ABSTRACT

Title of dissertation: THE LITTLE PUSH: ROLE OF INCENTIVES IN DETERMINING HOUSEHOLD BEHAVIOUR IN INDIA

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Doctor of Philosophy, 2018

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A substantial proportion of households in India suffer from multiple deprivations - low income, poor health, low education levels and poor housing conditions. Additionally, gender biases arising out of cultural norms disadvantage women even more. Programs that transfer cash to households are one way of attempting to break the cycle of poverty and gender bias.

In my dissertation I study the impacts of three different cash transfer programs in India that specifically target the rural poor and attempt to either change behaviours directly or impact education and health as unintended consequences. In my first chapter, I hypothesize that a crucial determinant of son preference in Indian households is the high future costs of raising girls which arise from cultural traditions such as dowry payments at the time of marriage and impost a huge economic burden on households. I explore the role of future costs in determining son preference through the evaluation of a government program that was implemented in one state in India which gave incentives to couples to give birth to girls. I empirically

show that an exogenous change in these future costs can have dramatic positive implications for fertility and sex-selective abortion behavior on the one hand, as well as positively impacting differential investment allocation within the household on the other. In my second essay, I examine whether provision of free rural health insurance in a developing country enables households to cope with health shocks by examining impacts on labour supply of individual household members. I find that, while men and women, both are increasingly likely to spend more hours per week on the labour market, the increase in labour supply for women is much steeper. I provide evidence that the program acts through two channels: a reduction in time spent at home for women in caregiving tasks and a reduction in time spent being unable to work due to major morbidities. Finally, in my third chapter, I examine if a public works program in India, that guaranteed employment to rural households, helps ameliorate the impacts of a negative agricultural productivity shock on child health.

THE LITTLE PUSH: ROLE OF INCENTIVES IN DETERMINING HOUSEHOLD BEHAVIOUR IN INDIA

by

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Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2018

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Dedication

To my beloved parents, Lakshmi and Balakrishnan Srinivasan,

for teaching me about hard work, love and kindness,

and to my husband and best friend, Kartik Misra,

for giving me courage, support and companionship.

Acknowledgments

I owe my gratitude to all the people who have made this thesis possible and because of whom my graduate experience has been one that I will cherish forever.

I would like to especially thank all of my dissertation committee who provided me with thoughtful advice and patient guidance throughout my graduate studies. I have learnt a lot from you. I am so grateful for having had the opportunity to work with you, Dr. Jakiela, Dr. Leonard, Dr. Alberini, Dr. Cai and Dr. Urzua.

I would like to thank my advisor, Professor Pamela Jakiela for giving me invaluable opportunities to work on challenging and extremely interesting projects over the past five years. She introduced me to field work in Kenya which remains one of my most treasured experiences in graduate school. Thank you, Pam, for pushing me to pursue excellence and rigor, for always being willing to give advice, and for inspiring me to be a better researcher.

I would also like to thank my co-chair, Professor Kenneth Leonard for always making me think hard about why my research matters and for pushing me to do meaningful research. Thank you, Ken, for always having your door open and giving me time and for showing me how to be an academic researcher who cares deeply, both, about his work and his students.

Finally, I would like to give special thanks to Magda Tsaneva for her friendship, help and guidance and for teaching me how to be better person.

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Chapter 1: Incentives for Girls and Gender Bias in India

1.1 Introduction

Over 35 million women are estimated to be 'missing' in India (Bongaarts and Guilmoto, 2015).[1] This is much starker between the ages of 0-6 years. In the Indian Census of 2011, the female-to-male child sex ratio was 919 girls for every 1000 boys.[2] If one were to expect equal populations of the two sexes then this would indicate an 8 percent deficit of girls; but, in countries where girls and boys receive similar care, girls outnumber boys, implying that the actual deficit of young girls in India is much larger.[3] Rising male-biased child sex ratios have been attributed to an increase in the deficit of girls at birth due to sex-selective abortions (Bhalotra and Cochrane, 2010, Anukriti et al., 2016, Jha et al., 2006) and a deficit of girls post-birth due to excess female mortality at early ages (Sen, 1990, Sen, 1992, Gupta, 1987). However, a more fundamental question that has not received enough attention is, what are the underlying motives that generate a preference for sons in the first place? On the one hand, economists have argued that poverty and deprivation are important determi-

[1]The term "missing girls" was first coined by Sen [1992]. Sen calculated that there were approximately 100 million missing women as a result of unequal health and nutrition inputs.

[2]Child sex ratio is defined as the female-to-male sex ratio in the 0-6 age group. Throughout the paper, I define the sex ratio as females to males.

[3]At birth, boys outnumber girls biologically with the biological sex ratio being 964 girls for every 1000 boys. However, in early childhood in countries where girls and boys receive equal care, biology favors women with the normal sex ratio being 1050 girls to 1000 boys (Sen, 1992).

nants of this behaviour. While there is some evidence for this (see e.g. Rosenzweig and Schultz, 1982), overall, this has been called into question. It is precisely during the period that India witnessed high economic growth (i.e. since the late 1980s), that the problem of sex-selective abortions has worsened (Sen, 2003, Sekher, 2012, Bhat, 2002).[4] On the other hand, there have been arguments made that there are intrinsic cultural factors like patriarchy and religious roles performed by sons, may be responsible for India's gender imbalance (Das Gupta et al., 2003, Jayachandran and Pande, 2015). But this also appears to be an incomplete explanation since there is enough evidence from representation of women in higher education and at higher levels of government in India to challenge the claim that 'only culture matters' (Sen, 1990). In this paper, I argue that the determinant of behaviours such as sex-selective abortions and neglect of girls lies at the confluence of these two extremes. *High future costs* of girls, which stem from cultural traditions, and impose a large economic strain on households are a crucial driving force of discrimination. I hypothesize that the high future costs of female children in India may be an important determinant of whether a girl is born and the (differential) allocation she receives within the household.

In India, sons are viewed as future benefits and daughters are viewed as future costs. Parents in India may prefer boys because the labour market returns to investments in girls may be lower than those for boys (Kingdon, 1998, Rosenzweig and Schultz, 1982). They may also prefer boys because culturally, sons stay with their parents and are expected to give a portion of their labour income to their parents

[4] Rosenzweig and Schultz [1982] and Qian [2008] show that households selectively allocate resources to children in response to changes in sex-specific earnings for adults in India and China respectively.

as opposed to daughters who leave their natal home after marriage. Finally, in India incentives for discrimination arise from a third, more ubiquitous, source: dowry payments for girls. Anderson [2003] finds that dowry payments are ubiquitous with more than 90 percent of marriages in India including such a payment. She also finds that these payments can be a huge strain on household finances with the average dowry payment ranging from 200 to 500 percent of median annual household income. Moreover, unlike differences in adult labour income which narrow over time, dowry costs actually increase with modernization in caste-based societies like India (Anderson, 2003).[5] Consequently, if the high future cost of girls are the predominant reason for son preference and the resultant male-biased child sex ratios, *and* if these costs are likely to rise with modernization; then it is unlikely that the gender imbalance will resolve itself without active policy intervention.

High future costs of girls are likely to have large impacts on excess female mortality through two mechanisms. First, son based fertility leads to higher fertility rates because, in the absence of state led social support for retirement, sons are expected to contribute their labour income to their parents. This is the household size effect, wherein even in the absence of active discrimination, girls are worse off because they grow up in larger families.[6] There is a large literature which dis-

[5]According to Anderson [2003], modernization leads to increases in both average wealth in a society and wealth dispersion within social groups. Increases in wealth dispersion within caste groups in India leads to dowry inflation. This is because, say high caste grooms witness an increase in the spread of their incomes. Then, a high caste groom who is low-ranked because of this dispersion, will be less valued by a bride from his own caste. However, a bride from his own caste will not reduce the amount of dowry she's willing to pay because she faces competition from other low caste brides. Thus, even though this groom has a lower income and is therefore less valued by brides in his caste, his caste status remains, and competition from lower caste brides partially insulates him from his lower earning power. This creates a floor for dowry payments for this particular caste. Other higher ranked grooms, thus receive even higher dowry payments since their higher income makes them more valuable. Thus, average dowry payments increase in such a caste-based society when wealth becomes more heterogeneous *within* groups.

[6]Son-biased stopping behaviour implies that couples are more likely to continue having children

cusses son-biased fertility stopping behavior (Jensen, 2012; Clark, 2000; Bhalotra and Van Soest, 2008;Rosenblum, 2013; Alfano, 2017). Second, high future costs of girls also lead to lower investments in their health and human capital, because of the sex-composition effect. That is, human capital investments in a girl are less if she grows up in a household with a higher proportion of girls. This is because from a life-cycle perspective, the higher the proportion of daughters in the household, the costlier it is for parents to keep those daughters alive, and hence, they invest less in all of them (Pande et al., 2006, Rosenblum, 2013).

High future costs of girls also potentially impact the use of sex-selective abortions. Couples with low desired fertility and easy access to sex detection technology use sex-selective abortions as a means to achieve their desired sex ratio with a smaller number of children.[7] That is, high future costs of girls could lead couples to defray these costs by choosing to not have girls altogether. In the long run, sex-selective abortions are expected to lead to girls less likely to be born and conditional on being born, increasingly growing up in larger families, thus indirectly also contributing to their higher mortality.[8] Moreover, the decline in fertility is expected to exacerbate this problem.[9,10]

if they have a higher proportion of girls in the hope of conceiving a son.

[7]See Hu and Schlosser [2015] and Anukriti et al. [2016] for information about ultrasound technology in India. Ultrasound scans are inexpensive and consequently their demand has seen a steep increase. The number of abortions in India has seen a steep increase since the late 1980s (see Figure 2 in Anukriti et al., 2016).

[8]A shift in the distribution of girls towards poorer families implies that in the long run girls are expected to be on a lower steady-state than boys because of the *types* of families girls are increasingly born into.

[9]Bhalotra and Cochrane [2010] show that ultrasound technology was mainly used by richer, more literate women. Thus poorer women continued to engage in the first mechanism, son-biased fertility stopping behaviour, in order to change the gender composition of their children.

[10]In a cross-sectional study using primary data collected from Haryana in northern India, Jayachandran [2017] shows that the desired male to female sex ratio increases sharply as fertility falls. More specifically, she finds that when the family size specified to the respondent is 3 children, the desired male to female sex ratio is 1.12, while with 2 children, it rises to 1.20. When the hypothetical family size falls to 1, the vast majority of people want a son, and the desired male to

This paper examines whether a *decrease* in the future costs of girls affects their deficit at young ages. I study the *Bhagyalakshmi* program introduced in the state of Karnataka in 2006. Under the program, couples who give birth to daughters after March 2006, receive a long-term savings bond in the name of the girl, which is redeemable once the girl turns 18 and is unmarried. The present discounted value of the incentives received is about Rs. 35,042 ($543) which is large given that average annual per capita expenditure in Karnataka in 2004-05 was about Rs. 16,800 ($240) in rural areas (National Sample Survey, 2004-05). I estimate the causal impact of the *Bhagyalakshmi* program on the overall child sex ratio. I then decompose this into impacts on girl deficit at birth on the one hand *and* impacts on girl deficit post-birth, on the other.

There are four primary contributions of this paper. First, I present evidence that the high future cost of girls is an important driver of their observed deficit at birth, by demonstrating the impact of the program on fertility choices and sex-selective abortion decisions of couples. Second, I show that the deficit of girls post-birth, measured through excess female infant mortality and differential health investments, is *also* a function of their actual future costs as opposed to these being the result of an inherent dislike for girls. Third, I provide evidence of substantial forward-looking behaviour by demonstrating that the expectation of a payment 18 years in the future can lead to improvements in long-term nutritional indicators. Finally, I provide the first quantitative estimates of the number of girls who were 'saved' because of a policy instrument that recognized the complex economic and cultural determinants of fertility and investment choices of couples.

female sex ratio rises to 5.6.

I start by first modeling some aspects of the high future costs of girls within the household and to investigate how changes in these costs might impact a couple's fertility, selective abortion and health investment decisions. Couples have children both for old age income support and because they gain intrinsic utility from children. I argue that boys always increase future parental consumption and girls, due to their high future costs, reduce future consumption. I then demonstrate that an exogenous decrease in the future costs of girls leads to an increase in fertility. This increase in fertility is a result of both more women choosing to conceive and less women choosing to selectively abort female fetuses. However, there is an ambiguous impact on girl mortality because investments in the health of girls can either increase or decrease when future costs change. The impact on investments is a combination of an indirect effect through changes in fertility and the direct effect through changes in income. For households for whom the former effect dominates, health investments in girls are predicted to decrease while households for whom the income effect dominates, health investments in girls increase. Finally, I show that if the decrease in future costs of girls is large, then girls are no longer disadvantaged by having a higher proportion of girl siblings. This in turn, reduces incentives for son-biased fertility stopping behavior. Thus, reductions in the future cost of girls is seen to be a potentially important factor in reversing the trend of missing women in India.

To empirically test these predictions, I first identify the impact of the *Bhagyalakshmi* program on child sex ratios at the most disaggregated level possible: villages and towns, I use the Indian Census data. My identification of causal impacts in this setting exploits temporal and regional variation in the introduction of the program. Regional variation arises because the program was only available in the state of

Karnataka and temporal variation stems from the timing of the program: the program was applicable to all births after March 2006. I use these sources of variation to measure the impact of *Bhagyalakshmi* on child sex ratios using a difference-in-differences framework. My estimates indicate that *Bhagyalakshmi* led to a large increase in the female to male child sex ratio by about 19 points. That is, there were 19 to 25 *additional* girls per 1000 boys in the 0-6 age group after the program. The observed impact on child sex ratio could stem from two sources: changes in sex ratio at birth and changes in differential girl mortality after birth. As predicted by the theoretical framework, it is possible that girl fetuses not aborted at birth could grow up with fewer resources. Decomposing the source of the impact on child sex ratio is important in understanding if there was a substitution of sex-selective abortions towards post-birth girl neglect after the introduction of the *Bhagyalakshmi* program.

I analyze if the impact on child sex ratio is the result of changes in fertility and selective abortion decisions before birth *or* a result of changes in excess female infant mortality and differential treatment of girls after birth *or* a combination of both. I use retrospective birth history data from three repeated cross sectional rounds of the District Level Household Survey (DLHS) to decompose the impacts on child sex ratio. First, in order to examine impacts on fertility and selective abortion decisions before birth, I construct a woman-year panel and examine impacts in a difference-in-differences framework similar to the one described above. My results suggest that decreasing the future costs of girls causes a large increase in the probability of a birth in Karnataka by 6%. This increase in fertility stems from both an increase in the unconditional probability of a female birth and an increase in the unconditional probability of a male birth. However, the increase in the unconditional probability of a female birth is larger, suggesting that not only were there new pregnancies, but there were also fewer selective abortions of existing pregnancies. Specifically, the unconditional probability of a female birth increased by 9%, about half of which is due to fewer sex-selective abortions and half is attributable to an increase in new pregnancies. This decline in the willingness to selectively abort female fetuses led to an increase in the sex ratio at birth of about 13 additional girls at birth for every 1000 boys. I also find that the probability of desiring the next child to be a girl increases by 22%. While self-reported desired fertility is subject to bias, this nevertheless indicates that future costs *do* drive son preference, since a reduction in these costs significantly changed the desired number of girls, even at smaller family sizes. Finally, there is also suggestive evidence that the program decreased fertility for couples with first born girls when compared to couples with first born boys, thus indicating a reduction in son-biased fertility stopping behaviour.

In order to examine the impact on excess female infant mortality and health investments, I perform the analysis at the child level and employ regional and cohort-specific variation in addition to gender specific variation. I compare outcomes of children born after March 2006 in Karnataka to outcomes of children born before 2006 and to children in control states. Additionally, since the program was only available for girls, I use boys as a third control group. In order to account for selection into conception and disentangle compositional and causal effects, I examine selection directly by examining if observable family characteristics of girls changed in Karnataka after the introduction of the program. I find that the introduction of the *Bhagyalakshmi* program led to a reduction in excess female infant mortality by 0.13

percentage points and led to a reduction of the baseline mortality gap by 81 percent. With respect to health investments which are proxied by exclusive breastfeeding in this study, I find that while there is only a small impact at the intensive margin (months of exclusive breastfeeding); there is large and significant impact at the extensive margin (probability of being exclusively breastfed for 6 months). A simple accounting exercise shows that this explained about 37.14 percent of the excess female infant mortality decline. Finally, I also document an improvement in the *long-run* nutritional outcomes of girls in Karnataka. I find a complete elimination of the baseline gap in the height-for-age z-scores of girls relative to boys and relative to children in control states. An important caveat is that the improvements in investments reflect the average impact, indicating that the proportion of households for whom the income effect dominates is large. Girls in households in which the household size dominates, could potentially be worse off.

My estimates imply that the *Bhagyalakshmi* program led to 28,124 *additional* girls surviving up to age 6, annually. Further, reductions in the number of sex-selective abortions and female infant deaths led to 17,368 additional girls. Thus, improvements in the sex-ratio at birth *and* reductions in excess female infant mortality contribute about 62 percent to the total increase in the number of girls in the 0-6 age group. These results imply that there was no substitution from sex-selective abortions towards post-birth girl neglect. Overall child sex ratio improved both because couples aborted fewer female fetuses *and* because they increased investments in girls.

By relating a long-term conditional cash transfer for giving birth to girls to fertility and investment decisions of couples, my paper shows that policies that alter the relative costs of raising boys and girls can be effective at reducing son-biased fertility stopping, sex-selective abortions and post-birth girl neglect. Many previous explanations for these behaviours attribute them to either cultural factors wherein parents have a a 'preference' for boys in the economic sense or to a symptom of underdevelopment and poverty. The results in this paper, by contrast, argue that the high cost of raising girls, which stem from cultural traditions, impose a huge economic burden on households, and thus create incentives to discriminate.

This study contributes to several literatures. First, I contribute to the literature which examines the manifestations of son preference such as son biased fertility stopping (Bhalotra and Van Soest, 2008; Rosenblum, 2013) and unequal human capital investments in girls versus boys (Jayachandran and Kuziemko, 2009; Bharadwaj and Lakdawala, 2013; Oster, 2005). While there is emerging acknowledgment of the role of income and costs in leading to such behaviours, there is limited evidence to support the same, barring two studies. Alfano [2017] examines the impact of Dowry Prohibition Rules on son-biased fertility stopping behaviour and Bhalotra et al. [2016] study the impact of gold price shocks on differential girl mortality. However, neither of these studies examine changes in actual monetary costs of girls. My estimates provide the first evidence that actual future economic costs of daughters are important determinants of the trend in missing women.

I also provide new evidence on the impact of conditional cash transfers on child mortality, health investments and long run nutritional outcomes in the South Asian context. CCT programs aim to reduce poverty in the short-term and improve human capital in the longer-term by encouraging behaviors related to health and education. These programs have been shown to improve a broad range of child health outcomes in many countries - Mexico (Gertler, 2000), Nicaragua (Maluccio and Flores, 2005), Brazil (Gilligan and Fruttero, 2011) and El Salvador (De Brauw et al., 2011). The impact of a CCT program with a payment in the long-term as opposed to regular payments in the present is not known. Additionally, the literature examining CCTs and their gender differentiated impacts on children in South Asia is sparse and I contribute to this limited literature.

Finally, I contribute to the more limited literature which provides evidence of forward-looking behaviour amongst the poor in developing countries. For instance, Jayachandran and Lleras-Muney [2009] show that if women have higher life expectancy then there are positive spillovers on girls' human capital outcomes as a result of adjusted expectations about mortality risk. Similarly, Beaman et al. [2012] show that the future expectation of female leadership positions in village councils in India increases human capital investments in girls in the present. Jensen [2012] and Khanna [2016] show that the expectation of a high wage job in the future increases educational attainment of younger cohorts. I contribute to this literature by providing evidence of the impact of the *expectation* of a long-term future payment on decreasing the selective abortion of girls and increasing health investments in them in the short to medium run.

The rest of the paper is structured as follows: Section 1.2 provides a theoretical motivation for the study and Section 1.3 provides details about the program. Section 1.4 introduces the data and provides descriptive evidence. Sections 1.5, 1.6 and 1.7 examine the impact of the program on the child sex ratio, girl deficit at birth and girl deficit after birth respectively. Section 1.8 examines heterogeneous impacts and Section 1.9 examines the long run nutritional impact of the program. Section 1.10

quantifies the impact of the program into the number of girls 'saved' and Section 1.11 concludes.

1.2 Conceptual Framework

I motivate the analysis using a model of fertility behaviour adapted from Rosenblum [2013] and Eswaran [2002]. The model presents conditions under which a reduction in the future cost of daughters can affect gender bias through its impact on fertility and selective abortion choices on the one hand and through changes in healthcare investments in girls, on the other.

Parents make decisions in two periods. In period one they decide how many children to have and also decide the amount of investment in each child in terms of health inputs. There are two types of costs the household faces: a fixed cost per child, and a gender specific health input for each child. The higher the investment in children in the first period, the greater their likelihood of surviving until the second period. In period two, parents derive the returns of investing in their children. One can think of the future benefits of sons as their labour income in the joint household as well as the labour income of their future wives. The future net costs of daughters are the costs of getting them married minus any labor income they may send back to their natal homes.[11]

The major difference between the following framework and traditional models of fertility is that in my model I assume that investing in a daughter reduces future income. In the model by Garg and Morduch [1998] who examine the impact

[11] In India, where joint households are common, married sons usually remain with and support their parents. By contrast, married daughters leave their parental home and are not expected to provide financial support to their parents.

of sex composition of siblings on child health in Ghana, the authors assume that investments in girls always increases future income, albeit at a lower rate than boys. In my model, I examine the Indian context, wherein, the more you invest in your daughter, the more expensive she becomes in the future since she is more likely to survive to the point, where costs are faced. While this might not be true for all households, the model represents the incentives faced by the average household in India.

1.2.1 Basic Model

Parental consumption in each period is c_j (where $j = 1, 2$). I follow Cigno [1998] and Eswaran [2002] and assume that there are N children, in a household with θ of them being boys and $1 - \theta$ being girls where $\theta < 1$. Parents also derive survival utility from their children. They can increase the number of children who survive by investing in child health in period 1. Health investments in child i are given by k_i. Following Rosenblum [2013], I define the fraction of children of gender i surviving as, $p(k_i)$.

The lifetime utility of parents is a sum of their utilities from periods 1 and 2 ($U_1(c_1)$ and $U_2(c_2)$) and utility from having children survive, U_S. It is given by:

$$U_T(N, k_g, k_b) = U_1(c_1) + U_2(c_2) + U_S[p(k_b)\theta N + p(k_g)(1 - \theta)N] \tag{1.1}$$

The model assumes that there are no intrinsic reasons that lead to parents caring more about sons. That is, parents derive equal utility from the survival of girls as they do from the survival of boys.

Parents face a budget constraint in each period. In the first period their exogenous income is Y_1 and they incur fixed and variable costs per child. In this model, variable costs include investments in health for child i, k_i. In the second period parents consume leftover income Y_2. Let the *future net benefit* from each surviving boy be B and the *future net cost* for each surviving girl be G. I assume that households cannot borrow, save or accumulate assets. All decisions are taken in period 1. The budget constraints for period 1 and period 2 respectively, are:

$$c_1 + k_b\theta N + k_g(1-\theta)N \leq Y_1$$

$$c_2 \leq Y_2 + \theta N p(k_b)B - (1-\theta)Np(k_g)G$$

In this basic model, I assume that sex-selective abortion is not a fertility option. I will introduce it explicitly in Proposition 4. Thus, utility is maximized by only choosing, N and the investments in each child, k_b, k_g. Human capital investments are determined by N and θ. Parents will keep girls alive if the survival utility of girls exceeds their consumption utility cost. I assume that there is always an interior allocation i.e. parents always invest a positive amount in girls.

From the model, the following propositions hold. All proofs are in **Appendix A.4**:

Proposition 1:

(a) *Household Size Effect: At the optimum of parents, fertility and investments in girls are perceived as substitutes i.e.* $\frac{\partial k_g^*(N,\theta)}{\partial N} < 0$

(b) *Sex-Composition Effect: At the optimum of parents, if the future costs of girls, G* *are large enough, then girls do better in terms of health investments, in households with a higher fraction of boys*

(a) The intuition for this is as follows: assuming an interior allocation, i.e. the survival utility of girls outweighs the consumption utility cost. An increase in fertility at current levels of health investment per girl will increase the couple's future consumption by increasing the expected number of surviving girls, thereby lowering the marginal utility of future consumption. Parents view fertility and health investments per girl as substitutes in the provision of a more secure future: if one is parametrically increased, the other decreases.[12] (b) A household with a higher proportion of sons will invest more in each girl or conversely, a household with more daughters will invest less in each additional daughter if the costs of girls are large enough. This is the 'sex-composition effect'. This is because, in a household with a larger fraction of girls the combined future dowry payments is large and is not offset by concomitant dowry receipts. Thus a girl with many brothers will be better off since her future costs are ameliorated by the presence of her brothers. Thus, a girl with many sisters will be worse off than a girl with many brothers.

Proposition 2:

(a) *An exogenous decrease in the future costs of girls, G, will lead to an increase in a couples' fertility and,*

(b) *Ambiguous changes in the health investments in girls. Health investments will decrease if the indirect effect through changes in fertility dominates, while invest-*

[12] While increasing health investments in girls results in higher future utility if an interior allocation is assumed, increases in health investments of boys will *always* result in even higher future utility in this model, thereby generating unequal resource allocations between girls and boys.

ments will increase if the direct effect through changes in income dominates

(a) The introduction of the *Bhagyalakshmi* program will reduce the future net cost of each surviving daughter G and thus increase period 2 utility. Intuitively, a decrease in G implies that period 2 marginal utility of consumption must rise. Thus, the willingness to conceive in this simple framework will increase.

(b) A decrease in the future costs of girls because of *Bhagyalakshmi* will cause health investments in girls to change because of two competing effects. First, the direct income effect of the program will lead to an increase in period 2 utility and thus increase the willingness to invest in girls. Second, there will be an indirect effect through the effect of *Bhagyalakshmi* on total fertility. From part (a) of this proposition, *Bhagyalakshmi* increases total fertility, however, from proposition 1 we also know that fertility and investments in girls are viewed as substitutes. Thus, the fertility increase under *Bhagyalakshmi* will indirectly reduce investments in girls. Thus, a reduction in the actual future costs of girls can increase health investments in girls if the direct income effect dominates, but will decrease investments if the indirect effect dominates. It is useful to think about the types of households for which these effects dominate. For households for whom the decline in future costs is very large, the direct income effect is expected to dominate over the indirect effect and therefore there would be an increase in health investments in girls in these households. For wealthier households, the indirect effect is expected to dominate. Then, if the poor are a sufficiently large part of the population, average investments in girls will increase.

Proposition 3: *Given a fixed total number of children (i.e. fixed N); if the exoge-*

nous decrease in future costs of girls is large enough then, the incentive to engage in son-biased fertility stopping behaviour falls. An exogenous decrease in G will lead to parents with relatively more girls having similar incentives to continue having more children as those with relatively more boys. That is, $\frac{\partial EU_{T,HH1}}{\partial N} \approx \frac{\partial EU_{T,HH2}}{\partial N}$ *after the introduction of the Bhagyalakshmi program*

Before the introduction of the *Bhagyalakshmi* program, couples with more girls had an incentive to continue having more children (see Rosenblum [2013] for more details). After the introduction of the *Bhagyalakshmi* program, for a household with a higher proportion of daughters, an *additional* daughter is no longer costly and they might invest equally in the extra girl or the extra boy. While high-daughter-proportioned households might still value an extra son since they still have to ameliorate the future costs of daughters born before the program; those girls born after the introduction of the program might *also* be valued. Thus, high-daughter-proportioned households now have a weaker incentive to continue having children apart from those households that have zero boys since they would want at least one boy to reduce the future burden from girls born before the program. More generally, if the future costs of girls goes down, then the relationship between the number of children one has and the choice of having another child is no longer dependent on the existing *proportion* of children of a particular gender. It then reduces to a simple quantity-quality trade-off independent of gender.

Proposition 4: *If the household size effect dominates, then an exogenous decrease in the future costs of girls will unambiguously lead to a decline in sex-selective abor-*

tions. If the income effect dominates, selective abortions will decline iff the reduction in G is large enough

After the introduction of the *Bhagyalakshmi* program, if couples decide to have a girl then, a decrease in future costs of girls, G because of the *Bhagyalakshmi* program will lead to an increase in period 2 marginal utility from consumption. Period 1 marginal utility can increase or decrease. It will increase if the health investments in girls, k_g decrease when G decreases. However, if health investments increase when G decreases then period 1 marginal utility will also decrease. On average then, the direction of change in period 1 marginal utility depends on the direction of the change in k_g for each household.

For households for whom the indirect effect through changes in fertility dominates, k_g will decrease leading to an increase in period 1 marginal utility. These households will thus choose pregnancy without sex determination after the introduction of the program i.e. the willingness to abort a female fetus should decline. This is because for such households a decrease in G leads to an increase in period 1 *and* an increase in period 2 utility. For households for whom the income effect dominates, k_g will increase leading to a decrease in period 1 marginal utility. If the reduction in G is large enough and the increase in period 2 marginal utility compensates for the reduction in period 1 utility, then these households should also see a decrease in the willingness to abort after the program.

1.3 Context

1.3.1 The *Bhagyalakshmi* Program in Karnataka

The *Bhagyalakshmi* program, which is the focus of this study, was introduced by the Government of Karnataka in March, 2006.[13] Karnataka is one of the richest states in India, with a GDP per capita of Rs. 143,305 ($2223) compared to a national average of Rs. 104,820 ($1626) in 2015-16. However, girls face discrimination both at birth and during early childhood. Infant mortality rates for Karnataka are higher on average than the control states in this study. More specifically, in 2005 the female infant mortality rate in Karnataka was 50 deaths per 1000 live births while the corresponding figure for the control states was 37 (Sample Registration System, 2014).[14] Further, child marriage rates in Karnataka are among the highest in the country. According to the National Family Health Survey (NFHS) 2005-06, 45% of women are married before the age of 18 years.[15] Son preference is also high with only 3-5% women wanting more daughters than sons (NFHS 2005-06). High son preference is also reflected in the increasing trend in the number of sex-selective and other abortions between the late 1990s and early 2000s in Figure A.1.[16]

In this context, the *Bhagyalakshmi* program was introduced to promote the

[13] In the 2015-2016 budget, the program was allocated about 0.3% of the state budget or about $73 million.

[14] Tamilnadu, Kerala, Maharashtra, Andhra Pradesh, and West Bengal are used as control states in this study. Section 1.3.3 provides more details about the control states.

[15] This is comparable to rates in other 'high' son preference states such as Bihar (60%), Rajasthan (50%), Uttar Pradesh (53%) and Madhya Pradesh (48%).

[16] While the southern states in the country fare better than the north both in terms of sex ratios at birth and child mortality, son preference is a massive problem even in the south. Early work on regional variation in sex ratios in India has tended to focus on the divide between the north and the south (see e.g. Sopher, 1980; Miller, 1981; Dyson and Moore, 1983). However more recent work based on Census data and state level surveys has revealed that this divide is no longer valid, with southern states like Tamilnadu and Karnataka witnessing declining sex ratios (see e.g. Agnihotri, 2003 and Srinivasan and Bedi, 2008).

birth of girl children. Couples are given a long-term savings bond at the birth of a girl. The bond is in the name of the child and is redeemable by the unmarried daughter once she turns 18. Enrollment is allowed up to one year after the birth of the child. In addition, interim payments such as scholarship and insurance benefits are made available to the beneficiary on continued fulfillment of the eligibility criteria outlined in the scheme.[17] Scholarship payments increase by grade level with Rs. 300 ($5) being paid for enrolling in grade 3 to Rs. 1000 ($15) for enrolling in grade 10. If the girl child falls sick, medical insurance up to a maximum of Rs. 25,000 ($375) is also provided. If a natural death or an accident of the insured person takes place, the family receives an insurance amount but does not receive the benefits of the program.

The program benefits are only applicable for births after March 2006. Incentives are restricted to two girls per family and couples can have a maximum of 3 children, including the beneficiary child. For couples who have three children at the time of enrollment, the benefit can be availed only if the couple has adopted a terminal method of family planning, so that, the total number of children per family does not exceed three. For couples with less than three children at the time of enrollment, a family planning certificate is not mandatory with the application. However, in order to obtain the maturity value of the bond after 18 years, couples will be required to furnish a family planning certificate at that time. An audit by the Department of Women and Child Development in 2013 found that in about 95% of

[17] These include (a) the child should be immunized as per the program of the Health Department (b) the child should be enrolled in the Anganwadi centre (c) the child should take admission in a school recognized by the Education Department (d) the child should not to become a child labourer (e) the child should not to marry until the age of 18 years, and (f) couples with less than three children at the time of enrollment should produce a family planning certificate at the time of maturity of the bond.

three-child couples submitted family planning certificates with the application. This implies that verification of this condition is high for three-child couples at the time of enrollment and is thus, likely to be high for other couples who have to furnish this certificate at the end of 18 years. Additionally, since 2010 the application process is completely online which automatically rejects an application if all the required certificates are not produced.[18]

The present discounted value of the incentive received for the first beneficiary girl is Rs. 35,042 ($543) and for the second beneficiary girl is Rs. 34,832 ($540).[19] For comparison, the average annual per capita expenditure in Karnataka in 2004-05 was about Rs. 16,800 ($240) in rural areas and Rs. 33,600 ($550) in urban areas (National Sample Survey, NSS estimates).

The *Bhagyalakshmi* program differs from traditional programs in two important ways. First, the amount of the long-term savings bond is large and is enough to cover almost all of average schooling expenses over the course of a girl's lifetime. According to the National Sample Survey (NSS 2007-08) rural households spend an average of Rs. 6000 ($90) per child on all levels of education in a year. Averaging over the entire schooling period of 12 years and taking the present discounted value implies that the PDV of education expenditure for a family is about Rs. 35,000

[18] One could argue that three-child couples could conceal the total number of children they have at the time of enrollment in order to avoid having to furnish the family planning certificate. However, it is difficult for couples to conceal the total number of children they have since the state department verifies the number of surviving children in a family during enrollment. Allotment of the bond to eligible children is made after due verification of fulfillment of the eligibility conditions by the concerned government department. In this sense, couples with more than three children are unlikely to be able to fool government officials into believing that they are eligible and have less than three children. In any case, couples who claim to have less than 3 children at enrollment will need to submit a family planning certificate in the future to obtain the bond value.

[19] After enrollment and due verification by the concerned government department, an amount of Rs. 100,052 ($1500) is available at the end of 18 years for the first girl beneficiary in the family and Rs. 100,037 ($1490) is available for the second girl beneficiary. These are the revised amounts from 2008 onwards. In 2006-07, the amounts were Rs. 34,751 ($532) for the first girl beneficiary and Rs. 40,918 ($628) for the second girl beneficiary.

($545). The present discounted value of the bond of $543 is almost equal to this amount. Alternatively, total benefits received under the program at the end of 18 years of Rs. 100,052 ($1500) are enough to cover average marital and dowry expenditures as well.[20] Interestingly, anecdotal evidence and newspaper reports suggest that some parents plan on using the final bond value to help their girl children study further and finish post-graduate education, as opposed to using the money for dowry payments.

The second way in which the *Bhagyalakshmi* program is different from other programs is that benefits accrued under the scheme do not go to the parents at regular intervals like other cash transfers, but are essentially a lump sum at the end of 18 years. Since the benefits are only available once the child turns 18, this is a more suitable program to study health investments in children since the intended beneficiary is the girl child as opposed to the parents. Any program that directly gives payments to parents is unlikely to be spent on the intended girl beneficiary.[21]

1.3.2 Program Scope and Take Up

The main feature of the *Bhagyalakshmi* program is the long-term nature of the bearer security. In order for families to change their behaviours because of the program they must first believe that the government will follow-through on the payment of the bond value in the future. There are at least three pieces of

[20] According to the Indian Human Development Survey (IHDS), in 2004-05 average wedding expenditures were about Rs. 90,000 ($1360) for the bride's family. Even among households in the lowest income quintile, the expenditure for the bride's family was about Rs. 64,000 ($1100). In addition to wedding expenses, gifts of large consumer durables in dowry seem to be quite prevalent. Average cash equivalent of dowry is about Rs. 25,000 ($380).

[21] There is a vast literature on the how cash transfers targeted to men are spent differently than transfers given to women. A number of papers test whether children in households where the recipient of the transfer is a woman have better outcomes (see e.g. Duflo, 2003; Case, 2004; Paxson and Schady, 2007; Gertler, 2004; Rivera et al., 2004).

evidence that show that this might indeed be the case. First, according to data from the Ministry of Women and Child Development in Karnataka, from 2006-07 to 2010-11 an amount of Rs. 13.78 billion (roughly $215 million) has been incurred in the distribution of the bonds to about 13,18,000 beneficiary girls. Total *girl* births in this 4-year period in the state were approximately 2 million. Assuming all the girls born during this period were eligible, this implies a 60 percent take-up rate.[22] Moreover, the budgetary allocation to the program has increased over the years from about $50 million in 2010-11 to about $73 million in 2015-16, once again indicating high take-up.[23] Second, the bond certificate is issued to parents by the Life Insurance Corporation (LIC) of India after verification of eligibility conditions.[24] In this sense, the bond cannot be revoked irrespective of changes in the state government. Additionally, LIC is one of the most trusted insurance companies in India. Finally, *as long as* the girl for whom the bond has been issued meets all her eligibility conditions such as immunization, school enrollment, being unmarried until 18 years and production of family planning certificate after 3 children, parents are guaranteed to receive the bond value at the end of 18 years.

[22] This is obtained by dividing the total number of births by the number of beneficiaries i.e. $\frac{13,18,000}{2,000,000}$.

[23] While one could argue that if parents have very high discount rates, then a payment of an amount eighteen years in the future may be worth little when the child is young. However, the extent to which parents discount the future depends on the certainty with which they believe they'll receive the benefits. Given the increasing outlay of expenditure towards the program and the high take up rate, it appears that there is a fair amount of trust amongst families about the bond.

[24] Life Insurance Corporation of India is an Indian central government-owned insurance group and investment company. It is the largest insurance company in India with an estimated asset value of $2500 trillion.

1.3.3 Control States

The *Bhagyalakshmi* program was introduced across the state of Karnataka in 2006. Hence, there is no intra-state variation that I can exploit while estimating the impact of the program on the outcomes of interest. Since Karnataka is a state in the southern part of the country I use other southern and central states as control states. Thus, the control states in this study are: Tamilnadu, Kerala, Andhra Pradesh, West Bengal and Maharashtra. Indian states are very heterogeneous with respect to geography, demography, socio-economic characteristics and son preference. These differences manifest in differential fertility preference and differential investment behaviour.[25] For instance, states in the northern and western part of the country witnessed a rapid decline in the female-to-male sex ratio from 1991 onwards after the advent of the ultrasound technology in India.[26] Figure 1.1 plots child sex ratio using data from the decadal Census. Karnataka and the control states seem to follow a similar pattern for both indicators. Recent literature examining programs at the state level in India have adopted a similar approach (see e.g. Nandi and Deolalikar, 2013; Anukriti, 2014 and Stopnitzky, 2012).

1.4 Data and Descriptive Evidence

In this study, I examine the impact of changes in the actual future costs of girls on fertility and investment choices in the present, by using the introduction

[25] Inter-state heterogeneity in India is well documented. For instance, Carranza [2014] finds that soil texture explains a large part of the variation in women's relative participation in agriculture and in infant sex ratios across districts in India. Other literature documenting heterogeneity across states includes: Rahman and Rao [2004]; Dyson and Moore [1983]; Chaudhuri [2012]; Bhaskar and Gupta [2007]; Sudha and Rajan [1999].

[26] The worsening of the child sex ratio after 1991, especially in northern India, is mostly due to the diffusion of prenatal sex-selection techniques (Sekher, 2012; Bhat, 2002).

of the *Bhagyalakshmi* program in a difference-in-differences and triple difference framework. I will compare geographic units within Karnataka to their counterparts in the control states, before and after 2006. I first start by examining the impact on child sex ratio at the most disaggregated level using the Census dataset and then decompose these impacts into impacts on girl deficit at birth and girl deficit post-birth using data at the individual level. More specifically, this paper uses two main sources of data: the Decennial Indian Census (3 rounds in 1991, 2001 and 2011) and the District Level Household Survey (DLHS: 3 rounds in 2002-04, 2007-08 and 2012-13).

