Political Aspirations in India: Evidence from Fertility Limits on Local Leaders*

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Abstract

Aspirations are a key correlate of economic mobility but are difficult to measure. We use a novel approach to quantify *political* aspirations in India by estimating individuals' willingness to trade-off family size for political office. Many Indian states bar individuals from contesting village council elections if their fertility exceeds the legal limits. We find that at least 3.65 million households altered their fertility due to the limits. This effect is not driven by the role-model influence of leaders but instead reflects strong leadership ambitions of Indian citizens. Thus policymakers in developing countries should take into account significant latent political aspirations for more effective policy-design.

JEL Codes: J13, J16, H75, O11

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1 Introduction

Aspirations failure is increasingly acknowledged as not just a consequence but also a cause of poverty. Poverty restricts one's "aspirations window," i.e., the set of visible successful people one can hope to feasibly emulate. This is due, among other things, to lack of information, incomplete markets, or simply pessimistic perceptions regarding social mobility. These constraints on the "capacity to aspire" foster fatalism, which reinforces poverty in a self-perpetuating cycle by eroding the incentives of the poor to invest in changing their circumstances (Appadurai (2004), Ray (2006)). Recent work has explored the link between aspirations failure and occupational segregation (Mookherjee et al. (2010)), productive investments (Macours and Vakis (2009), Bernard et al. (2011), Genicot and Ray (2014)), and poverty traps (Dalton et al. (2014)). However, little is known about the political ambitions of citizens, especially in low-income countries. We measure political aspirations in India by estimating individuals' willingness to trade-off fertility for eligibility to hold political office.

We analyze the impact of novel state-level laws in India that bar individuals with more than two children from contesting local (*Panchayat*) elections on fertility-related outcomes. The Panchayat system 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. 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.[1]

We exploit the quasi-experimental geographical and temporal variation in announcement of these laws to estimate their causal impacts on demographic outcomes of *the constituents*.

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

These effects may be directly driven by individuals' desire to maintain eligibility for Panchayat membership ("aspirations channel"). Alternatively, 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").[2]

We find that the fertility limits decrease the likelihood that a woman has more than two children in any given year after the grace-period by 6.84%. However, this fertility decline is preceded by a 67% increase in the probability of a third birth during the grace-period. This pattern of results is unlikely to be driven by the role-model channel, which requires sufficient time to pass after the law's announcement for constituents to observe and emulate their leaders' fertility decisions. Instead, the significant fertility increase during the grace-period and the immediate decline thereafter are more plausibly attributable to individuals attempting to have a third child during the grace-period without sacrificing eligibility for future elections. Individuals hence appear to be more strongly driven by their own leadership aspirations than their leaders' actions. We also find a significant fertility response in lower-caste households, which implies that political aspirations are strong even among historically under-represented groups.[3]

In addition, the fertility limits adversely affect the sex ratio, increasing the number of missing girls. Due to these laws, upper-caste families with firstborn girls are less likely to have a second birth, and if they do, it is more likely to be male. This decline in second births can be explained by increased sex-selection in favor of sons, as each abortion delays the next birth at least by a year (Bhalotra and Cochrane (2010)). Thus, among upper-caste couples whose first child is born before announcement of the law, those wishing to maintain

[2] The role-model channel appears to be the primary mechanism the policymakers had in mind when these laws were enacted. For example, `http://www.nytimes.com/2003/11/07/world/states-in-india-take-new-steps-to-limit-births.html`. In general, individuals in positions of authority do exert considerable influence on their followers' behaviors and outcomes (Bettinger and Long (2005), Jensen and Oster (2009), Chong et al. (2012), Olivetti et al. (2013), and Bassi and Rasul (2014)). Beaman et al. (2012) find that female Panchayat leaders raise the political aspirations and educational attainment of younger female constituents by serving as role-models.

[3] The political aspirations of lower-castes may have been strengthened by caste-based affirmative action.

eligibility restrict their fertility to two children, but ensure that the second child is male if the first is not. There is no effect on the sex ratio of second births for the lower-castes.

According to our estimates, at least 2.21% of the population aged 15-44 in the states that have enacted these laws, i.e., more than 3.65 million individuals, changed their fertility to remain eligible for Panchayat membership.[4] These impacts are large and consistent with the number of people of childbearing age who contest Panchayat elections in each cycle.[5] If we also take into account (i) individuals who consider running but do not actually file nominations, (ii) that each election has some new candidates, and (iii) that the "treatment" states have had 3-4 elections thus far, the number of affected individuals is even larger. Our estimated effects also reflect the importance of Panchayat membership in India, and the deep involvement of Indian citizens in democratic politics. Voter turnout in Panchayat elections routinely exceeds 70%. In the 2014 World Values Survey, 53% of the respondents (69% among the "lower class") say that politics is "very important" or "rather important" in their life and about 48% of the respondents are members of a political party.[6]

Much of the research on political participation examines the effects of quotas on the representation of disadvantaged groups (e.g., Chattopadhyay and Duflo (2004), Bhalotra et al. (2013), Kapoor and Ravi (2014)). Rigorous assessments of citizens' desire to participate in a democratic polity as candidates are rare. By examining the willingness to trade-off family size for political office, we provide the first estimates of political aspirations in the literature. Thus our results have important implications for the understanding of behavioral barriers to poverty reduction. Our paper also contributes to two other literatures: (i) on the relationship

[4] This estimate is based on data from the 2001 Census of India.

[5] Assuming two candidates per seat, in each election cycle the fertility limits directly target at least 2% of the childbearing population. Typically, a village Panchayat has 5-15 elected members. The treatment states in our sample (details in Section 3) had 912,597 seats across all three tiers of the Panchayat system in 2004. We assume that 30% of the population is of childbearing age; this is the minimum population share of the 20-39 age-group in the 2001 Census of India for our treatment states.

[6] About 72% 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% of households a member attended a public meeting called by the local council in the last year and in 10% of households someone from or close to the household is a member of the local council.

between fertility and career decisions and (ii) on the determinants of sex ratios. Improvements in labor market opportunities, especially for women, increase the opportunity cost of having children and thereby lower fertility (Chiappori et al. (2002), Rosenzweig and Wolpin (1980)). We examine a similar relationship between fertility and *political* careers where the change in the opportunity cost of children is caused by the two-child limits. Recent papers have also 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 second 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 our data and empirical strategy. Section 5 presents the estimation results. Section 6 conducts some robustness checks and Section 7 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 states enacted the two-child laws for Panchayat candidates.[7] These laws aim to lower fertility through the role-model channel. However, they 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 Panchayat 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

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

substantial funds from national and state governments,[8] and are authorized to implement development schemes.[9] Panchayats 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 monthly salary of a Panchayat chief is about USD 50 - USD 60; other council members are paid less.

The average population per village Panchayat is about 3,100, although the size varies widely. The minimum age to contest elections is 21 years. There are no term limits on Panchayat members. In Rajasthan and Uttar Pradesh, respectively, 19% and 33% of council chiefs were under 36 years old and 56% and 51% were in the 36-50 year age-group.[10] The council members are typically younger: 47% of Panchayat members in 2012 in Rajasthan were under 36 years of age and 41% were in the 36-50 year age-group. The PR Act requires that at least one-third of all member and chief positions are reserved for women.[11] Similarly, positions are reserved for Scheduled Castes (SC) and Scheduled Tribes (ST) in proportion to their population share. Quotas are implemented in a stratified manner—among positions reserved for SC, ST, and "general" castes, one-third are randomly chosen for women.

Rajasthan was the first state to introduce the two-child limit for its village councils in 1992;[12] this requirement was later included in the state's 1994 PR Act.[13] The governments of Andhra Pradesh and Haryana announced their legislations in 1994,[14] although the latter revoked its law in 2006. Orissa announced the limit for its district councils in 1993 and for

[8] For example, in Tamil Nadu, all Panchayats 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).

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

[10] 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)).

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

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

[13] The 1994 Act included a grace-period from April 23, 1994 to November 27, 1995. Effectively, this resulted in a nearly three-year grace-period since the original announcement was made in 1992.

[14] 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.

the village and block councils in 1994. Himachal Pradesh (HP), Madhya Pradesh (MP), and Chhattisgarh[15] 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[16] and Table A.1 shows the election years for which they were effective. The relevant clauses from each state's PR Act are presented in Section B.

Candidates do not have to explicitly state their number of children when filing their nomination papers. However, they have to declare that, to the best of their knowledge, they are qualified for the Panchayat seat. The Returning Officer (nominated by the Election Commission) is responsible for scrutinizing the information submitted by the nominees and any objections raised by the rival candidates, general public, or the media. Table 2 shows the number of Panchayat members that have been disqualified under these laws in Haryana, Rajasthan, MP, and AP during 2000-2004.[17]

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.[18] 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 Panchayat members for at least a few years and they remain in effect in seven states.

[15] 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.

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

[17] Data for the remaining states and years is not readily available and is being collected by the authors.

