.

Households that participate in NREGS are more likely to rank lower on the asset index than non-participating households in 2004-05, and are also more likely to have outstanding debt. Participating households appear to have lower levels of male education and fewer working age members relative to non-participating households. Moreover, there is a difference in caste composition between the two groups; household that participate in NREGS are more likely to belong to lower caste groups like Dalits and Adivasis than non-participating households. Lastly, eligible women in non-participant households are approximately the same age on average as eligible women in participating households, but appear to have more years of education and lower fertility.

3.5. Empirical Strategy

This study is interested in identifying the causal effect of household participation in NREGS on women's empowerment. A simple comparison of outcomes of interest between participant and non-participant households may yield biased estimates as household participation in the scheme in any given year is likely to be endogenously determined. This is because the demand-driven nature of work provision under NREGS results in partici-

[14]Approximately 15 percent of the observations have missing responses. Logit regressions of missing responses on eligible woman and household characteristics show that the probability of a missing response is positively associated with the eligible woman's education and negatively associated with the household's asset index.

pating households self-selecting into the workfare scheme. Households that participate in NREGS are therefore likely to differ from non-participant households on observable and unobservable characteristics that may simultaneously influence programme participation and the outcomes of interest.

To address any bias which may arise due to the potentially endogenous nature of NREGS participation, I exploit the panel structure of the dataset, estimating the following difference-in-differences specification with household fixed-effects:

$$Y_{it} = \alpha + \beta Post_t + \delta(Post \times Participation)_{it} + \gamma X_{it} + \nu_i + \epsilon_{it} \tag{3.1}$$

Here, Y_{it} is the outcome of interest for the eligible woman interviewed in household i at time t. $Post_t$ is a binary indicator which assumes the value 1 for observations in the post NREGS survey wave (2011-12); and 0 otherwise. $Participation_i$ is a binary indicator for household level NREGS participation; it equals 1 if any member of household i participated in the scheme in the twelve months preceding the survey date; and 0 otherwise.[15]

X_{it} is a vector of household and eligible woman controls that can likely influence a woman's empowerment outcomes. This includes the total number of working age members in the household, the eligible woman's age, and a quadratic in the eligible woman's age to account for any possible non-linear relationship between age and empowerment outcomes.[16,17] ν_i captures household fixed-effects, and ϵ_{it} is a random error term.

The coefficient of the interaction term, δ, provides the causal estimate of interest. It measures the change in empowerment outcomes for eligible women in participant households relative to eligible women in non-participant households, holding constant time-invariant unobserved household heterogeneity.[18] The validity of the causal interpretation of this estimate rests on the assumption that household fixed-effects sufficiently account for the potential endogeneity of household participation in NREGS. This would imply

[15] The un-interacted "$Participation_i$" term drops out from the specification due to the inclusion of household fixed-effects.

[16] The eligible woman's age might not correspond perfectly with the period between the two waves on account of survey duration.

[17] Results reported estimating specification (3.1) without household fixed-effects include the following additional controls: dummies for caste groups (Other Backward Castes, Dalits, Adivasi/Tribal caste and Muslims), and the eligible woman's education in completed years.

[18] As the analysis includes only one eligible woman per household, and eligible women do not move across households between survey rounds, household fixed-effects should control for any time-invariant sources of heterogeneity at the eligible woman level.

that any unobserved household characteristics that simultaneously determine a household's participation in the scheme and the eligible woman's empowerment levels within the household are assumed to be time-invariant or captured by the controls.

It is possible, however, given the time period under consideration, that some unobserved household characteristics influencing a household's decision to participate in the scheme as well as the eligible woman's empowerment levels, may vary over time. Some of these characteristics might include awareness of rights, income shocks, prior experience with the scheme, household beliefs and social norms, and attitudes towards unskilled work. For example, if an increase in the awareness of one's rights over time leads to improved empowerment outcomes but a decline in scheme participation due to better alternative options, then the fixed-effects specification is likely to underestimate the association between NREGS participation and women's empowerment. Failing to control for these potential sources of time-varying unobserved heterogeneity could lead to bias in the estimate derived from the fixed-effects specification (3.1).

To mitigate these concerns, I use an instrumental variables (IV) strategy that allows me to identify the causal effect of interest exploiting the plausibly exogenous variation in a household's NREGS participation, arising from whether the household resided in a village with an active scheme. Using data on household participation in NREGS, I define the scheme to be active in a village if at least one household in the village participated in the scheme.[19] The proposed instrument is, therefore, a binary indicator which equals 1 for households residing in a village with an active scheme; and 0 otherwise.[20] I will argue that the presence or absence of an active NREGS in a village was largely associated with village-level administrative factors, and is therefore potentially exogenous to omitted sources of unobserved household heterogeneity.

By the second round of the survey, NREGS was accessible to all rural districts in the country and it was reported that on average, at least one in four rural households was participating in the scheme (Joshi et al., 2015).[21] Figure 3.1 displays the distribution of village level NREGS participation rates for the main sample. The figure shows that for approximately 30 percent of the villages in the main sample, no surveyed household in the

[19] To account for any bias arising due to a household's own participation status being potentially endogenous, the instrument for a household equals 1 only if at least one *other* household in the village participated in the scheme.

[20] Given that I use a village-level instrument, standard errors in the estimation will be clustered at the village level.

[21] Official government reports show higher participation rates during this period.

village had participated in the scheme at the time of the second survey wave. Moreover, as seen in Figure 3.2, villages with no observed NREGS participants in the main sample are not confined to poorer states like Bihar, Jharkhand, Orissa and Uttar Pradesh. The lack of NREGS participation in a village could reflect either low demand for work under the scheme or the absence of an active scheme in the village. However, given that over 70 percent of rural households in the IHDS sample claim the lack of enough work to be the main reason for not participating in the scheme, it would be reasonable to assume that there likely was some demand for work in all villages (Desai, Vashishtha, Joshi et al., 2015).