1.4.1 Census of India

I begin my analysis by studying the impact of the *Bhagyalakshmi* program on child sex ratio using the census data. This is the highest level of disaggregation available in any data set in India. India has decennial census data dating back to 1951. In this study I use data from the 1991, 2001 and 2011 Censuses.[27] The census data is representative at the primary sampling unit (PSU) level, where the PSU in a rural area is a village and the PSU in an urban area is a town. More specifically, urban areas (i.e. towns) and rural areas (i.e. villages) partition the space of sampling units within a district in the census dataset.

There are two sources of the census data. The first is the Primary Census Abstract (PCA). The PCA in each census year provides demographic information at the PSU level. The second source is the Census Directory (CD) which provides village level infrastructure information. In each census year, the PCA data are

[27] These data are available at `http://www.censusindia.gov.in`.

merged with the Census Directory (CD). PSUs from the three census years, 1991, 2001 and 2011 are then matched, creating a three wave PSU-level panel dataset.[28]

In the analysis that employs the Census data, I analyze two different subsamples. In Subsample I, all PSUs in Karnataka and control states are included in the analysis. This subsample consists of 25,044 PSUs and 104,641 PSUs. In Subsample II, only PSUs in the districts of Karnataka that share a common border with districts in the control states are included in the analysis. This subsample consists of 13,590 PSUs and 9625 PSUs.[29,30] Descriptive statistics (pre-period means in 1991 and 2001) for the combined sample (Subsample I) are presented in Table A.1 for PSUs in Karnataka and the control states. There is a declining trend in the *mean* PSU level child sex ratio for both Karnataka and control states from the 1991 to 2001 census years. Finally, PSUs in Karnataka and control states seem to be similar with respect to other demographic characteristics in 1991 and 2001.

1.4.2 District Level Household Survey

In the second part of my analysis I decompose the estimated effects on child sex ratio into impacts on girl deficit at birth which I measure through the sex ratio at birth and impacts on girl deficit after birth which I examine through impacts on infant mortality and post-natal health investments measured by exclusive breast-

[28]The total number of PSUs in 1991 is smaller than in 2001 since many PSUs split between 1991 and 2001. Of the total PSUs in 1991, 96.1% PSUs are matched across all census years.

[29]Focusing on districts of Karnataka that share a common border (i.e. geographical neighbours) with districts in control states in Subsample II has the advantage that PSUs in these districts are most likely to be very similar to each other with respect to demographics and other characteristics. It is expected that there would be fewer differences in omitted variables, if any, in this sample of geographic neighbors than in Subsample I.

[30]Inter-state migration in India is low ranging from 0.1% (rural to rural migration) to 0.11% (rural to urban migration). See e.g. Munshi and Rosenzweig, 2009. Thus, concerns of spillover effects while analyzing border districts, are unlikely.

feeding.

For this purpose, I use three rounds of the District Level Household Survey (DLHS) conducted in 2002-04 (DLHS-2), 2007-08 (DLHS-3) and 2012-13 (DLHS-4).[31] These surveys are representative at the district level and are modeled on the Demographic Health Surveys (DHS) and collect limited fertility histories from women of reproductive ages (15-49 years). The DLHS-2, 3, 4 surveys include demographic questions about the woman and also include information on child health.[32]

For examining the impact of the *Bhagyalakshmi* program on girl deficit *at* birth, I use a woman level panel and I restrict my sample to women between the ages 15-44 years for comparability across surveys.[33] For the fertility analysis the pre-treatment period includes data from 1999-2006 and post-treatment data from 2007 to 2012.[34] Despite the program being applicable for all births after 31st March, 2006 I do not include births in 2006 as part of the post-program period for analyzing outcomes in the woman level analysis. This is because births in this time period were conceived before the program was announced and hence decisions regarding these births were made by couples in the pre-program period.

For examining the impact of the *Bhagyalakshmi* program on girl deficit *after*

[31]The second round of the DLHS (DLHS-2) interviewed 620,107 households (about 1000 in each of 593 districts) in India between 2002 and 2004 using multistage stratified sampling. The third round of the DLHS (DLHS-3) interviewed 720,320 households (1000 to 1500 from each of 611 districts) between late 2007 and early 2008 following a multistage stratified sampling method. The fourth round of the DLHS (DLHS-4) interviewed 391,772 households (100-1750 from each of 336 districts) between 2012 and 2013.

[32]Two rounds of the DLHS, DLHS-3 (2007-08) and DLHS-4 (2012-13), do not collect complete fertility histories. The survey in 2007-08 only collects information for all births after January 1, 2004 and the survey in 2012-13 collects information for all births after January 1, 2008.

[33]The DLHS-3, 4 were administered to women in the 15-49 age group while the DLHS-2 was administered to married women between the ages of 15-44 years.

[34]I exclude births from 2013 since 2013 has incomplete annual birth history data. Therefore, the annual birth history of most interviewed women during 2013 is incomplete. In particular, the lowest number of births reported in DLHS-4 data was from 2013. To avoid idiosyncrasies due to incomplete birth history reporting, all births from 2013 are excluded from the analysis.

birth I use a child level analysis which pools births across the three cross sections. I exclude twins or multiple births since it is difficult to assign birth order to such births.[35] I also include only children of birth orders 1, 2 and 3 since the *Bhagyalakshmi* program was only applicable to girls of these birth orders. My sample period for the child level analysis comprises of the pre-treatment period which includes data from 1995-2005 and post-treatment data which includes data from 2006 to 2012.[36]

I present pre-period descriptive statistics for the woman and child level samples in Tables A.2 and A.3, by Karnataka and control states. For both the samples, individuals seem to be similar with respect to demographic characteristics in the pre-program period. In Table A.3, in the pre-program period girls in Karnataka were 0.12 percentage points more likely to die in the first year of life than boys. Additionally, the gap in exclusive breastfeeding duration between girls and boys in Karnataka in the pre-program period is about 0.2 months. The final sample of mothers for the fertility analysis consists of 280,896 mothers in Karnataka and control states combined; while the final sample of children for the child level analysis consists of 201,230 children of birth order 1, 2 and 3 born over the entire pre and post program periods.

Finally, in order to ensure that there are no idiosyncrasies in the outcome variables due to sampling differences generated by pooling three different repeated cross-sectional datasets, I examine the trends in one of the outcome variables of interest, by survey round. In Figure A.2, I examine the annual probability of birth (only for control states) for each survey round separately and find no significant

[35]The percentage of the sample which has twins or multiple births is as follows: DLHS 2 (1.39%); DLHS 3 (1.40%); DLHS 4 (1.65%).

[36]For the child level analysis, I include all births after 31st March, 2006 as part of the *post* period since children born after this date are impacted by the program in terms of health investments before their first birthday.

jumps between survey rounds in the outcome variable.

1.5 *Bhagyalakshmi* and Child Sex Ratio

1.5.1 Identification Strategy

I first measure the impact of the *Bhagyalakshmi* program on the child sex ratio by exploiting spatial and temporal variation in program exposure in a difference-in-differences framework. This part of the analysis will employ the census data. Spatial variation arises because the program was only available in the state of Karnataka. Temporal variation stems from the timing of the program: the program was applicable to all births after March 31st, 2006. To examine the impact on the child sex ratio I use data from three decennial censuses of 1991, 2001 and 2011.

I define a binary treatment equal to one for Karnataka and zero for control states. I then consider the changes in outcomes in the treated state following the introduction of the program relative to changes in control states. My main estimating equation using the census data is of the following form:

$$Y_{jt} = \beta Program_{jt} + \gamma X_{jt} + \alpha_j + \tau_t + \delta_s * t + \epsilon_{jt} \tag{1.2}$$

where Y_{jt} reflects the child sex ratio in PSU j at time t=1991, 2001 or 2011. $Program_{jt}$ is a dummy variable if the program was in place in PSU j at time t. Thus for PSUs in control states this variable is 0 in all three time periods while for PSUs in Karnataka it takes a value of 1 in 2011. X_{jt} are time varying vectors of infrastructure and demographic characteristics. Finally, α_j and τ_t represent PSU and year fixed effects, respectively. I also include state level time trends, $\delta_s * t$. State specific trends control for potentially heterogeneous pre-program trends. I cluster my standard errors by state since the program being analyzed was at the state level. To account for the small number of clusters and associated asymptotic inconsistency, I use wild bootstrapping to correct for this as proposed by Cameron et al. [2008]. The results are qualitatively identical. For simplicity, therefore, I use the cluster-robust standard errors in all estimations, while also reporting the *p-values* with Wild-t small-bootstrapping cluster procedure. Results are also robust to clustering at the district level. The coefficient β is the estimate of the *Bhagyalakshmi* program on child sex ratio. Finally, specifications with infrastructure and demographic controls only serve as robustness checks to the parsimonious specifications without controls.

The fundamental identifying assumption in this framework is that the time trends in the pre-program period in the child sex ratio across Karnataka and control states should be the same. Figure 1.1 demonstrates common trends in child sex ratios going back to 1981.

Even with the above assumption, the estimated impact may be due to other state level programs that are correlated to the introduction of the program. To this end, I follow Nandi and Deolalikar [2013] and control for a range of time varying infrastructure and demographic characteristics. Infrastructure characteristics, include the availability of: either a primary health centre or sub-centre; tap water; all weather road; power supply; primary school; middle school and secondary school. I also include male and female literacy and labour force participation rates.

1.5.2 *Bhagyalakshmi* and Child Sex Ratio Results

Table 1.1 column (1) presents the standard difference-in-differences estimates from equation 1.2 and includes only year and PSU fixed effects. Each coefficient represents the marginal impact of the *Bhagyalakshmi* program on the female-to-male child sex ratio. Results are presented for both subsamples: Panel A is the pooled sample which includes all PSUs in Karnataka and the control states while Panel B includes only PSUs in the border districts of Karnataka and control states. Column (2) includes state specific trends. In the last column I include time fixed effects, PSU fixed effects, state specific linear trends and PSU level controls.

The results suggest that, for the pooled sample in panel A, villages and towns in Karnataka show a significant improvement in the child sex ratio between 19.33 and 25.64 points. That is, there were about 19 to 26 additional girls in the 0-6 age group for every 1000 boys in Karnataka in the post-program period. For specifications in columns (2)-(3), the improvement is significant at the 1% level. The estimates are robust to including state specific linear trends. In panel B, a similar story holds for the subsample that includes only PSUs in the border districts. One would expect that PSUs in border districts should have fewer differences in omitted variables. I find that, even when the sample is restricted to the small set of geographically neighbouring PSUs there is still a positive impact of the program in Karnataka. In particular, the improvement in panel B ranges from 18.37 points to 31 points in the child sex ratio across different specifications. These estimates are larger than the estimates in my base specification in panel A which is to be expected since PSUs in border districts are likely to be more similar to each other. Thus, these results serve to confirm that my original results are not caused primarily by some unobserved characteristics. Finally, I present wild-bootstrap *p-values* in square brackets under each estimate in column (3) in Table 1.1.

Using the most conservative estimate of an improvement of 20 points in column (3) of Table 1.1 (i.e. 20 additional girls in the 0-6 age group for every 1000 boys) in the child sex ratio, my calculations imply that the *Bhagyalakshmi* program led to an *annual additional* 28,124 girls aged 0-6 years in Karnataka.[37] Total girls in the 0-6 age group in 2011 in Karnataka were approximately 3 million. This implies that, annually, the program led to about about 1-2 percent fewer girls, dying either due to sex-selective abortions or due to post-birth girl neglect, and thus surviving up to age 6.

The improvement in the child sex ratio is similar in magnitude to the changes in child sex ratio estimated by Nandi and Deolalikar [2013] for the implementation of the Pre-Conception and Pre-Natal Diagnostics Techniques Act (PNDT) of 1994. The authors estimate that child sex ratio improved by about 13-20 points as a result of the PNDT Act in states that implemented the Act. While the PNDT was implemented in several states and thus had a wider reach, the results in this study imply that even state level programs can have far reaching consequences.

Finally, I conduct a placebo regression of the following nature. In Table A.4 I evaluate the impact of a hypothetical policy which was introduced in 1996: 10 years before the program was actually implemented. I then use the 1991 and 2001 census datasets in a difference-in-differences framework, treating the 2001 census as

[37] I use the total population sizes in the age group 0-6 years for 2011 in Karnataka to convert the difference-in-differences estimate of 20 into a number of *annual additional* female children. Detailed calculations are in Appendix A.2.

the hypothetical '*post*' period. The coefficients for this hypothetical policy are small and insignificant.

These results suggest that the *Bhagyalakshmi* program led to large and significant improvements in the child sex ratio of approximately 20 girls for every 1000 boys. Next, I examine whether these changes came from changes in girl deficit at birth or changes in girl deficit after birth.

1.6 Decomposing the Impact on Child Sex Ratio: Fertility and Sex Ratio at Birth

In this section, I examine whether the relationship between the *Bhagyalakshmi* program and child sex ratio established in Section 1.5.2 stems from changes in fertility and sex ratio at birth.

1.6.1 Identification Strategy

The empirical specification analyzing the impact on fertility and sex ratio at birth investigates two questions: First, does the probability of a birth change as a result of a financial incentive? Second, what is the probability of a female birth? The first question examines the fertility response to the program while the second question examines the impact on the sex ratio at birth.

In order to answer the first question and examine the fertility response of the program, I examine if a woman's fertility is impacted as a result of introduction of the *Bhagyalakshmi* program. Additionally, I also examine two other dependent variables: the unconditional probability of a male birth and the unconditional probability of a female birth.

For the analysis, I use the retrospective birth histories drawn from the DLHS-2, DLHS-3 and DLHS-4 and construct a woman year panel.[38] Each woman enters the panel 5 years before the survey took place and exits the panel in the year of the survey. So for instance, for the DLHS-4 women enter the panel in 2008 and exit in 2012; for the DLHS-3 women enter the panel in 2004 and exit in 2007 and finally, for the DLHS-2 women enter the panel in 1999 and exit in 2003.[39] Thus the pre-policy period is 1999-2006 and the post-policy period is 2007-2012. The dependent variable is an indicator for giving birth for woman i living in state s in year t; gives birth to a girl in year t or gives birth to a boy in year t:

$$Y_{ist} = \alpha + \beta Kar_s * Post_t + \gamma X_{ist} + \nu_s + \tau_t + \delta_s * t + \epsilon_{ist} \tag{1.3}$$

where $Post_t$=1 for $t > 2006$; X_{ist} is a vector of time varying covariates that includes woman's years of schooling, indicators for woman's birth cohort, indicator for household religion, indicator for residence in a rural area, dummy for belonging to scheduled caste and a household standard of living (SLI) index. I also include state and year fixed effects, ν_s and τ_t respectively, and state level linear trends, $\delta_s * t$. The reference group consists of women in control states. In this specification, β captures the effect on the probability of a birth (and the unconditional probabilities of girl and boy births) in Karnataka relative to control states, after the introduction

[38]This is similar to the approach adopted by Alfano [2017] who examines the introduction of the Dowry Prevention Act in India on fertility choices.

[39]Since the DLHS-2 was conducted in 2002-2004, the annual birth history of most interviewed women during 2004 is incomplete. To avoid any idiosyncrasies due to incomplete birth reporting I exclude this year from the DLHS-2. Further, for the DLHS-3 women enter the panel four years before the survey; so from 2004-2007. This is because, for this survey round birth histories were only collected starting in 2004. Additionally, for the DLHS-4 women also enter the panel for five years, 2018-2012 since the birth history reporting is incomplete for 2013.

of the *Bhagyalakshmi* program. Standard errors are clustered by state and I also show the *p-values* with a Wild-t small-cluster bootstrapping procedure. Like before, specifications with individual level controls serve as robustness checks to the parsimonious specifications without controls.

In order to answer the second question and examine the impact on sex ratios at birth I perform the analysis at a more aggregated level. I perform a district-by-year analysis of the sex ratio at birth similar to the PSU level analysis performed in the last section for the child sex ratio. I calculate the weighted district averages of the proportion of female births for the years 1999 to 2012. That is, I model the sex ratio at birth by employing the district-wise *weighted* proportion of girl births. More specifically, I model the following:

$$Y_{dt} = \beta Program_{dt} + \gamma X_{dt} + \alpha_d + \tau_t + \delta_s * t + \epsilon_{dt} \tag{1.4}$$

This specification mirrors equation 1.2 except it is estimated at the district level as opposed to the PSU level. Here Y_{dt} reflects the sex ratio at birth in district d at time t=1999-2012 and is proxied by the weighted proportion of female births (using sampling weights). $Program_{dt}$ is a dummy variable if the program was in place in district d at time t. Thus for control districts this variable is 0 in all time periods while for districts in Karnataka it takes a value of 1 after 2006. X_{dt} are time varying covariates at the district level. These are the same covariates as mentioned above, except aggregated at the district level: years of schooling of women, proportion in a district living in a rural area, proportion belonging to scheduled caste, proportion hindus and proportion belonging to low, medium and high standard of living index.

Finally, α_d and τ_t represent district and year fixed effects, respectively. I also include state level time trends, $\delta_s * t$ like before. Specifications with district level controls serve as robustness checks to the parsimonious specifications without controls.

The main identifying assumption in the difference-in-differences framework which examines the impact on sex ratio at birth and fertility is that pre-program trends should be the same across Karnataka and control states. Figure 1.2 demonstrates common trends from 1999 to 2006 for the sex ratio at birth. Additionally, the probability of a birth should also exhibit similar trends before the introduction of the program. That is, women in Karnataka and control states should exhibit similar fertility patterns before 2006. In Figure 1.3 I present the probability of a birth for each year from 1999-2006.

1.6.2 Effect on Fertility and Sex Ratio at Birth

I first begin my analysis by examining the impact of the program on the probability of a birth, the unconditional probability of a female birth and the unconditional probability of a male birth. I estimate equation 1.3 in Table 1.2. In column (1) I include only state and year fixed effects. In column (2) I add state specific linear trends and in column (3) I include covariates. The difference-in-differences estimates examine the change in the probability of a birth in year t, before and after 2006 in Karnataka and control states. The dependent variable in Panel A is an indicator for giving birth for woman i living in state s in year t, while in Panels B and C the dependent variable is 1 if woman i living in state s gives birth in year t to a female and male respectively. At baseline on average, 12.64 percent of women gave birth every year, with about 48 percent of these being girls.

The parameter estimates in Panel A suggest that the probability of a marginal birth changed differentially between Karnataka and the control states in the post-program period by 0.77 to 0.85 percentage points (significant at the 5% level) which represents a 6% increase over the baseline mean. In Panel B, the unconditional probability of a female birth sees a significant increase between 0.51 to 0.53 percentage points (significant at the 1% level in column (3) Panel B). At baseline, on average, about 6 percent women give birth to girls. Thus, the *Bhagyalakshmi* program leads to an almost 9 percent increase in the unconditional probability of giving birth to a girl amongst women in Karnataka compared to women in control states.[40] In contrast, while it is positive, the unconditional probability of a male birth does not significantly change after the introduction of the *Bhagyalakshmi* program (columns (1) to (3) of Panel C).

By examining the unconditional probabilities of male and female births, I can estimate how much of the increase in female births is due to eliminated abortions and how much is driven by new pregnancies. New pregnancies should increase the number of girls and boys born more or less equally. Thus, if only new pregnancies were driving the increase in fertility (Panel A) then the estimates of the unconditional probabilities of male and female births (Panels B and C) should be roughly equal. However, the estimate of the unconditional probability of a female birth is much higher than that of a male birth, suggesting that in addition to new pregnancies there were also fewer sex-selective abortions. That is, about 50% of the increase in the number of female births is due to new pregnancies and 50% of the increase is due to reduction in selective abortions.[41] In effect, this means that the increase

[40]This is obtained by dividing the point estimate of 0.53 by the baseline mean of 6 percent.

[41]This is because, if the increase in the unconditional probability of female births is 0.52 p.p.

in the probability of a birth that is seen in Panel A of Table 1.2 is a result, *both*, of an increase in the willingness to conceive, and a decrease in the willingness to abort female fetuses. Finally, a joint F-test fails to reject that all pre-*Bhagyalakshmi* coefficients are equal to zero for all three dependent variables (p-values > 0.45).

In Table 1.3 where I estimate equation 1.4, I find that the weighted district level sex ratio at birth improves significantly in Karnataka after the introduction of the *Bhagyalakshmi* program. The increase in the sex ratio at birth is about 0.71 to 0.92 percentage points (columns (1) to (3) of Panel A). Taking the most conservative estimate of 0.71 percentage points, this translates into an increase of approximately 13 girls for every 1000 boys.[42] If the increase in female births was *only* because of new pregnancies then the sex ratio at birth should not have changed. This is because, new pregnancies would have resulted in equal numbers of boys and girls, leaving the sex ratio unchanged. However, as pointed out above, about 50% of the increase in female births is due to fewer abortions, which therefore led to an improvement in the sex ratio at birth.

The estimated coefficients suggest large and significant impacts on the sex ratio at birth. The estimated impact on the sex ratio at birth is larger in magnitude to those found by Srinivasan and Bedi [2008] who find no impacts of two policy interventions in the state of Tamilnadu in the early 1990s, on the sex ratio at birth. One way to reconcile their finding of no impact on sex-ratio at birth is to examine the uptake of the program in Tamilnadu. The program was launched in 1992 and between 1992 -1997 only approximately 2000 families benefited from

(column (2), Panel B) then about 0.26 p.p. or 50% of this is attributable to new pregnancies since 0.26 p.p. is the change in the unconditional probability of a male birth (column (2), Panel C).

[42]Detailed calculations in Appendix A.2.

the scheme. This, in addition to the change of government in 1996 led to the program being placed on the back burner (Srinivasan and Bedi, 2008). Sinha and Yoong [2009] examine another program in the north Indian state of Haryana; the *Apni Beti, Apna Dhan* program. They examine impacts on sex ratio of *living* children of each mother. While this conflates estimates on sex ratio at birth and differential female mortality after birth and thus comparisons are difficult, their estimates are still indicative. They find positive impacts of the program on the sex ratio of living children of about 2.3 percentage points. However, this is their estimated impact on child sex ratio. In comparison, my estimates of child sex ratio are larger in magnitude. This is potentially because of the high take up rate of the program in Karnataka as compared to the program in Haryana. Anukriti [2014] who examines another program also in Haryana, *Devirupak* finds that sex ratios at birth actually declined as a result of the program. The author suggests that this is because the program offered incentives to both one boy and one girl couples and this led to skewed sex ratios among couples with strong son preference. This implies that the design of any such incentive is crucial since small changes in the incentive structure can impact fertility preferences very differently. Finally, my finding of large improvements in the sex ratio at birth are also comparable to Qian [2008] who finds that an increase of adult female labour income by USD 7.7 (10% of average rural household income in China) increases the number of surviving girls by 1 percentage-point on average.

1.6.3 Effect on Son-Biased Fertility Stopping Behaviour

Son-biased fertility stopping behaviour is characterized by parents continuing to have children till they conceive a son. One of the foremost manifestations of son-biased fertility stopping behaviour is that women with a first-born daughter have higher fertility rates than women with first-born sons. This is because women with first-born girls continue to have children till they get a son, thus leading to higher fertility rates for such women.

To the extent that *Bhagyalakshmi* reduces the future costs of daughters and consequently the desire to balance the gender composition of one's children, it is expected that couples would engage in less son-biased fertility stopping behaviour. Thus, I investigate the impact of the program on son-biased fertility stopping behaviour by estimating a variant of equation 1.3 in a triple difference framework, with the sex of the first born child as the third source of variation. Previous research from India has demonstrated the exogeneity of the gender of the first born child if fertility is greater than one (see e.g. Bhalotra and Cochrane [2010]; Rosenblum [2013]; Visaria [2005] and Das et al. [2005]).[43] More specifically, I examine the following specification where I compare fertility outcomes for women with first born sons versus women with first born daughters:

$$Y_{ist} = \alpha + \beta Kar_s * Post_t * FirstGirl_i + \gamma_1 Kar_s * FirstGirl_i + \gamma_2 FirstGirl_i + \omega_t FirstGirl_i + \\ + \delta X_{ist} + \pi_{st} + \epsilon_{ist} \quad (1.5)$$

[43] See Figures 1-4 of Bhalotra and Cochrane [2010] for evidence on the randomness of the gender of the first born child.

The dependent variable Y_{ist} is the same as before: an indicator if woman i, in state s gave birth in year t. $FirstGirl_i$ is 1 if the gender of the first born child is female. I control for state-by-year fixed effects, π_{st} and allow year fixed effects to vary by gender of the first born child, $\omega_t FirstGirl_i$. All the other variables are defined as before. Finally, I only include women who have ever given birth since this specification can be estimated for only those women who had at least one birth.

Table 1.4 presents the estimates from equation 1.5. Column (1) only includes state and year effects, while column (2) includes state by year fixed effects and year fixed effects that vary by gender of first child. Finally in column (3) I include covariates. At baseline, before the introduction of the program, women in Karnataka with a first born girl were 0.40 percentage points more likely to have another birth compared to women in control states and to women with first born boys. This baseline gap is however, not statistically significant, suggesting that women with first born girls were more likely to have another birth, *both* in Karnataka and control states, compared to women with first born boys.

However, after the introduction of the program in Karnataka, women with first born girls were about 1.4 percentage points *less likely* to have another birth (coefficient on *Karnataka*Post*First Girl*). This is statistically significant at the 1% level. This implies that, while in the pre-program period son-biased fertility stopping behaviour was characteristic of both Karnataka and control states; in the post-program period, Karnataka witnessed a reduction in such behaviour. The results from this section and the previous section on sex ratio at birth, demonstrate how the *Bhagyalakshmi* program simultaneously impacted selective abortions and son-biased fertility stopping. I find that not only are couples with a first born girl less likely to want to have another child, but they're also increasingly indifferent about the gender of that child.

1.6.4 Did The Desired Fertility Change?

One of the mechanisms through which the *Bhagyalakshmi* program could have changed the willingness to abort female fetuses is by changing a couples' *desired* or *ideal* number of girls. The questions used are: *Would you like to have another child or would you prefer not to have any more children? Would you prefer your next child to be a girl or boy?* I construct variables for ideal number of boys, ideal number of girls and ideal number of children using responses on these questions in addition to information on current composition of a woman's children.

I construct a similar difference-in-differences specification to the one in equation 1.3, except each woman contributes one observation for the survey year in which she appears as opposed to 5 observations. The dependent variables I examine include: ideal number of girls, ideal number of boys and the probability of a woman expressing the desire for her next child to be a girl.

I first examine if the trends in the ideal number of girls and ideal number of boys are similar between Karnataka and the control states in the pre-program period. I present this in Table A.5. Since I have only one pre-program survey round in my sample (i.e. DLHS-2), I use the National Family Health Survey (NFHS 2005-06) to show pre-program trends. I next present the regression estimates in Table 1.5. The program led to a significant increase in the ideal number of girls (panel B), while the number of ideal boys does not significantly change (panel C). More specifically, the ideal number of girls increased by about 9.5 percent over the baseline ideal number of girls.[44] Finally, the probability of desiring one's next child to be a girl increases by 2.5 percent, which represents a 22 percent increase over the baseline mean of 11 percent. (panel D).

The results on desired fertility are in line with those found in Section 1.6.2 on actual fertility. That is, the program led to an increase in the desired number of girls and no change in the desired number of boys. This, in conjunction with an increase in the actual probability of female births (which was larger than the change in male births) implies that the actual *and* the desired willingness to abort female fetuses declined. Additionally, from Figure A.3 it appears that the percentage of daughters desired at different family sizes follows a monotonic relationship in Karnataka after the introduction of the program. That is, after the introduction of the program, even at an actual family size of 1, the percentage of daughters desired is high. That is, couples desire about 0.8 girls at a family size of 1 after the program compared to 0.5 before the program.[45] This is in stark contrast to Jayachandran [2017] who finds that at smaller family sizes, the desired number of sons is extremely high. For instance, she finds that for a hypothetical family size of 1, 84.9 percent of respondents in her sample, would want this one child to be a son. This is because when faced with lower fertility, couples desire at least one son because of high son preference. My finding that, even at a family size of 1, the desired number of girls actually *increases* after the program suggests that the program worked through decreasing this innate son preference. After the introduction of the program, parents were increasingly indifferent about the gender of their child, even at smaller family sizes.

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[44]Percent increase is calculated by dividing the coefficient in column (2), Panel B by the baseline mean.

[45]Figure A.3 shows the relationship only for mother's in their prime childbearing years, 25-34 years. The results are similar for all other age groups as well.

While this is suggestive evidence of a reduction in son preference it doesn't necessarily tell us if post-natal discrimination against girls ended. This is what I examine in Section 1.7.

1.6.5 Robustness

I test the validity of my findings through two placebo tests. First, I run a falsification test that examines the impact of hypothetical programs introduced for each year from 1997 to 2002, on the probability of a birth, unconditional probability of a female birth and the unconditional probability of a male birth. Results are presented in Table A.6. I expand my original sample to span the period 1995-2005. Thus, for instance, *Post1997* (column (1) of Table A.6) examines a program introduced in 1997 with 1995-1997 as the pre-period and 1998-2005 as the post-period and *Post2003* (column (6) of Table A.6) examines a program introduced in 2002 with 1995-2002 as the pre-period and 2003-05 as the post-period.

The second robustness check re-estimates my main specification (equation 1.3) after dropping one control state at a time. I present the results of this robustness check in Table A.7.

1.7 Decomposing the Impact on Child Sex Ratio: Excess Female Mortality and Health Investments

In this section I examine the impact of the *Bhagyalakshmi* program on post-birth neglect measured by infant mortality and duration of exclusive breastfeeding. Exclusive breastfeeding is defined as the number of months a child is exclusively breastfed which implies no food or water during this period. Early and exclusive breastfeeding is an important investment in children, helping them survive longer and also supporting cognitive performance (World Health Organization, 2000).[46]

1.7.1 Empirical Strategy

To test the impact of the program on infant mortality and health investments I perform a cohort analysis. I compare cohorts born after the introduction of the program in Karnataka to cohorts born before the program and to cohorts in control states. I use the gender of the recipient as the third source of variation in a triple difference framework. According to the eligibility conditions, the program was applicable only for female children. Thus, I use male children as an additional control group. Concerns over treatment endogeneity are eased by including an additional control group that is also affected by the same time-varying state level variables. Since only girls of birth orders 1,2 and 3 were eligible for the program, I estimate the following specification for girls and boys of birth orders 1, 2 and 3. The 'control group' constitutes (i) cohorts born before 2006 (ii) cohorts born in any year in the control states and, (iii) boy cohorts. For child i of mother j in state s in year t, I estimate the following:

$$\begin{aligned} Y_{ijst} = \alpha + \beta Kar_s * Post_t * F + \gamma_1 Kar_s * F_i + \gamma_2 F_i + \omega_t F_i + \psi_b F_i \\ + \delta X_{ijst} + \rho_{bt} + \nu_{bs} + \pi_{st} + \epsilon_{ijst} \end{aligned} \quad (1.6)$$

[46] Jayachandran and Kuziemko [2011] provide evidence that in India, since breastfeeding acts as a natural contraceptive, women tend to breastfeed girls for fewer months. This is because, after the birth of a girl women try to get pregnant sooner, thus reducing the number of months of exclusive breastfeeding that a girl receives.

The dependent variable, Y_{ijst} is either an infant mortality indicator or measures the duration of exclusive breastfeeding. F_i is indicator for being a female and can be 0 or 1. $Post_t$ is equal to 1 if the child is born after March 31st, 2006 and 0 otherwise. Specifications with individual level controls serve as robustness checks to the parsimonious specifications without controls.

A key advantage of this approach is that it allows me to account for state specific shocks over the observation period through state-by-year fixed effects, π_{st}. Additionally I control for birth order specific time effects, ρ_{bt} and state specific birth order fixed effects, ν_{bs}. I also allow birth year, and birth order fixed effects to vary by child gender ($\omega_t F_i$, $\psi_b F_i$). Fixed effects for state and birth year are also included. The coefficient of interest is β which measures the effect of the program on the outcome of interest for female children of birth orders 1, 2, and 3 before and after 2006 in Karnataka, relative to other states.

Identification in the triple difference framework relies on the assumption that the differences in female and male infant mortality (which will henceforth be referred to as excess female infant mortality or EFM) and differences in female and male breastfeeding duration should follow a similar trend in Karnataka and control states before the introduction of the *Bhagyalakshmi* program. Figure 1.4 Panel A depicts the differential trend in excess female infant mortality (EFM) between Karnataka and control states, while Panel B depicts the differential trend for exclusive breastfeeding duration. The gap in EFM between Karnataka and control states is relatively constant before 2006 and the same story holds for exclusive breastfeeding duration providing credibility to the identification strategy.

1.7.2 Compositional vs. Causal Effects

Since there were changes in overall fertility, it is possible that I'm identifying compositional as opposed to causal effects. To disentangle compositional and causal effects, I estimate a specification similar to equation 1.6 where the dependent variables are the family characteristics.[47]

1.7.3 Impacts on Infant Mortality and Health Investments: Triple Difference Estimator

Table 1.6 presents estimates from equation 1.6. Panel A reports estimates for infant mortality while Panel B presents estimates for exclusive breastfeeding duration. Finally in Panel C the dependent variable is a dummy if the child is exclusively breastfed for at least 6 months and is restricted to children who were at least 6 months of age.

In column (1) I include the triple- and double-interactions of *Female* and *Karnataka* with *Post*. Column (2), adds state by birth year fixed effects, birth year fixed effects that vary by child gender, and covariates. Finally, in column (3) I add birth order by state fixed effects, birth order by birth year fixed effects, as well as birth order fixed effects that vary by child gender.[48]

[47] I do not include mother fixed effects in the estimating equations to account for selection as has been done by recent literature (see e.g. Anukriti et al., 2016). This is due to several reasons. First, in a mother fixed effects model, siblings of the opposite sex but in the same household are compared to each other. Given that, siblings of the opposite gender are not randomly selected within a household because of son-biased fertility stopping rules and sex-selective abortions, this would introduce bias. Additionally, given that health investment information in the data is available only for children born three years prior to the survey date, a mother fixed effects model would generate estimates for a selected sample of households (i.e. those who had births within the last three years of a survey) (Hu and Schlosser, 2015).

[48] In columns (2) and (3) I exclude the double interaction terms: *Karnataka*Post* and *Female*Post*.

In the second row of Panel A, the coefficients are positive and significant. Thus, before 2006, the likelihood of girls of birth orders 1, 2 and 3 dying was higher than boys of the same birth orders by 0.16 percentage points (column 3) in Karnataka compared to control states. The triple interaction coefficient, *Karnataka*Post*Female* in the first row of Panel A indicates that excess female infant mortality for children in Karnataka was 0.13 percentage points (in column 3) lower than for control states during the post-program period. This estimate indicates an 81 percent decline in the excess female infant mortality gap between Karnataka and control states relative to the baseline gap of 0.16 percentage points.[49,50]

In terms of post-natal health investments, the coefficients in Panel B, show that in the pre-program period girl children of birth orders 1, 2 and 3 were exclusively breastfed for a significantly shorter time period.[51] That is, girls were less likely to be breastfed by about 0.13 months in Karnataka as compared to the control states. The triple interaction terms, *Karnataka*Female*Post* indicate that in the post-program period girls were significantly *more* likely to be exclusively breastfed than boys in Karnataka as compared to the control states for about 0.17 to 0.20 months. This increase is however quite small in magnitude implying that the program did not have a large effect on the intensive margin i.e. with respect to duration of exclusive breastfeeding.

In Panel C, I examine the impact of the program on the extensive margin by

[49] The excess female infant mortality in the pre-program period for Karnataka is 0.11 percentage points and for control states it is -0.05 percentage points, which gives a baseline mean gap of 0.16 percentage points. See descriptives statistics in Table A.3.

[50] From before: the "control group" for the mortality sample constitutes (i) children born before 2006 in Karnataka and the control states (ii) female children in control states in the post-program period and, (iii) boy children.

[51] The number of observations in Panel B are mechanically lower than in Panel A since breastfeeding duration information was recorded only for children born three years preceding the survey.

measuring program impacts on the probability of being exclusively breastfed for at least 6 months. I find that the improvement on the extensive margin is large, with girls being more likely to be exclusively breastfed relative to boys, during the first six months of life by 5.10 percentage points (column (3) of Panel C). This is in contrast to Anukriti et al. [2016] who find that the introduction of the ultrasound technology leads to improvements in breastfeeding duration in the second year of life and find no changes in the first year of life. However, I examine *exclusive* breastfeeding which is defined as 'no other food, drink or water'. The recommended duration of exclusive breastfeeding by the World Health Organization is at least 6 months.

1.7.4 Compositional vs. Causal Effects: Are Girls Born into Better Endowed Families?

The improvement in girls' infant mortality rates and post-natal health investments in Karnataka relative to boys, could be the result of girls being born into better endowed families. I test whether family characteristics of girls (such as parental education, mother's age at first birth and household wealth status) improved relative to boys in Karnataka and relative to control states. I estimate specification 1.6 with mother's age at first birth, mother's years of education, father's years of education, residence in a rural area, standard of living (SLI) index, as dependent variables. The results are in column (1), rows 1-5 of Table A.8. Triple interaction coefficients for all these variables are small, with only one coefficient being marginally significant. These results suggest that there is no differential improvement in household characteristics among families with girls relative to families with boys and therefore the improvement in girls' outcomes is unlikely to be explained by the fact that girls are born into 'better' households.

1.8 Heterogeneity

I examine impacts separately for the following groups: mother's educational attainment (less than secondary education or secondary education and higher), household wealth (low, medium and high wealth index), household caste (scheduled caste (SC) and other), type of residence (rural versus urban) and religion (hindu versus muslim).

In Table A.9 I first examine the heterogeneous impact of the program on the probability of a birth, unconditional probability of a female birth and unconditional probability of a male birth. With respect to mother's education it appears that the program led to an increase in the probability of a birth for all mothers - those with either less than secondary education or those with secondary education or higher. However, the increase in fertility stems from different sources. For mother's with secondary education or more, it appears that the *Bhagyalakshmi* program impacted their willingness to abort in addition to increasing their willingness to conceive. That is, the increase in the probability of female births is *more* than the increase in the probability of male births suggesting a reduction in sex-selective abortions. For mother's with less than secondary education it appears the program impacted *only* the willingness to conceive The probability of a female birth *and* the probability of a male birth rise for this subsample and are roughly equal. Given that the baseline sex ratio at birth was more skewed for women with secondary education or higher (47 percent as opposed to 48 percent), it is heartening that the impact of the program on sex selective abortions is higher for precisely this subsample.[52]

With respect to household wealth I find that a very similar story holds. Households with a medium or high SLI index see the most impact on the sex ratio at birth, but again for different reasons. Relatively well off households, who have a more male biased sex ratio at baseline see a decrease in the willingness to abort in addition to a change in the willingness to conceive. The unconditional probabilities of male and female births are very different for this group. Households in the middle of the income distribution see only an increase in the willingness to conceive and a small decrease in the willingness to abort female fetuses (the unconditional probabilities are only marginally different). Thus on average, female children are born in both middle and high income households. Households on the lowest end of the distribution see no impact. One of the reasons for this could be that these households are unable to access the program because of lack of awareness and/or it is harder for such households to meet all the bureaucratic procedures of the application process. With respect to caste, religion and residence the same story holds. Probability of a female birth increases in both rural and urban areas but only through an increase in the willingness to conceive in the former and through both a decrease in the willingness to abort and an increase in the willingness to conceive in the latter.

With respect to heterogeneity in post-natal mortality and investments, Table A.10 (second row in both panels) shows that in the pre-program period, among families in Karnataka, excess girl infant mortality was greater in low to middle-class socio-economic groups (illiterate, poor to middle class, rural and lower caste). In the

[52] On average, this lines up with my finding in Table A.8 where I found that there was no differential selection of girls into families where mother's education was higher. The program led to more female births in households with all levels of education, with the reason for those births either stemming out of lower abortions (for women with more than secondary education), higher fertility or both (for women with less than secondary education).

post-program period (first row in both panels), poor households witnessed a larger decline in in female infant mortality. This implies that there are larger increases in investments in poorer households. For instance, the reduction in excess girl infant mortality is larger for households on the lower end of the education distribution. Similarly, households in rural areas see the largest reduction in excess girl mortality.