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

3 Data

We utilize three cross-sectional rounds of the National Family Health Survey (NFHS-1, 2, 3) and one round of the District-Level Household Survey (DLHS-2) of India.[19] Each round is representative at the state-level and includes a complete retrospective birth history for the woman interviewed, 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;[20] 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 15-44 age-group who were married at the time of survey.[21] 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.

Our final sample comprises 511,542 women and 1,261,711 births from 18 major states[22] 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. Since the most recent year in our sample is 2006, we cannot credibly examine the effect of revo-

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

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

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

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

cations that took place in 2005.[23] However, we have a large number of post-announcement years, ranging from 4 to 13 years, to estimate the relatively long-term effect of the fertility limits.

Table 3 displays the years we use for defining the treatment period for each affected state. Table 4 presents the sample means and standard deviations for the key variables used in our analyses, separately for never-treated and treated states. We further split the treated sample into pre- and post-treatment observations. About two-thirds of women in our sample live in a rural area. A majority of them are Hindus, with a larger share (90%) among treated relative to never-treated households (79%). In terms of caste-composition, upper-castes and other backward classes (OBC) comprise about 40% and 35% of the sample, while the rest are SC or ST. Educational attainment of women is low, with more than half the sample being uneducated; in comparison, 29% of the husbands are uneducated. Women in the post-treatment group are less likely to give birth and are more likely to have two children in a given year relative to women in the never-treated and pre-treatment sub-samples. The pre-treatment average terminal fertility (as measured by fertility of women more than 40 years old) in treated states is 2.8.

The sample means for the three groups in Table 4 are similar along practically all socioeconomic dimensions. Nevertheless, to ensure that our estimates are not confounded by underlying differences between these samples, we control for religion, caste, standard of living, husband's and wife's years of schooling, and residence in an urban area in all regressions. To take into account state-specific factors, we include state fixed effects and state-specific linear time trends (or state-year fixed effects). We also conduct several robustness checks to establish that our estimates capture the causal effect of the fertility limits.

[23] The only other source of demographic data after 2006 is the National Sample Survey (NSS) of India. However, the household roster in the NSS does not match mothers with their children.

4 Empirical Strategy

The goal of our empirical strategy is to estimate the causal effect of the fertility limits on local politicians in a state on fertility-related outcomes among residents in the same state. To do so, we utilize the quasi-experimental geographical and temporal variation in 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. The limits came into effect in Bihar and Gujarat after 2006, so in our sample these states are not treated. Although Uttarakhand announced its law for urban municipal elections in 2002, our analyses exclude it from the group of treatment states because Uttarakhand was a part of Uttar Pradesh until 2000 and we cannot distinguish between the two in the pre-2000 sample.[24] Our results, however, are robust to the exclusion of Uttar Pradesh. In addition to Bihar, Gujarat, and Uttarakhand, our control group comprises nine other states. Figure 1 depicts the treatment and control states in a map.

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

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

where $Treat_{st}$ is equal to one for women residing in the treated states if $t >$ the year of announcement, and zero otherwise; γ_s, θ_t, and ψ_a are fixed effects for state, year, and woman's age, respectively. We also control for state-specific linear time trends ($\nu_s * t$), state-mother's age fixed effects (μ_{sa}), and the following covariates (X_i): five categories each for a woman's

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

and her husband's years of schooling, indicators for the religion (five categories), caste (four categories), and the standard of living (three categories) of the household, residence in an urban area, and indicators for the year of interview.

The outcome variable is an indicator for a third birth. We restrict the sample to women who have at least two children and to years after the second birth. For the treatment states, we further restrict the sample to women whose first two children are born $before$ the law is announced in their state—thus, $Treat_{st}$ is zero for post-second birth years before the announcement and equal to one thereafter. Since the law is never announced in the control states, effectively all children in these states are born "before the law is announced," so $Treat_{st}$ is zero for all years for the women in control states. We also re-estimate (1) excluding the control states entirely as $Treat_{st}$ varies only for the treatment states. The coefficient of interest is β_1, which measures the effect of the two-child limits on the likelihood of a third birth.

The two-child laws may also affect second births for couples who have one child at announcement. For instance, if son preference is strong, couples who have one daughter when the law is announced may be more likely to practice sex-selection at second parity due to the two-child limit, which might delay their second birth. In addition to a DD specification similar to (1) for second births,[25] we estimate a triple-difference (DDD) specification by interacting $Treat_{st}$ with an indicator for whether the first child (born before treatment) is a girl ($Girl_i$):

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

The outcome variables are indicators for a second birth and, conditional on birth, the

[25] As earlier, we restrict the sample to women who have at least one child and to years after the first birth. For the treatment states, we further restrict the sample to women whose first child was born $before$ the law is announced in their state and $Treat_{st}$ is zero for all years for the women in control states.

likelihood that the child is male. The coefficient ϕ estimates the effect of the two-child laws on couples whose firstborn is a boy, while β_2 estimates the differential effect on couples whose firstborn is a girl. Prior literature on India has shown that, despite the availability of prenatal sex-determination technology, sex of the first birth is plausibly random (Bhalotra and Cochrane (2010)) and most instances of sex-selection occur for higher-order births.[26] In fact, Table 4 shows that the sex ratio at first birth in our sample is "normal" (i.e., between 0.515 and 0.525) in the never-treated states and in the treatment states (both pre- and post-treatment). Therefore, it is reasonable to compare the second-parity outcomes of couples with a firstborn son and couples with a firstborn daughter. Again, we restrict the sample to women whose first child is born before the year of treatment.

The inclusion of state and year fixed effects controls for any time-invariant state-level variables and state-invariant overall time trends that might affect our fertility outcomes. The state-specific time trends account for differential linear trends in fertility patterns across states over the sample period. In some specifications, we replace state-specific trends with state-year fixed effects to check the robustness of our estimates. We cluster standard errors at the state level when both treated and never-treated states are included in the sample. In specifications where the sample is restricted to only the treated states, we cluster at the state-year level to avoid econometric issues pertaining to a small number of clusters. For our key results, we also report standard errors based on a clustered wild bootstrap-t procedure explored in Cameron et al. (2008).[27]

Our underlying identifying assumption is that the state-year variation in the timing of law announcement is uncorrelated with other time-varying determinants of the outcomes of interest. In addition to controlling for state-specific linear trends in our regressions, in the

[26]However, Anukriti (2014) finds that this is not true for first births in Haryana after 2002 when firstborn children are *more* likely to be male due to the *Devirupak* scheme. Therefore, we drop post-2002 observations for Haryana from our sample while estimating (2).

[27]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.

next section we show that there are no significant differences in pre-treatment trends for our treatment and control groups. This supports our identifying assumption that the treatment and comparison women would have had similar trends in fertility rates in the absence of the two-child limits. Moreover, in Section 6 we show that the timing of announcement is uncorrelated with other socioeconomic characteristics that vary by state and time. Lastly, 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

5.1 Event-Study Analysis

We first present graphical evidence for the effect of the fertility limits. In Figure 2 we focus only on treated states and use an event-study framework to depict the evolution of the likelihood that a woman has more than two living children in a given year. The plotted coefficients show the differential trend in the likelihood of having more than two living children for women in treatment and control groups. Specifically, the figure plots the β_k coefficients from the following regression:

$$Y_{isat} = \sum_{k=-10}^{10} \beta_k Treat_{s,t+k} + X_i'\delta + \gamma_s + \theta_t + \psi_a + \mu_{sa} + \epsilon_{isat} \quad (3)$$

where $Treat_{s,t+k}$ indicates k years from the announcement of the law in state s and we control for socioeconomic characteristics of the woman and fixed effects for state, year, woman's age, and state-age. We examine the differential trends over ten years before and ten years after the year of announcement (which is the omitted year). The 95% confidence intervals are plotted from standard errors clustered by state-year.

There are no noticeable trends in the differential likelihood of having more than two living children in the pre-treatment years in Figure 2. The regression estimates in Table 5 verify that these coefficients are nearly all statistically insignificant during these years. This lack of differences in the pre-treatment years provides an important test for the validity of our identifying assumption; the trends in outcomes across comparison groups evolve smoothly except through the change in incentives for births in the treatment year.

After the fertility limits are announced, there is a sharp increase (17.9 percentage points or 67%) in the probability that a woman reports having more than two living children during the one-year grace-period in Figure 2. However, once the grace-period ends, the probability of having more than two living children starts declining sharply and drops below pre-announcement levels within three years after the grace-period, and declines further in the following years. The fertility drop is significant in every post-treatment year after the grace-period up to ten years after the law is announced, with a maximum decrease of about 14 percentage points in the sixth year.

Since there are only seven treatment states in this sample, we also conduct inference using a distribution of placebo treatment effects as outlined in Abadie and Gardeazabal (2003), Bertrand et al. (2004), and Abadie et al. (2010). We randomly assign treatment years within the period 1992-2003 to each state 900 times, and then estimate specification (3) for each of these treatments to create a distribution of placebo effects. We then compare the estimated impact of the "true" treatment relative to this distribution to ascertain if it is statistically significant.[28] As shown in Figure A.1, the grace-period treatment effect lies well outside the distribution of grace-period placebo effects, verifying that the 67% increase in the probability of having more than two living children during this year is highly significant. The same is true for the decline in this probability once the law is in effect.

