Financial Incentives, Son Preference and Household Behaviour: Evidence from India

Uttara Balakrishnan*

September 19, 2016

PRELIMINARY DRAFT: Please do not cite

Abstract

India is characterized by large gender gaps. One of the primary reasons for such gaps is the existence of son preference in India. This paper examines the effectiveness of financial incentives in increasing the relative value of daughters by analyzing the *Bhagyalakshmi* Program in Karnataka. *Bhagyalaksmi* provides households with monetary benefits on the birth of daughters, for up to two daughters in the family. As a result of this sudden implementation, households in one state in the country were 'rewarded' on the birth of a daughter. This paper uses temporal, compositional and geographic variation to identify the effect of the program. The focus is on two main outcomes: fertility choices and child mortality. I will use a triple difference approach to identify the impact of the program on the outcomes of interest. I find that *Bhagyalakshmi* significantly increases the marginal fertility for eligible woman. I find that women who had one or two children at the start of the program were more likely to have a marginal birth after the program. Further, I find that eligible girls under the program saw a decline in infant mortality rates and an increase in the probability of being immunized. That is, I find that the program has managed to significantly reduce gender gaps in post-neonatal child mortality and postnatal health investments, such as breastfeeding and vaccination.

JEL codes: : I15, J13, J16
Keywords: Financial incentives; gender; fertility; infant mortality; parental investments; India; health

*University of Maryland, Email:uttara@umd.edu

1 Introduction

India is characterized by gender gaps in many human development indicators. Young girls, in particular, remain the most disadvantaged (The World Bank, 2012) and face significant disparities on a number of welfare measures such as mortality (Bhargava, 2003; Arnold et al., 2002), nutrition (Jayachandran and Kuziemko, 2009; Borooah, 2004) and child care (Barcellos et al., 2014). One prominent explanation amongst economists for such disparities is the fact that parents invest more heavily in boys than in girls (see Jensen, 2003; Qian, 2009; for recent examples). Moreover, girls are also discriminated against in the womb. The development and spread of ultrasound technology has been associated with a rapid increase int he sex ratio at birth. In the Indian census of 2001, the sex ratio in the age group 0-6 was 107.8, with some northern states having ratios of 120-125. The potential negative consequences of male-biased sex ratios for old-age support, crime, violence, labor markets, prostitution, and sexually-transmitted diseases have prompted policymakers to introduce various policy measures which include banning sex-selective abortions and special financial incentives for giving birth to a girl child[1,2].

This paper examines the impact of financial incentives on child quantity and quality. In the absence of financial incentives parents can adjust the gender composition of their children in two ways. First, by continuing childbearing till they achieve, for instance, the desired number of sons. Several studies document son-biased fertility stopping behavior, which results in girls having more siblings that boys (Clark, 2000; Bhalotra and Van Soest, 2008; Jensen, 2012; Rosenblum, 2013). The quantity-quality trade-off, driven by the budget constraint, implies that, even if parents do not actively discriminate against daughters, a gender gap in outcomes will emerge simply because girls, on average, grow up in families with fewer per capita resources. The second option is to subject girls to deliberate neglect, culminating into girl mortality in early childhood (Gupta, 1987; Pitt et al., 1990; Sen, 1990; Rose, 2000; Oster, 2005; Bhalotra, 2010; Jayachandran and Kuziemko, 2009). In this paper I test the hypothesis that financial incentives at the birth of a girl child by increasing the relative value of girls, weakened both son-biased fertility stopping behavior and postnatal discrimination against girls, measured by child investments and mortality rates.

Increasing son preference has led to declining sex ratios and the prevalence of son stopping fertility behaviour[3,4].Fertility stopping rules lead to excessive female mortality for girls in families where the first born is a girl compared to a family where the first born is a boy as well as lower

[1]There is an enormous empirical literature on the subject of skewed sex ratios. Following Sen (1990) and many demographers (e.g., Ansley J. Coale 1991), economists are increasingly contributing to this debate (see Oster, 2005; Qian, 2009; Anderson and Ray, 2010).

[2]While sex selective abortion is illegal in India, the practice still flourishes. It is hard to see how such a law can be enforced given that neither ultrasound nor abortions are illegal, so that sex selective abortion is *unverifiable*.

[3]Son-biased stopping rules are reflected in the higher likelihood of the lastborn child being a boy and higher-order children being female.

[4]See e.g.Chung and Gupta (2007); Chung (2007); Gupta (1987); Zeng et al (1993); Park and Cho (1995).

investments for girls in such families. This missing girls phenomenon has led to the introduction of various financial incentives at the state level in India, each differing in its eligibility conditions and incentives offered[5]. While some provide benefits to parents who have only daughters and no sons (e.g. Balri Rakshak Yojana in the state of Punjab), others impose no limits on the number of sons, as long as parents have the requisite number of daughters (e.g. Ladli in the state of Haryana). By changing the relative value of daughters, these initiatives can have important consequences on the fertility behaviour of couples. If the incentives are large enough to make daughters more desirable, then one would expect a reduction in son stopping fertility behaviour and consequently a reduction in gender gaps in health outcomes and investments. Financial incentives for daughters could also reduce post-natal discrimination and reduce gender gaps in later childhood outcomes (Goodkind, 1996). On the other hand, if son preference is a predominant feature then a policy that imposes restrictions on the number of sons will not be able to incentivize households to change their fertility behaviour and thus gender gaps will persist[6].

This paper explores whether changes in the economic costs of daughters vis-a-vis sons affect their parents' fertility and investment decisions. I focus on a natural experiment: in 2006, the state of Karnataka introduced the Bhagyalakshmi scheme. As a result of this sudden implementation, households in one state in the country were 'rewarded' on the birth of a daughter. This paper uses temporal, compositional and geographic variation to identify the effect of the *Bhagyalakshmi* program. I construct a large woman-year panel dataset by combining complete retrospective birth histories from the National Family Health Surveys and the District-Level Household Survey of India. I compare women in Karnataka to their counterparts in a group of similar neighbouring states and employ the variation in incentives by the year of program implementation and the composition of surviving children at the time of the program to estimate the causal effect of the program in a double and triple differences-in-differences framework. This approach allows for time varying differences between Karnataka and other states as long as they affect eligible and non-eligible women and children in the same way. Further, I pool the sample on birth histories to examine gender gaps in mortality and health investments. Finally, since the data allows me to link biological siblings I use mother fixed effects to account for selection.

I estimate reduced form equations for three sets of outcomes. First, I estimate the relative survival of girls who were eligible for the program. Since the benefits of the program are available only once the girl turns 18, my expectation is that this decreased the mortality for eligible girls relative to non-eligible boys and girls[7]. Second, I estimate changes in parental investments in eligible girls

[5]The term missing girls was first coined by Sen (1990). He was the first economist to articulate the plight of missing women, estimating that 100 million more women would have been alive if given the same health and nutritional resources as males.

[6]Abrevaya (2009); Dubuc and Coleman (2007) and Almond and Edlund (2008) find evidence of son preference in Asian immigrants in the United States and the UK.

[7]I distinguish between neonatal and infant survival rates. This is because changes in prenatal investments are

relative to non-eligible girls and boys, so as to tie any changes in survival to parental behavioral choices[8]. Finally, I investigate impacts on fertility defined as the probability of a marginal birth. Given the financial incentive one would expect that the probability of a marginal birth would increase for women eligible under the program[9]. That is, one would expect a decline in son-biased fertility stopping in the post-program period. However, to the extent that inherent son preference is high[10], the effect on the probability of a marginal birth is ambiguous.

After estimating the effect of the program on fertility and child investments, I will examine the pathway through which parental behaviour may change. In particular, I examine if the impact of the program is driven through a change in the economic returns to daughters compared to sons. That is, I will investigate the hypothesis that the *Bhagyalakshmi* program impacted fertility by impacting differential stopping behaviour. The pervasiveness of stopping rules in India is well documented (Arnold et al. 2002). Stopping rules can lead to two main consequences: One, girls end up having more siblings and thus living in households with fewer resources per child. Two, an increase in the proportion of girls in a household causes an increase in discrimination against girls and in favor of boys. In other words, sons are better off with a higher proportion of sisters, and daughters are better off with a higher proportion of brothers (Rosenblum 2013). In order to study this mechanism, I evaluate the impact that the introduction of the program has had conditional on the gender composition of children born before the program. I also investigate the impact of the program on health investments, conditional on the gender composition of older siblings.

While a number of studies have examined how financial incentives impact fertility and investment behaviour in developed countries, the literature in high-son preference regions such as India is scarce. The literature is confined to studying China's One-Child Policy (Li et al., 2011; Qian, 2005). In India, Srinivasan and Bedi (2009), Sinha & Yoong (2009) and Anukriti (2013) are the only papers that examine three incentive schemes targeted at increasing the number of female births. Bedi & Srinivasan (2009) study Tamil Nadu's 1992 *Girl Child Protection Scheme* (GCPS) that tries to lower female infanticide. Sinha & Yoong (2009) evaluate a program called *Apni Beti, Apna Dhan* (APAD) implemented in Haryana in 1994 and conclude that it led to reductions in the sex ratio of living children. However, Sinha and Yoong (2009) do not account for state by year effects which makes their results less robust. Further, they do not account for the fact that a woman's composition of children changes each time she gives birth. This leads them to incorrectly classify women as being eligible under the scheme. The paper most closely related to mine is Anukriti. She examines the *Devirupak* also in Haryana and finds that *Devirupak* reduced the

more likely to exhibit in neonatal rates, while infant survival is more clearly a function of postnatal investments.

[8]My main outcomes for investments are vaccination and breastfeeding. Note that vaccination and breastfeeding are only two of the many markers of parental investments. However, I do not have data on the same.

[9]Define eligible

[10]Preference for sons can stem from both inherent cultural preferences as well as economic reasons (Rosenblum (2013)).

number of living children by 0.86%, mainly through a decrease in number of girls (1.92%). She also finds that the proportion of one-boy couples increased significantly, by 0.5%. That is, the program had the unintended consequence of increasing the proportion of one boy couples. However, her empirical approach does not take into account selection through a mother fixed effects estimator. Further, she does not examine child mortality and health investments. Neither does she examine the pathways through which the quantity and quality of children are impacted by the program. This paper adds to this limited literature by examining the fertility and health investment decisions as a result of a financial incentive for girls, in the presence of son preference and fertility stopping behaviour.

Several studies have documented widespread discrimination against girls in South Asia (Basu, 1989; Asfaw et al., 2007; Chen et al. 1981). However, less well understood are the economic incentives that cause this discrimination. In India, sons and daughters have opposite future income effects on their parents, and these differences are likely to cause differences in childhood health investment. Aside from any labor income children accrue, sons acquire dowries when they marry, while parents must pay dowries and wedding costs to get their daughters married. Anderson (2003) suggests that 9394 percent of marriages in India include a dowry payment, and that these payments can amount to as much as six times a household's annual income. In this context, understanding how economic incentives can reverse this discrimination, both by influencing fertility decisions and by changing health investments is of utmost importance.

The results of this study are of considerable importance. First, I find that the probability of a marginal birth increases for childless women, women with one child and women with two children. However, the impact for two child families is considerably smaller than for one child families. Further, I find that in the overall sample the proportion of 'two-child' families increases indicating that the impact is the largest for one child families who go on to have another child. Second, I find that infant mortality for girls eligible under the program reduces in the post-program period. Given that I find that mortality rates are higher in the pre-program period, an decrease in mortality in the post period points to a reduction of the gender gap after the program was introduced. Finally, eligible girls were more likely to be breastfed longer as well as more likely to have received at least one of the eight essential immunizations. I examine the main pathway through which these impacts can occur: an increase in the relative value of girls and hence a reduction in son stopping fertility behaviour. I find that women with at least one boy before the program are significantly more likely to have a birth after the program as opposed to women who had zero boys. If son preference were high one would expect to also see an increase in marginal births for women with zero boys. However the fact that I don't find an impact points to the increase in the relative value of girls and a decrease in son preference.

The rest of the paper is organized as follows. Section 2 describes the *Bhagyalakshmi* program. Sections 3 & 4 describe the data and empirical strategy respectively. Section 5 presents the results while Section 6 examines the main mechanisms, while Section 7 implements some robustness checks. Section 8 concludes.

2 Context

2.1 The *Bhagyalakshmi* Program in Karnataka

Karnataka is one of the richest states in India in terms of GDP (Planning Commission, 2015). However, son preference is high and girls face discrimination both at birth and during early childhood. Female mortality disadvantage increased in the state after 1991 (Sudha and Ranjan, 1999). Infant mortality rates for Karnataka are higher on average than the control states in this study(see Figure 3)[11]. Further child marriage rates in Karnataka are one of the highest in the country. According to the NFHS (2005-06) 45% of women, or 2 out of 5 girls are married before the age of 18 years. Son preference is also high. 12% of women and 13% of men want more sons than daughters. Only 3-5% wants more daughters than sons (NFHS 2005-06).