Existing empirical work shows that the demand for work under NREGS far exceeds its supply leading to rationing of work under the scheme.[22] Ravi and Engler (2015) in a study of the Medak district in Andhra Pradesh find that the rationing of work under NREGS stemmed from the inconsistent implementation of the scheme across villages. The authors also note that the incidence of rationing was higher in the early years of the programme mainly due to the lack of sufficient worksites. Using data from the National Sample Survey (NSS) for 2009-10 and 2011-12, Dutta et al. (2012) and Das (2015) find evidence that NREGS work was rationed across all states, suggesting that there was significant unmet demand for work under the scheme. Imbert and Papp (2015) note that disparities in the availability of work under the scheme are more likely to be a function of "supply factors" like political will and administrative capacity than "demand factors" like poverty levels.

Although all rural households in a village are entitled to participate in NREGS, the actual provision of work under the scheme calls for considerable administrative capacity, largely depending on the efficiency of local village officials in the Gram Panchayat. So, while households willing to participate in the scheme need to register their interest at the public meeting of the Gram Panchayat, it is important to note that the Gram Panchayat itself bears the primary responsibility of organising work under NREGS. In addition to first identifying the list of works in the village, the Gram Panchayat is responsible for implementing at least 50 percent of the NREGS projects. Limited administrative capacity at the village level could therfore impede the local implementation of the scheme, having a substantial impact on the ability of rural households to access work under the scheme.

[22]Rationing of work under NREGS can refer to receiving fewer days of work than demanded, or not receiving any work when demanded. The empirical work discussed here is concerned with the latter.

The two-stage least squares specification using the IV is estimated as follows:

- First stage:

$$(Post \times Participation)_{it} = \alpha + \beta Post_t + \delta(Post \times Instrument)_{it} + \gamma X_{it} + \nu_i + \omega_{it} \tag{3.2}$$

- Second stage:

$$Y_{it} = \alpha + \beta Post_t + \delta(Post \times \widehat{Participation})_{it} + \gamma X_{it} + \nu_i + \epsilon_{it} \tag{3.3}$$

For an instrument to be valid, it must satisfy two main conditions. The first is that the instrument must be correlated with the endogenous regressor of interest. This is known as the relevance assumption. In this analysis, the instrument captures whether or not NREGS is active in a village, which is likely to have a direct impact on the probability that a household participates in the scheme.

The idea behind the instrument is similar to an 'encouragement design'. In principle, households in all rural villages are eligible to participate in the scheme. However, in villages where NREGS is active, households receive an 'invitation' to participate in the scheme. The first stage regression is reported in Table 3.2, and shows that the instrument is strongly correlated with household participation in the scheme. In other words, households are more likely to participate in NREGS in villages with access to an active scheme. Further, the F-statistic is large, suggesting that the estimation is unlikely to suffer from bias due to weak identification.[23]

The second condition for a valid instrument is that it must be uncorrelated with the residual in the structural equation (3.3). This is known as the exclusion restriction and requires that the instrument must influence women's empowerment only through household participation in the scheme. More specifically, the presence or absence of the scheme in a village should not be systematically linked to any unobserved factors in the residual that relate independently to changes in empowerment. As explained earlier, "supply factors" such as the administrative capacity of the Gram Panchayat are more likely to determine the availability of an active scheme in a village, rather than "demand factors" such as poverty levels and/or characteristics of households willing to participate

[23]Given the construction of the IV, it is almost mechanically the case that the F-statistic will be large.

in the scheme (Imbert and Papp, 2015). Therefore, it is unlikely that the instrument is linked to unobservable factors that determine changes in a woman's intra-household empowerment levels.

Table 3.3 reports mean values of household and eligible woman characteristics in the pre-reform period for households based on whether or not they resided in a village with an active scheme. Household in villages with access to an active scheme do not appear to be significantly different from households in villages without an active scheme in terms of being members of village organisations like the Mahila Mandal or caste associations.[24] There also appears to be no significant difference in a household's past voting behaviour or access to government services as reflected in their ownership of a ration card. This shows that the presence of an active scheme in a village is not systematically associated with behaviours that are likely to be linked with increases in empowerment such as a household's membership in women centric organisations or a household's engagement in the voting process.

In addition, Table 3.3 shows that households in villages without an active scheme are more likely to be members of village level organisations like co-operative societies, savings groups, trade unions and religious groups, than households in villages with an active scheme. Moreover, households in villages without an active scheme appear to be associated with better initial empowerment levels, a higher asset index, lower debt levels and higher education levels. This would suggest that villages are positively selected into not having access to an active scheme rather than into having access to it. This is the opposite of what might be expected if household characteristics that could lead to independent changes in empowerment were also more likely to be associated with access to an active scheme, mitigating concerns that the instrument violates the exclusion restriction. Further, the inclusion of household fixed-effects accounts for any time-invariant factors that influence changes in empowerment other than through household participation in NREGS. Given this, it seems reasonable to conclude that condition on controls, and household fixed-effects, the instrument complies with the exclusion restriction.

The impact estimates for this study are obtained under the additional assumption that treatment is a monotonic function of the instrument. In other words, the probability of households participating in the scheme must be positively correlated with the scheme

[24]Mahila Mandals are informal social service clubs concerned with addressing grievances and promoting the welfare of women and girls in rural areas.

being active in a village. In this case, the construction of the instrumental variable necessarily ensures that the monotonicity assumption is satisfied. Given this, the IV estimates provide the Local Average Treatment Effect (LATE) which measures the impact of household participation in NREGS on compliers.

3.6. Results

Table 3.4 presents estimates for the effect of household participation in NREGS on the index of overall empowerment. In columns (1) to (3), I report regression results from the OLS specification, progressively adding controls with each column. The coefficient of interest is statistically significant across the columns, and shows that participation in the scheme is associated with an improvement in overall empowerment. The full specification with controls and household fixed-effects in column (3) indicates that household participation in NREGS increases the eligible woman's index of overall empowerment by 0.04 units, holding constant time-invariant unobserved household heterogeneity.