Given that average baseline terminal fertility in treated states is 2.8, these two-child limits most likely imposed a binding constraint on the fertility of the majority of individuals with two children in these states. It then follows that practically every such person who

[28] Additionally, we conduct wild cluster bootstrapping at the state-level for specification (3), albeit without controlling for state-age fixed effects to ease the computational burden. The results remain the same and are shown in Table A.2.

wishes to remain eligible for election would attempt to have another child in the grace-period. According to the 2001 Census of India, the number of women in our treatment states that had exactly two children and were 15-44 year-old (the age-group in our sample) was 15,152,395. The fertility response of women with two children is 17.9 percentage points in the grace-period, i.e., 2,712,278 of these women altered their marginal fertility. The number of Panchayat seats in these states in 2004 was 912,597. Together, these numbers imply that approximately three women per seat in the sub-sample with exactly two children altered their fertility in the grace-period.

We also re-estimate (3) to examine the likelihood of having more than three and four living children before and after announcement of the law. In Figure 3, the likelihood of more than three children shows a pattern similar to that for the likelihood of more than two children, but the increase during the grace-period and the subsequent decline are much smaller. The smaller average fertility increase in the grace-period is to be expected as the law is not a binding fertility constraint for many of these individuals as they already have three children. In the 2001 Census, the number of women with three children in treated states was 13,056,020. The grace-period effect for women with three children is about 8 percentage points, i.e., 1,044,482 of these women altered their marginal fertility. This implies that about 1.14 women (with three children) per Panchayat seat changed their fertility due to the limits. There is no change in the likelihood of having more than four children.[29]

This pattern of results points to leadership aspirations being the primary mechanism behind the fertility responses. A role-model effect is unlikely to be immediate as it would take a few years after the laws are enacted for the constituents to observe and emulate their leaders' fertility outcomes, especially since the first set of post-treatment elections took place a few years after the announcements (Table A.1). Instead, the shift in timing of childbirth to the grace-period is most plausibly explained by families attempting to have an

[29]We also verify the significance of the estimated effects on the probability of having more than three and four living children using the placebo effect distribution in Figures A.2 and A.3, respectively.

additional child without sacrificing future electoral eligibility. These results also rule out a third competing mechanism wherein the law lowers fertility by changing a family's intrinsic preference over the ideal number of children (independently of role-model and aspirations channels) as the fertility increase during the grace-period cannot be explained by this channel.

Thus, our results suggest that at least four individuals per Panchayat seat alter their fertility to remain eligible for election in response to the limits. The total number of individuals who aspire to become Panchayat members may in fact be larger, as (i) couples with four or more children, and (ii) couples with two or three children who do not want more children, may also contest elections. However, our results cannot capture these latter individuals as they do not visibly alter their fertility due to the limits.

5.2 Regression Estimates

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

In Panel A of Table 6, we present estimation results from specification (1) to describe the effects of the fertility limits on the likelihood of a third birth. We restrict the sample to women who have at least two children, to years after the second birth, and among the treatment states, to women whose first two children were born before the law was announced in their state. Column (1) controls for state and year fixed effects. In Column (2), we include additional covariates that comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area. In Columns (3) and (4), we gradually add state-specific linear time trends and state-mother's age fixed effects. The specification in Column (5) restricts the sample to the treated states but is otherwise similar to Column (4). Along with standard errors clustered by state, we also report wild cluster bootstrapped errors.

The coefficient for $Treat_{st}$ is negative in all columns and statistically significant in all but one column if we use the standard clustered errors and significant in Columns (1) and (5) if we use the bootstrapped errors. This implies that the two-child limits decreased higher-order fertility for couples who already had two children when the law was announced in their state. The bootstrapped errors are similar in magnitude to the standard cluster-robust errors. It is reassuring that the coefficient in Column (5) is larger relative to the other columns and is significant, as it is estimated for only the treatment states and is therefore not prone to bias caused by differential non-linear time trends across treated and untreated states. This coefficient translates into a 0.67 percentage point or a 6.84% decrease in the likelihood of a third birth from the baseline probability of 9.8%.

In Panel B of Table 6, the dependent variable is an indicator for a second birth. We restrict the sample to women who have at least one child, to years after the first birth, and among the treatment states, to women whose first child was born before the law was announced in their state. To maintain eligibility for elections, these families can have only one additional birth. Moreover, the grace-period is not relevant for them. Consequently, if son preference is sufficiently strong, they may be more likely to practice sex-selection at second parity, which will mechanically delay their second birth (in addition to a reduction in completed fertility caused by the limits). Second births may also be postponed for reasons other than sex-selection, such as to improve the survival probability of the last birth.

In all columns of Panel B, the coefficient is negative and, except Column (2), significant implying that the two-child limits decreased the likelihood of a second birth in a given year for women who had already borne their first child before the law was announced in their state. The coefficient in Column (5) translates into a 0.76 percentage point or a 7.24% decrease in the likelihood of a second birth from the baseline probability of 10.5%. To confirm that this decrease in the likelihood of second birth is indeed driven by greater sex-selection, we examine heterogeneity in this effect by the sex of the first child in the following sub-section.

5.2.1 Heterogeneous Effects

Next we examine if the findings in Table 6 vary by household caste, religion, and residence in an urban area. To do so, we re-estimate the specification in Column (4) of Table 6 for various sub-samples; these results are presented in Table 7.[30] To the extent that the fertility limits have mostly been enacted for rural Panchayats, we expect to find larger effects for rural women. Columns (1) and (2) show that the decline in second and third births is only significant for the rural sample, supporting our assertion that the fertility decline is being causally driven by the two-child limits.

We also expect the fertility decline to be stronger for Hindu families relative to non-Hindus as the former are politically dominant and are hence more likely to be concerned about maintaining electoral eligibility. Columns (3) and (4) confirm this: in both panels the decrease in marginal fertility is significant only for Hindus.

For the same reasons as Hindus, we expect the fertility decline to be stronger for upper-castes relative to lower-castes. Moreover, prior literature suggests that upper-caste families also have a stronger preference for sons and are more likely to practice sex-selection. Thus the delay in second births resulting from a desire to have one more son is also likely to be stronger for upper-castes. On the other hand, affirmative action in India has ensured that one-third of all Panchayat positions are reserved for lower-caste individuals. Chattopadhyay and Duflo (2004) find that caste-based reservations confer significant political power on lower-caste Panchayat leaders and improve provision of public goods to these disadvantaged groups. Consequently, the political aspirations of lower-caste individuals might be strong enough for the two-child limits to also cause a decrease in their fertility. Lastly, if upper-caste couples are more likely than lower-caste couples to take advantage of the grace-period to have an additional child (say, to have an extra son), their overall fertility decline might be lower as a result. The coefficients in Columns (5) and (6) capture the net effect of these channels.

In Panel A, the decrease in third births is larger and only significant for lower-castes. This is potentially due to the fact that upper-castes families are less likely (than lower-castes) to have a third birth even in the absence of the laws, as reflected in the control group means. We

[30] We also estimate specifications with the pooled sample where indicators for religion, caste, and urban residence are interacted with the treatment dummy; these results are available upon request.

do not find any significant difference in the grace-period response by caste,[31] suggesting that the decrease for lower-castes is being driven by their political aspirations. For second births in Panel B, the coefficients are negative and significant for both groups, but the magnitude is slightly larger for upper-castes, consistent with their higher propensity to sex-select. To confirm the sex-selection mechanism, we next examine if the effect of the limits on the sex ratio of second births varies by the sex of the first child and household caste.

In Table 8, we present results for the heterogeneous effects on the probability and sex of the second birth by sex of the first child. We restrict the sample to women whose first child was born before the law was announced and to years after the first birth. In Columns (1) and (4), we include state-year fixed effects that provide full non-parametric control for state-specific time effects that are common across cohorts. The remaining columns instead control for state-specific linear time trends. In Columns (3) and (6), the sample is restricted to treatment states.[32] Columns (1)-(3) show that, before the law is announced, a firstborn girl, relative to a firstborn boy, increases the probability of a second birth by 0.2 percentage points, reflecting parents' desire for at least one son. However, once the law is announced, the likelihood of a second birth decreases by 0.5 percentage point if the firstborn is a boy; the decrease for those with a firstborn girl is significantly larger (by 0.27 percentage points).

In Columns (4)-(6) of Table 8 we examine the effect of the limits on the likelihood that the second child is male. Before the law is announced, a firstborn girl increases the probability of a second birth being male by 1 percentage point relative to a firstborn boy, reflecting the greater propensity for sex-selection at second parity by parents whose first child is a girl. While the coefficients in the first two rows of Columns (4)-(6) are positive, there is no significant effect of the fertility limits on sex-selection in the overall sample.

Table 9 further splits these results by caste to understand the mechanisms underlying

[31] These results are available upon request.