The *Bhagyalakshmi* program was introduced by the Government of Karnataka in March, 2006. The program seeks to promote the birth of girl children and to improve parents' perceived value of daughters by offering them economic incentives. Couples are given an immediate grant at the birth of the girl coupled with a long term savings bond redeemable by the unmarried daughter once she turns 18[12]. The amount of monetary benefit varies with the number and sex composition of a couples' children in any given year. Incentives are restricted to two girls per family and couples can have a maximum of 3 children. Couples who are childless or have more than 3 children receive nothing. Further, couples are required to adopt a terminal method of family planning[13]. To be eligible, couples should register the birth of their child. Enrollment is allowed up to one year of the birth of the child on production of a birth certificate. In addition, interim payments such as scholarship and insurance benefits are made available to the beneficiary on continued fulfillment of the eligibility criteria out lined in the scheme[14]. If the girl child falls sick, medical insurance claims upto a maximum of Rs.25,000/- is also provided. If a natural death or an accident of the insured person takes place, the family receives insurance but does not receive the benefits of the program.

[11] Tamilnadu, Kerala, Goa, Maharashtra, Gujarat, Andhra Pradesh, Jharkhand and Chattisgarh are used as control states in this study.

[12] After enrollment and due verification by the concerned government department, an amount of Rs. 1,00,052 ($1500) is available at the end of 18 years for the first girl beneficiary in the family and Rs. 1,00,037 ($1490) is available for the second girl beneficiary.

[13] Vasectomy or tubectomy (also known as male or female sterilization, respectively).

[14] 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 and, (e) the child should not to marry until the age of 18 years.

Table 1 outlines the differential incentives under the scheme. The transfer varies by year and birth order for births after 2006.

The *Bhagyalakshmi* program differs in important ways to traditional programs. 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. Total benefits received under the scheme over 18 years are enough to cover average dowry expenditures[15]. This program is thus different from most well-known CCT programs (e.g. Mexico's Opportunidades or Bangladesh's Female Secondary School Stipend Program) in both the type of conditionality (daughter's birth and marriage delay), and the long 18-year period over which transfers are made. Further, there are compelling reasons for studying this program and its impact on fertility and child mortality. First, 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. Analyzing the impact of a program that seeks to promote the relative value of girls is an important step in understanding the mechanisms that drive son preference in the southern states. Second, 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[16]. Any program that directly gives payments to parents, such as the *Devirupak* in Haryana, is unlikely to be spent on the intended girl beneficiary[17]. Third, the present value of the benefits under this program are larger than other similar programs (see Table 1). Moreover, parents *also* receive lump sum benefits when their daughter(s) enroll in school and get immunized.

Maternal and infant health has, in general improved in India. The growth in income and the decline in poverty since the early 1980s has been widely documented; fertility decline set in from 1981 (Bhalotra and van Soest (2008)); and neonatal mortality rates have been decreasing. Maternal mortality is estimated to have declined (Bhat (2002)) and maternal age at birth has risen. However, improvements in fetal health tend to favor boys, whereas my hypothesis is that the trend, driven by the availability of financial incentives, has been in favor of girls.

A natural concern in exploiting changes in child transfers is that other government programs may have changed concurrently. However, changes in the child transfer were unanticipated and largely driven by changes in the government in power at the time. In 2006, *Bhagyalakshmi* was

[15] 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).

[16] One would expect that the benefits from this program are used for marriage expenses or as dowry. Dowry in India tends to impoverish families with daughters, lowering the utility from having a daughter, with the utility cost increasing in the cost of dowry. Further, dowry can motivate parents to eliminate female births (Bhalotra et al. (2016a)). A reduction in these costs, may reduce the preference for having sons.

[17] However, to the extent that parents are myopic and discount the future, this program would not be as persuasive as other programs that ensure regular payments in the present.

not accompanied by any other changes in government programs that impacted fertility and child mortality.

2.2 Previous Literature

The literature on the impact of financial incentives on fertility and child health in developed countries is quite extensive. Cohen et al. (2013) use panel data from Israel from 1999 to 2005 to identify the impact of changes in the price of a marginal child on fertility. They find a positive, statistically significant, and economically meaningful price effect on overall fertility and this result is consistent with the theoretical foundations in Becker and Lewis (1974) and Becker and Tomes (1994). Milligan (2005), using Canadian data, finds that the introduction of a child tax subsidy in the 1990s had a significant and positive effect on fertility. Laroque and Salanie' (2008), using French data and variation in the French tax code, conclude that tax incentives affect fertility decisions in France. In addition, Schellekens (2006) examines data from the period 1983 to 1995 in Israel and seeks to estimate the effect of the child subsidy on the hazard rate of childbirth. The length of the period examined makes it difficult for this study to disentangle the effect of child subsidies from that of long-run fertility trends. There is also a literature that looks at cross country effects of child transfers on fertility. These studies have found weak, mixed and insignificant effects[18].

The only paper that examines the simultaneous impact of a large government policy on fertility and child outcomes is Qian (2005). She examines a *relaxation* in China's One-child Policy on sex ratios at birth and family size as well as the impact on school enrollment for first born children. The relaxation allowed parents, in certain specified rural areas, who had one girl to have another child. She finds that contrary to previous literature, enrollment outcomes for existing children is positively impacted after the relaxation reform, even though fertility increases. Given that the quality and quantity of children are jointly determined, it is important to examine the impacts of financial incentive on both these domains.

3 Data and Descriptive Evidence

This paper uses three rounds of the District Level Household Survey (DLHS) conducted in 2002-04 (DLHS-2), 2007-08 (DLHS-3) and 2012-13 (DLHS-4) and one round of the National Family Health Survey (NFHS) conducted in 2005-06[19]. These are nationwide, repeated, cross-sectional surveys

[18] Demeny (1986), reviews the mixed evidence on pro-fertility policies in France, Romania, Germany, and Hungary; Gauthier and Hatzius (1997), provide cross-country evidence from 22 OECD countries; and Dunn (2003).

[19] 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. Finally, the NFHS interviewed 109,041 households between 2005 and 2006.

which are representative at the district level and report complete birth histories for all interviewed women (who are between 15-49 years of age)[20]. For the fertility regressions I combine the retrospective birth histories from these four cross section surveys to create a woman-year panel. A woman enters the panel in the first year of her marriage and exits in the year the survey is conducted[21]. For the mortality regressions I pool the sample of births.

The DLHS-2, 3, 4 surveys include questions similar to the NFHS such as demographic information (age, education, religion, caste, asset ownership, human capital), as well as questions about child and mother health. In particular, the surveys ask about the mother's complete birth history, including children's month, year, and order of birth, mother's age at birth and the age of death of a child if the child is dead. They also include detailed questions about the mother's most recently born children (antenatal care, vaccinations), family planning usage, and parents' health knowledge. Thus the DLHS surveys and the NFHS are comparable.

3.1 Sample Construction

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 survival information for all births after January 1, 2004 and the survey in 2012-13 collects information for births after January 1, 2008. Since it is impossible to construct the birth order of a woman's children without her complete fertility history, I only include those women in my sample (from these two datasets) who had either zero fertility at the time of the survey or whose first child was born after January 1, 2004 (for DLHS-3) or after January 1, 2008 (for DLHS-4). In order to construct the woman year panel, for each year the woman is a part of the panel, I construct variables such as an indicator for birth in that year, the sex of the child in case of birth, number and sex composition of previous births among other variables.

For the fertility sample which uses the woman-year panel I make the following restrictions: I restrict my sample to currently married woman in the 15-44 age group for comparability across surveys[22]. Second, I drop women who have had twins or multiple births since these mostly, do not reflect ones fertility preferences. Third, I exclude women who were younger than 13 years when they had their first child since they are likely to be fundamentally different from the rest of the sample[23]. Finally, I exclude women who were visiting the household when the survey took place and thus state of residence is not recorded for them.

[20] The NFHS is representative at the state level and has a significantly smaller sample size than the DLHS. However, it is reasonable to pool these two datasets since they have a similar survey context in terms of selection of respondents, modes of interviewing, and the exact questions asked.

[21] Year of marriage is defined as the year of consummation.

[22] The NFHS and DLHS-3, 4 were administered to ever married women in the 15-49 age group while the DLHS-2 was administered to currently married women in the 15-44 age group.

[23] About 0.22% of women in the pooled fertility sample are younger than 13.

For the mortality sample which pools births across the four cross sections, I make the following restrictions: I exclude twins or multiple births since it is difficult to assign birth order to such births.

My sample period comprises of the pre-treatment period which includes birth history data from 1977-2006 and post-treatment data which includes data from 2007 to 2013. I choose 1977 as the earliest year in my sample since there are very few observations for prior years and they do not cover all states. Despite the program being applicable for all births after 31st March, 2006 I do not include births in 2006 as part of the post-treatment period[24]. 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-treatment phase. My final sample comprises of 709,530 births for 326,852 mothers for the time period 1977-2013.

Figure 1 provides a descriptive picture of the survey rounds and the years covered by each round.

3.2 Outcomes of interest

My main outcome of interest in the fertility regression is the probability of a marginal birth for a woman. The DLHS and NFHS data give detailed information about a mother's birth history including the year in which a birth takes place. I use this information to define the probability of a marginal birth as an indicator if a woman gives birth in a particular year.

For the mortality regressions, my main outcomes are: neonatal mortality, defined as death within one month of birth and infant mortality, defined as death after the first month of birth but before the first birthday. In addition I also examine health investments. My main outcome variables include: an indicator for full immunization and number of months of breastfeeding[25,26].

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 treatment effects. Since Karnataka is a state in the south of the country I use other southern and central states as control states. 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. Thus, I attempt to restrict my sample of control states to those

[24] However in the mortality sample I include all births after 31st March, 2006 since children born after this date are impacted by the program before their first birthday.

[25] A child is defined to be fully immunized if she/he receives all the eight essential vaccines:BCG, Measles, DPT-1, DPT-2, DPT-3, Polio-1, Polio-2, Polio-3.

[26] Analysis for health investments is carried out for children aged at least 2 years at the age of interview in order to avoid censoring issues.

that are similar to Karnataka in terms of neo-natal mortality and infant mortality mortality since that is the main focus of this study[27]. I restrict control states to the following 7 states: Tamilnadu, Kerala, Andhra Pradesh, Orissa, West Bengal, Maharashtra and Goa. Figures 2 & 3 plot sex ratios at birth and infant mortality rates for India using data from the Census and Economic Survey. Karnataka and the control states seem to follow a similar pattern for both indicators. Recent literature examining programs at state level in India have adopted a similar approach. (See e.g. Anukriti, 2013 and Stopnitzky, 2012). As a robustness check I also estimate my main specifications using the border districts as well as estimating woman level propensity score and performing regression analysis adjusting for the propensity score in the sample of women with common support. As an additional robustness check, similar to Anukriti (2013), I re-estimate all my specifications by dropping one control state at a time[28].

3.4 Summary Statistics

Table 2 shows the summary statistics of control states and Karnataka, in the pre and post treatment periods[29]. Mother's age at birth has increased over time for both Karnataka and control states. In terms of religion, mothers are more likely to be Hindu and less likely to be Muslim. Further, a higher proportion of them reside on the rural sector and are more likely to belong to scheduled castes. In terms of standard of living, which is measured by the household wealth index, women in Karnataka are similar to women in control states. Mother's in control states are relatively more likely to be literate than in Karnataka. To ensure that the effects of the program are not confounded by the differences in socio-economic characteristics for women in Karnataka and control states, I control for all these variables in my regressions in addition to estimating specifications with women fixed effects.

Next, I examine the pre-program means of variables that are potentially impacted by the program. With respect to the woman year sample, women in Karnataka are more likely to be childless in any year in the pre-program period. They are more likely to have two children in the post-period[30]. With respect to the births sample, children are more likely to have received all the eight required vaccines in Karnataka than the control states. Further, children aged 2 and older at the time of the

[27]Inter-state heterogeneity in India is well documented. For instance, Carranza (2012) finds that soil texture explains a large part of the variation in womens 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).

[28]Since, Andhra Pradesh introduced a similar program during the same time frame. I re-estimate my specifications dropping these as control states. Andhra Pradesh introduced the Girl Child Protection Scheme (GCPS) in 2005.

[29]For the remainder of the paper, unless explicitly mentioned, the period from 2006 onwards is taken to be the 'post' period.

[30]As mentioned before, data from DLHS-3 and DLHS-4 include only births from January 1st, 2004 and January 1st, 2008 respectively. To recover complete fertility histories I restrict my sample to women with no living children prior to 2004 and 2008 for the two rounds respectively. Thus, artificially, the women in these two rounds are younger on average, and tend to have smaller number of children.

survey are breastfed for longer, on average in Karnataka as well. Karnataka also has lower neonatal and infant mortality than the other states. These differences across states will be accounted for in my regressions by state fixed effects and state specific time trends.

4 Empirical Strategy

The hypothesis being tested in this paper is that the provision of financial incentives for girls simultaneously modified the decisions to conceive and give an additional birth, and to invest, possibly differently, in eligible girls. Observable outcomes of these decisions are live birth, probability that the birth is female, gender differentiated investments (breastfeeding and vaccination) and mortality of eligible children relative to non-eligible children.

I exploit the quasi-experimental nature of the program's implementation. Conditional on the composition of their children at any point in time, couples within Karnataka are faced with different incentives for future births and health investments in these births. For instance, couples with less than three children in 2006 and any time after that, would be eligible under the program (except couples who had two girls in 2006 since the benefits are limited to two girls per family, born in or after 2006). However, couples with three or more children would not be eligible for the program. For women who were at different points in their fertility path in 2006, *Bhagyalakshmi* can thus be considered an exogenous shock that altered their incentives for subsequent decisions about births and health investments differently.