Results from the IV specification are reported in column (4) of Table 3.4. The IV estimate of the causal effect of interest is statistically significant and identical in direction to the OLS estimate. It is however, larger in magnitude, which typically indicates that the effect of scheme participation on compliers is larger than that on the rest of the population. The estimate suggests that for households that choose to participate in NREGS in response to the scheme being active in the village, the eligible woman's index of overall empowerment increases by 0.09 units corresponding to approximately 47 percent of a standard deviation at baseline.

Next, I explore the effect of NREGS participation separately on each of the three dimensions used in the construction of the overall empowerment index. Tables 3.5, 3.6 and 3.7 present regression results using the economic sub-index, the sociocultural sub-index, and the interpersonal sub-index, respectively, as the outcome of interest. Following Table 3.4, OLS results are reported in columns (1) to (3), and IV results are reported in column (4). Table 3.5 shows that there is a significant positive impact of NREGS participation on an eligible woman's access to or control over resources. As before, the IV estimate in column (4) is larger in magnitude relative to the OLS coefficient and indicates that household participation in the scheme is associated with a 0.19 unit increase in the sub-index of economic empowerment. The effect corresponds to approximately 70 percent of a standard deviation at baseline.

Similarly, in Table 3.6, NREGS participation appears to improve an eligible woman's freedom of mobility. While the household fixed-effects estimate is insignificant, the IV estimate is positive and significant. The coefficient from the IV specification indicates that household participation in the scheme increases an eligible woman's sub-index of sociocultural empowerment by 0.08 units corresponding to 24 percent of a standard deviation at baseline. In contrast, the OLS and IV estimates in Table 3.7 indicate no significant association between NREGS participation and changes in an eligible woman's relative status in the intra-household decision-making process as captured by the sub-index of interpersonal empowerment.

The findings in this section suggest that while household participation in NREGS did lead to positive changes in an eligible woman's index of overall empowerment, the impact was not uniform across the three constituent dimensions. The increase in overall empowerment appears to be driven mainly by changes in the economic and sociocultural sub-indices. Given that women accounted in part for a household's participation in the scheme and that NREGS wages were paid in cash or through bank accounts, the positive impact of the scheme on the economic sub-index is perhaps not surprising. The result aligns with findings from previous empirical work which suggest that a large proportion of women retain a part of their earnings from NREGS, and in many cases are able to exercise control over how these wages are spent (Khera and Nayak, 2009; Pankaj and Tankha, 2010).

The results also suggest that NREGS participation improves an eligible woman's freedom of mobility. This potentially reflects an increase in agency likely due to NREGS offering women an opportunity to participate in paid work outside their homes, and therefore 'visibly' contribute to economic activity (Khera and Nayak, 2009). However, the potential increase in agency does not appear to translate into improvements in an eligible woman's autonomy in the intra-household decision-making process. A potential reason for the lack of change in the sub-index of interpersonal empowerment is that norms governing changes in gender relations within a household are often rigid and therefore, slow to change.

The main findings are largely in line with existing empirical studies which suggest that NREGS leads to improvements in overall empowerment, particularly through increasing a woman's access to monetary resources (Khera and Nayak, 2009; Pankaj and Tankha, 2010; Desai, Vashishtha, Joshi et al., 2015; de Mattos and Dasgupta, 2017). In terms of magnitude, the effects found in this study are to some degree comparable to de Mattos and Dasgupta (2017) who find that women who participated in NREGS are twice as likely to have better empowerment outcomes than non-participant women. Desai, Vashishtha, Joshi et al. (2015) also report substantial improvements in empowerment indicators concerning financial inclusion and intra-household decision-making for households where women participated in the scheme.

Further, the main results find no evidence of an association between household participation in NREGS and an eligible woman's autonomy in the intra-household decision-making process. This finding is in contrast to a closely related study by Tagat (2020) which shows that NREGS results in an increase in the number of women who are major decision-makers in a household. However, Tagat (2020) explores the effect of the scheme across a few states at a later time-period, by which time long-term exposure to the scheme may have allowed for a more substantial shift in gender norms. It should be noted that the results in this section reflect changes in the index and therefore, a positive change does not necessarily imply that a woman is fully "empowered". It simply indicates that eligible women in households that participated in NREGS experience larger increases in their empowerment indices and in doing so, move in the direction towards becoming empowered.

3.6.1. Further Analysis

In the previous section, the analysis focused on NREGS participation at the household level. This intended to capture the direct impact of the scheme on eligible women's empowerment due to their own participation, as well as any indirect impact due to the participation of other household members. Moreover, the actual participation of eligible women in the scheme is arguably not required to improve empowerment as long as the woman's outside option changes, or is believed to have changed. However, to check if the main results are driven primarily by eligible women's participation in NREGS, I will re-estimate the IV specification separately for subsamples depending on whether or not the eligible woman in the household participated in the scheme.

Table 3.8 reports IV estimates for the effect of interest for the sample excluding participant households where the eligible woman participated in the scheme, and Table 3.9 reports IV estimates for the sample excluding participant households where household members other than the eligible woman participated in the scheme. The estimates indicate that the index of overall empowerment increases by 0.13 units if the eligible woman participated in the scheme, and by 0.09 units if other household members participated in the scheme. This implies that household participation in NREGS led to an improvement in overall empowerment irrespective of whether or not the eligible woman participated in the scheme.

The results in Tables 3.8 and 3.9 further suggest that the increase in the overall empowerment index in both cases can be attributed to improvements in an eligible woman's access to resources and freedom of mobility as reflected in the estimates for the economic index and sociocultural index, respectively. However, the estimates for the sociocultural index are only weakly significant. Moreover, the relative magnitude of the effect of NREGS on empowerment appears to be greater when eligible women themselves participated in the scheme.

The above subsample analysis suggests that while eligible women experience improvements in empowerment so long as their reservation wage is altered, they appear to benefit more if they themselves participate in the scheme. In other words, the eligible woman's participation in NREGS might play an important role in bringing about changes in empowerment levels, as opposed to the participation of any man, or woman, in the household.