[32] We do not report the bootstrapped standard errors for Columns (4)-(6) since the procedure is time-intensive; moreover, the coefficients of interest are insignificant when standard cluster robust errors are used.

the caste results we find in Columns (5)-(6) in Panel B of Table 7.[33] Before the laws are announced, both upper- and lower-caste women are more likely to have a second child and it is more likely to be a boy if the first child is a girl. Like Table 8, for lower-castes the effects do not vary by the sex of the first child (Column (2)). Upper-caste results in Column (1) of Panel B show that the fertility limits do not affect the sex ratio of second birth if the first child is a boy. However, if the firstborn is a girl, there is a significantly larger (3 percentage points) increase in the sex ratio of second birth. The fertility decline in Panel A is also significantly larger for upper-caste families with a firstborn girl suggesting that the decrease in second parity births we observe earlier reflects a delay induced by greater sex-selection. If their first child is a girl, upper-caste families increase sex-selection at second parity to ensure that they have at least one son whilst not sacrificing future eligibility for political office.

6 Robustness

In this section we perform some robustness checks to ensure that our previous results truly capture the causal effect of the fertility limits. First, we conduct a placebo test by reassigning the treatment to a year before the actual law was announced. If our results capture the causal effect of the fertility limits, we should not find significant effects in these placebo regressions. In Table 10, each column uses a different year as a placebo treatment year. For example, in Column (1), we assume that the fertility limits were announced in all treatment states in 1986. Since these laws are fictitious, a significant "effect" at the 5% level may be found roughly 5% of the time. There is no (one) cell where we find a significant effect on the likelihood of a third (second) birth in the same direction as our main results in Panel A (B) of Table 6. These findings lend support to our estimation strategy and make a causal interpretation more credible.

[33]Since inclusion of state-year fixed effects implies that we cannot separately estimate $Treat_{st}$ and our coefficients are remarkably similar across all columns in Table 8, in Table 9 we present results for specifications with linear time trends. Results do not vary if state-year fixed effects are used instead.

Column (1) of Table 11 shows that our results are unchanged when only NFHS data is used, addressing concerns about bias introduced by unobserved differences in data collection or variations in sampling methodology for the NFHS and the DLHS. In Column (2) we examine the effects of the fertility limits on third births in control state districts that border the treatment states. Specifically, we restrict the sample to control states and assume that the treatment occurred in control border districts in the years when the laws were passed in the respective treatment states.[34] This specification effectively compares border districts to non-border districts within control states. We only use DLHS-2 data since the NFHS does not report district identifiers. Since treatment now varies at the district-year level, we control for district fixed effects and district-specific linear time trends, and cluster standard errors by district. A significant negative effect on fertility in control border districts would imply that there are treatment externalities across states that bias our previous estimates of treatment effects. Moreover, the presence of externalities would suggest that our findings might be driven by the role-model channel rather than by aspirations, since the latter is not relevant for control states whereas the former might be. However, the coefficient of $Treat$ in Column (2) is positive and insignificant, eliminating concerns about treatment externalities and lending further support to the political aspirations channel.

The limits can also affect fertility through adjustments in age at marriage. Forward-looking individuals (or their parents) wishing to maintain future electoral eligibility may delay marriage, which could explain the decrease in likelihood of births we observe in Section 5. To test if this is the case, we estimate specification (1) with a woman's age at first marriage as the dependent variable. The results in Column (3) of Table 11 show that there is no impact of the two-child limits on age at first marriage.

Any effect of the fertility limits on marital separation or divorce is likely to be small due to their low prevalence rates among Indian marriages. Among women who were surveyed in

[34] In cases where a district borders more than one treatment state, we assign treatment based on whichever state passed the law earlier.

treatment states in post-treatment years, only 1.52% report being separated or divorced from their husbands. For rest of the sample, the corresponding number is equally low (1.66%).

Though we control for a number of socioeconomic variables in our regressions, to further support our findings we show that the timing of announcement of the limits across states is uncorrelated with changes in these socioeconomic characteristics across states and over time. In Table 12 we present the coefficients from regressions that use various maternal, paternal, and household characteristics as dependent variables in the estimation of equation (1) with state and year fixed effects, and state-specific time trends, but without any other controls. Out of 20 coefficients, the only marginally significant coefficient is a negative effect on the likelihood of the woman being Hindu.

7 Conclusion

We find that the two-child limits on candidates in Panchayat 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 are caused by constituents' political ambitions rather than the role-model influence of their leaders. 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. While we cannot separately identify these "altruistic" and "selfish" components of political aspirations, we show that these ambitions are substantial and represent a previously ignored channel of demographic change.

Policymakers should therefore account for citizens' political aspirations for more effective policy-design. For example, the fertility impact of the two-child limits was substantially weakened by the increase in births during the grace-period. It is likely that the policymakers underestimated the constituents' desire to run for political office, resulting in this unintended effect. Moreover, 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 aspirations for local leadership are stronger than state or national leadership ambitions, the proposed limits may be less effective than the laws we examine.

Fertility restrictions on Panchayat members also have implications for political representation of socioeconomically disadvantaged groups. The two-child limits impose a more severe constraint on poorer households, as they are likely to have weaker access to contraception as well as higher demand for children. Consequently they are more likely than wealthier households to trade-off electoral eligibility for more children, reducing their political representation. Since a large proportion of the poor belong to lower castes, this could impede the progress made by caste-based affirmative action. The limits 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 2 that were disqualified for violating the limits.

Most 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 Panchayat membership in Rajasthan if they have less than five years of schooling or do not have a functional toilet in their home.[35] Like the fertility limits, these laws may worsen caste and gender inequality. Although 50% of the Panchayat seats in Rajasthan are reserved for women, the female literacy rate is only 45.8% (2011 Census of India).[36] Moreover, lower-caste individuals face considerable discrimination in access to sanitation and education. The effects of such barriers to local leadership on political representation, discrimination, and aspirations are key to poverty reduction, and merit further investigation.