Thus, while the probability of a female birth changed across most types of households; richer, literate and urban families see a decline in the willingness to abort in addition to an increase in the willingness to conceive, while poorer, less educated households witness only an increase in the willingness to conceive. Further, if the results on post-birth mortality were driven only through compositional changes then improvements in sex selective abortions for richer and literate women should lead to girls being born into richer families and this should drive their post-birth outcomes. However, I find that post-birth, the decline in excess female mortality was higher only for poorer, lower caste, illiterate and rural women, suggesting causal as opposed to compositional changes.[53]

1.9 Do Girls Do Better in the Long Run?

In this study, the main mechanism driving a reduction in excess female infant mortality and an increase in duration of exclusive breastfeeding for girls in Karnataka after the introduction of the *Bhagyalakshmi* program is a *decline* in post-birth girl neglect in nutrition and health inputs. However, both these measures examine

[53]Interestingly, Anukriti et al. [2016] find analogous impacts of the introduction of the ultrasound technology in India. They find that wealthier women react to the ultrasound technology by reducing fertility or in other words, they increase sex-selective abortions. In my study, I find that educated women are most responsive to the program in terms of decreasing their sex-selective abortion behaviour. Both these findings point to the importance of actual economic costs as a driving force in generating sex-selective abortions in the first place.

investments by parents only in the first year of a child's life. To investigate if program impacts are sustained for children *beyond* their first year, I examine the impact of the program on a more robust and critical measure of health and nutrition: height-for-age z-score.

If health inputs are sustained beyond the first year of life then this would imply positive impacts on longer term nutrition of girls eligible for the program. However, given the long-term nature of the program, it is plausible that parents, who initially started out with investing more in their girls; gradually reduce their investments over the years. Additionally, given that the program has only one minimum condition attached with health - that girls must be immunized; a reduction in other critical health inputs for girls after age one is conceivable. That is, it is possible that the non-linearity in the benefit structure of the program *does not* manifest over the long run with parents willing to increase health investments in girls but only up to the minimum point. Finally, this study has demonstrated that for certain sub-groups of families, the program worked through increasing their willingness to conceive. In this case, girls born in these families in the post-program period will grow up in larger families and this could also lead to deficiencies in nutritional intake. Thus, my analysis of the impact of *Bhagyalakshmi* on nutritional outcomes is a test of the long run sustainability of program impacts: an increase in nutritional outcomes indicates that even a long-term conditional cash transfer with payments 18 years in the future can lead to higher cumulative health as measured by height of girl children.

Since the District Level Household Survey (DLHS) and the Census Data used until now for the analysis lack data on nutritional outcomes of children, I employ the Indian Human Development Survey (IHDS) to examine these effects. The IHDS is a nationally representative survey. There are 2 waves of the survey, collected in 2004-2005 and 2011-2012 (henceforth, 2005 and 2012, respectively). I use only the second wave of the IHDS, IHDS-2 for this part of the analysis. The IHDS-2 is representative at the district level and includes data from households in 375 districts.

The main outcome variable to measure long run impacts of the program is height-for-age z-score.[54] Brief pre-period descriptive statistics of the IHDS sample are presented in Table A.11. In Karnataka the average height-for-age z-score for girls was -2.041. This implies that stunting is common amongst girls in Karnataka in the pre-program period.

I consider a similar sample of children in Karnataka and control states to the one described in Section 1.7, i.e. children born between 1995 and 2012, with 1995-2005 as the pre-program period and 2006-2012 as the post-program period. I examine the impact of the *Bhagyalakshmi* program on nutritional outcomes by running a regression analogous to equation 1.6:

$$Y_{ihst} = \alpha + \beta Kar_s * Post_t * F_i + \gamma_1 Kar_s * F_i + \gamma_2 F_i + \omega_t F_i + \delta X_{ihst} + \pi_{st} + \epsilon_{ihst} \quad (1.7)$$

where Y_{ihst} is the height-for-age z-score of individual i in household h in state s at time t and all other variables are defined as before. β is the coefficient of interest, and measures the effect of the program on the outcome of interest for female children before and after 2006 in Karnataka, relative to other states and relative to boys.

[54] The World Health Organization (WHO) outlines three important indicators for child nutrition: height-for-age (this is a long-term indicator of chronic malnutrition), weight-for-height (this is an indicator of acute malnutrition and is being unable to gain weight) and weight-for-age (this is a combination of the above two and is used to give an overall indicator of malnutrition).

Finally, X_{ihst} includes the following set of covariates: highest years of education of male in household, highest years of education of female in household, dummy for residence in a rural area, dummies for belonging to scheduled caste, dummy for religion and finally, household monthly consumption per capita.

I present the results from the above specification in Table 1.7 where the dependent variable is the height-for-age z-score. At baseline, girls in Karnataka had height-for-age z-scores that were 0.10 standard deviations less than boys in Karnataka and relative to control states. However in the post-program period height-for-age improves significantly for girls in Karnataka relative to boys by 0.36 to 0.41 standard deviations. These results are robust to controlling for state by birth year fixed effects and female by birth year fixed effects in addition to covariates. This illustrates that the *Bhagyalakshmi* program not only eliminated the baseline gap between boys and girls with respect to nutritional inputs but that couples invested in girls in a sustained fashion over the course of their childhood.

There is ample empirical evidence linking height to improved outcomes (see e.g. Bozzoli et al., 2009; Thomas and Strauss, 1997; Schultz and Strauss, 2008; Waaler, 1984 and Fogel, 2004). More importantly, Jayachandran and Pande [2015] document that high son preference especially for elder sons, is linked to the high stunting rate amongst Indian girls. The authors document that parental preferences: specifically, a strong desire to have and invest in an eldest son, underlie much of India's child stunting especially amongst girls. They also find that the gender gap in height is larger amongst wealthier households which implies that even as India develops, the problem of stunting might be hard to address by policy. I find that, by addressing one of the main causes of stunting amongst girls i.e high son preference, through a reduction of actual future costs of girls, it is possible to achieve large reductions in stunting rates.

1.10 Quantifying the Impact and Cost Effectiveness

I follow the methodology provided in Bhalotra and Cochrane [2010] and Anukriti et al. [2016] to quantify the estimated impacts of the *Bhagyalakshmi* program. I use my estimates on the impact of the program on child sex ratio to first calculate the total number of *additional* girls who survived up to age 6. I then calculate what percent of this number can be explained by an increase in the sex ratio at birth (i.e. fewer sex-selective abortions) and a decrease in excess female infant mortality (i.e death up to age one). These calculations are detailed in Appendix A.2 and in Table A.12. I find that the increase in the child sex ratio led to 28,124 *additional* girls surviving up to age 6, on an annual basis. Further, after taking endogenous changes in fertility into account I find that 62 percent of this is accounted for by fewer sex-selective abortions (= 16,599 girls who were not aborted) and fewer infant female deaths (= 770 girls who survived up to age 1).

In Section A.3.1 I also present a simple cost effectiveness analysis of the *Bhagyalakshmi* program and find that the program was moderately cost effective. That is, the conditional incentive translates into 1.525 number of sex-selective abortions averted, 0.047 female infant deaths averted and 0.20 additional months of exclusive breastfeeding for every $1000 spent by the implementer *per year* (See Table A.13).

1.11 Conclusion and Policy Implications

"*A great many women are simply not there because women are neglected compared with men. If this situation is to be corrected, the reasons why there are so many missing women must first be better understood.*" (Sen, 1990)

In this paper I show that one important reason for high son preference and the resultant missing women at young ages in India is, the high future costs of girls. Using large scale Census data and retrospective birth history data, I show that a financial incentive that exogenously reduced the actual future costs of girls, led to a substantial increase in the female-to-male child sex ratio by 19 to 25 additional girls for every 1000 boys. My estimates imply that 28,124 additional girls were born in Karnataka or survived up to age 6 on an annual basis because of the program. This paper uses temporal, geographic and compositional variation in a difference-in-differences and triple difference framework to show that the improvement in child sex ratio stems from both an improvement in the female-to-male sex ratio at birth *and* more girls surviving up to age one.

I find that the probability of giving birth to a girl rises by 9% both due to new pregnancies and due to fewer sex-selective abortions. The reduction in the willingness to abort led to an improvement in the sex ratio at birth by 13 additional girls for every 1000 boys. This reduction is accompanied by an increase in the desired number of girls suggesting that the high future cost of girls was an important determinant of son preference before the introduction of the program. Women are also less likely to engage in son-biased fertility stopping behaviour after the program indicating that they are increasingly indifferent about the gender of their next child.

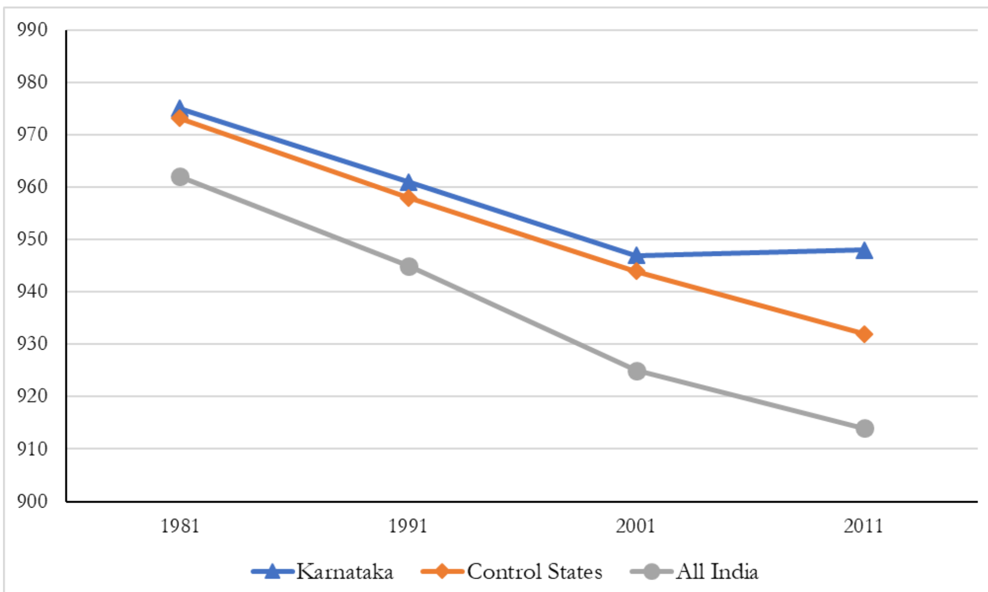
Further, I find that girls tend to do better even post-birth. The introduction of the program leads to an 81 percent decline in the excess female infant mortality gap in Karnataka. With respect to exclusive breastfeeding, while there was only a small improvement at the intensive margin, there was a large and significant increase at the extensive margin. The probability of being exclusively breastfed increased for girls by 5.1 percentage points. Height-for-age z-score, a long-run indicator for good nutrition increases by 0.4 standard deviations for girls in Karnatak.

There are several policy implications from the above study. First, my results indicate the importance of policy design. My analysis shows that a policy that provides incentives to couples to give birth to girls *and* simultaneously does not put a cap on the number of boys; is effective at reducing both girl deficit at birth *and* girl deficit post-birth. Second, my results also point to the importance of identifying heterogeneous impacts. Households at different spectrums of the income distribution engage in different kinds of discriminatory behaviour. For instance, if the policy goal is to reduce selective abortions, then relatively wealthier households with smaller family sizes need to be targeted. Additionally, girls who have more sisters tend to be the most disadvantaged which implies that these households should be targeted by policymakers. Finally, my results imply that economic incentives may be effective in dealing with the complex cultural roots of intra-household discrimination. Future costs of girls are expected to increase with modernization and are unlikely to be eradicated. My study provides evidence that changing actual costs of daughters, as opposed to trying to change the culture that gives rise to these costs, may be a crucial policy instrument.

The expected increase in female feticide as a result of declining desired and actual fertility has led to increased attention from policy makers and academics. From a policy standpoint increasingly male-biased sex ratios are undesirable for multiple reasons. First, sex-selective abortions lead to girls being consistently born in poorer families and this severely hampers the upward mobility of women.[55] Second, the expected scarcity of women has implications for the marriage markets, labour markets and could lead to increased prostitution and higher trafficking of women (Qian, 2008, Nandi and Deolalikar, 2013 and Anukriti et al., 2016). Thus, identifying the causes of high son preference is an important policy goal. This study, by identifying high future costs of girls as one important determinant of such behaviours, helps in moving us a step further towards tackling the critical problem of missing girls in India.

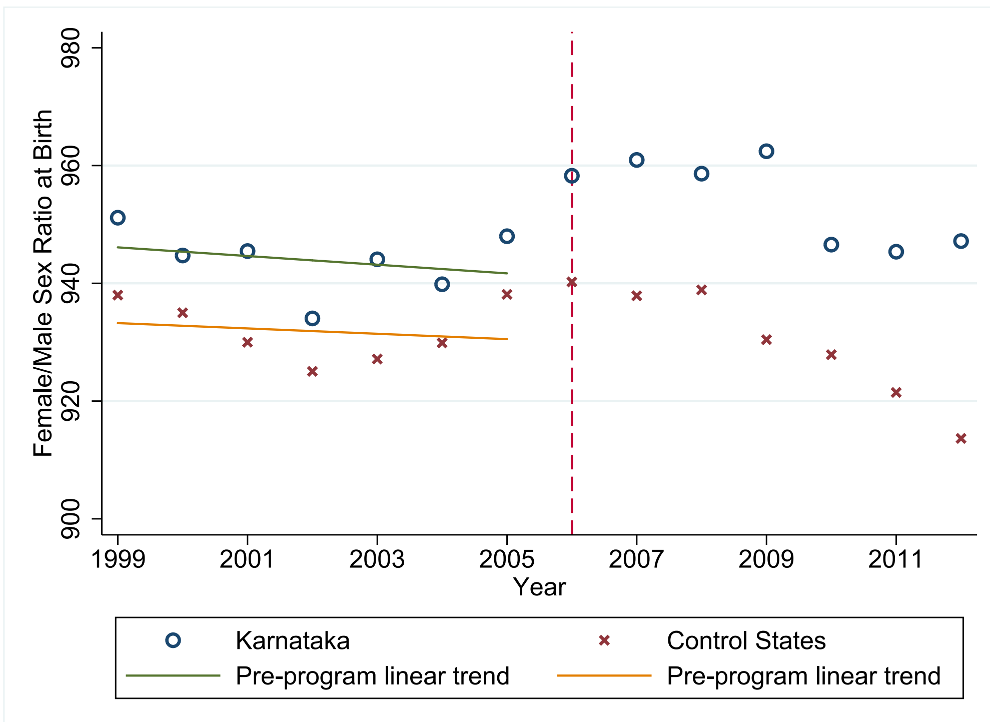
[55] The societal cost of having a female population that is less healthy and less educated is immense. For instance, it can result in the inter-generational transmission of poverty since it is widely known that more educated women tend to make higher investments in children. Thus, male-biased sex ratios lead to an externality wherein society would prefer women to be healthier and more educated but individual preferences are different.

Figure 1.1: Child Sex Ratio: Decadal Census Data 1981-2011



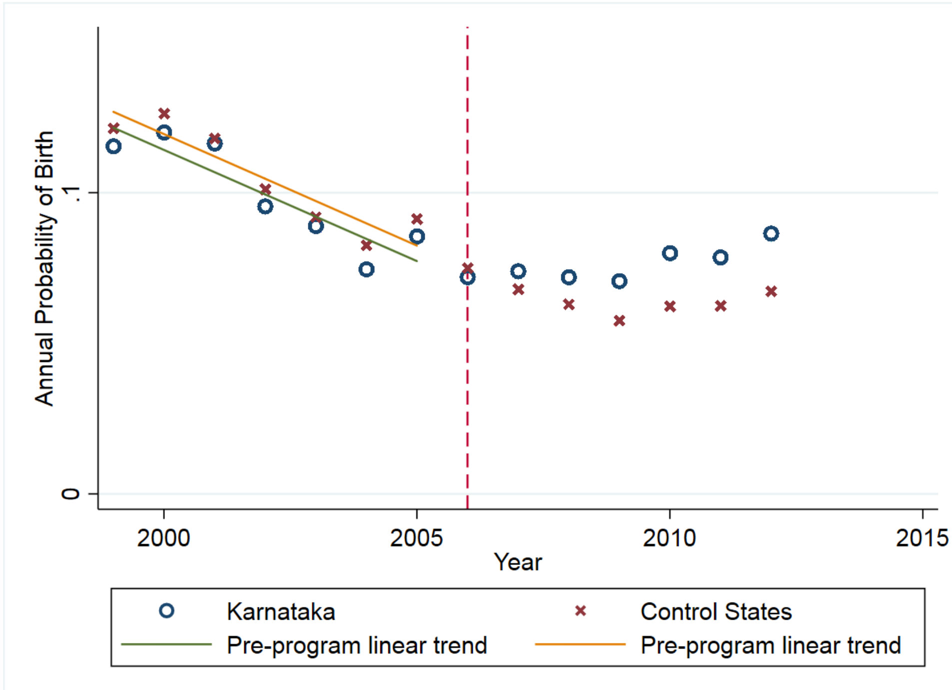
Note: This figure plots decadal child sex ratio rates from the Indian Census for 1981-2011 for Karnataka and the Control states separately for subsample I which includes all villages and towns of Karnataka and control states.

Figure 1.2: Sex Ratio at Birth: District Level Household Survey 1999-2012



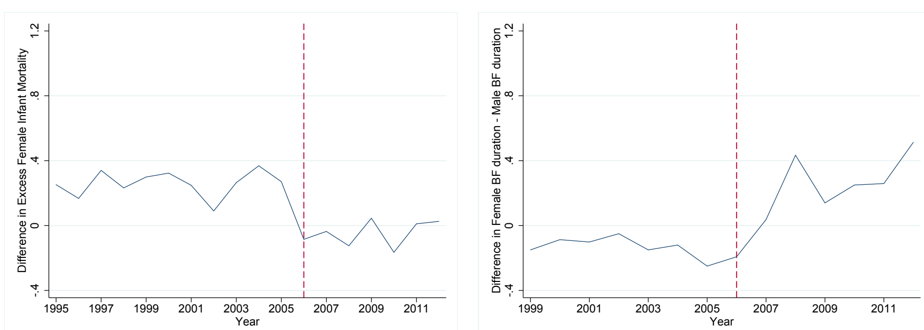
Note: Figure reports 5-year moving average of the sex ratio at birth for children of birth orders 1, 2 and 3 combined.

Figure 1.3: Annual Probability of Birth: District Level Household Survey 1999-2012



Note: Figure report annual probability of birth of women for Karnataka and control states. Includes women who were between 15-44 years of age at the time of the survey.

Figure 1.4: Trends in Excess Female Infant Mortality and Exclusive Breastfeeding Duration: District Level Household Survey 1995-2012



(a) Differential Trend in Excess Female Infant Mortality

(b) Differential Trend in Female Breastfeeding Duration - Male Breastfeeding Duration

Note: Panel (a) reports the *difference* in the 5-year moving averages of excess female infant mortality between Karnataka and control states. Panel (b) reports the *difference* in the gap between female breastfeeding duration and male breastfeeding duration for Karnataka and control states. Breastfeeding data is available only for the births 3 years before the survey and hence starts from 1999.

Table 1.1: Difference-in-differences estimates: Effect of *Bhagyalakshmi* on Child Sex Ratio

A. Pooled Sample	(1)	(2)	(3)
Karnataka*Post	25.641***	19.333***	19.528***
	(9.961)	(4.659)	(4.476)
Wild Bootstrap *p-value*	(0.061)	(0.000)	(0.002)
Observations	389,055	389,055	389,055
Number of PSUs	129,685	129,685	129,685
B. Border Districts Only	(1)	(2)	(3)
Karnataka*Post	31.243***	22.541***	18.370***
	(3.830)	(6.724)	(3.696)
Wild Bootstrap *p-value*	(0.000)	(0.001)	(0.000)
Observations	69,645	69,645	69,645
Number of PSUs	23,215	23,215	23,215
Year FE	x	x	x
PSU FE	x	x	x
State Specific Trends		x	x
Covariates			x
Covariates*Post			x

Note: This table reports the coefficients of variable $Program_{jt}$ from specification 1.2. In addition I include, female and male literacy rates; female and male labour force participation rates; fraction scheduled caste; log population of a PSU. *Post* is defined as = 2011. *Karnataka=1* if PSU j is in Karnataka and 0 otherwise. All regressions are weighted with the 1991 PSU level population size. Standard errors are clustered by state. *** 1%, ** 5%, * 10%. Wild bootstrap p-values are also presented

Table 1.2: Difference-in-Difference Estimates: Effect of *Bhagyalakshmi* on Fertility (all outcomes multiplied by 100)

A. Probability of a Birth	(1)	(2)	(3)
Karnataka*Post	0.8520**	0.7705*	0.8356**
	(0.406)	(0.451)	(0.420)
Wild Bootstrap *p-value*	(0.043)	(0.062)	(0.000)
Proportional Selection δ			[-1.564]
Baseline Mean	12.64	12.64	12.64
Observations	1,407,194	1,407,194	1,407,194
Mothers	280,896	280,896	280,896
B. Probability of a Female Birth	(1)	(2)	(3)
Karnataka*Post	0.5293**	0.5063***	0.5220***
	(0.228)	(0.211)	(0.209)
Wild Bootstrap *p-value*	(0.002)	(0.000)	(0.001)
Proportional Selection δ			[-1.751]
Baseline Mean	6.01	6.01	6.01
Observations	1,407,194	1,407,194	1,407,194
Mothers	280,896	280,896	280,896
C. Probability of a Male Birth	(1)	(2)	(3)
Karnataka*Post	0.3226	0.2641	0.2935
	(0.223)	(0.241)	(0.235)
Wild Bootstrap *p-value*	(0.251)	(0.130)	(0.653)
Proportional Selection δ			[-1.130]
Baseline Mean	6.63	6.63	6.63
Observations	1,407,194	1,407,194	1,407,194
Mothers	280,896	280,896	280,896
Year FE	x	x	x
State FE	x	x	x
State Specific Trends		x	x
Covariates			x
Covariates*Post			x

Note: Table reports estimates from specification 1.3. Parameter estimates reported are from a linear probability model; dependent variable takes value 1 if woman i gives a birth in year t in Panel A. In Panels B-C the dependent variable is 1 if woman i gives birth in year t to a female and male respectively. Each woman contributes 5 observations, one for each year before the survey year in which she appears. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%. Wild bootstrap p-values are also presented.

Table 1.3: Difference-in-Difference Estimates: Effect of *Bhagyalakshmi* on Sex Ratio at Birth (all outcomes multiplied by 100)

A. Pooled Sample	(1)	(2)	(3)
Karnataka*Post	0.9128***	0.7102**	0.9130***
	(0.309)	(0.345)	(0.363)
Wild Bootstrap *p-value*	(0.034)	(0.103)	(0.001)
Proportional Selection δ			[5.274]
Baseline Mean	47.91	47.91	47.91
Observations	2121	2121	2121
Districts	153	153	153
B. Border Districts Only	(1)	(2)	(3)
Karnataka*Post	1.0325**	2.2318**	2.5314***
	(0.472)	(1.015)	(0.994)
Wild Bootstrap *p-value*	(0.074)	(0.003)	(0.000)
Proportional Selection δ			[5.226]
Baseline Mean	47.68	47.68	47.68
Observations	419	419	419
Districts	30	30	30
Year FE	x	x	x
District FE	x	x	x
State Specific Trends		x	x
Covariates			x
Covariates*Post			x

Note: This table reports the coefficients of variable $Program_{dt}$ from specification 1.4. The dependent variable is the *weighted* proportion of female births. *Post* is defined as $>$ 2006. *Karnataka=1* if district d is in Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%. Wild bootstrap p-values are also presented.

Table 1.4: Difference-in-Difference Estimates: Effect of *Bhagyalakshmi* on Son-Biased Fertility Stopping Behaviour (all outcomes multiplied by 100)

A. Probability of a Birth	(1)	(2)	(3)
Karnataka*Post*First Girl	-1.4416***	-1.3255***	-1.4166***
	(0.547)	(0.517)	(0.551)
Wild Bootstrap *p-value*	(0.001)	(0.014)	(0.009)
Karnataka*First Girl	0.3767	0.3881	0.3968
	(0.383)	(0.381)	(0.375)
Baseline Mean	0.40	0.40	0.40
Observations	874,060	874,060	874,060
Mothers	161,381	161,381	161,381
Year FE	x	x	x
State FE	x	x	x
State-Year FE		x	x
Female-Year FE		x	x
Covariates			x
Covariates*Post			x

Note: This table reports estimates from specification 1.5. Parameter estimates reported are from a linear probability model; dependent variable takes value 1 if woman i gives a birth in year t. Each woman contributes 5 observations, one for each year before the survey year in which she appears. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%. Wild bootstrap p-values are also presented.

Table 1.5: Difference-in-Difference Estimates: Effect of *Bhagyalakshmi* on Desired Fertility

A. Ideal No. of Boys	(1)	(2)
Karnataka*Post	0.0150	0.0202
	(0.013)	(0.015)
Wild Bootstrap *p-value*	(0.134)	(0.196)
Baseline Mean	1.10	1.10
Observations	235,957	235,957
B. Ideal No. of Girls	(1)	(2)
Karnataka*Post	0.0835***	0.0844***
	(0.011)	(0.018)
Wild Bootstrap *p-value*	(0.002)	(0.013)
Baseline Mean	0.91	0.91
Observations	235,957	235,957
C. Ideal No. of Children	(1)	(2)
Karnataka*Post	0.0763***	0.0811**
	(0.029)	(0.039)
Wild Bootstrap *p-value*	(0.020)	(0.122)
Baseline Mean	2.21	2.21
Observations	235,957	235,957
D. Sex of Next Child is Girl=1	(1)	(2)
Karnataka*Post	0.0255**	0.0241**
	(0.011)	(0.010)
Wild Bootstrap *p-value*	(0.000)	(0.002)
Baseline Mean	0.11	0.11
Observations	165,614	165,614
Round FE	x	x
State FE	x	x
Covariates		x
Covariates*Post		x

Note: This table reports the coefficients from specification 1.3 where the each woman contributes 1 observation for the survey year in which she appears. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%. Wild bootstrap p-values are also presented.

Table 1.6: Triple difference estimates: Mortality and Health Investments (all outcomes multiplied by 100)

A. Infant Mortality Rate	(1)	(2)	(3)
Karnataka*Female*Post	-0.1386***	-0.1260***	-0.1319***
	(0.027)	(0.032)	(0.033)
Wild Bootstrap *p-value*	(0.000)	(0.005)	(0.046)
Karnataka*Female	0.1551***	0.1598***	0.1583***
	(0.026)	(0.029)	(0.028)
Baseline Mean Gap	0.16	0.16	0.16
Observations	204,230	204,230	204,230
B. Months of Exclusive Breastfeeding	(1)	(2)	(3)
Karnataka*Female*Post	0.1987**	0.1727**	0.1749**
	(0.093)	(0.081)	(0.079)
Wild Bootstrap *p-value*	(0.108)	(0.079)	(0.038)
Karnataka*Female	-0.1171***	-0.1283***	-0.1241***
	(0.020)	(0.019)	(0.017)
Baseline Mean Gap	0.13	0.13	0.13
Observations	73,251	73,251	73,251
C. Exclusively Breastfed $\geq$ 6 months	(1)	(2)	(3)
Karnataka*Female*Post	5.5679***	5.0815***	5.1054***
	(0.571)	(0.522)	(0.548)
Wild Bootstrap *p-value*	(0.006)	(0.011)	(0.002)
Karnataka*Female	-0.5332	-0.5205*	-0.5047*
	(0.320)	(0.273)	(0.269)
Baseline Mean Gap	0.52	0.52	0.52
Observations	69,084	69,084	69,084
Birth Year FE	x	x	x
State FE	x	x	x
State-Birth Year FE		x	x
Female-Birth Year FE		x	x
Covariates		x	x
Birth Order-State FE			x
Birth Order-Birth Year FE			x
Birth Order-Female FE			x

Note: This table reports the coefficients from specification 1.6. *Post* is defined as >= March 31st, 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%. Wild bootstrap p-values are also presented.

Table 1.7: Triple difference estimates: Long Run Nutritional Outcomes

A. Height-for-Age z-score	(1)	(2)	(3)
Karnataka*Female*Post	0.4094**	0.3772**	0.3588**
	(0.194)	(0.186)	(0.177)
Wild Bootstrap *p-value*	(0.000)	(0.008)	(0.001)
Karnataka*Female	-0.0965	-0.099	-0.0933
	(0.105)	(0.102)	(0.101)
Baseline Mean Gap	0.10	0.10	0.10
Observations	13,272	13,272	13,272
Birth Year FE	x	x	x
State FE	x	x	x
State-Birth Year FE		x	x
Female-Birth Year FE		x	x
Covariates			x
Covariates*Post			x

Note: This table reports the coefficients from specification 1.7. *Post* is defined as >= March 31st, 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%. Wild bootstrap p-values are also presented.

Chapter 2: Is She Free to Work? Impact of Rural Health Insurance on Labour Supply in India

2.1 Introduction

While there is much known about the labour market impacts of access to better quality healthcare in developed countries, relatively little is known about these effects in developing countries. Given that illness impacts the poor disproportionately more, it is important to understand whether access to better healthcare is welfare improving overall. A large body of literature shows that better health increases labour supply, wages and productivity in developing countries (see e.g. Strauss and Thomas, 2007). Better quality healthcare through insurance, by mitigating the adverse impacts of health shocks either through a reduction in the time spent caring for sick individuals or through direct effects on health can impact labour market outcomes.[1] Health insurance can also impact the utility derived from leisure activities. On the one hand, better health increases the utility derived from leisure. However, risk averse consumers might enjoy leisure less if leisure results in more uncertainty about health care expenditures (Currie and Madrian, 1999). Understanding how the provision of health insurance impacts labour supply decisions of the household

[1] According to the efficiency wage theory the link between health and labour supply would be stronger in less healthy populations, and to the extent that health insurance impacts own health, this could lead to impacts on labour supply as well.

is important not only because labour market outcomes are indicators of overall welfare but also because provision of non-contributory insurance to the underprivileged may be an important redistributive policy tool.

This study examines whether access to free formal rural healthcare has a causal effect on labour market outcomes by studying a plausibly exogenous policy change. In 2008, India introduced the *Rashtriya Swasthya Bima Yojana (RSBY)* which provides each below poverty line (BPL) household access to health insurance at no cost at selected private and public hospitals in the country. Designed to increase access to healthcare and reduce the financial burden of health expenses, RSBY has enrolled about 36 million households as of 2014 (about 12% of the total number of households in the same year). While enrolling into the program is voluntary and thus households self-select into the program, I take advantage of the phased rollout of the program from 2008 through 2012 at the district level and employ a differences-in-differences approach to examine the intent-to-treat impact of the program on labour supply. I also examine RSBY *beneficiary* households and estimate the average treatment effect on the treated.

India has one of the highest out-of-pocket (OOP) expenditures on healthcare in the world. According to the National Health Accounts Data (2004-05) individual OOP account for 78 percent of total expenditures while government expenditures account for only 20 percent (Azam, 2016). Given the high proportion of total health expenditures that are spent on out-of-pocket expenses, RSBY was devised to provide cashless services to rural households. Households enrolled under RSBY can use a 'smart card' to avail services and pay an annual nominal enrollment fee.

While there is a growing literature on RSBY's impact on out-of-pocket expenditures (Johnson and Krishnaswamy, 2012; Nandi et al., 2013a; Sun, 2011; Rajasekhar et al., 2011a), there is little evidence on the labour market impacts of the program. Illness exposes households in developing countries to increased risk. Risk pooling strategies such as social safety nets or formal health insurance are often lacking and this forces households to engage in a variety of risk coping strategies ranging from selling off durable assets, and reallocating labour away from productive activities (del Valle, 2014). Since health insurance is instrumental in reducing household level out-of-pocket expenditures on health care it can protect households from getting impoverished in addition to improving healthcare utilization and overall health. Given the scope for government intervention, large scale healthcare reforms have been introduced by many developing countries. One of the first of these was in China in 2003 when rural health insurance was adopted (Wagstaff and Manachotphong, 2012). In this context, using nationally representative data from two large datasets - the Indian National Sample Survey (NSS) and the Indian Human Development Survey (IHDS) - this paper seeks to understand the extent to which a national rural health insurance program, the Rashtriya Swasthya Bima Yojana (RSBY), impacts labour market outcomes of households in India, presenting both Intent to Treat as well as Average Treatment on the Treated estimates.

Existing evidence on the impact of the RSBY program on financial risk protection is mixed. Karan et al. [2017] find that the reduction in out-of-pocket health expenditures and catastrophic inpatient spending was small and statistically insignificant. Using primary data from Maharashtra and Gujarat, Rathi et al. [2012] and Devadasan et al. [2013] respectively, find that some families who have been enrolled into the program continue to spend substantial amounts on out-of-pocket health expenses. Johnson and Krishnaswamy [2012] find that the program led to a small, statistically significant decrease in out-of-pocket expenditures while finding no impact on inpatient expenditure. Similarly, Azam [2016] finds that while out-of-pocket health expenses were not impacted by the program, expenditures on medicines declined significantly in rural areas. One reason why Johnson and Krishnaswamy [2012] seem to find a significant reduction in outpatient expenditures is because they examine program impacts only up to 2009-2010. As pointed out by Nandi et al. [2013b] studies which examine impacts only up to 2009-2010 are potentially biased since the program was still in its infancy in terms of implementation. Studies that have examined program effects up to 2011-12 seem to consistently suggest low and statistically insignificant impacts of the RSBY on financial risk protection.

In contrast, with respect to hospital utilization, the literature *consistently* finds that RSBY led to an increase in utilization rates. Azam [2016] finds a positive impact on hospital utilization by households in rural areas. He also finds that RSBY led to lower expenditures on medicines in rural areas. Ghosh and Gupta [2017] using a matched sample of 'potential' RSBY treatment and control households, find that the impact on inpatient care utilization was statistically significant - RSBY increases the number of hospital admissions of insured families by 59% compared with the mean inpatient care utilization of uninsured families. Finally, Johnson and Krishnaswamy [2012] also find that the program impacted hospital utilization positively.

The evidence on the labour market impacts of non-contributory health insurance, especially in developing countries is scarce. Moreover, most of the research that exists focuses on the shift in employment from the formal to the informal sector. This is because in most countries (e.g. Mexico, Thailand, Colombia) non-contributory health insurance is only offered to those working in the informal sector. In a study from Colombia, Camacho et al. [2013] find that public health insurance led to an increase in informal employment of approximately 4 percentage points.

The present study is different from previous work in at least four ways. First, this is the only study, in a developing country setting, that looks at the labour market outcomes of a health insurance program that did not use the labour market status as a determinant of eligibility, but instead had a pure income criterion for eligibility. Thus, this study indirectly examines the ability of health insurance to alleviate poverty because of health shocks and thereby impact labour market outcomes. Second, this is the first study that examines the impact of household access to health insurance, an important determinant of relative productivity for individual members, on short term allocations across different types of work for children. Third, it contributes to the nascent literature on the economic consequences of provision of health insurance in developing countries. While much is known about the impact of health shocks, little is known about how households reallocate labour supply as a result of access to higher quality healthcare. The exception in this regard is Adhvaryu and Nyshadham [2012] who estimate the effects of higher quality healthcare through access to formal healthcare on labour supply of acutely sick individuals in Tanzania and find that the ability to choose formal care led to individuals spending more labour hours on the farm. Given that impoverishment due to health-related expenses is a leading cause of poverty in India, it is important to understand the extent to which social insurance programs trigger direct and indirect behavioural responses that can mitigate this impoverishment. Finally, my paper contributes to the literature on gender differentiated impacts of health insurance. It links the impact of health insurance to a wider literature on the role of economic development in helping either mitigate or reinforce gender inequalities.

Overall, I find significant positive effects of the RSBY program on the labour supply of women in the private casual labour market using both the Intent-to-treat (ITT) and Average treatment effect on the treated (ATT) estimates. On average, household level access to free health insurance increases the number of days spent in the past week by women on private casual labour by 0.29 days in early treatment districts (i.e. districts that received RSBY treatment on or before March 2010). Mean days spent in private casual work by women at baseline in 2004-05 is 0.45 days in a week. Thus, an ITT impact of 0.29 days is large; time spent in private casual work increases by approximately 50 percent for women. This translates into approximately 15 *more* days of work annually for women in the private labour market. The ATT estimates are larger – time spent in private casual work increases by about 81 percent for women. There was also a significant increase in time spent, for women, in the private agricultural wage market by 0.25 days in the past week. Importantly, this increase in labour supply in private sector work is accompanied by a significant decrease in the number of days spent in the past week on domestic work by 0.69 days. I examine various mechanisms that may explain the relationship between the program and women's labour supply. I consistently find that, for all labour supply measures, the effect of accessing health insurance is largest for women in households with fewer working age members and higher number of dependents. Thus, I provide suggestive evidence that the impact of the program for women may be due to the reduction in time spent at home in caregiving tasks.

For men, there is a smaller change in labour supply in private sector work. On average, household level access to free health insurance increases the number of days spent in the past week by men on private casual labour by 0.19 days (ITT estimate) to 0.37 days (ATT estimate). This corresponds to a 20 percent increase over the baseline mean. This increase in time spent in the private casual labour market is accompanied by a decline in the time spent by men in self-employment activities. This points to the fact that there is increased healthcare utilization by men which improves their health and enables them to reduce time in self-employment activities at home and work in the private casual labour market.[2]

Finally, I find that children substitute for adults in the two domains that men and women are moving away from: domestic work and self-employment activities. Consequently, I find that for children, time spent at educational institutions declines. Overall, given positive changes in female and male private casual employment (and significant negative changes in male self-employment and female domestic work), I attribute the effect of the program to a reduction in caregiving tasks for women, a reduction in self-employment activities for men and finally increased healthcare utilization by both men and women.

Vulnerability to health shocks is a serious concern in India. Nearly 10% of India's population record out-of-pocket expenditures in excess of 25% of non-food consumption (World Bank, 2008). There is a significant literature that shows that households in India use productive assets to smooth consumption when faced with

[2]Self-employment activities at home here refers to working in household enterprises as own-account worker or as a helper. For e.g. opening a small shop inside one's house if one is too week for manual labour in the private labour market. Such self-employment is not the same as entrepreneurship. The goal of self-employment in this case, is to earn money for a time—preferably, a short time—before transitioning to a more remunerative activity when one's physical health improves.

negative productivity shocks. Rosenzweig and Wolpin [2017] find that in rural India, bullocks, while also used as sources of mechanical power in agricultural production, are sold to smooth consumption in the face of income shocks. This provides evidence that efficiency in crop production is sacrificed to ensure low volatility in consumption. Jodha [1978] again using data from India, argues that sales of productive assets when faced with shocks (a drought in this case) is very common. While the specific shock examined in this paper is different, the same argument is applicable in this context too - the poor engage in costly consumption smoothing techniques in the short run in response to catastrophic health shocks which can have severe long-term impacts, both in terms of productive activity and labour market outcomes.

Accounting for close to USD 160 million in the union budget of 2012-13, RSBY is one of the largest programs of its kind in a developing country. In this setting, this study contributes to the ongoing policy debate regarding the benefits of increased access to quality healthcare. While existing literature has failed to find a significant change in out-of-pocket health expenditures, it has consistently found an increase in health care utilization rates. This implies that the poor are availing of the RSBY program which impacts their labour supply; *but* the maximum coverage of Rs. 30,000 (about 440 USD in 2010-11) under the program is insufficient. Thus, impacts on labour supply due to RSBY are not the result of an income effect of the program, but instead are a consequence of the improved health of men and reduced time in caregiving tasks for women.

The paper is organized as follows. Section 2.2 provides a brief review of the previous developments in the literature on health insurance in India. It also provides a description of the RSBY. Section 2.4 discusses the intent-to-treat estimation strategy and results, while Section 2.5 describes the average treatment on the treated estimation strategy and results. Section 2.6 examines the impact on health and healthcare utilization and Section ?? performs robustness checks. Section 2.7 concludes.

2.2 Context

2.2.1 Background of Health Insurance in India

Illness and poverty are closely linked to each other. The poor are often unable to smooth consumption during periods of ill health and catastrophic health expenses often push families into poverty (Xu et al., 2007; Rajasekhar et al., 2011b). Vulnerability to shocks is an important cause for deprivation (Dercon, 2001a). This is compounded by the presence of weak financial instruments and the adoption of sub optimal coping mechanisms such as asset sales, migration and child labour (Rosenzweig and Wolpin, 2017; Haughton and Khandker, 2009). According to Krishna [2004], the most common reason given by poor people for their descent into poverty is high out-of-pocket expenditures on health care (Rajasekhar et al., 2011b). Shahrawat and Rao [2012] note that the extent to which health related out-of-pocket (OOP) expenditures exacerbate poverty is at a maximum for households below the poverty line (BPL) in comparison to those above. The only government owned health insurance company that exists in India is the 'New India Assurance'. New India Assurance provides health insurance at slightly lower rates than private providers. One has to purchase health insurance directly through this company as one would purchase from a private provider. Thus, before RSBY was introduced in India, access to health insurance of any kind depended on one's ability to pay for insurance through the marketplace.[3] In the absence of adequate social safety nets households can become impoverished not only because of out-of-pocket expenses and ill health, but also because of missed work, disability or premature death (Fan et al., 2012). In this context, to reduce the inequalities in health access and decrease health related expenses for uninsured households, rural health insurance was introduced.