A potential concern with the analysis in this study is that it fails to account for an eligible woman's labour force participation history. These variables have not been considered in the analysis for reasons of potential endogeneity. The IHDS provides data on whether an eligible woman participated in paid work during the first survey wave, prior to the implementation of NREGS. Here, paid work refers to salaried work, as well as agricultural and non-agricultural wage work. Using this data, I re-estimate the IV specification separately for subsamples of women based on whether or not they worked for pay in the first survey wave.

Tables 3.10 and 3.11 present IV estimates for the effect of interest for women who engaged in paid work prior to the implementation of NREGS and for those who did not, respectively. The estimates suggest that household participation in NREGS is associated with an increase in overall empowerment for both categories of women, but the effect appears to be larger for women who did not engage in paid work in the first survey wave. The same pattern holds for the effect of NREGS on the dimension capturing a woman's access to resources. The estimates indicate that the scheme increases the index of economic empowerment by 0.17 units for women who participated in the labour force in the first survey wave, and by 0.20 units for those who did not.

The results in Table 3.10 also show that for women who had previously participated in the labour force, the increase in overall empowerment seems to be mainly driven by an improvement in economic empowerment. While the coefficient on sociocultural empowerment for these women is positive, it is not statistically significant. On the other hand, women who did not engage in paid work in the first survey wave experience improvements in both economic and sociocultural empowerment, though the coefficient for the effect of the scheme on the sociocultural index is only weakly significant ($\rho = 0.0$). Similar to the main results, the effect of the scheme on the interpersonal index is not statistically significant for either group of women.

Desai, Vashishtha, Joshi et al. (2015) indicate that NREGS likely offered the first opportunity for many rural women to engage in paid work and earn an income. This offers a potential explanation for the relatively larger impact of the scheme on the empowerment of women who had not engaged in paid work in the first survey wave. It is also interesting to note that NREGS benefits women who had participated in the labourforce prior to the implementation of the scheme. This could indicate that in some way, NREGS might be different from other types of paid work. Sarkar et al. (2019) find that the implementation of NREGS significantly reduced the exit probability of women from the labour market. This may suggest that access to NREGS enables rural women to retain their labour force participation status and thus, lead to sustained improvements in their intra-household autonomy.

Lastly, if the effect on empowerment is linked to the outside option rather than actual participation, then there may be spillover effects for non-participant households in villages with an active scheme. To check for such effects, I run the following specification on the subsample of non-participant households:

$$Y_{it} = \alpha + \beta Post_t + \delta(Post \times Instrument)_{it} + \gamma X_{it} + \nu_i + \epsilon_{it} \tag{3.4}$$

Here, Y_{it} is the empowerment index of interest for the eligible woman in non-participant household i at time t. ν_i captures household fixed-effects. $Post_t$ is a binary indicator of time that equals 1 for observations in the post-NREGS survey wave (2011-12); and 0 otherwise. $Instrument_i$ is a binary indicator capturing non-participant household i's access to NREGS. It equals 1 if household i resides in a village with an active scheme; and 0 otherwise. X_{it} is a vector of household and eligible woman characteristics. ϵ_{it} is a random error term clustered at village level.

The results, reported in Table 3.12, suggest that for eligible women residing in non-participant households in villages with a active scheme, the index of overall empowerment increases by 0.02 units. Here, the improvement in overall empowerment appears to be driven largely by improvements along the economic dimension of empowerment. There also appears to be a positive impact on a woman's freedom of mobility reflected in an increase in the sociocultural sub-index, but the effect is only weakly significant.

The above results potentially point to the presence of spillover effects. This could be the result of possible demonstration effects or perhaps a gradual shift in beliefs or norms in villages where households participate in the scheme. However, the decision to not participate in the scheme is potentially endogenous, and as such these results should be interpreted with caution.

3.7. Conclusion

In this chapter, I examine the effect of household participation in India's employment guarantee scheme (NREGS) on women's empowerment using a longitudinal survey of households covering all Indian states and union territories. To quantify the effects on empowerment, I construct an index which measures a woman's autonomy along the economic, sociocultural and interpersonal dimensions of empowerment.

In order to identify the effect of interest, I employ an instrumental variables strategy where the availability of an active NREGS in a village is used as an instrument for the potentially endogenous relevant explanatory variable; household participation in the scheme. The main results indicate that household participation in NREGS is associated with an improvement in overall empowerment, and that the effect is mainly driven through an increase in the sub-indices reflecting an eligible woman's access to resources and freedom of mobility. I find no evidence of a significant impact on an eligible woman's autonomy in the intra-household decision-making process. Further, I find that the scheme is associated with improvements in empowerment irrespective of whether or not the eligible woman herself participated in the scheme. The scheme also appears to lead to better empowerment outcomes for all eligible women regardless of their labour force participation history.

The main findings are largely consistent with existing literature which shows that NREGS leads to better empowerment outcomes for women. In addition, this study highlights the need to more carefully assess the scheme's impact separately on multiple dimensions of empowerment as the findings show that the effect of the scheme may vary across the different dimensions of empowerment. Moreover, given that norms governing gender relations are often slow to change, an evaluation of the longer-term impacts of the scheme on a more comprehensive range of empowerment indicators would be better able to assess if NREGS has the potential to bring about substantial shifts in gender norms.

These results have important implications for policy in India particularly given the recent debate surrounding the replacement of NREGS with an alternative form of social insurance. In addition to its intended benefits as a social safety net in rural areas, existing literature suggests that NREGS is associated with many unintended beneficial consequences for the welfare of women and children. The findings in this study confirm one such benefit related to the scheme's potential to empower women. It should be noted that the scheme appears to have had positive impacts despite reports suggesting that the gender provisions of the scheme are often inadequately implemented (Khera and Nayak, 2009; Chopra, 2019). Policy directed towards improvements in implementation may therefore be able to enhance the scheme's potential to shift gender norms.