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

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

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8 Figures

Figure 1: Treatment and Control States

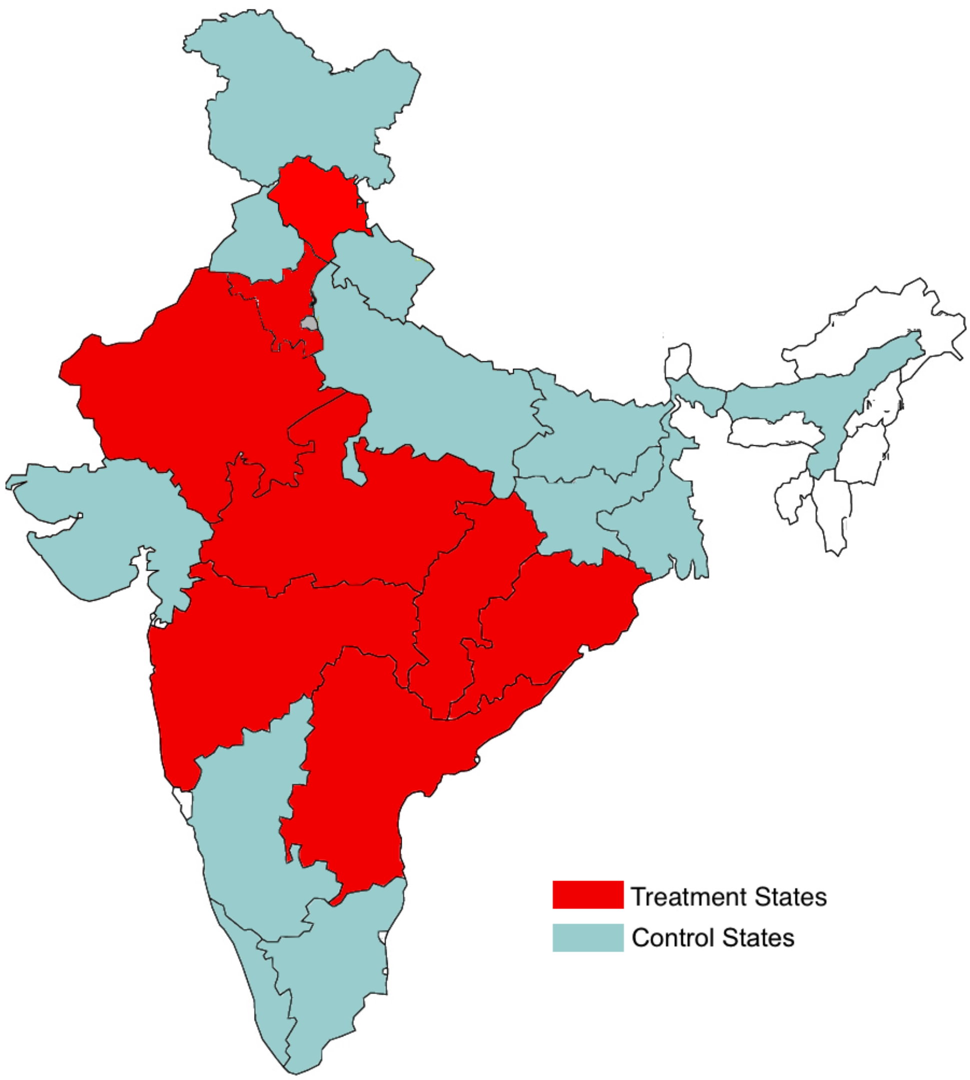
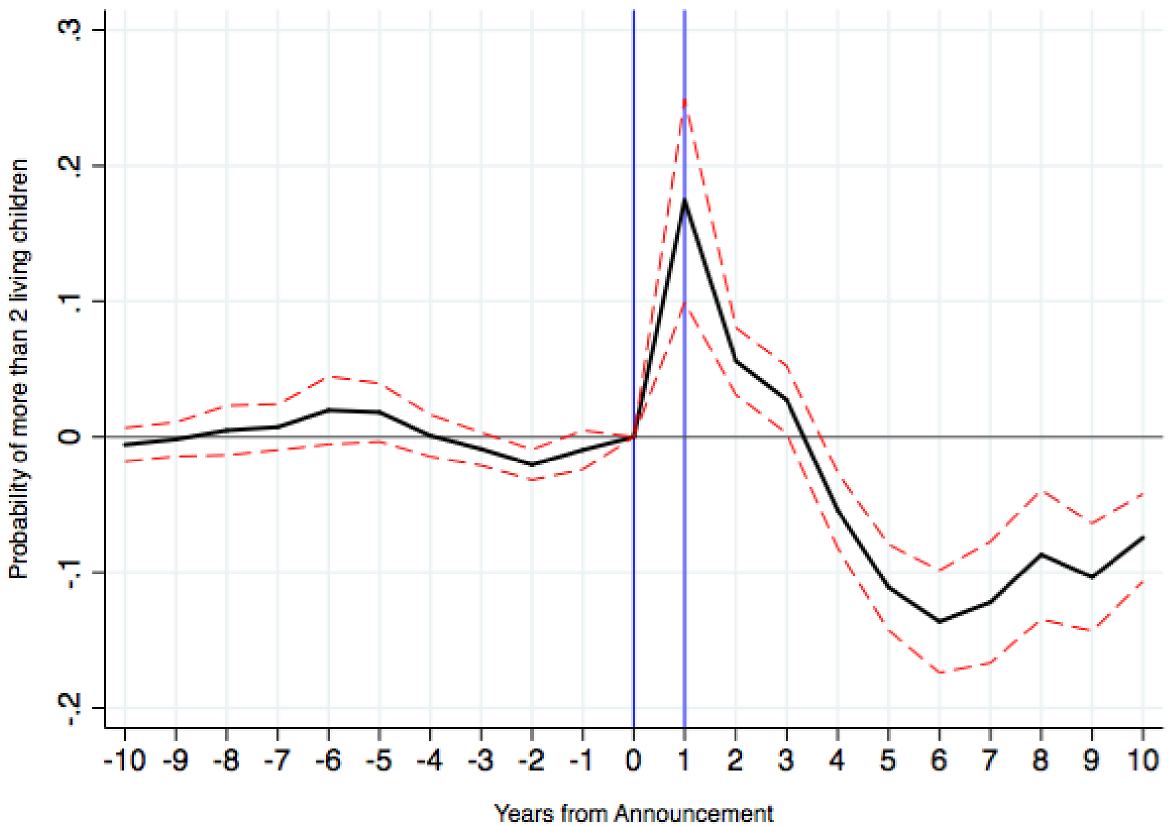
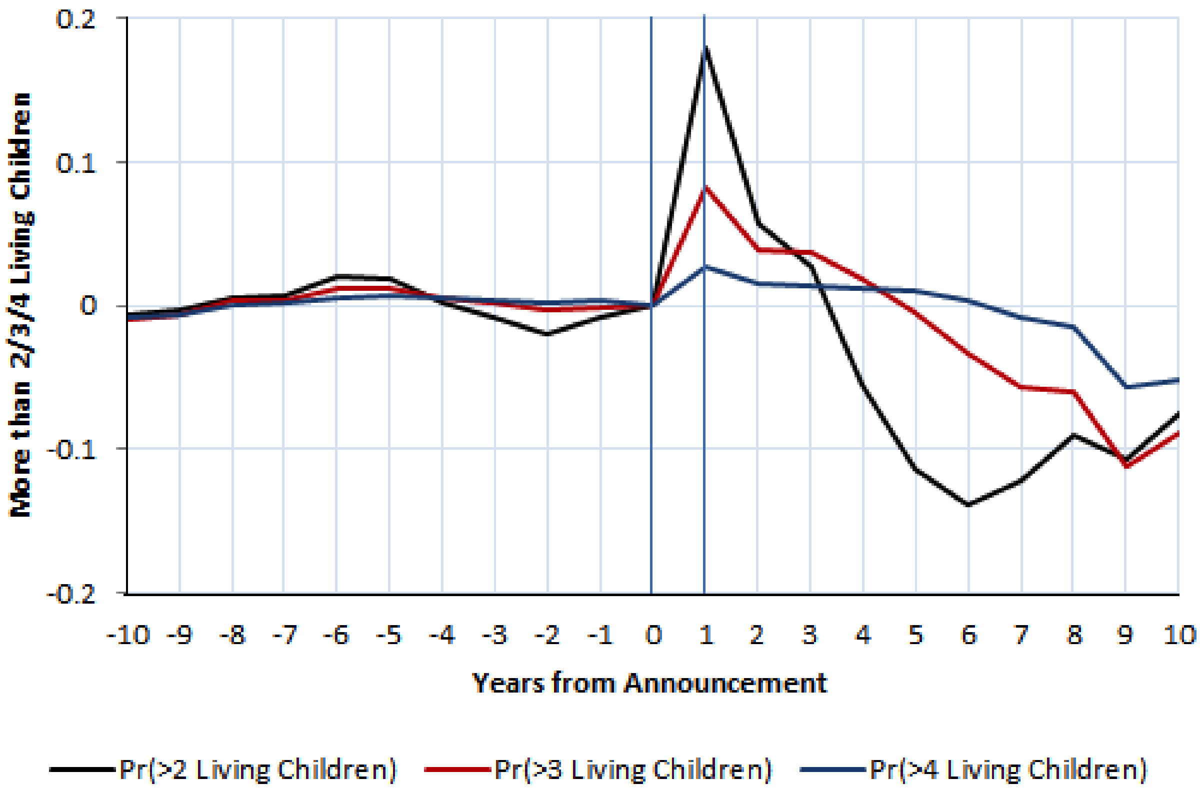


Figure 2: Likelihood of More Than Two Living Children, by Year



NOTES: This figure plots the β_k coefficients and their 95% confidence intervals (dashed lines) from estimating the following equation for a woman i in state s of age a in year t:
$Y_{isat} = \sum_{k=-10}^{10} \beta_k Treat_{s,t+k} + X_i^{\prime}\delta + \gamma_s + \theta_t + \psi_a + \mu_{sa} + \epsilon_{isat}$, where $Treat_{s,t+k}$ indicates k years from the announcement of the law in state s. Standard errors are clustered by state-year. The first vertical line (at $k = 0$) indicates the year of announcement. The second vertical line indicates the end of the one-year grace-period. The sample is restricted to women in treatment states. Other covariates comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area.

Figure 3: Likelihood of More Than Two, Three, and Four Living Children, by Year



NOTES: This figure plots the β_k coefficients from estimating the following equation for a woman i in state s of age a in year t:
$Y_{isat} = \sum_{k=-10}^{10} \beta_k Treat_{s,t+k} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \mu_{sa} + \epsilon_{isat}$, where $Treat_{s,t+k}$ indicates k years from the announcement of the law in state s. The dependent variables are indicators for more than two, three, and four living children in a given year. The first vertical line (at $k = 0$) indicates the year of announcement. The second vertical line indicates the end of the one-year grace-period. Other covariates comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area.

9 Tables

Table 1: Timeline for Fertility Limits Across States

State	**Announced**	**Grace Period**	**In effect**	**End**
Rajasthan	1992	Apr 23, 1994 - Nov 27, 1995	Nov 27, 1995 -	
Haryana	1994	Apr 21, 1994 - Apr 24, 1995	Apr 25, 1995 - Dec 31, 2004	Jul 21, 2006 (retro. impl. Jan 1, 2005)
Andhra Pradesh	1994	May 30, 1994 - May 30, 1995	Jun 1995 -	
Orissa	1993/1994[38]	Apr 1994 - Apr 21, 1995	Apr 22, 1995 -	
Himachal Pradesh	2000	Apr 18, 2000 - Apr 18, 2001	Apr 2001 - Apr 2005	May 30, 2005
Madhya Pradesh	2000[39]	Mar 29, 2000 - Jan 26, 2001	Jan 2001 - Nov 2005	Nov 20, 2005
Chhattisgarh	2000	2000 - Jan 2001	Jan 2001- 2005	2005 (earliest mention)[37]
Maharashtra	2003[40]	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 -	

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

[39] 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.

