Fertility Limits on Local Politicians in India*

S Anukriti[†] Abhishek Chakravarty[‡]

September 19, 2014

Abstract

We examine the demographic implications of fertility limits on local politicians. Several Indian states disbar individuals with more than two children from contesting Panchayat and municipal elections. These two-child limits are intended to decrease fertility among the constituents through a role-model effect and by incentivizing individuals who intend to run for elections in the future to plan smaller families. We find that fertility limits on elected representatives decrease voters' likelihood of having more than two children. However, they also increase the sex ratio of second births for politically dominant upper-caste families with historically stronger preference for sons. Our results point towards a novel source of demographic influence: political leaders.

Keywords: India, Local Elections, Fertility Limits, Sex Ratios, Population Control

*We thank Heather Banic for excellent research assistance.

[†]Department of Economics, Boston College. anukriti@bc.edu.

[‡]Department of Economics, University of Essex. achakrb@essex.ac.uk.

1 Introduction

Developing countries with large populations often employ policy tools to decrease fertility. Recent measures include direct fertility limits on citizens (e.g., China's One Child Policy), conditional cash transfer programs (e.g., Haryana's *Devirupak* scheme), and incentives to promote contraceptive prevalence (e.g., sterilization incentives in India). This paper examines a novel policy experiment that imposes fertility limits on political candidates running for leadership positions in the local government. Specifically, we analyze the impact of state-level legislations that disbar individuals with more than two children from contesting *Panchayat* and municipal elections on constituents' fertility-related outcomes in India.

The rationale behind these laws is that limiting fertility of elected representatives can decrease fertility among their constituents through a "role-model" effect. In addition, these limits directly incentivize individuals who aspire to run for local office in the future to have fewer children. The effectiveness of this policy measure in decreasing fertility, thus, depends on the extent to which individuals' fertility behavior is influenced by role-models and on the attainability of a local leadership position for an ordinary citizen.

Starting with Rajasthan in 1992, eleven Indian states have enacted two-child limits on local elected representatives. While four states revoked these laws after a few years of implementation, it remains in effect in seven states. In all cases, the law included a one year post-announcement grace-period, births during which were not counted towards the limit. Our identification strategy exploits the quasi-experimental variation in the state and the year of announcement of these laws to estimate their causal impact on fertility-related outcomes in a differences-in-differences framework. We combine complete retrospective birth histories from large-scale household surveys to construct a woman-year panel that spans the years between 1973 to 2006. Broadly, we find that two-child limits on electoral candidates decrease fertility in the general population but have unintended consequences for the sex ratio at birth when son preference is strong.

Overall, the fertility limits significantly decrease the likelihood that a woman has more than two children in a given year. Women who gave birth to two children before the two-child limit was announced in their state are significantly less likely to have a third birth. Consistent with the one-year grace-period, event-study analysis reveals that the likelihood of third births does not change significantly in the first year after announcement, but declines sharply thereafter. In addition, we examine how these effects vary across treatment states by generating state-wise regression discontinuity (RD) type graphs.

We also examine the effect on the likelihood of second births and their sex ratio for women whose first child was born before the laws were announced. We conduct this analysis in a triple differences-in-differences setting by utilizing the variation in the sex of the firstborn child. Prior literature shows that the sex of first births in India is close to random, despite availability of prenatal sex-determination technology. We find that, due to the two-child limits, women with firstborn girls are less likely to have a second birth and are more likely to have a male child if there is a second birth, but only in upper-caste households. The negative effect on second births can be explained by increased sex-selection as each abortion delays the next birth, at the minimum, by a year (Bhalotra and Cochrane (2010)). These results indicate that, among those whose first child was born before the treatment year, households wishing to remain eligible for village council leadership restrict their fertility to two children, but ensure that the second child is male if the first is not. Thus, a preference for sons can cause policies that target lower fertility to have unintended effects on sex ratios.

We conduct a number of robustness checks to establish the causality of our findings. To the best of our knowledge, these two-child limits in India are the first instance of a democratic country instituting a fertility ceiling policy for candidates aspiring to elected political office. This paper is, hence, the first to examine how households trade-off the chance to hold political office against the option of having more children. We also investigate the role of son preference in this trade-off by examining how fertility and sex selection responses to the policy differ based on the number of sons when it is implemented.

Our paper makes novel contributions to two distinct literatures: on the effects of leaders' characteristics on followers' behaviors and on determinants of fertility and sex ratios in high-son preference countries. Apart from the large literature on peer-effects, the socioeconomic characteristics of individuals in positions of authority (e.g., teachers, mothers, and religious leaders) have been shown to exert considerable influence on their followers' behaviors and outcomes (Fernandez et al. (2004), Bettinger and Long (2005), Olivetti et al. (2013), Bassi and Rasul (2014)). Beaman et al. (2012) find that greater exposure to female leaders reduces gender gaps in aspirations and educational attainment through a role-model effect. In a related set of papers, exposure to television and specific social content has been shown to affect viewers' fertility rates (e.g., Jensen and Oster (2009), Chong et al. (2012)). Our novel contribution is to show that fertility restrictions on local leaders affect their constituents' demographic outcomes.

The literature on the determinants of fertility in developing countries is vast. Our findings show that role-models and incentives for political office also affect fertility and sex ratios. Recent work has highlighted the causal relationship between fertility decline and rising sex ratios in societies like India where sons are preferred (Ebenstein (2010), Anukriti (2014), Jayachandran (2014)). We augment this literature by investigating a new source of fertility decline that has an unintended effect on sex ratios, similar to programs like the One Child Policy and *Devirupak*.

In recent policy and political discussions in India, similar limits have been proposed for state legislative assembly members as well as members of the national Parliament. To the extent that individuals might find it easier to aspire to becoming a local leader and the socioeconomic characteristics, especially fertility, of local politicians might be more salient due to greater visibility, policies that affect local officials might be more effective than limits on leaders situated in state or national capitals. Moreover, fertility-related policies often ignore son preference. Given that sex-selective abortions are illegal in India, a biased sex ratio at birth among local leaders can also have a negative role-model effect, as shown by our sex ratio results, highlighting the need for more careful policy design.

The remainder of the paper is organized as follows. Section 2 discusses the two-child limits in detail. Sections 3 and 4 describe our data and empirical strategy. Section 5 presents the results and Section 7 concludes.

2 Background

India is the world's second most populous country and houses a third of the world's poorest 1.2 billion citizens (Olinto et al. (2013)). Consequently, fertility reduction continues to be atop its policy agenda. Based on the recommendations of the Committee on Population set up by the National Development Council (NDC) in 1992, several Indian states have enacted legislations that disbar individuals with more than two children from contesting local elections. The rationale behind these laws is that two-child norms for elected representatives will decrease fertility among their constituents through a role-model effect. In addition, they incentivize individuals who intend to run for elections in the future to plan smaller families.

In most states, the two-child limits have been enacted for elections to rural *Panchayats*, however, a few states have also imposed these norms on urban municipalities. India has a three-tiered decentralized system of local governance in rural areas, known as the Panchayati Raj. It comprises village-level councils (*Gram Panchayat*), block-level councils (*Panchayat Samiti*), and district-level councils (*Zila Parishad*). Although the Panchayat system has existed in several Indian states since the 1950s, it was granted constitutional status in 1992 through the 73rd Amendment of the Indian Constitution (The Panchayati Raj (PR) Act). Since then, regular Panchayat elections have taken place in most states. These elected local councils receive funds from the national and the state governments and are authorized to plan and implement developmental schemes as well as to levy and collect taxes. The Act requires that at least one-third of all member and chief positions are reserved for women. Similarly, positions are reserved for Scheduled Castes (SC) and Scheduled Tribes (ST) in proportion to their share in the village, block, or district population. Reservations for these groups are implemented in a stratified manner—among positions or seats reserved for SC, ST, and “general” castes, one-third are randomly chosen for women.

