Democracy and Demography: Societal Effects of Fertility Limits on Local Leaders*

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Abstract

We investigate whether restricting elected leadership positions to candidates with "desirable" characteristics leads to society-wide adoption of those characteristics in a low-income democracy with high political participation. Exploiting quasi-experimental variation in fertility limits imposed on village council members in India to curb population growth, we find that rural couples significantly decreased fertility in response. Socially disadvantaged families exhibit the greatest reduction in marginal fertility immediately following the passage of these limits, but the decline in the long-run "stock" of children is more evenly spread across socioeconomic groups. The limits however also increased the already male-biased sex ratio at birth in castes with strong son preference. Therefore, while restricting access to elected office may aid in achieving social objectives, it can also worsen inequality.

JEL Codes: J13, J16, H75, O11

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1 Introduction

Representative democracy promotes stable economic growth (Rodrik (2000), Mobarak (2005), Papaioannou and Siourounis (2008)), narrows large income inequalities (Acemoglu and Robinson (2000), Gradstein (2007)), and prevents elite capture of property rights and productive resources (Bardhan and Mookherjee (2000), Brown and Mobarak (2009), Foster and Rosenzweig (2004)), all of which are critical for welfare improvement in developing countries. The literature on political selection suggests that these positive effects of democracy may partly be due to the higher quality of leaders in democratic relative to autocratic systems (Besley and Coate (1997), Besley and Reynal-Querol (2011), Osborne and Slivinski (1996)) as voters in the former choose from a wider pool of candidates.[1] Good leaders also serve as role models and effect social change by raising aspirations.[2] Consequently perhaps, legal restrictions on who can become an elected leader in a democracy are usually minimal, concerning little beyond citizenship, age, and linguistic proficiency.

Despite the theoretically open access to the candidate pool in a democracy, in practice, political networks, campaign costs, and other socioeconomic inequities create substantial entry barriers for a significant share of citizens.[3] Moreover, democratic accountability may not work if voters have imperfect information on candidates' characteristics, or if they prefer to elect leaders who can provide patronage at the expense of other constituents.[4] Therefore, several countries have sought to improve candidate quality, and thus policy outcomes, by imposing "desirable" characteristics on candidates, such as minimum education levels and no criminal convictions.[5] However, little is known about the effects of these laws on the behavior of citizens. Ours is the first paper to provide causal estimates of the impact of one such law on citizens' outcomes, and to examine the implications for political representation and inequality when access to political leadership is restricted to achieve social objectives.

[1]Democratic leaders may also perform better than autocrats due to greater accountability (Acemoglu and Robinson (2006), Bidner and Francois (2013), Mueller and Stratmann (2003)).

[2]Individuals in positions of authority have been shown to exert influence on their followers' behaviors and outcomes (Bassi and Rasul (2014), Beaman et al. (2012), Bettinger and Long (2005), Chong et al. (2012), Jensen and Oster (2009), Olivetti et al. (2013)).

[3]Several countries have legislated political quotas to combat these barriers, such as reservation for women and lower castes in India (Bhalotra et al. (2013), Chattopadhyay and Duflo (2004), Kapoor and Ravi (2014)).

[4]Mookherjee (2014) discusses the limitations of elections as an accountability device in India. Banerjee et al. (2011) and Ferraz and Finan (2008) show that increased transparency can improve accountability.

[5]For instance, Angola, Azerbaijan, and Turkey set minimum education levels for Presidential candidates.

We examine the impact of a unique policy experiment in India, whereby several states legally restrict individuals with more than two children from contesting rural local government (*Panchayat*) elections, on fertility in the general population. This manipulation of the candidate pool aims to curb population growth, and is not intended to directly improve leaders' performance. Instead, these legislations seek to improve economic outcomes by precipitating fertility decline. The fertility limits also impose costs however, as they incentivize couples to deviate from their preferred fertility path and shrink the candidate pool. To the extent that individuals from lower socioeconomic strata have higher fertility and lower contraceptive access, the limits may increase inequality in political representation. Moreover, fertility decline may increase the male-bias in sex ratios in a high son preference society like India. The overall effectiveness of the two-child laws thus depends on the magnitude of welfare gains from fertility decline relative to these costs.

The rural local government system in India comprises village-, block-, and district-level councils that exercise considerable power in their constituencies. Starting in 1992, eleven states have enacted the fertility limits for at least some years and they remain in effect in seven major states. In all states, these laws provided a one-year grace-period from the time of announcement, during which an individual could have additional children and still remain eligible for election. However, for people with two or more children by the end of the grace-period, a subsequent birth leads to disqualification. Individuals with fewer than two children by the end of the grace-period are limited to at most two children afterwards to maintain eligibility.[6] We exploit the quasi-experimental geographical and temporal variation in announcement of these fertility limits across states to estimate their causal impacts on citizens' demographic outcomes in a difference-in-differences framework.

We find that among couples who had two children when the law was announced in their state, the hazard of third birth declined by 2.06 to 2.65 percentage points (p.p.), which is about 10 to 11 percent of the baseline hazard of third birth. These magnitudes are comparable to the effects of well-known fertility control programs such as the Matlab intervention in Bangladesh, which reduced child-to-mother ratios by 16 percent, and the combined Navrongo interventions in Ghana, which reduced children ever born to treated women by 9 percent (Canning and Schultz (2012)). There were no significant effects on

[6] The same rules apply for dismissal of an elected member who exceeds the fertility limit while in office.

marginal fertility of couples who had three or more children at the time the laws were announced, as these families had likely already achieved their desired family size.[7] We also examine the impact of the limits on the "stock" of fertility and find that the probability of having a total of three or more children in any given year declined by 1.33 p.p. or 4.42 percent and the probability of having only two children increased by 0.75 p.p. or 3.41 percent in enacting states. However, the fertility limits also adversely affected the already male-biased sex ratio among social groups with high son preference, thus increasing the number of missing girls. The human capital and income gains from fertility decline are therefore undermined by worsening gender inequality (Doepke et al. (2012)). Our estimates suggest that 1,230,780 individuals, or 1.33 percent of married rural couples of childbearing age in treatment states, responded to the fertility limits by restricting their fertility to two or fewer children.

There are at least three different channels that may underlie any causal effect of the two-child limits on statewide fertility. If elected representatives serve as role models, their constituents may be indirectly affected by these limits as they emulate their leaders' fertility choices ("role model channel"); in fact, this appears to be the primary mechanism the policymakers had in mind when these laws were enacted.[8] Societal fertility decline may also be driven by citizens' desire to maintain eligibility for future village council candidacy ("aspirations/ incentives channel").[9] Additionally, even if citizens do not have political ambitions, they may adjust fertility if the limits signal that similarly restrictive policies may be enacted for government jobs, for instance ("anticipation channel"). While we cannot credibly separate the aspirations, the role-model, and the anticipation channels as potential explanations for our results, at the minimum we show that policy interventions in the political sphere can have substantial impacts in a highly participatory democratic society.

The fertility limits potentially have crucial implications for the efficacy of democratic institutions in protecting the welfare of the socially disadvantaged, who may have higher fertility than elites due to lower access to contraception and higher risk of child mortality, and depend more on political representation to obtain resources prone to elite capture. On

[7]Average terminal fertility (as measured by fertility of women more than 40 years old) in enacting states before announcement of the law was 2.8.

[8]Source: `http://www.nytimes.com/2003/11/07/world/states-in-india-take-new-steps-to-limit-births.html`.

[9]For more on aspirations in developing countries, see Appadurai (2004), Mookherjee et al. (2010), Ray (2006), Genicot and Ray (2014), Dalton et al. (2014), and Bernard et al. (2012).

the other hand, low socioeconomic status families are more "treatable" by the limits as they have a higher baseline hazard of third birth relative to couples of higher socioeconomic status. We find that the declines in the hazard of third birth are concentrated in families with low socioeconomic status (i.e., couples with low wealth or no schooling), and from lower castes that rely on mandated reservations of village council seats for elected political representation. Moreover, the substitution away from more than two children and towards exactly two children, while not as precisely estimated, is similar between poorer, uneducated households and wealthier, educated households. This reduces concerns that public resources may become more prone to capture by local elites (Bardhan and Mookherjee (2000)) once the limits are in effect. Our results suggest that disadvantaged households are not only aware of the two-child laws but also respond in a manner that allows them to continue benefitting from mandated gender- and caste-based quotas for village council membership.[10]

Our paper makes novel contributions to the literature on family planning interventions, which can promote economic growth, human capital accumulation, and women's empowerment when effective (Miller (2010), Ashraf et al. (2013), Joshi and Schultz (2007), Rosenzweig and Zhang (2009)). We also contribute to the literature on determinants of sex ratios. Recent papers have highlighted the effect of fertility decline on rising sex ratios in societies like India where sons are preferred (Ebenstein (2010), Anukriti (2014), Jayachandran (2014)). We augment this literature by analyzing a new source of fertility decline and show that it too has an unintended effect on sex ratios.

The rest of the paper is organized as follows. Section 2 discusses the legislations in detail. Sections 3 and 4 describe the data and the empirical strategy, respectively. Section 5 presents the results; Section 6 describes some robustness checks; and Section 7 discusses the magnitudes of our estimates. Lastly, Section 8 concludes the paper.

2 Background

India is the world's second most populous country and houses a third of its poorest citizens (Olinto et al. (2013)). Consequently, population control remains a policy priority. Based on the recommendations of the 1992 Committee on Population, several Indian states have

[10] Beaman et al. (2012) and Chattopadhyay and Duflo (2004) show that quotas-based improvements in the representation of women and lower castes in village councils enhances targeting of public expenditure towards these groups and reduces discrimination against them.

enacted fertility limits for village council candidates,[11] seeking to lower fertility through the role-model channel and by conveying policymakers' seriousness about curbing population growth. Additionally, the fertility limits also incentivize individuals who intend to contest elections to plan smaller families.

India has a three-tiered 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*). Regular village council elections have taken place every five years in most states. The village councils are the building blocks of the Indian democratic system and exercise considerable power in their constituencies. They receive substantial funds from national and state governments,[12] and are authorized to implement development schemes.[13] Village councils are also responsible for providing public goods such as village roads, wells, and water-works. They can collect taxes and license fees, and receive seignorage from the auction of local mineral and forestry resources.

The typical monthly salary of a village council head is about USD 50 - USD 60 and other council members are paid less. While these official wages are not substantial, the potential private returns from political rents and corrupt practices may provide a strong incentive for becoming an officeholder. According to the Association for Democratic Reforms, an average candidate spends USD 400 - USD 800 during a village council election.[14] However, the benefits from even one term as a council member are likely to be much higher. The average declared wealth of re-contesting candidates (for elections to the Parliament and state legislative assemblies) in 2004 was 134 percent higher than their wealth during the first election (Sastry (2014)) suggesting high political rents and significant corruption. Fisman et al. (2014) show that the annual asset growth of winners in state elections is 3-5 p.p. higher than that of runners-up. Although similar statistics are not available for village council candidates, the returns from council membership and the resulting opportunity cost of violating the limits are likely to also be large.

[11] In fact, the Committee recommended these restrictions for all elected positions—from village councils to the national Parliament.

[12] For example, in Tamil Nadu, all councils received at least USD 4,900 in annual state grants in 2009-10, and 35% of them received funds in the range of USD 16,330-40,800. These are significant budgets considering that India's annual per capita income was USD 1,570 in 2013 (Source: The World Bank).

[13] Village councils are often authorized to identify local beneficiaries of major central and state development schemes, such as the National Rural Employment Guarantee Scheme.

[14] Source: `http://www.ndtv.com/india-news/the-rs-81-500-crore-lie-565175`

The average population per village council is about 3,100, although the size varies widely. The minimum age to contest elections is 21 years. There are no term limits on village council members; thus violation of fertility limits has lifetime consequences for an individual's candidacy. In Rajasthan and Uttar Pradesh, respectively, 19 percent and 33 percent of council chiefs were under 36 years old and 56 percent and 51 percent were in the 36-50 year age-group.[15] The council members are typically younger: 47 percent of village council members in 2012 in Rajasthan were under 36 years of age and 41 percent were in the 36-50 year age-group. The age-composition of council members suggests that the limits impact a large share of potential candidates of childbearing age.

The PR Act requires that at least one-third of all member and chief positions are reserved for women.[16] Similarly, positions are reserved for Scheduled Castes (SC) and Scheduled Tribes (ST) in proportion to their population share.[17] As lower-castes have higher fertility and lower contraceptive access, the limits may increase caste inequality in political representation and thus reverse the gains from affirmative action. To examine these heterogeneous impacts, we also present results separately for SC, ST, and Other Backward Class (OBC) families.[18]

Voter turnout in village elections routinely exceeds 70 percent. In the 2014 World Values Survey, 53 percent of the respondents (69 percent among the "lower class") say that politics is "very important" or "rather important" in their life and about 48 percent of the respondents are members of a political party.[19] Thus our results have significant implications for the understanding of the relationship between political participation and social change in low-income democracies.

