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

S Anukriti† Abhishek Chakravarty‡

July 2017

Abstract

We investigate whether restricting elected leadership positions to candidates with "desirable" characteristics leads to the society-wide adoption of those characteristics in a 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 and child survival improved. The limits however also increased the already male-biased sex ratio at birth in castes with strong son preference.

JEL Codes: J13, J16, H75, O12

Keywords: India, Local Elections, Fertility Limits, Sex Ratio

---

*An earlier version of this paper was circulated as "Political Aspirations in India: Evidence from Fertility Limits on Local Leaders." We thank Heather Banic and Priyanka Sarda for research assistance. We also thank Sonia Bhalotra, Prashant Bharadwaj, V. Bhaskar, David Canning, Donald Cox, Esther Duflo, Eliana la Ferrara, Andrew Foster, Scott Fulford, Rema Hanna, Rachel Heath, Jonas Hjort, Lakshmi Iyer, Tarun Jain, Seema Jayachandran, Melanie Khamis, Adriana Kugler, Annemie Maertens, Giovanni Mastrobuoni, Grant Miller, Dilip Mookherjee, Vijayendra Rao, Debraj Ray, Laura Schechter, Shing-Yi Wang, and participants at various seminars and conferences for their helpful comments and suggestions.

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

‡Department of Economics, University of Manchester. abhishek.chakravarty@manchester.ac.uk.

1 Introduction

Developing countries often employ policy measures to lower fertility, such as direct fertility limits on citizens (e.g., China's One Child Policy), conditional cash transfers (e.g., the Devirupak program in the Indian state of Haryana), and incentives to promote contraceptive use (e.g., sterilization incentives in India). We examine a novel policy experiment that, in an attempt to decrease fertility, sets limits on the number of children that candidates for local political offices can have. Specifically, we analyze the impact of state-level legislations in India that disbar individuals with more than two children from contesting Village Council (*Panchayat*) elections on fertility-related outcomes in the general population.

The rural local government system in India comprises village-, block-, and district-level councils that exercise considerable power. Starting in 1992, eleven states enacted 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. We exploit geographical and temporal variation in announcement of these fertility limits across states to estimate their impacts on citizens' demographic outcomes.

This manipulation of the candidate pool aims to curb population growth, and was not intended to directly improve leaders' performance.[1] Instead, the laws seek to improve economic outcomes by precipitating fertility decline. The fertility limits however potentially impose costs as they incentivize couples to deviate from their preferred fertility path and shrink the candidate pool. To the extent that individuals from certain socioeconomic strata have higher desired fertility and lower contraceptive access, the limits may increase inequality in political representation. An understanding

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

of the trade-off between the intended effect on citizens' fertility behaviors and the unintended effect of disenfranchising certain socioeconomic groups is therefore crucial in determining the overall effectiveness of the laws.

We find that among couples who had two children when the law was announced in their state, the hazard of third birth declined by 1.97 percentage points (p.p.), which is 9 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 the 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.[2] 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. The fertility decline appears permanent as our event study analysis shows sustained decreases even 6 to 9 years after the limits were announced.

We find improvements in survival rates of children due to the limits. However, the fertility restrictions 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. 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 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

[2]Average terminal fertility in enacting states before announcement of the law was 2.8.

fact, this appears to be the primary mechanism the policymakers had in mind when these laws were enacted.[3] Societal fertility decline may also be driven by citizens' desire to maintain eligibility for future village council candidacy ("aspirations/ incentives channel"). 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 these 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 welfare of the socially disadvantaged who, relative to high socioeconomic status groups, have higher fertility, face greater risk of child mortality, and depend more on political representation to obtain resources prone to elite capture. On the other hand, higher baseline fertility makes low socioeconomic status families more "treatable" by the limits. We find that the decline in third births is concentrated in families with low wealth, with no schooling, and in lower castes that rely on mandated reservations for political representation. Moreover, the increase in the probability of having two children, while not as precisely estimated, does not differ by socioeconomic status. These findings reduce concerns about greater elite capture of public resources due to the limits.

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.

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

The rest of the paper is organized as follows. Section 2 discusses the legislations. 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. 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 enacted fertility limits for village council candidates,[4] seeking to lower fertility through the role-model channel.[5] Additionally, the fertility limits also incentivize individuals who intend to contest elections to plan smaller families.

2.1 India's Local Political System

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 take place every five years in most states. The village councils exercise considerable power in their constituencies, receiving substantial funds from national and state governments and implementing development schemes.[6] 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.