Table 1 presents the timeline for the enactment and implementation of the two-child laws across Indian states[1] and Table A.1 shows the local election years for which they were effective. Rajasthan was the first state to introduce such a law in 1992[2] and provided for a one year grace-period—any births during the grace-period were not counted towards the two-child limit. A candidate who had two or more children at the start of the cut-off and had an additional child after the grace-period cut-off date was disqualified. However, no elections were held under this amended law. The two-child norm was then included in Rajasthan’s 1994 PR Act which stipulated that anyone who had a third or higher-order birth after April 1994 would be ineligible to contest elections. Due to popular pressure, a grace-period was provided whereby births during April 23, 1994 - November 27, 1995 were not counted towards the two-child cut-off. As a result, the law came into effect after Rajasthan’s first post-73rd Amendment Panchayat election (that took place in 1995). A similar law was also passed for municipal elections in urban areas.

In Haryana, the law was announced through the PR Act in 1994 with a one-year grace period (until April 24, 1995). However, the first Panchayat elections had already taken place in 1994 and since members of the local councils are elected for a period of five years, no one was disqualified during 1995-2000. The Haryana government revoked this law in July 2006 and the repeal came into effect retroactively from January 1, 2005.

Andhra Pradesh (AP) introduced the fertility limit in its 1994 PR Act and also provided a one-year grace period. Orissa announced the law first for its district councils in November 1993 and then for the village and block councils in April 1994.[3] Himachal Pradesh (HP),

[1]This information is largely based on Buch (2005) and Buch (2006).

[2]Rajasthan’s law predates the NDC recommendations.

[3]Additionally, in Orissa, an individual who cannot read and write Oriya or who has more than one living spouse is also disqualified. The illiteracy criterion is not applicable to the village council elections.

Madhya Pradesh (MP), and Chhattisgarh[4] introduced their laws in 2000 and, like Haryana, repealed them in 2005. Maharashtra adopted the norm in 2003 with retrospective effect from September 21, 2002. Lastly, Bihar and Uttarakhand have adopted the law for municipal elections, but not for Panchayat elections.

Although their formulation is quite similar across states, the two-child laws are ambiguous in some cases. For example, the laws in Haryana and MP explicitly mention two *living* children, whereas in AP, Orissa, and Rajasthan the clauses do not distinguish between births and living children. In Rajasthan, twins are considered as one birth and a still-birth is not counted as a birth, while in MP the District Collector has discretion over disqualification in these events. However, children given up for adoption are counted towards the two-child limit for disqualification in all states. In most states, for a disqualification, a complaint has to be filed with the appropriate adjudicating authority, except in Orissa (for village councils) and MP where the competent authority can initiate action on its own.

Thus, starting in 1992, eleven Indian states have imposed a two-child limit on their local politicians for at least a few years—four states revoked them in 2005,[5] but they remain in effect in seven major states. In all cases, a one year grace-period was provided, i.e., births during one year after the announcement of the law were not counted towards the limit.

3 Data

We utilize repeated cross-sectional data from three rounds of the National Family Health Survey (NFHS-1, 2, 3) and one round of the District-Level Household Survey (DLHS-2) of India.[6] Each survey-round is representative at the state-level and includes a complete retrospective birth history for every woman interviewed, containing information on the month

[4] Chhattisgarh inherited the law when it was carved out of MP in 2000. Since 2004, candidates below 30 years of age in Chhattisgarh are also required to be literate.

[5] The central government and the Union Ministry of Panchayati Raj encouraged these revocations (`http://policydialogue.org/files/events/Aiyar_Key_Role_of_Panchayati_Raj_in_India.pdf`).

[6] The years of survey are 1992-93, 1998-99, and 2005-06 for the NFHS and 2002-04 for the DLHS.

and the year of child's birth, birth order, and mother's age at birth. We combine these birth histories to construct an unbalanced woman-year panel;[7] a woman enters the panel in her year of first marriage and exits in her year of survey.

For consistency across rounds, we limit the sample to currently-married women in the 15-44 age-group at the time of survey.[8] We also drop women (i) who were married more than 20 years prior to the survey to avoid issues related to imperfect recall, (ii) whose husband's age was below 15 or above 80 in the year of survey, and (iii) who have given birth to more than ten children, to prevent any composition-bias since these women are likely to be fundamentally different from rest of the sample. Lastly, we exclude mothers who have had twins since multiple births in our context are largely unplanned and do not reflect parents' fertility preferences.[9] However, our results are not driven by any of these selection criteria.

Our final sample comprises 511,542 women and 1,261,711 births from 18 major states[10] and covers the time period 1973-2006. As discussed earlier, the two-child laws were announced, enacted, and became effective (during an election) over several years. Moreover, a one-year grace-period was provided in all instances. To err on the side of caution, we define treatment based on the year of announcement, i.e., the earliest and the most conservative year when the law might have had an effect. Since the most recent year in our sample is 2006, we cannot credibly examine the effect of revocations that took place in 2005. However, we have a large number of post-announcement years, ranging from 4 to 13 years, to estimate the relatively long-term effect of the fertility limits.

[7] The DLHS and the NFHS are similar in terms of the selection of respondents, the conduct of interviews, and the questionnaires used. Sample sizes, however, are much larger for the DLHS since it is also representative at the district-level. As shown in Section 6, our results do not change if only one of these datasets is used.

[8] Survey questionnaires were administered to 13-49-year old ever-married women in NFHS-1, 15-49-year old ever-married women in NFHS-2,3, and 15-44-year old currently-married women in DLHS-2.

[9] Additionally, we drop women who were visiting the household when the survey took place, and were interviewed as a result, since there is no information on their actual state of residence.

[10] The states of Uttarakhand, Jharkhand, and Chhattisgarh were, respectively, carved out from Uttar Pradesh (UP), Bihar, and MP in 2000. Since our data does not include districts-identifiers for all rounds, we subsume these three new states into their parent states for our analyses.

Table 3 displays the years we use for defining the pre- and post-treatment period for each affected state. Table 4 presents the sample means and standard deviations for the key variables used in our analyses, separately for never-treated and treated states. We further split the treated sample into pre- and post-treatment observations. About two-thirds of women in our sample live in a rural area. A majority of them are Hindus, with a larger share (90%) among treated relative to never-treated households (79%). In terms of caste-composition, upper-castes and other backward classes (OBC) comprise about 40% and 35% of the sample, while the rest belong to Scheduled Castes (SC) and Scheduled Tribes (ST). Educational attainment is low for women, with more than half the sample being uneducated; in comparison, 29% of the husbands are uneducated. In terms of our outcome variables, women in the post-treatment group are less likely to give birth and are more likely to have two children relative to women in the never-treated and pre-treatment sub-samples.

The sample means for the three groups in Table 3 are similar along most, if not all, socio-economic dimensions. Nevertheless, to ensure that our estimates are not confounded by any underlying differences between these samples, we control for religion, caste, standard of living, husband's and wife's years of schooling, and residence in an urban area in our regressions. To take into account state-specific factors, we include state fixed effects and also control for state-specific linear time trends. In addition, we conduct several robustness checks to establish that our estimates are measuring the causal effect of fertility limits.

4 Empirical Strategy

The goal of our empirical analyses is to estimate the causal effect of the two-child limits imposed on local politicians in a state on fertility-related outcomes among residents in the same state. To do so, we utilize the quasi-experimental geographical and temporal variation in the announcement of these laws across Indian states. Although eleven states have enacted such a law thus far, due to data limitations we can estimate its impact for only eight states: Rajasthan, Haryana, AP, Orissa, HP, MP, Chhattisgarh, and Maharashtra. The law came into effect in Bihar and Gujarat after 2006, so in our sample these states are not treated. Although Uttarakhand announced its law for urban municipal elections in 2002, our analysis excludes it from the group of treatment states because Uttarakhand was a part of Uttar Pradesh until 2000 and we cannot distinguish between the two in the pre-2000 sample.[11] Our results, however, are robust to the exclusion of Uttar Pradesh. In addition to Bihar, Gujarat, and Uttarakhand, our control group comprises nine other states. Figure 1 depicts the treatment and control states in a map.