The empirical evidence on the effectiveness of health insurance in India is sparse. Acharya et al. [2012] detail the myriad health insurance schemes in India and analyze their effectiveness. The impact of these small community based schemes has been mixed, at best. Aggarwal [2010] assesses the impact such a scheme in Karnataka. The study finds that the Yeshasvini insurance reduced out-of-pocket expenditures on health care out of savings, in addition to reducing borrowing money to finance healthcare payments. However, Raza et al. [2016] using randomized controlled trials from Bihar and Uttar Pradesh evaluate the impact of three CBHI schemes and find no impact on both hospital utilization as well as on financial risk protection. Using qualitative surveys the authors find that the main reason for the lack of impact of these schemes is that most households tend to drop out after a year, because of poor service quality and high premiums.

While the impact of health insurance on health utilization and expenditures is relatively well studied, evidence on the impact of rural health insurance on labour market outcomes in developing countries is limited. Given the importance of such programs in mitigating the effects of a contraction in incomes due to health shocks

[3] India has a public health care system, wherein the government spends 1-2% of the GDP on public heath care. Thus, in the event that one cannot afford insurance from the marketplace, one can, in theory, access government hospitals. However, free, public health system is so underfunded and understaffed that poor people are forced to pay exorbitant amounts to private-sector doctors for treatment.

and hence impacting labour supply, this lack of evidence seems stark. In one of the first papers in this domain, Wagstaff et al. [2009] examine the universal health coverage scheme in Thailand which was rolled out in 2001. They find that, at the extensive margin the probability of employment increased for married women and at the intensive margin increased time spent in informal employment for women.

Levy and Meltzer [2008] have argued that social insurance schemes, like rural health insurance, can have unintended consequences such as encouraging an increase in time spent in the informal sector. However, given that employer provided health insurance in India is rare coupled with the fact that, unlike health insurance schemes in other countries, RSBY is not restricted to informal workers, a decrease in the share of formal sector workers post-RSBY is unlikely. The most critical part of RSBY is that it allows households to transfer risk to the government while simultaneously increasing access to health services. Impacts on labour supply are thus driven either through impacts on own health or through decreased time spent caring for sick dependents.

2.2.2 Overview of the Rashtriya Swasthya Bima Yojana

The Rashtriya Swasthya Bima Yojana was introduced in India in 2008. The scheme aims at improving access of quality medical care to below poverty line (BPL) families by providing cashless treatment at private and public hospitals for a variety of ailments. RSBY provides cashless coverage up to Rs. 30,000 (about 440 USD in 2010-11) each year to each enrolled household for hospitalization procedures in empaneled private or public hospitals. This coverage is large in purchasing power terms: the median level of income of the average person in my sample is about 616 USD and the average household spends about 7-10% of their income towards healthcare costs. The policy covers maternity care, hospitalization, day care treatment and related tests, consultations, medicines and pre- and post-hospitalization expenses excluding expenses for out-patient treatment. While the scheme covers most surgical and non-surgical procedures, not all diagnostic tests are covered. Importantly, RSBY covers pre-existing conditions.

Under RSBY each below poverty line (BPL) household is issued an insurance card or 'smart card' and up to five household members can be registered under one card. To obtain treatment an individual just needs to present this smart card at any participating hospital. The hospital receives reimbursement for treatment costs by the insurance company. Both private and public hospitals are empaneled under the scheme and thus households are free to choose between them.

2.2.3 Enrolment and Utilization of RSBY

RSBY is implemented at the state level. State governments choose an insurance company through an open tender process. The central and state government split the premium cost.

During the enrollment process, to ensure widespread coverage, states are required to prepare in advance a roadmap for the enrollment campaign in each village in a district or taluk, and give the village prior notice of the enrollment team's visit. In addition, to ensure that eligible households are aware of the scheme in each village and can plan to be present on the day of enrollment, a roster of eligible households is displayed at the enrollment station. Households have to pay Rs. 30 (45 cents) as annual registration fees.

Prior schemes of the government have been plagued by insufficient publicity and lack of prior notice. The enrollment process of the RSBY aims to correct both these flaws. Policies are issued for one year and are renewed on an annual basis. Beneficiaries can utilize the scheme from the start date of the policy which is usually three to four months after the enrollment process has been completed.

The state-wise number of beneficiaries enrolled from 2012-2015 in major states is given in Appendix Table B.1. Up to October 2016, the program has covered 460 districts in India. About 41 million RSBY insurance cards have been issued which cover about 150 million people (i.e. RSBY insurance cards are given one per household). The district-wise enrollment ratio (share of eligible households enrolled) has exhibited a fair amount of variation. For instance, enrolment ratios in certain districts of Uttar Pradesh were as low as 3-6%, while enrollment was nearly universal in districts of Chhattisgarh and Kerala (Karan et al., 2017). The national enrollment rate was about 57% suggesting low enrollment. There are at least two reasons for this: first, it is possible that even in the districts which have begun implementing RSBY, all eligible households have not yet been enrolled due administrative inefficiencies (Sun, 2010; Rathi et al., 2012). The second reason for the low enrollment rate is that information and outreach campaigns by enrollment agencies might have been insufficient (Rajasekhar et al., 2011b; Sun, 2010; Rathi et al., 2012; Devadasan et al., 2004).

2.2.4 Rollout of RSBY

RSBY was introduced at the national level in 2008 and each state government was expected to adopt the scheme in a phased manner over the next five years. States are responsible for selecting districts for inclusion into the scheme. In proposing districts for inclusion there were three criteria that were considered. Districts selected under RSBY should have: (a) a sufficient network of private and public hospitals, (b) intermediaries who can assist in spreading awareness about the program and, (c) basic infrastructure necessary for implementation such as roads and electricity. States began rolling out the program from 2008 in different districts and in different years.

2.3 Theoretical Framework

This section formalizes the intuition that in the presence of health shocks, the standard labour-leisure choice model of a poor household, without adequate coping mechanisms, will be affected by their demand for health insurance. In this model, it is assumed that health insurance can potentially impact the labour supply choice in two ways. First, it may impact the labour force participation margin of the working age population. This could also include the formal-informal work margin since it is largely people with informal jobs who stand to benefit most from the RSBY.[4] This could arise both from improved health as a result of the program or the ability to now enter the labour market since caregiving tasks are reduced.

Second, the program can change the number of hours spent in the labour market. This can happen in two ways. First, better health impacts productivity. If we assume that people don't alter their consumption of leisure because of better health, then health insurance should increase labour supply. Further, RSBY covers

[4]Section 2.2 outlines the details of the scheme. Eligibility was based on one's poverty status. A household was deemed eligible for RSBY if the per capita consumption expenditure of that household was below the government specified poverty line. While one's employment status was not included in the definition of eligibility, most informal workers are part of BPL population.

hospitalizations only. Since major morbidities are more likely to result in hospitalizations than short term illnesses, the potential for RSBY to impact labour supply is greater.

Table 2.1 presents health related summary statistics in the last 4 weeks from the Indian Human Development Survey (2004 - 05). From the table, it is clear that a significant amount of time is lost because of major and minor illnesses especially for those in the age group 0-5 years or in the age group 55 and above. Access to free healthcare has the potential to treat these illnesses quickly, which can impact labour supply for both men and women. Secondly, in the absence of health insurance, individuals in households with a higher number of dependents would spend a large amount of time in caregiving activities for the sick. Access to healthcare at zero cost can, potentially, allow individual household members to spend more time on the labour market. Given that household members from uninsured households spend a significant amount of time in helping cope with illness, RSBY can potentially help in freeing up household resources towards more productive activities.

A typical household spends on average about 5 days in a month with at least one household member being unable to work due to short-term illness and about 4 days because of some major illness (see Table 2.1).[5] It is also plausible that the provision of RSBY could lead to health gains; particularly among dependents.[6] Prompt medical attention coupled with the greater vulnerability of these sub-groups means that RSBY is well-equipped to generate health impacts. This could be happening through both the availability of and accessibility of prompt medical attention that

[5] Here major illnesses refer to: cataract, tuberculosis, heart disease, leprosy, cancer, polio, paralysis, epilepsy, mental illness, asthma and diabetes.

[6] Levy and Meltzer, 2008 review the literature on the impact of health insurance on health.

the RSBY generates.

2.4 Intent-to-Treat Effect

2.4.1 Program Rollout Data

Information about the rollout of the RSBY comes from administrative records maintained by the Ministry of Health, Government of India. This is available online on the RSBY website from where this information was accessed.

2.4.2 Household Survey Data

I use repeated cross sections of a nationally representative district level household survey, the Employment and Unemployment Survey (NSS Survey). My analysis uses cross-sectional data for the years 2004-2005, 2007-2008 and 2011-2012 of the NSS.

The NSSO has conducted national household level surveys since the early 1950s in India. The surveys conducted in 2004-2005 (61^{st} Round), 2007-2008 (64^{th} Round) and 2011-2012 (68^{th} Round) are large-sample surveys. The NSS collects data on various individual and household level characteristics such as religion, caste, employment status and household consumption. My identification uses the district-wise phased rollout of the program. While constructing the sample the following exclusion rules have been used: one, the states of Tamilnadu, Karnataka and Andhra Pradesh have been excluded since these states had similar state run rural health insurance programs during the period under consideration in this study. Two, I also exclude the 3 Union Territories: Andaman & Nicobar Islands, Dadra & Nagar

Haveli and Daman & Diu. Three, individuals between 18 and 60 years have been included for the adult labour market outcomes, while those between 6 and 17 years have been included for the child outcomes. Four, I drop observations that have missing information for age and gender. Finally, I only include districts that have rural populations. The final sample includes households from 531 districts.

Implementation of the RSBY began in 2008-09. Thus, the 2011-12 wave (68^{th} round of the NSS) represents the post-intervention period. The 2004-05 and 2007-08 waves (61^{st} and 64^{th} rounds of the NSS respectively) represent the pre-intervention period. Districts that began implementing the RSBY in 2008 did so in the latter half of the year from August to December, and thus the 2007-08 NSS survey (64^{th}round) can be treated as pre-program data since it is canvassed from July 2007 – June 30, 2008, before any district began implementing RSBY.[7]

2.4.3 Outcomes of Interest

My main outcomes of interest are measures of employment for individual household members. Each NSS survey records the daily activities of all individuals above the age of four in a household. I construct the employment outcome as follows. For each day and each activity, the NSS survey records whether the activity was performed at an intensity of 0, 0.5 or 1 day. For each adult individual I calculate the number of days in a week spent in five activities: (a) private casual work (b) private salaried work (c) private agricultural wage work (d) domestic work and (e) self-employment. Private casual work and private salaried work together constitute private wage work. I also examine private agricultural wage work separately which

[7] Concerns of bias due to inter-district migration is likely to be low. Migrating from a rural district to another rural district for employment is uncommon.

can come under either private casual work or private salaried work. While domestic work could be part of not in labour force, households in India also work in small -scale agriculture which can be classified as domestic work. Additionally, domestic work also includes time spent in caregiving tasks and thus I study this employment measure separately.

2.4.4 District Level Information

To construct district controls I use data from two sources: first, I use individual-level data from the 2002-2004 District Level Household Facility Survey, Wave 2 (DLHS-2) aggregated to the district level. The District Level Household Facility Survey is a nationwide repeated cross-section survey which is representative at the district level. The main district controls from the DLHS-2 are: the proportion of villages in a district connected by a road, proportion of villages in a district with a primary health center, proportion of villages in a district with a government hospital, proportion of villages in a district with a health sub-center and the average distance of a village in a district to the nearest town. I also use the 61^{st} Round of the National Sample Survey (NSS 2004-05) to control for baseline district-level characteristics - fraction of scheduled castes and scheduled tribes, illiteracy rate, male and female labor force participation and fraction living under the poverty line.

2.4.5 Empirical Framework

Previous studies have used several different approaches to identify treatment effects of the RSBY program. In Karan et al. [2017], the authors define the treatment group as all eligible households in RSBY treated districts. On the other hand in

Johnson and Krishnaswamy [2012], the authors use a difference-in-difference strategy on districts that have first been matched on observable characteristics. Finally, Ravi [2012] also use a similar district level difference-in-difference strategy and identify intent-to-treat (ITT) effects.

In this context, I employ two definitions of treatment and controls groups. In the first definition, Sample I, I follow Karan et al. [2017] and classify all 'poor' i.e. below poverty line (BPL) households in districts that implemented RSBY as treated households. While the 61^{st}(2004-05) and 68^{th} (2011-12) rounds of the NSS data enquire about the below poverty line (BPL) status of households, this is not true for the 64^{th} round (2007-08). As pointed out by Karan et al. [2017], the two lowest per capita expenditure quintiles account for about 70% of BPL households.[8] Thus, I use households in the bottom two quintiles of consumer expenditure as a proxy for 'poor' households. When constructing quintiles of asset-based poverty I use the whole NSS sample by year i.e. this distribution is *not* constructed within district. This is because the distribution may vary considerably at the district level, leading to very different compositions of the bottom quintiles across districts.

Given that by 2011-12 the eligible population also includes National Rural Employment Guarantee Scheme (NREGS) workers and other unorganized sector workers, in the second definition, Sample II, I use an expanded sample and include all households in the *bottom two quartiles* of consumer expenditure.

I consider two discrete cut-off points, March 2010 and June 2012 and identify two distinct treatment districts similar to Karan et al. [2017]. These are: (a) poor households in districts which participated in RSBY before March 2010 ('early'

[8] See Appendix Table A-III in Karan et al. [2017] for the percentage distribution of BPL households by quintile groups in 2004-05.

treatment districts) and, (b) those living in districts which began participating between April 2010 and June 2012 ('late' treatment districts). Out of a total of 531 districts there are 191 districts which introduced the policy on or before March 2010 (these will be called the 'early' districts), 200 districts that introduced the policy between April 2010 and June 2012 (these will be called the 'late' districts) and 140 districts that either introduced the program after June 2012 or have yet not introduced the program (these are the 'control' districts). As mentioned before, states such as Tamilnadu, Karnataka and Andhra Pradesh which had similar state run rural health insurance programs during the period under consideration in this study, have been excluded. Appendix Table B.2 details the state-wise number of districts in early and late treatment groups.

State governments decided the order in which RSBY was rolled out at the district level. While it is unlikely that labour supply was one of the factors that impacted the decision to implement the program; it is still possible that program placement might be endogenous. Thus, I compare changes over time between districts that implemented RSBY early ('early' districts) and districts which implemented RSBY later ('late' districts) to 'control' districts. I estimate the following difference-in-difference specification, similar to Karan et al. [2017]:

$$Y_{idt} = \beta_0 + \beta_1 Early_d + \beta_2\ Late_d + \sum_{t=2}^{3} time_t * Early_d * \theta_t + \sum_{t=2}^{3} time_t * Late_d * \psi + time_t + X_{idt}\gamma + Z_d * \mathbf{1}_{\{t>2008\}}\delta + \eta_d + \epsilon_{idt} \quad (2.1)$$

where Y_{idt} is the outcome variable for individual i in district d in year $t =$ 2004-05, 2007-08 and 2011-12. $Early_d$ and $Late_d$ are dummies for early and late districts respectively. $Time_t$are time dummies ($time_2$ corresponds to 2007-08 and $time_3$ corresponds to 2011-12). Z_d are time invariant district controls (detailed in Section 4.4 and summarized in Table 3) and $\mathbf{1}\{t > 2008\}$is a dummy variable equal to 1 after 2008.

X_{idt} includes individual and household controls: dummy variables for age categories 30 to 40, 40 to 50 and older than 50, dummy variables for years of education less or equal to 4, between 5 and 8, between 8 and 12, and equal to 12[9], dummies for scheduled caste and scheduled tribe (omitted category "other"), dummies for Muslim religion or other religion, dummies for single and widowed marital status and household size and month of survey fixed effects. Finally, I include district fixed effects, η_d, and cluster standard errors at the district level.

For early districts which implemented RSBY before March 2010, θ_2 is the pre-intervention differences-in-difference estimate (comparing 2007-08 to the baseline year of 2004-05) and θ_3 is the post-intervention differences-in-difference estimate (comparing 2011-12 to the base line year of 2004-05). Similarly, for the treatment group that joined RSBY between April 2010 and June 2012 ('late districts'), ψ_2 is the pre-intervention differences-in-difference estimate; ψ_3 is the post-intervention differences-in-difference estimate. Thus, the intent-to-treat treatment effect for early districts is $\theta_3 - \theta_2$ and the treatment effect for late districts is, $\psi_3 - \psi_2$.

This is a standard difference-in-difference specification. If unobservable differences between treatment and control districts are correlated to the probability of being treated, then this assumption might not hold. For instance, districts with

[9]I only keep observations of working-age adults (between 18 to 60 years old) for the main analysis. I study the labour supply responses of children between 6-17 years of age separately in Section 2.4.9.

better government implementation infrastructure might be more equipped to diffuse information about the program and consequently enroll more households. Not accounting for differential trends between treatment and control districts with result in over estimation of the effects in the difference-in-difference framework (Heckman and Vytlacil, 2007).

In Figures 2.1 & 2.2 I examine the trends of 'early' districts relative to 'control' districts and of 'late' districts relative to 'control' districts respectively. In Figure 2.1 which compares early and control districts the pre-period comprises 2004-5 to 2007-08; while in Figure 2.2 which compares late and control districts the pre-period comprises 2004-5 to 2009-10. The outcome variables in both figures range from 0 to 7. I find that in both figures, pre-program trends in the main outcome variables seem to be parallel.

In Table 2.2, I also use only the 2004-05 and 2007-08 (pre-program rounds) to run a similar specification to the one presented in equation (2.1) except the program dummy is equal to 1 for 2007-08 (i.e. 2007-08 is taken to be the hypothetical *post* period). In the absence of the program, there should be no significant differences between treated and control districts by future district treatment status. I present results separately for males and females. The results in Table 2 show no significant differences between early and control districts and late and control districts for the main outcome variables (interaction terms *Post*Early Treatment* & *Post*Late Treatment* in the first and third rows of Panels A and B), providing support for using control districts as a comparison group for early and late districts.

Finally, Appendix Table B.3 presents the baseline means in 2004-05 for early, late and control districts for the main outcome variables used in this study. All outcome variables range from 0-7 and are defined as the number of days in the past week spent in a particular activity.[10]

2.4.6 Results: Intent-to-Treat Estimates

My primary interest in this section is to examine the effect of RSBY on the decision to change the number of hours worked in the labour market, among those already employed. Table 2.3 presents the main estimates for sample I (i.e. households in the bottom two expenditure *quintiles*) while Appendix Table B.4 presents the same for sample II (i.e. households in the bottom two expenditure *quartiles*).

In Table 2.3 Panel A, for women, among those who are already employed, RSBY significantly (at the 5% level) increases labour supply by increasing the number of days worked in the past week in private casual work in early districts as compared to control districts (column 2 in Panel A). That is, the number of days worked in a week in private casual work increases by 0.286 days (effects of RSBY for early districts is derived by subtracting the coefficient on *EarlyTreatment*Time2* from the coefficient on *EarlyTreatment*Time3* and similarly for later districts). Mean days spent in private casual work by women at baseline in 2004-05 is 0.45 days in a week. Thus, an impact of 0.286 days is large; time spent in private casual work approximately increases by 50 percent for women. This translates into approximately 15 *more* days of work annually for women in the private labour market.

While the increase in labour supply is large for private casual work; private

[10] It is also important to note that confounding impacts of the National Rural Employment Guarantee Scheme (NREGS) which is a central government program may not be relevant in this context. This is because in the period of interest in this paper this program was already fully rolled out. The NREGS was rolled out in three phases in the country from February 2006 to May 2008.

salaried work (column 4 of Panel A) does not change for women after the introduction of RSBY. This makes intuitive sense since RSBY helps in reducing health related disruptions which would increase labour supply in spot markets and other types of short-term private contractual work which come under private casual work as opposed to private salaried work which includes salaried longer-term regular employment. Late treatment districts also see an increase in private casual work and private salaried work for women; however, these are not significant. This is potentially because information about RSBY, like other government programs is slow to trickle in and thus households in late treatment districts are still unaware of the program.

Women also see an increase in private agricultural wage work and a significant decline in domestic activities (columns 6 & 8 in Table 2.3 Panel A). For instance, number of days in a week spent on private agricultural wage work increases by 0.254 days while it *decreases* by 0.694 days for domestic work. This implies that one of the main mechanisms through which the program is operating is through a reduction in time spent at home in caregiving duties after the introduction of the RSBY. Once again, the effect of RSBY on private agricultural wage work and domestic work is present only in early districts.

The significant increase in labour supply that I find for women in private casual work and private agricultural wage work combined with the significant decline in domestic work provides initial evidence that the RSBY program frees up some time from domestic work in the week for women which allows them to engage in temporary short-term employment. Finally, since *only* early districts see a program impact this is consistent with low enrollment ratios of RSBY documented in Section 2.2.3. That is, information about RSBY is slow to diffuse and impacts are only seen 2-3 years after the program is already in place. Thus, late districts do not witness any impact of the program in 2011-12.

Estimates from Sample II (i.e. households in the bottom three expenditure quintiles) in Appendix Table B.4 reveal the same broad picture. Women witness an increase in labour supply in private casual work and private salaried work and a decline in labour supply in domestic work. All these effects are once again concentrated in the early districts.

For men, in Table 2.3 Panel B, there is an increase in the number of days in the past week spent in the private casual labour market by about 0.198 days. However, this is not statistically significant. This increase in private casual labour is accompanied by a significant decrease in the number of days spent in the last week in early districts on self-employment activities for men. A significant fraction of men in poor countries are self-employed (Datt and Ravallion, 1994). Indeed, according to NSS 2004-05 about 58 percent of the male workforce across the country was self-employed. For those men who were self-employed *either* to be close to home if they were caregivers *or* they were too sick themselves to find outside employment; the introduction of the RSBY should reduce self-employment for both these types.

2.4.7 Female Labour Supply Heterogeneity

In this section, I decompose the labour supply of women by household size. First, in Panel A of Table 2.4, the program effect is allowed to vary by the number of working age members in a household where working age is defined as between 18 and 60 years of age. As a simplification, I use data only from NSS

Rounds 64 & 68 (2007-08 and 2011-12 respectively) in Equation (2.1). Thus, I add the following terms to the regression model described in Equation (2.1): *Working Age*EarlyTreatment*Post; Working Age*LateTreatment*Post; Working Age*EarlyTreatment; Working Age*LateTreatment;* and *Working Age*Post*. Here *Post* refers to the year 2011-12 and *Working Age* refers to the number of working age members (i.e. those in the 18-60 age group) in a household.[11]

The results in Panel A, Table 2.4 examine heterogeneity in labour supply for women by the number of working age members in a household. The interaction term between working age, early program districts and the post period is negative and significant for most private labour supply outcomes for women (private casual work and private agricultural wage work) suggesting that women living in households with more working age members experience a larger increase in labour supply. Specifically, if rural women in early districts experience an average increase in private casual work of 0.650 days in a week (coefficient on *Early Treatment*Post* in column (2.2) in Panel A of Table 2.4) associated with the program; then women who have working age members one standard deviation below the mean experience 0.83 days increase in labour supply, while women who have working age members one standard deviation above the mean experience only an increase of 0.47 days.[12]

These estimates imply that the effect of the program is concentrated amongst households with fewer working age members. There are potentially two explanations

[11] That is, I estimate the following equation: That is, I estimate the following equation: $Y_{idt} = \beta_0 + \beta_1 Early_d + \beta_2 Late_d + \theta Early_d * Post + \psi Late_d * Post + \delta_1 WorkingAge * Early_d * Post + \delta_2 WorkingAge * Late_d * Post + \delta_3 WorkingAge * Early_d + \delta_4 WorkingAge * Late_d + \delta_5 WorkingAge * Post + Post + X_{idt}\gamma + \eta_s + \epsilon_{idt}$

[12] Given a standard deviation of 1.71 for the number of working age members, the effect of a one-standard deviation decrease in working age members is -0.179. The total effect is thus: 0.650 – (-0.179) = 0.83. Similarly, the total effect associated with an increase in working age members is: 0.650 + (-0.179) = 0.47.

for this. One, the higher the number of working age members, the less likely it is that the household as a whole is affected by one person's illness. In smaller households on the other hand, where the number of working age members is low, even one person being ill can impact the labour supply of women adversely. If RSBY impacts labour supply by improving health and health utilization then smaller households stand to benefit more from it. Second, the larger the household, the less are the number of hours spent on caregiving by any one person and hence the smaller is the impact of RSBY on household labour supply. Thus, RSBY has a bigger potential to impact labour supply for women in smaller households. In addition, heterogeneity in program response according to household size acts as a robustness check that unobserved factors are not driving the results. This is because, one would assume that poverty increases with household size, and we see that program effect decreases with the number of members in the household.

Panel B of Table 2.4 examines heterogeneity in labour supply for women by the number of dependents in a household. Dependents are defined as those younger than 18 years *or* older than 65 years in a household. I add similar interaction terms to those described above for working age members: *Dependents*EarlyTreatment*Post; Dependent*LateTreatment*Post; Dependent*Early Treatment; Dependent*LateTreatment and Dependent*Post*. The interaction term between dependent, early program districts and post period is not significant for any of the private labour supply outcomes. However, the interaction term is positive suggesting once again that households with more dependents experience a larger *increase* in private labour supply. Specifically, if rural women in early districts experience an average increase in private casual work of 0.223 days in a week (coefficient on *Early Treatment*Post* in column (2) in

Panel B of Table 2.4) associated with the program, then women who have dependents one standard deviation below the mean experience 0.19 days increase in labour supply, while women who have dependents one standard deviation above the mean experience an increase of 0.26 days.[13] These estimates while not significant, provide further evidence that the decrease in domestic labour supply of women and a concomitant increase in private labour supply is consistent with the hypothesis that in the post-RSBY period, caregiving tasks for women decreased. That is, women with fewer working age members in the household and/or a higher number of dependents witness larger program impacts.

2.4.8 Intensity of Treatment

As a further test of the robustness of my empirical specification I also estimate how the length of time a household has been exposed to RSBY affects labour market outcomes. The number of days elapsed between the start of the policy and the date of the NSS survey in a district varies substantially across households since each survey round took a year to complete. The longer the length of time between the start of the RSBY policy and the survey date, the higher the likelihood that someone may have had the need for hospitalization and utilized RSBY for the same. There are several other reasons why the impact of RSBY may vary with the time since implementation. First, in many districts, awareness of RSBY has only spread gradually and knowledge about the program was been extremely low in the first few

[13] Given a standard deviation of 2.7 for the number of dependents, the effect of a one-standard deviation decrease in dependents is 0.035. The total effect is thus: $0.223 - (0.035) = 0.19$. Similarly, the total effect associated with an increase in dependents is: $0.223 + (0.035) = 0.26$.

months after initial rollout despite households being given smart cards.[14] Second, hospitals may take time to understand and become familiar with the scheme.

I estimate the impact of the duration of RSBY exposure using a modified difference-in-difference model. The degree of RSBY exposure in a district is likely to be exogenous and should not be correlated to other district characteristics since I define *intensity* as the years since the program was implemented in the district. Specifically, I estimate the following:

$$Y_{idt} = \beta_0 + \beta_1 RSBYIntensity_d + \sum_{t=2}^{3} time_t * RSBYIntensity_d * \theta_t + \\ X_{idt}\gamma + Z_d * \mathbf{1}_{t>2008}\delta + \eta_s + \epsilon_{idt} \quad (2.2)$$

where $RSBYIntensity_d$ is defined as the number of years of RSBY exposure in district d. All other variables are defined as before. The estimated average program effect in this case, is the coefficient on (*RSBYIntensity*Time3*) – coefficient on (*RSBYIntensity*Time2*).

Table 2.5 examines if the treatment effect of RSBY is larger when the number of years of exposure to RSBY is higher. As expected, the results show that men and women who face more years of exposure to the RSBY program have larger program effects. A one standard deviation increase in RSBY exposure raises the labour supply of women in the private casual and private agricultural wage market by 0.034 to 0.043 days in the past week (columns (2) and (6) in Panel A). Increased RSBY exposure also decreases time spent in the last week by women in domestic

[14] For instance, Johnson and Kumar (2011) find that a large proportion of enrolled households are only vaguely aware of the purpose of the RSBY smart card.

work by 0.17 days. Men also witness an increase in private casual work and private agricultural wage work (significant at the 10% level) as RSBY exposure increases. Additionally, a one standard deviation increase in RSBY exposure decreases self-employment for men by 0.102 days in the past week (significant at the 5% level). Thus, the results in this section provide support for the main identification strategy outlined in Section 2.4.

2.4.9 Labour Force Participation of Children Aged 6-17 years

Labour force participation of dependents can be impacted in two ways as a result of RSBY. One, following from the luxury and substitution axioms of the Basu and Van (1998) model of child labour supply, in which children can substitute for adults in the labour market, a family will send children to the labour market only if the family's income from non-child labour sources falls below some threshold amount. Thus, if the program leads to income security for households then one would expect that time spent by children in the labour market should decrease. Second, in general adults have a comparative advantage in home production (i.e. in caregiving tasks). That is, in the absence of health insurance, children will substitute for adults in the labour market. In such a case, if total household labour hours rise with the provision of health insurance, then labour hours of children should fall and time spent at school should rise.

However, existing evidence on the impact of RSBY on reducing financial burden due to health expenditures is mixed and a higher proportion of studies find that RSBY did not significantly contribute to raising the income security of households. In this scenario then, the mechanism that is driving the labour supply impacts of

RSBY is an increase in healthcare *utilization*. In this case, one would expect children to substitute towards activities which adult men and women are moving away from such as domestic work and self-employment. Thus, the overall impact of RSBY on the labour supply of children and time spent on education activities is ambiguous.

Table 2.6 presents estimates of the fraction of days spent by children in different activities in a week. That is, I estimate Equation (2.1) but for children from 6-17 years of age. I find that the average number of days spent in the last week in early districts in the post-RSBY period in self-employment and domestic activities significantly increases for children. On the other hand, the time spent in educational institutions sees a significant decrease. For instance, children spend 0.601 less days in the past week in school and while they spend about 0.243 days more on self-employment and domestic activities combined. This implies that after the availability of free health insurance, in early districts, children are substituting *into* the activities like domestic work and self-employment that their parents are moving *out* of. Thus, availability of health insurance does not appear to have an income effect for child labour but does seem to have a substitution effect with respect to time. This is in line with existing evidence of RSBY which show that the program did not significantly impact out-of-pocket expenditures on health care while significantly *increasing* healthcare utilization.

2.5 Average Treatment Effect on the Treated

The Intent-to-treat (ITT) estimates, presented up to now, can approximate the Average Treatment Effect on the Treated (ATT) in the scenario where enrolment ratios are high. Since the national enrollment ratio is about 57%, the ITT parameter might not be very informative. While most studies on RSBY provide ITT estimates of the program, Raza et al. [2016] and Azam [2016] provide Average Treatment on the Treated estimates. In this section I follow the methodology outlined in Azam [2016] and evaluate the labour market impacts of RSBY on *beneficiary* households using the nationally representative Indian Human Development Survey (IHDS).

2.5.1 Data

The IHDS is a national survey, representative at the national, state and district level, with a sampling frame similar to the NSS. There are 2 waves of the survey, collected in 2004-2005 and 2011-2012 (henceforth, 2005 and 2012, respectively). There were 42,152 households interviewed in 1503 villages in IHDS-2. Like before, I drop Andhra Pradesh, Karnataka, and Tamil Nadu from the sample since these states had state-run health insurance programs operating at the same time as RSBY was implemented. Similarly, like before, I restrict my analysis to rural areas.

Identification in this piece of the analysis comes from utilizing household's response to a direct question in the 2012 IHDS survey about having the RSBY card. Approximately 13 percent of households had an RSBY card in the IHDS-2. The official percentage pf population covered under RSBY was about 13.6 percent in 2012. Thus, the IHDS-2 captures RSBY exposure well (Ministry of Labour & Employment, 2012).

Similar to the NSS, the IHDS defines labour force participation through the following 4 work categories: (1) private casual work (2) private salaried work (3) business work (4) family farm work. There are two ways in which the labour supply variables defined in this section differ from the analysis using NSS data – (a) the

IHDS does not record time spent in "domestic activities" and "self-employed" separately and, (b) I include time spent on the family farm and on business activities as separate work categories, which were not available in the NSS.

2.5.2 Empirical Framework

I use the longitudinal nature of the IHDS data to estimate a matching difference-in-difference strategy. The main advantage of such estimators relative to alternative methods used in the presence of non-experimental data relies on their semi-parametric nature, allowing the estimation of treatment effects without imposing restrictive distributional assumptions to the data generating process.

Following the methodology outlined in Azam [2016], I first start with the following simple model:

$$Y_{iht} = f(x_{it}) + \beta_1 RSBY_{ht} + \psi_t + \eta_h + \epsilon_{iht} \tag{2.3}$$

where Y_{iht} is the labour market outcome for individual i in household h at time t; η_h are time invariant household level characteristics and ψ_t are year fixed effects. The treatment variable is $RSBY_{ht}$ which takes the value 1 for households that were enrolled in the program (i.e. beneficiary households who had an RSBY card) in the post-time period i.e. 2011-12. That is, the variable takes the value 0 for all households in 2004 and takes the value 1 in 2012 for those households who were covered under the program. The effect of the program is given by β_1.

The expectation of the difference between pre- and post-period changes in RSBY beneficiary households (given by 'T') and pre- and post-period changes in non-RSBY households (given by *'C'*) is:

$$E(\mathit{\Delta} Y^T_{ih}) - E(\mathit{\Delta} Y^C_{ih}) = E[f(\mathit{\Delta} x^T_{ih})] - E[f(\mathit{\Delta} x^C_{ih})] + \beta_1 + [\mathit{\Delta}\psi^T - \mathit{\Delta}\psi^C] + E(\mathit{\Delta}\varepsilon^T_{ih}) - E(\mathit{\Delta}\varepsilon^C_{ih}) \quad (2.4)$$

There are three assumptions required to recover the impact of RSBY on labour supply in this framework as outlined in Azam [2016]: First, differences in observables between the treated and control households should be zero. By carefully matching each treated household (those who have the RSBY card i.e beneficiary) with one or more control households (those who didn't have the RSBY card) to ensure that the matched sample is similar with respect to observable characteristics, the first term of Equation 2.4 i.e. $E[f(\mathit{\Delta} x^T_{ih})] - E[f(\mathit{\Delta} x^C_{ih})]$ can be eliminated.

The second assumption is that $\mathit{\Delta}\psi^T = \mathit{\Delta}\psi^C$. Since longitudinal data is used in this piece of the analysis i.e. the same household is observed for both time periods, this assumption seems reasonable. That is, the composition of treated households and control households does not change in the pre- and post-program periods. This combined with the fact that under assumption (1) detailed above, treatment and control households have been matched so that they are very similar with respect to observables, implies that the trend in the aggregate shock between treatment and control groups could plausibly be the same.

Finally, the third assumption is that $E(\mathit{\Delta}\varepsilon^T_{ih}) = E(\mathit{\Delta}\varepsilon^C_{ih})$. For instance, it is possible that there is some adverse selection wherein households who are more vulnerable to health shocks could potentially be *more likely* to enroll in the program, while relatively healthier households might be *less likely* to enroll. As pointed out by Azam [2016] some features of the RSBY program helps mitigate concerns of time varying idiosyncratic shocks to some extent. The average enrollment costs under RSBY are very low, about Rs. 30 per *year* and there are no insurance premiums. Given that the average *daily* wage rate in rural areas is about Rs. 140, this implies that all households, irrespective of health-related vulnerabilities should be equally likely to enroll into the program.

Now, there are two potential control groups in this setting – (a) non-RSBY households in all districts in the country and, (b) non-RSBY households in only those districts that implemented the program by 2012. I follow Azam [2016] and restrict the analysis to only those districts that had implemented the program by 2012. That is, the control group includes only those households without an RSBY card who resided in districts that implemented the program by 2012 (henceforth, RSBY exposed districts). The intuition behind choosing this control group is straightforward – the time specific aggregate shock is more likely to exhibit a similar trend for treatment and control households residing in the same geographic area rather than for households residing in different geographic areas.

Following Blundell and Dias [2009], for longitudinal data the matching difference-in-differences (MDID) estimator is given by:

$$\alpha^{MDIM,L} = \sum_{j \,\epsilon\, T} \{[Y_{i1} - \; Y_{i0}] \;-\; \sum_{j \,\epsilon\, C} w_{ij}[Y_{j1} - \; Y_{j0}]\} \; w_i \tag{2.5}$$

where T and C are defined as treated and control households respectively. w_{ij} is defined as the weight on a control household j for the treated household i (Blundell and Dias, 2009). Thus, I compare pre- and post-program labour market outcomes between treated and control households in RSBY districts, after controlling for baseline differences with matching.

Using the baseline data, Appendix Table B.5 reports summary statistics for the main outcome variables. Panel A reports the intensive margin of labour supply variables (number of hours worked in a day) and Panel B reports the extensive margin (indicator variables for labour force participation). At both the extensive and intensive margins, RSBY households are more likely to be engaged in private casual work and private agricultural wage work and are also more likely to spend more hours in a day engaged in these types of work (these differences are not statistically significant).

2.5.3 Matching

Appendix Table B.6 reports the descriptive statistics of household level characteristics (from the baseline 2005 data) which might potentially be correlated with RSBY status. Column (1) reports the means for non-RSBY households and column (2) reports means for RSBY households. In general, RSBY card holder households have lower income and consumption and a larger proportion of RSBY households are below the poverty line. Similarly, a larger proportion of RSBY household members report short term illness in the past 30 days and a higher number of per capita days being unable to work in the past month due to an illness.

To estimate the propensity score, $P(T = 1|X) = P(X)$, i.e. the probability of a household participating in RSBY given their observed covariates X, I use the household characteristics described in Appendix Table B.6 and run a probit model. The dependent variable is whether a household had an RSBY card in 2011. The probit model is run using independent variables from only the baseline 2005 data. Since RSBY was not available in 2005, the independent variables could not have been affected by RSBY enrollment. The model also includes district fixed effects. The results from the probit model are presented in Appendix Table B.7. As expected, log per capita consumption and log income are negatively correlated with having an RSBY card i.e. relatively richer households are less likely to enroll into the program. Similarly, households where the household head has a formal government job are less likely to enroll into RSBY. Socially disadvantaged groups like Scheduled Caste and Scheduled Tribe households are more likely to enroll into the program. Households without access to basic services like electricity and flush toilet are more likely to enroll into the program. Additionally, households with good social networks such as being part of a self-help group and attending local body meetings are more likely to enroll into RSBY. None of the coefficients on prior household health status such as proportion of household members with a short term or long-term illness, per capital inpatient and outpatient expenditure and per capita hospital days predict RSBY enrollment. Thus, this provides evidence in favor of potentially low adverse selection and the validity of third assumption in the identification strategy.

Sub-setting the full sample of households to those with a common support of the propensity score reduces the number of households to 19,906 (out of 21,943).

I use kernel matching as my main matching method. Figure 2.4 compares the standardized bias before and after matching. The standardized median (mean) bias before matching is 6.6 (7.9) percent and after matching is 0.9 (1.0) percent indicating that the matching was effective at creating a good control group with balanced covariates.

2.5.4 Results: ATT Estimates

Appendix Table B.8 presents the results. Column (3) presents the ATT estimates while column (4) depicts the baseline average for RSBY households. The results are larger than the ITT estimates presented in Section 2.4. In Panel A column 1, for women belonging to RSBY households in RSBY exposed districts, among those already employed, the number of days spent in the past week in private casual work increases by 0.335 days. Mean days spent in private casual work by women in RSBY households at baseline in 2004-05 is 0.41 days in a week. Thus, an impact of 0.335 days is large; time spent in private casual work approximately increases by 81 percent for women (column 3, Panel A), compared to a 50 percent increase in the ITT analysis. Number of days in the past week spent in business work also increases for women by almost 50 percent compared to baseline 2004 levels (column 3, Panel A).

In contrast to the ITT estimates, there is also a significant increase for men in private casual labour. The number of days in the past week spent in the private casual labour market increases by 0.373 days, which corresponds to a 20 percent increase over the baseline mean. While the absolute increase in the number of days in private casual labour is the same for both men and women, given the large differences in their baseline levels, the increase for women is much larger.