Rajasthan was the first state to introduce the two-child limit for its village councils in 1992;[20] this requirement was later included in the state's 1994 PR Act.[21] Andhra Pradesh

[15] In West Bengal, the average age of chiefs was 36 years in 2000 (Chattopadhyay and Duflo (2004)) and in Andhra Pradesh it was 43 years in 2011 (Afridi et al. (2014)).

[16] In 14 states, half of all seats are reserved for women.

[17] Quotas are implemented in a stratified manner—among positions reserved for SC, ST, and "general" castes, one-third are randomly chosen for women.

[18] OBCs are non-SC/ST castes that have been identified by individual states or the central government as economically and socially disadvantaged, and therefore qualify for affirmative action.

[19] About 72 percent say that a democratic political system is a "very good" or "fairly good" way of governing the country. According to the 2005 India Human Development Survey, in 28 percent of households a member attended a public meeting called by the local council in the last year and in 10 percent of households someone from or close to the household is a member of the local council.

[20] Rajasthan's law predates the recommendations of the Committee on Population.

[21] The 1994 Act included a grace-period from April 23, 1994 to November 27, 1995. Effectively, this resulted

and Haryana announced their legislations in 1994,[22] although the latter revoked its law in 2006. Orissa announced the limit for its district councils in 1993 and for the village and block councils in 1994. Himachal Pradesh (HP), Madhya Pradesh (MP), and Chhattisgarh[23] introduced their laws in 2000 and repealed them in 2005. In Maharashtra, the law has been in retrospective effect since 2002. Lastly, Bihar and Uttarakhand adopted the limit respectively in 2002 and 2007, but only for municipal elections. Table 1 presents a more detailed timeline for the announcement, grace-period, and implementation of these laws[24] and Table A.2 shows the election years for which they were effective. The relevant clauses from each state's PR Act are presented in Appendix B.

Village council candidates do not have to explicitly state their number of children when filing the nomination papers. However, they have to declare that, to the best of their knowledge, they are qualified for the council seat.[25] Table A.1 shows the number of village council members that were disqualified under these laws in Haryana, Rajasthan, MP, and AP during 2000-2004.[26] Newspaper reports suggest that, in some instances, the fertility limits have led to abandonment of wives, selective abortion of female fetuses, and giving up of children for adoption to avoid disqualification. Consequently, implementing states have faced criticism from women's rights advocates and civil society organizations, as well as from the central government.[27] The revocation of the limits in four states may have been in response to this pressure. To summarize, eleven states have imposed fertility limits on village council members for at least a few years and they remain in effect in seven states.

3 Data

We utilize three cross-sectional rounds of the National Family Health Survey (NFHS-1, 2, 3) of India that were conducted in 1992-93, 1998-99, and 2005-06. Each round is representative at the state-level and includes a complete retrospective birth history for the woman inter-

in a nearly three-year grace-period since the original announcement was made in 1992.

[22]However, since the 1994 elections in Haryana took place before the announcement and since members are elected for a period of five years, no one was disqualified during 1995-2000.

[23]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.

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

[25]The Returning Officer is responsible for scrutinizing the information submitted by the nominees and any objections raised by other candidates, the general public, or the media.

[26]Data for the remaining states and years is not readily available.

[27]`http://policydialogue.org/files/events/Aiyar_Key_Role_of_Panchayati_Raj_in_India.pdf`

viewed, containing information on the month and the year of birth, birth order, and mother's age at birth. We combine these birth histories to construct an unbalanced woman-year panel; 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 women in the 15-49 age-group who were married at the time of survey.[28] We also drop women (i) whose marriage took place more than 20 years before 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 had 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. However, all our results are robust to the inclusion of these observations.

We restrict our analysis to the rural sub-sample as almost all the fertility limits in our sample period, with the exception of Uttarakhand, were enacted for rural councils; in fact, we find no significant effects for urban areas.[29] Our final sample comprises 99,804 women and 256,267 births from 18 major states[30] and covers the time period 1973-2006. We define treatment based on the year of announcement of the law, i.e., the earliest year when the law might have had an effect in a state. For instance, the announcement year is 1994 for Haryana. Since the most recent year in our sample is 2006, we cannot credibly examine the effect of revocations that took place in 2005.

Table 2 presents the sample means and standard deviations for the key variables used in our analysis, separately for never-treated and treated states. We further split the treatment state sample into pre- and post-treatment observations. The majority of the sample is Hindu. In terms of caste-composition, SCs comprise 16 to 19 percent of the sample. Educational attainment of women is low, with nearly 61 percent of the sample being uneducated; in comparison, 33 to 37 percent of the husbands are uneducated. Only 6 to 8 percent of the families have a high wealth status, and the majority have low standard of living index (SLI).[31]

[28]The questionnaires were administered to 13-49-year old ever-married women in NFHS-1 and 15-49-year old ever-married women in NFHS-2, 3.

[29]The urban estimates are available upon request.

[30]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 analysis.

[31]Low and High SLI are equal to one if the household belongs to the bottom-third or the top-third of household wealth distribution in all of India (i.e., rural as well as urban areas).

The sample means for the three groups in Table 2 are similar along many socioeconomic dimensions, but there are several significant differences as, like most natural experiment settings, the enactment of the limits was not randomized across states. Nevertheless, to ensure that our estimates are not confounded by underlying differences between these samples, we control for religion, caste, standard of living, and husband's and wife's years of schooling in all regressions. To take into account state-specific factors, we include state fixed effects and state-specific linear time trends. Crucially, we also show (in the next section) that the timing of announcement of the limits across states is uncorrelated with changes in these socioeconomic characteristics across states and over time.

4 Empirical Strategy

The goal of our empirical strategy is to estimate the causal effect of the two-child limits on candidates in village council elections 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 announcement of these laws across Indian states. Although eleven states have enacted such a law thus far, due to data limitations we can estimate the impact for only seven (eight) states: Rajasthan, Haryana, AP, Orissa, HP, MP (including Chhattisgarh), and Maharashtra. This is because the limits came into effect in Bihar and Gujarat after 2006, which is the most recent year in our dataset; so in our sample these states are in the control group. Gujarat announced its law in 2005, so we can potentially include it in the treatment group and use 2006 as the post-treatment year; doing so makes no difference to our results. Uttarakhand announced its law for urban municipal elections in 2002, however, we exclude it from the treatment group because Uttarakhand was a part of Uttar Pradesh until 2000 and we cannot distinguish between the two in the pre-2000 sample.[32] Moreover, we do not find significant effects in urban areas, and thus focus on the rural sample, and Uttarakhand has not enacted a limit for rural councils. For these reasons, we keep Uttarakhand in the control group. 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.

We begin by examining how the hazard of birth evolved before and after the laws were announced, in an event-study framework. Specifically, for a woman i of age a in state s and

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

year t we estimate the following regression specification:

$$Y_{iast} = \sum_{k=-6}^{5} \alpha_k T_s * Post_{s,t+k} + \sum_{k=-6}^{5} \beta_k Post_{s,t+k} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \nu_s * t + \epsilon_{iast} \quad (1)$$

We include both treatment and control states in specification (1). For a treatment state, $Post_{s,t+k}$ indicates k years during which the law is in place; we assign the year before the year of announcement as the omitted year. For a control state, $Post_{s,t+k}$ indicates k years during which a fictitious law is in place; we assign the same announcement year to a control state as its neighboring treatment state. If a control state borders multiple treatment states, we randomly assign it the treatment year of one of its neighbors. Later, in section 6, we show that our results are robust to alternate assignments of placebo announcement years to control states. Since the laws were announced in different years, the number of post-announcement years varies across states in our sample. Therefore, for the event-study analysis in (1), we restrict $k \leq 5$ to equalize the number of post-treatment years across states. The outcome variables Y_{iast} are indicators for first, second, third, fourth, and fifth birth. In regressions where the outcome indicates a birth of order b, the sample is restricted to years after birth $(b-1)$, until birth b, and to women whose previous $(b-1)$ children were born before announcement of the law in their respective states. For example, the hazard of third birth is estimated using years after the year of second birth and excluding the years after the year of third birth for women whose first two children were born before announcement of the fertility limits. We control for fixed effects for state, year, and woman's age (γ_s, θ_t, and ψ_a, respectively), state-specific linear 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 religion (five categories), caste (four categories), and SLI (three categories) of the household, and for the year of interview.

The β_k coefficients in (1) capture the evolution of the hazard of birth in control states and the α_k coefficients capture the changes in the hazard of birth in treatment states after differencing out the control group. If there is no noticeable pre-trend in the differential hazard of birth across treatment and control states, we can interpret the α_k coefficients during the post-announcement years as the causal effect of the limits. In order to measure the net effect of the limits on birth hazards we pool the event study coefficients in (1) and estimate:

$$Y_{iast} = \omega + \alpha Treat_{st} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \nu_s * t + \mu_{sa} + \epsilon_{iast} \tag{2a}$$

$$Y_{iast} = \omega + \alpha T_s * Post_{st} + \beta Post_{st} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \nu_s * t + \mu_{sa} + \epsilon_{iast} \tag{2b}$$

Specification (2b) corresponds to (1), except that we pool the α_k and β_k coefficients into α and β, respectively. Additionally, we estimate (2a) where instead of assigning fictitious treatment years to control states, we define treatment as zero for them throughout the sample, i.e., treatment is defined in terms of the variable $Treat_{st}$ which is equal to one for women residing in the treated states if $t \geq$ the year of announcement, and zero otherwise; thus, $Treat_{st}$ is always zero in (2a) for control states. We also control for state $\times$ mother's age fixed effects (μ_{sa}) and fixed effects for years since last birth or, in case of the hazard of first birth, years since marriage. Where the outcome indicates a birth of order b, we again restrict the sample to women whose previous $(b-1)$ births took place before the law was announced in their respective states. These regressions, therefore, capture the effects of the fertility limits on *marginal* fertility of affected households. Note that, unlike (1) where $k \in [-6, 5]$, specifications (2a) and (2b) use all available pre- and post-announcement years for each state in our sample that satisfy the previously mentioned restrictions.

While specifications (1), (2a), and (2b) tell us how marginal fertility was affected for couples who already had a certain number of children when the limits were announced in their state, they do not measure the overall impact of the laws on the "stock" of fertility. Therefore, we re-estimate equations (2a) and (2b) using indicators for whether a woman i of age a in state s and year t reports having one, two, three, four, and five living children in year t as the outcome variables. Unlike the hazard analysis, however, in this case we do not impose any restrictions in terms of the prior number of children and use all available years for each woman. If the two-child limits are effective, we expect the likelihood of having two children to increase in the treatment states after the laws have been announced.

The two-child laws may also affect the sex ratio of births. For instance, parents who do not have the desired number of sons when the law is announced and who can afford to have an additional birth without violating the limit may be more likely to practice sex-selection for that last birth. Therefore, we estimate the impact of the fertility limits on the sex ratio of second and higher parity births for couples whose first child was born before the limits were announced in their state. Specifically, we estimate the following specifications where the outcome variable is an indicator for the child being male:

$$Male_{iast} = \alpha + \beta Treat_{st} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \nu_s * t + \mu_{sa} + \phi Girl_i + \epsilon_{iast} \quad \text{(3a)}$$

$$Male_{iast} = \alpha + \beta_1 T_s * Post_{st} + \beta_2 Post_{st} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \nu_s * t + \mu_{sa} + \phi Girl_i + \epsilon_{iast} \quad \text{(3b)}$$

These specifications are similar to (2a) and (2b) except that we also control for the sex of the first child, $Girl_i$. We focus on second and higher parity births as 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), Das Gupta and Bhat (1997), Visaria (2005)) and most instances of sex-selection occur for higher-order births. This finding is consistent with recent survey data that suggests that Indian parents do not always prefer having a son over a daughter—Jayachandran (2014) finds that although the vast majority of families want to have a son if they can only have one child, at a family size of two they prefer having one daughter and one son over having two sons. As desired and actual fertility in India are well above one, it is reasonable to assume that parents are not averse to having one daughter, despite a strong desire for at least one son. In fact, Table 2 shows that the sex ratio at first birth in our sample is "normal" (i.e., is between 0.516 and 0.519) in the never-treated states and in the treatment states (both pre- and post-treatment). It is also well-established that parents whose first child is a girl are more likely to practice sex-selection at higher-parity births (e.g., Pörtner (2010), Rosenblum (2013), Anukriti et al. (2016)) since they desire at least one son. Therefore, we control for the sex of the first child in (3a) and (3b). Although we explicitly show that the fertility limits did not change the sex ratio of first births, to ensure that our results are not biased by sex-selection at first parity, we restrict the sample to couples whose first child was born before the limits were announced in their state.