Finally, a few points are worth noting here. First, in all my specifications I define *Post* as being > 2006, except for the mortality regressions. This is because, for the fertility analysis, births from March-December 2006 were conceived before the program was announced and hence they're excluded. For mortality, I include these births since the outcomes that are analyzed (mortality and health investments) are relevant only once a child is born. Second, one of the eligibility conditions of the program was that households should belong to the below poverty line group. However, my data does not include any measure of individual or household income. Further, according to Sekhri (2012) there is widespread leakage and households from the above poverty line group also receive the benefits. Thus, it is unlikely that this is a binding condition for eligibility. Nevertheless, I examine heterogeneity in effects by household wealth using an asset ownership index.

4.1 Fertility

My empirical analysis is based on triple differences-in-differences estimation where the three sources of variation are: year of program implementation, state of residence and differences in composition of children prior to the start of the program. I first examine the impact of the program on fertility. Based on the eligibility conditions of the program, women in Karnataka who had zero, one or two children at the start of the program constituted the 'treatment' group (except women who already had two daughters). Based on the number and sex composition of her surviving children at the beginning of period *t*, I assign each woman to one of four mutually exclusive groups: No living children (g = 1); One child only (g = 2); Two children only (g = 3); and the Rest (g = 4). For a woman *i*, in state *s*, of age *a* in year *t* who belonged to group g at the beginning of year *t*, I estimate the following:

$$Y_{isatg} = \alpha + \sum_{g=1}^{g=3} \beta_g Kar_s * Post_t * 1[Group_{i,t-1} = g] + X_i\delta + \gamma_{st} + \theta_{sg} + \phi_{ag} + \nu_g * t + \epsilon_{isatg} \quad (1)$$

Y_{isatg} are the outcomes of interest, such as an indicator for birth in year *t*. Kar_s is a dummy for residence in Karnataka, $Post_t$ equals 1 if year > 2006, and 0 otherwise[31]. $Group_{it-1}$ refers to the composition of children at the beginning of period *t-1* and can take four possible values (g = 4 is the omitted group). X_i is a vector that includes woman's years of schooling, indicators for household's religion, caste, residence in the urban sector and standard of living index. I also control for state-year fixed effects (γ_{st}), state-group fixed effects (θ_{sg}), age-group fixed effects (ϕ_{ag}), and group-specific linear time trends[32]. β_g are the coefficients of interest, and measure the effect of the program on the outcome of interest for women in group *g* relative to group 4 before and after 2006 in Karnataka, relative to other states. β_g are identified under the assumption that, in the absence of the scheme, women in Karnataka would have had the same birth outcomes as women with similar fertility history and socio-economic characteristics before the scheme. A triple difference specification frees different groups to have different responses to the program while allowing me to flexibly control for state-year fixed effects. Robust standard errors are clustered at the state-group level.

The *Bhagyalakshmi* program is a dynamic incentive as opposed to a static one-time payment. Thus, there might be selection once the scheme has started. That is, it is possible that some women are more or less likely to have two girls after the scheme has begun. Thus, I re-estimate specification (1) in two alternate ways. One, I define group composition for all years after 2006 to be the same as their group composition in 2006. That is the group a treated woman belongs to in the post period (2007-2013) is defined at the start of the program i.e. in 2006. Two, I restrict my sample to two years after the introduction of the program i.e. 2007-2008. I drop all years after 2008 from the sample.

[31] The survey only provides information about the state of residence at the time of interview. Thus I am assuming that a woman lived in the same state for the entire duration of her marriage. This seems like an innocuous assumption given that women migrate only when they are married and have to relocate and inter-state migration post-marriage is uncommon.

[32] Age-group fixed effects allow for the likelihood of women in certain ages belonging to certain groups. For instance, younger women will be in the group with zero children. State-group fixed effects control for state specific group effects.

Alternately I also estimate a mother fixed effects specification. Even though I include individual covariates, there might still be some unobserved heterogeneity among women which might impact fertility decisions. I address this concern by introducing mother fixed-effects, comparing outcomes for children born to the same mother but differentially exposed to the program:

$$Y_{isat} = \alpha + \beta Karnataka * Post + X_i\delta + \pi_i + \omega_a + \theta_t + \epsilon_{isat} \quad (2)$$

Here π_i are fixed effects for each woman. Here, β estimates the change in the likelihood that a woman reports a particular child composition before and after 2006 in Karnataka, relative to control states. The dependent variables are stock measures of fertility: indicators for having one child, two children or zero children in each year the woman is in the survey. Robust standard errors are clustered at the state level.

4.2 Mortality & Health Investments

Next, I examine the impact of the program on child mortality and health investments. The sources of variation for this analysis are: year of program implementation, state of residence and differences in sibling composition at the start of the program. Girls born after 2006, in families with at most 2 children and at most one girl were eligible for the program.

For this part of the analysis I use the pooled births sample collected from mothers with young children, in which detailed vaccination and health outcomes are recorded. Based on the eligibility conditions of the program, children who were either the first or second daughter in a household with a maximum of three children constituted the "treatment" group. I assign each child into two mutually exclusive groups: Eligible (*e=1*) if she is the first or second daughter and there are at most 3 children in the household and *e=0* otherwise. The "control group" for the mortality sample constitutes (i) children born before 2006 in Karnataka and the control states (ii) children in families with more than 3 children and, (iii) boys in families with less than 3 children. For child i of mother j in state s in year t, I estimate the following:

$$Y_{ijst} = \alpha + \beta Kar_s * Post_t * E_i + \nu Post_t * Kar_s + \phi E_i * Post_t + \theta Kar_s * E_i + X_i\delta + \gamma_{st} + \nu_{bs} + \rho_{bt} + \epsilon_{ijst} \quad (3)$$

The dependent variable, Y_{ijst} is either a mortality indicator or measures health investments such as breastfeeding and vaccination status[33]. E_i refers to the eligibility of a child and can be 0 or 1. X_i is a vector that includes a child's gender as well as a child's mother's years of schooling, indicator's for mother's age cohort, mother's age at birth, indicators for household's religion, caste, residence in a rural area and a standard of living index. I also control for state-birth year fixed effects, γ_{st} (e.g., differential growth rates of state GDP or availability of abortion and other health services), birth order specific time effects, ρ_{bt} and state specific birth order fixed effects, ν_{bs}. β

[33]More details on the variables used in the regression analysis are available in Appendix A

is the coefficient of interest, and measures the effect of the program on the outcome of interest for eligible children before and after 2006 in Karnataka, relative to other states. The identifying assumption is that, in the absence of the scheme, eligible children in Karnataka would have had the same outcomes as children who met the eligibility criteria and had similar socio-economic characteristics before the scheme. I also include fixed effects for state, birth order and birth year, although these have not been mentioned above for notational ease. This rich set of fixed effects enables me to rule out a wide range of confounding variables and trends that can interfere with a causal interpretation of our findings, and I thereby provide better identification than earlier related research.

Since my data comprises multiple births per woman I also estimate specification (3) using mother fixed effects, exploiting the differential exposure of siblings to the program. This strategy addresses concerns pertaining to selection on underlying preferences or socioeconomic characteristics correlated with investments in girls versus boys.

Alternately, for infant mortality I also conduct survival analysis. This is because, infants who are less than 12 months old at the time of the survey have to be dropped from the sample in a binary response model. However, in the duration model I can include the full sample since duration analysis takes into account censoring for those observations that are not fully exposed. I focus on children born in the years after the *Bhagyalakshmi* program was implemented. They were thus exposed to the program at (or immediately after) birth. I investigate the effect of the program on the survival probabilities of children born immediately after its introduction by comparing infant mortality rates of children born around the year 2006. The sample consists of children born between 2000 and 2013. The empirical specification follows each child for up to 12 months and defines a child as being affected by the *Bhagyalakshmi* program if she is born in the year 2006 or later and is defined as eligible according to the eligibility definition outlined above.

The DLHS-2, DLHS-3 & DLHS-4 as well as the NFHS provide complete survival histories of all children born to respondents[34]. For each child, the dataset records the month and year of birth as well as the age at death in months. Using this information, I divide the one year observation window into a set of discrete time intervals, each of one month length. The resulting dataset is a panel where every child contributes between 1 and 12 observations, depending on its survival status. For each of these child-month observations I denote the survival status as a binary indicator taking the value of 0 if the child is alive and 1 if the child had died by the end of the time interval. The resulting dependent variable for the child's survival status is vector of a set of 0s and 1s for each child. The length of this column vector will depend on the age at which the child died. Using this panel, I estimate the hazard rate for the likelihood that a child dies in any given month, m,

[34] As mentioned above, the DLHS-3 included births only after January 1st, 2004 upto 2008 and the DLHS-4 includes births from January 1st 2008 upto 2013.

conditional on survival up to that month, *h(m, X)*. I use a discrete form of the proportional hazard model, the complementary log-log hazard model. In this specification, the hazard is a function of each month the child is alive m, the age in months t and individual and family characteristics X. I compare monthly survival rates of *eligible* children in Karnataka and control states using the following equation:

$$h(m, t, X) = \alpha_m + \beta Kar_s * Post_t + \gamma X_i + \delta_t + \mu_m + \phi_s \tag{4}$$

where $Kar_s = 1$ if state is *Karnataka* and 0 otherwise, $Post_t = 1$ if month m falls in the year 2006 or later, X_i is a vector of observable child and family specific characteristics, μ_m dummies for every month, ϕ_s state dummies and δ_t is the interval specific intercept that informs us about the shape of the hazard. The coefficient β reflects the impact of the program on child survival of eligible children. I account for unobserved heterogeneity within mothers by including mother fixed effects. Robust standard errors are clustered at the state-year level.

Figure 4 uses the panel data described above to calculate the Kaplan-Meier survival rates for each month. The estimates are reported for Karnataka and control states separately. The sample consists of children defined as eligible under the program. The blue and red lines denote children born before and after the program was implemented, respectively. The figure on the right reports the survival estimates for control states. For this sample, survival rates do not change significantly over time. This stands in stark contrast to the figure on the left, which reports the survival estimates for individuals in Karnataka. For *eligible* children in Karnataka born after the *Bhagyalakshmi* program show substantially improved survival chances compared to children born before 2006.

5 Results

5.1 Fertility

5.1.1 Identifying Assumption

I first examine the trends in composition of children in the state of Karnataka as compared to other states from 2000-2013 after controlling for various socio-economic characteristics, and fixed effects for state, year and woman's age. Figure 5 presents this graphical evidence. The dependent variables are indicators for belonging to one of three groups: no children, one child and two children. Women in these categories constitute our 'treatment' group[35]. More specifically, I plot the regression coefficients, β_j from the following regression. The omitted year is 2006: $Y_{isat} = \sum_{j=2000}^{2012} \beta Karnataka * 1[Year_t = j] + X_i\delta + \gamma_s + \omega_a + \theta_t + +\phi_s * t + \epsilon_{isat}$. This figure presents evidence of the identifying assumption that the trends in marginal births for these three

[35] From before, 'treatment' group is defined as women with less than equal to 2 children (except women with 2 girls).

groups evolve smoothly before the policy i.e. before 2006. I find that there is a decrease in probability of being childless after 2006. Further there is an increase in probability of having two children and a slight increase and then a decrease in the probability of having one child.

I also examine my identifying assumption slightly differently. I match eligible and non-eligible women based on their caste, religion, household wealth index and age at marriage. Eligible women are defined as those with zero, one or two children in *Karnataka*. I then plot their annual probability of childbirth in the pre-program period (1991-2003). Figure 6 examines these trends. I find that during the period leading up to the introduction of the *Bhagyalakshmi* program, fertility rates of eligible and non-eligible women followed a parallel trend.

Finally, I examine the trends in fertility in another way. I regress the annual probability of childbirth or pregnancy of each woman on caste, religion, age at marriage and household wealth index. Also included are indicators for eligibility, binary variables for years, and the interaction between eligibility and year indicators. The coefficients of the eligible-year interaction and their 95 % confidence intervals are presented in Figure 7. Reassuringly I find no consistent upward or downward trends in the figure. If there were clear trends in the figure then this would mean that there were other policies or program in the pre-2006 period that were causing the increase in probability of childbirth. However, this is not the case for my data as seen from this figure.

5.1.2 Impact on Marginal Births

In Table 3 I present the impact of the program on the probability of a marginal birth (Panel A). The most basic specification in column (1) controls for the main triple interaction effects of specification 1 as well as fixed effects for state-year, state-group and group specific time trends. In column (3) I add age-group fixed effects as well as controls for demographic and socio-economic characteristics of the woman.

The triple difference coefficients compare the change in probability of a marginal birth in period t in Karnataka versus other states, before and after 2006, by the group a woman belonged to in period *t-1*. The treatment group (groups 1,2 and 3) include women with less than equal to two children, while group 4 includes women with three or more children. The introduction of the program should lead to an increase in the probability of a marginal birth for women in groups 1, 2 and 3 compared to women in group 4. Indeed, this is what I find. Couples with one child are more likely to have another child after the introduction of the program. Further, the same is also true for couples with two children. However, the magnitudes are smaller for couples with two children suggesting that more one child families go on to have another child. I also find that childless couples are more likely to give birth after the program.