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

[5]According to Buch (2005), the introduction of these laws "and the widespread fascination for [it] draw inspiration from China's one child policy." Although, the legal documents pertaining to these laws do not explicitly spell out their ultimate objectives, these legislations are in tune with IndiaÕs national population policy that seeks to, in the long-run, "achieve a stable population...at a level consistent with the requirements of sustainable economic growth, social development, and environmental protection." This suggests that policymakers perceive fertility reduction as a way to decrease poverty and to improve social indicators such as health and education.

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

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 large, the potential private returns from political rents and corrupt practices may provide a strong incentive to run for office. According to the Association for Democratic Reforms, an average candidate spends USD 400 - USD 800 during a village council election.[7] 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 to the Parliament and state legislative assemblies in 2004 was 134 percent higher than their wealth during the first election (Sastry (2014)), suggesting high rents. Fisman et al. (2014) also shows 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 are likely to also be large.

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. 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. 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 fertility limits could 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.[8] Similarly, positions are reserved for Scheduled Castes (SC) and Scheduled Tribes (ST) in proportion to their population share.[9] As lower-castes have higher fertility and lower contraceptive access, the limits may increase caste inequality in political representation and thus reverse affirmative action gains. To examine these heterogeneous impacts, we also present results

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

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

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

separately for minority groups.

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. Thus our results have significant implications for the understanding of the relationship between political participation and social change in low-income democracies.

2.2 The Fertility Limits

Eleven states have imposed fertility limits on village council members for at least a few years, and they remain in effect in seven states. Rajasthan was the first state to introduce the two-child limit for its village councils in 1992;[10] this requirement was later included in the state's 1994 PR Act.[11] Andhra Pradesh and Haryana announced their legislations in 1994,[12] 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[13] 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.[14] 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.

---

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

[11] 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.

[12] 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.

[13] 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.

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

These laws potentially disproportionately affect candidates who are Muslim, who belong to a scheduled caste or tribe, and who are relatively poor. Table A.1 displays the fraction of women in a socioeconomic group who report that their ideal number of children is more than two. As compared to 48 percent of upper caste women, 56 percent of SC-ST women ideally desire more than two children. Among Muslims, 65 percent women and their husbands would be disqualified if they had the woman's desired number of children relative to 49 percent of Hindus. Similarly, the limits would disbar 59 percent of the couples in the bottom-third of the wealth distribution, and only 23 percent of those in the top-third of the wealth distribution.

Table A.2 shows the number of village council members that were disqualified under these laws in Haryana, Rajasthan, MP, and AP during 2000-2004.[15] Newspaper reports suggest that, in some cases, the fertility limits have led to the abandonment of wives or children, or selective abortion of female fetuses 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.[16] The revocation of the limits in four states may have been in response to this pressure.

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 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; 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. 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

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

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

below 15 or above 80 in the year of survey, and (iii) who had more than ten children, to prevent any composition-bias since these women are likely different from rest of the sample. Lastly, we exclude mothers with twins since multiple births in our context are largely unplanned and do not reflect fertility preferences. However, 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.[17] Our final sample comprises 99,804 women and 256,267 births from 18 major states[18] and covers the time period 1973-2006. We define treatment based on the year of the announcement of the law, i.e., the earliest year when the law might have had an effect in a state.

Table 2 presents the sample means and standard deviations for our key variables, 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).[19] The sample means for the three groups in Table 2 are similar along many socioeconomic dimensions, but there are several significant differences as the enactment of the limits was not randomized across states. To ensure that our estimates are not confounded by underlying differences, 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 policy announcements across states is uncorrelated with changes in these socioeconomic characteristics across states and over time.

[17]The urban estimates, available upon request, show no impacts.

[18]We group the new states of Uttarakhand, Jharkhand, and Chhattisgarh with their original states of Uttar Pradesh (UP), Bihar, and MP, respectively, in 2000.

[19]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).

4 Empirical Strategy

We aim to estimate the causal effect of the two-child limits on candidates in village council elections in a state on fertility-related outcomes among citizens. To do so, we utilize 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 we include these states 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.[20] 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 the treatment states, in an event-study framework. Specifically, for a woman i of age a in state s and year t, we estimate the following model:

$$Y_{iast} = \sum_{k=-10}^{9} \alpha_k Treat_{s,t+k} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \nu_s * t + \epsilon_{iast} \tag{1}$$

We include only treatment states in specification (1) since there is no unique reference year that can be used to split the sample period into pre- and post-years for the control states; this is because the announcement year varies across the treated states. $Treat_{s,t+k}$ indicates k years during which the law is in place; we assign the year before the year of announcement as the omitted year. The outcome variables Y_{iast} are indicators for first, second, third, fourth, and fifth birth. We control for

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

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 α_k coefficients capture the evolution of the hazard of birth in treatment states before and after the announcement of the fertility limits over a 20 year period. Note that for the hazards of fourth and fifth birth $k \in [-9, 6]$ as there are not enough observations to estimate (1) over the full period.