The inclusion of state and year fixed effects in our specifications controls for all time-invariant state-level variables and state-invariant time effects that might affect the outcomes of interest. The state-specific time trends account for differential linear trends in fertility and sex-selection patterns across states over time (e.g., due to differential growth rates of state GDP or availability of abortion and other health services). The inclusion of state × mother's age fixed effects controls for any confounding differences in the age composition of mothers across states. Since treatment varies at the state level, we cluster standard errors by state to allow for correlations in the error terms of women in each state. As the total number of treatment and control states in our sample is 18, we also report standard errors based on a clustered (by state) wild bootstrap-t procedure described in Cameron et al. (2008) to address econometric issues pertaining to a small number of clusters.[33]

The underlying identifying assumption in our analysis is that the state-year variation in the timing of law announcement is uncorrelated with other time-varying determinants of the outcomes of interest. Although we control for state-specific linear trends in our regressions, we also explicitly test if the timing of announcement is correlated with other socioeconomic characteristics that vary by state and time. In Table 3 we present the coefficients from regressions that use various maternal, paternal, and household characteristics as dependent variables in the estimation of equation (2a) with state and year fixed effects, and state-specific time trends, but without any other controls for the rural sample. None of the 21 coefficients in Table 3 are significant, thus eliminating any concerns about endogenous timing of announcements.[34] Moreover, to the best of our knowledge, during the time-period we examine, there were no other state-specific programs in the treatment states that promoted smaller families and whose timing coincided with the fertility limits.

5 Results

In this section we present regression estimates of the causal impact of the fertility limits on marginal fertility, on the total number of living children in a year, on contraceptive use, and on the sex ratio of second and higher parity births.

5.1 Effects on marginal fertility

We start by examining the evolution of the hazard of birth (for birth orders one to five) in treatment and control states using specification (1). The estimated α_k coefficients for the annual hazard of births over a 11-year period are presented in Figure 2. The corresponding regression coefficients with standard errors clustered by state are shown in Appendix Table A.3. The vertical line in Figure 2 indicates the year before announcement for treatment

[33]We use the STATA code written by Busso et al. (2013) that computes the errors by assessing the fraction of bootstrap test statistics (in 1,000 repetitions) greater in absolute value than the sample test statistic.

[34]The same is true if we include the urban sample in this regression.

states and the year before the fictitious announcement year for control states. The difference between treatment and control states is quite flat for all birth orders in the years before announcement, indicating that pre-trends in the hazard of birth are parallel between treated and control states. In fact, none of the lagged α_k coefficients in Appendix Table A.3 are significant, supporting the parallel trends assumption. However, after the fertility limits are announced there is an immediate increase in the relative hazard of second births in treated states, which declines in the following years. Moreover, the hazard of third birth shows a visible and sustained decline during all post-announcement years. There is no visible impact on the hazards of first and fourth births.[35]

The decline in the hazard of third birth suggests that the fertility limits induced couples in treatment states who had two children at the time of announcement, and were thus at the margin of electoral eligibility, to sacrifice third births. Although this does not necessarily imply a decrease in completed fertility, the fact that the hazard of third birth continues to decrease for at least five years after the announcements indicates that our results are less likely to be driven by a mere postponement of third births. On the other hand, the shape of the second birth hazard does suggest a shift in timing of second births. Although the fertility limits do not impose any restrictions on the timing of second birth, it appears that couples who had one child at announcement have their second child sooner than they would have in the absence of these eligibility requirements. This response could, for example, be driven by fear or anticipation of even stricter limits on fertility in the near future. The lack of a significant decline in the hazard of fourth births is intuitive as these couples already had three children before announcement and had, therefore, most likely achieved their desired fertility; so they were less likely to have a fourth birth even in the absence of the new laws. Similarly, an insignificant impact on the hazard of first birth makes sense as desired fertility is above one in India and the fertility limits do not impose any constraints on first births.

Next we present the net impacts of the limits on marginal fertility estimated using specifications (2a) and (2b). Table 4 presents results for the hazard of third birth while the estimates for first, second, and fourth births are in Appendix Table A.4. The hazard of third birth is presumably the most relevant margin at which we expect the two-child limits to operate on completed fertility—at births below birth order three the policy does not bind,

[35]We also find no effect on the hazard of fifth birth; the corresponding figure is available upon request.

and at birth orders beyond three, households have largely satisfied their desired fertility by the time the policy is announced. Panel A of Table 4 corresponds to specification (2a) and panel B corresponds to (2b). The standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. In column (1), where we control for fixed effects for state, year, and years since second birth, and for the vector of socioeconomic characteristics, we find that the limits decreased the hazard of third birth, but insignificantly. The effect remains negative and becomes significant in column (2) once we control for state-specific linear time trends. We find that the probability of third birth declines by 2.13 p.p. in column (2) of panel A and by 2.65 p.p. in column (2) of panel B. These effects are large, respectively translating into 10 percent and 11 percent declines from the baseline hazard of 21.31 percent of having a third birth in a given year before announcement. The inclusion of state $\times$ mother's age fixed effects in column (3) changes the estimated decline in third births marginally to 2.06 p.p. in panel A and to 2.63 p.p. in panel B. Although the event-study graphs in Figure 2 do not reveal any substantial grace-period driven increase in fertility, any such effect is likely to bias our estimated decline in the hazard of third birth in the downward direction. Appendix Table A.4 shows no significant net effects on the net hazard of first, second, and fourth births. Thus it appears that the effect on second births visible in Figure 2 was limited to a shift in timing of second births and did not imply a change in the net hazard of second birth.

In Table 5, we present results from estimating (2a) and (2b) inclusive of state-specific linear time trends and state $\times$ mother's age fixed effects, separately by socioeconomic group. Columns (1)-(4) of panel A show that among caste groups, the greatest declines in the hazard of third birth are in SC and OBC families, of 4.10 p.p. and 2.88 p.p., respectively. This represents a 16.57 percent decline from the baseline pre-announcement hazard of third birth for SC households, and a 14.47 percent corresponding decline for OBC households. As described in Section 2, both these caste groups are legally recognized as socially and economically disadvantaged by central and state governments, and village council positions are reserved for candidates from these castes in every state to ensure adequate political representation. As such, it is unsurprising that these households show the greatest response to the law at the margin, as they have the most to lose from reduced access to political power if they violate the fertility limits. In comparison, there is no significant decline in third births among the "general" or Upper castes (and for ST families). In a similar vein, columns (5)-(6)

reveal a statistically significant decline in third births among poorer households (low SLI) of 2.14 p.p. (8.64 percent), whereas wealthier households with a high SLI score show no visible decline in the probability of having a third birth in column (6).[36]

In the last four columns, we split the sample by husband's and wife's years of schooling (no schooling versus some schooling). While the coefficients in columns (7) and (8) of panel A are negative, they are both insignificant based on bootstrapped standard errors. On the other hand, in columns (9) and (10), the hazard of third birth significantly declines both in households where the father/ husband has no schooling and some schooling. In line with the pattern of results in the rest of panel A, the decline is larger if the father has no schooling (2.94 p.p. or 11.29 percent) than if he has some schooling (1.59 p.p. or 8.29 percent).

The smaller decreases in the hazard of third birth for relatively higher socioeconomic status families also reflect the fact that these families have a lower baseline probability of third birth even in the absence of the limits and hence are, *ceteris paribus*, "less treatable" than lower socioeconomic status households. For instance, the baseline hazard of third birth is only 11 percent for high SLI mothers as compared to 25 percent for low SLI mothers.

The estimates in panel B of Table 5 are slightly larger in magnitude and more precisely estimated than in Panel A, but the overall pattern of heterogeneity is similar. The hazard of third birth declines significantly by 4.67 p.p. (16.50 percent) for SCs, by 2.52 p.p. (10.20 percent) for OBCs, and by 2.96 p.p. (11 percent) for low SLI families. The effects are also significant in the last four columns, with the coefficients being larger in magnitude in families where the father has no schooling.

5.2 Effects on the total number of children

The results thus far have focused on the marginal effect of the limits on an additional birth conditional on a woman already having a certain number of children at announcement. However, they do not tell us the extent of substitution from, say, having four children to having only two children. In order to evaluate the overall impact of the laws on total "stock" of fertility, we re-estimate specifications (2a) and (2b) using indicators for whether a woman reports having one, two, three, four, and five living children in a given year as the outcome variables. Unlike the hazard analysis, in these regressions we do not impose any restrictions

[36]Low and High SLI are equal to one if the household belongs to the bottom-third or the top-third of household wealth distribution in India.

in terms of the prior number of children and use all available years for each woman. If the two-child limits are effective, we expect the likelihood of having two children to increase and the likelihood of more than two children to decrease after the laws have been announced in treatment states relative to control states and relative to pre-treatment years. These regressions capture the marginal effects on couples who had begun childbearing before the laws were announced as well as the behavioral response of new parents who started having children in the post-announcement years.

Table 6 presents the estimated effects of the fertility limits on these outcomes. In panel A, we also show results from specification (2a) estimated only for the sample of treatment states. The coefficients of $Treat_{st}$ in panel A imply that after the limits were announced, the probability of having two living children in a given year increased significantly by 0.75 p.p. or 3.41 percent in treatment states. The rest of the columns in panel A indicate that this increase in the likelihood of two children is a result of substitution away from higher fertility levels—the probability of having three, four, or five living children declined respectively by 0.42 p.p. (2.48 percent), 0.47 p.p. (5.62 percent), and 0.28 p.p. (8.70 percent). There is no significant impact on the likelihood of one child. Panels B and C in Table 6 show a similar pattern of results, although the coefficients are not always as significant as those in panel A.

Since our dataset is a retrospective unbalanced panel, one concern is that the women in post-announcement years may be systematically younger than those in the pre-announcement period. Although our specifications control for age fixed effects as well as for state $\times$ age fixed effects, in order to test the robustness of our findings we re-estimate the prior set of regressions for women who were no older than age 33 in a given year. This age restriction reduces sample selection due to changes in mother cohort composition across NFHS rounds. As Figure A.1 displays, the average age of mothers evolves smoothly over time across birth parities with the age restriction. Appendix Table A.5 shows that the estimated effects on the number of children remain similar to those in Table 6 despite the age restriction.

Next we examine heterogeneity in the effects on the total number of children by socioeconomic characteristics of the parents using specification (2a). To avoid issues related to a large number of subgroup estimates for five different outcomes, for these heterogeneity results we use an indicator for more than two living children as the outcome variable. Panel A in Table 7 focuses only on treatment states whereas panel B shows the estimates for all states.[37] In panel A, the probability of having more than 2 children declines significantly by 1.33 p.p. or 4.42 percent in treatment states, but the coefficient is insignificant in panel B. Although both high and low SLI families exhibit a significant decline, the magnitude of the effect is larger for low SLI households (1.9 p.p. versus 1 p.p. for high SLI). Similarly, couples where the wife has no schooling display a slightly larger decline (1.6 p.p.) than couples where the wife has some schooling (1.1 p.p.), although both are significant. In terms of husband's schooling, the coefficients are quite similar for both groups, but only significant if the husband has some schooling. In terms of caste affiliation, the only significant coefficient is for STs. On the whole, Table 7 is roughly consistent with the differential effects on hazard rates across socioeconomic groups that we described in the previous sub-section, but we lose significance at conventional levels in panel B of Table 7.

5.3 Effects on contraceptive use

Our previous results show that the limits induced couples to decrease marginal and possibly completed fertility. To the extent that modern methods of contraception are accessible to these rural families, we expect contraceptive prevalence to increase after the limits were announced. Unfortunately, we do not have individual-level or couple-level panel data for all modern methods of contraception, except for sterilization. Therefore, we estimate the effect on the use of modern contraceptive methods using specification (2a), but modify it so that the time subscript now refers to the year of interview. Thus, the coefficient α now estimates the difference in contraceptive use for women interviewed before and after the fertility limits were enacted in their respective states, after conditioning on fixed effects for state, year of interview, years since last birth (or marriage in case the mother has only one child), age at the time of interview, and state $\times$ age at interview, the vector of socioeconomic characteristics, and linear state-specific time trends. Unlike prior regressions, these specifications use repeated cross-sections of data.

Appendix Table A.7 presents these estimates. In all columns the dependent variable is an indicator that equals one if a woman reports using any modern method of contraception at the time of her interview, and zero otherwise. Modern methods of contraception comprise male or female sterilization, pills, condoms, intrauterine devices, diaphragms, and injections.

[37] The estimates for the effects on probability of less than three living children are by construction exactly the same as those in Table 7 but of the opposite sign.

In columns (1)-(3), the sample is restricted to years after the year of second birth for women whose second birth took place before the limit was announced in her state. The reported coefficients therefore estimate the effect on contraceptive use of women who had two children when the law was announced. All three columns show that there was a significant increase in the use of a modern method of contraception among women who were interviewed in a post-treatment year. The coefficient in column (3) implies a 3.8 p.p. increase from a baseline prevalence of 47.3 percent, i.e., an 8.03 percent increase.