Since the group a woman belongs to in period *t-1* is not exogenous for the period after the program i.e. after 2006, I look at the impact of the program only in the first two years of the scheme i.e. I drop the period after 2008. In Table 4 I present these results. In this table the group a woman belongs to is not impacted by the program since we examine outcomes only for two years after the program. I find that women who had no children in 2006, were more likely to have children after the program. The magnitude of this impact is smaller than in Table 3. I also find that couples with one child are also more likely to have another child after the program. The magnitude of the effect is slightly bigger than in Table 3 and is also highly significant. However, there is no significant effect for couples with two children as we'd found in the previous table. This suggests that the positive effect found earlier was driven by women who had two children in the post 2006 period.

In Table 5 I redefine the treatment group for women after 2006. That is, for all the years after 2006, I define their treatment group as being the same as it was in 2006. Thus, the group to which a woman belongs to does not change in the post-program period. This ensures that the coefficients of interest are not impacted by changes in group composition after the introduction of the program[36]. I find that the results are similar to Table 3. Women in all three groups are more likely to have another birth with the magnitude being higher for women with zero children and one child in 2006.

In Table B.1 I redefine the control group to include only those with 3 children. That is, before, women in group 4 included all those with three or more children. However, women with more than 3 children are very different to women with less than 3 children. Even after controlling for women level characteristics and state-year, state-group and age-group fixed effects, there might still be unobserved differences between women. Thus, I restrict group 4 to women with only 3 children. Reassuringly, I find that the results are similar to the main results with the magnitude of the results being larger.

5.2 Mortality

5.2.1 Identifying Assumption

I first test the identifying assumption that the trends in infant and neo-natal mortality for eligible and non-eligible children evolve smoothly before the policy. I match eligible and non-eligible children on their caste, religion, household wealth index and mother's education. I then plot the annual trends in infant mortality rates for both groups of children for the pre-program period (1991-2003). Figure 8 examines these trends. I split the sample of non-eligible children into boys and girls. I find that mortality rates of eligible and non-eligible girls and non-eligible boys follow

[36]As mentioned in the previous section, since *Bhagyalakshmi* is a dynamic payment and not a one-time static payment, defining group composition right before the start of the program helps in eliminating any selection *because* of the program.

parallel trends in the years leading up to the program.

5.2.2 Impact on Neo-natal and Infant Mortality

In Table 6 I present the effects of the *Bhagyalakshmi* program on neonatal (Panel A) and infant mortality (Panel B). The most basic specification in column (1) controls for the main effects of *Eligible* and *Karnataka*, and their triple- and double-interactions with *Post* along with fixed effects for state by birth year, state and birth year. In column (2), I add birth order fixed effects and in column (3) I add controls for demographic and socioeconomic characteristics of the parents.

For both mortality measures, the coefficients in the second row are positive and highly significant for infant mortality. This implies that in the period before 2006, girls in households with at most two daughters and at most three children in total were more likely to die neonatally, and during their first year than non-eligible children in Karnataka as compared to the control states[37]. For instance, infant child mortality for eligible children in Karnataka was 1.17 percentage points (in column (3) of Panel B) higher than for non-eligible children in Karnataka during the pre-program period.

The triple interaction coefficients in the second rows in both panels are negative indicating that neonatal and infant mortality decreased once the program was introduced. The coefficients in panels A & B translate into a complete elimination of pre-program mortality after the first month of birth and until age one. For neonatal mortality (in panel A), the decline is smaller in magnitude than the corresponding decline in infant mortality in panel B. The fact that I find significant negative effects in columns (5) of panels A & B imply that the program decreased the gap in mortality (between eligible and non-eligible children) across births even within the same family as the program incentivized parents to decrease post-natal discrimination against girls by making it 'feasible' to bear daughters. These mother fixed-effects estimates are free from any compositional bias driven by differences in factors such as son preference across mothers, bolstering the causal interpretation of my estimates.

The difference in the effects on neonatal and infant mortality is consistent with the fact that neonatal deaths are primarily caused by poor maternal health and delivery conditions while post-neonatal mortality is caused by a poor disease environment, insufficient postnatal investments, and inadequate hospital care. If the main mechanism through which the program decreases female mortality is the decline in gender gap in postnatal parental investments or intra-household resource allocation, it follows that the decline in mortality should also be largest for infant mortality[38].

[37] From before: the "control group" for the mortality sample constitutes (i) children born before 2006 in Karnataka and the control states (ii) children in families with more than 3 children and, (iii) boys in families with less than 3 children.

[38] In Table B.2 I also examine the impact of the program for post-neonatal child mortality (mortality before the

Prior literature has found similarly heterogeneous results for neonatal and post-neonatal mortality. Bozzoli et al. (2009) show that adult height increases in the United States and Europe are more strongly associated with decline in post-neonatal mortality relative to neonatal mortality. Chay et al. (2009) find that the convergence in black-white gap in average test scores in the United States is highly correlated with decline in post-neonatal mortality rates but not with neonatal mortality.

5.3 Postnatal Health Investments

Table 7 examines if the program altered the gap between eligible and non-eligible children in postnatal health investments in Karnataka versus the control states. In panel A, the dependent variable is the number of months of breastfeeding and the sample is restricted to children who were at least 24 months at the time of the survey. In panel B, the dependent variable is if the child has received all 8 required immunizations and in panel C it is a dummy variable indicating that the child has received at least one vaccine. The specifications across columns are similar to those in Table 6. In Table B.3, I also examine various durations of breastfeeding. The dependent variables are indicators for breastfeeding duration being at least 12 months, 24 months, and 36 months, with the sample being restricted to children who are at least 12, 24, and 36 months old, respectively.

The coefficients in columns (3) & (4), of panels A and C show that in the pre-program period 'eligible' children were breastfed for a significantly shorter time period as well as significantly less likely to have received at least one vaccine. The financial transfer nearly eliminated these gaps in breastfeeding and vaccination. The triple interaction coefficients for receiving at least one vaccine and months of breastfeeding are highly significant and positive across all the columns in panels A and C.

The second row in Table B.3 shows that the gaps in breastfeeding between eligible and non-eligible children duration emerge after age one and persist thereafter. This finding is consistent with the fact that most Indian children are breastfed through the first year of life and any improvement that we find should be in the second and third years of life. The first row of the same table shows that the *Bhagyalakshmi* program significantly increased the probability of being breastfed in the second year of life. There is also an increase in the probability of being breastfed in the third year of life though this is not statistically significant.

In terms of full immunization, in panel B of Table 7, I find that eligible children are less likely to have received all the 8 required vaccines. Further, Table B.4, reports regressions where the dependent variables are indicators for BCG, Measles, any of the 3 polio vaccines, any of 3 DPT vaccines and a variable measuring the total number of DPT vaccines (where the maximum can be 3 DPT doses). I find that the program increased the likelihood of being vaccinated for all the

child is 60 months) and mortality at 24 and 36 months. The sample is restricted to children aged at least 5, 2 and 3 years respectively at the time of the survey. My results are similar to those for neonatal and infant mortality.

above mentioned vaccines. The results for having been immunized for at least one dose of DPT and measles are strongest.

In Table B.5, I also examine if the program affected postnatal health investments on first births. To the extent that the bulk of immunization and breastfeeding investments in the first child are made before the second birth, I do not expect to see any effects on these variables for first birth. However, if the program acted through the channel of reducing son preference, then mothers might be less likely to wean off first born girls sooner to try to conceive a son and might increase health investments in first births as well. I find that first births in the pre-program period are significantly less likely to have at least one vaccine. However, after the introduction of the program there was a significant increase in the probability of both full immunization as well as receiving at least one vaccine. The duration of breastfeeding also increases for first births though this is insignificant.

5.4 Heterogeneity

I examine the responses to the program along different margins. I examine if my results differ by mother's educational attainment (illiterate versus literate), household wealth (bottom 40 versus top 20 percent), household caste (scheduled caste (SC), scheduled tribe (ST) and other), and rural versus urban residence.

I find that the strongest impacts for both fertility and health investments are for women belonging to scheduled castes and women being illiterate. The probability of having no children declines for all sub-samples except for poorer women (low SLI women) for whom it increases. Further, paradoxically, the probability of having one or two children decreases for poorer women. This implies that the financial incentive seems to be working in the opposite direction for this sub-sample of women and discouraging marginal births for these women. On the other hand for slightly wealthier women (high SLI) the incentive encouraged marginal births and the probability of having two children increased. Further for this sub sample of women infant mortality for eligible children reduced and investments increased. For women with little or no education the results are similar to the main results with marginal births increasing and investments in eligible children rising. In terms of caste, for women belonging to scheduled castes, neonatal and infant mortality for eligible children declined and months of breastfeeding increased. For women living in rural areas, the results were similar. This is consistent with the fact that for a given change in investment, survival impacts are larger for poorer families where other causes of child mortality, such as infection rates, are higher.

6 Pathways of Impact

In this section I present possible pathways through which the program could have impacted parental behaviour. I focus on one important mechanisms: changes in the economic returns to children

6.1 Economic returns to children

Economic models of households view children as not only goods but also a means of investment. Inter-generational transfer of resources from children to parents implies that parents gain utility in the future from investing in the health of their children in the present. Moreover, in high son preference regions like India, it is mainly the son who is expected to maintain his parents in old age. After marriage, a woman leaves her parental family to move with her husband's. Further, prior to 2005, under the Hindu Law women could not inherit family property. Both these factors decrease the relative value of girls and increase the relative value of having a male offspring. This 'son-preference' leads to son stopping fertility behaviour. Two consequences of such fertility stopping behaviour are: first, women aim to ensure the birth of at least one son and second, investment in girls tends to be lower the higher the proportion of girls in the family.

One of the reasons why the *Bhagyalakshmi* program increased fertility and investments in girls could be that by increasing the relative value of girls the program decreased son preference. In order to examine if the results for fertility and mortality are driven by changes in the economic returns to girls, I examine the impact of the program, conditional on the gender composition of children born before the program. I divide the sample of mothers into two groups: women with at least one son before the introduction of the program and women without sons. For each group I evaluate the impact of the program using the following specification:

$$Y_{isat} = \alpha + \beta Karnataka * Post + X_i\delta + \gamma_s + \omega_a + \theta_t + \phi_s * t + \epsilon_{isat} \tag{5}$$

The dependent variable is a dummy for birth in year t. All other variables are defined as before. If the program worked through the mechanism of reducing son preference then for women with only girls before the program, the program should have no effect while for women with at least one son, the program should have a positive effect.

Table 9 presents these estimates. In column (1) I find that there is no statistically significant impact on the probability of a marginal birth for women with only girls and zero boys at the start of the program. However, for women with at least one boy born before the start of the program the program has a statistically significant positive impact on the probability of a marginal birth.

After analyzing fertility choices I examine if the program caused women to change their investment preferences in such a way as to prioritize the survival of girls born after the program. I analyze the impact of the program on mortality and health investments distinguishing along the gender composition of the eligible children's elder siblings born before the program. Eligible children born into a family with a first born girl should fare just as well as eligible children born into a family with a first born boy if the main mechanism through which the program had an impact was through changes in incentives faced by parents. That is, before the program, girls born into families with first born girls would tend to have higher mortality and lower investments in health (Rosenblum (2013); Anukriti et al (2016)). However, once the relative value of girls increased, this gender gap between girls from first born girl families and from first born boy families should vanish.

I use specification (5) to analyze the impact. Table 10 reports these estimates for two sub-samples: eligible children in families with a first born girl (Panel A) and eligible children in families with a first born boy (Panel B). For both panels, there is no significant difference in the outcomes of eligible children in first born girl families and in first born boy families. The impact for health investments is not significant for either sample. In terms of mortality, there is an increase in neonatal mortality for eligible children in both sub-samples.

7 Robustness Checks

7.1 Alternate treatment years

In the main analysis, I incorporated pre-program period and controlled for underlying trends in outcomes. I nevertheless confirm that my findings are not driven by any pre-trends by restricting the sample to the pre-program period and then re-estimating specification. More specifically, I assign each year from 1996-2002 as the "treatment" year. That is, $Post$ is defined as $> T$ where T=1996,...,2002. In order to claim that my main results are causal, I should not find any significant effects in these placebo regressions. Table B.7 presents these results. Reassuringly, I do not find any evidence of an underlying convergence in mortality and fertility outcomes that is unrelated to the introduction of the program.

7.2 Reassigning treatment to control states

I also conduct placebo regressions of the following nature. I reassign treatment to each control state. I should not find any impact of the policy when treatment is reassigned to these states since there was no program present in these states. That is, $State_s = 1$ if $State$=Tamilnadu, Orissa, West Bengal, Andhra Pradesh, Goa or Maharashtra. The results are presented in Table B.8.

7.3 Excluding one control state at a time

Even though I control for socio-economic characteristics in my specification one might still be concerned with unobservable heterogeneity that might make some control states less suitable as controls. Thus, in order to ensure that no one control state is driving my results I reestimate my results after excluding one control state at a time. Table B.6 presents these results. The results for infant mortality are almost identical to my main results. I find that infant mortality declines after the program even after excluding one control state at a time. The results for marginal births are also similar to the main results.