To measure the net effect of the limits on the hazard of an additional birth, we estimate:

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

In specification (2) we include both treatment and control states: $Treat_{st}$ is equal to one for women residing in the treated states if $t \geq$ the year of announcement, and zero otherwise; for control states, $Treat_{st}$ is always zero. In addition to the variables included in specification (1), we also control for state × 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 in specification (2). Where the outcome indicates a birth of order b, we restrict the sample to women whose previous ($b - 1$) births took place before the law was announced in their respective states.[21] These regressions, therefore, capture the effects of the fertility limits on *marginal* fertility of affected households. Note that, unlike specification (1), specification (2) uses all available pre- and post-announcement years for each state in our sample that satisfy the previously mentioned restrictions. Also, we restrict the sample to up to ten years after birth ($b - 1$), as after this the probability of another birth of order b converges to zero in both treatment and control states.

While specification (2) 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, it does not measure the

[21]Note that in the event-study regressions for the hazard of birth b we do not impose any constraints on the year of birth ($b - 1$) which may also have been affected by the fertility limits. Later, we also check if the results are similar when no restrictions are imposed on the timing of prior births while estimating specification (1).

overall impact of the laws on the "stock" of fertility. Therefore, we re-estimate equation (2) 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 on when prior children were born, 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 were announced.

The two-child laws may also affect the sex ratio at birth. For example, parents who do not have the desired number of sons when the law is announced and who can have an additional birth without violating the limit may be more likely to practice sex-selection. 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 specification where the outcome variable is an indicator for the child being male:

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

This specification is similar to specification (2) except that we also control for the sex of the first child, $Girl_i$. We focus on second and higher parity births as the prior literature has shown that, despite the availability of prenatal sex-determination technology, sex of the first birth is plausibly random in India (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. 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 specification (3). 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.[22]

The two-child limits may also improve child health. If the laws lead to fertility decline, children in smaller families are likely to receive higher per capita resources. Moreover, parents who decrease fertility to maintain office eligibility are more likely to plan fertility timing better and to invest more in these children to ensure their survival, relative to when there was no fertility constraint. In other words, the limits induce parents to improve the quality of children. We test if this is the case by estimating the impact of the fertility limits on the probability of neonatal, infant, or under-5 mortality (Mor_{jbsta}) of child j of parity b born in state s and in year t to mother of age a using the following specification:

$$Mor_{jbsta} = \alpha + \beta_1 Treat_{st} * Male_j + \beta_2 Treat_{st} + \psi Male_j + X_j^{'}\delta + \gamma_s + \theta_t + \phi_b + \nu_s * t + \mu_{sa} + \epsilon_{jbsta} \quad (4)$$

Neonatal mortality implies death during the first month of birth; infant mortality means death during the first year of birth; and under-5 mortality is defined as death before age five. The variable $Male_j$ indicates that child j is male. For mortality regressions, we drop children that are less than one month, or one year, or five years old to allow each child in the sample "full exposure" to the risk of, respectively, neonatal, infant, and under-5 mortality. In addition to the socioeconomic variables described previously, we include fixed effects for state, year of birth, parity, and state x mother's age, and state-specific linear time trends in this specification. The coefficient β_2 estimates the causal effect of the limits on the mortality for girls and β_1 estimates the differential effect on the mortality for boys.

The inclusion of state and year fixed effects controls for all time-invariant state-level variables

[22]Later, we also check if the results are similar when no restrictions are imposed on the timing of the first birth while estimating specification (3).

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. As the total number of 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.[23]

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 maternal, paternal, and household characteristics as dependent variables in the estimation of equation (2) 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. Moreover, to the best of our knowledge, during the sample time-frame, 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 Effects on marginal fertility

We start by examining the evolution of the hazard of birth (for birth orders one to five) in treatment states using specification (1). The estimated α_k coefficients for the annual hazard of birth over a 20-year period (16-year period for the hazards of fourth and fifth birth) are presented in Figures 2

[23]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.

and 3 and in Table A.3. The year before announcement is the omitted year and standard errors are clustered by state.