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

Table 2: Panchayat Members Disqualified During 2000-04, 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 3: Treatment Years, by State

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

Table 4: Summary Statistics

	Never treated		Treated			
			$Post = 0$		$Post = 1$	
Variable	Mean (1)	Std. Dev. (2)	Mean (3)	Std. Dev. (4)	Mean (5)	Std. Dev. (6)
Urban	0.343	0.475	0.329	0.470	0.320	0.466
Hindu	0.786	0.410	0.897	0.304	0.898	0.303
Muslim	0.161	0.367	0.066	0.249	0.063	0.243
Sikh	0.041	0.198	0.010	0.100	0.013	0.113
Christian	0.027	0.162	0.011	0.103	0.014	0.117
SC	0.180	0.384	0.160	0.367	0.177	0.382
ST	0.062	0.240	0.149	0.356	0.134	0.341
OBC	0.365	0.481	0.298	0.457	0.374	0.484
Wife's years of schooling:						
Zero	0.514	0.500	0.563	0.496	0.544	0.498
5-10 years	0.244	0.429	0.229	0.420	0.235	0.424
10-12 years	0.091	0.287	0.074	0.261	0.082	0.275
12-15 years	0.048	0.214	0.031	0.173	0.039	0.193
$\geq$ 15 years	0.045	0.207	0.037	0.188	0.046	0.209
Husband's years of schooling:						
Zero	0.278	0.448	0.291	0.454	0.289	0.453
5-10 years	0.301	0.459	0.309	0.462	0.310	0.462
10-12 years	0.153	0.360	0.149	0.357	0.149	0.356
12-15 years	0.093	0.290	0.070	0.255	0.079	0.270
$\geq$ 15 years	0.096	0.294	0.089	0.285	0.101	0.302
Low SLI	0.446	0.497	0.460	0.498	0.425	0.494
High SLI	0.242	0.428	0.233	0.423	0.250	0.433
Mother's age at birth	24.853	6.163	23.008	5.474	26.507	6.341
Birth = 1	0.213	0.410	0.239	0.426	0.161	0.367
Has 2 children	0.260	0.438	0.234	0.423	0.287	0.442
1st birth is male	0.520	0.500	0.517	0.500	0.521	0.500
N	3,568,675		1,458,849		941,801	

NOTES: *Post* is defined using the year of announcement of the law (see Table 3). SC, ST, and OBC indicate Scheduled Caste, Scheduled Tribe, and Other Backward Class 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 India.

Table 5: Effect on the Likelihood of More than Two Living Children

Dep var: More than 2 living children = 1	(1)		(2)
$t-10$	-0.007	$t+1$	0.179
	[0.005]		[0.039]***
$t-9$	-0.003	$t+2$	0.057
	[0.005]		[0.012]***
$t-8$	0.005	$t+3$	0.027
	[0.007]		[0.011]**
$t-7$	0.007	$t+4$	-0.056
	[0.007]		[0.012]***
$t-6$	0.021	$t+5$	-0.114
	[0.012]*		[0.012]***
$t-5$	0.019	$t+6$	-0.139
	[0.010]*		[0.015]***
$t-4$	0.002	$t+7$	-0.123
	[0.007]		[0.018]***
$t-3$	-0.009	$t+8$	-0.090
	[0.005]		[0.019]***
$t-2$	-0.020	$t+9$	-0.110
	[0.005]***		[0.015]***
$t-1$	-0.009	$t+10$	-0.075
	[0.007]		[0.012]***
N		2,400,650	
N(clusters)		237	

NOTES: This table presents the β_k coefficients from estimating the following equation for a woman i in state s of age a in year t: $Y_{isat} = \sum_{k=-10}^{10} \beta_k Treat_{s,t+k} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \mu_{sa} + \epsilon_{isat}$, where $Treat_{s,t+k}$ indicates k years from the announcement of the law. All coefficients are from the same regression. Standard errors in brackets are clustered by state-year. The sample is restricted to women in treatment states. Other covariates comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area. *** 1%, ** 5%, * 10%

Table 6: Effects on Marginal Fertility

	(1)	(2)	(3)	(4)	(5)
A. 3rd birth = 1:					
$Treat_{st}$	-0.0200	-0.0042	-0.0050	-0.0049	-0.0067
	[0.0054]***	[0.0049]	[0.0028]*	[0.0025]*	[0.0023]**
	(0.0080)**	(0.0049)	(0.0031)	(0.0029)	(0.0035)**
N	2,899,022	2,899,022	2,899,022	2,899,022	1,063,251
Control group mean	0.080	0.080	0.080	0.080	0.098
B. 2nd birth = 1:					
$Treat_{st}$	-0.0229	-0.0037	-0.0060	-0.0061	-0.0076
	[0.0051]***	[0.0041]	[0.0028]**	[0.0026]**	[0.0018]***
	(0.0084)***	(0.0042)	(0.0031)*	(0.0031)*	(0.0034)**
N	4,122,755	4,122,755	4,122,755	4,122,755	1,531,067
Control group mean	0.089	0.089	0.089	0.089	0.105
Year FE & State FE	x	x	x	x	x
Covariates		x	x	x	x
State-specific linear trends			x	x	x
State x Age FE				x	x

NOTES: This table reports the coefficients of $Treat_{st}$ from specification (1). Each coefficient is from a separate regression. The dependent variables are indicators for a third birth in a given year in Panel A and a second birth in a given year in Panel B. In Panel A, the sample is restricted to women whose second child was born before the law was announced in her state and only years after the second birth are included. In Panel B, the sample is restricted to women whose first child was born before the law was announced in her state and only years after the first birth are included. In Column (5), the sample is restricted to women in treatment states. Covariates comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area. Standard errors in brackets are clustered by state and wild-cluster bootstrapped standard errors (by state) are in parentheses. *** 1%, ** 5%, * 10%.

Table 7: Heterogeneity in Effects on Marginal Fertility

	Rural (1)	**Urban** (2)	**Hindu** (3)	**Non-Hindu** (4)	**Upper-caste** (5)	**Lower-caste** (6)
A. 3rd birth = 1:						
$Treat_{st}$	-0.0059	-0.0030	-0.0050	-0.0062	-0.0022	-0.0059
	[0.0025]**	[0.0026]	[0.0025]*	[0.0036]*	[0.0027]	[0.0027]**
	(0.0033)*	(0.0025)	(0.0030)*	(0.0036)	(0.0025)	(0.0034)*
N	1,938,087	960,935	2,369,751	529,271	1,111,028	1,787,994
Control group mean	0.085	0.069	0.080	0.078	0.073	0.084
B. 2nd birth = 1:						
$Treat_{st}$	-0.0079	-0.0028	-0.0063	-0.0038	-0.0065	-0.0059
	[0.0026]***	[0.0028]	[0.0023]**	[0.0052]	[0.0031]**	[0.0026]**
	(0.0034)*	(0.0027)	(0.0028)*	(0.0049)	(0.0034)*	(0.0031)*
N	2,717,772	1,404,983	3,388,712	734,043	1,599,970	2,522,785
Control group mean	0.091	0.084	0.089	0.086	0.086	0.090

NOTES: Each coefficient is from a separate regression. The specification, variables, and sample restrictions are similar to Column (4) in Table 6. Other covariates comprise indicators for the year of survey, woman's age, wealth, husband's and wife's years of schooling, household's religion (in (1)-(2)), caste (in (1)-(4)), and residence in an urban area (in (3)-(6)). Standard errors in brackets are clustered by state and wild-cluster bootstrapped standard errors (by state) are in parentheses. *** 1%, ** 5%, * 10%.

Table 8: Effects on Second Births, by First Child's Sex

	2nd birth = 1			**2nd birth is male**		
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{st}$ * $First - born\ girl$	-0.0027 [0.0012]** (0.0015)*	-0.0027 [0.0013]** ()	-0.0027 [0.0013]* (0.0015)*	0.0085 [0.0064]	0.0080 [0.0064]	0.0080 [0.0067]
$Treat_{st}$		-0.0044 [0.0021]* (0.0026)	-0.0056 [0.0015]** (0.0028)**		0.0034 [0.0050]	0.0051 [0.0062]
$First - born\ girl$	0.0023 [0.0007]***	0.0022 [0.0007]***	0.0024 [0.0007]**	0.0097 [0.0013]***	0.0095 [0.0013]***	0.0093 [0.0013]***
N	4,114,143	4,114,143	1,522,455	332,002	332,002	118,660
Year FE & State FE	x	x	x	x	x	x
Covariates	x	x	x	x	x	x
State x Year FE	x			x		
State-specific linear trends		x	x		x	x
State FE x First-born girl	x	x	x	x	x	x
State x Age FE	x	x	x	x	x	x

NOTES: The sample is restricted to women whose first child was born before the law was announced in her state. Only years after the first birth are included. In Columns (1)-(3), the dependent variable is one if there is a second birth in a given year, and zero otherwise. Columns (4)-(6) are conditional on a second birth and the dependent variable is one if the second birth is male, and zero otherwise. Post-2002 observations for Haryana are excluded. In Columns (3) and (6), the sample is restricted to women in treatment states. Each coefficient is from a separate regression. Standard errors in brackets are clustered by state and wild-cluster bootstrapped standard errors (by state) are in parentheses. Covariates comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area. *** 1%, ** 5%, * 10%.