If the two-child laws are effective, we expect to observe changes in the probability of third parity births for couples who already have two children when the law is announced. To examine if this is the case, we estimate the following differences-in-differences type regression specification for a woman i of age a in state s and year t:

$$Y_{isat} = \alpha + \beta_1 Treat_{st} + X_i'\delta + \gamma_s + \theta_t + \psi_a + \nu_s * t + \epsilon_{isat} \tag{1}$$

where $Treat_{st}$ is equal to one for women residing in the treated states if $t >$ the year of announcement, and zero otherwise; γ_s, θ_t, and ψ_a are fixed-effects for state, year, and woman's age. We also control for state-specific linear time trends ($\nu_s * t$) and the following covariates (X_i): five categories each for a woman's and her husband's years of schooling, indicators for the religion (five categories), caste (four categories), and the standard of living (three categories) of the household, residence in an urban area, and indicators for the year of interview. We restrict the sample to women whose first two children are born before the treatment is announced in their state. The key coefficient of interest is β_1, which measures the effect of two-child limits on our outcomes variable which is an indicator for a third birth.

It is likely that the two-child laws also affect second parity births for couples who have one child at announcement. For example, if son preference is strong, women who have one daughter when the law is announced may be more likely to practice sex-selection at second

[11]Note that Uttar Pradesh has never enacted a two-child limit for its local politicians.

parity due to the two-child limit. Therefore, we also estimate a triple differences-in-differences version of (1) by interacting $Treat_{st}$ with an indicator ($Girl_i$) for whether the first child (born before treatment) is a girl:

$$\begin{aligned} Y_{isat} = \alpha + \beta_2 Treat_{st} * Girl_i + \phi Treat_{st} + \omega Girl_i \\ + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \nu_s * t + \tau_s * Girl_i + \epsilon_{isat} \end{aligned} \tag{2}$$

The outcome variables are indicators for a second birth and, conditional on birth, the likelihood that the child is male. The coefficient ϕ estimates the effect of the two-child laws on couples whose firstborn is a boy while β_2 estimates the differential effect on couples whose firstborn is a girl. Prior literature on India has shown that, despite the availability of prenatal sex-determination technology, sex of the first birth is plausibly random (Bhalotra and Cochrane (2010)) and most instances of sex-selection occur for higher-order births. However, Anukriti (2014) finds that this is not true for first births in Haryana after 2002 when firstborn children are *more* likely to be male due to the *Devirupak* scheme. Therefore, we drop post-2002 observations for Haryana from our sample while estimating (2). In addition, we restrict the sample to women whose first child is born before the year of treatment.

The inclusion of state and year fixed effects controls for any time-invariant state-level variables and state-invariant overall time trends that might affect fertility outcomes. Moreover, state-specific time trends account for differential linear trends in fertility patterns across states over the time period of analysis. We cluster standard errors at the state level when both treated and never-treated states are included in the sample. In specifications where the sample is restricted to only the treated states, we cluster at the state-year level to avoid econometric issues pertaining to a small number of clusters.

Our underlying identifying assumption is that the state-year variation in the timing of law announcement is uncorrelated with other time-varying determinants of the outcomes of interest. In addition to controlling for state-specific linear trends in our regressions, in the next section we show that there are no statistically significant differences in pre-treatment trends for our treatment and control groups. This supports our identifying assumption that the treatment and comparison women would have had similar trends in fertility rates in the absence of the two-child limits. Moreover, we show that the timing of announcement is uncorrelated with other socio-economic characteristics that vary by state and time.

5 Results

5.1 Graphical Evidence

Before discussing the regression results, we first present some graphical evidence for the effect of the two-child limits. In Figure 2 we use the event-study framework to depict the evolution of the likelihood that a woman has more than two living children in a given year. The plotted coefficients show the differential trends in the likelihood of having more than two living children for women in treatment and control groups, after controlling for socio-economic characteristics of the woman and fixed effects for state, year, and woman's age.[12]

There are no noticeable trends in the differential likelihood of having more than two children in the pre-treatment years. This lack of significant differences in the years prior to the two-child limits provides an important test for the validity of our identifying assumption; the trends in outcomes across comparison groups evolve smoothly except through the change in incentives for births in the treatment year. After the two-child limits are announced, there is a sharp decrease in the probability that a woman reports having more than two living children. However, this decrease is not immediate. The delayed effect can be explained by the grace-period provision in the two-child laws that does not count births during one year after the announcement of the law towards the fertility limit.

We also examine the effects of the law separately for each treatment state by plotting the smoothed values of our outcome variable for years before and after the announcement.

[12] Specifically, Figure 2 plots the β_k coefficients from the following regression: $Y_{isat} = \sum_{k=-10}^{5} \beta_k Treat_{s,t+k} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \epsilon_{isat}$, where $Treat_{s,t+k}$ indicates k years from the announcement of the law in state s. The year of announcement is the omitted year.

Specifically, we perform kernel-weighted local polynomial smoothing using the Epanechnikov kernel and a bandwidth of 0.5. In Figures 3 and 4, the dependent variable is an indicator for a third or higher parity birth. AP, Haryana, Rajasthan, and Orissa show strong increasing trends in the likelihood of families having a third parity birth in the years leading up to the two-child law. In all four states, however, this trend reverses after the law is announced and the likelihood of third parity births declines sharply. MP shows no such trend before the law, but also shows a sharp decline in third parity births once it is announced. The effects are weaker for HP and Maharashtra, perhaps because the law was announced more recently in these states and they already displayed a declining trend in third births preceding the law.

5.2 Regression Results

In this section we present regression estimates for the causal effects of the two-child limits on (i) third parity births for women whose first two children were born before the laws were announced, and (ii) second births for women whose first child was born before the laws were announced.

5.2.1 Third births

Table 5 present estimation results for specification (1) to describe the effects of the two-child limits on the likelihood of a third birth. We restrict the sample to women whose first two children were born before the law was announced and to years after the second birth. Column (1) controls for state and year fixed effects. In Column (2), we include additional covariates that comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area, and state-specific linear time trends. Columns (3) and (4) restrict the sample to the treated states. In Columns (5)-(7), we use observations from all states but restrict the sample by the sex-composition of the first two births. The standard errors are clustered at the state-level except in Column (4) where the sample is restricted to the treated states and hence we cluster at the state-year level to avoid inference issues due to the small number of clusters.

The coefficients for $Treat_{st}$ are negative and significant in the first two columns implying that the two-child limits decreased higher-order fertility for couples who had already borne two children by the time the law was announced in their state. The coefficient in Column (2) translates into a 0.5 percentage point or a 5.5 percent decrease in the likelihood of a third birth from the baseline probability of 9 percent. The coefficients remain negative and significant when we restrict the sample to the treated states in Columns (3) and (4).

Next we examine if these effects vary by the sex-composition of pre-existing children when treatment began. To the extent that son preference is quite strong in some of the treated states, we expect couples who have one or two sons prior to the treatment to be more likely to stop childbearing relative to those without any sons. On the other hand, as reflected by the control group means, these women are more likely (than those without any sons) to stop childbearing even in the absence of the two-child limits and hence the incremental effect of the two-child limits may not be large. In addition, couples who have two female births to begin with perhaps have a weaker preference for sons and hence may be more likely to reduce fertility at the margin. Although all three coefficients in Columns (5)-(7) are negative, we do in fact find that the decrease in marginal fertility is significant only for women whose first two births were female.