Figure 2 shows clear declines in the hazards of third, fourth, and fifth birth during the post-treatment period. The magnitude of the decline is the largest for the probability of a third birth. The smaller effects on the hazards of fourth and fifth births, relative to that of the third birth, are reasonable as couples that have three or four children have most likely achieved their desired fertility; so they are less likely to have a fourth or fifth birth even in the absence of the fertility limits. The decline in the hazards of third and higher-order births suggests that the two-child limits induced couples to sacrifice additional births to maintain electoral eligibility. Although this does not necessarily imply a decrease in completed fertility, the fact that the hazards of birth continue to decrease for six to nine years after the announcements indicates that our results are less likely to be driven by a mere postponement of these births.

Although the fertility limits do not impose any constraints on first or second births; and desired fertility is above one in India, we may still expect to see a decline in the hazards of the first two births after the announcements for two reasons. First, couples that reduce fertility to maintain electoral eligibility may deliberately increase birth spacing to ensure healthier births. Second, any potential sex-selection for second births induced by the fertility limits may translate into lower hazard of second birth in the event study graphs. These shifts in timing should lead to a U-shaped pattern for the event study graph in the post-treatment period, i.e, a decrease in the hazard of birth in the years immediately following the announcement and an eventual increase. On the other hand, some couples may rush to have their first two children if they fear or anticipate even stricter limits on fertility in the near future. The estimates in Figure 3 capture these combined effects. The hazard of first birth does appear to have a slight U-shape in the post-treatment period. Similarly, for the second birth, there seems to be a decline, although several years after the announcement.

Next we present the net impacts of the limits on marginal fertility estimated using specification (2) where we constrain the year of last birth to be in the pre-treatment period. Table 4 presents results for the hazard of third birth while the estimates for first, second, and fourth births are in

Appendix Table A.5. 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, and at birth orders beyond three, several households may have already satisfied their desired fertility by the time the policy is announced. 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 for couples that had two children when the limits were announced in their state, 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 1.97 p.p. in column (2). This effect is large, translating into a 9 percent decline from the baseline hazard of 22.21 percent of having a third birth in a given year before announcement. The inclusion of state × mother's age fixed effects in column (3) changes the estimated decline in third births marginally to 1.91 p.p.. Note that these estimates are net of any grace-period driven increase in the hazard of third birth. Appendix Table A.5 shows no significant net effects on the hazard of first and second births for couples that, respectively, were childless or had one child at announcement.[24]

In Table 5, we present results from estimating specification (2) inclusive of state-specific linear time trends and state × mother's age fixed effects, separately by socioeconomic group. Columns (1)-(4) show that among caste groups, the greatest statistically significant decline in the hazard of third birth is in SC families of 3.65 p.p., which represents a 14 percent decline from the baseline pre-announcement hazard of third birth for these households. As described in Section 2, SC households 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

[24]We re-estimated column (3) of Table 4 without imposing any restrictions on when the first two births occurred; see column (1) of Table A.4 for the results. The coefficient of $Treat_{st}$ remains negative and significant while the magnitude only changes slightly.

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) and the coefficients are smaller than they are for SCs. In a similar vein, columns (5)-(6) reveal a statistically significant decline in third births among poorer households (low SLI) of 2.37 p.p. (9 percent), whereas wealthier households with a high SLI score show no visible decline in the probability of having a third birth in column (6).

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)-(10) are always negative, they are significant only if the husband has no schooling. In line with the pattern of results in the rest of the table, the decline is larger for uneducated husbands than for those who are educated.

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 12 percent for high SLI mothers as compared to 25 percent for low SLI mothers.

5.2 Effects on the total number of children

The results in the previous two tables examine 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 specification (2) 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 specification (2), in these regressions we do not impose any restrictions on when prior children were born, 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 having 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 (2) 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. Panel B in Table 6 shows a similar pattern of results, although the coefficients are not always as significant as those in panel A unless we exclude state x mother's age fixed effects (see Table A.6).[25]

Figure 4 displays the probability that a couple has more than two living children in a given year over a 16-year period that includes 9 pre-treatment and 6 post-treatment years. This graph presents estimates from specification (1) for the treatment state sample. Consistent with the findings in Table 6, the fertility limits led to a sharp and sustained decline in the likelihood that a couple has more than two children.

Next, we examine heterogeneity in the effects on the total number of children by socioeconomic characteristics of the parents using specification (2). 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.[26] In panel A, the probability of

[25]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. The estimated effects on the number of children remain similar to those in Table 6 despite the age restriction; these results are available upon request.