Table 9: Heterogeneity in Effects on Second Births, by Caste and First Child's Sex

	Upper-caste (1)	**Lower-caste** (2)
A. 2nd birth = 1:		
$Treat_{st} * First-born\ girl$	-0.0030**	-0.0023
	[0.0012]	[0.0015]
$Treat_{st}$	-0.0046	-0.0043*
	[0.0028]	[0.0022]
$First-born\ girl$	0.0022***	0.0023**
	[0.0006]	[0.0009]
N	1,595,754	2,518,389
B. 2nd birth is male:		
$Treat_{st} * First-born\ girl$	0.0325***	-0.0016
	[0.0089]	[0.0060]
$Treat_{st}$	-0.0092	0.0051
	[0.0085]	[0.0052]
$First-born\ girl$	0.0066***	0.0116***
	[0.0015]	[0.0013]
N	127,382	204,620

NOTES: The specifications, variables, and sample restrictions in Panels A and B are respectively similar to Columns (2) and (5) in Table 8. Other covariates comprise indicators for the year of survey, woman's age, wealth, husband's and wife's years of schooling, household's religion, and residence in an urban area. *** 1%, ** 5%, * 10%.

Table 10: Placebo Test for Marginal Births

	Placebo treatment year:							
	1986	**1987**	**1988**	**1989**	**1990**	**1991**	**1992**	**1993**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. 3rd birth =1								
$Treat_{st}$	0.010**	0.005	0.002	0.001	-0.002	-0.002	-0.002	-0.006
	[0.004]	[0.004]	[0.003]	[0.003]	[0.002]	[0.004]	[0.003]	[0.004]
N	2,899,022							
B. 2nd birth =1								
$Treat_{st}$	0.004	0.004	0.003	0.002	0.001	-0.001	-0.004*	-0.006
	[0.003]	[0.003]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]	[0.003]
N	4,122,755							

NOTES: Each coefficient is from a separate regression with a different placebo treatment year (same for all treated states). The dependent variable in Panel A (Panel B) is one if there is a third (second) birth in a given year, and zero otherwise. In Panel A, the sample is restricted to women whose first two children were born before the law was announced in her state and only years after the second birth are included. In Panel B, the sample is restricted to women whose first child was born before the law was announced in her state and only years after the first birth are included. Standard errors are in brackets and are clustered by state. Specifications are similar to column (4) in Table 6. *** 1%, ** 5%, * 10%.

Table 11: Additional Robustness Checks

	3rd birth = 1		**Age at 1st marriage**
	NFHS only	Bordering control districts	
	(1)	(2)	(3)
Treat	-0.0054**	0.0023	0.004
	[0.0021]	[0.0023]	[0.336]
N	880,129	1,679,669	62,818
Year FE & State FE	x	x	x
State x Age FE	x		
District FE		x	
Covariates	x	x	x
State-specific linear trends	x		x
District-specific linear trends		x	

NOTES: Each coefficient is from a separate regression. In Column (1), the sample is restricted to NFHS data. In Column (2), only DLHS-2 data is used and the sample is restricted to control states. In Column (2), *Treat* is equal to one for years post-announcement for women in control state districts bordering treatment states, and zero for the remaining years and control state districts; the year used to define treatment is the year of announcement in the bordering treatment state. In Column (3), the sample is restricted to one observation per woman and *Treat* is equal to one if a woman's first marriage took place after the law was announced in her state, and zero otherwise. Standard errors are clustered by state in (1) and (3) and by district in (2). *** 1%, ** 5%, * 10%.

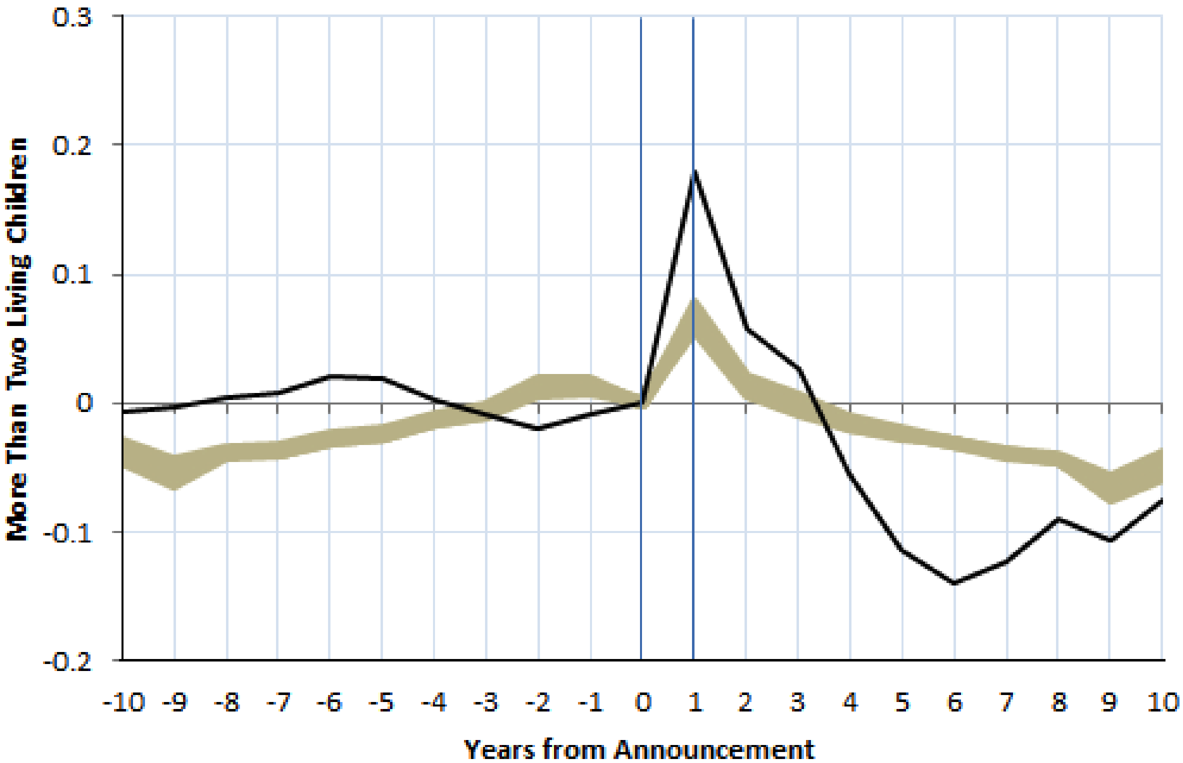
Table 12: Correlations between Law Announcements and Socioeconomic Variables

Dependent Variable	**Coefficient of $Treat_{st}$** (1)	**Std. Error** (2)
Urban	0.008	[0.009]
SC	-0.003	[0.002]
ST	0.006	[0.005]
OBC	0.007	[0.006]
Hindu	-0.005*	[0.003]
Muslim	0.001	[0.002]
Sikh	0.0005	[0.001]
Christian	-0.001	[0.002]
Low SLI	-0.002	[0.005]
High SLI	0.003	[0.005]
Wife's years of schooling:		
Zero	-0.002	[0.003]
5-10 years	0.001	[0.004]
10-12 years	0.001	[0.002]
12-15 years	0.002	[0.002]
$\geq$ 15 years	-0.0001	[0.002]
Husband's years of schooling:		
Zero	-0.001	[0.002]
5-10 years	0.00009	[0.002]
10-12 years	0.001	[0.002]
12-15 years	0.003	[0.003]
$\geq$ 15 years	-0.001	[0.002]
N	5,969,325	

NOTES: Each coefficient is from a separate regression that includes state, year, and state-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 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. *** 1%, ** 5%, * 10%.