5.2.2 Second births

In Table 6, we present results for the probability and sex of the second birth by sex of the first child. We restrict the sample to women whose first child was born before the law was announced and to years after the first birth. Column (1) shows that before the law is announced, a firstborn girl, relative to a firstborn boy, increases the probability of a second birth by 0.2 percentage points, reflecting parents' desire for at least one son. However, once the law is announced, there is a decrease in the likelihood of a second birth, with a larger decrease for those with a firstborn girl. Splitting the sample by the caste of the household in Columns (2) and (3) reveals that this effect is primarily driven by upper-caste families with a firstborn girl. While this decrease could imply a permanent decrease in fertility, it could also reflect a relative delay in second births due to greater sex-selection by women with firstborn girls. If the latter is true, we expect the sex ratio of second births to increase for families with a stronger preference for sons and a greater likelihood of contesting local elections. Thus, in Columns (4)-(6), we examine the effect of the two-child limits on the likelihood that the second child is male.

While the coefficients in the first two rows of Column (4) are positive, there is no significant effect of the two-child laws on sex-selection behavior in the overall sample. However, consistent with Columns (2) and (3), disaggregating the sample by caste reveals that the limits increase sex-selection for second births by 3 percentage points among upper-caste families if their first child is a girl. Lower-caste households, on the other hand, do not respond in the same manner, despite being 1.23 percentage points likelier to have male second-born children if their first child is a girl (relative to a boy) before the law is announced. This pattern of results suggests that the decrease in second parity births reflects a delay induced by greater sex-selection. Due to their dominant socioeconomic status, we expect upper-caste households to be more concerned about remaining eligible for village council leadership than lower-caste households. Moreover, prior literature suggests that they also have a stronger preference for sons. Consequently, if their first child is a girl, they increase sex-selection at second births to ensure that they at least have one son whilst not sacrificing future eligibility for political office.

6 Robustness

In this section we perform a few robustness checks to ensure that our previous results truly capture the causal effect of fertility limits on politicians. First, we conduct a placebo test by reassigning the intervention or treatment to a year before the actual law was announced. If our results are truly capturing the causal effect of the two-child laws, we should not find significant effects in these placebo regressions. Table 7 presents the results from these regressions. Each column uses a different year as a placebo treatment year. For example, in Column (1), we assume that the two-child laws were announced in all treatment states in 1980. Since these laws are fictitious, a significant "effect" at the 5% level may be found roughly 5% of the time. There is no cell where we find a significant effect in the same direction as our main results in Table 5. These findings lend support to our differences-in-differences estimation strategy and make a causal interpretation more credible.

Next we examine the effect of these laws on the likelihood of fourth births for couples whose first three children were born before the announcements. If our findings are indeed causal, we should not find a significant effect since the two-child limits are irrelevant for couples who had already given birth to three children prior to the treatment. The coefficient in Column (1) of Table 8 is insignificant, further suggesting that our results are not driven by a general decline in marginal births for all parities. Column (2) shows that our results also remain robust when only NFHS data is used, thereby addressing concerns about the bias introduced by any unobserved differences in data collection, or small variations in the sampling methodology for NFHS and DLHS.

One potential mechanism through which these laws can affect fertility outcomes is through adjustments in the age at marriage. Forward-looking individuals (or their parents) wishing to maintain eligibility for local elections in the future may take into account the lower completed fertility requirements (i.e., a maximum of two children) and delay marriage, which could explain the decrease in likelihood of birth we observe in Section 5. To test if this is the case, we estimate specification (1) with age at first marriage as the dependent variable. The results are presented in Column (3) of Table 8 and show that there is no impact of the two-child limits on the age at first marriage.

Although we control for a number of socio-economic variables in our regressions, to further support our identification strategy, we show that the timing of announcement of these laws across states is uncorrelated with changes in these characteristics that vary across states and over time. Specifically, in Table 9 we present the coefficients from regressions that use various maternal, paternal, and household characteristics as dependent variables in the estimation of equation (1) with state and year fixed effects, and state-specific time trends, but without any other controls. Out of 20 coefficients, the only marginally significant coefficient is a negative effect on the likelihood of the mother being Hindu.

7 Conclusion

This paper examines whether demographic characteristics of locally elected representatives affect their constituents' fertility and sex ratio outcomes. To do so, we utilize quasi-experimental variation in the enactment of two-child eligibility requirements for individuals running for office in India. Our results show that fertility limits on local officials successfully lower fertility among the general populace, but also lead to an unintended increase in the already male-biased sex ratio in certain socioeconomic groups. We highlight a new channel of demographic influence, namely local politicians.

Our results thus far can be explained by the role-model effect as well as the incentive effect for individuals aspiring to run for office in the future. The data from NFHS and DLHS do not allow us to distinguish between these two channels. In ongoing work, we seek to exploit variation in the gender- and caste-based reservation status of village councils as an exogenous shock to the "attainability" of these leadership positions. Moreover, we plan to use data from the Rural Economic and Demographic Survey (REDS) to examine heterogeneity in the effects of these laws by the presence of a family member who has contested or been elected to a local council in the past.

Lastly, to the extent that women and low-caste households might have relatively less control over their fertility decisions, these laws may have unintended consequences for the political representation of socioeconomically disadvantaged groups who have relatively higher fertility. Therefore, in future work, we would also like to examine the interactions between these fertility limits and caste- and gender-based reservations in terms of their effects on the characteristics of elected candidates.

References

Anukriti, S. (2014): "The Fertility-Sex Ratio Trade-off: Unintended Consequences of Financial Incentives," *IZA Discussion Paper No. 8044*.

Bassi, V. and I. Rasul (2014): "Persuasion: A Case Study of Papal Influence on Fertility Preferences and Behavior," .

Beaman, L., E. Duflo, R. Pande, and P. Topalova (2012): "Female Leadership Raises Aspirations and Educational Attainment for Girls: A Policy Experiment in India," *Science*, 335, 581–586.

Bettinger, E. P. and B. T. Long (2005): "Do Faculty Serve as Role Models? The Impact of Instructor Gender on Female Students Do Faculty Serve as Role Models? The Impact of Instructor Gender on Female Students," *American Economic Review*, 95, 152–7.

Bhalotra, S. and T. Cochrane (2010): "Where Have All the Young Girls Gone? Identification of Sex Selection in India," *IZA Discussion Paper No. 5381*.

Buch, N. (2005): "Law of Two-Child Norm in Panchayats: Implications, Consequences and Experiences," *Economic and Political Weekly*, XL.

——— (2006): *The Law of Two Child Norm in Panchayats*, Concept Publishing Company.

Chong, A., S. Duryea, and E. La Ferrara (2012): "Soap Operas and Fertility: Evidence from Brazil," *American Economic Journal: Applied Economics*, 4, 1–31.

Ebenstein, A. (2010): "The "Missing" Girls of China and the Unintended Consequences of the One Child Policy," *Journal of Human Resources*, 45, 87–115.

Fernandez, R., A. Fogli, and C. Olivetti (2004): "Mothers and Sons: Preference Formation and Female Labor Force Dynamics," *Quarterly Journal of Economics*, 119, 1249–99.

Jayachandran, S. (2014): "Fertility Decline and Missing Women," *NBER Working Paper 20272*.

Jensen, R. and E. Oster (2009): "The Power of TV: Cable Television and Women's Status in India," *Quarterly Journal of Economics*, 124, 1057–94.

Olinto, P., K. Beegle, C. Sobrado, and H. Uematsu (2013): "The State of the Poor: Where are the Poor, Where is Extreme Poverty Harder to End, and What is the Current Profile of the World's Poor?" *Economic Premise*.

Olivetti, C., E. Patacchini, and Y. Zenou (2013): "Mothers, Friends and Gender Identity," *NBER Working Paper 19610*.