3.8. Tables

Table 3.1: Summary statistics

	Non-participants		Participants		Difference	
	mean	sd	mean	sd	diff	se
Outcomes						
Overall index of empowerment	0.32	(0.19)	0.31	(0.20)	0.02***	(0.00)
Sub-index of economic empowerment	0.48	(0.27)	0.44	(0.26)	0.04***	(0.01)
Sub-index of sociocultural empowerment	0.31	(0.33)	0.31	(0.33)	-0.00	(0.01)
Sub-index of interpersonal empowerment	0.18	(0.28)	0.17	(0.29)	0.01*	(0.01)
Household characteristics						
Highest male education (in completed years)	6.64	(4.78)	4.56	(4.41)	2.08***	(0.09)
Total number of working age memebers	3.78	(1.80)	3.42	(1.57)	0.35***	(0.03)
Asset index (0-30)[1]	10.60	(5.31)	7.84	(3.79)	2.76***	(0.10)
Debt status[2]	0.56	(0.50)	0.67	(0.47)	-0.11***	(0.01)
Brahmin	0.04	(0.20)	0.02	(0.15)	0.02***	(0.00)
Forward caste	0.17	(0.38)	0.10	(0.30)	0.07***	(0.01)
Other backward castes (OBC)	0.36	(0.48)	0.34	(0.47)	0.03***	(0.01)
Dalit	0.20	(0.40)	0.32	(0.47)	-0.12***	(0.01)
Adivasi/Tribal caste	0.09	(0.29)	0.14	(0.35)	-0.05***	(0.01)
Muslim	0.10	(0.30)	0.07	(0.26)	0.03***	(0.01)
Christian, Jain & Sikhs	0.03	(0.17)	0.01	(0.08)	0.02***	(0.00)
Eligible woman characteristics						
Age (in years)	32.96	(7.86)	32.84	(7.64)	0.11	(0.15)
Education (in completed years)	3.55	(4.23)	1.91	(3.18)	1.64***	(0.08)
Number of living children	2.83	(1.56)	3.00	(1.57)	-0.16***	(0.03)
Observations	11064		3333		14397	

Notes: The overall empowerment index is the average of the economic, sociocultural and interpersonal sub-indices, and ranges from 0 to 1. The economic sub-index is the average of binary variables based on the following eligible woman survey questions: "Do you yourself have cash in hand to spend on household expenditures?" and "Is your name on any bank account?". The sociocultural sub-index is the average of binary variables based on the following eligible woman survey questions: "Do you have to ask permission of your husband or a senior family member to go to the local health centre?" and "Do you practice the ghungat/purdah/pallu?". These questions are coded to reflect their inverse relationship with empowerment. The interpersonal sub-index is the average of binary variables based on the following eligible woman survey questions: "Do you have the most say in what to do if your child falls sick?", "Do you have the most say in how many children you have?" and "Do you have the most say in whether to buy an expensive item such as a TV or fridge?". [1] The asset index measures a household's ownership of consumer goods and household quality. [2] Debt status is a binary variable which equals 1 if a household has outstanding debt; and 0 otherwise. ***p< 0.01, **p< 0.05, *p<0.10.

Table 3.2: First stage: Effect of availability of an active NREGS in a village on household participation in the scheme

	(1) Post × NREGS participation
Post × Instrument	0.347***
	(0.012)
Observations	28,794
F-stat on excluded instrument	873.22
Controls	Yes
Household FE	Yes

Notes: NREGS is defined to be active in a village if at least one surveyed household in the village participated in the scheme in the year prior to the survey. Controls include total number of working age members in the household, eligible woman's age and a square in eligible woman's age. The un-interacted "NREGS participation" and "Instrument" terms drop out from the specification due to the inclusion of household fixed-effects. Standard errors clustered at the village-level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 3.3: Household and eligible woman characteristics by presence of an active NREGS in a village

	Villages with no NREGS activity		Villages with NREGS activity		Difference	
	mean	sd	mean	sd	b	se
Outcomes						
Overall index of empowerment	0.33	(0.18)	0.31	(0.19)	0.02***	(0.00)
Sub-index of economic empowerment	0.48	(0.26)	0.46	(0.27)	0.02***	(0.00)
Sub-index of sociocultural empowerment	0.33	(0.33)	0.29	(0.33)	0.04***	(0.01)
Sub-index of interpersonal empowerment	0.18	(0.26)	0.18	(0.29)	-0.00	(0.01)
Household memberships						
Member of Mahila Mandal[1]	0.09	(0.29)	0.10	(0.30)	-0.00	(0.01)
Member of caste association	0.15	(0.36)	0.14	(0.35)	0.00	(0.01)
Member of self-help group	0.10	(0.30)	0.13	(0.34)	-0.03***	(0.01)
Member of non-governmental organisation	0.02	(0.14)	0.01	(0.12)	0.01***	(0.00)
Member of co-operative society	0.09	(0.28)	0.04	(0.19)	0.05***	(0.00)
Member of trade union	0.04	(0.18)	0.03	(0.16)	0.01***	(0.00)
Member of credit and savings group	0.10	(0.29)	0.08	(0.27)	0.01***	(0.00)
Member of religious group	0.20	(0.40)	0.13	(0.34)	0.06***	(0.01)
Household voted in 2004 elections	0.92	(0.28)	0.92	(0.27)	-0.00	(0.00)
Household owns a ration card	0.86	(0.35)	0.86	(0.35)	0.00	(0.01)
Household has relatives/acquaintances in government service	0.32	(0.47)	0.29	(0.46)	0.03***	(0.01)
Household characteristics						
Asset index (0-30)[2]	11.49	(5.39)	9.23	(4.84)	2.27***	(0.09)
Debt status[3]	0.53	(0.50)	0.62	(0.49)	-0.09***	(0.01)
Total number of working age memebers	3.86	(1.82)	3.62	(1.72)	0.24***	(0.03)
Highest male education (in completed years)	6.89	(4.70)	5.81	(4.78)	1.07***	(0.09)
Brahmin	0.03	(0.18)	0.04	(0.20)	-0.01**	(0.00)
Forward caste	0.20	(0.40)	0.13	(0.34)	0.06***	(0.01)
Other backward castes (OBC)	0.36	(0.48)	0.36	(0.48)	0.01	(0.01)
Dalit	0.19	(0.39)	0.24	(0.43)	-0.06***	(0.01)
Adivasi/Tribal caste	0.08	(0.26)	0.12	(0.32)	-0.04***	(0.01)
Muslim	0.10	(0.29)	0.09	(0.29)	0.01	(0.01)
Eligible woman characteristics						
Age (in years)	33.08	(7.82)	32.86	(7.81)	0.22	(0.14)
Education (in completed years)	3.87	(4.31)	2.84	(3.91)	1.03***	(0.07)
Number of living children	2.80	(1.52)	2.91	(1.59)	-0.11***	(0.03)
Observations	4672		9725		14397	