In columns (4)-(6), we examine the sub-sample of women who have one, two, and three living children in the year of interview, and do not impose any restrictions on when these births took place. If the fertility limits are driving more people to stop childbearing after two children, we should observe a larger increase in the contraceptive use of women with two children relative to women with one or three children. The pattern of results in the last three columns of Appendix Table A.7 are consistent with this hypothesis. While there is a 4 p.p. increase in contraceptive use for women or couples who had two children at the time of survey, the coefficient is much smaller for women who had three children (0.018 and insignificant) and is small and negative for women who had one child (-0.014 and insignificant). These findings are consistent with a relatively low baseline contraceptive prevalence for couples with one child (11.5 percent) and a relatively higher prevalence for three-child couples (55 percent) compared to the 44.7 percent prevalence for two-child couples. Families with one child are not directly affected by the limits and those with three children have either violated the norm or had three children even before announcement, and are more likely to have completed their desired fertility. For families with two children, the coefficient translates into a 9.8 percent increase in use of modern contraceptive methods.

We also estimate the effect of the two-child limits on sterilization (male or female) in a hazard framework using the woman-year panel and specification (2a).[38] For each woman, we drop the years after the year of sterilization from our sample, and thus estimate the effect of the limits on the hazard of sterilization in a given year. Like the first three columns of Appendix Table A.7, we focus on years after the year of second birth for women whose second birth took place before the limit was announced in her state. While the estimated coefficient of $Treat_{st}$ is positive for the entire sample, it is insignificant. Heterogeneity analysis, however,

[38] For brevity, we do not report these results as a table but they are available upon request.

reveals significant increases in sterilization rates for uneducated spouses (i.e., couples where either the husband or the wife have zero years of schooling), who are most likely to use sterilization instead of other modern methods of contraception at baseline. The smaller effects for sterilization as compared to all modern methods may reflect a preference for temporary methods, such as pills and condoms, especially among couples who may want to run for office in the near future but may eventually want to have more children.

5.4 Effects on the sex ratio at birth

Next we examine the effects of the fertility limits on the sex ratio of second and higher parity births for all households and by household caste using specifications (3a) and (3b). Caste is a unique phenomenon of Indian society. As opposed to other dimensions of socioeconomic status, caste is exogenous in the sense that an individual is born into a caste and cannot choose it. The caste hierarchy is quite rigid and has been preserved by the low prevalence of inter-caste marriages despite substantial economic development.[39] In order to maintain their superior social position, higher caste households have historically laid greater emphasis on ritual purity and adherence of religious texts, and this has often been at the expense of women's position within these households (Das Gupta et al. (2003), Das Gupta (2010)). The essential role played by a son in Hindu rituals is also considered to be an important factor underlying the strong preference for sons among upper caste Hindus.[40] For these reasons, we focus on heterogeneity in the sex ratio effects by caste.

Specifically, we divide our sample into four caste groups: SCs, STs, OBCs, and upper castes. Although OBC families have a lower caste status, they are superior to SCs in the caste hierarchy, as well as in their socioeconomic status. The castes that are included in the OBC category vary across states; for instance, the *Jat* caste group is included in the OBC category in Rajasthan, but not in Haryana. Although OBC status is meant to improve the socioeconomic situation of historically backward classes, quite often politically dominant castes are able to lobby for OBC status in order to benefit from caste-based quotas in public employment and education. This often leads to socioeconomically privileged castes having OBC status in particular states, such as the influential landowning *Reddy* and *Kamma*

[39] According to the 2005 India Human Development Survey, only 4.4 percent of women were married to a spouse from a different caste.

[40] Although caste is primarily a Hindu phenomenon, the notion of caste-based hierarchy remains well-preserved among many other religious groups in India.

castes in Andhra Pradesh (Deshpande and Ramachandran (2013)).[41] Moreover, the process of *Sanskritization* (Srinivas (1962)) suggests that the relatively well-off lower castes tend to emulate the rituals and practices of the upper castes seeking upward mobility within the caste hierarchy. Thus it is likely that the sex-selective behavior of OBCs is more similar to that of upper castes as opposed to SCs and STs.

For the sex ratio analysis, we restrict the sample to women whose first child was born before announcement of the limits and control for the gender of this child as firstborn child's gender is known to be exogenous and is a strong predictor of future fertility and sex-selective behavior.[42] These results are displayed in Table 8. Columns (1)-(5) correspond respectively to the entire sample, and sub-samples of SCs, STs, OBCs, and upper castes. Panel A focuses on the sample of treatment states and shows that the limits have no significant impact on the sex ratio of second and higher parity birth in the total sample or for SCs, STs, and surprisingly also for the upper caste sample. However, in column (4) there is a large and highly significant 5.28 p.p. or a 10.32 percent increase in the sex ratio for the OBC sample over a baseline probability of 51.17 percent. We find the same pattern of results in panels B and C with OBC households exhibiting a significant 5.57 p.p. (10.74 percent) to a 5.70 percentage point (11.00 percent) increase in the probability that a birth is male.

While the upper castes are believed to have the strongest son preference in India, they also have the lowest fertility. In fact, in prior tables, we did not find large or significant fertility decline for upper castes likely because they have low baseline fertility to begin with. Thus, it is possible that the lack of significant sex ratio effects for upper castes also reflects that they are "less treatable" than other caste groups due to high prevalence of sex-selection at baseline. For the reasons mentioned above, it is not too surprising that OBC households respond to the law with greater sex-selection, as sex-selective behavior has been shown to be concentrated in non-SC and non-ST households with higher socioeconomic status (e.g., see Bhalotra and Cochrane (2010) and Anukriti et al. (2016)). Further, OBCs constitute significant fractions of the populations in our treatment states, such as Haryana (28.1 percent), Rajasthan (47.5 percent), Madhya Pradesh (41.2 percent), and Maharashtra

[41] In recent years, the *Jat* community in Haryana and the *Patels* in Gujarat have been proactively seeking OBC status.

[42] In Appendix Table A.6, we verify that the fertility limits did not affect the sex ratio of first births.

(27.1 percent),[43] that have highly adverse sex ratios of respectively 861, 922, 920, and 922 females per 1000 males in the 2001 Census of India.

6 Robustness

Our results thus far are based on random assignment of placebo treatment years to each control state from one of its neighboring treatment states. In this section, we conduct three tests to check the robustness of our findings to alternate ways of defining the control group for each treatment state and to different assignments of placebo treatment years to the given control group.

First we use the synthetic control method proposed by Abadie and Gardeazabal (2003) and Abadie et al. (2010) to show that our results are not driven by the specific control state that we have assigned to each treatment state.[44] Although we have shown that the pre-trends in our outcome variables are not significantly different across treatment and control states, this method allows us to construct a synthetic control state that best approximates the treatment states during the pre-treatment period. To avoid the computational complexities associated with the synthetic control approach in case of multiple treatment states with different treatment years, we combine all treatment states into one group and redefine the time variable in terms of years from announcement. As before, we assign placebo treatment years to neighboring control states to define years from announcement. We collapse the data to state-year level, and using a vector of demographic and socioeconomic characteristics, construct a synthetic control state that best approximates the pooled treatment state during the pre-treatment period. We then use the post-announcement outcomes for this synthetic control state to approximate the outcomes that would have been observed for the treatment state(s) in the absence of the fertility limits. The donor pool comprises all never-treated states. Figure 3 shows the evolution of the probability of having two living children in a given year before and after the limits were announced. The synthetic control resembles the treatment states quite closely before the limits were announced.[45] However, after the announcement, couples in treatment states are much more likely to report having two living

[43] This information is from the 64th round of the National Sample Survey of India (2007-08).

[44] We use the *synth* command in STATA.

[45] The exact weights assigned to each donor state in the construction of the synthetic control state are available upon request.

children as compared to women in the synthetic control state. It appears that the likelihood of having two children also goes up in control states, especially towards the end of the sample period—this could be driven by anticipation effects or perhaps the role-model effect that comes into play once the treatment states have elected their village council members and the laws have been in effect there for several years. Nevertheless, Figure 3 shows a clear effect on fertility in treatment states immediately after announcement of two-child limits over and above any potential spill-overs in control states. This implies that our findings are not driven by a specific assignment of control state to each treatment state.

In the second robustness check, we test the sensitivity of our findings to alternate assignments of treatment years to the control states. Irrespective of which treatment state it borders, we assign the same treatment year to each control state before re-estimating (2a) and (2b), and repeat this exercise using each year during 1993-1999. Table 9 reports these results for the effect on the net hazard of third birth. In both panels, the results are remarkably similar in magnitude to those reported earlier in Table 4, and the estimated coefficient α is significant in 12 out of 14 cases. Moreover, in panel B, the coefficient of $Post_{st}$ is always insignificant, lending further credibility to our claim of causality. Thus, our findings are not driven by a specific assignment of placebo announcement years to control states.

In our final robustness check, we exploit all available information in our data by using *all* neighboring control states as matches for each treatment state. As mentioned earlier, the control states in our sample often border multiple treatment states. For our main set of results, we had only matched each treatment state with one control state. Now, however, we match each treatment state with all its neighboring control states and create a new dataset in which control states that border multiple treatment states appear multiple times. For instance, women from Gujarat serve as control group for both neighboring treated states—Maharashtra and Rajasthan. But in one case the placebo treatment year assigned to Gujarat is the same as the treatment year of Maharashtra and in the other case it is the same as that of Rajasthan. In other words, we duplicate the Gujarat sample and define $Treat_{st}$ and $Post_{st}$ using different years depending on which treatment state Gujarat is being matched with. To take into account the duplication of observations from control states, we weight each observation by the square root of the inverse of the number of times an observation appears in the sample. Then we re-estimate (2a) and (2b) on this new weighted sample; these results are presented in panels A and B of Table 10, respectively. In both panels, we find that our main findings are unchanged. Panel A reveals a statistically significant decline in the net hazard of third births of 1.99 p.p. in column (2) and of 1.88 p.p. in column (3). In panel B the estimated declines are slightly larger, at 2.16 p.p. in column (2) and 2.17 p.p. in column (3). The effects are quite similar to those reported in Table 4 reassuring us that our findings are capturing the true causal effect of the fertility limits and are not a figment of the empirical strategy.

7 Discussion

Given that average baseline terminal fertility in the treatment states is 2.8, the two-child limits impose a binding constraint on childbearing for a large fraction of individuals in these states. According to our sample, nearly 30 percent of couples where the wife is 15-49 years old had more than two children at baseline in treatment states. We find that the probability of having more than two children declines significantly by 1.33 p.p. in treatment states, i.e., 1.33 percent of couples where the wife is 15-49 years old decreased their fertility due to the limits. According to the 2001 Census of India, the number of married rural women in the 15-49 age group in treatment states is 46,269,920.[46] Thus, 615,390 (which is 1.33 percent of 46,269,920) couples responded to the limits. If we treat husbands and wives as separate individuals, the number doubles, i.e., 1,230,780 individuals responded to the limits.

We can do the same calculation using our estimated effects on the hazard of third birth. We find that the probability of having a third child declines by 2.06 to 2.65 p.p. in treatment states, i.e., 2.06 to 2.65 percent of couples where the wife is 15-49 years old and who had two children in treatment states decreased their marginal fertility due to the limits. According to the 2001 Census of India, the number of married rural women in the 15-49 age group in treatment states who had two children is 10,562,944.[47] Thus, 217,597 to 279,918 (which is 2.06 to 2.65 percent of 10,562,944) couples responded to the limits. Again, if we treat husbands and wives as separate individuals, the numbers double, i.e., 435,194 to 559,836 individuals gave up a third child due to the limits. The total number of couples who adjusted fertility

[46]We use the number of married women in the 15-49 age group in treatment states (= 71,184,492) and multiply it with 0.65 (the share of rural women in our sample) to get the corresponding number for the rural sector, i.e., 46,269,920.

[47]We use the number of married women in the 15-49 age group with two children in treatment states (= 16,250,683) and multiply it with 0.65 (the share of rural women in our sample) to get the corresponding number for the rural sector, i.e., 10,562,944.

(in the previous paragraph) is higher since it also includes couples who started childbearing after the limits were announced and those who had one child at announcement and stopped after having the second child.