8 Discussion

The rapid increase in female feticide as well as the gender gap in health and eduction outcomes has led policymakers in India to introduce various financial incentives aimed at increasing the relative value of girls. In this paper, I examine one such incentive, *Bhagyalakshmi* in the state of Karnataka in India. Couples with less than 3 children are provided a financial transfer at the birth of a girl, for up to 2 girls per family. The transfer is in the form of a long term savings bond which is redeemable by the daughter once she turns 18. I utilize a large retrospective panel data set to examine the joint determination of the number of children a woman has and the investments she makes in each child. I find that the program significantly impacted the probability of a marginal birth for women who were either childless, had one child or had two children. Moreover, I also find that son stopping fertility behaviour reduces. Women with zero boys at the start of the program are *less* likely to have an additional birth after the program. Further, women with at least one boy are more likely to give a birth after the program. In terms of investments in children eligible under the program, I find that infant mortality for these children declines and investments in these children rises. Eligible children are more likely to be immunized and breastfed for a longer duration. However, this comes at the cost of non-eligible children in the household. Investments in non-eligible children decline. Thus there seems to be a quality-quantity tradeoff as a result of the program.

Table 1: Description of the incentives under the *Bhagyalakshmi* Program

Surving child ↓	**1st additional boy**	**1st additional girl**	**2nd additional girl**	**2nd additional boy**
No children	-	$\$X_1$	$\$X_2$	-
One girl	-	$\$X_1$	$\$X_2$	-
One boy	-	$\$X_1$	$\$X_2$	-
Two girls	-	$\$X_1$	-	-
Two boys	-	$\$X_1$	-	-
One boy, one girl	-	$\$X_1$	-	-
Three or more	-	-	-	-

Note: Here $\$X_1$ is the present discounted value of the incentive received under the program for the first girl beneficiary in the family and $\$X_2$ is the present value of the incentive received for the second girl beneficiary. X_1 is equal to 530$ and X_2 is equal to 500$.

Table 2: Summary Statistics

Variable	Karnataka Post=0	Karnataka Post=1	Other Post=0	Other Post=1
<u>Outcome Variables</u>				
Woman Year Sample				
Birth dummy	0.41	0.23	0.17	0.20
Childless	0.16	0.35	0.05	0.43
One child	0.13	0.35	0.05	0.34
Two children	0.28	0.26	0.11	0.20
Births Sample				
Neonatal Mortality	0.045	0.013	0.047	0.013
Infant Mortality	0.016	0.005	0.017	0.006
Months Breastfed	1.394	5.253	1.273	4.938
Full Vaccination	0.636	0.615	0.554	0.508
<u>Characteristics</u>				
SC	0.187	0.201	0.181	0.218
ST	0.087	0.105	0.152	0.167
Rural	0.696	0.644	0.669	0.686
Hindu	0.825	0.808	0.824	0.800
Muslim	0.155	0.175	0.122	0.127
Mother is Literate	0.494	0.803	0.527	0.914
Mother's age at birth: 13-18	0.175	0.047	0.127	0.038
Mother's age at birth: 19-24	0.509	0.567	0.507	0.536
Mother's age at birth: 25-30	0.192	0.274	0.234	0.296
Mother's age at birth: 31-44	0.043	0.063	0.062	0.086
Mother's age: 13-18	0.007	0.016	0.004	0.013
Mother's age: 19-24	0.169	0.423	0.143	0.426
Mother's age: 25-30	0.273	0.419	0.271	0.400
Mother's age: 31-44	0.541	0.116	0.576	0.136
Low HH Std of Living	0.446	0.196	0.464	0.329
Medium HH Std of Living	0.357	0.497	0.314	0.369
High HH Std of Living	0.197	0.307	0.222	0.303
N (Births)	68086	23310	523796	94338
N (Mothers)	27294	17176	208991	73391

Note: This table presents means of the main outcomes used in the paper for different samples. *Post* is defined as > 2006. Schooling and standard of living variables are at the time of the survey.

Table 3: Effect of *Bhagyalakshmi* on Marginal Births

A. Marginal Birth	(1)	(2)	(3)
$Karnataka * Post * Childless_{t-1}$	0.1207***	0.1197***	0.1086***
	(0.029)	(0.027)	(0.023)
$Karnataka * Post * OneChild_{t-1}$	0.1387***	0.1425***	0.1292***
	(0.028)	(0.026)	(0.021)
$Karnataka * Post * TwoChildren_{t-1}$	0.0591**	0.0599**	0.0592**
	(0.029)	(0.028)	(0.023)
Observations	549,379	549,379	549,379
State-Year FE	x	x	x
State-Group FE	x	x	x
Group Time Trends	x	x	x
Age-Group FE		x	
Age FE			x
Covariates		x	x

Note: This table reports the coefficients from specification (1). Child composition is defined in year *t-1*. Each column in a panel is a separate regression. The vector X_i comprises household wealth quintiles, caste, religion, residence in a rural area and educational attainment of mother. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table 4: Effect of *Bhagyalakshmi* on Fertility in first two years of implementation: 2007 & 2008

Dependent Variable	**Birth=1**
$Karnataka * Post * Childless_{t-1}$	0.0682
	(0.012)
$Karnataka * Post * OneChild_{t-1}$	0.1424***
	(0.012)
$Karnataka * Post * TwoChildren_{t-1}$	-0.0040
	(0.013)
Observations	421,435
State-Year FE	x
State-Group FE	x
Age-Group FE	x
Group Time Trends	x
Covariates	x

Note: This table reports the coefficients from specification (1). Child composition is defined in year *t-1*. Each column in a panel is a separate regression. The sample is restricted to the first two years of implementation of the program i.e. 2007 and 2008. The vector X_i comprises household wealth quintiles, caste, religion, residence in a rural area and educational attainment of mother. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table 5: Effect of *Bhagyalakshmi* on Fertility by Child Composition in 2006

Dependent Variable	(1)	(2)	(3)
$Karnataka * Post * Childless_{t-1}$	0.1348***	0.1329***	0.1217***
	(0.030)	(0.028)	(0.024)
$Karnataka * Post * OneChild_{t-1}$	0.1526***	0.1555***	0.1417***
	(0.030)	(0.028)	(0.023)
$Karnataka * Post * TwoChildren_{t-1}$	0.0601*	0.0604*	0.0600**
	(0.031)	(0.031)	(0.025)
Observations	549,379	549,379	549,379
State-Year FE	x	x	x
State-Group FE	x	x	x
Group Time Trends	x	x	
Age-Group FE		x	
Age FE			x
Covariates		x	x

Note: This table reports the coefficients from specification (1). Child composition for all the post-program years are defined as in 2006. Each column in a panel is a separate regression. The vector X_i comprises household wealth quintiles, caste, religion, residence in a rural area and educational attainment of mother. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table 6: Effect of *Bhagyalakshmi* on Mortality

A. Neonatal Mortality	(1)	(2)	(3)	(4)
Karnataka X Eligible X Post	-0.0182***	-0.0205***	-0.0100***	-0.0025***
	(0.000)	(0.000)	(0.000)	(0.000)
Karnataka X Eligible	0.0015	0.0019	0.0037*	0.0008
	(0.003)	(0.003)	(0.002)	(0.001)
Observations	263,972	263,972	263,972	263,972
B. Infant Mortality	(1)	(2)	(3)	(4)
Karnataka X Eligible X Post	-0.0271***	-0.0296***	-0.0224***	-0.0164***
	(0.000)	(0.000)	(0.000)	(0.000)
Karnataka X Eligible	0.0012	0.0015	0.0117***	0.0106**
	(0.005)	(0.005)	(0.003)	(0.001)
Observations	263,972	263,972	263,972	263,972
State-Birth Year FE	x	x	x	x
Birth Year FE	x	x	x	x
State FE	x	x	x	x
Birth Order FE		x	x	x
Covariates			x	x
Mother FE				x

Note: This table reports the coefficients from specification (2). Each column in a panel is a separate regression. The estimates for infant mortality include children who were at least 12 months old at the time of each of the survey rounds to account for censoring. The vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table 7: Effect of *Bhagyalakshmi* on Postnatal Health Investments

A. Months Breastfed	(1)	(2)	(3)	(4)
Karnataka X Eligible X Post	0.0450***	0.0593***	0.2216***	0.2101***
	(0.000)	(0.004)	(0.015)	(1.040)
Karnataka X Eligible	0.0952***	0.0880***	-0.1268**	0.0202
	(0.022)	(0.017)	(0.051)	(0.051)
Observations	528,151	528,151	528,151	528,151
B. Full Immunization	(1)	(2)	(3)	(4)
Karnataka X Eligible X Post	0.0010***	-0.0141***	-0.0171***	-0.0128
	(0.000)	(0.001)	(0.001)	(0.013)
Karnataka X Eligible	-0.0043	0.0021	0.0030	0.0087
	(0.011)	(0.007)	(0.007)	(0.013)
Observations	126,541	126,541	126,541	126,541
C. At least 1 Vaccine	(1)	(2)	(3)	(4)
Karnataka X Eligible X Post	0.0391***	0.0093***	0.0081***	0.0097***
	(0.000)	(0.000)	(0.001)	(0.003)
Karnataka X Eligible	-0.0156***	-0.0060**	-0.0081**	-0.0099**
	(0.004)	(0.002)	(0.003)	(0.003)
Observations	126,541	126,541	126,541	126,541
State-Birth Year FE	x	x	x	x
Birth Year FE	x	x	x	x
State FE	x	x	x	x
Birth Order FE		x	x	x
Covariates			x	x
Mother FE				x

Note: This table reports the coefficients from specification (2). Each column in a panel is a separate regression. The estimates for all three panels include children who were at least 24 months old at the time of each of the survey rounds to account for censoring. The vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table 8: Heterogeneous Effects of *Bhagyalakshmi*

A.1 Has Zero Children	SC	ST	Low SLI	High SLI	Illiterate	Rural
Karnataka X Post	-0.0161***	0.0050	0.0307***	-0.0716***	-0.0588***	-0.0141
	(0.012)	(0.013)	(0.012)	(0.016)	(0.013)	(0.016)
A.2 Has One Child	(1)	(2)	(3)	(4)	(5)	(6)
Karnataka X Post	-0.0169*	0.0008	-0.0131**	0.0236**	0.0197**	-0.0058
	(0.010)	(0.012)	(0.011)	(0.011)	(0.010)	(0.010)
A.3 Has Two Children	(1)	(2)	(3)	(4)	(5)	(6)
Karnataka X Post	0.0450***	-0.0108	-0.0121	0.0378***	0.0064*	0.0130*
	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
B.1 Neonatal Mortality	(1)	(2)	(3)	(4)	(5)	(6)
Karnataka X Eligible X Post	-0.0089***	0.0121*	0.0210**	0.0005	0.2101***	0.0011*
	(0.002)	(0.006)	(0.004)	(0.001)	(0.004)	(0.009)
B.2 Infant Mortality	(1)	(2)	(3)	(4)	(5)	(6)
Karnataka X Eligble X Post	-0.0083***	0.0008	-0.0095***	-0.0020*	-0.0077**	-0.0072***
	(0.004)	(0.004)	(0.015)	(0.004)	(0.004)	(0.010)
C.1 Months Breastfed	(1)	(2)	(3)	(4)	(5)	(6)
Karnataka X Eligible X Post	0.4807***	0.1727	-0.7736***	0.1164	1.1419	0.1685
	(0.236)	(0.190)	(0.179)	(0.227)	(0.307)	(0.097)
C.2 At least 1 Vaccine	(1)	(2)	(3)	(4)	(5)	(6)
Karnataka X Eligible X Post	-0.0326***	0.0094	0.0095	0.0118***	0.0242**	0.0084**
	(0.008)	(0.010)	(0.015)	(0.004)	(0.010)	(0.004)

Note: This table reports the coefficients from specifications (2) & (3) for different sub-samples. Each cell is a separate regression. The vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table 9: Mechanism - Changes in Economic Returns to Children

A. Birth dummy	Has Zero Boys	Has at least 1 boy
$Karnataka * Post$	0.0066	0.0395***
	(0.009)	(0.010)
Observations	1,438,379	2,072,254
State FE	x	x
Year FE	x	x
State Time Trends	x	x
Age FE	x	x
Covariates	x	x

Note: This table reports the coefficients from specifications (5). The vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table 10: Mechanism - Differences in Sibling Composition

A. First Born Girl	Neonatal Mortality	Infant Mortality	Months Breastfed	Any Vaccine
$Karnataka * Eligible * Post$	0.0912**	-0.0134	0.2114	0.0099
	(0.027)	(0.019)	(0.702)	(0.039)
Observations	38,829	38,829	38,829	38,829
B. First Born Boy	(1)	(2)	(3)	(4)
$Karnataka * Eligible * Post$	0.0080**	-0.0059**	0.0595	0.0026
	(0.002)	(0.008)	(0.127)	(0.003)
Observations	198,743	198,743	198,743	198,743
State FE	x	x	x	x
Year FE	x	x	x	x
State Time Trends	x	x	x	x
Age FE	x	x	x	x
Covariates	x	x	x	x

Note: This table reports the coefficients from specifications (5). *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