Notes: [1] Mahila Mandals are informal social service clubs for rural women. [2] The asset index measures a household's ownership of consumer goods and household quality. [3] Debt status is a binary variable which equals 1 if a household has outstanding debt; and 0 otherwise. ***p< 0.01, **p< 0.05, *p<0.10.

Table 3.4: Effect of household participation in NREGS on the index of overall empowerment

	OLS			IV
	(1)	(2)	(3)	(4)
Post	0.047***	0.018***	0.033***	0.020**
	(0.004)	(0.004)	(0.006)	(0.008)
NREGS participation	-0.011*	-0.014***	-	-
	(0.006)	(0.004)	(.)	(.)
Post $\times$ NREGS participation	0.038***	0.040***	0.040***	0.089***
	(0.006)	(0.006)	(0.006)	(0.021)
Mean of dependent variable	0.32	0.32	0.32	0.32
Observations	28794	28794	28794	28794
Controls	No	Yes	Yes	Yes
Household FE	No	No	Yes	Yes

Notes: The index of overall empowerment is the average of the economic, sociocultural and interpersonal sub-indices, and ranges from 0 to 1. The economic sub-index is the average of binary variables based on the following eligible woman survey questions: "Do you yourself have cash in hand to spend on household expenditures?" and "Is your name on any bank account?". The sociocultural sub-index is the average of binary variables based on the following eligible woman survey questions: "Do you have to ask permission of your husband or a senior family member to go to the local health centre?" and "Do you practice the ghungat/purdah/pallu?". These questions are coded to reflect their inverse relationship with empowerment. The interpersonal sub-index is the average of binary variables based on the following eligible woman survey questions: "Do you have the most say in what to do if your child falls sick?", "Do you have the most say in how many children you have?" and "Do you have the most say in whether to buy an expensive item such as a TV or fridge?". Column (2) includes controls for total number of working age members in the household, dummies for caste categories, and an eligible woman's age, square of age and education in completed years. Time-invariant controls drop out in columns (3) and (4). Standard errors clustered at the village-level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 3.5: Effect of household participation in NREGS on the sub-index of economic empowerment

	OLS			IV
	(1)	(2)	(3)	(4)
Post	0.159***	0.125***	0.137***	0.114***
	(0.006)	(0.006)	(0.009)	(0.011)
NREGS participation	-0.041***	-0.043***	-	-
	(0.007)	(0.007)	(.)	(.)
Post × NREGS participation	0.106***	0.108***	0.108***	0.195***
	(0.010)	(0.010)	(0.010)	(0.029)
Mean of dependent variable	0.47	0.47	0.47	0.47
Observations	28794	28794	28794	28794
Controls	No	Yes	Yes	Yes
Household FE	No	No	Yes	Yes

Notes: The economic sub-index ranges from 0 to 1, and is computed as the average of binary variables based on the following eligible woman survey questions: "Do you yourself have cash in hand to spend on household expenditures?" and "Is your name on any bank account?". Column (2) includes controls for total number of working age members in the household, dummies for caste categories, and an eligible woman's age, square of age and education in completed years. Time-invariant controls drop out in columns (3) and (4). Standard errors clustered at the village-level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 3.6: Effect of household participation in NREGS on the sub-index of sociocultural empowerment

	OLS			IV
	(1)	(2)	(3)	(4)
Post	-0.063***	-0.085***	-0.086***	-0.104***
	(0.007)	(0.007)	(0.010)	(0.015)
NREGS participation	0.010	0.001	-	-
	(0.011)	(0.007)	(.)	(.)
Post × NREGS participation	0.007	0.010	0.009	0.077**
	(0.010)	(0.010)	(0.010)	(0.039)
Mean of dependent variable	0.31	0.31	0.31	0.31
Observations	28794	28794	28794	28794
Controls	No	Yes	Yes	Yes
Household FE	No	No	Yes	Yes

Notes: The sociocultural sub-index ranges from 0 to 1, and is computed as the average of binary variables based on the following eligible woman survey questions: "Do you have to ask permission of your husband or a senior family member to go to the local health centre?" and "Do you practice the ghungat/purdah/pallu?". These questions are coded to reflect their inverse relationship with empowerment. Column (2) includes controls for total number of working age members in the household, dummies for caste categories, and an eligible woman's age, square of age and education in completed years. Time-invariant controls drop out in columns (3) and (4). Standard errors clustered at the village-level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 3.7: Effect of household participation in NREGS on the sub-index of interpersonal empowerment

	OLS			IV
	(1)	(2)	(3)	(4)
Post	0.045***	0.014**	0.048***	0.050***
	(0.007)	(0.007)	(0.011)	(0.014)
NREGS participation	-0.002	-0.001	-	-
	(0.008)	(0.007)	(.)	(.)
Post × NREGS participation	-0.001	0.003	0.002	-0.004
	(0.011)	(0.011)	(0.011)	(0.037)
Mean of dependent variable	0.18	0.18	0.18	0.18
Observations	28794	28794	28794	28794
Controls	No	Yes	Yes	Yes
Household FE	No	No	Yes	Yes

Notes: The interpersonal sub-index ranges from 0 to 1, and is computed as the average of binary variables based on the following eligible woman survey questions: "Do you have the most say in what to do if your child falls sick?", "Do you have the most say in how many children you have?" and "Do you have the most say in whether to buy an expensive item such as a TV or fridge?". Column (2) includes controls for total number of working age members in the household, dummies for caste categories, and an eligible woman's age, square of age and education in completed years. Time-invariant controls drop out in columns (3) and (4). Standard errors clustered at the village-level are in parentheses. ***$p< 0.01$, **$p< 0.05$, *$p<0.10$.