[26]The estimates for the effects on probability of less than three living children are by construction exactly the same

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

To the extent that modern methods of contraception are accessible, 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[27] using specification (2), but modify it so that the time subscript 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, after conditioning on the year of interview, on years since last birth (or marriage, if the mother has only one child), on the mother's age at the time of interview, and on the previous set of covariates. Unlike prior regressions, here we use repeated cross-sections of data.

Appendix Table A.8 presents the estimates. 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. 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. All three

as those in Table 7 but of the opposite sign.

[27]Modern methods of contraception comprise male or female sterilization, pills, condoms, intrauterine devices, diaphragms, and injections.

columns show a significant increase in contraceptive use due to the limits; column (3) implies a 3.8 p.p. or an 8.03 percent increase from a baseline prevalence of 47.3 percent.

In columns (4)-(6), we examine the sub-sample of women who had 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 reduced childbearing after two children, we should observe a larger increase in the contraceptive use of women with two children as compared to the rest. The results are consistent with this hypothesis. While there is a 4 p.p. increase in contraceptive use for women who had two children, the coefficient for women who had three children is 0.018 and insignificant, and is -0.014 and insignificant for women who had one child. For families with two children, the coefficient translates into a 9.8 percent increase in the use of modern contraceptive methods.

We also estimate the effect of the limits on wife's or husband's sterilization in a hazard framework using the woman-year panel and specification (2).[28] For each woman, we drop the years after the year of sterilization from our sample and estimate the impact on the hazard of sterilization in a given year. We find significant increases in sterilization rates for couples where either the husband or the wife have zero years of schooling; these couples also have the highest baseline usage of sterilization.

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 specification (3). 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.[29] 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

[28] These results are available upon request.

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

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.[30] 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 above 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* castes in Andhra Pradesh (Deshpande and Ramachandran (2013)).[31] 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.[32] 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

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

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

[32]In Appendix Table A.7, we verify that the fertility limits did not affect the sex ratio of first births.

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 panel B with OBC households exhibiting a significant 5.57 p.p. (10.74 percent) increase in the probability that a birth is male.[33]

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 (27.1 percent),[34] that have highly adverse sex ratios of respectively 861, 922, 920, and 922 females per 1000 males in the 2001 Census of India.

5.5 Effects on child health

Table 9 reports the results on neonatal, infant, and under-5 mortality from specification (4).[35] For neonatal mortality, there is a decline for both boys and girls, albeit an insignificant one. Examining the impacts separately by birth order shows that the limits decreased neonatal mortality among first births (albeit insignificantly), consistent with the slight delay in the timing of first births.[36] Neonatal

[33]Columns (2)-(6) in Table A.4 show that our sex ratio findings remain the same even without any restriction on the year of first birth. These results correspond to Panel B of Table 8 that includes both treatment and control states; the findings for the sample of only treatment states also remain unchanged.

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

[35]The results for the treatment state sample are similar and are available upon request.

[36]Mortality results by birth order are available upon request.

mortality for male second births declined significantly, but not for females. As neonatal survival is quite closely linked to prenatal and delivery conditions, these improvements suggest that parents also altered the fetal and at-birth environment, either because of greater per capita availability of resources or deliberately to improve child quality. The gender differences likely arise from the fact that male children are more fragile in early life. Thus, the improvements in prenatal care and delivery environment due to the limits benefitted male children more than female children.

On the other hand, the improvements in infant and under-5 mortality are larger for girls (respectively, 10.34 percent and 20.62 percent) than for boys, who also experience mortality declines.[37] As infant and child mortality rates for boys are smaller than for girls at baseline (and substantially so for higher-order births and those preceded by a first girl (Anukriti et al. (2016)), this heterogeneity by gender is not surprising because girls have "more room" for improvement in survival.

These coefficients are likely to underestimate the true gains in survival since children born pre-announcement (the comparison group in specification (4)) may also benefit from lower completed fertility of their parents.

6 Robustness

In this section we conduct several checks to ensure we are capturing the causal impact of the fertility limits. First, in Appendix Table A.9 we re-estimate the effect on the hazard of third birth (using specification (2)) by dropping one treatment state at a time. Our findings remain the same; this shows that our estimates are not driven by any one particular treatment state.