2.6 Impact on Health and Health Care Utilization

One of the primary mechanisms through which labour supply could increase after the introduction of the RSBY is through effects on own health and health care utilization. Theoretically, better health leads to an increase in worker productivity. Dow et al. [1997] find that in Indonesia, a reduction in access to health services reduces objective heath indicators, such as activities of daily living, as well as female labour force participation. Further, Baicker et al. [2014] find positive impacts of Medicaid on health care use as well as on self-reported general health, even though they don't find impacts of Medicaid on employment of low income adults. Increased access to insurance could also lead to increased financial security. While Karan et al. [2017] do not find an effect of the program on out of pocket expenditures, Azam [2016] finds that health utilization rates go up. This implies that the program helps in preventing families from becoming impoverished because of a health shock. That is, in the absence of the insurance, treatment for an illness would not have been possible or would have been prohibitively expensive. However, after the program, households need to only spend the amount that exceeds the insurance cap amount (Rs. 30,000). This implies that one would not see any impact on out-of-pocket expenditures, even though families are better off now, if their total healthcare bill exceeds the insurance cap. However, an increase in health care utilization rates would indicate that people are *actually using* the program. This is what I examine next.

While Azam [2016] provides some of the first estimates of the program on health care utilization, he excludes districts that have not been treated by 2010 from his analysis. In the following analysis, I define treatment and control groups as detailed in Section 2.4 i.e. 'control' districts received the program after June 2012 or have not yet received it, 'early' districts are those received program before March 2010 and 'late' districts are those that received program between April 2010 and

June 2012. While the NSS data lacks information on health outcomes, I use the Indian Human Development Survey (IHDS), described in the previous section, to examine these effects.

I use IHDS Waves I (2004-2005) and II (2011-2012) for my analysis with IHDS-I forming the pre-program period and IHDS-II forming the post-program period. More specifically, I estimate the following specification:

$$Y_{idt} = \beta_0 + \beta_1 Early_d + \beta_2 Late_d + \theta Early_d * Post + \psi Late_d * Post + time_t + X_{idt}\gamma + Z_d * \mathbf{1}_{\{t>2008\}}\delta + \eta_s + \epsilon_{idt} \quad (2.6)$$

Here *Post* refers to 2011-12 (IHDS-2). All other variables are defined as in Equation 1. Since there is only one pre-program round of data, θ is the differences-in-difference estimate (comparing 2011-12 to the base line year of 2004-05) for early districts and similarly ψ is the differences-in-difference estimate for late districts. The sample, like before is restricted to individuals in the 18-60 years age group. The main dependent variables I examine are: body mass index (BMI), short term morbidity (number of days in a month an individual is unable to work because of a short-term illness such as a fever or cough), long term morbidity (number of days in a month an individual is unable to work because of a long-term illness), number of days ill because of a short-term morbidity and an indicator for seeking formal care for a major morbidity.

Table 2.7 examines the physical health outcomes and healthcare utilization patterns from the above specification. I find that access to RSBY significantly improves women's body mass index (BMI) by 0.737 points in early districts and by 0.87 points in late districts (column 2, Panel A). Men see an insignificant increase in BMI. Both men and women see a decrease in the number of days in a month affected by a short-term morbidity such as a fever or cough (column (3) in Panels A and B). Surprisingly, I find that short term morbidity is also impacted positively by the program. This could be explained by the fact that greater income security makes households better equipped to handle short term morbidities.

Importantly, the number of days in a month that *both* men and women are unable to work because of a *major* morbidity decreases significantly in both early and late districts (column (8) in Panels A and B). For women, the decline in early districts is almost 3.5 days in a month, while the decline for men is about 2.3 days in a month. That is, men and women *gain* an additional 2.3 and 3.5 days in a month respectively, because they can now get treatment for their major morbidities under RSBY. This is an important contribution of the program. By enabling the poor to access cashless hospitalization in hospitals of their choice means that acutely sick individuals can get care in time as opposed to when it's too late. Seeking formal care for major morbidities is thus an important mechanism through which labour supply is affected.

Days unable to work because of a short-term morbidity does not significantly change possibly because short term morbidities are not debilitating enough to take time off work. Finally, for both men and women healthcare utilization goes up significantly. I also find that the program impacts the number of days of hospitalization, for men (significant at the 10% level).

Thus, overall, an increase in health and health care utilization, in addition to a decrease in caregiving tasks and self-employment activities documented earlier, seem to be the main mechanisms driving the labour supply impacts of the rural health insurance program. That is, the RSBY program enables all members of the household (men and women) to get treated for major morbidities. This in turn, decreases the time they're unable to work because of major morbidities *and* decreases the time a caregiver needs to spend at home.

2.7 Conclusion

Many developing countries are beginning to experiment with free or subsidized health insurance for the poor. Understanding the general equilibrium effects of such policies particularly on labour supply is an important step towards designing effective policies. In this paper, I use the phased implementation of a rural health insurance scheme in India and apply a difference-in-difference design to examine the link between health insurance and labour supply. My main finding is that the program, *Rashtriya Swasthya Bima Yojana (RSBY)* increases the number of hours spent in the private labour market per week for both men and women, but the increase for women is disproportionately larger. I provide some suggestive evidence that this increase is due to an increase in health care utilization as well as a decrease in domestic caregiving tasks.

More specifically, I find that individuals are significantly more likely to seek formal care for major morbidities after the program. Time spent out of the labour force because of a major morbidity declines. Further, I find that there is a significant reduction in time spent on domestic tasks in a week for women which implies that the labour supply results for women are driven by a decrease in domestic caregiving tasks *as well as* a decline in time spent being unable to work because of a major morbidity.

Overall, this study finds that rural health insurance programs can have important beneficial spillover effects on labour supply. Most importantly, I find that women stand to gain the most from any improvements in the health care system in India. That is, household level health shocks impact women disproportionately *more* than men because their domestic duties increase. Rural health insurance, by allowing households to access healthcare more easily allows women to spend more hours on the labour market which leads to increases in overall welfare.

Table 2.1: Health in Past Four Weeks

	Control Districts	Early Districts	*p-value*	Control Districts	Late Districts	*p-value*
Panel A: 0-5 years						
Days Ill - ST Morbidity	5.704	6.930	0.096	5.704	7.053	0.071
Proportion with Fever	0.853	0.887	0.505	0.853	0.838	0.786
Proportion with Cough	0.701	0.768	0.310	0.701	0.766	0.351
Proportion with Diarrhoea	0.265	0.264	0.990	0.265	0.402	0.058
Panel B: 6-17 years						
Days Ill - ST Morbidity	5.744	5.995	0.729	5.744	6.609	0.256
Proportion with Fever	0.876	0.890	0.760	0.876	0.851	0.644
Proportion with Cough	0.639	0.755	0.091	0.639	0.723	0.251
Proportion with Diarrhoea	0.151	0.087	0.192	0.151	0.283	0.034
Days unable to work - ST Morbidity	3.723	3.371	0.574	3.723	4.233	0.463
Days unable to work - LT Morbidity	0.447	0.620	0.907	0.447	0.712	0.863
Panel C: 18-55 years						
Days Ill - ST Morbidity	6.788	8.036	0.172	6.788	8.140	0.162
Proportion with Fever	0.879	0.894	0.765	0.879	0.821	0.284
Proportion with Cough	0.591	0.686	0.182	0.591	0.641	0.507
Proportion with Diarrhoea	0.144	0.104	0.422	0.144	0.289	0.020
Days unable to work - ST Morbidity	4.500	4.534	0.967	4.500	5.262	0.392
Days unable to work - LT Morbidity	2.064	2.986	0.769	2.064	2.652	0.850
Panel D: 55+ years						
Days Ill - ST Morbidity	8.132	9.464	0.214	8.132	8.830	0.519
Proportion with Fever	0.878	0.885	0.886	0.878	0.832	0.417
Proportion with Cough	0.682	0.764	0.237	0.682	0.696	0.853
Proportion with Diarrhoea	0.174	0.122	0.353	0.174	0.306	0.048
Days unable to work - ST Morbidity	5.713	6.225	0.616	5.713	6.396	0.524
Days unable to work - LT Morbidity	9.016	15.555	0.396	9.016	10.299	0.857

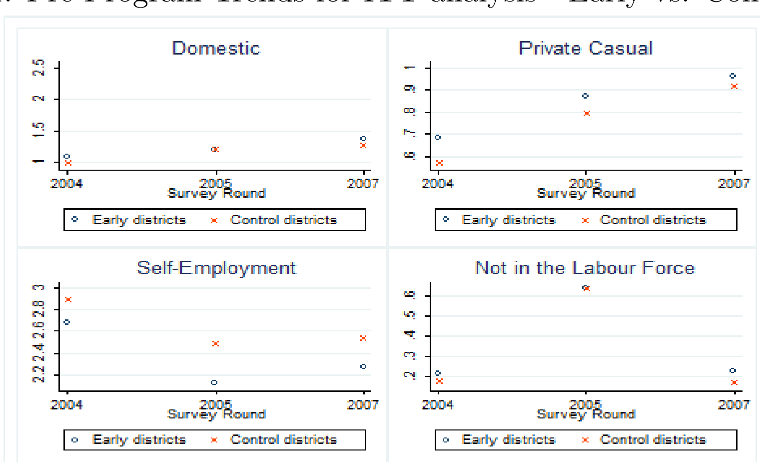
Note: Data is from the Indian Human Development Survey (2004-05). 'ST' refers to short-term, 'LT' refers to long-term. Columns (1) & (4) are restricted to control districts that received the program after June 2012 or have not yet received it. Column (2) is restricted to 'early' RSBY districts (received program before March 2010). Column (5) includes only districts that received the program between April 2010 and June 2012.

Table 2.2: Pre-program tests using NSS 61st and 64th Rounds

	Private Casual Work		Private Salaried Work		Private Agricultural Wage Work		Domestic Work		Self-Employed	
Panel A: Females										
Post*Early Treatment	-0.014	-0.134	0.035	0.028	0.059	0.043	0.049	0.182	0.034	0.061
	(0.0878)	(0.0904)	(0.0370)	(0.0376)	(0.0465)	(0.0493)	(0.1734)	(0.1824)	(0.1309)	(0.1419)
Post*Late Treatment	-0.167	-0.166*	0.035	0.041	0.038	0.037	-0.022	-0.007	0.201	0.186
	(0.0856)	(0.0890)	(0.0370)	(0.0360)	(0.0431)	(0.0505)	(0.1749)	(0.1875)	(0.1300)	(0.1348)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	71506	69869	71506	69869	71506	69869	71506	69869	71506	69869
Panel B: Males										
Post*Early Treatment	-0.056	-0.182	-0.063	-0.063	-0.094	-0.2	-0.014	-0.032	0.102	0.282
	(0.1542)	(0.1479)	(0.0649)	(0.0657)	(0.1478)	(0.1413)	(0.0227)	(0.0241)	(0.1389)	(0.1457)
Post*Late Treatment	-0.014	-0.14	0.097	0.147	-0.092	-0.269*	-0.028	-0.026	0.137	0.212
	(0.1552)	(0.1564)	(0.0604)	(0.0647)	(0.1578)	(0.1551)	(0.0334)	(0.0332)	(0.1429)	(0.1403)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	69809	67687	69809	67687	69809	67687	69809	67687	69809	67687

Note: *Post* indicates the hypothetical post round 2007-08. Regressions include state and month of interview fixed effects. District controls include fraction ST and SC, fraction illiterate, female and male employment rates, and fraction under poverty line separately for the whole district and for the rural areas of the district - calculated from NSS 2004-05. Also includes: the proportion of villages in a district connected by a road, proportion of villages in a district with a primary health center, proportion of villages in a district with a government hospital, proportion of villages in a district with a health sub-center and the average distance of a village in a district to the nearest town calculated from DLHS 2002-04. Individual controls include dummies for age 30 to 40, 40 to 50, and greater than 50, dummies for years of education under 4, between 5 and 8, between 8 and 12, and 12, as well as marital status, and household size. Standard errors in parentheses, clustered at the district level. Number of districts=531. * denotes significance at the 10% level, ** denotes significance at the 5% level, and *** denotes significance at the 1% level.

Figure 2.1: Pre-Program Trends for ITT analysis - Early vs. Control Districts



Note: NSS 61, 62 and 64 rounds. 'Control' districts - received the program after June 2012 or have not yet received it; 'Early' districts - received program before March 2010.

Table 2.3: Difference-in-differences - Individual Level Results

	Private Casual Work		Private Salaried Work		Private Agricultural Wage Work		Domestic Work		Self-Employed	
Panel A: Females	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early Treatment*Time3	0.181**	0.199**	0.05	0.055	0.144*	0.153**	-0.481**	-0.489**	-0.131	-0.103
	(0.0819)	(0.0838)	(0.0360)	(0.0377)	(0.0747)	(0.0774)	(0.2000)	(0.2063)	(0.1508)	(0.1556)
Early Treatment*Time2	-0.042	-0.087	0.031	0.018	-0.059	-0.101	0.178	0.205	0.022	0.051
	(0.0781)	(0.0826)	(0.0356)	(0.0375)	(0.0781)	(0.0825)	(0.1814)	(0.1844)	(0.1275)	(0.1281)
Late Treatment*Time3	0.006	0.051	0.026	0.043	0.057	0.09	-0.338	-0.364	0.104	0.099
	(0.0844)	(0.0862)	(0.0377)	(0.0388)	(0.0781)	(0.0799)	(0.2496)	(0.2532)	(0.1542)	(0.1608)
Late Treatment*Time2	-0.202**	-0.244**	0.03	0.037	-0.195**	-0.238**	0.058	0.074	0.199	0.205
	(0.0773)	(0.0815)	(0.0355)	(0.0371)	(0.0752)	(0.0796)	(0.2009)	(0.1991)	(0.1269)	(0.1287)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	98231	95250	98231	95250	98231	95250	98231	95250	98231	95250
Proportional Selection δ (Early Districts)		[-4.895]		[0.552]		[-1.454]		[-6.491]		[0.602]
Panel B: Males	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early Treatment*Time3	0.123	0.14	-0.014	-0.031	0.189	0.195	0.023	0.027	-0.340*	-0.339**
	(0.1715)	(0.1675)	(0.0647)	(0.0641)	(0.1728)	(0.1671)	(0.0285)	(0.0294)	(0.1747)	(0.1670)
Early Treatment*Time2	0.019	-0.054	-0.061	-0.078	-0.085	-0.15	-0.013	-0.01	0.079	0.154
	(0.1477)	(0.1459)	(0.0654)	(0.0645)	(0.1446)	(0.1467)	(0.0225)	(0.0231)	(0.1381)	(0.1302)
Late Treatment*Time3	0.042	0.096	0.121**	0.119*	0.182	0.217	-0.042	-0.043	-0.184	-0.21
	(0.1869)	(0.1851)	(0.0601)	(0.0615)	(0.1943)	(0.1867)	(0.0346)	(0.0353)	(0.1864)	(0.1835)
Late Treatment*Time2	-0.111	-0.235	0.1	0.130**	-0.089	-0.185	-0.028	-0.027	0.128	0.165
	(0.1505)	(0.1500)	(0.0610)	(0.0614)	(0.1551)	(0.1552)	(0.0334)	(0.0339)	(0.1431)	(0.1379)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	96624	93156	96624	93156	96624	93156	96624	93156	96624	93156
Proportional Selection δ (Early Districts)		[0.938]		[-0.171]		[-0.365]		[0.090]		[1.740]

Note: Estimates from Equation (2.1) for Sample I. Regressions includes state and month of interview fixed effects. District controls include fraction ST and SC, fraction illiterate, female and male employment rates, and fraction under poverty line separately for the whole district and for the rural areas of the district - calculated from NSS 2004-05. Also includes: the proportion of villages in a district connected by a road, proportion of villages in a district with a primary health center, proportion of villages in a district with a government hospital, proportion of villages in a district with a health sub-center and the average distance of a village in a district to the nearest town calculated from DLHS 2002-04. Individual controls include dummies for age 30 to 40, 40 to 50, and greater than 50, dummies for years of education under 4, between 5 and 8, between 8 and 12, and 12, as well as marital status, and household size. Standard errors in parentheses, clustered at the district level. Number of districts=531. * denotes significance at the 10% level, ** denotes significance at the 5% level, and *** denotes significance at the 1% level.

Figure 2.2: Pre-Program Trends for ITT analysis - Late vs. Control Districts

Note: NSS 61, 62 and 64 rounds.'Control' districts - received the program after June 2012 or have not yet received it; 'Late' districts that received the program between April 2010 and June 2012.

Table 2.4: Difference-in-difference for Women - by Working Age Members and Dependents

	Private Casual Work		Private Salaried Work		Private Agricultural Wage Work		Domestic Work		Self-Employed	
Panel A: Heterogeneity by Working Age Members	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Working Age*Early Treatment*Post	-0.106**	-0.105**	0.027	0.028	-0.082*	-0.082*	0.025	0.045	-0.015	-0.025
	(0.0437)	(0.0454)	(0.0217)	(0.0221)	(0.0436)	(0.0455)	(0.0964)	(0.1007)	(0.0619)	(0.0664)
Working Age*Late Treatment*Post	-0.152**	-0.163**	0.005	0.006	-0.125**	-0.139**	-0.052	-0.059	0.074	0.073
	(0.0440)	(0.0460)	(0.0198)	(0.0201)	(0.0437)	(0.0457)	(0.0988)	(0.1044)	(0.0672)	(0.0718)
Early Treatment*Post	0.634**	0.650**	-0.078	-0.061	0.525**	0.531**	-0.771*	-0.837**	-0.116	-0.083
	(0.2004)	(0.1956)	(0.0808)	(0.0789)	(0.1984)	(0.1967)	(0.3951)	(0.3925)	(0.2172)	(0.2198)
Late Treatment*Post	0.798**	0.847**	-0.023	-0.016	0.741**	0.801**	-0.256	-0.244	-0.347	-0.345
	(0.2037)	(0.2021)	(0.0755)	(0.0730)	(0.2005)	(0.2002)	(0.4104)	(0.4149)	(0.2215)	(0.2267)
Working Age*Early	0.071**	0.086**	-0.024*	-0.028**	0.046	0.051	-0.007	-0.029	-0.011	-0.009
	(0.0324)	(0.0325)	(0.0127)	(0.0138)	(0.0327)	(0.0330)	(0.0508)	(0.0501)	(0.0403)	(0.0423)
Working Age*Late	0.088**	0.101**	-0.011	-0.009	0.074**	0.084**	0.019	0.002	-0.039	-0.032
	(0.0305)	(0.0304)	(0.0138)	(0.0152)	(0.0304)	(0.0308)	(0.0574)	(0.0568)	(0.0384)	(0.0402)
Working Age*Post	0.159**	0.151**	-0.012	-0.009	0.124**	0.120**	-0.013	-0.013	-0.04	-0.031
	(0.0353)	(0.0368)	(0.0170)	(0.0171)	(0.0358)	(0.0376)	(0.0796)	(0.0826)	(0.0521)	(0.0561)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	62213	60233	62213	70327	62213	60233	62213	60233	62213	60233
Panel B: Heterogeneity by Dependents	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependents*Early Treatment*Post	0.012	0.013	0.001	-0.006	-0.005	-0.005	0.05	0.1	0.024	0.008
	(0.0345)	(0.0363)	(0.0192)	(0.0181)	(0.0321)	(0.0344)	(0.0728)	(0.0750)	(0.0574)	(0.0606)
Dependents*Late Treatment*Post	-0.003	-0.003	-0.002	-0.009	-0.007	-0.01	0.012	0.043	0.021	0.007
	(0.0353)	(0.0375)	(0.0186)	(0.0177)	(0.0345)	(0.0371)	(0.0852)	(0.0891)	(0.0566)	(0.0605)
Early Treatment*Post	0.181	0.223	0.023	0.054	0.205	0.229*	-0.822**	-0.922**	-0.235	-0.204
	(0.1430)	(0.1390)	(0.0639)	(0.0609)	(0.1378)	(0.1356)	(0.2619)	(0.2566)	(0.1846)	(0.1804)
Late Treatment*Post	0.263**	0.322**	0.002	0.029	0.313**	0.366**	-0.464	-0.535*	-0.184	-0.161
	(0.1306)	(0.1270)	(0.0638)	(0.0615)	(0.1266)	(0.1256)	(0.2970)	(0.2921)	(0.1852)	(0.1837)
Dependent*Early	-0.037*	-0.036	0.008	0.006	-0.032	-0.036*	0	-0.014	-0.049	-0.053
	(0.0223)	(0.0222)	(0.0106)	(0.0097)	(0.0220)	(0.0220)	(0.0450)	(0.0463)	(0.0339)	(0.0344)
Dependent*Late	0.025	0.026	0	0.003	0.018	0.018	-0.009	-0.03	-0.055	-0.058
	(0.0235)	(0.0235)	(0.0105)	(0.0100)	(0.0234)	(0.0240)	(0.0517)	(0.0539)	(0.0358)	(0.0357)
Dependent*Post	0.013	0.006	-0.006	-0.001	0.023	0.017	0.042	-0.007	-0.056	-0.038
	(0.0271)	(0.0295)	(0.0167)	(0.0155)	(0.0262)	(0.0289)	(0.0593)	(0.0623)	(0.0483)	(0.0515)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	62213	60233	62213	60233	62213	60233	62213	60233	62213	60233

Note: *Post* indicates NSS 68th Round 2011-12. Estimates from Equation (2.1) for Sample I for women. Working Age includes adults between 18-60 years of age. Dependents includes children below 18 years of age and adults above 65 years of age. Regressions include state and month of interview fixed effects. District controls include fraction ST and SC, fraction illiterate, female and male employment rates, and fraction under poverty line separately for the whole district and for the rural areas of the district - calculated from NSS 2004-05. Also includes: the proportion of villages in a district connected by a road, proportion of villages in a district with a primary health center, proportion of villages in a district with a government hospital, proportion of villages in a district with a health sub-center and the average distance of a village in a district to the nearest town calculated from DLHS 2002-04. Individual controls include dummies for age 30 to 40, 40 to 50, and greater than 50, dummies for years of education under 4, between 5 and 8, between 8 and 12, and 12, as well as marital status, and household size. Standard errors in parentheses, clustered at the district level. Number of districts=531. * denotes significance at the 10% level, ** denotes significance at the 5% level, and *** denotes significance at the 1% level.

Table 2.5: Difference-in-differences - Treatment Intensity

	Private Casual Work		Private Salaried Work		Private Agricultural Wage Work		Domestic Work		Self-Employed	
Panel A: Females	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RSBY Intensity*Time3	0.069**	0.065**	0.012	0.011	0.048**	0.044**	-0.141**	-0.135**	-0.063	-0.055
	(0.0228)	(0.0231)	(0.0098)	(0.0098)	(0.0212)	(0.0217)	(0.0603)	(0.0607)	(0.0394)	(0.0396)
RSBY Intensity*Time2	0.035	0.022	0.006	0.000	0.021	0.01	0.027	0.032	-0.039	-0.03
	(0.0217)	(0.0219)	(0.0088)	(0.0089)	(0.0215)	(0.0217)	(0.0558)	(0.0554)	(0.0342)	(0.0343)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	98231	95250	98231	95250	98231	95250	98231	95250	98231	95250
Panel B: Males	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RSBY Intensity*Time3	0.075*	0.079*	-0.014	-0.031	0.189	0.071*	0.023	0.011*	-0.340*	-0.118**
	(0.0403)	(0.0431)	(0.0647)	(0.0198)	(0.1728)	(0.0400)	(0.0285)	(0.0065)	(0.1747)	(0.0450)
RSBY Intensity*Time2	0.019	0.062	-0.061	-0.059**	-0.085	0.013	-0.013	0.002	0.079	-0.016
	(0.1477)	(0.0389)	(0.0654)	(0.0178)	(0.1446)	(0.0360)	(0.0225)	(0.0064)	(0.1381)	(0.0373)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	96624	93157	96624	93157	96624	93157	96624	93157	96624	93157

Note: Estimates from Equation (2.2). RSBY Intensity is defined as years of RSBY exposure by 2011-12. Regressions include state and month of interview fixed effects. District controls include fraction ST and SC, fraction illiterate, female and male employment rates, and fraction under poverty line separately for the whole district and for the rural areas of the district - calculated from NSS 2004-05. Also includes: the proportion of villages in a district connected by a road, proportion of villages in a district with a primary health center, proportion of villages in a district with a government hospital, proportion of villages in a district with a health sub-center and the average distance of a village in a district to the nearest town calculated from DLHS 2002-04. Individual controls include dummies for age 30 to 40, 40 to 50, and greater than 50, dummies for years of education under 4, between 5 and 8, between 8 and 12, and 12, as well as marital status, and household size. Standard errors in parentheses, clustered at the district level. Number of districts=531. * denotes significance at the 10% level, ** denotes significance at the 5% level, and *** denotes significance at the 1% level.

Table 2.6: Difference-in-differences - Child Level Results

	Private Casual Work		Private Salaried Work		Private Agricultural Wage Work		Domestic Work		Education		Self-Employed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Early Treatment*Time3	0.023	0.027	0.009	0.011	0.019	0.017	0.126**	0.072	-0.654**	-0.571**	0.144**	0.161**
	(-0.0331)	(0.0267)	(0.0137)	(0.0136)	(0.0260)	(0.0230)	(0.0534)	(0.0527)	(0.1319)	(0.1214)	(0.0497)	(0.0475)
Early Treatment*Time2	0.033	-0.031	0.005	0.007	-0.042	-0.041	0.045	0.034	-0.270*	-0.206	0.044	0.01
	(0.0299)	(0.0302)	(0.0123)	(0.0118)	(0.0302)	(0.0270)	(0.0613)	(0.0580)	(0.1564)	(0.1466)	(0.0486)	(0.0500)
Late Treatment*Time3	0.001	0.006	-0.006	-0.004	0.03	0.023	0.108**	0.049	-0.196	-0.112	0.120**	0.142**
	(0.0347)	(0.0291)	(0.0102)	(0.0100)	(0.0274)	(0.0254)	(0.0516)	(0.0538)	(0.1324)	(0.1281)	(0.0458)	(0.0439)
Late Treatment*Time2	-0.055	-0.044	-0.004	0.000	-0.048	-0.046	0.189**	0.182**	-0.208	-0.145	0.093*	0.052
	(0.0235)	(0.0325)	(0.0105)	(0.0102)	(0.0313)	(0.0287)	(0.0675)	(0.0655)	(0.1641)	(0.1559)	(0.0499)	(0.0507)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	88984	87772	88984	87772	88984	87772	88984	87772	88984	87772	88984	87772

Note: Estimates from Equation (2.1) for Sample I for children from 6-17 years of age. Regressions include state and month of interview fixed effects. District controls include fraction ST and SC, fraction illiterate, female and male employment rates, and fraction under poverty line separately for the whole district and for the rural areas of the district - calculated from NSS 2004-05. Also includes: the proportion of villages in a district connected by a road, proportion of villages in a district with a primary health center, proportion of villages in a district with a government hospital, proportion of villages in a district with a health sub-center and the average distance of a village in a district to the nearest town calculated from DLHS 2002-04. Individual controls include dummies for age 30 to 40, 40 to 50, and greater than 50, dummies for years of education under 4, between 5 and 8, between 8 and 12, and 12, as well as marital status, and household size. Standard errors in parentheses, clustered at the district level. Number of districts=531. * denotes significance at the 10% level, ** denotes significance at the 5% level, and *** denotes significance at the 1% level.

Table 2.7: Difference-in-difference - Health and Healthcare Utilization

	Body Mass Index (BMI)		Days Affected by Short term Morbidity		Days Unable to Work due to Short term Morbidity		Days Unable to Work due to Major Morbidity		Days Hospitalized		Seeks Formal Care for Major Morbidity	
Panel A: Females	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Early Treatment*Post	0.742**	0.737**	-0.893**	-0.727	0.076	0.22	-1.880*	-3.417**	0.071	0.065	0.047**	0.031**
	(0.2949)	(0.2592)	(0.4037)	(0.4449)	(0.4001)	(0.4465)	(1.0177)	(0.8989)	(0.0604)	(0.0656)	(0.0132)	(0.0136)
Late Treatment*Post	0.857**	0.870**	-0.792*	-0.625	-0.196	-0.124	-0.29	-1.776**	0.045	0.053	0.026**	0.01
	(0.3210)	(0.2894)	(0.4180)	(0.4602)	(0.4529)	(0.4951)	(1.0050)	(0.8897)	(0.0616)	(0.0655)	(0.0121)	(0.0125)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	67433	63172	67433	63172	67433	63172	67433	63172	67433	63172	67433	63172

Note: Estimates from Equation 2.6 using the Indian Human Development Survet (IHDS). Regressions include state fixed effects. District controls include fraction ST and SC, fraction illiterate, female and male employment rates, and fraction under poverty line separately for the whole district and for the rural areas of the district - calculated from NSS 2004-05. Also includes: the proportion of villages in a district connected by a road, proportion of villages in a district with a primary health center, proportion of villages in a district with a government hospital, proportion of villages in a district with a health sub-center and the average distance of a village in a district to the nearest town calculated from DLHS 2002-04. Individual controls include dummies for age 30 to 40, 40 to 50, and greater than 50, dummies for years of education under 4, between 5 and 8, between 8 and 12, and 12, as well as marital status, and household size. Standard errors in parentheses, clustered at the district level. Number of districts=531. * denotes significance at the 10% level, ** denotes significance at the 5% level, and *** denotes significance at the 1% level.

Figure 2.3: Density of Households in each Propensity Score Bin higher than 0.4 for ATT analysis

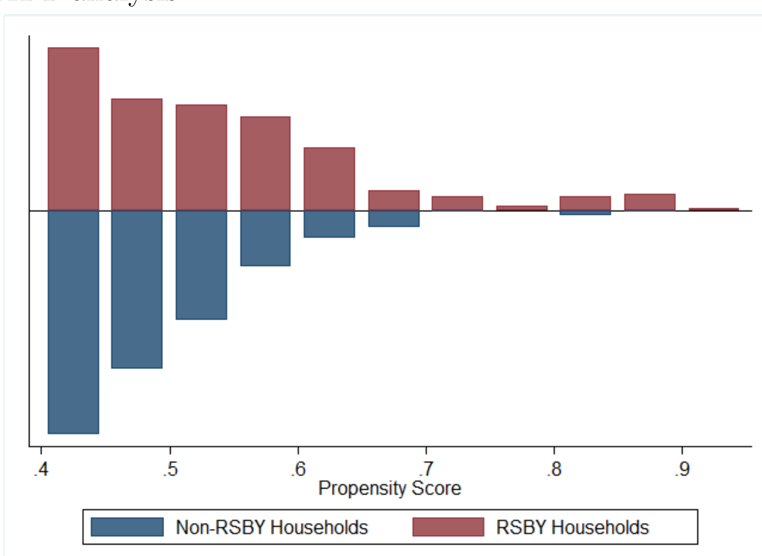
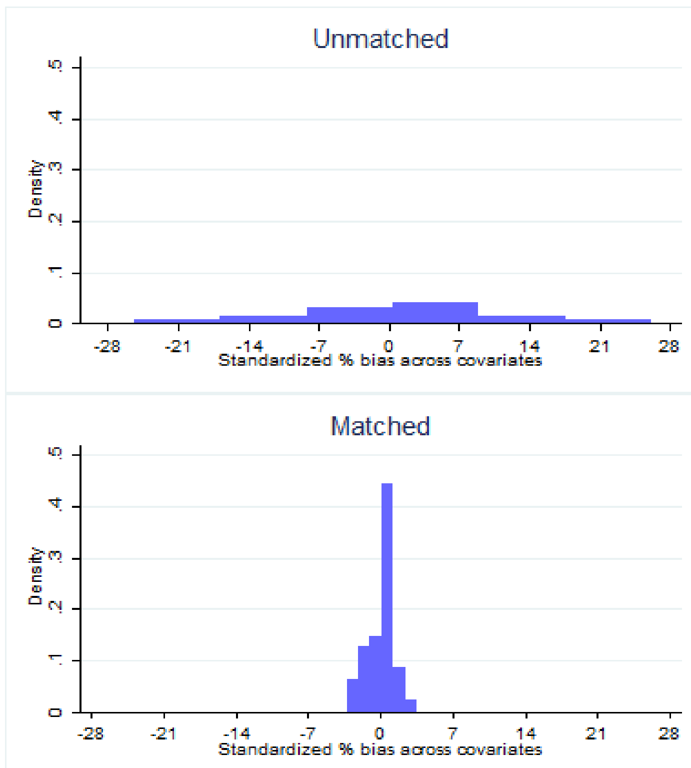


Figure 2.4: Standardized Bias in Unmatched and Matched Samples



Chapter 3: Income Shocks, Public Works and Child Nutrition

3.1 Introduction

It is well known that poor households in developing countries are limited in their ability to smooth consumption in the face of income shocks. This generates large variations in consumption which can have adverse consequences for the well-being of these households. The impact of such shocks on children has been well researched (see e.g Jensen, 2000, Alderman et al., 2009 and Alderman et al., 2006). Early life exposure to economic shocks has important implications not only for subsequent adult health and other socio-economic outcomes but also for perpetuating intergenerational poverty (Maccini and Yang, 2009). In such an event, social protection schemes such as the National Rural Employment Guarantee Scheme (NREGS) in India may have the potential to mitigate the adverse impacts of an income shock on child health by serving as a nutritional safety net.

Envisaged as being both a safety net and as providing alternative employment, the NREGS is the largest employment guarantee program of its kind in the world with annual expenditures equaling about 1 percent of India's GDP.[1,2] While the use of public work programs in developed countries has declined, there has been

[1] The NREGS offers a legal guarantee to every rural household: about 68 percent of the population.

[2] About Rs. 33000 crores ($5500 million) in 2013-14.

a resurgence of such programs in developing countries.[3] They have evolved into long term anti-poverty measures rather than merely serving to reduce temporary unemployment (Zimmermann, 2014). Using data from Andhra Pradesh in India, this paper seeks to understand the extent to which a safety net like the NREGS buffers the impact of income shocks on investments in children.[4]

While there is a growing literature on the impact of the NREGS on the labour market (e.g. Berg et al., 2012, Azam, 2012, Imbert et al., 2012) there has been a lack of focus on its role as a buffer for poor households in the event of a shock and the consequent impact on children.[5] To address this gap in the literature and using pre and post-intervention data from the Young Lives Panel study in India, I use a quasi-experimental approach to examine the extent to which the NREGS buffers the impact of income fluctuations on child health. That is, I examine the intent-to-treat effects of the program. I also assess the differential impact of the NREGS across wealth quartiles, land ownership and gender. Since the NREGS was implemented in a phased manner with the poorest districts getting it first, selection bias is a serious issue. By assuming that program placement is additive and time invariant, this study corrects for selection bias by using a difference-in-difference framework.

There are multiple channels through which the NREGS could impact child health in the event of a shock. First, the NREGS will result in changes in the labour supply of the household and as long as the income effect outweighs the substitution effect, child health will not be negatively affected in the event of a shock. Second, given that the NREGS leads to the strengthening of community level infrastructure

[3] Subbarao et al. [2012] provide an extensive review of public works programs - their design, implementation issues, poverty impacts and cost effectiveness - in developing countries.

[4] On June 2, 2014 Andhra Pradesh was split into two states: Telangana and Andhra Pradesh. This however doesn't impact my analysis since the data used in this paper is from 2002 to 2009.

[5] Zimmermann [2014] and Dasgupta et al. [2013] are notable exceptions.

such as water supplies and roads, child health is positively impacted as a result of this improved community infrastructure. Third, as a result of the NREGS, village economies become functional again and this leads to an increase in the income/per capita consumption of households which can again impact child welfare (Mani et al., 2014).

There are a number of important findings that emerge. First, the NREGS has large and positive mitigating effects on child health in the aftermath of a negative productivity shock. There is a cumulative improvement of about 0.08 standard deviations in height-for-age z-scores for those who had access to the NREGS in the event of a shock. Second, the program has strong differential impacts. Those belonging to higher wealth quartiles seem to benefit more from the NREGS after a shock suggesting significant rent seeking. Moreover, there is an asymmetric burden of shocks on girls with boys benefiting more from the safety net feature of the NREGS. These results are robust to alternative specifications.

These findings are potentially very relevant from a policy perspective because transitory shocks such as variable rainfall can induce path dependence and generate long term losses in health and education. While households in developing countries face both idiosyncratic and covariate shocks, covariate risks such as droughts and floods are harder to cope with since everyone in a community is impacted by the shock to some degree.[6] Ferreira and Schady [2009] show that the long term impact on children of such covariate shocks can be significant. A social safety net like the NREGS is expected to support poor households by acting as a basic insur-

[6]Idiosyncratic risks include events such as death or illness of a family member. These are usually uncorrelated among members of a community. There is some evidence in the literature that households have managed to offset the damages caused by idiosyncratic risks through various mechanisms such as informal risk sharing (e.g. Townsend, 1995, Mobarak and Rosenzweig, 2013).

ance mechanism after a shock. By ensuring income in times of economic downturn it prevents distress reactions such as asset sales which harm long run productive possibilities. Further it also enables poor risk averse households to lengthen their planning horizons and invest in high return-high risk activities such as newer technologies (Dasgupta et al., 2013, Ravi and Engler, 2009).

The remainder of the paper proceeds as follows. Section 3.2 provides a brief review of the previous developments in the literature. It also provides a description of the NREGS. Section 3.3 provides a basic theoretical framework linking intra-household time allocation to child health. Section 3.4 discusses the data. Section 3.5 describes the econometric model and related methods. Section 3.6 presents the results while Section 3.7 concludes.

3.2 Background

3.2.1 Previous Work

Rural households in developing countries often operate in risky environments.[7] Vulnerability to shocks is an important cause of deprivation (Dercon, 2001b). This is compounded by the presence of weak financial instruments and the adoption of other sub-optimal coping mechanisms such as asset sales, migration and child labour (Rosenzweig and Wolpin, 1993, Morduch, 1994 and Dercon, 2005). There is now growing evidence on the permanence of income shocks on human capital formation, nutrition and incomes. For instance, Foster [1995] finds that child growth (measured by weight) is highly responsive to fluctuations in income and prices and the effect

[7] For instance the average coefficient of variation of household income of farmers is almost 40 percent in the villages of the ICRISAT survey (Walker and Ryan, 1990).

is greater for credit constrained households. Alderman et al. [2006] have looked at the impact of a drought in Zimbabwe on child nutrition outcomes. They find that children aged 1-2 years lost 1.5 to 2 cm of height attainment after the drought and catch up was very slow even four years after the drought. Using DHS data from Peru, Paxson and Schady [2005] find that there was an increase of 2.5 percentage points in infant mortality rates for children born during the Peruvian economic crisis of the 1980s.

One of the more notable interventions used to alleviate the impacts of droughts on child growth has been food aid. This has been motivated, among other things, for its beneficial impact on child nutrition (Yamano et al., 2005). Quisumbing [2003] has shown that food aid and food-for-work interventions have a positive direct impact on weight-for-height z-scores of Ethiopian children in the aftermath of a shock. Yamano et al. [2005] also estimate the impact of food aid on offsetting impacts of shocks in communities that received the aid.

While direct interventions such as food aid and their impacts have been relatively well studied, the role of larger safety nets like large scale public works programs has been comparatively less researched. While their design has received much attention the evidence on the impact of such programs on protecting households from income shocks is sparse. Given the importance of such public works in mitigating the effects of a contraction in incomes due to a shock as pointed out by Ravallion [1991], this lack of evidence seems even starker. Moreover, as Alderman [2010] points out while short term climate shocks have long term impacts on children that persist into adulthood, safety nets whether in the form of income transfers or targeted nutrition interventions, can go a long way in mitigating the impacts of these shocks.

3.2.2 The National Rural Employment Guarantee Scheme

Enacted into law by the Government of India in 2005, the National Rural Employment Guarantee Scheme (NREGS) guarantees a minimum of 100 days of unskilled wage-employment in a financial year to rural households on productive public works at state prescribed minimum wages. Currently available to 56 million households, it is the largest safety net scheme in the world (Subbarao et al., 2012). It differs from other previous schemes in that it promises employment as an entitlement and there are no eligibility requirements (Azam, 2012). The Act also stipulates that one-third of all beneficiaries be women (Mani et al., 2014).

The NREGS was rolled out in three phases beginning in 2005. Districts in India were ranked based on a Backwardness Index designed by the Planning Commission. Based on this index the 200 poorest districts were covered in the first phase of the program between September 2005 and February 2006. The second phase commenced in May 2007 and covered 130 districts while in April 2008 all the remaining districts were covered.

The NREGS applies only to rural areas. The Act provides adult members of a household casual manual labour at the statutory minimum wage which is about Rs. 120 (2 USD) per day (Azam, 2012).[8,9] Given the susceptibility of rural households in India to periodic weather shocks and seasonal variations, the NREGS has been tailored to meet the objective of livelihood security by reducing the dependence on agricultural wages (Subbarao et al., 2012).

The NREGS was designed to be a program based on self-selection. Work

[8] Farm wages in comparison are usually about 100-150 rupees, varying somewhat depending on the agricultural season (Dasgupta et al., 2013).