Table 3.8: IV results for the subsample excluding households where the eligible woman participated in NREGS

	Empowerment Index			
	Overall	Economic	Sociocultural	Interpersonal
Post	0.020**	0.113***	-0.102***	0.049***
	(0.008)	(0.012)	(0.015)	(0.014)
Post × NREGS	0.098***	0.199***	0.121*	-0.027
participation	(0.035)	(0.050)	(0.067)	(0.062)
Mean of dependent variable	0.32	0.47	0.31	0.18
Observations	24854	24854	24854	24854
Controls	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes

Notes: Participant households in the sample include only those households where household members (men or women) other than the eligible woman participated in the scheme. The index of overall empowerment is the average of the economic, sociocultural and interpersonal sub-indices, and ranges from 0 to 1. The economic sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you yourself have cash in hand to spend on household expenditures?" and "Is your name on any bank account?". The sociocultural sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you have to ask permission of your husband or a senior family member to go to the local health centre?" and "Do you practice the ghungat/purdah/pallu?". These questions are coded to reflect their inverse relationship with empowerment. The interpersonal sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you have the most say in what to do if your child falls sick?", "Do you have the most say in how many children you have?" and "Do you have the most say in whether to buy an expensive item such as a TV or fridge?". All columns include controls for total number of working age members in the household, and an eligible woman's age and square of age. Standard errors clustered at the village-level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 3.9: IV results for the subsample excluding households where other household members participated in NREGS

	Empowerment Index			
	Overall	Economic	Sociocultural	Interpersonal
Post	0.021**	0.118***	-0.106***	0.049***
	(0.008)	(0.012)	(0.016)	(0.015)
Post × NREGS	0.133***	0.293***	0.145*	-0.037
participation	(0.045)	(0.063)	(0.085)	(0.079)
Mean of dependent variable	0.32	0.47	0.31	0.18
Observations	23792	23792	23792	23792
Controls	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes

Notes: Participant households in the sample include only those households where the eligible woman participated in the scheme. The index of overall empowerment is the average of the economic, sociocultural and interpersonal sub-indices, and ranges from 0 to 1. The economic sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you yourself have cash in hand to spend on household expenditures?" and "Is your name on any bank account?". The sociocultural sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you have to ask permission of your husband or a senior family member to go to the local health centre?" and "Do you practice the ghungat/purdah/pallu?". These questions are coded to reflect their inverse relationship with empowerment. The interpersonal sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you have the most say in what to do if your child falls sick?", "Do you have the most say in how many children you have?" and "Do you have the most say in whether to buy an expensive item such as a TV or fridge?". All columns include controls for total number of working age members in the household, and an eligible woman's age and square of age. Standard errors clustered at the village-level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 3.10: IV results for the subsample of women who worked for pay in 2004-05

	Empowerment Index			
	Overall	Economic	Sociocultural	Interpersonal
Post	0.037***	0.156***	-0.117***	0.072***
	(0.011)	(0.017)	(0.020)	(0.018)
Post × NREGS	0.069***	0.171***	0.059	-0.023
participation	(0.020)	(0.029)	(0.037)	(0.033)
Mean of dependent variable	0.32	0.47	0.31	0.18
Observations	14796	14796	14796	14796
Controls	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes

Notes: Here, women who worked for pay refers to women who egnaged in salaried work, agricultural wage work, and non-agricultural wage work. The index of overall empowerment is the average of the economic, sociocultural and interpersonal sub-indices, and ranges from 0 to 1. The economic sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you yourself have cash in hand to spend on household expenditures?" and "Is your name on any bank account?". The sociocultural sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you have to ask permission of your husband or a senior family member to go to the local health centre?" and "Do you practice the ghungat/purdah/pallu?". These questions are coded to reflect their inverse relationship with empowerment. The interpersonal sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you have the most say in what to do if your child falls sick?", "Do you have the most say in how many children you have?" and "Do you have the most say in whether to buy an expensive item such as a TV or fridge?". All columns include controls for total number of working age members in the household, and an eligible woman's age and square of age. Standard errors clustered at the village-level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 3.11: IV results for the subsample of women who did not work for pay in 2004-05

	Empowerment Index			
	Overall	Economic	Sociocultural	Interpersonal
Post	0.008	0.080***	-0.095***	0.039**
	(0.009)	(0.014)	(0.018)	(0.017)
Post × NREGS	0.113**	0.202***	0.160*	-0.023
participation	(0.050)	(0.071)	(0.091)	(0.091)
Mean of dependent variable	0.32	0.47	0.31	0.18
Observations	13984	13984	13984	13984
Controls	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes

Notes: Here, women who did not work for pay refers to women who did not egnage in salaried work, agricultural wage work, and non-agricultural wage work. The index of overall empowerment is the average of the economic, sociocultural and interpersonal sub-indices, and ranges from 0 to 1. The economic sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you yourself have cash in hand to spend on household expenditures?" and "Is your name on any bank account?". The sociocultural sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you have to ask permission of your husband or a senior family member to go to the local health centre?" and "Do you practice the ghungat/purdah/pallu?". These questions are coded to reflect their inverse relationship with empowerment. The interpersonal sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you have the most say in what to do if your child falls sick?", "Do you have the most say in how many children you have?" and "Do you have the most say in whether to buy an expensive item such as a TV or fridge?". All columns include controls for total number of working age members in the household, and an eligible woman's age and square of age. Standard errors clustered at the village-level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

Table 3.12: OLS estimates of access to an active scheme on non-participant households

	Empowerment Index			
	Overall	Economic	Sociocultural	Interpersonal
Post	0.026***	0.126***	-0.098***	0.051***
	(0.006)	(0.012)	(0.015)	(0.014)
Post ×	0.018***	0.033***	0.025*	-0.005
Village with an active NREGS	(0.004)	(0.011)	(0.014)	(0.013)
Mean of dependent variable	0.32	0.48	0.30	0.18
Observations	21022	21022	21022	21022
Controls	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes