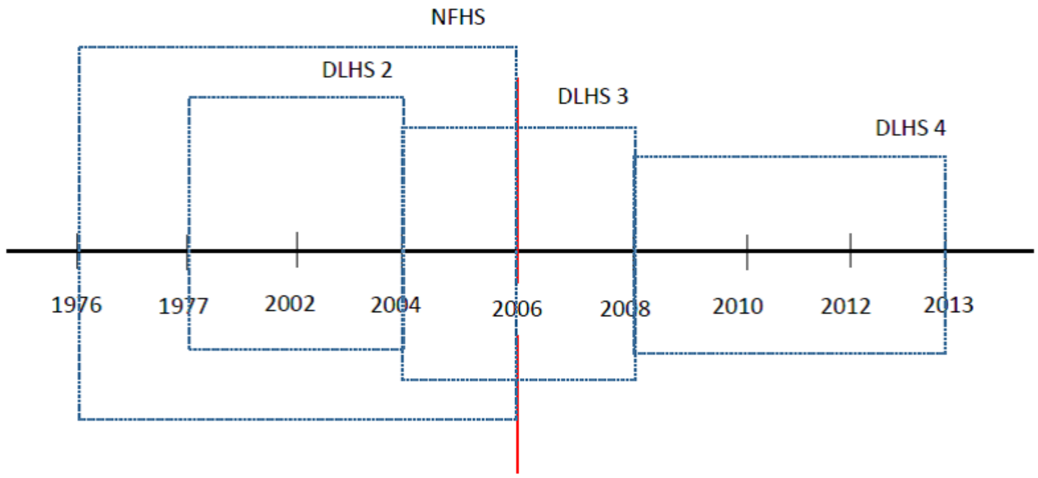

Figure 1: Timing of Survey Rounds

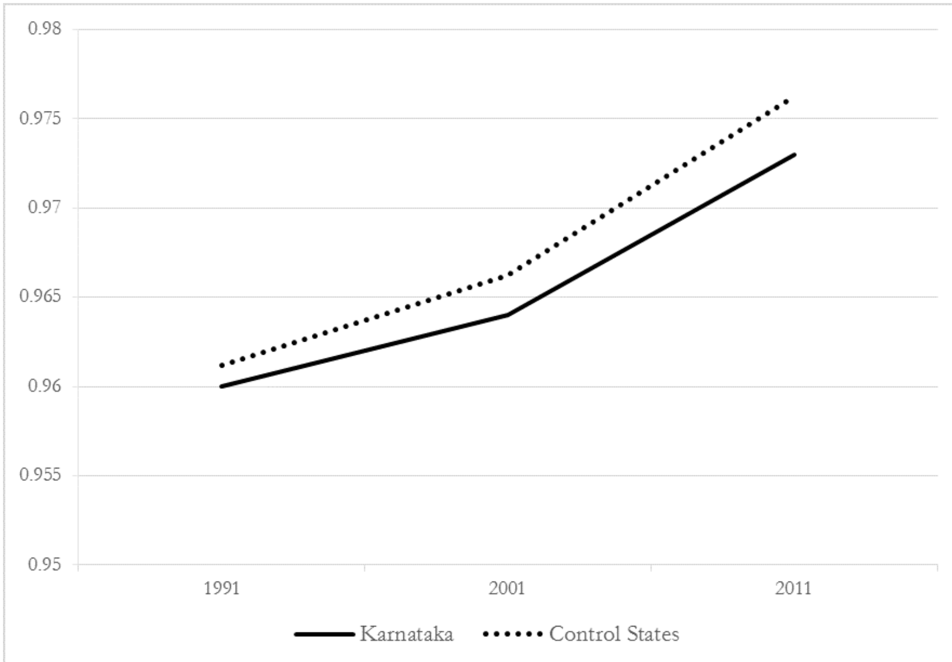

Figure 2: Female-Male Sex Ratio at Birth

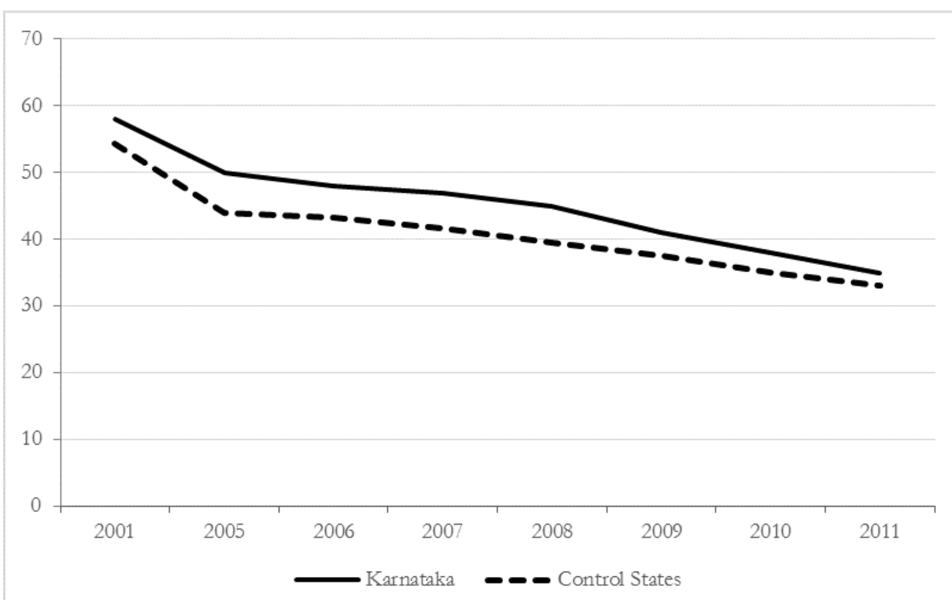

Figure 3: Infant Mortality Rate per 1000 Population

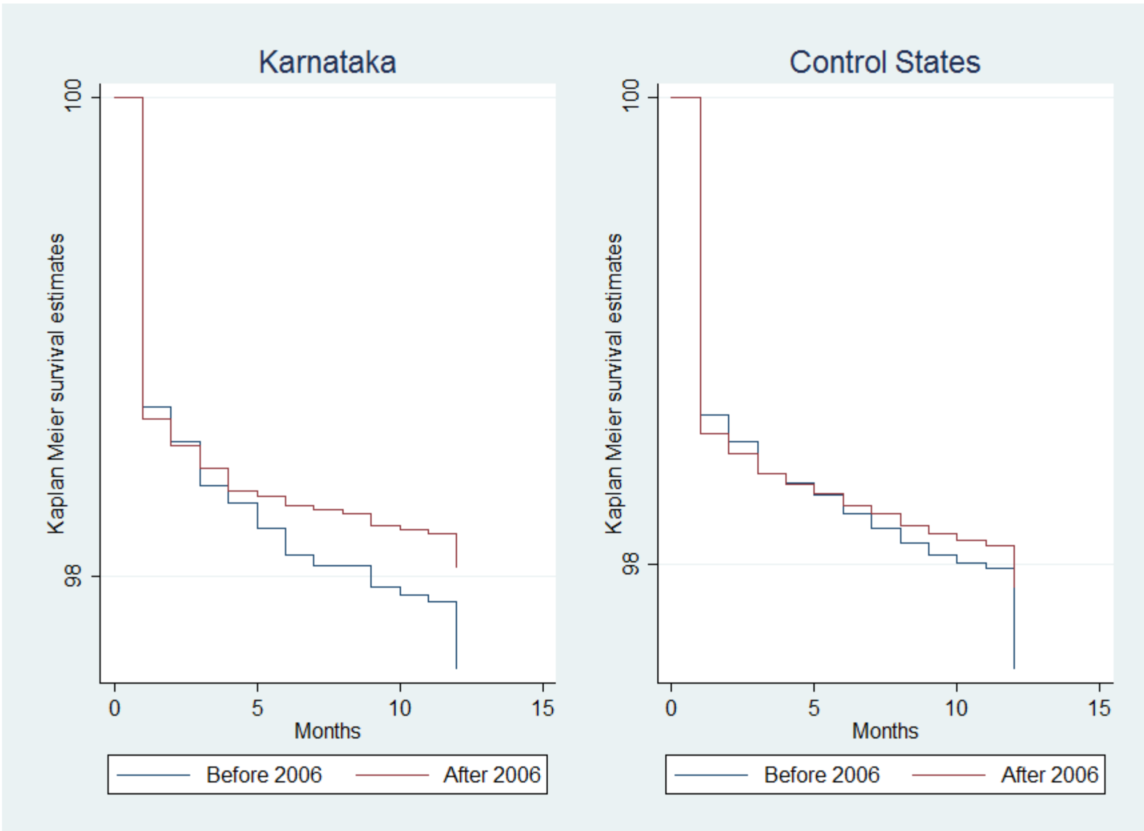

Figure 4: Survival estimates for children born between 2000-2013

Note: Figures report Kaplan Meier survival curves by year of birth and state of residence; each child is followed for one year and contributes one observation for each month alive before its first birthday, all observations are artificially censored after the first birthday, each child contributes between 1 and 12 observations depending on its survival status; sample consists of children born between 2000 and 2013; Panel a: the figure plots children currently residing in Karnataka; Panel b: the figure plots children currently residing in control states.

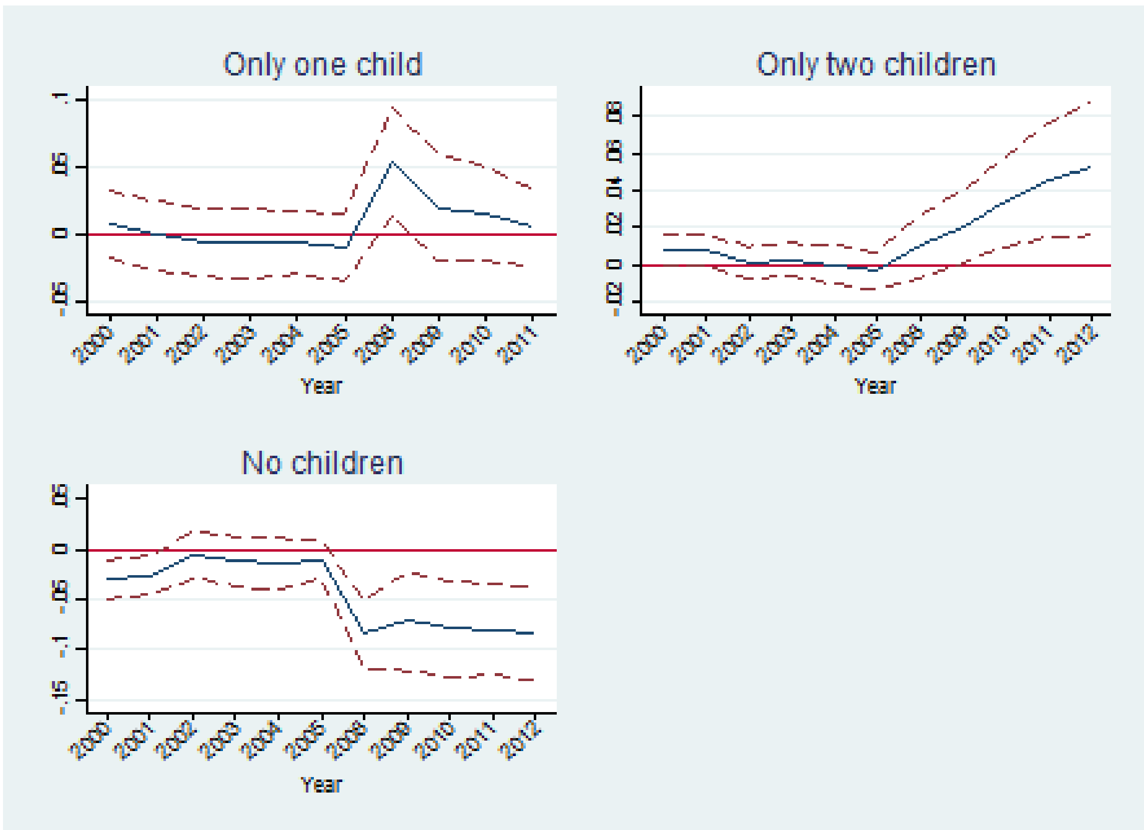

Figure 5: Trends in child composition, in Karnataka vs. Other States before & after 2006

Note: This figure plots the regression coefficients, β_j from the following regression. The omitted year is 2006: $Y_{isat} = \sum_{j=2000}^{2012} \beta Karnataka * 1[Year_t = j] + X_i\delta + \gamma_s + \omega_a + \theta_t + +\phi_s * t + \epsilon_{isat}$.