Although we cannot credibly distinguish between the relative importance of the aspirations, role-model, and anticipation channels, our estimated impacts are large and consistent with the high participation of voters and candidates in local politics, making both the aspirations and role-model channels plausible. However, if the role-model channel is the primary underlying mechanism, we also expect the residents of neighboring control states to respond to the fertility limits in treatment states. This is not what we find in the years immediately following the announcements, as our identification strategy measures the differential impacts in treatment states relative to control states. Hence, the aspirations channel is potentially a significant explanation for our results. If we assume that the fertility response is entirely driven by political aspirations, we can also translate the numbers in the previous paragraphs into response per council seat. Our treatment states had 912,597 seats across all three tiers of the Panchayat system in 2004. The 1,230,780 figure thus translates into a response rate of 1.35 individuals per seat. The Association for Democratic Reforms reports an average of 2.43 candidates per village council seat. This implies that slightly more than half of all potential contestants per seat in each election cycle in treatment states altered their fertility. In reality, not all incumbents would get disqualified by these limits. According to survey data from South India (Besley et al. (2003)), about 35 percent of village council heads or members had more than two children in 2002. Assuming the same fertility distribution for enacting states and assuming that all incumbents who have two or less children would be able to get re-elected, 1,230,780 individuals adjusted fertility for 593,188 seats that became "vacant" due to disqualification of the incumbents, which is 2.07 people per seat or 85 percent of contestants per seat. Even if we assume that only one of the spouses runs for office, 28 to 43 percent of contestants per seat adjusted fertility due to these laws if aspirations are the only mechanism behind the results.

How do our estimates compare with other fertility reduction programs in developing countries? Although fertility decline appears to be primarily driven by decreases in desired fertility (Pritchett (1994)), access to family planning also seems to matter (Canning and Schultz (2012)). For instance, the family planning interventions in Matlab (Bangladesh) decreased the number of living children by 17 to 23 percent in treatment areas. Similarly, in

Navrongo (Ghana) health interventions that improved access to and information about contraception, led to a 9 percent decrease in the number of children ever born. Other large-scale population control programs have also been shown to impact fertility rates—e.g., Almond et al. (2013) find that China's One Child Policy that imposes fines on couples who violate the one-child limit decreased fertility by 2 percent. In the context of India, Anukriti (2014) shows that a financial incentive scheme that targets both fertility and sex ratios reduced the number of children by 1 percent. While our setting is not directly comparable to these other studies, the fertility limits decreased the likelihood of third birth among couples who had two children at announcement by 10 to 11 percent and reduced the overall probability of having more than two living children by 3.41 percent. Thus, our estimated effects are somewhat larger than those achieved by direct financial incentive and disincentive programs, but smaller than the effects of interventions like Matlab that increase contraceptive access.

8 Conclusion

We find that the two-child limits on candidates in village council elections decrease fertility among constituents, but also lead to an unintended increase in the already male-biased sex ratio in certain socioeconomic groups. These effects may be caused by constituents' political ambitions, the role-model influence of their leaders, or anticipation of stricter restrictions in other non-political arenas. Political aspirations may not only reflect the desire to effect positive social change, but could also be driven by rent-seeking behavior. The potential income from political rents and corrupt practices may be a strong incentive for becoming an officeholder in low-income countries. While we cannot separately identify these "altruistic" and "selfish" components of political aspirations, we show that these ambitions may be substantial and represent a previously ignored channel of demographic change.

Our findings reiterate that population control measures that ignore son preference can worsen the sex ratio at birth. Similar limits have been proposed for members of state legislative assemblies and the national parliament in India. If incentives for local leadership are stronger than state or national leadership ambitions, or if the role-model potential of local leaders is higher, the proposed limits may be less effective than the laws we examine.[48]

[48] Genicot and Ray (2014) formalize a related idea as follows: "...the "best" aspirations are those that lie at a moderate distance from the individual's current economic situation standards, large enough to incentivize but not so large as to induce frustration."

Fertility restrictions on elected members also have implications for political representation of various socioeconomic groups. The limits impose a more severe constraint on couples with weaker access to contraception or higher demand for children, increasing their risk of disqualification and reducing their political representation. The limits could also impede the progress made by caste-based affirmative action if only the “creamy-layer” of the lower-castes are able to meet the eligibility criteria. The limits could also undermine gender-based quotas as aspiring female leaders may not have autonomy over their fertility due to intra-household gender disparities. Indeed, women comprise the overwhelming majority of individuals in Table A.1 that were disqualified for violating the limits. However, our hazard results suggest that the fertility decline is significant even for low socioeconomic status families, diminishing some of these concerns, but we cannot rule out differential impacts for the most disadvantaged households within the groups we examine. An explicit examination of the impact of the fertility limits on political outcomes is crucial but is beyond the scope of this paper.

Our findings are timely and policy-relevant as recently some Indian states have enacted similar restrictions to meet policy goals in the areas of education and sanitation. As of 2014, individuals are barred from village council membership in Rajasthan if they have less than primary schooling or if they do not have a functional toilet in their home.[49] Although 50 percent of the council seats in Rajasthan are reserved for women, the female literacy rate is only 45.8 percent (2011 Census of India).[50] Moreover, lower castes face considerable discrimination in access to sanitation and education. Another north Indian state, Haryana, has also imposed education and sanitation requirements in the 2016 village council elections. Our results show that these new restrictions may have substantial social impacts, and merit further investigation.

[49] The minimum schooling requirements for block and district councils are eight and ten years, respectively.

[50] For tribal women, the literacy rate is even lower (25.22 percent).

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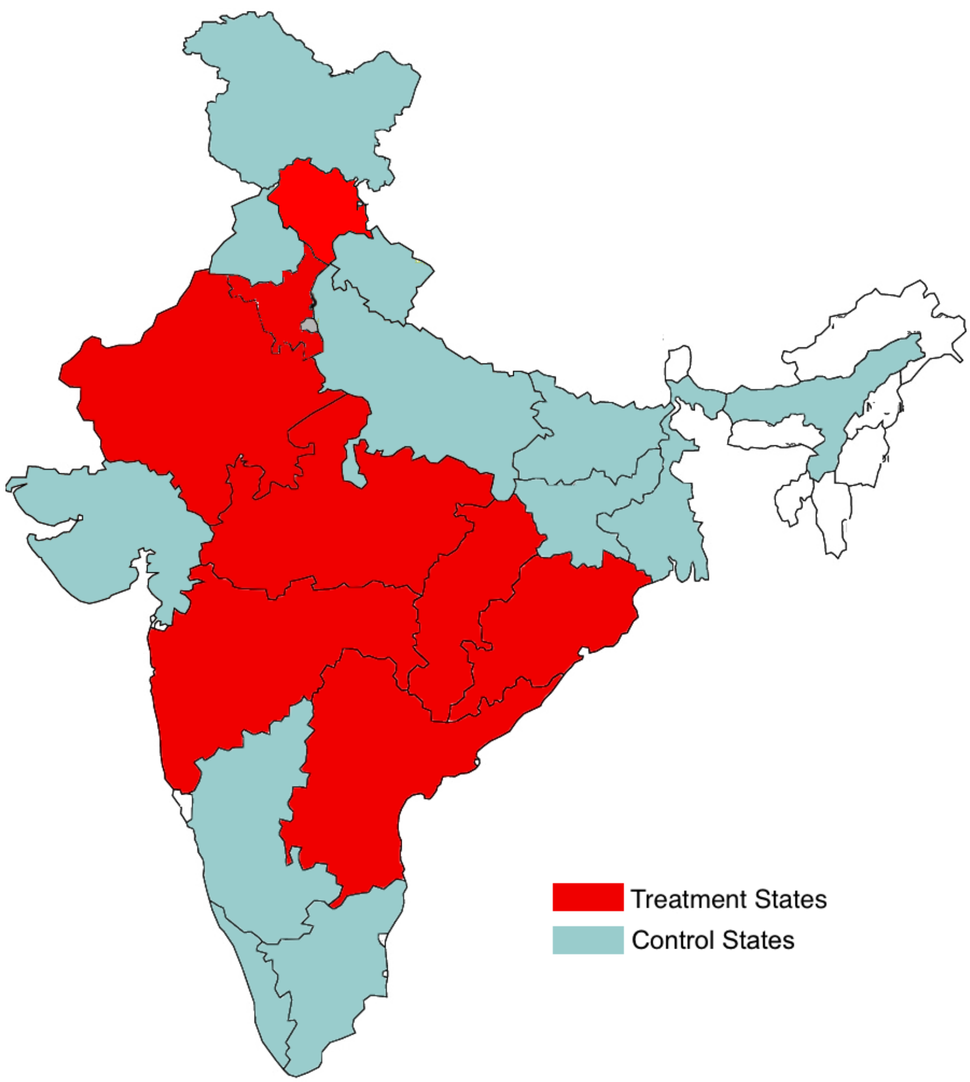
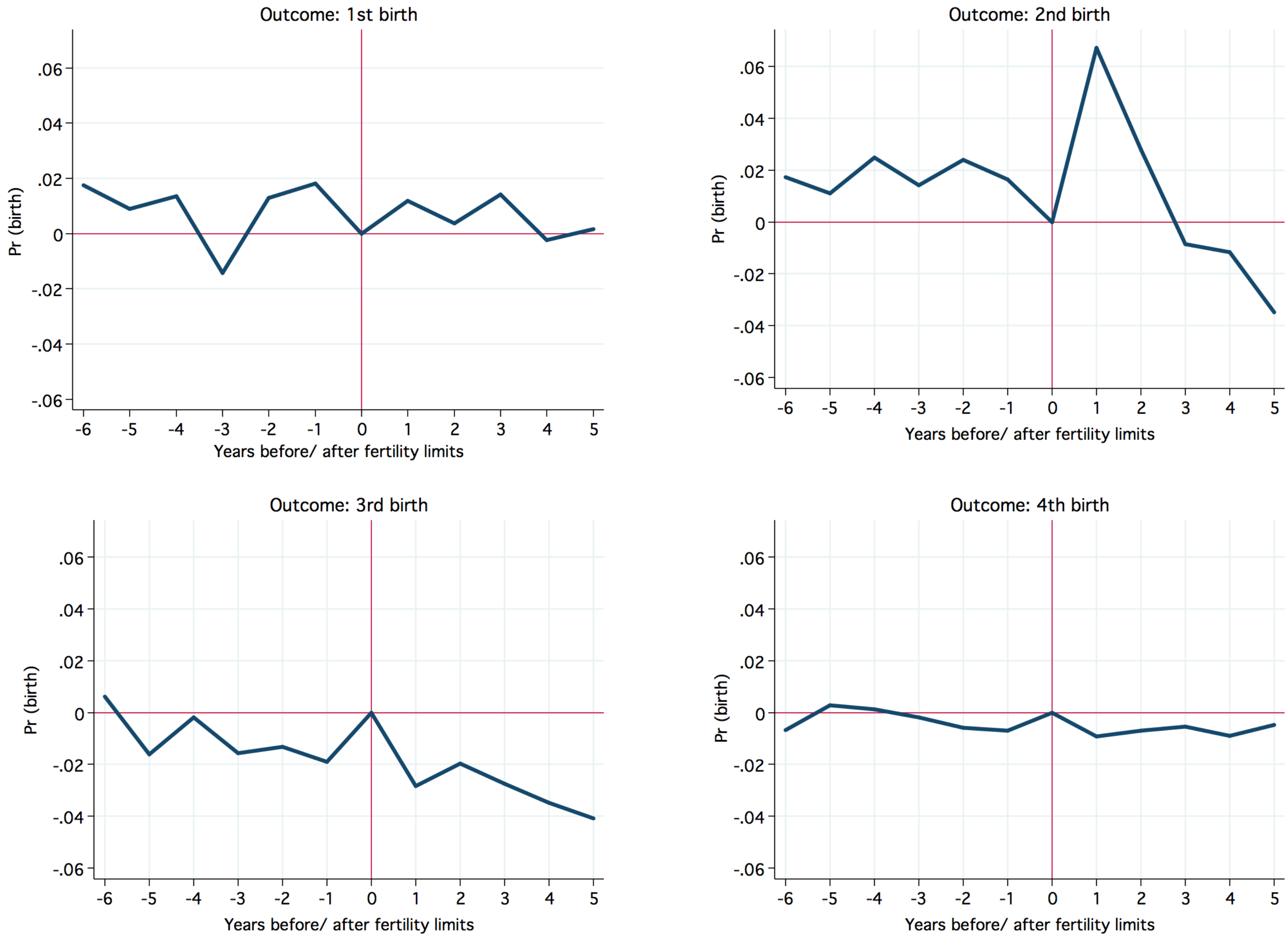
9 Figures and Tables

Figure 1: Treatment and control states



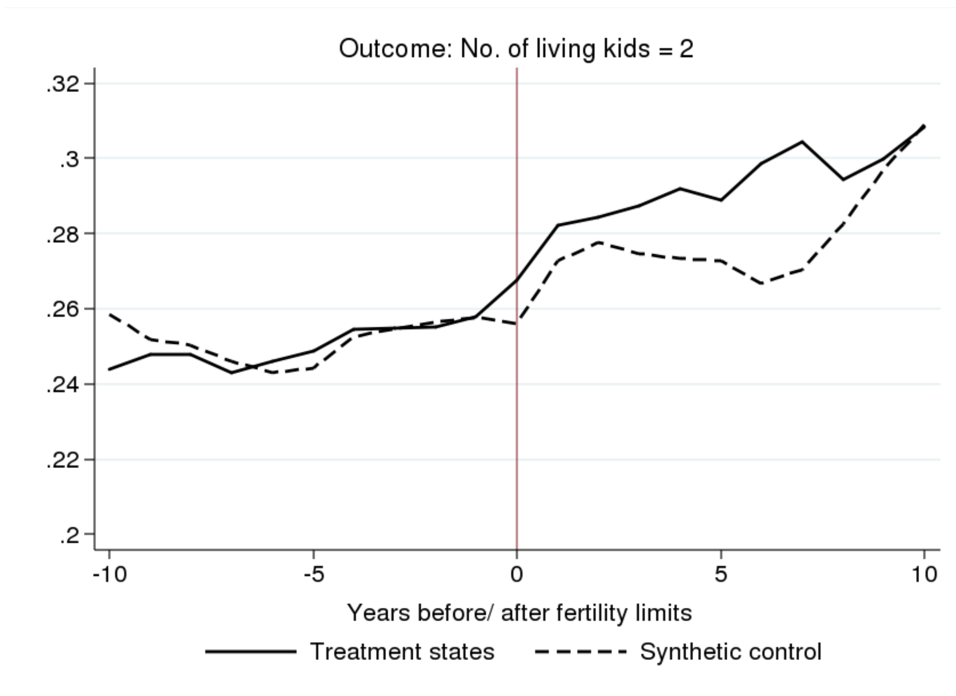
NOTES: Treatment states are those that have enacted a fertility limit at some point during our sample period. Control or never-treated states are those that did not.