Visaria, L., A. Acharya, and F. Raj (2006): "Two-Child Norm: Victimising the Vulnerable?" *Economic and Political Weekly*, XLI.

8 Figures

Figure 1: Treatment and Control States

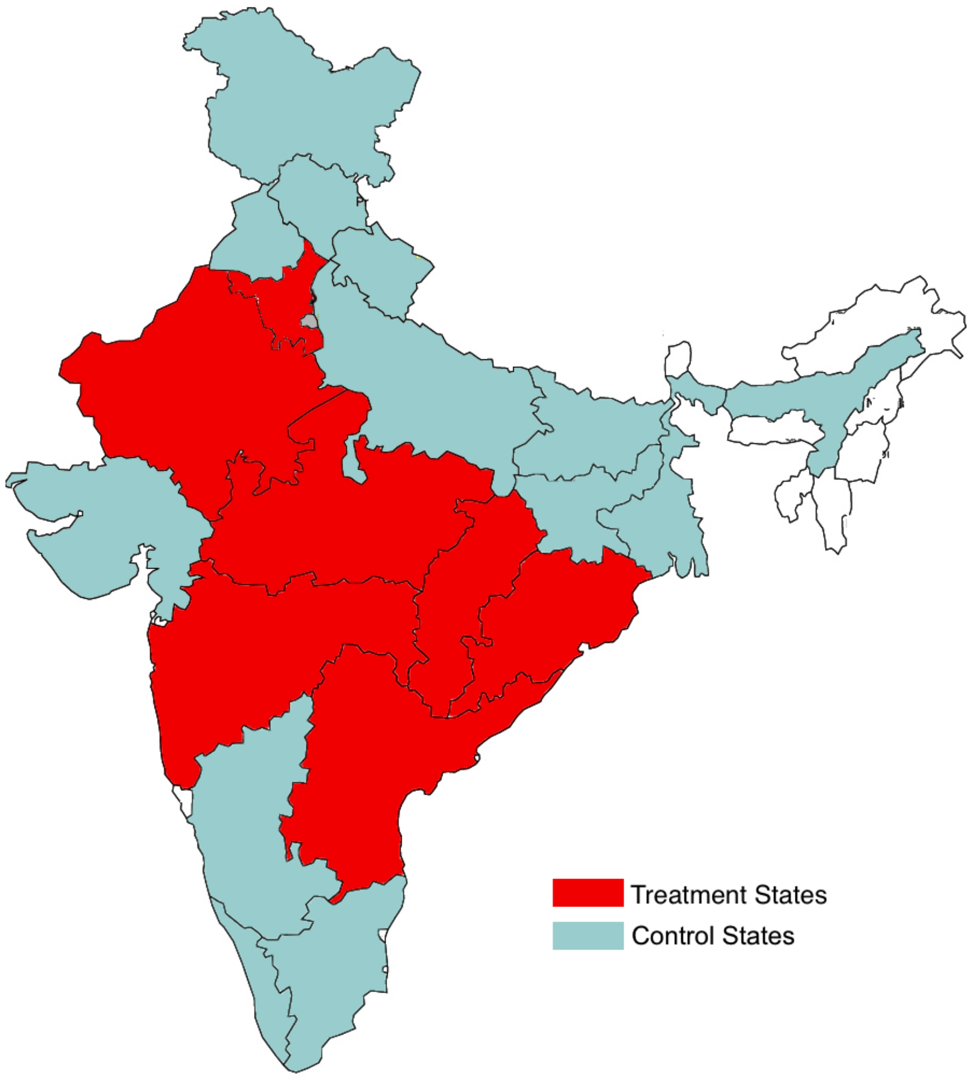
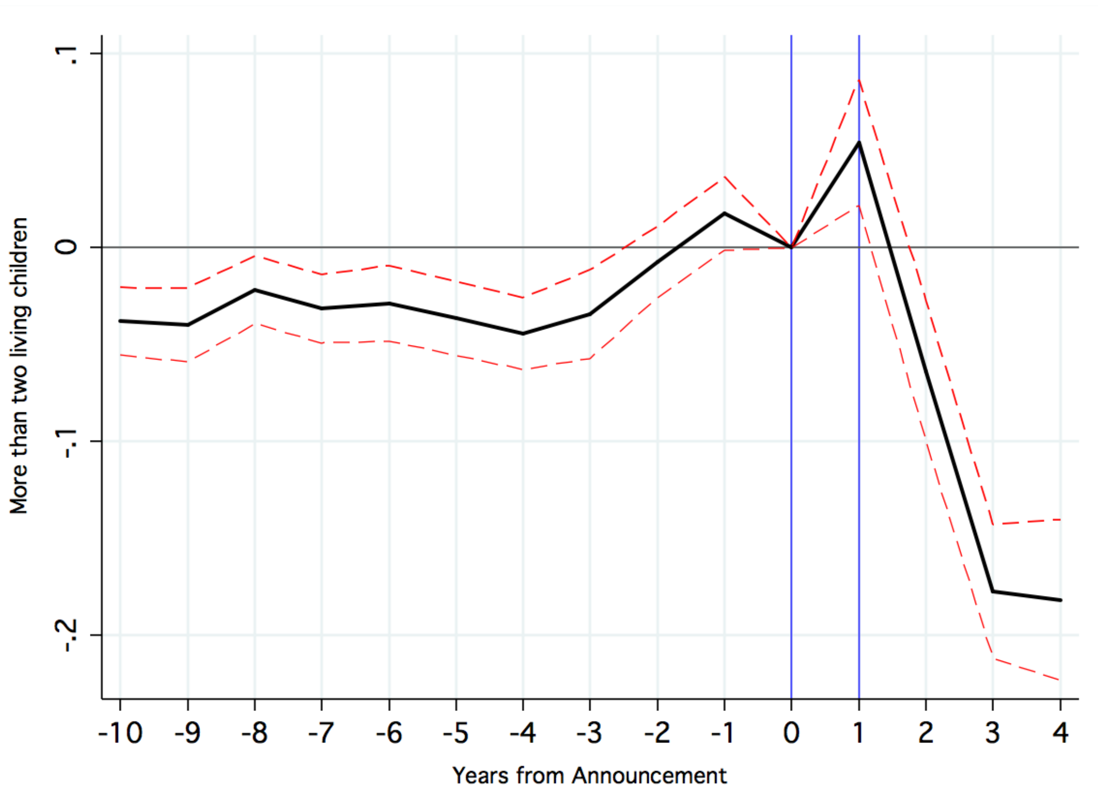
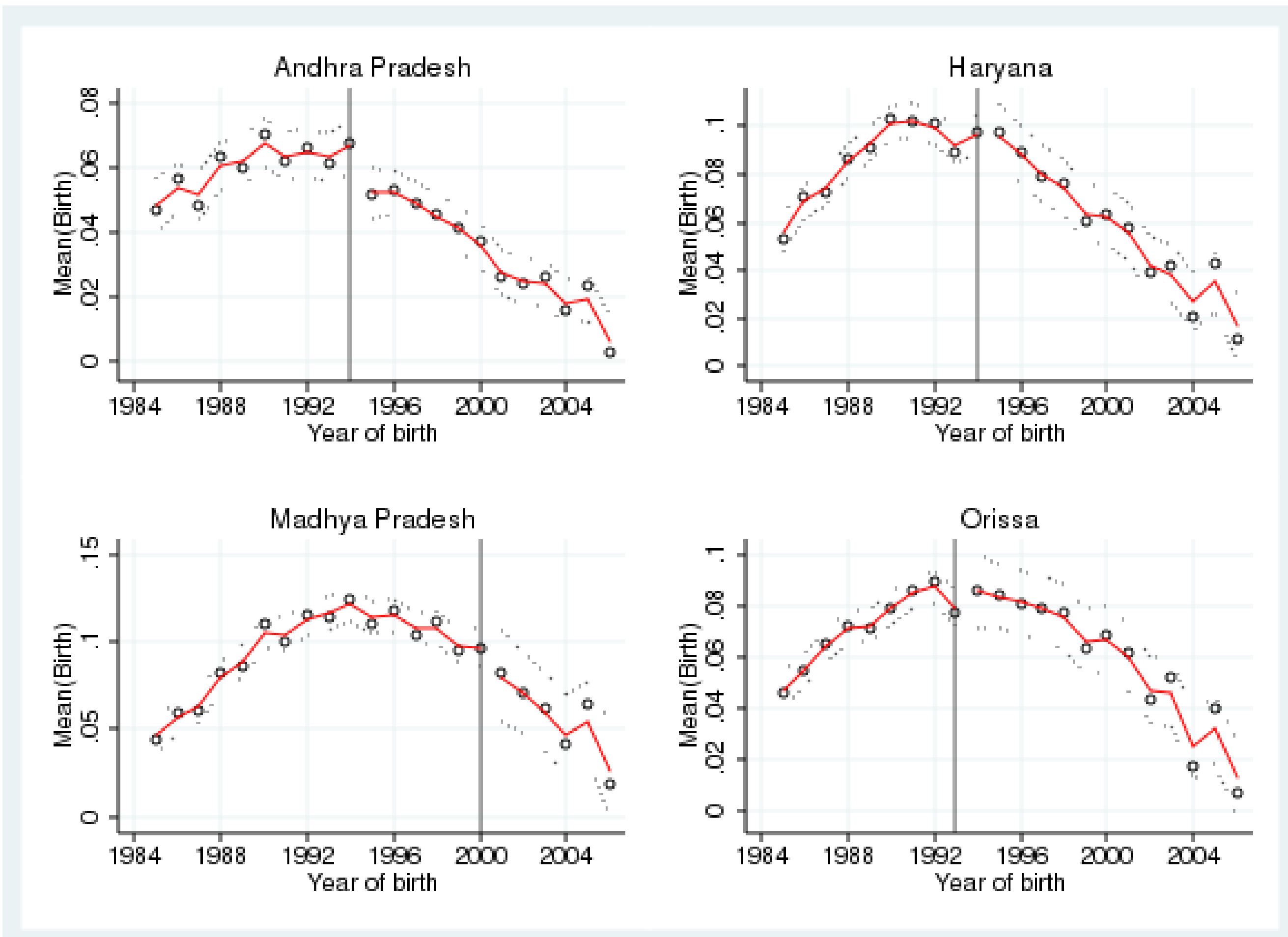