Next we conduct a placebo test by reassigning the treatment to a year before the actual law was announced in a treatment state, and re-estimating the effect on the hazard of third birth using specification (2). We maintain the same gap between announcements across treatment states as in reality; so, a placebo treatment year (t_0) in the first state determines the placebo announcement year for the remaining treatment states as ($t_0 + k$), where k is the number of years between the

[37]Based on wild-cluster bootstrapped errors, there is no significant difference in the under-5 mortality decline for boys and girls.

two actual announcements. Hence, the latest placebo "first law" that we can examine is 1983 (i.e., the first state enacts in 1983 and the last state enacts in 1993), as we drop the years after actual announcement in treatment states. If our results are capturing the causal effect of the limits, we should not find significant effects in these placebo regressions. In Appendix Table A.10, each column uses a different year as a placebo treatment year for the first law. The coefficients are always insignificant.[38]

Our results so far set $Treat_{st} = 0$ for the control states for the entire sample period. Moreover, in specification (2), for the hazard of birth b regressions we restricted the sample to observations where the $(b-1)$ birth took place before the announcement. Since no limits exist in the control states, there is no restriction on the year of $(b-1)$ birth for them. This differential restriction may bias our findings if women in the treated states had their last birth much earlier than women in the control states. Although we include fixed effects for years since last birth in our hazard regressions, we nevertheless conduct two further tests to check the robustness of our findings by assigning fictitious treatment years to our control group. We estimate the following equation:

$$Y_{iast} = \omega + \alpha T_s * Post_{st} + \beta Post_{st} + X_i^{'}\delta + \gamma_s + \theta_t + \psi_a + \nu_s * t + \mu_{sa} + \epsilon_{iast} \tag{5}$$

For a treatment state, $Post_{st}$ indicates years during which the law is in place; the year before the year of announcement is the omitted year. For a control state, $Post_{st}$ indicates 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. The β coefficients estimate the "effect" of fictitious laws on control states and α measures the actual policy effect on the treatment states after differencing out the "effect" on the control group. Appendix Table A.11 shows the effects on the hazard of third birth, on various child compositions, and on the sex ratio from specification (5). Reassuringly, none

[38]We conducted an alternative test by assigning the same placebo treatment year to all treatment states, thus ignoring the spacing in announcement across states. Out of 18 placebo treatment years (1976-1993), we find a significant negative "effect" in only two cases, further supporting our findings.

of the β coefficients are significant. Moreover, estimates of the coefficient α are in line with our previous findings.

One may still worry that assigning fictitious treatment years to a control state based on its bordering treatment states is not a strict enough test. Therefore, we also perform a check where, irrespective of which treatment state it borders, we assign the same treatment year to each control state, and repeat this exercise using each year during 1993-1999. Appendix Table A.12 shows that the results are remarkably similar in magnitude to those in Table 4. The estimated coefficient α is significant in all but one column, and β is never significant.

Lastly, we use the synthetic control method proposed by Abadie and Gardeazabal (2003) and Abadie et al. (2010) as an alternative estimation approach.[39] 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 multiple treatment states with different treatment years, we combine all treatment states into one group and redefine the time variable as years from announcement. We again assign placebo treatment years to neighboring control states to define this time variable for them. We collapse the data to state-year level, and using a vector of demographic and socioeconomic characteristics, construct a 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 5 shows the evolution of the probability of having two living children and the likelihood of a third birth in a given year before and after the limits. In both cases, the synthetic control state resembles the treatment states closely before the limits were announced.[40] However, for nearly the whole period after the announcement, couples in treatment states are much more likely to report having two living children and less likely to have a third birth compared to the synthetic control state. These results should however be interpreted with caution as a lot of states receive zero weight

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

[40]The exact weights assigned to each donor state in the construction of the synthetic control state are available in Table A.13.

in the construction of the synthetic control.

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. In 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.[41] 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 1.91 p.p. in treatment states, i.e., 1.91 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.[42] Thus, 208,090 (which is 1.97 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., 416,180 individuals gave up a third child due to the limits. The total number of couples who adjusted fertility (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

[41]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.

[42]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.

the second child. Notably, we also find that the decline in fertility due to the limits reduced early-life child mortality by a significant margin.

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. 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 example, 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 9 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 and improve children's health 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, through the role-model influence of their leaders, or by 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 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. Another north Indian state, Haryana, has also imposed education and sanitation requirements in the 2016 village council elections. A key difference between fertility limits and education requirements is that while prospective candidates of childbearing ages can choose to not have children in response to the fertility limits, they are likely to be too old to increase years of schooling to prevent disbarment. In this sense, the effect of fertility limits on birth rate is more likely to be immediate whereas the education requirements are more likely to affect schooling of the younger cohorts who will become candidates after several years. Moreover, there is a lot more state-year variation in fertility policies relative to other candidate requirements, making the current set-up a useful one to examine. Further, we show 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.