Notes: The sample includes only non-participant households. NREGS is defined to be active in a village if at least one surveyed household in the village participated in the scheme in the year prior to the survey. The index of overall empowerment is the average of the economic, sociocultural and interpersonal sub-indices, and ranges from 0 to 1. The economic sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you yourself have cash in hand to spend on household expenditures?" and "Is your name on any bank account?". The sociocultural sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you have to ask permission of your husband or a senior family member to go to the local health centre?" and "Do you practice the ghungat/purdah/pallu?". These questions are coded to reflect their inverse relationship with empowerment. The interpersonal sub-index ranges from 0 to 1 and is the average of binary variables based on the following eligible woman survey questions: "Do you have the most say in what to do if your child falls sick?", "Do you have the most say in how many children you have?" and "Do you have the most say in whether to buy an expensive item such as a TV or fridge?". All columns include controls for total number of working age members in the household, and an eligible woman's age and square of age. Standard errors clustered at the village-level are in parentheses. ***p< 0.01, **p< 0.05, *p<0.10.

3.9. Figures

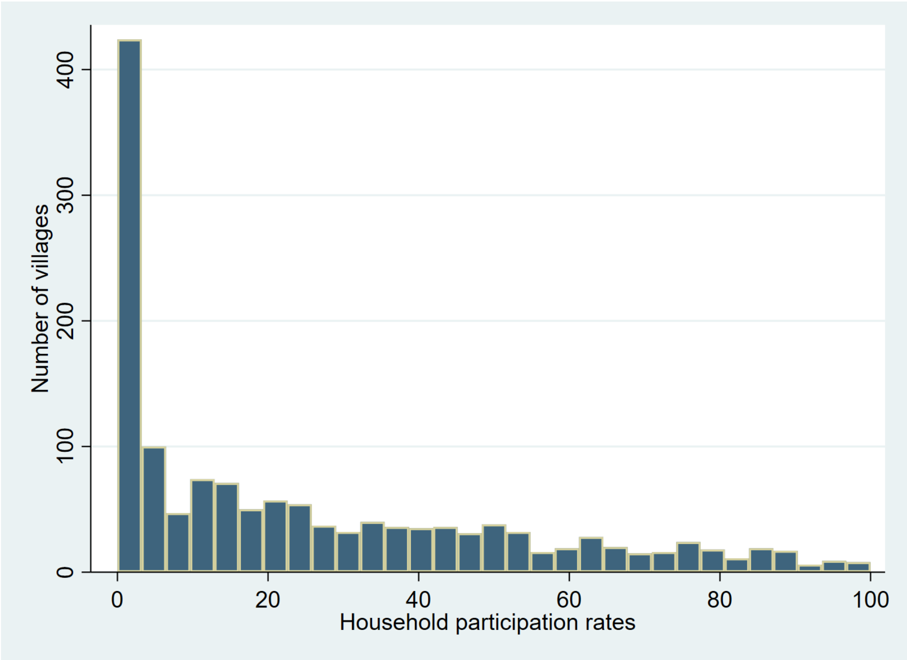


Figure 3.1: Distribution of household NREGS participation rates in villages

Notes: The household participation rate for each village is calculated using the self-reported NREGS participation status of all surveyed households in the village. There are a total of 1420 villages in the sample. Source: IHDS-II

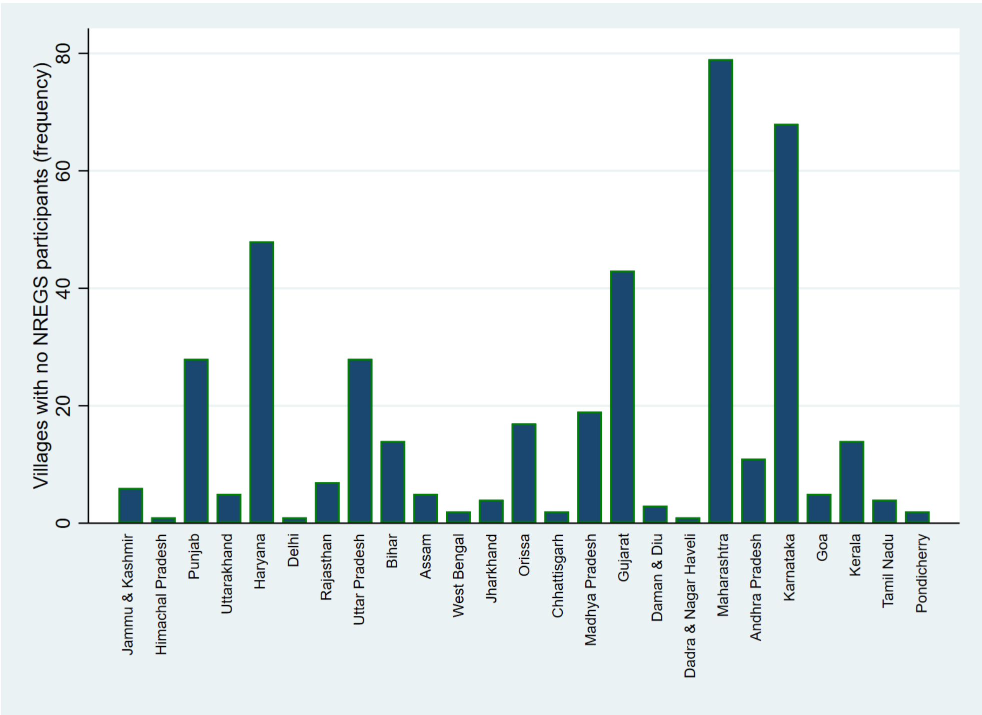


Figure 3.2: State-wise distribution of villages with no NREGS participants

Notes: Villages in the sample where no surveyed household participated in the scheme in the year prior to the survey are categorised as 'villages with no NREGS participants'. There are a total of 1420 villages in the sample. Source: IHDS-II

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