Figure 2: Likelihood of more than two living children, by year



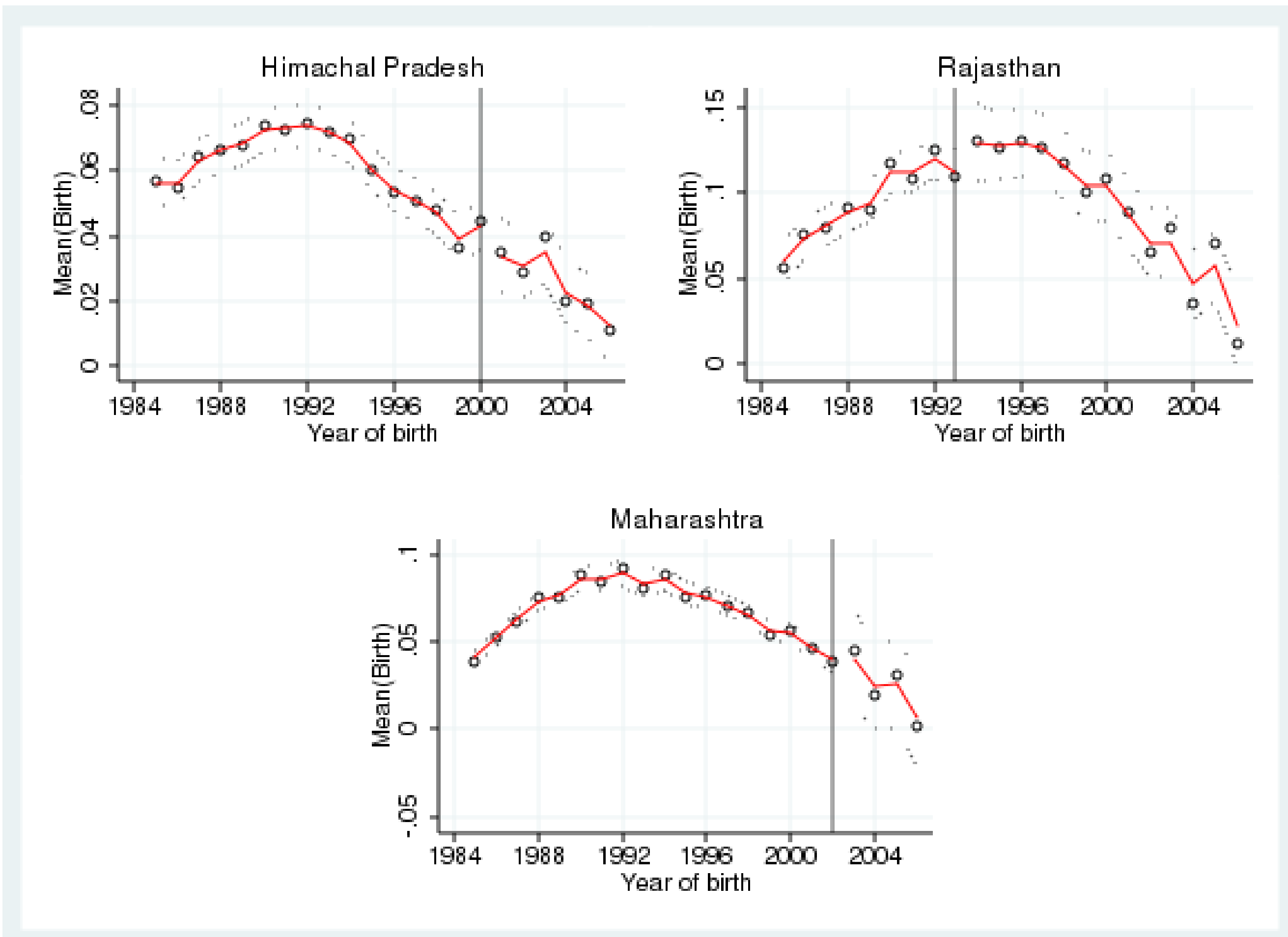
NOTES: This figure plots the β_k coefficients and their 95% confidence intervals (dashed lines) from estimating the following equation:
$Y_{isat} = \sum_{k=-10}^{4} \beta_k Treat_{s,t+k} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \epsilon_{isat}$, where $Treat_{s,t+k}$ indicates k years from the announcement of the law in state s. Standard errors are clustered by state-year. The first vertical line (at $k = 0$) indicates the year of announcement. The second vertical line indicates the end of the one-year grace period. The sample is restricted to women in treatment states.

Figure 3: Likelihood of third or higher order births, by state



NOTES: This figure plots the smoothed values of the outcome variable (an indicator for third or higher order birth) for years before and after the announcement of the law using kernel-weighted local polynomial smoothing with an Epanechnikov kernel and a bandwidth of 0.5. The dashed lines indicate the 95% confidence intervals.

Figure 4: Likelihood of third or higher order births, by state



NOTES: This figure plots the smoothed values of the outcome variable (an indicator for third or higher order birth) for years before and after the announcement of the law using kernel-weighted local polynomial smoothing with an Epanechnikov kernel and a bandwidth of 0.5. The dashed lines indicate the 95% confidence intervals.