Fertility restrictions on elected leaders also have implications for political representation of various 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 relatively well-off among the lower-castes are able to meet the eligibility criteria. Gender-based quotas could also be undermined 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.2 that were disqualified for violating the limits. Our hazard results suggest that the fertility decline is significant even for low socioeconomic status families, diminishing some of these concerns, but the possibility remains for the most disadvantaged households within each group we examine. An explicit examination of the impact of these limits on political outcomes is crucial but is beyond the scope of this paper.

References

Abadie, A., A. Diamond, and J. Hainmueller (2010): "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program," *Journal of the American Statistical Association*, 105.

Abadie, A. and J. Gardeazabal (2003): "The Economic Costs of Conflict: A Case Study of the Basque Country," *American Economic Review*, 93.

Almond, D., H. Li, and S. Zhang (2013): "Land Reform and Sex-Selection in China," *NBER Working Paper 19153*.

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

Anukriti, S., S. Bhalotra, and H. Tam (2016): "On the Quantity and Quality of Girls: New Evidence on Abortion, Fertility, and Parental Investments," *IZA Discussion Paper No. 10271*.

Ashraf, Q. H., D. N. Weil, and J. Wilde (2013): "The Effect of Fertility Reduction on Economic Growth," *Population and development review*, 39, 97–130.

Bardhan, P. and D. Mookherjee (2000): "Capture and Governance at Local and National Levels," *American Economic Review: Papers and Proceedings*, 90, 135–139.

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

Besley, T., R. Pande, L. Rahman, and V. Rao (2003): "The Politics of Public Good Provision: Evidence from Indian Local Governements," *Journal of the European Economic Assocation*, 2, 416–426.

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

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

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

Busso, M., J. Gregory, and P. Kline (2013): "Assessing the Incidence and Efficiency of a Prominent Place Based Policy," *American Economic Review*, 103, 897–947.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008): "Bootstrap-Based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, 90, 414–27.

Canning, D. and T. P. Schultz (2012): "The Economic Consequences of Reproductive Health and Family Planning," *The Lancet*, 380, 165–171.

Chattopadhyay, R. and E. Duflo (2004): "The Impact of Reservation in the Panchayati Raj: Evidence from a Nationwide Randomized Experiment," *Economic and Political Weekly*, 39, 979–986.

Das Gupta, M. (2010): "Family Systems, Political Systems, and Asia's 'Missing Girls': The Construction of Son Preference and Its Unraveling," *Asian Population Studies*, 6, 123–152.

Das Gupta, M. and P. Bhat (1997): "Fertility Decline and Increased Manifestation of Sex Bias in India," *Population Studies*, 51, 307–315.

Das Gupta, M., J. Zhenghua, L. Bohua, X. Zhenming, W. Chung, and B. Hwa-Ok (2003): "Why is Son Preference So Persistent in East and South Asia? A Cross-country Study of China, India and the Republic of Korea," *Journal of Development Studies*, 40, 153–187.

DESHPANDE, A. AND R. RAMACHANDRAN (2013): "How Backward are the Other Backward Classes? Changing Contours of Caste Disadvantage in India," *Center for Development Economics Working Papers*.

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

FISMAN, R., F. SCHULZ, AND V. VIG (2014): "The Private Returns to Public Office," *Journal of Political Economy*, 122.

GENICOT, G. AND D. RAY (2014): "Aspirations and Inequality," *NBER Working Paper 19976*.

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

JOSHI, S. AND T. P. SCHULTZ (2007): "Family Planning as an Investment in Development: Evaluation of a Program's Consequences in Matlab, Bangladesh," *Yale University Economic Growth Center Discussion Paper*.

MILLER, G. (2010): "Contraception as Development? New Evidence from Family Planning in Colombia," *The Economic Journal*, 120, 709–736.

OLINTO, P., K. BEEGLE, C. SOBRADO, AND H. UEMATSU (2013): "The State of the Poor: Where are the Poor, Where is Extreme Poverty Harder to End, and What is the Current Profile of the World's Poor?" *Economic Premise*.

PÖRTNER, C. C. (2010): "Sex Selective Abortions, Fertility and Birth Spacing," *University of Washington, Department of Economics, Working Paper UWEC-2010-4-R*.