Figure 2: Differential hazards of birth in treatment states relative to control states



NOTES: This figure plots the α_k coefficients from specification (1). The outcome variables are indicators for births of various orders. Each graph represents a different birth order. For the regression where birth of order b is the dependent variable, the sample is restricted to years after birth $(b-1)$ and until birth b for mothers whose $(b-1)^{th}$ child was born before the year of announcement in their respective states. The vertical line (at $k = 0$) indicates the year before announcement. The corresponding coefficient estimates are in Appendix Table A.3.

Figure 3: Effect on the likelihood of two living children using the synthetic control method



NOTES: The outcome variable is an indicator for two living children in a given year. Treatment states are those that have enacted a fertility limit at some point during our sample period. The synthetic control is constructed using the *synth* command in STATA based on the method proposed by Abadie and Gardeazabal (2003) and Abadie et al. (2010). The corresponding weights assigned to each control state in the donor pool are available upon request. The vertical line (at 0) indicates the year before announcement.

Table 1: Timeline for fertility limits across states

State	Announced	Grace Period	In effect	End
Rajasthan	Oct 1992	Apr 23, 1994 - Nov 27, 1995	Nov 27, 1995 -	
Orissa	Sep 1993/1994*	Apr 1994 - Apr 21, 1995	Apr 22, 1995 -	
Andhra Pradesh	Mar 1994	May 30, 1994 - May 30, 1995	Jun 1995 -	
Haryana	Apr 1994	Apr 21, 1994 - Apr 24, 1995	Apr 25, 1995 - Dec 31, 2004	Jul 21, 2006 (retro. impl. Jan 1, 2005)
Himachal Pradesh	Jan - Apr 2000	Apr 18, 2000 - Apr 18, 2001	Apr 2001 - Apr 2005	May 30, 2005
Madhya Pradesh	Jan - Mar 2000**	Mar 29, 2000 - Jan 26, 2001	Jan 2001 - Nov 2005	Nov 20, 2005
Chhattisgarh	2000	2000 - Jan 2001	Jan 2001- 2005	2005 (earliest mention)[51]
Maharashtra	2003***	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 -	

NOTES:

*For district councils in 1993 and for village and block councils in 1994.

**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.

***In retrospective effect from Sep 21, 2002.

Table 2: Summary statistics

Variable	Never treated		Treated			
			$Post_{st} = 0$		$Post_{st} = 1$	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)
Hindu	0.810	0.392	0.954	0.210	0.934	0.249
Muslim	0.166	0.372	0.038	0.190	0.038	0.190
Sikh	0.051	0.220	0.012	0.109	0.010	0.101
Christian	0.041	0.198	0.011	0.105	0.013	0.112
SC	0.182	0.386	0.161	0.368	0.193	0.395
ST	0.067	0.250	0.172	0.377	0.159	0.366
OBC	0.236	0.425	0.184	0.387	0.345	0.475
Wife's years of schooling:						
Zero	0.610	0.488	0.698	0.459	0.613	0.487
5-10 years	0.215	0.411	0.175	0.380	0.227	0.419
10 Ð 12 years	0.058	0.234	0.035	0.184	0.054	0.226
12-15 years	0.021	0.145	0.009	0.094	0.019	0.136
>= 15 years	0.011	0.105	0.004	0.067	0.010	0.098
Husband's years of schooling:						
Zero	0.334	0.472	0.374	0.484	0.338	0.473
5-10 years	0.311	0.463	0.308	0.462	0.327	0.469
10 Ð 12 years	0.136	0.343	0.133	0.340	0.130	0.336
12-15 years	0.068	0.252	0.044	0.205	0.060	0.238
>= 15 years	0.042	0.200	0.031	0.173	0.046	0.209
Low SLI	0.631	0.483	0.665	0.472	0.560	0.496
High SLI	0.083	0.276	0.065	0.246	0.096	0.294
Mother's age at birth	24.025	5.995	22.497	5.476	25.915	6.262
Birth = 1	0.227	0.419	0.243	0.429	0.160	0.367
N	683,764		328,638		130,086	
1st birth is male	0.515	0.500	0.515	0.500	0.511	0.500
N	52,005		27,960		6,058	
N (mothers)	60,181		33,350		6,273	

NOTES: $Post_{st} = 1$ for years $\geq$ announcement year of the law. SC, ST, and OBC indicate Scheduled Caste, Scheduled Tribe, and Other Backward Class women, respectively. Low and High SLI (standard of living index) are equal to one if the household belongs to the bottom-third or the top-third of household wealth distribution in all of India (i.e., rural as well as urban areas).

Table 3: Correlations between law announcements and socioeconomic variables

Dependent Variable ↓	**Coefficient of $Treat_{st}$** (1)	**Std. Error** (2)
SC	-0.004	[0.008]
ST	0.009	[0.008]
OBC	-0.008	[0.010]
Upper caste	0.003	[0.011]
Hindu	0.012	[0.009]
Muslim	0.003	[0.006]
Sikh	0.001	[0.002]
Christian	0.001	[0.007]
Low SLI	0.009	[0.008]
Med SLI	-0.001	[0.006]
High SLI	-0.007	[0.005]
Wife's years of schooling:		
Zero	-0.005	[0.007]
5-10 years	0.009	[0.010]
10-12 years	0.002	[0.002]
12-15 years	0.001	[0.004]
$\geq$ 15 years	-0.002	[0.002]
Husband's years of schooling:		
Zero	0.003	[0.008]
5-10 years	-0.002	[0.008]
10-12 years	-0.001	[0.003]
12-15 years	0.002	[0.005]
$\geq$ 15 years	-0.000	[0.003]
N	1,143,057	

NOTES: Each coefficient is from a separate regression that includes state, year, and state x mother's age fixed effects, and state-specific linear time trends. Standard errors are in brackets and are clustered by state. SC, ST, and OBC indicate Scheduled Caste, Scheduled Tribe, and Other Backward Class households, respectively. Low, Med, and High SLI (standard of living index) are equal to one if the household belongs to the bottom-third, middle-third, or the top-third of household wealth distribution in India. *** 1%, ** 5%, * 10%.

Table 4: Net effect on the hazard of third birth

3rd birth = 1	(1)	(2)	(3)
Panel A:			
$Treat_{st}$	-0.0143	-0.0213	-0.0206
	[0.0096]	[0.0088]**	[0.0078]**
	(0.0095)	(0.0098)**	(0.0093)**
Baseline mean		0.2131	
Panel B:			
$T_s * Post_{st}$	-0.0196	-0.0265	-0.0263
	[0.0114]	[0.0117]**	[0.0103]**
	(0.0117)	(0.0131)*	(0.0120)**
$Post_{st}$	0.0099	0.0077	0.0083
	[0.0101]	[0.0090]	[0.0082]
Baseline mean		0.2431	
N		182,082	
State FE	x	x	x
Year FE	x	x	x
Years since 2nd birth FE	x	x	x
X_{it}	x	x	x
Linear state trends		x	x
State x Age FE			x

NOTES: This table reports the coefficients from specifications (2a) and (2b). The outcome variable is an indicator for third birth. The sample is restricted to years after second birth and until third birth for mothers whose second child was born before the year of announcement. Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. The baseline mean is calculated for observations where $Treat_{st} = 0$ in panel A and for observations where $Post_{st} = 0$ in panel B. *** 1%, ** 5%, * 10%.

Table 5: Heterogeneity in the net effect on the hazard of third birth

3rd birth = 1	**SC**	**ST**	**OBC**	**Upper**	**Low SLI**	**High SLI**	**Wife has schooling**	**Wife has no schooling**	**Husband has schooling**	**Husband has no schooling**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A:										
$Treat_{st}$	-0.0410	0.0029	-0.0288	-0.0098	-0.0214	-0.0024	-0.0160	-0.0182	-0.0159	-0.0294
	[0.0126] ***	[0.0215]	[0.0104]**	[0.0124]	[0.0100]**	[0.0123]	[0.0103]	[0.0097]*	[0.0077]*	[0.0119]**
	(0.0168)***	(0.0207)	(0.0142)*	(0.0119)	(0.0098)*	(0.0119)	(0.0095)	(0.0103)	(0.0083)*	(0.0146)**
Baseline mean	0.2475	0.2521	0.1990	0.2015	0.2478	0.1143	0.1468	0.2629	0.1917	0.2604
Panel B:										
$T_s * Post_{st}$	-0.0467	-0.0094	-0.0252	-0.0182	-0.0296	-0.0062	-0.0227	-0.0246	-0.0221	-0.0336
	[0.0157]***	[0.0314]	[0.0117]**	[0.0162]	[0.0123]**	[0.0131]	[0.0114]*	[0.0119]*	[0.0099]**	[0.0171]*
	(0.0180)***	(0.0300)	(0.0132)*	(0.0162)	(0.0133)**	(0.0127)	(0.0119)*	(0.0126)*	(0.0107)**	(0.0178)*
$Post_{st}$	0.0083	0.0171	-0.0046	0.0125	0.0119	0.0054	0.0097	0.0090	0.0090	0.0058
	[0.0110]	[0.0286]	[0.0114]	[0.0122]	[0.0113]	[0.0100]	[0.0073]	[0.0114]	[0.0076]	[0.0147]
Baseline mean	0.2831	0.2690	0.2470	0.2257	0.2669	0.1543	0.1830	0.2777	0.2257	0.2769
N	28,074	17,868	38,288	97,852	110,159	17,200	72,165	109,917	122,979	59,103

NOTES: This table reports the coefficients from specifications (2a) and (2b). The outcome variable is an indicator for third birth. The sample is restricted to years after second birth and until third birth for mothers whose second child was born before the year of announcement. Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. SC, ST, OBC, and Upper indicate Scheduled Caste, Scheduled Tribe, Other Backward Class, and upper caste households, respectively. Low and High SLI (standard of living index) are equal to one if the household belongs to the bottom-third or the top-third of household wealth distribution in India. The last four columns split the sample into wives and husbands who have zero and non-zero years of schooling. The baseline mean is calculated for observations where $Treat_{st} = 0$ in panel A and for observations where $Post_{st} = 0$ in panel B. *** 1%, ** 5%, * 10%.

Table 6: Effects on the number of living children

	Kids = 1	**Kids = 2**	**Kids = 3**	**Kids = 4**	**Kids = 5**
	(1)	(2)	(3)	(4)	(5)
Panel A:			**Only treatment states**		
$Treat_{st}$	0.0066	0.0075	-0.0042	-0.0047	-0.0028
	[0.0039]	[0.0035]*	[0.0021]*	[0.0025]	[0.0013]*
	(0.0046)	(0.0042)*	(0.0023)*	(0.0030)	(0.0017)*
N			459,293		
Baseline mean	0.2394	0.2199	0.1693	0.0836	0.0322
Panel B:					
$Treat_{st}$	0.0008	0.0090	-0.0018	-0.0052	-0.0024
	[0.0055]	[0.0068]	[0.0055]	[0.0026]*	[0.0020]
	(0.0054)	(0.0065)	(0.0053)	(0.0030)*	(0.0053)
N			1,143,057		
Baseline mean	0.2351	0.2351	0.1711	0.0878	0.0379
Panel C:					
$T_s * Post_{st}$	0.0001	0.0139	-0.0009	-0.0080	-0.0014
	[0.0063]	[0.0100]	[0.0073]	[0.0037]**	[0.0026]
	(0.0062)	(0.0104)	(0.0072)	(0.0042)*	(0.0026)
$Post_{st}$	0.0009	-0.0067	-0.0013	0.0040	-0.0014
	[0.0043]	[0.0062]	[0.0037]	[0.0025]	[0.0026]
N			1,143,057		
Baseline mean	0.2425	0.2220	0.1650	0.0855	0.0364

NOTES: This table presents the regression estimates corresponding to specification (2a) in panels A and B, and to specification (2b) in panel C using indicators for 1/ 2/ 3/ 4/ 5 living children as the outcome variables. No sample restrictions are imposed, except that the sample is limited to treatment states in panel A. Each column within a panel is a different regression. Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. The baseline mean is calculated for observations where $Treat_{st} = 0$ in panels A and B, and for observations where $Post_{st} = 0$ in panel C. *** 1%, ** 5%, * 10%.