9 Tables

Table 1: Timeline for Two-Child Norms across States

State	Announced	Grace Period	In effect	End
Rajasthan	1992	Apr 23, 1994 - Nov 27, 1995	Nov 27, 1995 -	
Haryana	1994	Apr 21, 1994 - Apr 24, 1995	Apr 25, 1995 - Dec 31, 2004	Jul 21, 2006 (retro. impl. Jan 1, 2005)
Andhra Pradesh	1994	May 30, 1994 - May 30, 1995	Jun 1995 -	
Orissa	1993/1994[13]	Apr 1994 - Apr 21, 1995	Apr 22, 1995 -	
Himachal Pradesh	2000	Apr 18, 2000 - Apr 18, 2001	Apr 2001 - Apr 2005	Apr 5, 2005
Madhya Pradesh	2000[14]	Mar 29, 2000 - Jan 26, 2001	Jan 2001 - Nov 2005	Nov 20, 2005
Chhattisgarh	2000	2000 - Jan 2001	Jan 2001- 2005	2005 (earliest mention)[13]
Maharashtra	2003[15]	Sep 21, 2002 - Sep 20, 2003	Sep 2003 -	
Uttarakhand (municipal only)	2002			
Gujarat	2005	Aug 2005 - Aug 11, 2006	Aug 11, 2006 -	
Bihar (municipal only)	Jan 2007	Feb 1, 2007 - Feb 1, 2008	Feb 1, 2008 -	

[13] For district councils in 1993 and for village and block councils in 1994.

[14] Notified on May 31, 2000. This created problems since people whose third child was born in Jan 2001 contested their disqualification for birth within 8 months of the new law.

[15] In retrospective effect from Sep 21, 2002.

Table 2: Panchayat members disqualified during 2000-04, selected states

State	**Number of disqualifications**
Haryana	1,342
Rajasthan	548
Madhya Pradesh	862
Andhra Pradesh	94*

SOURCE: Visaria et al. (2006). *Data available for 15 out of 23 districts.

Table 3: Treatment years, by state

State	$Treat_{st} = 1$ **if year** $>$
Rajasthan	1993
Orissa	1993
Haryana	1994
Andhra Pradesh	1994
Himachal Pradesh	2000
Madhya Pradesh (inc. Chhattisgarh)	2000
Maharashtra	2002

Table 4: Summary Statistics

Variable	Never treated		Treated			
			Post = 0		*Post* = 1	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)
Urban	0.343	0.475	0.329	0.470	0.320	0.466
Hindu	0.786	0.410	0.897	0.304	0.898	0.303
Muslim	0.161	0.367	0.066	0.249	0.063	0.243
Sikh	0.041	0.198	0.010	0.100	0.013	0.113
Christian	0.027	0.162	0.011	0.103	0.014	0.117
SC	0.180	0.384	0.160	0.367	0.177	0.382
ST	0.062	0.240	0.149	0.356	0.134	0.341
OBC	0.365	0.481	0.298	0.457	0.374	0.484
Wife's years of schooling:						
Zero	0.514	0.500	0.563	0.496	0.544	0.498
5-10 years	0.244	0.429	0.229	0.420	0.235	0.424
10-12 years	0.091	0.287	0.074	0.261	0.082	0.275
12-15 years	0.048	0.214	0.031	0.173	0.039	0.193
$\geq$ 15 years	0.045	0.207	0.037	0.188	0.046	0.209
Husband's years of schooling:						
Zero	0.278	0.448	0.291	0.454	0.289	0.453
5-10 years	0.301	0.459	0.309	0.462	0.310	0.462
10-12 years	0.153	0.360	0.149	0.357	0.149	0.356
12-15 years	0.093	0.290	0.070	0.255	0.079	0.270
$\geq$ 15 years	0.096	0.294	0.089	0.285	0.101	0.302
Low SLI	0.446	0.497	0.460	0.498	0.425	0.494
High SLI	0.242	0.428	0.233	0.423	0.250	0.433
Mother's age at birth	24.853	6.163	23.008	5.474	26.507	6.341
Birth = 1	0.213	0.410	0.239	0.426	0.161	0.367
Birth is male	0.111	0.315	0.124	0.330	0.085	0.278
Has 2 children	0.260	0.438	0.234	0.423	0.287	0.442
N	3,568,675		1,458,849		941,801	

NOTES: *Post* is defined using the year of announcement of the law (see Table 3). SC, ST, and OBC indicate Scheduled Caste, Scheduled Tribe, and Other Backward Class households, respectively. Low and High SLI are equal to one if the household belongs to the bottom-third or the top-third of household wealth distribution.

Table 5: Effects on Third Births

			Only treated states		BB	BG	GG
Dep Var: **3rd birth = 1**	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Treat_{st}$	-0.0200***	-0.0049*	-0.0068**	-0.0068***	-0.0033	-0.0050	-0.0069**
	[0.0054]	[0.0028]	[0.0024]	[0.0022]	[0.0023]	[0.0032]	[0.0032]
N	2,899,022	2,880,757	1,059,213	1,059,213	773,470	1,442,666	664,621
Control group mean	0.080	0.080	0.098	0.098	0.072	0.078	0.091
Year FE	x	x	x	x	x	x	x
State FE	x	x	x	x	x	x	x
Covariates		x	x	x	x	x	x
State-specific linear trends		x	x	x	x	x	x
Clustering	State	State	State	State-Year	State	State	State
N (clusters)	18	18	18	224	18	18	18

NOTES: This table reports the coefficients of $Treat_{st}$ from specification (1). Each coefficient is from a separate regression. The dependent variable is one if there is a third birth in a given year, and zero otherwise. The sample is restricted to women whose first two children were born before the law was announced in her state. Only years after the second birth are included. Other covariates comprise indicators for the year of survey, woman's age, household's religion (Hindu, Muslim, Sikh, Christian), caste (SC, ST, OBC), wealth (low and high SLI), husband's and wife's years of schooling (5 categories each), and residence in an urban area. In columns (3)-(4), the sample is restricted to women in treatment states. BB, BG, GG respectively indicate the sub-samples of women whose first two births were two boys, one boy-one girl, and two girls. *** 1%, ** 5%, * 10%.

Table 6: Effects on second birth, by first child's sex

	2nd birth = 1			**2nd birth is male**		
		Upper-caste	Lower-caste		Upper-caste	Lower-caste
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{st}$ * $First - born\ girl$	-0.0029**	-0.0030**	-0.0026	0.0073	0.0307***	-0.0017
	[0.0012]	[0.0013]	[0.0015]	[0.0064]	[0.0093]	[0.0061]
$Treat_{st}$	-0.0041*	-0.0044	-0.0037	0.0039	-0.0065	0.0054
	[0.0023]	[0.0028]	[0.0025]	[0.0050]	[0.0082]	[0.0051]
$First - born\ girl$	0.0024***	0.0023***	0.0023**	0.0103***	0.0070***	0.0123***
	[0.0007]	[0.0006]	[0.0009]	[0.0013]	[0.0016]	[0.0014]
N	4,088,203	1,587,439	2,500,764	329,905	126,712	203,193

NOTES: The sample is restricted to women whose first child was born before the law was announced in her state. The dependent variable is one if there is a second birth in a given year, and zero otherwise. Only years after the first birth are included. Columns (4)-(6) are conditional on a second birth. We drop post-2002 observations for Haryana. Each coefficient is from a separate regression that includes state-specific linear time trends, fixed effects for state, year, and the interaction between state indicators and first-born girl dummy. Standard errors are in brackets and are clustered by state. Covariates comprise indicators for the year of survey, woman's age, household's religion (Hindu, Muslim, Sikh, Christian), (except in columns 3 and 4) caste (SC, ST, OBC), wealth (low and high SLI), husband's and wife's years of schooling (5 categories each), and residence in an urban area. Lower-caste refers to SC, ST, OBC households; Upper-caste comprises the rest. *** 1%, ** 5%, * 10%.