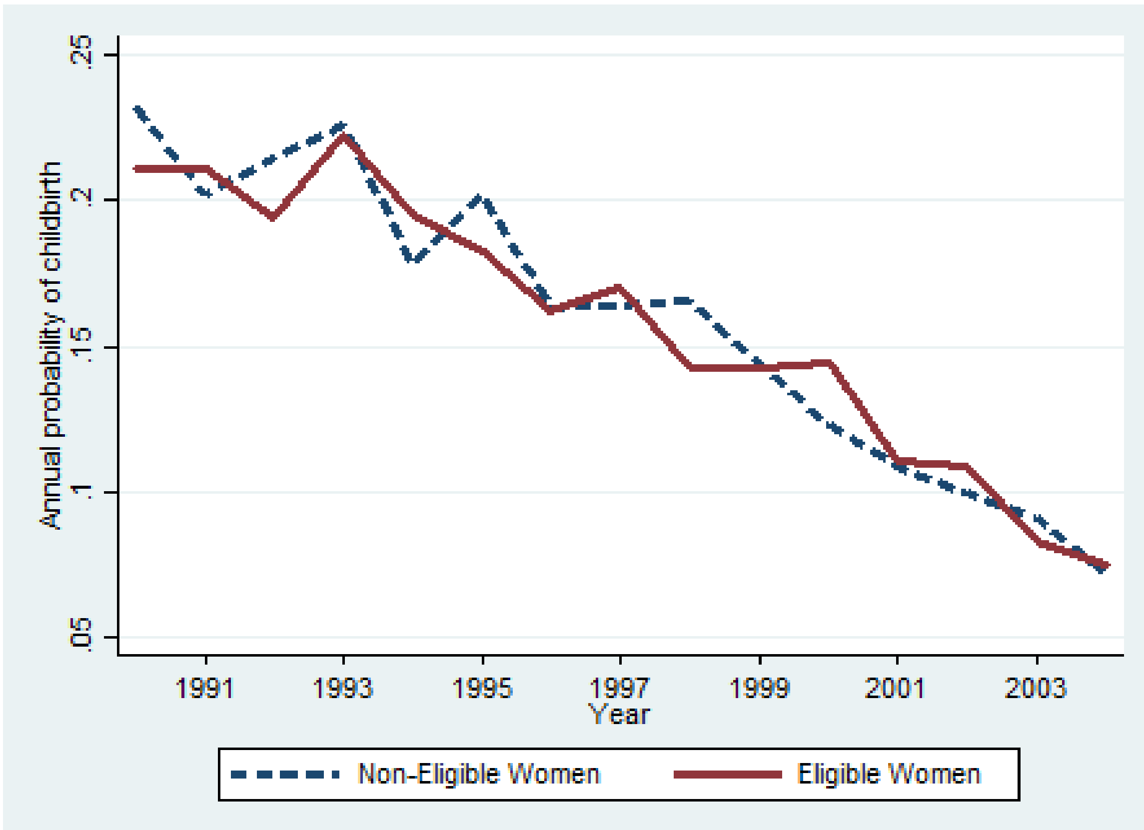

Figure 6: Trend of annual probability of childbirth among eligible and non-eligible women **after matching**

Note: Data are from NFHS (1991-2005). The series in the graph are the annual probability trends of at least one childbirth of eligible and non-eligible women. Eligible women are defined as women with less than equal to 2 children in Karnataka and non-eligible children are defined as women with 3 or more children in Karnataka and women in control states. Eligible and non-eligible women are matched on caste, religion, household wealth index and age at marriage of the woman. Then, eligible women are matched with similar non-eligible women based on the estimated propensity score (nearest-neighbor matching with replacement and within a radius of 0.01).

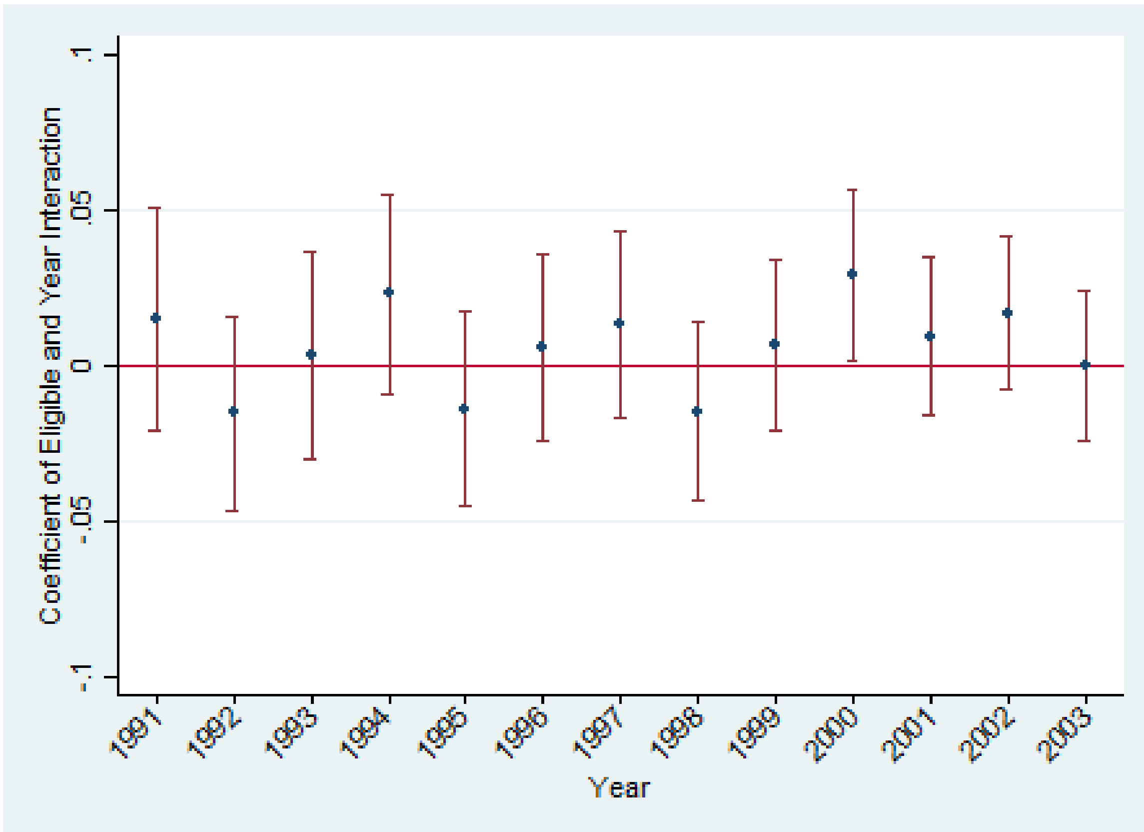

Figure 7: Coefficient of (Eligible x Year) in the linear regression of the probability of childbirth among eligible and non-eligible women

Note: Data are from NFHS (1991-2005). I regress the annual probability of childbirth or an ongoing pregnancy of each woman on a set of explanatory variable - caste, religion, household wealth index and age at marriage of the woman. Also included are an indicator for eligibility, binary variables for years, and interaction between eligible and year indicators. The coefficients of the Eligible-year interaction terms and their 95% confidence intervals are presented in the graph. 1990 is the base year. Eligible women are defined as women with less than equal to 2 children in Karnataka and non-eligible children are defined as women with less than 2 children in control states.

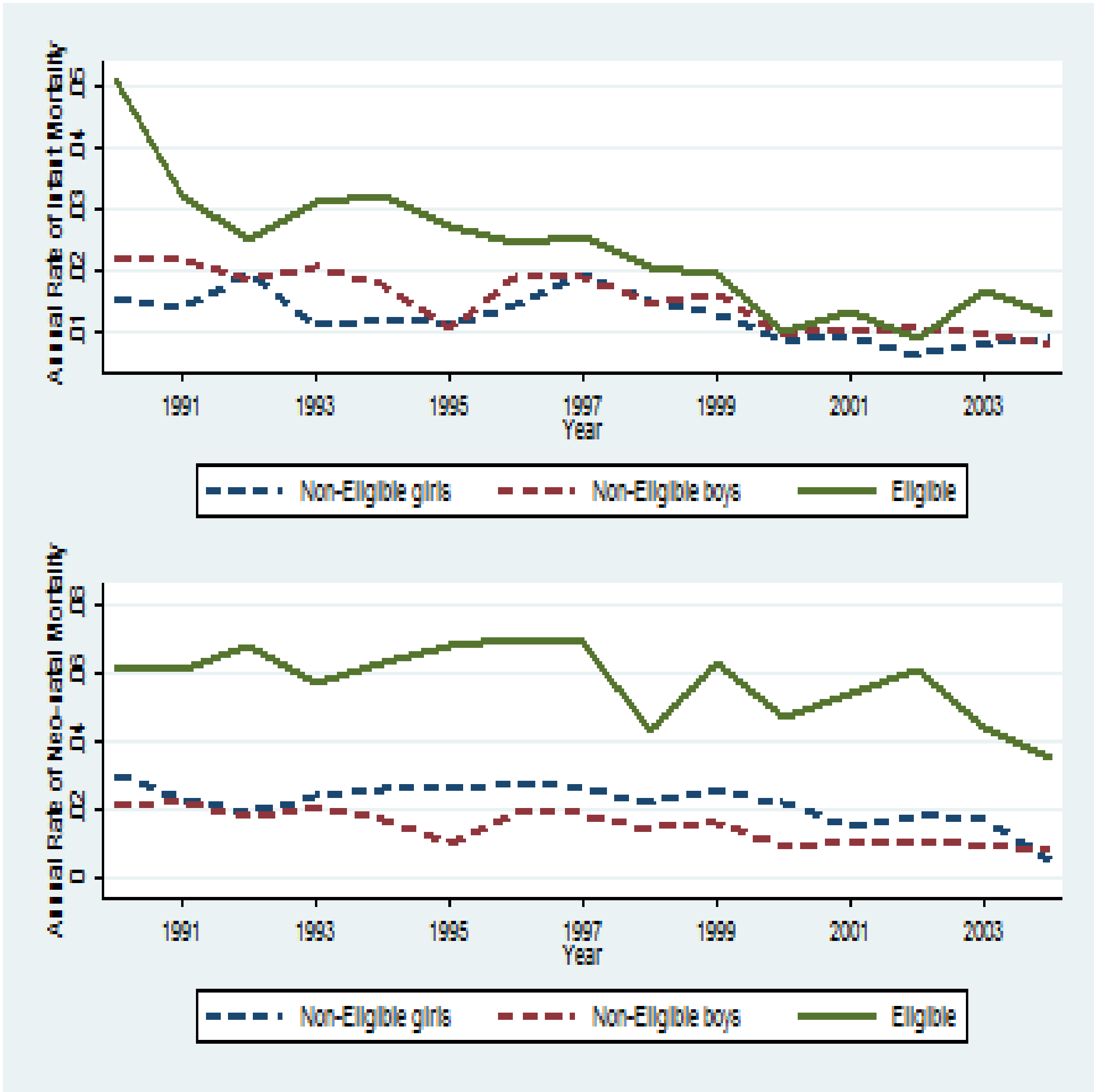

Figure 8: Trend of annual rate of mortality of eligible and non-eligible children

Note: Data are from NFHS & DLHS-2 (1991-2005). The series in the graph are the annual rates of infant and neo-natal mortality. Eligible children are defined as children who are the first or second daughter in a family of up to 3 children in Karnataka.

A Data Appendix

- Neonatal mortality: Death within one month of birth
- Infant mortality: Death before the first birthday
- Months Breastfed: Number of months of breastfeeding for children who were at least 2 years old at the time of the survey
- Full Vaccination: indicator variable for receiving all 8 required vaccines
- Any Vaccination: indicator variable for receiving any of the 8 required vaccines
- $Post_t$: indicator variable for $t > 2006$
- Categories for Mother's age at birth: 13-18 years, 18-24 years, 25-30 years, and 31-44 years
- Categories for Mother's age: 13-18 years, 18-24 years, 25-30 years, and 31-44 years
- Religion categories: Hindus, Muslims
- Caste categories: Scheduled Castes (SC), Scheduled Tribes (ST)
- Household Wealth Index: I categorize households into 3 groups (not necessarily of equal size) based on their wealth index score - low, medium and high SLI. NFHS, DLHS-3 and DLHS-4 use a principle components analysis to compute the HH wealth index score. But in DLHS-2 a different methodology was adopted. To make the wealth index comparable across rounds, I recreate scores for NFHS, DLHS-3 and DLHS-4 using the same method as DLHS-2, which is as follows:

  A household's wealth score is calculated by adding the following scores:
  Source of drinking water: 3 for Tap (own), 2 for Tap (shared), 1 for hand pump and well, and 0 for other;
  Type of house: 4 for pucca, 2 for semi-pucca, and 0 for kachcha;
  Source of lighting: 2 for electricity, 1 for kerosene, and 0 for other;
  Fuel for cooking: 2 for LPG gas/electricity, 1 for kerosene and 0 for other;
  Toilet facility: 4 for own flush toilet, 2 for own pit toilet, 2 for shared toilet and 0 for no toilet;
  Ownership of durables: 4 each for car and tractor, 3 each for television, telephone and motorcycle/ scooter, and 2 each for fan, radio/transistor, sewing machine and bicycle.

  The scores when added may vary from a lowest of 0 to a maximum of 40. On the basis of this total score, households are divided into three categories:
  Low SLI - if the total wealth score ≤ 9,

Medium SLI - if the total score > 9 but ≤ 19, and
High SLI - if the total score > 19.