A Additional Figures and Tables

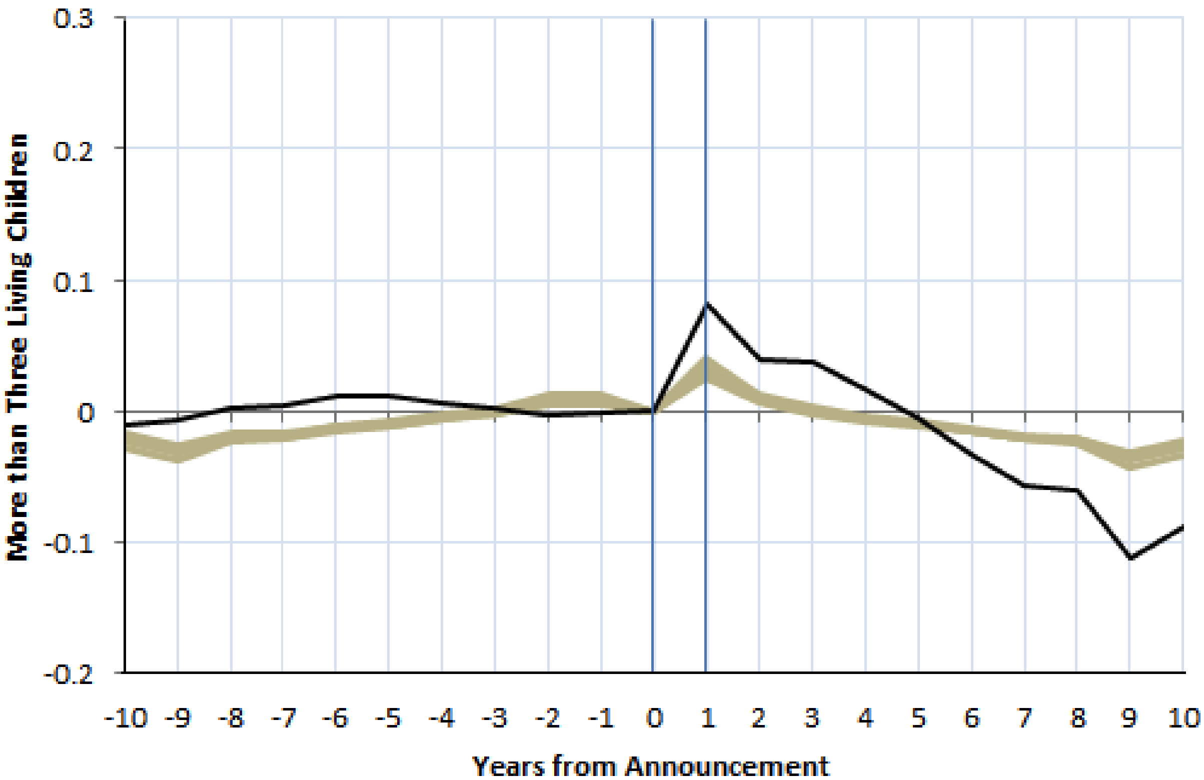
Figure A.1: Likelihood of More Than Two Living Children, by Year



NOTES: This figure plots the β_k coefficients from estimating the following equation for a woman i in state s of age a in year t:
$Y_{isat} = \sum_{k=-10}^{10} \beta_k Treat_{s,t+k} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \mu_{sa} + \epsilon_{isat}$, where $Treat_{s,t+k}$ indicates k years from the announcement of the law in state s. The first vertical line (at $k = 0$) indicates the year of announcement. The second vertical line indicates the end of the one-year grace-period. The shaded brown area is the range of 900 placebo treatment effect estimates. The sample is restricted to women in treatment states. Other covariates comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area.

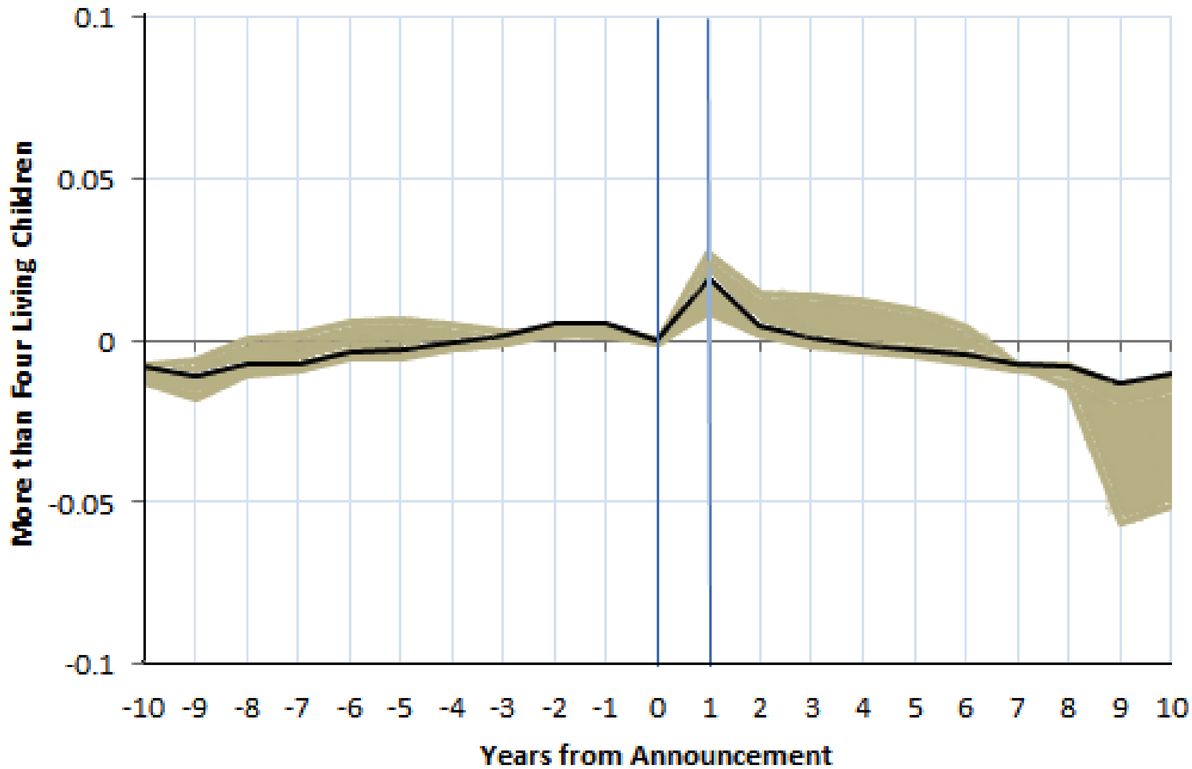
[21]Despite the fact that the two-child norm was officially introduced after the Panchayat elections were over in 2000, the new government began disqualifying elected representatives earlier (Visaria et al. (2006)).

Figure A.2: Likelihood of More Than Three Living Children, by Year



NOTES: This figure plots the β_k coefficients from estimating the following equation for a woman i in state s of age a in year t:
$Y_{isat} = \sum_{k=-10}^{10} \beta_k Treat_{s,t+k} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \mu_{sa} + \epsilon_{isat}$, where $Treat_{s,t+k}$ indicates k years from the announcement of the law in state s. The first vertical line (at $k = 0$) indicates the year of announcement. The second vertical line indicates the end of the one-year grace-period. The shaded brown area is the range of 900 placebo treatment effect estimates. The sample is restricted to women in treatment states. Other covariates comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area.

Figure A.3: Likelihood of More Than Four Living Children, by Year



NOTES: This figure plots the β_k coefficients from estimating the following equation for a woman i in state s of age a in year t:
$Y_{isat} = \sum_{k=-10}^{10} \beta_k Treat_{s,t+k} + X_i^{'} \delta + \gamma_s + \theta_t + \psi_a + \mu_{sa} + \epsilon_{isat}$, where $Treat_{s,t+k}$ indicates k years from the announcement of the law in state s. The first vertical line (at $k = 0$) indicates the year of announcement. The second vertical line indicates the end of the one-year grace-period. The shaded brown area is the range of 900 placebo treatment effect estimates. The sample is restricted to women in treatment states. Other covariates comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area.

Table A.1: Panchayat Elections

State	Election Years	
	Without the limits	With the limits
Rajasthan	1995	2000, 2005, 2010
Haryana	1994, 2010	2000, 2005
Andhra Pradesh		1995, 2001, 2006, 2011
Orissa		1997, 2002, 2007, 2012
Himachal Pradesh	1995, 2005, 2010-11	2000
Madhya Pradesh	1994, 2010	2000,[21] 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

Table A.2: Effect on the Likelihood of More than Two Living Children (with bootstrapping)

Dep var: More than 2 living children = 1			
	(1)		(2)
$t-10$	-0.006	$t+1$	0.174
	(0.004)		(0.073)**
$t-9$	-0.002	$t+2$	0.056
	(0.004)		(0.022)***
$t-8$	0.005	$t+3$	0.027
	(0.007)		(0.015)**
$t-7$	0.007	$t+4$	-0.054
	(0.006)		(0.022)**
$t-6$	0.020	$t+5$	-0.111
	(0.012)**		(0.044)***
$t-5$	0.018	$t+6$	-0.136
	(0.013)		(0.056)**
$t-4$	0.001	$t+7$	-0.122
	(0.007)		(0.052)**
$t-3$	-0.009	$t+8$	-0.087
	(0.005)		(0.036)**
$t-2$	-0.021	$t+9$	-0.103
	(0.010)***		(0.043)***
$t-1$	-0.010	$t+10$	-0.074
	(0.007)		(0.031)***
N		2,390,087	

NOTES: This table presents the β_k coefficients from estimating the following equation for a woman i in state s of age a in year t: $Y_{isat} = \sum_{k=-10}^{10} \beta_k Treat_{s,t+k} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \epsilon_{isat}$, where $Treat_{s,t+k}$ indicates k years from the announcement of the law. All coefficients are from the same regression. Standard errors in parentheses are wild-cluster bootstrapped (one at a time) by state. The sample is restricted to women in treatment states. Other covariates comprise indicators for the year of survey, woman's age, household's religion, caste, wealth, husband's and wife's years of schooling, and residence in an urban area. *** 1%, ** 5%, * 10%

B State-wise Regulations

1. Rajasthan:[38]

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[39], "...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:[40] "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

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

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

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

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).[41]

3. Andhra Pradesh:[42]

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."[43]

4. Orissa:[44]

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:[45]

"...condition to disqualify an office bearer of the Panchayat for holding the post: (1) that he

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

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

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

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

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

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:[46]

"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...."

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