[9] The statutory minimum wages varies across states.

carried out on identifying participation in the NREGS has found that holders of Antodaya cards (below poverty line cards issued to the poorest) are 20 percent more likely to register for the program (Shariff, 2009 and Uppal, 2009). Likewise, Jha et al. [2009] find that targeting of the program has been satisfactory with there being wide participation from groups such as scheduled castes and scheduled tribes. Thus the NREGS seems to be encouraging participation from those who need it the most.

3.3 Theoretical Framework

This section presents a simple model on the impact of intra-household time allocation on child growth. I use this framework to show the mechanisms through which the NREGS mitigates the impact of an income shock on child health.

3.3.1 Basics

An income shock affects household income adversely and thus impacts consumption and consequently child health. Implicit in this is the assumption that households are unable to insure their consumption fully from such income shocks. This is plausible because my data includes only rural households who for the most part have few, if any formal coping mechanisms for such shocks (see e.g. Dercon, 2001b for a review).

An employment guarantee program such as the NREGS can have two effects in the event of an income shock. First, it is expected to have a positive impact on child growth because it supplements household income during a shock and thus enhances the households ability to purchase nutrition enhancing items (Yamano et al., 2005).[10] This can be directly through actual wages paid on the NREGS works or indirectly through strengthening of village infrastructure, increased resilience to shocks etc. Second, unlike food aid, under the NREGS households supply labour on public works. This means that it could take away from the time that would have been spent on child care. Moreover for about 57 percent of my sample the child's mother has participated in the NREGS. Assuming that the mother is the primary caregiver especially for younger children this 'substitution' effect could negatively impact child growth in the event of a shock.[11]

3.3.2 Wage Implications of a Transitory Shock

The NREGS functions both as an alternative source of employment ex-ante and as an ex-post coping mechanism after an income shock. Assuming there are no constraints on off-farm labour supply, a household will work either in the private off-farm sector or in ex-ante NREGS employment and will work wherever the wage is higher. This is because both these forms of employment are perfect substitutes and contribute in the same manner to household utility. Exogenous income shocks however, change the ex-post wage in the private off-farm sector. This is because in the event of an income shock, when labour productivity is low, demand for labour (whether own or hired) on one's own farm decreases. This leads to an increase in off-farm labour supply and a reduction in wages. Since the off-farm wage is now

[10] However as Debela et al. [2014] point out the degree to which an increase in income boosts nutrition depends on the marginal propensity to consume health and nutrition goods out of money income. Moreover the effect of the NREGS on child growth further depends not only on intra-household allocation but also on the gender and age preferences of the parents.

[11] Most rural households in India have large families and thus it is true that even if the parents are out on the NREGS works any other adult member of the family can take care of the child. Moreover, the NREGS also has provisions for child care outfits to be set up on the work site so that the mother need not leave the child unattended at home. Both of these imply that the impact of the negative substitution effect may not be very large.

contingent on the weather it is less useful as a risk mitigation tool. In the event of a shock not only are households adversely impacted because of direct losses in farm income/profits because of the shock itself, but also from the reduction (and increased variability) in off-farm wages. This can be easily seen in a simple one-period model. Assume that each household in the economy has k units of land. Before the introduction of the NREGS, households allocate time, T, between working in the off-farm sector, l and working on their own family-farm, f.[12] The period ends and total income which is a combination of the wages earned working off-farm, w_o and profits from the family farm, y is realized. Household utility, u is a function of total income earned from the two activities and $u' > 0$, $u'' < 0$.[13]

Production on the farm is Cobb-Douglas in land and labour:

$$y\left(k, d\right) = \tilde{A} d^{\beta} k^{1-\beta} \tag{3.1}$$

where $\beta \in (0,\, 1)$, $\tilde{A}$ is total factor productivity, and d is labour demand. Let this be stochastic and of the following form:

$$\tilde{A} = \begin{cases} A_H \text{ with probability } \frac{1}{2} \\ \\ A_L \text{ with probability } \frac{1}{2} \end{cases}$$

with $A_H > A_L$. Thus during a good year total factor productivity is high (A_H) and during a drought it is low (A_L).

Labour demand and supply decisions are made ex-post, once the shock, whether high or low, has been realized. Assuming that the labour demand decision on the

[12] This could include working as an agricultural labourer for a landowner.
[13] Thus I assume that households are risk averse.

family farm is separable from other choices, the labour demand decision is given by equating the marginal product of labour to the agricultural wage (w_f):

$$\frac{\partial y}{\partial d} = \tilde{A}\beta \left(\frac{k}{d}\right)^{1-\beta} = w_f$$

$$d^* = k \left(\frac{\tilde{A}\beta}{w}\right)^{\frac{1}{1-\beta}} \tag{3.2}$$

From equation 3.2 it is clear that labour demand is lower in periods of low productivity (A_L) such as when a drought occurs, than in periods of high productivity/in good years (A_H).

Given this reduction in demand for labour because of low agricultural productivity, households supply less labour on the farm and instead increase their off-farm labour supply.[14] The interior solution to the labour supply decision of the household is given by:

$$\max_{l} \quad u((T-l)y + lw_o) \tag{3.3}$$

The first order condition for off-farm labour supply is:

$$\phi(l^*) = u'((T-l)y + lw_o)(w_o - y) = 0 \tag{3.4}$$

Equation 3.4 pins down the optimal off-farm labour supply, l^* and by extension f^*.

Thus when the weather is more variable and total factor productivity is low, labour demand on the farm is also low and there is a tendency to shift away from employment on one's own farm and towards the private casual sector. Off-farm

[14]This implicitly assumes that households are unable to borrow and/or draw on their savings to smooth consumption and have to resort to looking for a job in the off-farm sector.

labour supply increases in the event of an exogenous shock. This excess supply of labour drives down the off-farm wage further exacerbating the adverse impacts of the shock.[15]

The introduction of the NREGS however changes the situation in a fundamental way. In the event of an income shock households can now work on the NREGS works and are thus *guaranteed* employment after a shock. The wages paid on the NREGS are fixed and thus there are no fluctuations in wages. Thus while households are still worse off compared to the situation without any shock, in the event of a shock, they are better off with the NREGS than without it.[16] This is because the NREGS absorbs all the excess workforce *without* impacting the wage, which is fixed. Thus not only is total household income higher in places where the NREGS is available, it is also less variable. This is the particularly powerful role of the NREGS - as a safety net.

3.3.3 Implications of NREGS Access for Child Health

A child's height at time t, h_t is a function of height last period, h_{t-1}, overall income Y_t (where $Y = fy + lw_o$), labour allocated to child rearing activities, L^H, observable household and community characteristics, X_t, unobservable individual characteristics, ϵ_t and unobservable household and community characteristics,

[15] Jayachandran [2006] has found that productivity shocks causes large swings in wages and impacts rural households adversely. Contrary to what is assumed here, she also finds that poorer households have fairly inelastic labour supply and thus are not easily able to switch labour from the farm to the off-farm sector.

[16] This is because shocks such as droughts lower agricultural labour productivity and thus reduce real agricultural wages.

u_t[17,18]:

$$h_t = f(h_{t-1}, Y_t, L^H, X_t, \epsilon_t, u_t) \tag{3.5}$$

Overall income, Y_t in turn is a function of exogenous shocks such as drought or pest damage, S_t, transfers in the form of income earned on the NREGS works, F_t, observable household and community characteristics, X_t and unobservable household and community characteristics, u_t:

$$Y_t = f(S_t, F_t, X_t, u_t) \tag{3.6}$$

Substituting equation 3.6 in 3.5 we get:

$$h_t = f(h_{t-1}, S_t, F_t, L^H, X_t, \epsilon_t, u_t) \tag{3.7}$$

An income shock results in the familiar substitution effect where time spent at home decreases and time spent on the NREGS works increases. However, the income effect is positive because not only does the NREGS lead to wage payments in the event of actual participation, it also indirectly enhances income through improved community infrastructure, increased resilience to shocks etc. Thus if the increase in income compensates for the 'cost' of reallocation of labour from activities at home, then the overall impact on health outcomes could be positive. The critical point is that these wage payments are both higher and less variable (in a relative sense) than they would be in the absence of the NREGS. That is, were the NREGS not

[17] I model child growth by the widely used measure of height. In the empirical section this is measured using height-for-age z-scores.

[18] As Ferreira and Schady [2009] point out these time intensive activities at home can be very important for children. These include antenatal checkups for pregnant women or preventive health checkups for children, cooking healthy meals or ensuring clean water.

available to buffer the impact of the shock, the income effect would be negative and this combined with the substitution effect would unambiguously lead to a negative impact on child health.

The net impact of the NREGS on health outcomes in the event of a shock depends on the reallocation of labour, the size of the wage payment received and the marginal impact of this payment on child health. Based on the theoretical framework this study poses the following hypothesis: children from households that have access to the NREGS in the event of an income shock are likely to have better health outcomes. This is because the opportunity cost of reallocating labour to the NREGS is most likely low and total household income in the presence of the NREGS is most likely higher than in the absence of it.

3.4 Data

3.4.1 Data

In this study I use data from the Young Lives Longitudinal Study. The Young Lives dataset is a rich panel that tracks two cohorts of children (younger cohort born in 2001-02 and older cohort born in 1994-95) from 2002 to 2016. The study is tracking approximately 12,000 children in four countries: Peru, Vietnam, Ethiopia and India. In India the study has been conducted in the state of Andhra Pradesh.[19]

The sites chosen in India cover all three agro-climatic zones in Andhra Pradesh. The survey covered the six districts: Cuddapah, Anantapur, Mahbubnagar, Karimnagar, West Godavari, and Srikakulam and also the capital city of Hyderabad. Since

[19]The data is thus not nationally representative. However, by comparing certain basic attributes and characteristics Outes-Leon and Dercon [2008] have found the data to be representative of the broader region.

our main focus here is on the causal impact of the NREGS in mitigating the impact of shocks I focus only on rural areas. Thus I exclude Hyderabad from the sample.

Since the NREGS was rolled out only starting in 2005, none of the districts in our sample had the NREGS in 2002. By 2007 when the second round of the Young Lives data was collected, four districts (Cuddapah, Anantapur, Mahbubnagar, Karimnagar) had been covered under the scheme (these will be referred to as the early phase-in/early treatment districts). By the third round in 2009-10 the remaining two districts (West Godavari, and Srikakulam) were also covered (these will be referred to as the late phase-in/late treatment districts).

In this paper the latest available data from the first three waves (mid-2002, early 2007 and mid-2009) is used. For the purpose of this study the younger cohort consisting of approximately 2011 children is used.[20] The overall rate of sample attrition is low with only about 4 percent of children lost over a seven year period. While constructing the final data set the following exclusion rules have been used: one, only children living in rural areas in all three periods have been included. Two, for econometric reasons I include only those children that are present in all three rounds of the survey. Three, I exclude children with missing information on the dependent variable of interest: height-for-age z-score. My dataset after these exclusions contains data on 4018 children across three years in 6 districts. I use the older cohort for a falsification test.

[20]Attrition in in my sample is below the international comparison with other longitudinal studies (Outes-Leon and Dercon, 2008)

3.4.2 Variables of interest

The World Health Organization (WHO) outlines three important indicators for child nutrition: height-for-age (this is a long term indicator of chronic malnutrition), weight-for-height (this is an indicator of acute malnutrition and is being unable to gain weight) and weight-for-age (this is a combination of the above two and is used to give an overall indicator of malnutrition).

In most survey data these indicators are standardized with respect to a reference population and are presented as z-scores. In this paper I focus my attention on the first indicator: height-for-age. This is because when analyzing the impact of shocks, height-for-age is the only accurate indicator of long term impact. Weight-for-height and weight-for-age are short term indicators and children can make up lost weight easily. They would thus give an inaccurate representation of the actual impact of the shocks.

Since my sample consists of rural households it is reasonable to assume that one of the main income shocks they face is variations in rainfall.[21] To construct the income shock variable, the long term average (1951-2009) for each district is used. Standard deviation for the same period is also calculated at the district level. Then rainfall shock is defined as the deviation of actual rainfall last year from the long term average divided by the standard deviation. The shock variable is thus normalized. I use prior rainfall deviations in order to give rainfall shocks some time to feed through and for them to influence household decision making.[22]

I also include child and household level controls: age, household size, wealth

[21]Over 70 percent of the population in the state of Andhra Pradesh is engaged in agriculture.
[22]Rainfall data has been obtained from the Indian Meteorological Department.

quartiles.[23], land owned in acres, dummies for if head of the household has completed primary education, gender, Hindu, SC/ST[24], OBC[25] and debts.[26] There is a significant amount of literature that points to the current health status being determined by attainment till the previous period (Strauss and Thomas, 2007). This can be captured by including a lagged dependent variable as a regressor. While I don't include this in my main specification I do re-estimate the above regressions using the Arellano-Bond GMM estimator and use lagged height-for-age z-score in period *t-2* as an instrument (Arellano and Bond, 1991).

3.4.3 Descriptive Statistics

Tables 3.2 & 3.3 describe the relevant summary statistics for all three years. The height-for-age z-scores deteriorates from the time of birth (round 1) to when the children are five years (round 2) improving slightly by round 3. As Dasgupta et al. [2013] points out this is more or less consistent with the case of developing countries where height-for-age z-scores decline in the first few years and then stabilize. In round 1 of the survey the mean height-for-age z-scores for those in the early phase-in districts was -1.30 which goes down substantially to -1.82 in round 2 before recovering slightly to -1.63 in round 3. The districts in the late phase-in group follow a similar pattern of declining z-scores from rounds 1 to 2 with some improvement being seen between rounds 2 and 3. What is also interesting is that the late phase-in

[23] Including the wealth index implies that I don't include income explicitly as an explanatory variable not only because it would be endogenous but also because the wealth index captures all the impact that income would have captured because the index is a far more comprehensive measure.

[24] Scheduled Caste/Scheduled Tribe

[25] Other Backward Caste

[26] In this data, gender is the only variable that is time invariant. Caste and religion do change slightly over the three waves for some households. It is difficult but not uncommon in India for people to change castes and/or change their religion. Thus dummies for SC/ST, OBC and Hindu are not time invariant.

districts (which includes those districts that got the NREGS later which means that they were higher up on the development index) had worse height-for-age z-scores than the early phase-in districts in 2002 before the NREGS was in force.

There are no significant differences between the two groups in terms of age, gender and religion. However, there are significant differences across the groups in terms of caste, schooling of household head, household size, debt, land owned and access to supplementary feeding programs. This is to be expected because the NREGS was not a randomly assigned program. The poorest districts received the program first. This implies that the early phase-in districts and late phase-in districts are markedly different on a number of indicators. This however, as detailed in the next section, is resolved by using a difference-in-difference methodology and assuming that the placement bias is additive and time invariant.

Examining the other control variables I find that on average 53 percent of the sample is male while the average age in 2002 and 2009-10 is approximately one year and 7.5 years respectively. The average child has approximately four other members co-residing with her. The children are primarily Hindu (92 percent) and a majority belong to backward castes, SC/ST or OBC. Not surprisingly, the proportion of the sample with caregivers having completed primary education is low (about 30 percent). The wealth index takes a value between 0 and 1. The average value of 0.19 suggests that households have only about 20 percent of all of these assets suggesting that the children in the sample come from very poor households.

3.5 Empirical Strategy

The impact of the NREGS on child growth in the event of an income shock can be multi-dimensional. First, the NREGS leads to wage payments and this income effect may alone lead to higher expenditures on child health.[27] Even if it doesn't lead to increased expenditures it is reasonable to assume that given this 'insurance' in the event of a shock, expenditures on child health would potentially not be reduced. Second, the NREGS will lead to increased labour supply by the parents in the aftermath of the shock and this may lead to a substitution effect away from time spent at home on activities beneficial for child health. Third, the NREGS leads to the creation of village infrastructure such as roads and this may increase the risk bearing capabilities of households after an income shock without necessitating an increase in labour force participation.[28] Fourth as Mani et al. [2014] point out, for about a quarter of the sample of children from the Young Lives data, the NREGS workers from the household are not biological parents. This means that considering only parent's labour force participation could bias the true impact of the NREGS. This is because if none of the parents participate then only the income effect remains and thus we would underestimate the true impact of the program if we focused only on parental participation. Finally, the NREGS also reduces the ex-ante risk that communities face as a result of covariate shocks regardless of actual participation in the program. This leads to a rise in overall village income and consequently better child outcomes. The study therefore estimates the intent-to-treat effects of

[27] This does depend on the marginal propensity to consume health goods out of cash income.

[28] Skoufias [2003] points out that publicly provided mechanisms such as sound infrastructure play an important role in reducing the risk for poor households vulnerable to shocks.

the program which includes both the income and substitution effects.[29]

3.5.1 Measuring overall impact

One of the main challenges in this study is that there is no untreated counterfactual group since all six districts received the program at some point in time. This identification issue can be potentially serious because before and after evaluations can be biased if there is no comparison to untreated groups (Clemens and Demombynes, 2010). Thus, it is necessary to evaluate benefits by comparing beneficiaries to other beneficiaries (Hanson et al., 2013).

First, in order to eliminate the program placement bias, the main specification I run is a fixed effects model given by the following equation:

$$H_{idt} = \beta_0 + \beta_1 NREGS_{d,t} + \beta_2 Shock_{d,t-1} + \beta_3 [NREGS * Shock]_{d,t} + \epsilon_i + \epsilon_d + \epsilon_h + \epsilon_{it}$$

The dependent variable H_{idt} refers to the height-for-age z-score for child i in district d at time t. The variable *NREGS* measures access to the NREGS and varies by district and time. $Shock_{d,t-1}$ is the income shock at the district level in the *previous* period. The coefficient on the interaction term, β_3 is the parameter of interest and captures the mitigating effects of the NREGS in the event of a shock.[30] This specification includes year dummies.

3.5.2 Measuring multi-year impacts

The above specification however does not allow me to disentangle the multi-year impacts of the program. In order to do so, I examine two groups: the early

[29] Using this approach however means that I am unable to disentangle the income and substitution effects since the intent-to-treat effects estimate the total impact of the program.

[30] The impact of the shock in the absence of the NREGS is given by β_2 while in the presence of the NREGS the corresponding value is given by $\beta_3 - \beta_2$

phase-in districts and the late phase-in districts. The NREGS was rolled out in two waves with four districts (Cuddapah, Anantapur, Mahbubnagar, Karimnagar) getting the program in 2007 and the last two districts (West Godavri and Srikakulam) getting it in 2009. Thus in the final year the differences between the two groups are caused only by differential access to the program. Thus I examine two phases of treatment (2007 and 2009) in addition to one pre-intervention year (2002).

Thus, in order to disentangle the multi-year impacts of the program I run the following difference-in-difference model:

$H_{idt} = \beta_0 + \beta_1 Shock_{d,t-1} + \beta_2 Time1_{d,t} + \beta_3[Time1 * Shock]_{d,t} + \beta_4 Time2_{d,t} + \beta_5[Time2 * Shock]_{d,t} + \epsilon_i + \epsilon_d + \epsilon_h + \epsilon_{it}$

As before H_{idt} refers to the height-for-age z-score for child i in district d at time t. $Time1_{d,t}$ and $Time2_{d,t}$ are indicator variables for NREGS assignment to the first and second years of exposure respectively. They vary by district and time (e.g. $Time1_{d,t}$ takes a value of 1 for the first four districts in 2007 and for the last two districts in 2009. Similarly, $Time2_{d,t}$ takes a value of 1 for the first four districts in 2009 since it was the second year of the program for these districts. Table 3.1 outlines the implementation framework.). In this framework, β_3 captures the average mitigating effect of the first year of exposure to the NREGS on child health in the event of a shock and β_5 is the analogous buffering effect of the second year of exposure to the NREGS.[31] This specification also includes year dummies.

Both specifications also include a set of observable individual and household level controls. Including these variables helps control for pre-treatment differences that

[31]Like above, the impact of the shock in the absence of the NREGS is given by β_1 while the impact of the shock with NREGS access for one year is given by $\beta_3 + \beta_2 - \beta_1$. The impact for two years of NREGS exposure is $\beta_5 + \beta_4 - \beta_1$

Table 3.1: NREGS Implementation Framework

Group	Districts Covered
First year of treatment	1, 2, 3, 4 in 2007 & 5, 6 in 2009
Second year of treatment	1, 2, 3, 4 in 2009
Control	1, 2, 3, 4, 5, 6 in 2002 & 5, 6 in 2007

were present between the early and late phase-in districts. However, there may be household level and even some child level unobserved heterogeneity. My specification thus includes time invariant child (ϵ_i), household (ϵ_h) and district (ϵ_d) characteristics.

While it is desirable to estimate the impact of actually participating in the program (Treatment Effect on the Treated) both because that would be more realistic given my theoretical model and from the perspective of the NREGS literature, the Intent-to-Treat (ITT) estimate is useful from the viewpoint of policy. As Azam [2012] and Yamano et al. [2005] point out, the ITT parameter is useful for policymakers designing similar policies for the same population because policymakers have little influence over actual participation by individuals.

3.5.3 Challenges

There are a number of challenges that arise while estimating the causal impact of the NREGS. The first is that of selection bias. There are two sources of selection bias that need to be considered. One, as mentioned before, assignment to NREGS was non-random. The poorest districts were covered first under the program. However as outlined in other studies dealing with the NREGS (Mani et al., 2014 and Azam, 2012) if we assume that program placement is correlated with time invariant individual, household and community level characteristics which enter additively into my specification then using a fixed effects model should resolve the problem. This is because a fixed effects specification will eliminate the program placement effect leaving only the causal effect of the NREGS in buffering the impact of an income shock on child nutrition. The second source of selection bias is that participants in the NREGS are not randomly assigned. It is possible that people less able to cope with shocks are more likely to participate in program thus biasing simple OLS estimates of the causal impact. This however, as mentioned above can be dealt with by examining the overall intent-to-treat effects of the program rather than the treatment effect on the treated which depends on actual participation.

The other main challenge with my empirical analysis is that the number of districts (for which I have data) over which the NREGS was implemented is small (six districts). This means that the usual methods which are employed to correct for clustering often lead to an over-rejection of the null hypothesis of no effect when the number of clusters is small (Cameron et al., 2008). Following Cameron et al. [2008] the standard practice is to report the bootstrapped standard errors clustered at the district level. However since these were about the same as the robust clustered standard errors, I report the latter (where the number of clusters/districts is six).

3.6 Results

3.6.1 Overall impact on child nutrition

Tables 3.4 & 3.5 show the results for running both the above mentioned specifications. As seen from column 2 in Table 3.4 being exposed to an income shock significantly reduces the height-for-age z-score. For those without access to the

NREGS, a one standard deviation change in rainfall from its mean results in loss of height-for-age z-score by about 0.50 standard deviations. On the other hand, for those who did have access to the NREGS, the impact of the shock is about 0.08 standard deviations.[32] That is, the impact of the shock is differential based on NREGS access.

3.6.2 Multi-year program impacts

Table 3.5 reports the multi-year impacts of the program. The coefficients for each year of NREGS exposure and the mitigating role of the NREGS are cumulative and not marginal. The impact of exposure to the NREGS in all years is significantly different from zero. The mitigating role of the NREGS in the event of a shock is clear. In the absence of the program, an income shock would have led to a decline of height-for-age z-scores by 0.61 standard deviations. Exposure to one year of the NREGS in the event of a shock, led to a positive and statistically significant differential impact of 0.32 standard deviations in height-for-age z-scores.[33] Thus the shock fails to adversely impact children if they have access to the NREGS.

Being exposed to two years of the program led to a similar positive and statistically significant improvement of 0.50 standard deviations for those who had access to the NREGS during a shock. The joint test that both coefficients are zero is rejected. However height-for-age increases by less in the second year.[34] Moreover, the coefficients for the first and second years of exposure are significantly different

[32] The magnitude of the impact is found by subtracting the impact of the shock from the impact due to the NREGS (0.58-0.50)=0.08 standard deviation increase in height for age.

[33] The magnitude of the impact is found by subtracting the impact of the shock from the cumulative impact due to the NREGS (0.871 + 0.058 - 0.610)=0.32 standard deviation increase in height for age.

[34] Impact of second year = Cumulative impact - impact of first year = 0.50 - 0.32 = 0.18 standard deviations increase in height-for-age.

than each other.

Since the impact of the second year is lower than that of the first year, this suggests that there is a decay in the short run effects. That is, the late phase-in districts tend to catch up to the early phase-in districts and there are no long term persistent effects of the program in buffering children against income shocks. This however ties in with what one would expect given the dual nature of the NREGS in enhancing capabilities and serving as a safety net. Since I am analyzing the same cohort of children and their families it is possible that over time the NREGS has enabled these households to become better equipped in dealing with shocks. This can be either through the enhanced income that the NREGS provides or its implicit role as an insurance mechanism which makes households less risk averse and more able to cope with shocks. It is possible thus that for the same household, utility of the NREGS as a safety net may diminish over time as they enhance their capabilities. Thus there is no differential impact of the shock because of NREGS access in the long run. Alternatively, it is also possible that there are diminishing returns to improvements in height-for-age z-score i.e. improvements in height-for-age z-score are bounded above.

Moreover, improvements in height-for-age are most significant when children are younger and therefore between 2007 and 2009-10 (the time frame of the medium run effects) when the children in my sample are between 5 and 7 years, there might not be significant improvements in height. This is even more conceivable because in both my specifications, the coefficient on age is negative which means the older the child the lower is the increase in height-for-age.

Columns (3) and (2) in Tables 3.4 & 3.5 detail the regression results for both specifications after including the food supplement variable. As is evident my results don't change much and the mitigating effect of the NREGS is still positive and statistically significant. However, the supplement variable is also positive and statistically significant implying that access to supplementary feeding programs like the ICDS and the Mid-Day Meal Scheme contribute significantly to children's height attainment.

Within the described controls, I perform a series of F-tests of the null hypothesis that the coefficients of these variables are jointly equal to each other. I reject the null hypothesis that the coefficients of the time dummies are jointly zero (p-value=0.000). This implies that there are significant time effects. I am unable to reject the null hypothesis that all the individual level controls are jointly equal to zero (p-value=0.3775). I also fail to reject the null hypothesis that all the household level controls are jointly zero (p-value=0.2474). Finally, I reject the null hypothesis that the two variables of interest, access to NREGS and the income shock are jointly zero (p-value=0.000).

These results validate those that have been found in similar studies. For instance Dasgupta et al. [2013] finds an improvement of 0.27 standard deviations in height-for-age as a result of the NREGS in India. Quisumbing [2003] finds that food-for-work and food aid have positive impacts on weight-for-age for children between 0-5 years from low-asset households in Ethiopia. Similarly Yamano et al. [2005] also find that food aid significantly helps in mitigating the impacts of income shocks on height-for-age z-scores in rural Ethiopia.

3.6.3 Heterogeneous impacts —By wealth quartiles, land ownership and gender

The role of the NREGS in helping mitigate the impacts of a shock may hide large heterogeneity of impacts across households belonging to different socio-economic groups. In order to address this I run both my specifications by an indicator of household wealth, gender and land ownership. I examine children from households with less than the median and more than the median wealth level index (the median is the sum of the first two wealth quartiles and is based on the pooled sample which includes all three years).

The results are given in Tables 3.6, 3.7 and 3.8. The mitigating impact of the NREGS on children's height after an income shock is positive and statistically significant. Ideally one would expect that the NREGS should be most beneficial for the poorest. However, contrary to expectations, the mitigating impact of the NREGS is higher for households with wealth levels higher than the median level. That is, for richer households, the differential impact of the shock for those who have NREGS access is 0.08 standard deviations while for the poorer households the improvement is only 0.07 standard deviations. This can be explained by a number of factors. One, public works in developing countries are plagued by implementation problems like corruption and underpayment of wages. If true, then this attenuates the mitigating impact that the NREGS has in the event of a shock, especially for the poor who are more dependent on the NREGS after a shock relative to richer households. Two, an audit by the Indian government in 2012 reveals that awareness (on a relative basis) about the NREGS and its entitlements is still very low. Richer households with better connections (political and social) might be more aware of the program and thus more able to access it in the event of a shock as opposed to poorer households. Three, using nationally representative data Dutta et al. [2012] have found that the demand for NREGS far outstrips the supply which leads to rationing of projects. This leads to rent seeking and consequently richer households are better placed to find work than poorer households after a shock. Thus, the overall results I find above are significantly driven by the sub-sample of those children who belonged to richer households for whom the impact of the NREGS contributes more to improvements in child health than the total sample.[35]

I find a similar differential impact when I disaggregate my sample by gender. Columns (3) & (4) of Table 3.6 detail the results of my specifications. The mitigating impact of the NREGS is higher and statistically significant for boys relative to girls. This implies that girls are impacted more by income shocks. That is, for those who had access to the NREGS after a shock, there is an improvement of 0.12 standard deviations in height-for-age z-scores for boys compared to 0.04 for girls. This ties in with our expectations about the male preference bias in Indian households. While because of data constraints I don't examine it here, but it would be interesting to see if the increase in female labour force participation that the NREGS has seen leads to an increase in the bargaining power of women and an improvement in female child health.

Table 3.7 highlights multi-year heterogeneous impacts. I find similar results to those found above. The impact of the shock for those who were exposed to the NREGS in the first year is 0.39 for those with wealth less than the median wealth

[35] It is important to remember that here 'richer' is used in a relative sense because my entire sample consists of the rural poor.

level and 0.49 for those with wealth more than the median level.

The results from decomposing the sample by land ownership are given in Table 3.8. The mitigating effect of the NREGS is significant for households both above and below median land ownership. For those with land less than the median level, the impact of the shock for those with access to the NREGS is 0.15 standard deviations. The analogous figure for those with land more than the median level is -0.02 standard deviations. Thus, households with smaller plots of land seem to do better during a shock than those with larger plots of land. There are several explanations for this. One, assuming land quality is similar across various plots of land, those with smaller plots of lands are poorer in a relative sense.[36] When agricultural wages fall because of a productivity shock, the income effect leads workers to supply more labour while the substitution effect makes them supply less of it. For poorer households the income effect outweighs the substitution effect. The presence of the NREGS thus benefits them disproportionately more. Two, those with more land are going to be worse off in the case of an income shock such as highly variable rainfall. This is because, households with smaller plots of land may be less affected by a rainfall shock as they depend less on agriculture to meet their subsistence requirements. They are consequently less impacted by a shock that lowers agricultural productivity and hence overall profits.

[36] While this is not a reasonable assumption to make in most cases, since my sample is from one state in India it is reasonable to assume that there is less heterogeneity in terms of land quality. However, ideally one would want to control for land quality before any robust conclusions can be drawn.

3.6.4 Accounting for lagged height

In line with my theoretical model I also consider including the lagged dependent height-for-age z-score as an explanatory variable. This is because height in period *t+1* is a function of height attained till a period before. Thus the health production function from Section 3.3 is dynamic because it includes lagged health as an explanatory variable for current health (Yamano et al., 2005, Quisumbing, 2003 and Alderman et al., 2006). Thus following Quisumbing [2003] I use the Arellano-Bond estimator. Since I have only three years of data, I use the Anderson-Hsiao/Difference-GMM and use height-for-age in period *t-2* as an instrument for the lagged endogenous variable, H_{it-1}. The results of running this model are given in Column (3) in Table 3.5. The impact of the shock is negative for those without access to the NREGS. This however is exactly offset by having access to the NREGS in the first year. The net impact on height-for-age z-scores is hence zero.

3.6.5 Robustness Checks

In order to test that the treatment effects identified above are consistent and unbiased, I perform robustness checks in this section. Thus I estimate the above analysis using a placebo group (instead of the actual treatment group) comprising of the older cohort of children from the Young Lives study. Table 3.9 reports the results of running the fixed effects model with the 'fake' treatment group. Given that the treatment group now consists of older children, one would expect that there are no mitigating impacts of the NREGS as older children are not affected by nutrition as much. In line with expectations, the buffering impact of the NREGS is not statistically significant. Since the treatment group in this case consisted of older children an income shock does not have a statistically significant impact on their health. Therefore the mitigating effect of the NREGS is insignificant as well.

3.6.6 Testing Parallel Trends

The key assumption with the fixed effects specification is that of parallel trends. That is, in the absence of the NREGS the treatment and control districts would have had parallel time trends and it is the introduction of the NREGS that introduced a deviation in that trend. I test the parallel trend assumption both using the pre intervention data from the Young Lives data (first wave of the survey 2002) and using the Indian Human Development Survey (IHDS), 2005. The IHDS was administered between November 2004 and October 2005, and the first phase of the NREGS was rolled out in September 2005. So the IHDS is suitable to test for pre-intervention time trends. Out of the six districts in my sample the IHDS was administered in five of those excluding Srikakulam. I plot the mean height for children from the early phase-in and late phase-in districts in Figure 3.1.[37] Figure 3.2 plots the height-for-age z-scores for the early and late phase-in districts using Round 1 (pre-NREGS) of the Young Lives data. Both figures, for the most part, highlight parallel trends between the two groups. Given these, it is reasonable to assume in this context that pre-intervention outcomes follow parallel trends.

[37] The IHDS recorded anthropometric outcomes differently than the Young Lives data. The data does not contain z-scores but only has absolute numbers measured in centimeters.

3.7 Policy Implications

Public works programs have a long history. More recently they have evolved into policy instruments for generating employment and alleviating long term poverty. Well designed programs provide income transfers to poor households in periods of critical need. Evidence on their use as safety nets, however is low. In this regard, this paper tests the effectiveness of one such program. Using longitudinal data from the 2002, 2007 and 2009-10 waves of the Young Lives Survey in Andhra Pradesh, India I assess the effectiveness of the National Rural Employment Guarantee Scheme (NREGS) in mitigating the adverse impacts of income shocks on child health.

The main finding in this paper is that the NREGS has large and positive mitigating effects on child health in the aftermath of a shock. This has important policy implications. First, while the NREGS wasn't designed to be a program that helps in tackling child malnutrition, its ability to function effectively as a safety net means that it can prevent large fluctuations in child health. If this were to be combined with the proper functioning of on-site child care facilities then this would further enhance the ability of the NREGS to buffer rural households. Second, from my empirical analysis richer households benefit more from the mitigating impact of the NREGS than poorer households. This suggests rent seeking and indicates that there are implementation and institutional problems with the program. Given that the poorest are the least able to cope with shocks it is important that the program should benefit them positively as well. This differential impact suggests that the way in which public programs are designed is of vital importance and has consequences beyond the immediate aims of these programs. Third, while the results derived in this paper rest on the unitary household assumption, the NREGS in practice leads to increased female labour supply. This implies that the impact of the NREGS could be stronger given that an increase in labour supply could enhance the bargaining power of females and thus lead to improved child outcomes. Finally, given that the early phase-in districts see large impacts of the NREGS it therefore implies that the timing of these programs is critical. The gains from receiving the program early on are significant and policymakers need to be cognizant of this.

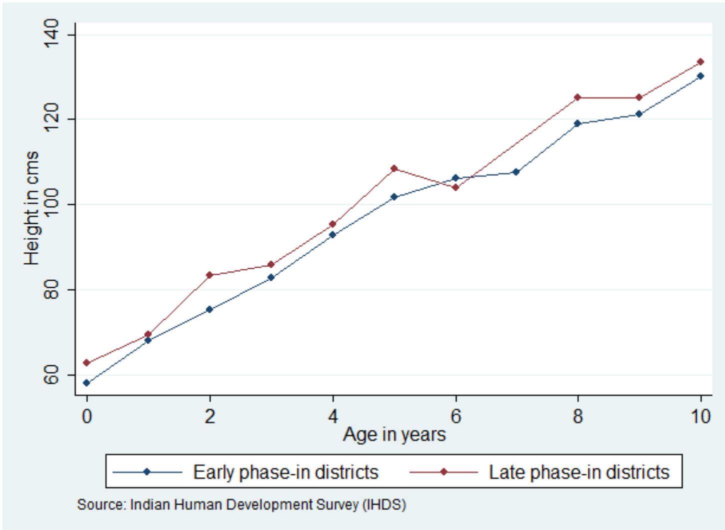


Figure 3.1: Pre-intervention (2004) Height in cms by Age in Years

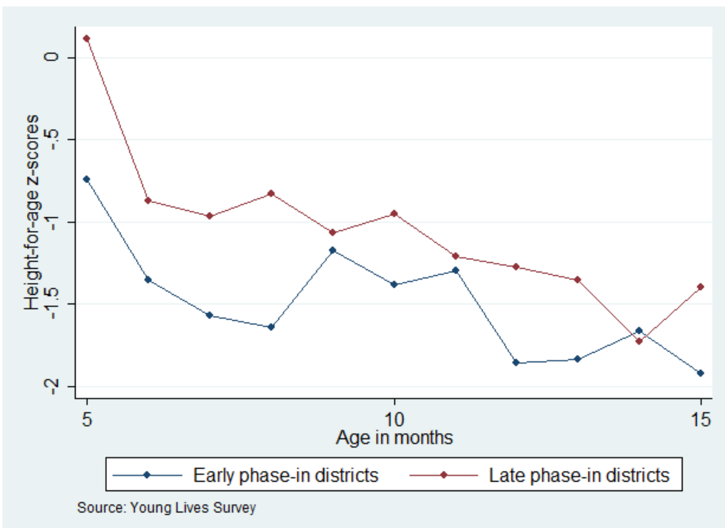


Figure 3.2: Pre-intervention (2002) Height-for-age by Age in months

Table 3.2: Summary statistics

Variables	**2002**	**2007**	**2009-10**
Height-for-age z-score	-1.43	-1.76	-1.58
	(1.65)	(1.33)	(1.19)
Gender	0.53	0.53	0.53
	(0.50)	(0.50)	(0.50)
Age (in months)	11.83	64.29	95.15
	(3.53)	(3.78)	(3.76)
SC/ST dummy	0.39	0.37	0.37
	(0.49)	(0.48)	(0.48)
OBC dummy	0.48	0.49	0.51
	(0.50)	(0.50)	(0.50)
Religion dummy	0.92	0.92	0.97
	(0.26)	(0.16)	(0.16)
Schooling Head dummy	0.30	0.50	0.50
	(0.46)	(0.50)	(0.50)
Household size	5.65	5.67	5.68
	(2.47)	(2.33)	(2.34)
Debt dummy	0.61	0.75	0.38
	(0.49)	(4.19)	(0.48)
Land owned (acres)	2.73	1.91	3.04
	(2.87)	(2.68)	(3.97)
Supplement dummy	0.42	0.44	0.44
	(0.49)	(0.50)	(0.50)
Wealth Index	0.14	0.17	0.27
	(0.15)	(0.16)	(0.17)
Rainshock	-0.12	0.57	0.59
	(0.76)	(0.83)	(0.93)

Table 3.3: Summary statistics contd.