B Additional Figure and Tables

Table B.1: Effect of *Bhagyalakshmi* on Fertility: Different definition of Eligible

Dependent Variable	(1)	(2)	(3)
$Karnataka * Post * Childless_{t-1}$	0.3835***	0.3759***	0.2463***
	(0.030)	(0.028)	(0.026)
$Karnataka * Post * OneChild_{t-1}$	0.4034***	0.4004***	0.2691***
	(0.030)	(0.029)	(0.027)
$Karnataka * Post * TwoChildren_{t-1}$	0.3182***	0.3123***	0.1913***
	(0.030)	(0.029)	(0.026)
Observations	511,076	511,076	511,076
State-Year FE	x	x	x
State-Group FE	x	x	x
Group Time Trends	x	x	x
Age-Group FE		x	
Age FE			x
Covariates		x	x

Note: This table reports the coefficients from specification (1). Group 4 includes only mothers with 3 children. Child composition for all the post-program years are defined as in 2006. Each column in a panel is a separate regression. The vector X_i comprises household wealth quintiles, caste, religion, residence in a rural area and educational attainment of mother. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table B.2: Impact on Mortality: 60 months, 24 months, 36 months

	Post Neonatal Mortality	24 Months	36 Months
	(1)	(2)	(3)
Karnataka X Post X Eligible	-.002	-.005	-.005
	(0.001)*	(0.001)***	(0.001)***
Karnataka X Eligible	0.002	0.005	0.004
	(0.0009)**	(0.0006)***	(0.0008)***
Obs.	237588	237588	237588

Note: This table reports the coefficients from specification (2) for mortality at 60 months, 24 months and 36 months. Each column in a panel is a separate regression. The estimates for mortality at 60 months, 24 months and 36 months include children who were at least 60 months, 24 months and 36 months at the time of each of the survey rounds to account for censoring. The vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table B.3: Impact on Breastfeeding at 12 months, 24 months and 36 months

	Breastfed ¿ 12	Breastfed ¿ 24	Breastfed ¿ 36
	(1)	(2)	(3)
Karnataka X Post X Eligible	0.013	0.014	0.006
	(0.009)	(0.005)***	(0.005)
Karnataka X Eligible	0.001	-.016	-.012
	(0.008)	(0.004)***	(0.007)*
Obs.	88952	58352	32995

Note: This table reports the coefficients from specification (2) for duration of breastfeeding of 12 months, 24 months and 36 months. The estimates for breastfeeding duration include children who were at least 12 months, 24 months and 36 months respectively at the time of the survey. The vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table B.4: Impact on 8 Required Vaccines

	BCG	Measles	Any Polio	Any DPT	DPT Number
	(1)	(2)	(3)	(4)	(5)
Karnataka X Post X Eligible	0.003	0.007	0.003	0.016	0.034
	(0.007)	(0.004)*	(0.005)	(0.005)***	(0.023)
Karnataka X Eligible	0.003	-.003	-.006	-.013	-.043
	(0.006)	(0.006)	(0.005)	(0.005)***	(0.021)**
Obs.	90624	90624	90624	90624	90624

Note: This table reports the coefficients from specification (2) for 8 required vaccines: BCG, Measles, any of the 3 polio vaccines, any of the 3 DPT vaccines. The vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table B.5: Heterogeneity in the effects on health investments for first births

	Months Breastfed	Full Vaccine	Any Vaccine
	(1)	(2)	(3)
Karnataka X Post X Eligible	0.128	0.029	0.028
	(0.242)	(0.017)*	(0.003)***
Karnataka X Eligible	-.066	-.015	-.012
	(0.149)	(0.008)*	(0.002)***
Obs.	30890	37013	37013

Note: This table reports the coefficients from specification (2) for 8 required vaccines: BCG, Measles, any of the 3 polio vaccines, any of the 3 DPT vaccines. The vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table B.6: Robustness Check I

A. Marginal Births ↓/State Excluded →	**Kerala**	**Tamilnadu**	**West Bengal**	**Maharashtra**	**Andhra Pradesh**	**Goa**
$Karnataka * Post * Childless_{t-1}$	0.1087***	0.1146***	0.1155***	0.1272***	0.1011***	0.0931***
	(0.023)	(0.027)	(0.025)	(0.028)	(0.025)	(0.018)
$Karnataka * Post * OneChild_{t-1}$	0.1324***	0.1312***	0.1379***	0.1423***	0.1270***	0.1221***
	(0.022)	(0.026)	(0.024)	(0.027)	(0.024)	(0.019)
$Karnataka * Post * TwoChildren_{t-1}$	0.0607**	0.0655**	0.0639**	0.0537*	0.0491*	0.0612***
	(0.023)	(0.028)	(0.026)	(0.026)	(0.027)	(0.015)
Observations	531,576	449,907	471,152	417,294	469,754	513,823
B. Post-Neonatal Mortality↓/State Excluded →	**Kerala**	**Tamilnadu**	**West Bengal**	**Maharashtra**	**Andhra Pradesh**	**Goa**
Karnataka X Eligible X Post	-0.0063***	-0.0074***	-0.0061***	-0.0068***	-0.0063***	-0.0064***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Karnataka X Eligible	0.0068***	0.0069***	0.0069***	0.0066***	0.0067***	0.0066***
	(0.001)	(0.002)	(0.003)	(0.000)	(0.000)	(0.001)
Observations	233,026	210,241	211,499	193,012	214,748	230,494

Note: Each column is a separate regression. vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Post* is defined as > 2006. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table B.7: Robustness Check II

A. Dependent Variable ↓	**Post1996**	**Post1997**	**Post1998**	**Post1999**	**Post2000**	**Post2001**	**Post2002**
$Karnataka * Post * Childless_{t-1}$	0.0055	0.0029	0.0067	0.0015	0.0017	0.0010	0.0041
	(0.048)	(0.044)	(0.041)	(0.038)	(0.035)	(0.030)	(0.032)
$Karnataka * Post * OneChild_{t-1}$	-0.0007	0.0004	0.0049	-0.0043	-0.0090	-0.0060	-0.0037
	(0.071)	(0.067)	(0.063)	(0.061)	(0.059)	(0.053)	(0.056)
$Karnataka * Post * TwoChildren_{t-1}$	0.0045	0.0075	0.0109	0.0061	0.0078	0.0099	0.0110
	(0.113)	(0.102)	(0.096)	(0.089)	(0.082)	(0.071)	(0.072)
Observations	1,335,161	1,335,161	1,335,161	1,335,161	1,335,161	1,335,161	1,335,161
B. Post-Neonatal Mortality↓	**Post1996**	**Post1997**	**Post1998**	**Post1999**	**Post2000**	**Post2001**	**Post2002**
Karnataka X Eligible X Post	-0.0087	-0.0096	-0.0075	-0.0189	-0.0252*	-0.0420***	0.0515
	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Karnataka X Eligible**	0.0163**	0.0161**	0.0150**	0.0174**	0.0172**	0.0181***	0.0162***
	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.005)	(0.005)
Observations	81,513	81,513	81,513	81,513	81,513	81,513	81,513

Note: Each column is a separate regression. *PostT=1* if *year*> T vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

Table B.8: Robustness Check III

Dependent Variable ↓	**Tamilnadu**	**West Bengal**	**Goa**	**Andhra Pradesh**	**Maharashtra**
$State * Post * Childless_{t-1}$	-0.3389	-0.2988	-0.4794**	-0.3232	-0.3565*
	(0.207)	(0.205)	(0.207)	(0.217)	(0.181)
$State * Post * OneChild_{t-1}$	-0.2311	-0.2163	-0.3752	-0.2051	-0.2674
	(0.240)	(0.246)	(0.246)	(0.250)	(0.196)
$State * Post * TwoChildren_{t-1}$	-0.0363	-0.0500	-0.1405	-0.0624	-0.0861
	(0.195)	(0.189)	(0.191)	(0.177)	(0.174)
Observations	2,132,607	2,132,607	2,132,607	2,132,607	2,132,607
State-Year FE	x	x	x	x	x
State-Group FE	x	x	x	x	x
Group Time Trends	x	x	x	x	x
Age-Group FE	x	x	x	x	x
Covariates	x	x	x	x	x

Note: Each column is a separate regression. *PostT=1* if *year*> T vector X_i comprises mother's age at birth, household wealth quintiles, caste, religion, residence in a rural area, educational attainment of child's parents, and mother's birth cohort. *Karnataka=1* if state of residence is Karnataka and 0 otherwise. Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10%.

References

Jason Abrevaya. Are there missing girls in the united states? evidence from birth data. *American Economic Journal: Applied Economics*, 1(2):1–34, 2009.

Douglas Almond and Lena Edlund. Son-biased sex ratios in the 2000 united states census. *Proceedings of the National Academy of Sciences*, 105(15):5681–5682, 2008.

Siwan Anderson. Why dowry payments declined with modernization in europe but are rising in india. *journal of political Economy*, 111(2):269–310, 2003.

Siwan Anderson and Debraj Ray. Missing women: age and disease. *The Review of Economic Studies*, 77(4):1262–1300, 2010.

S Anukriti. Economic incentives and the fertility-sex ratio trade-off.

Abay Asfaw, Klaus Frohberg, KS James, and Johannes Jütting. Fiscal decentralization and infant mortality: empirical evidence from rural india. *The Journal of Developing Areas*, 41(1):17–35, 2007.

Silvia Helena Barcellos, Leandro S Carvalho, and Adriana Lleras-Muney. Child gender and parental investments in india: Are boys and girls treated differently? *American Economic Journal: Applied Economics*, 6(1):157–189, 2014.

Alaka Malwade Basu. Is discrimination in food really necessary for explaining sex differentials in childhood mortality? *Population studies*, 43(2):193–210, 1989.

Gary S Becker and H Gregg Lewis. Interaction between quantity and quality of children. In *Economics of the family: Marriage, children, and human capital*, pages 81–90. University of Chicago Press, 1974.

Gary S Becker and Nigel Tomes. Human capital and the rise and fall of families. In *Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education (3rd Edition)*, pages 257–298. The University of Chicago Press, 1994.

Sonia Bhalotra. Fatal fluctuations? cyclicality in infant mortality in india. *Journal of Development Economics*, 93(1):7–19, 2010.

Sonia Bhalotra and Arthur Van Soest. Birth-spacing, fertility and neonatal mortality in india: Dynamics, frailty, and fecundity. *Journal of Econometrics*, 143(2):274–290, 2008.

Vani K Borooah. Gender bias among children in india in their diet and immunisation against disease. *Social Science & Medicine*, 58(9):1719–1731, 2004.

Woojin Chung and Monica Das Gupta. The decline of son preference in south korea: The roles of development and public policy. *Population and Development Review*, 33(4):757–783, 2007.

Shelley Clark. Son preference and sex composition of children: Evidence from india. *Demography*, 37(1):95–108, 2000.

Alma Cohen, Rajeev Dehejia, and Dmitri Romanov. Financial incentives and fertility. *Review of Economics and Statistics*, 95(1):1–20, 2013.

Sylvie Dubuc and David Coleman. An increase in the sex ratio of births to india-born mothers in england and wales: Evidence for sex-selective abortion. *Population and Development Review*, 33 (2):383–400, 2007.

Daniel Goodkind. On substituting sex preference strategies in east asia: Does prenatal sex selection reduce postnatal discrimination? *Population and Development Review*, pages 111–125, 1996.

Monica Das Gupta. Selective discrimination against female children in rural punjab, india. *Population and development review*, pages 77–100, 1987.

Seema Jayachandran and Ilyana Kuziemko. Why do mothers breastfeed girls less than boys? evidence and implications for child health in india. Technical report, National Bureau of Economic Research, 2009.

Robert Jensen. Do labor market opportunities affect young women's work and family decisions? experimental evidence from india. *The Quarterly Journal of Economics*, page qjs002, 2012.

Robert Todd Jensen. *Equal Treatment, Unequal Outcomes?: Generating Sex Inequality Through Fertility Behaviour*. Bazaar Chintan, 2003.

Hongbin Li, Junjian Yi, and Junsen Zhang. Estimating the effect of the one-child policy on the sex ratio imbalance in china: identification based on the difference-in-differences. *Demography*, 48(4):1535–1557, 2011.

Kevin Milligan. Subsidizing the stork: New evidence on tax incentives and fertility. *Review of Economics and statistics*, 87(3):539–555, 2005.

Emily Oster. Hepatitis b and the case of the missing women. *Journal of Political Economy*, 113 (6):1163–1216, 2005.

Chai Bin Park and Nam-Hoon Cho. Consequences of son preference in a low-fertility society: imbalance of the sex ratio at birth in korea. *Population and Development Review*, pages 59–84, 1995.

Mark M Pitt, Mark R Rosenzweig, and Md Nazmul Hassan. Productivity, health, and inequality in the intrahousehold distribution of food in low-income countries. *The American Economic Review*, pages 1139–1156, 1990.

Nancy Qian. Quantity-quality and the one child policy: The only-child disadvantage in school enrollment in rural china. Technical report, National Bureau of Economic Research, 2009.

Elaina Rose. Gender bias, credit constraints and time allocation in rural india. *The Economic Journal*, 110(465):738–758, 2000.

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

Amartya Sen. More than 100 million women are missing. *The New York Review of Books*, 1990.

Nistha Sinha and Joanne Yoong. Long-term financial incentives and investment in daughters: evidence from conditional cash transfers in north india. *World Bank Policy Research Working Paper Series, Vol*, 2009.

Sharada Srinivasan and Arjun S Bedi. Girl child protection scheme in tamil nadu: an appraisal. *Economic and Political Weekly*, pages 10–12, 2009.