Table 7: Heterogeneous effects on the likelihood of ≥ 3 living children

	All	**SC**	**ST**	**OBC**	**Upper**	**Low SLI**	**High SLI**	**Wife has schooling**	**Wife has no schooling**	**Husband has schooling**	**Husband has no schooling**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Treatment states only											
$Treat_{st}$	-0.0133	0.0045	-0.0402	-0.0068	-0.0168	-0.0190	-0.0109	-0.0110	-0.0155	-0.0136	-0.0131
	[0.0043]**	[0.0126]	[0.0185]*	[0.0062]	[0.0118]	[0.0088]*	[0.0039]**	[0.0037]**	[0.0078]*	[0.0033]***	[0.0097]
	(0.0072)**	(0.0119)	(0.0276)**	(0.0062)	(0.0111)	(0.0135)	(0.0062)*	(0.0057)**	(0.0115)	(0.0063)***	(0.0125)
N	459,293	78,174	77,278	105,475	198,366	291,535	33,708	149,776	309,517	292,311	166,982
Baseline mean	0.3007	0.3124	0.3013	0.2860	0.3023	0.3064	0.2561	0.2674	0.3152	0.2938	0.3124
B. All states											
$Treat_{st}$	-0.0087	0.00002	-0.0206	-0.0097	-0.0141	-0.0100	-0.0111	-0.0121	-0.0081	-0.0110	-0.0049
	[0.0065]	[0.0108]	[0.0147]	[0.0068]	[0.0129]	[0.0064]	[0.0062]*	[0.0069]	[0.0063]	[0.0070]	[0.0067]
	(0.0069)	(0.0104)	(0.0194)	(0.0072)	(0.0133)	(0.0077)	(0.0074)	(0.0077)	(0.0073)	(0.0075)	(0.0073)
N	1,143,057	202,619	123,071	267,024	550,343	722,793	90,528	416,265	726,792	747,865	395,192
Baseline mean	0.3189	0.3399	0.3180	0.3129	0.3144	0.3317	0.2498	0.2592	0.3527	0.3027	0.3495

NOTES: This table presents the regression estimates corresponding to specification (2a) using an indicator for ≥ 3 living children in a given year as the outcome variable. No sample restrictions are imposed, except that the sample is limited to treatment states in panel A. Each coefficient is from a different regression. Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. SC, ST, OBC, and Upper indicate Scheduled Caste, Scheduled Tribe, Other Backward Class, and upper caste households, respectively. Low and High SLI (standard of living index) are equal to one if the household belongs to the bottom-third or the top-third of household wealth distribution in India. The last four columns split the sample into wives and husbands who have zero and non-zero years of schooling. The baseline mean is calculated for observations where $Treat_{st} = 0$. *** 1%, ** 5%, * 10%

Table 8: Sex ratio of second and higher parity births

Male = 1	**All**	**SC**	**ST**	**OBC**	**Upper**
	(1)	(2)	(3)	(4)	(5)
Panel A:			**Only treatment states**		
$Treat_{st}$	0.0086	-0.0265	0.0071	0.0528	-0.0048
	[0.0103]	[0.0419]	[0.0082]	[0.0111]***	[0.0210]
	(0.0111)	(0.0368)	(0.0066)	(0.0252)**	(0.0196)
N	61,490	11,054	11,627	12,677	26,132
Baseline mean	0.5211	0.5235	0.5142	0.5117	0.5267
Panel B:					
$Treat_{st}$	0.0109	-0.0249	0.0071	0.0557	0.0061
	[0.0060]*	[0.0232]	[0.0157]	[0.0119]***	[0.0148]
	(0.0078)	(0.0218)	(0.0154)	(0.0221)**	(0.0143)
N	165,016	31,169	18,757	35,858	79,232
Baseline mean	0.5186	0.5215	0.5177	0.5185	0.5178
Panel C:					
$T_s * Post_{st}$	0.0088	-0.0231	-0.0137	0.0570	0.0022
	[0.0078]	[0.0208]	[0.0241]	[0.0145]***	[0.0168]
	(0.0077)	(0.0201)	(0.0241)	(0.0202)***	(0.0164)
$Post_{st}$	0.0032	-0.0026	0.0288	-0.0017	0.0060
	[0.0082]	[0.0144]	[0.0272]	[0.0100]	[0.0123]
N	165,016	31,169	18,757	35,858	79,232
Baseline mean	0.5181	0.5214	0.5174	0.5184	0.5170

NOTES: This table reports the coefficients from specifications (3a) and (3b). The sample is restricted to second and higher order births to women whose first child was born before the law was announced in her state. Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. SC, ST, OBC, and Upper indicate Scheduled Caste, Scheduled Tribe, Other Backward Class, and upper caste households, respectively. In panels A and B, the baseline mean is calculated for observations where $Treat_{st} = 0$ and in panel C for observations where $Post_{st} = 0$. *** 1%, ** 5%, * 10%.

Table 9: Net effect on the hazard of third birth using alternate placebo treatment years for control states

	Treatment year assigned to control states:						
	1993	1994	1995	1996	1997	1998	1999
3rd birth = 1	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:							
$Treat_{st}$	-0.0205	-0.0200	-0.0212	-0.0232	-0.0241	-0.0244	-0.0223
	[0.0084]**	[0.0085]**	[0.0083]**	[0.0082]**	[0.0082]***	[0.0083]***	[0.0086]**
	(0.0102)**	(0.0098)*	(0.0097)**	(0.0098)**	(0.0101)**	(0.0102)**	(0.0103)**
Panel B:							
$T_s * Post_{st}$	-0.0222	-0.0210	-0.0213	-0.0238	-0.0230	-0.0221	-0.0341
	[0.0099]**	[0.0092]**	[0.0122]*	[0.0120]*	[0.0135]	[0.0166]	[0.0144]**
	(0.0118)**	(0.0102)**	(0.0129)	(0.0132)*	(0.0152)	(0.0181)	(0.0170)**
$Post_{st}$	0.0028	0.0015	0.0001	0.0007	-0.0013	-0.0030	0.0177
	[0.0115]	[0.0105]	[0.0127]	[0.0117]	[0.0117]	[0.0153]	[0.0130]
N	164,843	171,975	178,584	184,751	190,071	195,029	199,100

NOTES: This table reports the coefficients from specifications (2a) and (2b). Each column within a panel is a separate regression. All never-treated or control states are assigned the same "fake" or placebo treatment year that varies across columns. The outcome variable is an indicator for third birth. The sample is restricted to years after second birth and until third birth for mothers whose second child was born before the year of announcement. Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. *** 1%, ** 5%, * 10%.

Table 10: Net effect on the hazard of third birth using all neighboring states as control

3rd birth = 1	(1)	(2)	(3)
Panel A:			
$Treat_{st}$	-0.0149	-0.0199	-0.0188
	[0.0097]	[0.0088]**	[0.0079]**
	(0.0093)	(0.0103)*	(0.0090)**
Panel B:			
$T_s * Post_{st}$	-0.0170	-0.0216	-0.0217
	[0.0100]	[0.0092]**	[0.0083]**
	(0.0100)	(0.0107)*	(0.0097)**
$Post_{st}$	0.0061	0.0031	0.0051
	[0.0042]	[0.0035]	[0.0035]
N		292,514	
State FE	x	x	x
Year FE	x	x	x
Years since 2nd birth FE	x	x	x
X_{it}	x	x	x
Linear state trends		x	x
State x Age FE			x

NOTES: This table reports the results from the final robustness check described in Section 6 that uses all neighboring states as control group for each treatment state. The outcome variable is an indicator for third birth. The sample is restricted to years after second birth and until third birth for mothers whose second child was born before the year of announcement. Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. *** 1%, ** 5%, * 10%.

A Appendix Figures and Tables

Table A.1: Village council members disqualified during 2000-04, for selected states

State	**Number of disqualifications** (excluding rejected nominations)
Haryana	1,350
Rajasthan	548
Madhya Pradesh	1,140
Chhattisgarh	766
Andhra Pradesh	94*

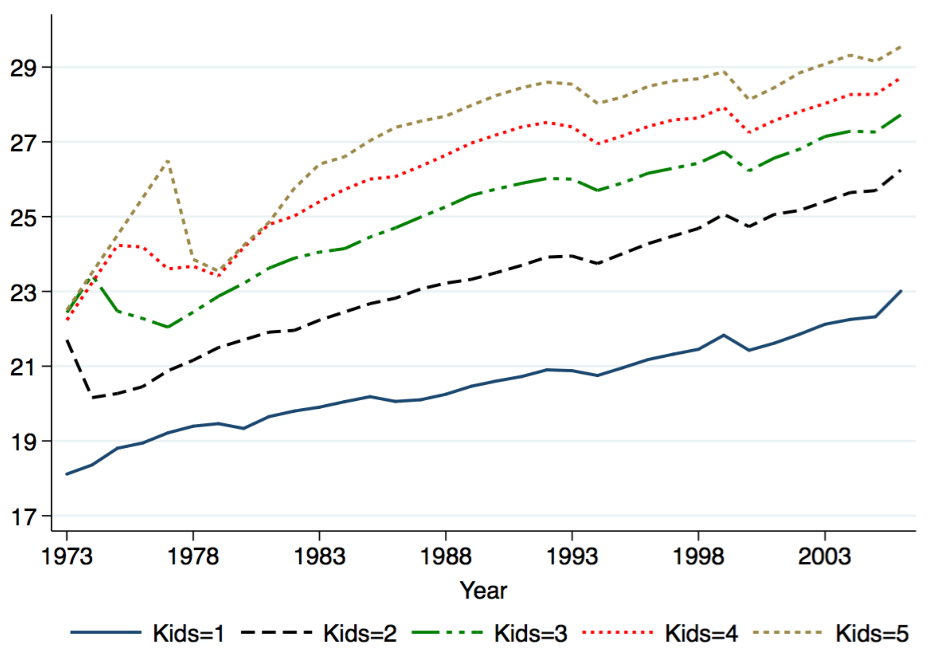
NOTES: *Data available for 15 out of 23 districts. Source: Buch (2005) and Visaria et al. (2006).

Table A.2: Timeline of village council elections

State	**Election Years**	
	Without the limits	With the limits
Rajasthan	1995	2000, 2005, 2010, 2015
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*, 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

NOTES: *Although the fertility limits were officially introduced after the elections were over in 2000, the new government began disqualifying elected representatives earlier (Visaria et al. (2006)).

Figure A.1: Average mother's age at birth, by year and number of living children



NOTES: This graph plots the trends in average age of mothers who have a given number of children in a year. The sample is restricted to mothers $\leq$ age 33 in a given year.

Table A.3: Effects on hazards of birth

Coefficients of $T_s * Post_{s,t+k}$	Outcome: Birth = 1 **1st** (1)	 **2nd** (2)	 **3rd** (3)	 **4th** (4)
$t-6$	0.0174	0.0173	0.0062	-0.0068
	[0.0249]	[0.0259]	[0.0226]	[0.0274]
$t-5$	0.0088	0.0111	-0.0161	0.0029
	[0.0204]	[0.0189]	[0.0143]	[0.0189]
$t-4$	0.0136	0.0247	-0.0019	0.0013
	[0.0177]	[0.0197]	[0.0140]	[0.0141]
$t-3$	-0.0144	0.0141	-0.0156	-0.0018
	[0.0225]	[0.0184]	[0.0142]	[0.0115]
$t-2$	0.0129	0.0238	-0.0132	-0.0059
	[0.0185]	[0.0168]	[0.0137]	[0.0116]
$t-1$	0.0182	0.0163	-0.019	-0.0069
	[0.0272]	[0.0245]	[0.0201]	[0.0157]
t	0	0	0	0
$t+1$	0.0119	0.0670**	-0.0283	-0.0093
	[0.0220]	[0.0268]	[0.0218]	[0.0286]
$t+2$	0.0036	0.028	-0.0197	-0.007
	[0.0185]	[0.0270]	[0.0242]	[0.0125]
$t+3$	0.0142	-0.0085	-0.0275**	-0.0055
	[0.0209]	[0.0255]	[0.0112]	[0.0125]
$t+4$	-0.0024	-0.0117	-0.0348***	-0.0089
	[0.0212]	[0.0270]	[0.0114]	[0.0135]
$t+5$	0.0016	-0.0348	-0.0408**	-0.0047
	[0.0179]	[0.0342]	[0.0159]	[0.0133]
N	121,093	77,765	88,476	74,473

NOTES: This table presents the regression estimates corresponding to Figure 2 or specification (1). Each column is from a different regression. The outcome variables are indicators for births of various orders. For the regression where birth of order b is the dependent variable, the sample is restricted to years after birth $(b-1)$ and until birth b for mothers whose $(b-1)^{th}$ child was born before the year of announcement. Standard errors in brackets are clustered by state. The year before announcement is the omitted year. *** 1%, ** 5%, * 10%.