Table 7: Placebo Test for Likelihood of Third Birth

	Placebo treatment year:							
↓ Dep var:	**1982**	**1983**	**1984**	**1985**	**1986**	**1987**	**1988**	**1989**
Third birth =1	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Treat_{st}$	0.013	0.011*	0.006	0.006	0.005	0.001	-0.002	-0.002
	[0.007]	[0.006]	[0.005]	[0.006]	[0.005]	[0.005]	[0.004]	[0.003]
N	2,880,757							

NOTES: Each coefficient is from a separate regression with a different placebo treatment year (same for all treated states). The dependent variable is one if there is a third birth in a given year, and zero otherwise. The sample is restricted to women whose first three children were born before the law was announced in her state. Only years after the third birth are included. Standard errors are in brackets and are clustered by state. Specifications are similar to column (3) in Table 5. *** 1%, ** 5%, * 10%.

Table 8: Robustness Checks

Dep Var →	**4th birth = 1**	**NFHS only**	**Age at first marriage**
	(1)	(2)	(3)
$Treat_{st}$	-0.0057	-0.006**	0.005
	[0.0033]	[0.002]	[0.336]
N	1,631,630	876,382	62,401
Year FE	x	x	x
State FE	x	x	x
Covariates	x	x	x
State-specific linear trends	x	x	x

NOTES: Each coefficient is from a separate regression. In Column (1), the specification is similar to that in Table 5. The dependent variable is one if there is a fourth birth in a given year, and zero otherwise. The sample is restricted to women whose first three children were born before the law was announced in her state. Only years after the third birth are included. In Column (2), the sample is restricted to NFHS data. In Column (3), the sample is restricted to one observation per woman. $Treat_{st}$ is equal to one if a woman's first marriage took place after the law was announced in her state, and zero otherwise. Standard errors are in brackets and are clustered by state. Other covariates comprise indicators for the year of survey, household's religion (Hindu, Muslim, Sikh, Christian), caste (SC, ST, OBC), wealth (low and high SLI), husband's and wife's years of schooling (5 categories each), and residence in an urban area. *** 1%, ** 5%, * 10%.

Table 9: Correlations between Law Announcements and Socioeconomic Variables

Dependent Variable	**Coefficient of** $Treat_{st}$ (1)	**Std. Error** (2)
Urban	0.007	[0.009]
SC	-0.003	[0.002]
ST	0.006	[0.005]
OBC	0.007	[0.006]
Hindu	-0.005*	[0.003]
Muslim	0.001	[0.002]
Sikh	0.0005	[0.001]
Christian	-0.001	[0.002]
Low SLI	-0.001	[0.005]
High SLI	0.002	[0.005]
Wife's years of schooling:		
Zero	-0.002	[0.003]
5-10 years	0.001	[0.003]
10-12 years	0.001	[0.002]
12-15 years	0.002	[0.002]
$\geq$ 15 years	-0.0001	[0.001]
Husband's years of schooling:		
Zero	-0.001	[0.002]
5-10 years	0.00009	[0.002]
10-12 years	0.001	[0.002]
12-15 years	0.002	[0.003]
$\geq$ 15 years	-0.001	[0.002]
N	5,969,325	

NOTES: Each coefficient is from a separate regression that includes state and year fixed effects and state-specific linear time trends. Standard errors are in brackets and are clustered by state. *** 1%, ** 5%, * 10%.

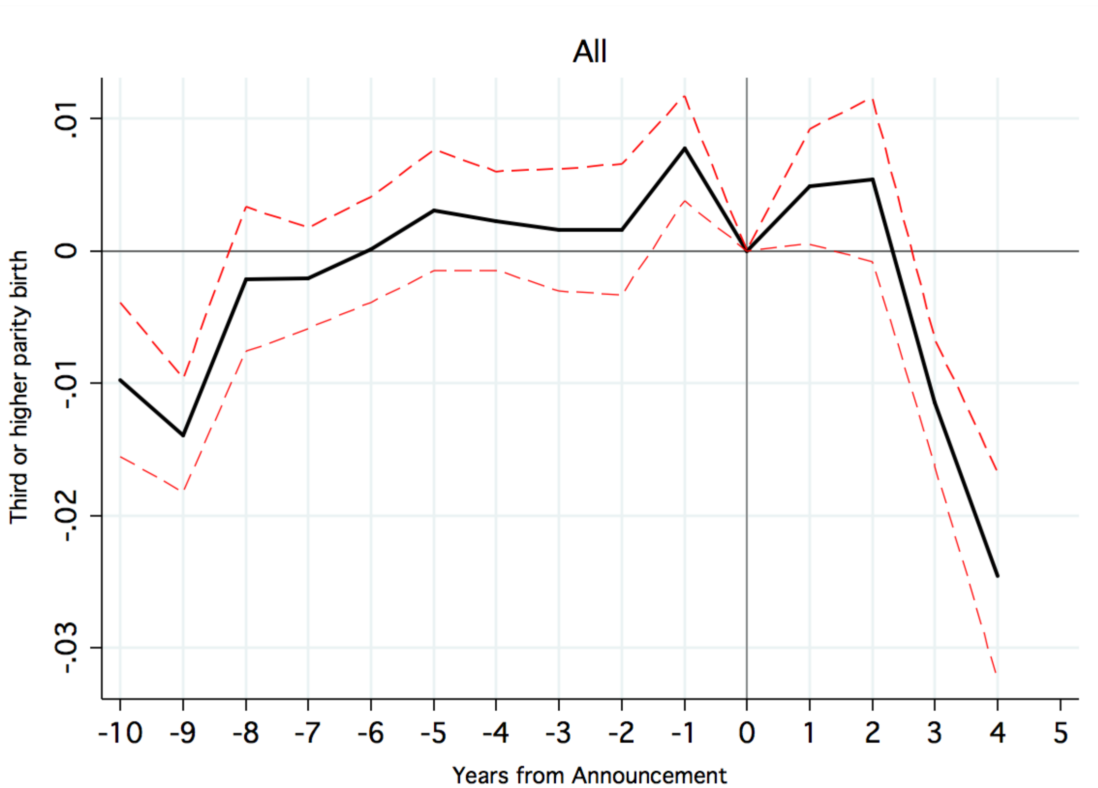
A Appendix

Table A.1: Panchayat Elections

State	**Election Years**	
	W/o the norm	With the norm
Rajasthan	1995	2000, 2005, 2010
Haryana	1994, 2010	2000, 2005 (?)
Andhra Pradesh		1995, 2001, 2006, 2011
Orissa		1997, 2002, 2007, 2012
Himachal Pradesh	1995, 2005, 2010-11	2000
Madhya Pradesh	1994, 2010	2000[16], 2005
Chhattisgarh	2010	2000, 2005
Maharashtra	1995, 2000	2007, 2010, 2013
Uttarakhand	2003, 2008, 2014	
Jharkhand	2010	
Gujarat	2001, 2005-06	2010-11
Bihar	2006	2011

[16] Despite the fact that the two-child norm was introduced after the panchayat elections were over in 2000, the new government started disqualifying elected representatives Visaria et al. (2006).

Figure A.1: Likelihood of third births, by year



NOTES: This figure plots the β_k coefficients and their 95% confidence intervals (dashed lines) from estimating the following equation:
$Y_{isat} = \sum_{k=-10}^{4} \beta_k Treat_{s,t+k} + X_i'\delta + \gamma_s + \theta_t + \psi_a + \epsilon_{isat}$, where $Treat_{s,t+k}$ indicates k years from the announcement of the law in state s. Standard errors are clustered by state-year. The vertical line (at $k = 0$) indicates the year of announcement. The sample is restricted to women in treatment states whose first two children were born before the law was announced in her state. Only years after the second birth are included.