Variable	Early Phase-in Districts	Late Phase-in Districts	t[a]
Height-for-age (in 2002)	-1.30	-1.61	-3.97*
Height-for-age (in 2007)	-1.82	-1.63	2.84*
Height-for-age (in 2009-10)	-1.63	-1.48	2.21*
Height-for-age if caregiver completed primary	-1.50	-1.37	2.22*
Height-for-age if caregiver not completed primary	-1.66	-1.73	-1.34
Height-for-age (Females)	-1.51	-1.48	0.44
Height-for-age (Males)	-1.65	-1.69	-0.64
Gender	0.53	0.52	-1.12
Age (in months)	56.07	56.33	0.21
SC/ST dummy	0.34	0.42	4.77**
OBC dummy	0.49	0.49	-0.17
Religion dummy	0.96	0.95	-1.70
Schooling Head dummy	0.45	0.39	-3.96*
Household Size	5.84	5.30	-6.79**
Debt dummy	0.66	0.41	-3.12*
Land owned (acres)	2.89	1.55	-11.31***
Supplement dummy	0.37	0.56	11.31***
Wealth Index	0.39	0.38	-2.72*

[a] Test of equality of means, *Significant at the 5 percent level

Table 3.4: Regression Results: Specification I - Dependent Variable: Height-for-age z-score

Variables	(1) OLS	(2) FE	(3) FE
NREGS	-0.003	0.093	0.081
	(0.144)	(0.187)	(0.183)
Shock	-0.477**	-0.496**	-0.507***
	(0.170)	(0.133)	(0.122)
NREGS*Shock	0.428**	0.576***	0.583***
	(0.141)	(0.079)	(0.069)
Debt	-0.007**	0.008**	0.008**
	(0.002)	(0.002)	(0.002)
Age	-0.024*	-0.009	-0.008
	(0.011)	(0.022)	(0.023)
Wealth Quartile 1	-0.404***	-0.174	-0.172
	(0.077)	(0.091)	(0.092)
Wealth Quartile 2	-0.191	-0.108	-0.111
	(0.110)	(0.091)	(0.091)
Wealth Quartile 3	-0.147**	-0.056	-0.053
	(0.050)	(0.033)	(0.036)
SC/ST	-0.221**	0.047	0.026
	(0.075)	(0.088)	(0.079)
OBC	-0.339**	0.024	0.019
	(0.090)	(0.064)	(0.060)
Gender	-0.161**		
	(0.061)		
Geligion	-0.009	-0.036	-0.040
	(0.233)	(0.110)	(0.107)
Schooling Head	0.221**	0.049	0.052
	(0.062)	(0.068)	(0.060)
Household Size	-0.019*	-0.001	-0.003
	(0.007)	(0.010)	(0.011)
Land Owned (acres)	0.010	-0.006	-0.006
	(0.009)	(0.005)	(0.005)
Food Supplement			0.139
			(0.083)
Constant	-0.533	-1.206***	-1.257***
	(0.325)	(0.240)	(0.255)
Year Dummies	Yes	Yes	Yes
Observations	3,232	3,232	3,232
R-squared	0.115	0.145	0.149

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Regression Results: Specification II - Dependent Variable: Height-for-age z-score

Variables	(1) Diff-in-diff	(2) Diff-in-diff	(3) Difference-GMM
Shock	-0.610**	-0.606***	-0.116**
	(0.167)	(0.149)	(0.052)
Time1	0.058	0.013	-0.153***
	(0.103)	(0.100)	(0.028)
Time2	0.416	0.311	
	(0.251)	(0.210)	
Time1*Shock	0.871**	0.859***	0.261***
	(0.228)	(0.203)	(0.089)
Time2*Shock	0.593***	0.584***	0.018
	(0.113)	(0.098)	(0.042)
Food Supplement		0.139	
		(0.080)	
Debt	0.008**	0.009**	0.001
	(0.002)	(0.002)	(0.002)
Age	-0.016	-0.015	-0.035
	(0.024)	(0.025)	(0.025)
Wealth Quartile 1	-0.177	-0.176	-0.079
	(0.092)	(0.094)	(0.059)
Wealth Quartile 2	-0.113	-0.117	-0.084
	(0.092)	(0.092)	(0.071)
Wealth Quartile 3	-0.060	-0.056	-0.045
	(0.033)	(0.035)	(0.036)
SC/ST	0.033	0.010	-0.240***
	(0.088)	(0.082)	(0.045)
OBC	0.013	0.006	-0.114***
	(0.064)	(0.060)	(0.043)
Religion	-0.013	-0.022	
	(0.128)	(0.123)	
Schooling Head	0.042	0.048	
	(0.072)	(0.063)	
Household Size	-0.001	-0.003	0.015
	(0.009)	(0.010)	(0.186)
Land Owned (acres)	-0.006	-0.005	0.001
	(0.005)	(0.005)	(0.006)
Lagged Height-for-age			0.063**
			(0.029)
Constant	-1.150***	-1.190***	1.291
	(0.266)	(0.284)	(0.788)
Year Dummies	Yes	Yes	Yes
Observations	3,232	3,232	887
R-squared	0.150	0.154	

Cluster robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.6: Regression Results: Specification I (by Wealth Quartiles and Gender) - Dependent Variable: Height-for-age z-score

Variables	(1) **FE (Wealth ≤ Median)**	(2) **FE (Wealth > Median)**	(3) **FE (Female)**	(4) **FE (Male)**
NREGS	0.141	0.142	0.243	-0.054
	(0.161)	(0.261)	(0.195)	(0.186)
Shock	-0.549***	-0.469*	-0.458**	-0.530***
	(0.108)	(0.184)	(0.144)	(0.123)
NREGS*Shock	0.616***	0.545***	0.499***	0.647***
	(0.072)	(0.129)	(0.097)	(0.066)
Debt	0.001	0.010**	0.007	0.008*
	(0.003)	(0.004)	(0.006)	(0.003)
Age	-0.030	-0.005	0.001	-0.013
	(0.020)	(0.031)	(0.022)	(0.033)
Wealth Quartile 2	0.062		-0.083	-0.134
	(0.042)		(0.093)	(0.091)
SC/ST	0.200**	-0.446	-0.112	0.230
	(0.064)	(0.298)	(0.352)	(0.150)
OBC	0.087	0.098	-0.037	0.051
	(0.176)	(0.095)	(0.089)	(0.130)
Religion	-0.022	-0.127	-0.123	0.101
	(0.059)	(0.260)	(0.219)	(0.184)
Schooling Head	-0.065	-0.003	0.205*	-0.098
	(0.110)	(0.120)	(0.098)	(0.097)
Household Size	-0.070*	0.021	-0.002	0.005
	(0.032)	(0.025)	(0.018)	(0.022)
Land Owned (acres)	-0.004	-0.001	-0.026	0.006
	(0.018)	(0.004)	(0.022)	(0.014)
Wealth Quartile 3		-0.071**	-0.101*	-0.024
		(0.025)	(0.047)	(0.053)
Wealth Quartile 1			-0.214*	-0.132
			(0.101)	(0.081)
Constant	-0.880	-1.178***	-0.995*	-1.522***
	(0.442)	(0.180)	(0.423)	(0.335)
Year Dummies	Yes	Yes	Yes	Yes
Observations	1,654	1,578	1,497	1,735
R-squared	0.141	0.161	0.165	0.150

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Regression Results: Specification II (by Wealth Quartiles and Gender) - Dependent Variable: Height-for-age z-score

	(1)	(2)	(3)	(4)
Variables	**DID (Wealth $\leq$ Median)**	**DID (Wealth $>$ Median)**	**DID (Female)**	**DID (Male)**
Time1*Shock	0.931***	0.927**	0.820**	0.918***
	(0.160)	(0.312)	(0.229)	(0.219)
Time2*Shock	0.619***	0.581**	0.512***	0.651***
	(0.088)	(0.167)	(0.120)	(0.106)
Time1	0.141	0.056	0.202	-0.121
	(0.090)	(0.176)	(0.111)	(0.111)
Time2	0.509**	0.532*	0.571**	0.183
	(0.176)	(0.261)	(0.162)	(0.305)
Shock	-0.687***	-0.597**	-0.582**	-0.627**
	(0.134)	(0.211)	(0.164)	(0.165)
Debt	0.003	0.010**	0.007	0.008*
	(0.004)	(0.003)	(0.006)	(0.004)
Age	-0.042	-0.009	-0.010	-0.017
	(0.024)	(0.032)	(0.021)	(0.034)
Wealth Quartile 2	0.062		-0.093	-0.136
	(0.046)		(0.097)	(0.091)
SC/ST	0.160*	-0.465	-0.133	0.219
	(0.072)	(0.291)	(0.356)	(0.148)
OBC	0.035	0.094	-0.047	0.038
	(0.167)	(0.088)	(0.080)	(0.142)
Religion	0.014	-0.096	-0.087	0.104
	(0.065)	(0.282)	(0.242)	(0.190)
Schooling Head	-0.075	-0.008	0.197*	-0.102
	(0.111)	(0.124)	(0.097)	(0.102)
Household Size	-0.069*	0.022	-0.003	0.006
	(0.032)	(0.025)	(0.018)	(0.022)
Land Owned (acres)	-0.002	-0.001	-0.025	0.006
	(0.018)	(0.004)	(0.023)	(0.014)
Wealth Quartile 3		-0.069**	-0.111*	-0.021
		(0.026)	(0.045)	(0.053)
Wealth Quartile 1			-0.229*	-0.127
			(0.106)	(0.086)
Constant	-0.742	-1.185***	-0.889*	-1.480***
	(0.439)	(0.212)	(0.409)	(0.349)
Year Dummies	Yes	Yes	Yes	Yes
Observations	1,654	1,578	1,497	1,735
R-squared	0.147	0.169	0.172	0.154

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Regression Results (by Land Ownership) - Dependent Variable: Height-for-age z-score

	(1)	(2)
Variables	**FE (Land ≤ Median)**	**FE (Land > Median)**
NREGS	0.073	0.129
	(0.142)	(0.343)
Shock	-0.563***	-0.515*
	(0.109)	(0.206)
NREGS*Shock	0.715***	0.494**
	(0.062)	(0.136)
Debt	-0.005	0.010**
	(0.003)	(0.003)
Age	-0.003	-0.042
	(0.021)	(0.026)
Wealth Quartile 1	-0.055	-0.158
	(0.066)	(0.119)
Wealth Quartile 2	-0.045	-0.072
	(0.081)	(0.158)
Wealth Quartile 3	0.045	-0.109
	(0.036)	(0.073)
SC/ST	-0.211**	0.401**
	(0.082)	(0.151)
OBC	-0.074	-0.045
	(0.146)	(0.147)
Religion	-0.197	0.017
	(0.172)	(0.119)
Schooling Head	0.085	0.076
	(0.073)	(0.084)
Household Size	0.014	0.001
	(0.016)	(0.011)
Land Owned (acres)	-0.062	0.002
	(0.063)	(0.003)
Constant	-1.132**	-0.964*
	(0.303)	(0.387)
Year Dummies	Yes	
Observations	2,104	1,128
R-squared	0.135	0.235

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Regression Results: Robustness Check - Dependent Variable: Height-for-age z-score

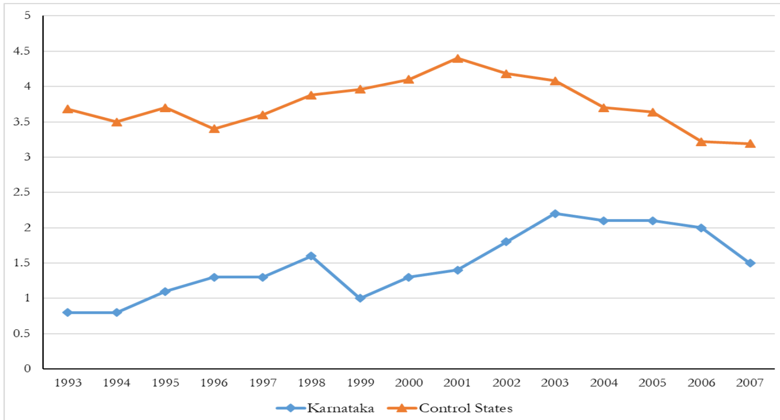
Variables	(1) FE
NREGS	0.160*
	(0.047)
Shock	0.028
	(0.027)
NREGS*Shock	0.002
	(0.055)
Debt	0.047**
	(0.017)
Age	-0.005***
	(0.001)
Wealth Quartile 1	-0.116
	(0.117)
Wealth Quartile 2	-0.048
	(0.062)
Wealth Quartile 3	-0.056*
	(0.025)
SC/ST	-0.496***
	(0.044)
OBC	-0.240***
	(0.058)
Gender	0.085
	(0.073)
Religion	-0.017
	(0.094)
Schooling Head	0.182***
	(0.018)
Household Size	0.024*
	(0.010)
Land Owned (acres)	0.001*
	(0.000)
Constant	-1.253***
	(0.194)
Year Dummies	Yes
Observations	2,097
Number of childid	704
R-squared	0.069

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix A: Appendix for Chapter 1

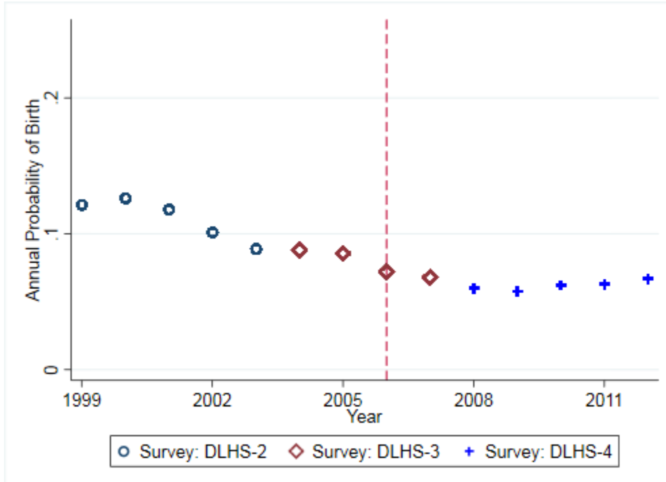
A.1 Additional Figures and Tables

Figure A.1: Percentage of Pregnancies Aborted



Note: This figure shows the percentage of pregnancies aborted for Karnataka and Control States using data from `http://www.johnstonsarchive.net.html`.

Figure A.2: Annual Probability of Birth for Control States: Survey Round Wise



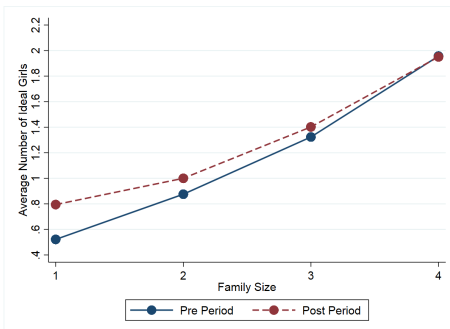
Note: Data is from each survey round included in the sample. Estimates are only for control states. Includes women who were between 15-44 years of age at the time of the survey.

Table A.1: Census Data: Pre-Period Descriptive Statistics

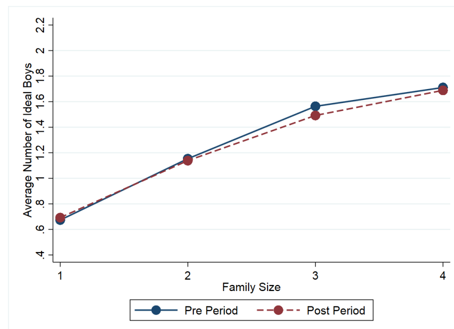
	1991			**2001**		
	Karnataka	Control	*p-value*	Karnataka	Control	*p-value*
Average Child Sex Ratio	964	961	0.810	944	940	0.936
Log of village population	6.643	6.861	0.287	6.761	6.988	0.264
Fraction Literate - Male	0.585	0.598	0.731	0.703	0.735	0.277
Fraction Literate - Female	0.323	0.336	0.719	0.474	0.503	0.370
Labour Supply Rate - Male	0.705	0.686	0.539	0.602	0.614	0.625
Labour Supply Rate - Female	0.295	0.314	0.539	0.602	0.614	0.624
Fraction Scheduled Caste	0.195	0.191	0.912	0.196	0.189	0.849
Primary School	0.910	0.899	0.859	0.877	0.906	0.665
Middle School	0.416	0.507	0.376	0.593	0.374	0.032
Secondary School	0.106	0.483	0.000	0.174	0.187	0.861
Atleast 1 public health facility	0.064	0.103	0.458	0.242	0.232	0.909
Tap water	0.187	0.195	0.922	0.575	0.467	0.293
Paved approach road	0.671	0.441	0.020	0.726	0.724	0.978
Power supply	0.979	0.801	0.000	0.989	0.616	0.000
Number of PSUs	25,044	104,641		25,044	104,641	

Note: Data is from the Census datasets 1991 and 2001 for subsample I which includes all PSUs of Karnataka and control states. Observations with child sex ratio values outside the Mean +/- 3 S.D. range have been dropped. Only PSUs which were matched in 1991, 2001 and 2011 have been retained. At least 1 public health facility includes the existence of at least one primary health centre or sub-centre.

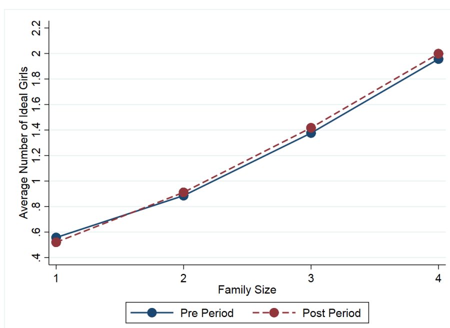
Figure A.3: Average Number of Children Desired at Different Family Sizes: Mother's Age 25-34 years



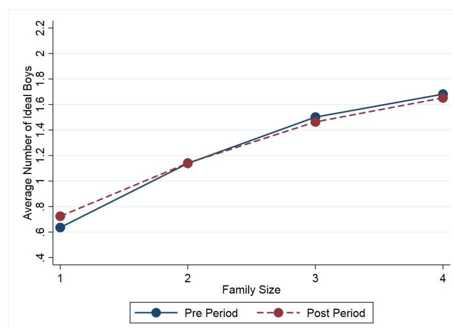
(a) Karnataka: Ideal Girls vs Family Size



(b) Karnataka: Ideal Boys vs Family Size



(c) Control: Ideal Girls vs Family Size



(d) Control: Ideal Boys vs Family Size

Note: Data is from the District Level Household Survey. Figures (a)-(b) show how desired girls and boys respectively, vary with family size in Karnataka in the pre- and post-program periods, while figures (c)-(d) do the same for control states. Sample is restricted to women between 25-34 years of age.

Table A.2: District Level Household Survey: Pre-Period Descriptive Statistics of Woman Level Analysis

	Pre-Period 1999-2006		
	Karnataka	Control	*p-value*
Total Fertility Rate	1.698	1.592	0.772
Annual Probability of Birth	13.051	12.547	0.953
Annual Probability of Female Birth	6.175	6.033	0.981
Annual Probability of Male Birth	6.880	6.513	0.955
Proportion of Females at Birth	0.479	0.483	0.812
Ideal No. of Boys	1.147	1.100	0.092
Ideal No. of Girls	0.996	0.977	0.056
Fraction Scheduled Caste (SC)	0.182	0.209	0.798
Fraction Rural	0.705	0.679	0.832
Fraction Hindu	0.851	0.797	0.581
Fraction Muslim	0.130	0.136	0.948
Woman's age cohort: 15-18	0.045	0.031	0.774
Woman's age cohort: 19-24	0.319	0.312	0.954
Woman's age cohort: 25-30	0.359	0.372	0.915
Woman's age cohort: 31-44	0.234	0.251	0.877
Woman's Years of Education	4.821	5.573	0.539
Lowest Wealth Quintile	0.417	0.350	0.592
Middle Wealth Quintile	0.368	0.382	0.912
Highest Wealth Quintile	0.215	0.269	0.625

Note: Data is from the woman-year panel for the pre-program period from 1999-2006. This table presents means of the main outcomes and woman level covariates used in the paper for the woman level analysis.

Table A.3: District Level Household Survey: Pre-Period Descriptive Statistics of Child Level Analysis

	Pre-Period 1995-2005		
	Karnataka	Control	*p-value*
Number of months exclusively breastfed (Girls)	4.807	3.241	0.079
Number of months exclusively breastfed (Boys)	4.995	3.195	0.058
Probability Exclusively Breastfed (Girls)	0.315	0.190	0.972
Probability Exclusively Breastfed (Boys)	0.328	0.188	0.813
Infant Mortality Rate (Girls)	1.269	1.061	0.067
Infant Mortality Rate (Boys)	1.153	1.113	0.717
Fraction Scheduled Caste (SC)	0.185	0.206	0.840
Fraction Rural	0.685	0.654	0.800
Fraction Hindu	0.828	0.782	0.655
Fraction Muslim	0.152	0.145	0.939
Mother's age cohort: 15-18	0.020	0.012	0.805
Mother's age cohort: 19-24	0.370	0.332	0.763
Mother's age cohort: 25-30	0.424	0.449	0.851
Mother's age cohort: 31-44	0.161	0.191	0.759
Mother's Years of Education	8.360	8.328	0.968
Father's Years of Education	8.975	8.668	0.730
Lowest Wealth Quintile	0.446	0.386	0.641
Middle Wealth Quintile	0.354	0.377	0.857
Highest Wealth Quintile	0.200	0.237	0.727

Note: Data is from the child level sample for the pre-program period from 1995-2005. Sample restricted to children of birth orders 1, 2 and 3. This table presents means of the main outcomes and child level covariates used in the paper for the child level analysis. Infant mortality rate is defined as % of all births that do not survive.

Table A.4: Effect of Hypothetical *Bhagyalakshmi* on Child Sex Ratio

A. Pooled Sample	(1)	(2)
Karnataka*Post	4.196	1.778
	(11.561)	(5.193)
Observations	259,370	259,370
Number of PSUs	129,685	129,685
B. Border Districts Only	(1)	(2)
Karnataka*Post	4.775	1.298
	(6.047)	(3.598)
Observations	46,430	46,430
Number of PSUs	23,215	23,215
Year FE	x	x
PSU FE	x	x
Covariates		x

Note: This table reports the coefficients of variable $Program_{jt}$ from specification 1.2 for a hypothetical program introduced in 1996. In addition I include, female and male literacy rates; female and male labour force participation rates; fraction scheduled caste; log population of a PSU. *Post* is defined as = 2001. *Karnataka=1* if PSU j is in Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%.

Table A.5: Ideal Number of Children: Pre-Program Trends

A. Ideal Girls	**Survey Round DLHS-2**	**Survey Round NFHS**	**Change**
Karnataka	0.859	0.690	-0.169
Control States	0.885	0.773	-0.162
B. Ideal Boys	(1)	(2)	(3)
Karnataka	1.023	0.795	-0.228
Control States	0.970	0.746	-0.224

Note: Data is from the DLHS-2 and the National Family Health Survey (NFHS 2005-06). Reports levels of self-reported ideal number of girls and ideal number of boys for Karnataka and the control states in the pre-program period. The question used is: *Would you like to have another child or would you prefer not to have any more children? Would you prefer your next child to be a girl or boy?*

Table A.6: Difference-in-Difference Estimates: Effect of Hypothetical Programs on Fertility

A. Probability of a Birth	*Post1997*	*Post1998*	*Post1999*	*Post2000*	*Post2001*	*Post2002*
Karnataka*Post	0.5106	-0.1621	-0.2862	-0.1052	-0.2226	-0.3909
	(0.517)	(0.471)	(0.307)	(0.278)	(0.418)	(0.447)
Observations	1,285,682	1,285,682	1,285,682	1,285,682	1,285,682	1,285,682
Mothers	259,870	259,870	259,870	259,870	259,870	259,870
B. Probability of a Female Birth	*Post1997*	*Post1998*	*Post1999*	*Post2000*	*Post2001*	*Post2002*
Karnataka*Post	0.1844	-0.1834	-0.3384**	-0.4256*	-0.3318	0.0014
	(0.245)	(0.228)	(0.153)	(0.240)	(0.210)	(0.202)
Observations	1,285,682	1,285,682	1,285,682	1,285,682	1,285,682	1,285,682
Mothers	259,870	259,870	259,870	259,870	259,870	259,870
C. Probability of a Male Birth	*Post1997*	*Post1998*	*Post1999*	*Post2000*	*Post2001*	*Post2002*
Karnataka*Post	0.3262	0.0212	0.0521	0.3203**	0.1092	-0.3924
	(0.274)	(0.244)	(0.198)	(0.144)	(0.244)	(0.251) 3)
Observations	1,285,682	1,285,682	1,285,682	1,285,682	1,285,682	1,285,682
Mothers	259,870	259,870	259,870	259,870	259,870	259,870

Note: Sample is restricted to years 1995-2005. *PostT=1* if *year*> *T*. Estimates include state fixed effects, year fixed effects, state specific linear trends and covariates. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%.

Table A.7: Difference-in-Difference Estimates: Effect of *Bhagyalakshmi* on Fertility Excluding One State at a Time

A. Probability of a Birth/State Excluded →	**Tamilnadu**	**Kerala**	**Maharashtra**	**West Bengal**	**Andhra Pradesh**
Karnataka*Post	0.9382*	0.4097**	0.8844**	0.9298*	1.0223*
	(0.498)	(0.198)	(0.402)	(0.554)	(0.527)
Observations	1,159,348	1,300,851	1,045,331	1,205,321	1,220,519
Mothers	231,641	259,468	208,406	240,464	243,957
B. Probability of a Female Birth/State Excluded →	**Tamilnadu**	**Kerala**	**Maharashtra**	**West Bengal**	**Andhra Pradesh**
Karnataka*Post	0.5735**	0.3221***	0.5432**	0.5803**	0.6031***
	(0.280)	(0.094)	(0.250)	(0.266)	(0.251)
Observations	1,159,348	1,300,851	1,045,331	1,205,321	1,220,519
Mothers	231,641	259,468	208,406	240,464	243,957
C. Probability of a Male Birth/State Excluded →	**Tamilnadu**	**Kerala**	**Maharashtra**	**West Bengal**	**Andhra Pradesh**
Karnataka*Post	0.3647	0.0876	0.3411	0.3495	0.4192
	(0.318)	(0.107)	(0.384)	(0.308)	(0.276)
Observations	1,159,348	1,300,851	1,045,331	1,205,321	1,220,519
Mothers	231,641	259,468	208,406	240,464	243,957

Note: Each cell is a separate regression. Each cell excludes one of the control states and re-estimates specification 1.3. Estimates include state fixed effects, year fixed effects, state specific linear trends and covariates. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%.

Table A.8: Effect of *Bhagyalakshmi* on Mortality and Breastfeeding: Family Characteristics

Dependent Variable ↓	**Karnataka*Post*Female**	**Karnataka*Female**
Mother's Age at 1st Birth	-0.0091	-0.0326***
	(0.005)	(0.007)
Mother's Years of Education	0.0394	-0.0075
	(0.030)	(0.013)
Father's Years of Education	-0.0412	-0.0087
	(0.044)	(0.011)
Rural	0.0019	0.0017
	(0.004)	(0.004)
Standard of Living Index	0.0054*	0.0079**
	(0.003)	(0.004)
Observations	204,230	204,230

Note: Each row is a separate regression with the dependent variable in column (1) and the relevant independent variables in columns (2) and (3). This table reports the coefficients from specification 1.6 where dependent variables are: mother's age at 1st birth (row 1), mother's years of education (row 2), father's years of education (row 3), residence in a rural area (row 4) and standard of living index (row 5). Each regression includes birth fixed FE, state FE, state-by-birth year FE and female-by-birth year FE. *Post* is defined as >= March 31st, 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%.

Table A.9: Heterogeneity: Fertility - Coefficients on *Karnataka*Post*

	Woman's Education		**Household Wealth**			**Caste**		**Sector**		**Religion**	
	Less than Secondary	Secondary or Higher	SLI Index = 1	SLI Index = 2	SLI Index = 3	SC	General/OBC	Rural	Urban	Hindu	Muslim
Probability of Birth	0.6701**	1.1565*	-0.0016	1.0738***	0.1019*	-0.1544	0.9746**	1.1432***	0.0994**	0.6703*	1.4604**
	(0.278)	(0.619)	(0.112)	(0.357)	(0.054)	(0.403)	(0.425)	(0.414)	(0.032)	(0.381)	(0.679)
Probability of Female Birth	0.3165**	0.7815***	-0.0240	0.6160***	0.1322**	-0.1839	0.5867***	0.6161***	0.1318**	0.4239**	0.9703***
	(0.136)	(0.390)	(0.095)	(0.159)	(0.065)	(0.206)	(0.188)	(0.198)	(0.061)	(0.182)	(0.361)
Probability of Male Birth	0.3536***	0.3749	0.0224	0.4578**	-0.0303	0.0284	0.3879	0.5270**	-0.0323	0.2464	0.4901
	(0.144)	(0.459)	(0.068)	(0.202)	(0.355)	(0.202)	(0.267)	(0.216)	(0.250)	(0.199)	(0.403)
Observations	961,775	442,837	388,476	574,093	442,043	299,943	1,104,669	914,730	489,882	1,129,401	194,568

Note: Each column reports estimates of specification 1.3 for different subsamples. SLI Index refers to the standard of living index SLI ranges from 1 to 3 for low, medium and high standard of living respectively. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%.

Table A.10: Heterogeneity: Infant Mortality and Breastfeeding - Coefficients on *Karnataka*Female*Post and Karnataka*Female*

	Mother's Education		Household Wealth			Caste		Sector		Religion	
	Less than Secondary	Secondary or Higher	SLI Index = 1	SLI Index = 2	SLI Index = 3	SC	General/OBC	Rural	Urban	Hindu	Muslim
Infant Mortality											
Karnataka*Female*Post	-0.4626***	-0.0298	-0.6398**	-0.2490***	-0.2798	-0.4048***	-0.1650	-0.4075***	-0.2182	-0.0844***	-0.4649**
	(0.139)	(0.031)	(0.327)	(0.019)	(0.223)	(0.169)	(0.146)	(0.117)	(0.156)	(0.035)	(0.228)
Karnataka*Female	0.1298*	0.0480	0.3988***	0.1322***	-0.2083	0.3749***	0.0892	0.2566***	0.0792	0.1718***	0.0681
	(0.075)	(0.036)	(0.068)	(0.056)	(0.155)	(0.049)	(0.035)	(0.043)	(0.073)	(0.036)	(0.075)
Observations	48,885	137,869	58,044	75,280	53,428	40,439	146,315	121,755	64,999	148,937	26,908
Months of Exclusive Breastfeeding											
Karnataka*Female*Post	0.2461*	0.1745**	0.2027	0.4252***	-0.0297	0.0788	0.2258***	0.2357***	0.1389	0.2179**	0.1602
	(0.127)	(0.085)	(0.780)	(0.132)	(0.614)	(0.257)	(0.055)	(0.099)	(0.091)	(0.108)	(0.147)
Karnataka*Female	-0.2515***	0.0139	-0.5331	-0.1351***	0.0303	0.0347	-0.1110***	-0.0769***	-0.1132***	-0.1247***	0.1259
	(0.046)	(0.027)	(0.765)	(0.023)	(0.544)	(0.038)	(0.025)	(0.026)	(0.013)	(0.017)	(0.082)
Observations	31,197	42,054	21,286	28,568	23,396	15,497	57,754	47,177	26,074	58,990	9929

Note: Each column reports estimates of specification 1.6 for different subsamples. SLI Index refers to the standard of living index SLI ranges from 1 to 3 for low, medium and high standard of living respectively. *Post* is defined as >= March 31st, 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors are clustered by state. *** 1%, ** 5%, * 10%.

Table A.11: Indian Human Development Survey: Pre-Period Descriptive Statistics for Analysis of Nutritional Outcomes

	Pre-Period 1995-2005		
	Karnataka	Control	*p-value*
Height-for-Age z-score (Girls)	-2.041	-1.758	0.594
Height-for-Age z-score (Boys)	-1.872	-1.694	0.738
Weight-for-Age z-score (Girls)	-1.835	-1.512	0.399
Weight-for-Age z-score (Boys)	-1.851	-1.503	0.428
Fraction Scheduled Caste (SC)	0.201	0.238	0.741
Fraction Rural	0.750	0.614	0.249
Fraction Hindu	0.813	0.772	0.700
Fraction Muslim	0.165	0.163	0.987
Highest Years of Education of Adult Male	6.628	7.198	0.668
Highest Years of Education of Adult Female	5.266	6.077	0.521
Monthly Consumption per Capita	1916	1833	0.960

Note: Data is from the Indian Human Development Survey (IHDS) for the pre-program period from 1995-2005. This table presents means of the main outcomes and child level covariates used in the paper for the analysis of long run nutritional outcomes. Monthly consumption per capita is in rupees.

Table A.12: Quantification of Program Impacts

A. Regression Estimates	**Pre-Program Period**	**Post-Program Period**	**Δ (Post-Pre)**
F_K	0.4791	0.4862	0.0071
EFM_K	0.0016	0.0002	-0.0013
B. Decomposition			
m_K (number of mothers in Karnataka)	14,300,000		
N_K	1,144,000	1,158,243 (=1,144,000 + 0.00092*m)	13,213
C. Change in the number of missing girls			
(1) $N_{K,post} * \Delta F_K * \frac{-1}{0.51}$	= 16,124		
(2) $\Delta N_K * (\frac{0.49}{0.51} - \frac{F_{K,pre}}{0.51})$	= 475		
(3) = (1) + (2)	16,599		
D. Change in the female infant mortality			
(1) $N_{K,post} * F_{K,post} * \Delta EFM_K$	= -738		
(2) $N_{K,post} * \Delta F_K * EFM_{K,pre}$	= -19		
(3) $\Delta N_K * F_{K,pre} * EFM_{K,pre}$	= -11		
(4) = (1) + (2) + (3)	-770		

Table A.13: Cost-Effectiveness of the Incentive

A. Outcome Measure	**Discount Rate (%)**	**Outcome per \$1000 Spent (implementer & beneficiary)**	**Outcome per \$1000 Invested (implementer)**
Number of sex-selective abortions averted	3	0.732	0.909
	6	1.227	1.525
	9	2.021	2.521
Number of excess female infant deaths averted	3	0.022	0.028
	6	0.038	0.047
	9	0.063	0.079
Additional Months of Exclusive Breastfeeding	3	0.095	0.118
	6	0.160	0.198
	9	0.264	0.328

A.2 Quantitative Estimates of the Impact of the *Bhagyalakshmi* Program

I first use the regression estimates on the impact of the *Bhagyalakshmi* program on child sex ratio to calculate the number of girls who survived up to age 6. Then, I decompose this number into the number of girls who were *not* selectively aborted as well as the number of girls who survived up to age one.

Following the methodology outlined in Anukriti et al. [2016], I first define the following:

- N: Number of births in Karnataka and control states
- N_K: Number of births in Karnataka
- M_K: Fraction of male births among N_K
- F_K: Fraction of female births among N_K
- Δ: Pre-post difference
- m_K: Number of mothers in Karnataka

From Anukriti et al. [2016], the expression for the number of "missing girls" is:

$$\frac{0.49}{0.51}(N_K * M_K) - N_K * F_K = N_K(\frac{0.49}{0.51} - \frac{F_K}{0.51})$$

The first term on the left hand side of the above equation represents the expected number of female births while the second term is the observed number of female births in Karnataka.

To calculate the impact of the program on the total number of missing girls I estimate the number of missing girls in the pre- and post program periods (see Anukriti et al., 2016 for derivation of the following expression):

$$\Delta N_K * (\frac{0.49}{0.51} - \frac{F_{K,pre}}{0.51}) + N_{K,post} * \Delta F_K * \frac{-1}{0.51} \tag{A1}$$

As outlined in Anukriti et al. [2016], the impact of *Bhagyalakshmi* on the number of missing girls is comprised of two major changes: changes in overall fertility or the change in the probability of conceiving and changes in the probability of selectively aborting a girl.

A.2.1 Quantification of Impact on Child Sex Ratio

I convert the difference-in-differences estimates of 20 (estimate in column (3), Panel A of Table 1.1) into the number of additional female children who survived up to age 6. That is, the total (0-6) population was approximately 7,182,100 and

7,161,033 in 2001 and 2011 respectively. An additional 20 females per 1000 males implies an average annual additional 28,124 girls who survived up to age 6.[1]

A.2.2 Quantification of Impact on Sex-Selective Abortions

In Panel A of Table A.12 I present the regression estimates for sex-ratio at birth from the study. Column (1) reports the baseline mean of the sex ratio at birth (from Panel A of Table 1.3) and column (3) reports the regression estimate (column (2), Panel A of Table 1.3). Column (2) adds up columns (1) and (3). In Panel B, m_K is calculated using the total number of births in Karnataka divided by the gross fertility rate for Karnataka from the Sample Registration System (SRS, 2001) as 80 births per 1000 women. That is $m_K = \frac{N_K}{0.080}$.

Next, in Panel B, N_K is the total number of births in Karnataka in 2001. I use the Census 2001 to calculate the number in the pre-program period in column (1).[2] In column (2) to calculate the number of births in Karnataka in the post-program period I use the number of mothers in Karnataka and the endogenous change in fertility. To calculate the endogenous change in fertility I use the regression estimate in column (2), Panel A of Table 1.2 i.e 0.0077. I multiply this by a factor, 0.12 to arrive at 0.00092. Then, to calculate the total number of births in the post-program period I add total births in the pre-program period in column (1) to the total new births because of changing fertility (0.00092*m). The total change in the number of births is in column (3).

Finally in Panel C I row (1) represents the calculation of the "sex-selective abortion effect" and row (2) represents the calculation of the "conception" effect. From row (3) I get that the *Bhagyalakshmi* program led to 16,599 *additional* girls being born.

A.2.3 Quantification of Impact on Excess Female Mortality

Next, I estimate the number of girls that survived up to age 1 as a result of the program. Following the methodology outlined in Anukriti et al. [2016], excess female infant deaths annually is defined as:

$$N_K * F_K * EFM_K$$

where N_K and F_K are defined as before and EFM_K is the difference in the probability of death by age one between girls and boys in Karnataka. Then the reduction in female infant deaths as a result of *Bhagyalakshmi* is the difference between the

[1]This is calculated as follows from equation A1: $\Delta N_K * (\frac{0.49}{0.51} - \frac{F_{K,pre}}{0.51}) + N_{K,post} * \Delta F_K * \frac{-1}{0.51}$ =((0.49/0.51)-(0.48/0.51))*(-21067) + (7161033*0.020*(-1/0.51)) = 281,239. Averaging over the period of 2001-2011, I get an additional 28,124 girls in Karnataka.

[2]The male birth rate in Karnataka in the 2001 Census was 22 and the male population was 27 million, translating into 594,000 male births. Total male births=Birth Rate x Total male population. Similarly total female births is about 550,000. So total births in Karnataka were about 1,144,000 in 2001.

number of excess postnatal female deaths during the pre- and post-program periods (see Anukriti et al. [2016] for derivation of the following expression):

$$N_{K,post} * F_{K,post} * \Delta EFM_K + N_{K,post} * \Delta F_K * EFM_{K,pre} + \Delta N_K * F_{K,pre} * EFM_{K,pre}$$

The first term in the above expression is the decline in EFM due to better investments such as breastfeeding; the second term is the increase in the fraction of post-program births that are female due to a reduction in sex-selective abortions and finally the third term is the same endogenous fertility effect of the program.

In row 2, Panel A of Table A.12 I present the regression estimates from equation 1.6. Column (1) reports the baseline mean of excess female infant mortality while column (3) reports the triple difference estimate from Panel A, column 2, Table 1.6. In Panel D I calculate that *Bhagyalakshmi* program led to about 770 additional girls who survived up to the age of one.

Thus, in total 17,368 girls (= 16,599 + 770) were 'saved' *per year* as a result of the introduction of the *Bhagyalakshmi* program. Thus, out of the total annual average additional girls that survived up to age 6 i.e. 28,124; about 62 percent is attributable to fewer sex selective abortions and a larger fraction of girls surviving up to age 1.

A.3 Discussion

A.3.1 Cost Effectiveness Analysis

The most natural benchmark for the cost effectiveness of the *Bhagyalakshmi* program would be similar long-term conditional cash transfers that are offered to households with any or all of the following conditions: remaining unmarried till age 18, being enrolled in school and being immunized. Unfortunately, there are no credible evaluations of CCT programs in India and no experimental evaluations in South Asia more broadly (unlike in Africa and Latin America) which examine the four main outcomes from this study: child sex ratio; sex ratio at birth; infant mortality and exclusive breastfeeding duration. Nevertheless, I present a cost-effectiveness analysis of the *Bhagyalakshmi* program in Table A.13. These estimates are meant to give a sense of the relative efficiency of the program at meeting particular outcomes without requiring the full set of assumptions of a cost-benefit analysis.

To calculate the cost-effectiveness, I estimate the amount of a given outcome achieved by a given investment. For example, I use the estimates from the previous section on the total number of sex-selective abortions and number of excess female infant deaths averted by the program in a year (16,599 and 770 respectively). I then divide the present value of the costs of the program by the number of sex-selective abortions averted to determine the cost per sex-selective abortion averted and cost per female infant death averted. I then divide 1,000 by the cost per outcome measure to express the figure in terms of returns to a $1,000 investment.

The present value of the costs of the program include implementation costs as well as costs to the beneficiary. For the cost to the implementer, I consider actual program costs detailed in Section 1.3.[3] For the cost to the beneficiary I consider two types of couples - (a) couples for whom the *Bhagyalakshmi* did not change their total number of children born but only resulted in fewer sex-selective abortions. The opportunity cost of this group is the foregone old-age income support and dowry receipts from having fewer than the desired number of sons and, (b) couples for whom the *Bhagyalakshmi* program changed both the number and gender composition of children since such couples now had more than their expected number of children and more than their expected number of girls. For such couples, in addition to the opportunity costs of foregone old-age income support and dowry receipts I also include the opportunity cost of raising an additional child.[4,5]

My estimates imply that the *Bhagyalakshmi* program is relatively cost-effective. The conditional incentive translates into 1.525 number of sex-selective abortions averted, 0.047 female infant deaths averted and 0.20 additional months of exclusive breastfeeding for every \$1000 spent by the implementer *per year* (See Table A.13). While comparable cost-effectiveness estimates for similar outcomes are not available, Nandi and Deolalikar [2013] estimate the number of girls saved as a result of the Pre Conception Pre Natal Diagnostics Act (PNDT) of 1994 to be 81,500 over a 10-year period from 1991 to 2001 across 6 Indian states. Using these estimates and the district-wise budgetary allocation under the PNDT Act and performing similar calculations I estimate that the PNDT Act translated into 0.853 number of sex selective abortions averted for every \$1000 spent by the implementer. In contrast, the *Bhagyalakshmi* was more cost-effective in spite of being only a state-level program as opposed to the PNDT Act which was a nation-wide legislation.[6]

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[3]This is by all accounts an underestimate of the actual costs since I do not include monitoring costs of the program.