Table A.4: Net effects on birth hazards

	(1)	(2)	(3)
A. 1st birth = 1			
$Treat_{st}$	0.0008	0.0004	-0.0006
	[0.0062]	[0.0062]	[0.0060]
	(0.0059)	(0.0061)	(0.0058)
$T_s * Post_{st}$	0.0063	-0.0004	-0.0017
	[0.0069]	[0.0101]	[0.0099]
	(0.0067)	(0.0097)	(0.0098)
$Post_{st}$	-0.0121	0.0013	0.0017
	[0.0088]	[0.0099]	[0.0101]
N		323,174	
B. 2nd birth = 1			
$Treat_{st}$	0.0057	-0.0011	0.0015
	[0.0094]	[0.0120]	[0.0104]
	(0.0099)	(0.0114)	(0.0108)
$T_s * Post_{st}$	0.0130	-0.0020	0.0023
	[0.0121]	[0.0164]	[0.0147]
	(0.0127)	(0.0155)	(0.0143)
$Post_{st}$	-0.0113	0.0014	-0.0013
	[0.0117]	[0.0160]	[0.0155]
N		186,219	
C. 4th birth = 1			
$Treat_{st}$	0.0066	0.0003	-0.0019
	[0.0093]	[0.0054]	[0.0042]
	(0.0092)	(0.0054)	(0.0041)
$T_s * Post_{st}$	0.0004	-0.0040	-0.0077
	[0.0116]	[0.0089]	[0.0077]
	(0.0110)	(0.0090)	(0.0078)
$Post_{st}$	0.0114	0.0066	0.0087
	[0.0071]	[0.0074]	[0.0069]
N		139,408	
State FE	x	x	x
Year FE	x	x	x
Years since last birth FE	x	x	x
X_{it}	x	x	x
Linear state trends		x	x
State x Age FE			x

NOTES: This table reports the coefficients of $Treat_{st}$ from specifications (2a) and (2b). The outcome variables are indicators for births of various orders. For regressions where birth of order b is the dependent variable, the sample is restricted to years after birth $(b-1)$ and until birth b for mothers whose $(b-1)^{th}$ child was born before the year of announcement. In Panel A, FE for years since last birth are replaced with FE for years since marriage. Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. *** 1%, ** 5%, * 10%.

Table A.5: Effects on the number of living children, mother's age ≤ 33

	Kids = 1	**Kids = 2**	**Kids = 3**	**Kids = 4**	**Kids = 5**
	(1)	(2)	(3)	(4)	(5)
Panel A:					
$Treat_{st}$	0.0030	0.0091	-0.0021	-0.0063**	-0.0024**
	[0.0061]	[0.0061]	[0.0057]	[0.0022]***	[0.0020]
	(0.0022)	(0.0021)	(0.0058)	(0.0031)	(0.0030)
Baseline mean	0.2462	0.2350	0.1651	0.0812	0.0332
Panel B:					
$T_s * Post_{st}$	0.0029	0.0125	0.0009	-0.0088	-0.0014
	[0.0069]	[0.0095]	[0.0077]	[0.0035]**	[0.0028]
	(0.0067)	(0.0097)	(0.0076)	(0.0042)**	(0.0028)
$Post_{st}$	0.0001	-0.0048	0.0040	0.0036	-0.0013
	[0.0046]	[0.0064]	[0.0025]	[0.0030]	[0.0029]
Baseline mean	0.2508	0.2241	0.1609	0.0795	0.0322
N			1,060,282		

NOTES: This table presents the regression estimates corresponding to specification (2a) in panel A and to specification (2b) in panel B using indicators for 1/ 2/ 3/ 4/ 5 living children as the outcome variables. No sample restrictions are imposed, except that the sample is limited to women ≤ 33 years of age in a given year. Each column within a panel is a different regression. Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. The baseline mean is calculated for observations where $Treat_{st} = 0$ in panel A and for observations where $Post_{st} = 0$ in panel B. *** 1%, ** 5%, * 10%.

Table A.6: Effects on the sex ratio of first births

Male = 1	**All** (1)	**SC** (2)	**ST** (3)	**OBC** (4)	**Upper** (5)
Panel A:			**Only treatment states**		
$Treat_{st}$	-0.0081	0.0334	0.0120	-0.0474	0.0081
	[0.0110]	[0.0332]	[0.0445]	[0.0401]	[0.0217]
	34,018	5,818	5,783	7,511	14,906
Baseline mean	0.5152	0.5062	0.5103	0.5162	0.5198
Panel B:					
$Treat_{st}$	-0.0007	0.0325	-0.0063	-0.0304	0.0093
	[0.0076]	[0.0295]	[0.0339]	[0.0228]	[0.0162]
	86,023	15,245	9,265	19,345	42,168
Baseline mean	0.5150	0.5128	0.5096	0.5173	0.5158
Panel C:					
$T_s * Post_{st}$	-0.0042	0.0179	-0.0367	-0.0263	0.0092
	[0.0122]	[0.0379]	[0.0486]	[0.0219]	[0.0199]
$Post_{st}$	0.0056	0.0226	0.0435	-0.0053	0.0003
	[0.0126]	[0.0275]	[0.0491]	[0.0175]	[0.0185]
N	86,023	15,245	9,265	19,345	42,168
Baseline mean	0.5144	0.5224	0.5056	0.5151	0.5128

NOTES: This table reports the coefficients from specifications (2a) and (2b). The sample is restricted to first births and the outcome variable is an indicator for the birth being male. Standard errors in brackets are clustered by state. SC, ST, OBC, and Upper indicate Scheduled Caste, Scheduled Tribe, Other Backward Class, and upper caste households, respectively. In panels A and B, the baseline mean is calculated for observations where $Treat_{st} = 0$ and in panel C for observations where $Post_{st} = 0$. *** 1%, ** 5%, * 10%.

Table A.7: Effects on contraceptive use

Dep var: Currently using a modern method of contraception						
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{st}$	0.083	0.040	0.038	0.044	0.018	-0.014
	[0.020]***	[0.018]**	[0.019]*	[0.025]*	[0.024]	[0.017]
	(0.035)***	(0.022)*	(0.024)*	(0.031)	(0.023)	(0.017)
N		52,951		24,281	20,911	16,633
Baseline mean		0.473		0.447	0.548	0.115
Year of Interview FE	x	x	x	x	x	x
State FE	x	x	x	x	x	x
X_{ist}		x	x	x	x	x
Years since last birth FE		x	x	x	x	x
State x Age FE			x	x	x	x
Linear state trends			x	x	x	x

NOTES: This table reports the coefficients from specification (2a) where the time subscript refers to the year of interview. Modern methods of contraception comprise, male or female sterilization, pills, condoms, intrauterine devices, diaphragms, and injections. In columns (1)-(3), the sample is restricted to years after the year of second birth for women whose second birth took place before the limit was announced in her state. In columns (4), (5), and (6), the sample is restricted to women who respectively had two, three, and one living child in the year of interview. In column (6), FE for years since last birth are replaced with FE for years since marriage. Standard errors in brackets are clustered by state. The baseline mean is calculated for observations where $Treat_{st} = 0$. *** 1%, ** 5%, * 10%.

B State-wise Regulations

1. Rajasthan:[52]

According to the the Rajasthan Panchayati Raj Act, 1994, "...Every person registered as a voter in the list of voters of a Panchayati Raj Institution shall be qualified for election as a Panch or, as the case may be, a member of such Panchayati Raj Institution unless such person-...(l) has more than two children."..."The birth during the period from the date of commencement of the Act (23rd April, 1994), hereinafter in this proviso referred to as the date of such commencement, to 27th November, 1995, of an additional child shall not be taken into consideration for the purpose of the disqualification mentioned in Clause (l) and a person having more than two children (excluding the child, if any, born during the period from the date of such commencement to 27th November, 1995) shall not be disqualified under that clause for so long as the number of children he had on the date of commencement of this Act does not increase."

2. Haryana:

According to the 1994 Act[53], "...No person shall be a Sarpanch or a Panch or a Gram Panchayat or a member of a Panchayat Samiti or Zila Parishad or continue as such who– (q) has more than two living children: Provided that a person having more than two children on or upto the expiry or one year of the commencement of this Act, shall not be deemed to be disqualified."

Prior to revocation:[54] "Person shall be disqualified for being elected to a Gram Panchayat, Panchayat Samiti or Zila Parishad if:

...(xvii) has more than two living children; provided that this disqualification of more than two living children shall not apply for the persons who had more than two living children before 21st April, 1995 unless he had additional child after the said date."

The Haryana government amended Section 175(q) of the Haryana Panchayati Raj Act, 1994, retrospectively with effect from January 1, 2005 to omit the section (q).[55]

[52] Source: `http://www.rajpanchayat.gov.in/common/toplinks/act/act.pdf`

[53] Source: `http://www.panchayat.gov.in/documents/10198/350801/The%20Haryana%20Panchayati%20%20Raj%20Act%201994.pdf`

[54] Source: http://secharyana.gov.in/html/faq1.htm

[55] Source: http://hindu.com/2006/07/22/stories/2006072207150500.htm

3. Andhra Pradesh:[56]

According to Section 19 (3) of the Andhra Pradesh Panchayati Raj Act, 1994,"...A person having more than two children shall be disqualified for election or for continuing as member:

Provided that the birth within one year from the date of commencement of the Andhra Pradesh Panchayat Raj Act, 1994 hereinafter in this clause referred to as the date of such commencement, of an additional child shall not be taken into consideration for the purposes of this clause;

Provided further that a person having more than two children (excluding the child if any born within one year from the date of such commencement) shall not be disqualified under this clause for so long as the number of children he had on the date of such commencement does not increase;

Provided also that the Government may direct that the disqualification in this section shall not apply in respect of a person for reasons to be recorded in writing."[57]

4. Orissa:[58]

A person shall be disqualified for being elected to a PR institution if he "...has more than one spouse living or has more than two children. The last named disqualification shall not apply if the person had had more than two children before 21.04.1995 unless he begot an additional child after the said date. Rule 25 of O.G.P. Act gives full description of the disqualifications."

5. Madhya Pradesh:[59]

"...condition to disqualify an office bearer of the Panchayat for holding the post: (1) that he must have more than two living children, and (2) out of whom one is born on or after the 26th day of January, 2001..."

The Population Policy of Madhya Pradesh states that "persons having more than two children after January 26, 2001 would not be eligible for contesting elections for *panchayats*, local bodies, *mandis* or cooperatives in the state. In case they get elected, and in the meantime they have the third child, they would be disqualified for that post."

6. Chhattisgarh:[60]

[56]Source: http://www.ielrc.org/content/e9412.pdf

[57]Further explanation at: http://www.apsec.gov.in/RLBS_GPs/CLARIFICATIONS%202013/877%20-%20Qualification.pdf.

[58]Source: http://secorissa.org/download/FAQ2.pdf

[59]Source: http://www.indiankanoon.org/doc/1285129/

[60]Source: http://www.the-laws.com/Encyclopedia/Browse/ShowCase.aspx?CaseId=023002211000

“Section 36: Disqualification for being office bearer of Panchayat:- 36(1) No person shall be eligible to be an office bearer of Panchayat who:...(m) has more than two living children one of whom is born on or after the 26th day of January, 2001.”

7. Maharashtra:

“...(j-1) No person shall be a member of a Panchayat or continue as such, who has more than two children:

Provided that, a person having two children on the date of commencement of the Bombay Village Panchayats and the Maharashtra Zila Parishads and Panchayat Samitis (Amendment) Act 1995 (hereinafter in this clause referred to as “the date of such commencement”) shall not be disqualified under this clause so long as the number of children he had on the date of such commencement does not increase;

Provided further that, a child or more than one child born in a single delivery within the period of one year from the date of such commencement shall not be taken into consideration for the purpose of disqualification mentioned in this clause.

... For the purposes of clause (j-1):

Where the couple has only one child on or after that date of such commencement, any number of children born out of a single subsequent delivery shall be deemed to be one entity.

“Child” does not include an adopted child or children....”