[4]I estimate average net dowry receipts to a boy's family to be Rs. 30,000 (\$465) based on the estimates in the Indian Human Development Survey (IHDS 2004-05). This includes dowry receipts for a boy's family minus their wedding expenses. I multiply this amount by the number of additional girls born due to fewer sex-selective abortions in the previous section i.e. due to the "sex-selective abortion effect" under the assumption that each girl represents one household.

[5]I estimate the average expenditure on health and education of a child for 18 years based on the National Sample Survey (2007-08) to be Rs. 96,000 (\$1490) and multiply this by the number of additional girls born due to the "conception effect" from the previous section, again assuming that each girl represents one household.

[6]Buchmann et al. [2017] provide cost-effectiveness estimates for a conditional cash transfer randomized trial in Bangladesh which provided financial incentives to adolescent girls (as opposed to the incentive given at birth in *Bhagyalakshmi* in the form of an 18-year bond) to delay marriage and child bearing. The authors find that the financial incentive delayed marriage by 6.6 years, averted 1.5 child marriages, and led to 3.6 years of additional schooling for every \$1,000 invested by the implementer.

A.4 Proofs

A.4.1 Proposition 1: Part (a)

First, substitute the budget constraints into Equation 1.1:

$$U_T = U_1(Y_1 - \theta N k_b - (1-\theta) N k_g) + U_2(Y_2 + \theta N p(k_b) - (1-\theta) N G p(k_g)) + U_S(p(k_b)\theta N + p(k_g)(1-\theta)N)$$

For a given fertility level, the optimal health care expenditure in girls and boys is:

$$\frac{\partial U_T(N,k_b,k_g)}{\partial k_g} = -U_1^{'}(1-\theta)N - U_2^{'}(1-\theta)Np'(k_g)G + U_S^{'}(1-\theta)Np'(k_g) = 0 \quad \text{(A2)}$$

$$\frac{\partial U_T(N,k_b,k_g)}{\partial k_b} = -U_1^{'}\theta N + U_2^{'}\theta N p'(k_b)B + U_S^{'}\theta N p'(k_b) = 0 \quad \text{(A3)}$$

Now, totally differentiating equations A2 and A3 with respect to N:

$$\frac{\partial^2 U_T(N,k_g,k_b)}{\partial k_g^2}\frac{\partial k_g^*(N,\theta)}{\partial N} + \frac{\partial^2 U_T(N,k_g,k_b)}{\partial k_b \partial k_g}\frac{\partial k_b^*(N,\theta)}{\partial N} + \frac{\partial^2 U_T(N,k_b,k_g)}{\partial N \partial k_g} = 0 \quad \text{(A4)}$$

$$\frac{\partial^2 U_T(N,k_g,k_b)}{\partial k_b^2}\frac{\partial k_b^*(N,\theta)}{\partial N} + \frac{\partial^2 U_T(N,k_g,k_b)}{\partial k_b \partial k_g}\frac{\partial k_g^*(N,\theta)}{\partial N} + \frac{\partial^2 U_T(N,k_b,k_g)}{\partial N \partial k_b} = 0 \quad \text{(A5)}$$

Below are the partial derivatives:

$$\to \frac{\partial^2 U_T}{\partial k_b^2} = U_1^{''}\theta^2 N^2 + U_2^{''}\theta^2 N^2 B^2 p'(k_b)^2 + U_2^{'}\theta N B p''(k_b) + U_S^{''}\theta^2 N^2 p'(k_b)^2 + U_S^{'}\theta N p''(k_b) < 0 \quad \text{(A6)}$$

$$\to \frac{\partial^2 U_T}{\partial k_g^2} = U_1^{''}(1-\theta)^2 N^2 + U_2^{''}(1-\theta)^2 N^2 G^2 p'(k_g)^2 - U_2^{'}(1-\theta)NGp''(k_g) + U_S^{''}(1-\theta)^2 N^2 p'(k_g)^2 + U_S^{'}(1-\theta)Np''(k_g) < 0 \quad \text{(A7)}$$

$$\to \frac{\partial^2 U_T}{\partial k_g \partial k_b} = U_1^{''}\theta(1-\theta)N^2 - U_2^{''}\theta(1-\theta)N^2 GBp'(k_g)p'(k_b) + U_S^{''}\theta(1-\theta)N^2 p'(k_g)p'(k_b) \quad \text{(A8)}$$

which is positive if G is large enough

$$\to \frac{\partial^2 U_T}{\partial N \partial k_g} = \Big[U_1^{''}(1-\theta)N(\theta k_b + (1-\theta)k_g) + U_2^{''}p'(k_g)(1-\theta)NG(Gp(k_g)(1-\theta) - Bp(k_b)\theta) + U_S^{''}p'(k_g)(1-\theta)N(\theta p(k_b) + (1-\theta)p(k_b))\Big] + \Big[-U_1^{'}(1-\theta) + U_S^{'}p'(k_g)(1-\theta) - U_2^{'}Gp'(k_g)(1-\theta)\Big] \quad \text{(A9)}$$

which is negative if G is large enough

$$\rightarrow \frac{\partial^2 U_T}{\partial N \partial k_b} = \Big[U_1^{''}\theta N(\theta k_b + (1-\theta)k_g) + U_2^{''}p'(k_b)\theta NB(Bp(k_b)\theta - Gp(k_g)(1-\theta)) + \\ + U_S^{''}p'(k_b)\theta N(\theta p(k_b) + (1-\theta)p(k_g))\Big] + \\ \Big[-U_1^{'}\theta + U_S^{'}p'(k_b)\theta + U_2^{'}Gp'(k_b)\theta\Big] \quad \text{(A10)}$$

which is positive if G is large enough

From equations A4 and A5 and applying Cramer's rule:

$$\frac{\partial k_g^*}{\partial N} = -\frac{Det\begin{vmatrix} \frac{\partial^2 U_T}{\partial N \partial k_g} & \frac{\partial^2 U_T}{\partial k_g \partial k_b} \\ \frac{\partial^2 U_T}{\partial N \partial k_b} & \frac{\partial^2 U_T}{\partial k_b^2} \end{vmatrix}}{Det\begin{vmatrix} \frac{\partial^2 U_T}{\partial k_g^2} & \frac{\partial^2 U_T}{\partial k_b \partial k_g} \\ \frac{\partial^2 U_T}{\partial k_g \partial k_b} & \frac{\partial^2 U_T}{\partial k_b^2} \end{vmatrix}} = -\frac{Det\begin{vmatrix} - & + \\ + & - \end{vmatrix}}{Det\begin{vmatrix} - & + \\ + & - \end{vmatrix}}$$

The denominator is positive if $\frac{\partial^2 U_T}{\partial k_b^2}\frac{\partial^2 U_T}{\partial k_g^2} > \frac{\partial^2 U_T}{\partial k_g \partial k_b}\frac{\partial^2 U_T}{\partial k_g \partial k_b}$ and the numerator is positive if $\frac{\partial^2 U_T}{\partial N \partial k_g}\frac{\partial^2 U_T}{\partial k_b^2} > \frac{\partial^2 U_T}{\partial k_g \partial k_b}\frac{\partial^2 U_T}{\partial N \partial k_b}$

Multiplying the numerator and denominator and simplifying, I get the conditions that, both of the determinants are positive if G is large enough, $U_S^{'} > U_2^{'}G$ and $U_S^{''} > U_2^{''}GB$. That is, if the marginal utility of survival is larger than the marginal consumption utility in period 2. From equation A2, $U_S^{'} > U_2^{'}G$ must be true because $\frac{U_1^{'}}{p'(k_g)} = U_S^{'} - U_2^{'}G$ which is positive because $\frac{U_1^{'}}{p'(k_g)} > 0$. From equation A8 $U_S^{''} > U_2^{''}GB$ must also be true since $-\frac{U_1^{''}}{p'(k_g)p'(k_b)} = U_S^{''} - U_2^{''}GB$ which is positive since $-\frac{U_1^{''}}{p'(k_g)p'(k_b)} > 0$

Thus,

$$\frac{\partial k_g^*(N,\theta)}{\partial N} < 0 \quad \text{(A11)}$$

This result says that fertility and expenditure per girl are perceived as *substitutes* at the optimum.

A.4.2 Proposition 1: Part (b)

Totally differentiating equations A2 and A3 with respect to θ:

$$\frac{\partial^2 U_T(N,k_g,k_b)}{\partial k_g^2}\frac{\partial k_g^*(N,\theta)}{\partial \theta} + \frac{\partial^2 U_T(N,k_g,k_b)}{\partial k_b \partial k_g}\frac{\partial k_b^*(N,\theta)}{\partial \theta} + \frac{\partial^2 U_T(N,k_b,k_g)}{\partial \theta \partial k_g} = 0 \quad \text{(A12)}$$

$$\frac{\partial^2 U_T(N,k_g,k_b)}{\partial k_b^2}\frac{\partial k_b^*(N,\theta)}{\partial \theta} + \frac{\partial^2 U_T(N,k_g,k_b)}{\partial k_b \partial k_g}\frac{\partial k_g^*(N,\theta)}{\partial \theta} + \frac{\partial^2 U_T(N,k_b,k_g)}{\partial \theta \partial k_b} = 0 \quad \text{(A13)}$$

Calculating the remaining partials

$$\rightarrow \frac{\partial^2 U_T}{\partial\theta\partial k_g} = \Big[U_1^{''}(1-\theta)N^2(k_b-k_g) - U_2^{''}(1-\theta)N^2p'(k_g)G(p(k_b)B+p(k_g)G)+ \\ +U_S^{''}(1-\theta)N^2p'(k_g)(p(k_b)-p(k_g))\Big] + \\ \Big[U_1^{'}N - U_S^{'}p'(k_g)N + U_2^{'}Gp'(k_g)N\Big] \tag{A14}$$

which is positive if G is large enough

$$\rightarrow \frac{\partial^2 U_T}{\partial\theta\partial k_b} = \Big[U_1^{''}\theta N^2(k_b-k_g) + U_2^{''}\theta N^2p'(k_b)G(p(k_b)B+p(k_g)G)+ \\ +U_S^{''}\theta N^2p'(k_b)(p(k_b)-p(k_g))\Big] + \\ \Big[U_1^{'}N - U_S^{'}p'(k_b)N - U_2^{'}Bp'(k_b)N\Big] < 0 \tag{A15}$$

From equations A12 and A13 and applying Cramer's rule:

$$\frac{\partial k_g^*}{\partial\theta} = -\frac{Det\begin{vmatrix}\frac{\partial^2U_T}{\partial\theta\partial k_g} & \frac{\partial^2U_T}{\partial k_g\partial k_b}\\ \frac{\partial^2U_T}{\partial\theta\partial k_b} & \frac{\partial^2U_T}{\partial k_b^2}\end{vmatrix}}{Det\begin{vmatrix}\frac{\partial^2U_T}{\partial k_g^2} & \frac{\partial^2U_T}{\partial k_b\partial k_g}\\ \frac{\partial^2U_T}{\partial k_g\partial k_b} & \frac{\partial^2U_T}{\partial k_b^2}\end{vmatrix}} = -\frac{Det\begin{vmatrix}+ & +\\ - & -\end{vmatrix}}{Det\begin{vmatrix}- & +\\ + & -\end{vmatrix}}$$

The determinant in the denominator will be positive as before. The numerator will be negative if $\frac{\partial^2U_T}{\partial\theta\partial k_g}\frac{\partial^2U_T}{\partial k_b^2} > \frac{\partial^2U_T}{\partial k_g\partial k_b}\frac{\partial^2U_T}{\partial\theta\partial k_b}$

Multiplying the numerator and simplifying I get the condition that the determinant is negative if G is large enough. Thus,

$$\frac{\partial k_g^*(N,\theta)}{\partial\theta} > 0 \tag{A16}$$

This result says that as the proportion of boys increases, the expenditure per girl also increases.

A.4.3 Proposition 2: Part (a)

I will do the optimization over N and k_g, k_b in two stages: first, I will maximize the objective function with respect to k_g, k_b for given N, and then I will choose the optimal N. For a given level of fertility, the first order conditions with respect to k_g and k_b are given in equations A2 and A3.

The first order condition for N in the maximization of the second stage objective function (that obtains after k_g and k_b have been "maximized out") is:

$$\frac{\partial U_T(N,k_b,k_g)}{\partial N} = -U_1'(1-\theta)k_g^* - U_1'\theta k_b^* - U_2'(1-\theta)p(k_g^*)G + U_2'\theta p(k_b^*)B + U_S'(1-\theta)p(k_g^*) + U_S'\theta p(k_b^*) = 0 \tag{A17}$$

Let $N^*(G)$ be the solution to equation A17. Totally differentiating equation A17 with respect to G, I obtain:

$$\frac{dN^*}{dG}\Big[U_1''(1-\theta)^2k_g^2 + U_1''\theta^2k_b^2 + U_2''(1-\theta)^2p(k_g)^2G^2 + U_2''\theta^2p(k_b)^2B^2 + U_S''(1-\theta)^2p(k_g)^2 + U_S''\theta^2p(k_b)^2\Big] + \Big[-U_2'(1-\theta)p(k_g)\Big] = 0 \tag{A18}$$

The term in the square brackets on the left is clearly negative, while the term on the square brackets on the right is also negative. Then in order for equation A18 to sum to zero it must be true that,

$$\frac{dN^*(G)}{dG} < 0 \tag{A19}$$

The introduction of the *Bhagyalakshmi* program will reduce the future net cost of each surviving daughter G and thus increase the willingness to conceive in this simple framework.

A.4.4 Proposition 2: Part (b)

Using the chain rule it follows that:

$$\frac{dk_g^*(N^*(G),\theta,G)}{dG} = \underbrace{\frac{\partial k_g^*(N^*(G),\theta,G)}{\partial N}\frac{dN^*(G)}{dG}}_{\text{Indirect Effect}} + \underbrace{\frac{\partial k_g^*(N^*(G),\theta,G)}{\partial G}}_{\text{Direct Effect}} \tag{A20}$$

<u>Direct Effect</u>: Calculating the remaining partials

$$\rightarrow \frac{\partial^2 U_T}{\partial G \partial k_g} = \Big[U_2''(1-\theta)^2N^2p'(k_g)^2G^2 - U_2'(1-\theta)Np'(k_g)\Big] < 0 \tag{A21}$$

$$\rightarrow \frac{\partial^2 U_T}{\partial G \partial k_b} = 0 \tag{A22}$$

By the implicit function theorem:

$$\frac{\partial k_g^*}{\partial G} = -\frac{Det\begin{vmatrix}\frac{\partial^2 U_T}{\partial G \partial k_g} & \frac{\partial^2 U_T}{\partial k_g \partial k_b} \\ \frac{\partial^2 U_T}{\partial G \partial k_b} & \frac{\partial^2 U_T}{\partial k_b^2}\end{vmatrix}}{Det\begin{vmatrix}\frac{\partial^2 U_T}{\partial k_g^2} & \frac{\partial^2 U_T}{\partial k_b \partial k_g} \\ \frac{\partial^2 U_T}{\partial k_g \partial k_b} & \frac{\partial^2 U_T}{\partial k_b^2}\end{vmatrix}} = -\frac{Det\begin{vmatrix} - & + \\ 0 & - \end{vmatrix}}{Det\begin{vmatrix} - & + \\ + & - \end{vmatrix}}$$

The denominator is positive as before. The numerator is also positive. Thus,

$$\frac{\partial k_g^*}{\partial G} < 0 \tag{A23}$$

Indirect Effect: From equations A19 and A11 we have,

$$\frac{\partial k_g^*(N^*(G), \theta, G)}{\partial N} < 0$$

$$\frac{dN^*(G)}{dG} < 0$$

Thus, $\frac{\partial k_g^*(N^*(G),\theta,G)}{\partial N}\frac{dN^*(G)}{dG} > 0$

Thus, from equation A23 the direct effect is negative i.e. a decrease in future costs leads to an increase in investments in girls, while from equations A19 and A11 the indirect effect is positive i.e. a decrease in future costs of girls leads to a decrease in investments in girls.

Thus, the overall effect of $\frac{dk_g^*(N^*(G),\theta,G)}{dG}$ is ambiguous.

A.4.5 Proposition 3

In order to examine the impact of *Bhagyalakshmi* on son-biased fertility stopping behaviour I will calculate the following:
(i) Change in investments in girls as the proportion of boys goes up after *Bhagyalakshmi*, $\frac{\partial k_g}{\partial \theta}$
(ii) Change in investments in boys as proportion of boys goes up after *Bhagyalakshmi*, $\frac{\partial k_b}{\partial \theta}$ and finally,
(iii) Impact on parents' expected utility from having marginally more children as the proportion of boys goes up, $\frac{(1/2)\frac{\partial U_T}{\partial g}+(1/2)\frac{\partial U_T}{\partial b}}{\partial \theta}$ where $g = (1-\theta)N$ and $b = \theta N$

Part (i): After the introduction of the *Bhagyalakshmi* program, for those households for whom the program results in a *large* decrease in G; equations A8 & A14 which depend on G being large, will flip signs. That is, a large decrease in G due to *Bhagyalakshmi* will result in: $\frac{\partial^2 U_T}{\partial k_g \partial k_b} < 0$ and $\frac{\partial^2 U_T}{\partial k_g \partial \theta} < 0$.

Thus,

$$\frac{\partial k_g^*}{\partial \theta} = -\frac{Det\begin{vmatrix} \frac{\partial^2 U_T}{\partial\theta\partial k_g} & \frac{\partial^2 U_T}{\partial k_g \partial k_b} \\ \frac{\partial^2 U_T}{\partial\theta\partial k_b} & \frac{\partial^2 U_T}{\partial k_b^2} \end{vmatrix}}{Det\begin{vmatrix} \frac{\partial^2 U_T}{\partial k_g^2} & \frac{\partial^2 U_T}{\partial k_b \partial k_g} \\ \frac{\partial^2 U_T}{\partial k_g \partial k_b} & \frac{\partial^2 U_T}{\partial k_b^2} \end{vmatrix}} = -\frac{Det\begin{vmatrix} - & - \\ - & - \end{vmatrix}}{Det\begin{vmatrix} - & - \\ - & - \end{vmatrix}}$$

The denominator is positive if $\frac{\partial^2 U_T}{\partial k_b^2}\frac{\partial^2 U_T}{\partial k_g^2} > \frac{\partial^2 U_T}{\partial k_g \partial k_b}\frac{\partial^2 U_T}{\partial k_g \partial k_b}$ which is true from Proposition 1(a). The numerator is positive if $\frac{\partial^2 U_T}{\partial\theta\partial k_g}\frac{\partial^2 U_T}{\partial k_b^2} > \frac{\partial^2 U_T}{\partial k_g \partial k_b}\frac{\partial^2 U_T}{\partial\theta\partial k_b}$ which is also true from Proposition 1(a). Thus,

$$\frac{\partial k_g^*(N,\theta)}{\partial \theta} < 0 \tag{A24}$$

Part (ii):

$$\frac{\partial k_b^*}{\partial \theta} = -\frac{Det\begin{vmatrix} \frac{\partial^2 U_T}{\partial k_g^2} & \frac{\partial^2 U_T}{\partial\theta\partial k_g} \\ \frac{\partial^2 U_T}{\partial k_g \partial k_b} & \frac{\partial^2 U_T}{\partial\theta\partial k_b} \end{vmatrix}}{Det\begin{vmatrix} \frac{\partial^2 U_T}{\partial k_g^2} & \frac{\partial^2 U_T}{\partial k_b \partial k_g} \\ \frac{\partial^2 U_T}{\partial k_g \partial k_b} & \frac{\partial^2 U_T}{\partial k_b^2} \end{vmatrix}} = -\frac{Det\begin{vmatrix} - & - \\ - & - \end{vmatrix}}{Det\begin{vmatrix} - & - \\ - & - \end{vmatrix}}$$

The denominator and numerator are both positive from part (i) above and thus,

$$\frac{\partial k_b^*(N,\theta)}{\partial \theta} < 0 \tag{A25}$$

Thus, if *Bhagyalakshmi* leads to a large decrease in the future costs of girls i.e. the *reduction* in G is large enough then, $\frac{\partial k_b}{\partial \theta} < 0$ and $\frac{\partial k_g}{\partial \theta} < 0$. That is, after the introduction of the program, investments in girls and boys are no longer determined by the proportion of children of a particular gender.

Part (iii):

Finally, I examine how $(1/2)\frac{\partial U_T}{\partial g} + (1/2)\frac{\partial U_T}{\partial b}$ changes as the proportion of boys (or the proportion of girls) goes up, where $g = (1-\theta)N$ and $b = \theta N$:

$$(1/2)\frac{\partial U_T}{\partial g} + (1/2)\frac{\partial U_T}{\partial b} = -U_1^{'}k_g - U_2^{'}Gp(k_g) - U_1^{'}k_b + U_2^{'}Bp(k_b) + U_S^{'}(p(k_g) + p(k_b))$$

After *Bhagyalakshmi* if the proportion of boys goes up, investments in boys decline and investments in girls also decline (from parts i and ii above). Similarly, if the proportion of girls goes up investments in children of both genders falls. In both scenarios, period 1 and survival marginal utility fall and period 2 marginal utility of consumption must also fall. Thus, if the proportion of either boys goes up or the proportion of girls goes up, parents will gain less utility from an extra child independent of the existing proportion of children of a particular gender, if the decrease in the future costs of girls is large enough.

A.4.6 Proposition 4

I extend the above framework to incorporate selective abortion behaviour. At the beginning of period 1, parents have 3 possible choices: no pregnancy, a pregnancy without a prenatal scan determination, or a pregnancy with a prenatal scan determination followed by an abortion if the fetus is a female. When a child is born parents incur a fixed cost, and gender specific investment costs in the first period. If parents select prenatal sex determination they also pay the cost of the scan, and the cost of a son if the fetus is a boy and therefore carried to term.[7] Let the probability of conceiving a child of either gender be $\frac{1}{2}$. Additionally, let b and g be less than the total number of children parents have in their lifetime. Then I can examine the impact of a marginal birth on expected utility. For simplicity, I will assume that the 'no pregnancy' option is not available and the cost of sex-selective abortions to be negligible.

Expected change in utility from a pregnancy without a prenatal scan determination, ΔU_T^{NS}:

$$\Delta U_T^{NS} = \frac{1}{2}\frac{\partial U_T}{\partial g} + \frac{1}{2}\frac{\partial U_T}{\partial b} \tag{A26}$$

If instead they decide to have a pregnancy without a prenatal scan determination their change in expected utility will be, ΔU_T^{S}:

$$\Delta U_T^{S} = \frac{1}{2}0 + \frac{1}{2}\frac{\partial U_T}{\partial b} \tag{A27}$$

where the second term on the right is the utility from having a scan, finding out it's a boy and thus carrying it to term and the second term is the utility from having a scan, finding out it's a girl and aborting it.

Before *Bhagyalakshmi*

Before the introduction of *Bhagyalakshmi*, parents will choose a pregnancy with sex determination if $\Delta U_T^{NS} < \Delta U_T^{S}$. Intuitively, parents choose a pregnancy with a prenatal scan over a pregnancy without a prenatal scan if the marginal period 2 consumption utility from having a girl is *smaller* than the expected impact on consumption utility in period 1.

After *Bhagyalakshmi*

From equations A26 and A27:

$$\Delta U_T^{NS} - \Delta U_T^{S} = \frac{1}{2}\frac{\partial U_T}{\partial g} \tag{A28}$$

[7] The prenatal sex determination scan is best thought of as a bundled service that includes both the prenatal sex determination scan and the abortion if the fetus is female. This is because, it is more tractable than including additional steps in the decision process, especially since the only motivation for a prenatal scan in the model is sex selection, and a scan would therefore be "wasted" unless a female fetus was aborted.

Taking the derivative of equation A28 with respect to G I get:

$$\frac{\partial[\Delta U_T^{NS} - \Delta U_T^{S}]}{\partial G} = -U_1^{'}\frac{\partial k_g}{\partial G} + U_2^{''} gGp(k_g)^2 - U_2^{'}p(k_g) - U_2^{'}\frac{\partial p(k_g)}{\partial G}G + U_S^{'}\frac{\partial p(k_g)}{\partial G} \tag{A29}$$

<u>*Case I:*</u>
If a decrease in G leads to a decrease in health investments, k_g then, $\frac{\partial k_g}{\partial G} > 0$ and $\frac{\partial p(k_g)}{\partial G} > 0$. Then, from equation A29,

$$\frac{\partial[\Delta U_T^{NS} - \Delta U_T^{S}]}{\partial G} = -U_1^{'} + U_2^{''} gGp(k_g)^2 - U_2^{'}p(k_g) - U_2^{'}G + U_S^{'} < 0 \tag{A30}$$

<u>*Case II:*</u>
If a decrease in G leads to an increase in health investments, k_g then, $\frac{\partial k_g}{\partial G} < 0$ and $\frac{\partial p(k_g)}{\partial G} < 0$. Then, from equation A29,

$$\frac{\partial[\Delta U_T^{NS} - \Delta U_T^{S}]}{\partial G} = U_1^{'} + U_2^{''} gGp(k_g)^2 - U_2^{'}p(k_g) + U_2^{'}G - U_S^{'} \tag{A31}$$

which is negative if $U_2^{''} gGp(k_g)^2 - U_2^{'}p(k_g) - U_S^{'} > U_2^{'}G + U_1^{'}$. This will hold *only*, when the reduction in G because of *Bhagyalakshmi* is large enough.

Appendix B: Appendix for Chapter 2

Table B.1: State-wise number of beneficiaries enrolled to avail the benefit under RSBY (2012 - 2015)

Major States	Enrolled Families 2012-13	Enrolled Families 2013-14	Enrolled Families 2014-15
Assam	176906	1416919	1416919
Bihar	7634503	6102774	Enrolment in progress
Chhattisgarh	2285345	2265370	2142035
Gujarat	1883179	1900903	1923732
Haryana	560241	465797	419919
Himachal Pradesh	286492	341818	448000
Jammu & Kashmir	35521	4988	Not participated in RSBY
Jharkhand	1462235	1923138	1619697
Kerala	2743665	3662511	3194638
Madhya Pradesh	116510	608748	203208
Maharashtra	1747157	234252	Not participated in RSBY
Manipur	66753	68140	30674
Meghalaya	78395	108321	156086
Mizoram	103545	145842	109744
Nagaland	143585	151806	80229
Odisha	3388096	4238040	4306702
Puducherry	9486	9486	Enrolment not done
Punjab	226878	236764	234169
Rajasthan	732889	2511663	2692626
Tripura	505327	505327	505327
Uttar Pradesh	5396503	5541225	3307372
Uttarakhand	334694	285435	285435
West Bengal	5766731	5748689	5749646

Note: Data is from Government of India, Ministry of Health and Family Welfare

Table B.2: Total number of districts covered under the RSBY up to March 2010 and June 2012

States in the Study	All Districts	Upto March 2010 Participating Districts	Between April 2010 & March 2012 Participating Districts	After June 2012 Participating Districts
Arunachal Pradesh	16	0	10	6
Assam	27	0	5	22
Bihar	38	0	38	0
Chandigarh	1	1	0	0
Chhattisgarh	18	10	6	2
Delhi	8	8	0	0
Goa	2	2	0	0
Gujarat	25	0	25	0
Haryana	20	19	1	0
Himachal Pradesh	12	4	8	0
Jammu & Kashmir	14	0	1	13
Jharkhand	22	5	17	0
Kerala	14	14	0	0
Madhya Pradesh	50	0	0	50
Maharashtra	34	28	0	6
Manipur	9	0	4	5
Meghalaya	7	1	4	2
Mizoram	8	0	8	0
Nagaland	11	3	8	0
Odisha	30	6	24	0
Puducherry	4	0	0	4
Punjab	20	20	0	0
Rajasthan	32	0	7	25
Tripura	4	0	4	0
Uttar Pradesh	71	68	3	0
Uttarakhand	15	2	12	1
West Bengal	19	0	15	4
Total Districts	531	191	200	140

Note: Data is from Government of India, www.rsby.in

Table B.3: Summary Statistics for Outcomes for 2004-05

	Control Districts	Early Districts	*p-value*	Control Districts	Late Districts	*p-value*
Number of Days in a Week						
Private Wage Work	0.86	0.86	0.994	0.86	0.90	0.875
Private Casual Work	0.48	0.46	0.893	0.48	0.57	0.649
Private Salaried Work	0.38	0.40	0.900	0.38	0.33	0.783
Public Casual Work	0.01	0.00	0.774	0.01	0.01	0.904
Private Wage Work - Agriculture	0.34	0.29	0.745	0.34	0.40	0.716
Domestic Work	1.33	1.33	0.991	1.33	1.51	0.558
Self-Employed	1.55	1.40	0.633	1.55	1.35	0.523

Note: This table presents baseline means of the main outcome variables used in the paper for different samples. Columns (1) & (4) is restricted to control districts that received the program after June 2012 or have not yet received it. Column (2) is restricted to 'early' RSBY districts (received program before March 2010). Column (5) includes only districts that received the program between April 2010 and June 2012.

Table B.4: Sample II Difference-in-difference - Individual Level Results

	Private Casual Work		Private Salaried Work		Private Agricultural Wage Work		Domestic Work		Self-Employed	
Panel A: Females	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early Treatment*Time3	0.167**	0.166**	0.054	0.054	0.147**	0.142*	-0.452**	-0.423**	-0.102	-0.082
	(0.0773)	(0.0773)	(0.0345)	(0.0354)	(0.0732)	(0.0739)	(0.2048)	(0.2071)	(0.1553)	(0.1578)
Early Treatment*Time2	-0.055	-0.09	0.034	0.011	-0.068	-0.098	0.233	0.249	0.036	0.059
	(0.0736)	(0.0751)	(0.0343)	(0.0350)	(0.0735)	(0.0743)	(0.1758)	(0.1775)	(0.1232)	(0.1218)
Late Treatment*Time3	-0.004	0.005	0.036	0.049	0.067	0.074	-0.274	-0.25	0.08	0.08
	(0.0797)	(0.0806)	(0.0359)	(0.0366)	(0.0760)	(0.0768)	(0.2525)	(0.2563)	(0.1563)	(0.1607)
Late Treatment*Time2	-0.211**	-0.255**	0.038	0.039	-0.202**	-0.240**	0.147	0.138	0.171	0.169
	(0.0735)	(0.0744)	(0.0346)	(0.0352)	(0.0716)	(0.0723)	(0.1932)	(0.1918)	(0.1243)	(0.1245)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	122992	119246	122992	119246	122992	119246	122992	119246	122992	119246
Panel B: Males	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early Treatment*Time3	0.148	0.178	-0.02	-0.04	0.181	0.203	0.022	0.023	-0.335**	-0.341**
	(0.1576)	(0.1524)	(0.0596)	(0.0596)	(0.1569)	(0.1503)	(0.0248)	(0.0256)	(0.1617)	(0.1549)
Early Treatment*Time2	0.01	-0.041	-0.029	-0.057	-0.084	-0.128	-0.035	-0.032	0.083	0.155
	(0.1397)	(0.1379)	(0.0621)	(0.0612)	(0.1320)	(0.1326)	(0.0242)	(0.0245)	(0.1269)	(0.1195)
Late Treatment*Time3	0.062	0.12	0.115**	0.110*	0.184	0.226	-0.041	-0.041	-0.186	-0.226
	(0.1717)	(0.1704)	(0.0562)	(0.0576)	(0.1785)	(0.1716)	(0.0311)	(0.0312)	(0.1772)	(0.1739)
Late Treatment*Time2	-0.072	-0.163	0.103*	0.126**	-0.063	-0.132	-0.038	-0.039	0.076	0.103
	(0.1443)	(0.1423)	(0.0581)	(0.0582)	(0.1436)	(0.1420)	(0.0352)	(0.0355)	(0.1344)	(0.1300)
Individual Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	121143	116853	121143	116853	121143	116853	121143	116853	121143	116853

Note: Estimates from Equation (2.1) for Sample II. Regressions include state and month of interview fixed effects. District controls include fraction ST and SC, fraction illiterate, female and male employment rates, and fraction under poverty line separately for the whole district and for the rural areas of the district - calculated from NSS 2004-05. Also includes: the proportion of villages in a district connected by a road, proportion of villages in a district with a primary health center, proportion of villages in a district with a government hospital, proportion of villages in a district with a health sub-center and the average distance of a village in a district to the nearest town calculated from DLHS 2002-04. Individual controls include dummies for age 30 to 40, 40 to 50, and greater than 50, dummies for years of education under 4, between 5 and 8, between 8 and 12, and 12, as well as marital status, and household size. Standard errors in parentheses, clustered at the district level. Number of districts=531. * denotes significance at the 10% level, ** denotes significance at the 5% level, and *** denotes significance at the 1% level.

Table B.5: Summary Statistics for Outcomes (ATT Analysis)

	Non-RSBY Households	RSBY Households	*p-value*
Panel A: Intensive Margin - Number of Days in a Week			
Private Casual Work	1.18	0.98	0.38
Private Salaried Work	0.40	0.48	0.57
Family Farm Work	1.96	2.32	0.10
Self-Employed	0.44	0.47	0.86
Panel B: Extensive Margin - Labour Force Participation			
Private Casual Work	0.21	0.18	0.37
Private Salaried Work	0.07	0.09	0.55
Family Farm Work	0.43	0.48	0.34
Self-Employed	0.09	0.09	0.89

Note: This table presents baseline means of the main outcome variables used in the paper for the IHDS sample. Column (1) is restricted to households who did not have RSBY card in RSBY exposed districts. Column (2) is restricted to households who had an RSBY card in RSBY exposed districts.

Table B.6: Baseline Household Variables Before Matching (ATT analysis)

	Non-RSBY Households	RSBY Households	*p-value*
Hindu (0/1)	0.88	0.88	0.890
Scheduled caste/Scheduled Tribe (0/1)	0.33	0.40	0.000
OBC caste (0/1)	0.34	0.33	0.589
HH size	6.24	6.01	0.000
Highest Years of Schooling by Adult	6.57	6.15	0.000
No. of loans in the last 5 years	1.40	1.13	0.000
Log per capita consumption	6.34	6.20	0.000
Log household income	10.24	10.09	0.000
HH has ration card (0/1)	0.87	0.87	0.919
HH is Below Poverty Line (0/1)	0.25	0.32	0.000
Age of HH Head (in Years)	48.57	47.87	0.011
HH Head is Female (0/1)	0.08	0.09	0.192
HH Head work type - casual (0/1)	0.44	0.56	0.000
HH Head work type - govt (0/1)	0.56	0.44	0.000
HH has piped water (0/1)	0.24	0.21	0.000
HH has hand pump (0/1)	0.42	0.41	0.363
HH does not have flush toilet (0/1)	0.89	0.88	0.273
HH has no electricity (0/1)	0.35	0.41	0.000
HH has health insurance (0/1)	0.02	0.01	0.315
HH social network: doctor (0/1)	0.29	0.31	0.022
HH social network: teacher (0/1)	0.37	0.37	0.951
HH social network: govt official (0/1)	0.28	0.26	0.009
Any member part of Self Help Group (0/1)	0.06	0.10	0.000
Any member part of any religious/social group (0/1)	0.16	0.16	0.775
Any member attended local body meeting (0/1)	0.36	0.43	0.000
Any death in the last year (0/1)	0.05	0.05	0.325
HH has confidence in medical staff (0/1)	0.99	1.00	0.203
HH has confidence in state government (0/1)	0.98	0.99	0.531
Per capita inpatient expenditure (in Rupees)	362.29	376.96	0.733
Per capita outpatient expenditure (in Rupees)	45.67	49.57	0.170
% HH member with fever in last 30 days	0.12	0.14	0.000
% HH member with cough in last 30 days	0.10	0.11	0.000
% HH member with diarrhea in last 30 days	0.03	0.04	0.000
% HH member with ST Illness	0.14	0.16	0.000
% HH member treated for ST Illness	0.13	0.15	0.000
% HH member treated by Govt doctor for ST Illness	0.03	0.03	0.001
% HH member treated by Private Doctor for ST Illness	0.11	0.12	0.004
% HH member with LT Illness	0.05	0.06	0.000
Per capita Hospital Days	0.23	0.31	0.008
Per capita days unable to work due to Illness in past month	3.79	4.17	0.161

Note: Raw baseline sample means for ATT analysis sample. Sample restrictions include (1) Excluded states - Tamilnadu, Andhra Pradesh and Karnataka (2) only rural areas included (3) districts with no RSBY by 2012 have been dropped. OBC refers to other backward caste. ST refers to short-term. LT refers to long-term.

Table B.7: Probit Estimates: Dependent Variable - HH has RSBY in 2011 (ATT analysis)

	Coefficient	Standard Error
Independent Variables		
Hindu (0/1)	-0.09	(0.1006)
Scheduled caste/Scheduled Tribe (0/1)	0.285**	(0.0777)
OBC caste (0/1)	0.11	(0.0794)
HH size	-0.01	(0.0209)
Highest Years of Schooling by Adult	0.00	(0.0064)
No. of loans in the last 5 years	-0.02	(0.0132)
Log per capita consumption	-0.308**	(0.0692)
Log household income	-0.108**	(0.03070)
HH has ration card (0/1)	0.386**	(0.0769)
HH is Below Poverty Line (0/1)	-0.05	(0.0734)
Age of HH Head (in Years)	0.00	(0.0021)
HH Head is Female (0/1)	-0.03	(0.0857)
HH Head work type - casual (0/1)	0.01	(0.1362)
HH Head work type - govt (0/1)	-0.304**	(0.1362)
HH has piped water (0/1)	0.08	(0.0840)
HH has hand pump (0/1)	0.122*	(0.0708)
HH does not have flush toilet (0/1)	0.14	(0.0940)
HH has no electricity (0/1)	0.183**	(0.0626)
HH has health insurance (0/1)	-0.08	(0.1879)
HH social network: doctor (0/1)	0.08	(0.0642)
HH social network: teacher (0/1)	-0.120*	(0.0632)
HH social network: govt official (0/1)	-0.02	(0.0661)
Any member part of Self Help Group (0/1)	0.324**	(0.0913)
Any member part of any religious/social group (0/1)	-0.02	(0.0841)
Any member attended local body meeting (0/1)	0.107**	(0.0538)
Any death in the last year (0/1)	0.02	(0.1071)
HH has confidence in medical staff (0/1)	0.752*	(0.3879)
HH has confidence in state government (0/1)	-0.01	(0.2048)
Per capita inpatient expenditure (in Rupees)	0.00	(0.0000)
Per capita outpatient expenditure (in Rupees)	0.00	(0.0002)
% HH member with fever in last 30 days	0.48	(0.3735)
% HH member with cough in last 30 days	-0.15	(0.2377)
% HH member with diarrhea in last 30 days	0.10	(0.2794)
% HH member with ST Illness	-0.27	(0.5273)
% HH member with LT Illness	0.02	(0.2235)
Per capita Hospital Days	0.00	(0.0141)
Per capita days unable to work due to Illness in past month	0.00	(0.0020)

Note: Model also includes district fixed effects. Sample restrictions: (1) Excluded states - Tamilnadu, Andhra Pradesh and Karnataka (2) only rural areas included (3) districts with no RSBY by 2012 have been dropped. Robust standard errors in parantheses. ***p ¡ 0.01, **p ¡ 0.05, *p ¡ 0.1.

Table B.8: Matching Difference-in-Differences Estimates (ATT analysis)

	ATT	Standard Error	ATT as % of RSBY HH's 2005 average	2005 Average for RSBY HH's
Panel A: Women	(1)	(2)	(3)	(4)
Private Casual Work (number of days in a week)	0.335***	(0.057)	81.71	0.41
Private Salaried Work (number of days in a week)	0.054	(0.042)	39.13	0.138
Family Farm Work (number of days in a week)	0.063	(0.054)	5.82	1.083
Business Work (number of days in a week)	0.051**	(0.025)	52.58	0.097
Panel B: Men	(1)	(2)	(3)	(4)
Private Casual Work (number of days in a week)	0.373***	(0.117)	20.79	1.794
Private Salaried Work (number of days in a week)	-0.013	(0.073)	-1.49	0.872
Family Farm Work (number of days in a week)	0.007	(0.067)	0.49	1.436
Business Work (number of days in a week)	0.075	(0.066)	16.13	0.465

Note: ATT is the Average Treatment Effect on the Treated. Standard errors are in parenthesis. ***p ¡ 0.01, **p ¡ 0.05, *p ¡ 0.1.

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