PRITCHETT, L. (1994): "Desired fertility and the impact of population policies," *Population and development review*, 20, 1–55.

ROSENBLUM, D. (2013): "The effect of fertility decisions on excess female mortality in India," *Journal of Population Economics*, 26, 147–180.

ROSENZWEIG, M. R. AND J. ZHANG (2009): "Do Population Control Policies Induce More Human Capital Investment? Twins, Birth Weight and China's "One-Child" policy," *The Review of Economic Studies*, 76, 1149–1174.

SASTRY, T. (2014): "Towards Decriminalisation of Election and Politics," *Economic and Political Weekly*, XLIX, 34–41.

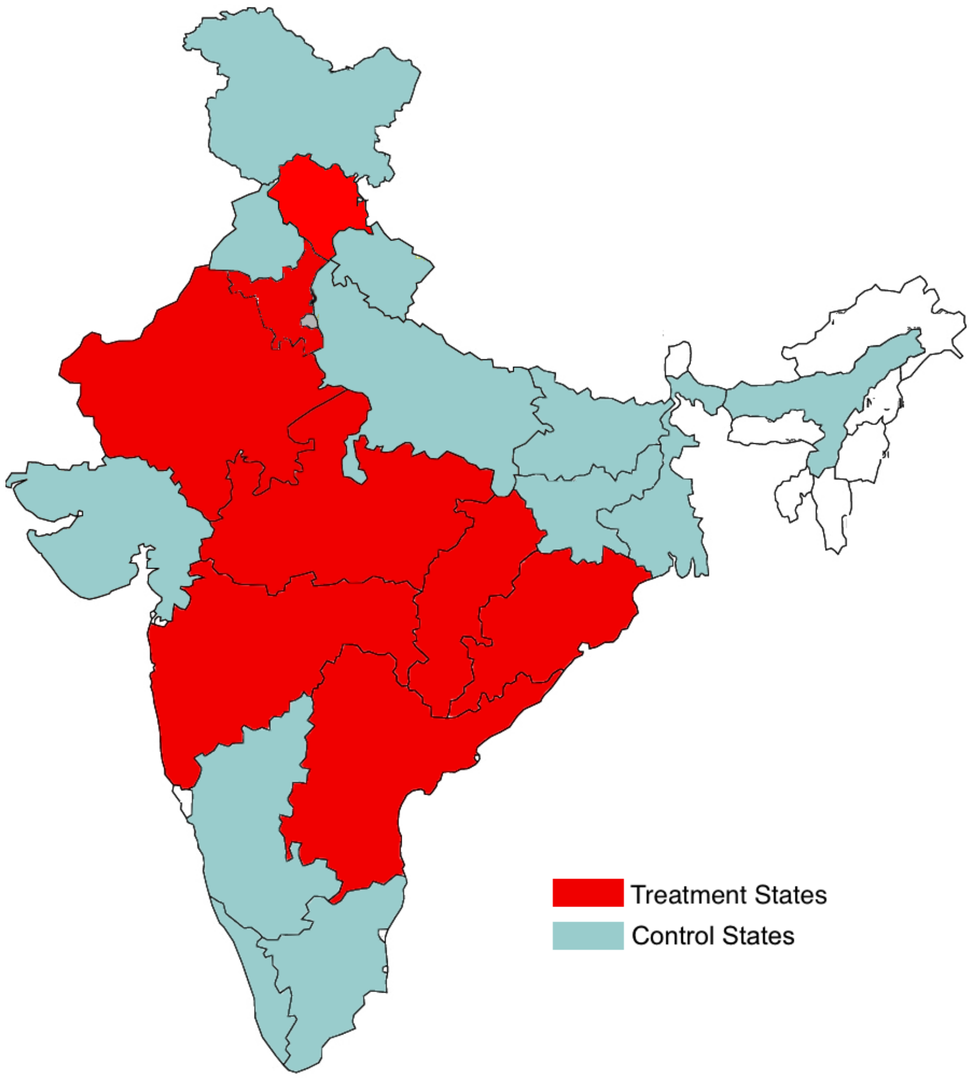
SRINIVAS, M. (1962): *Caste in Modern India: And Other Essays*, Asia Publishing House, Bombay.

VISARIA, L. (2005): "Female Deficit in India: Role of Prevention of Sex Selective Abortion Act," *mimeo*.

VISARIA, L., A. ACHARYA, AND F. RAJ (2006): "Two-Child Norm: Victimising the Vulnerable?" *Economic and Political Weekly*, XLI.

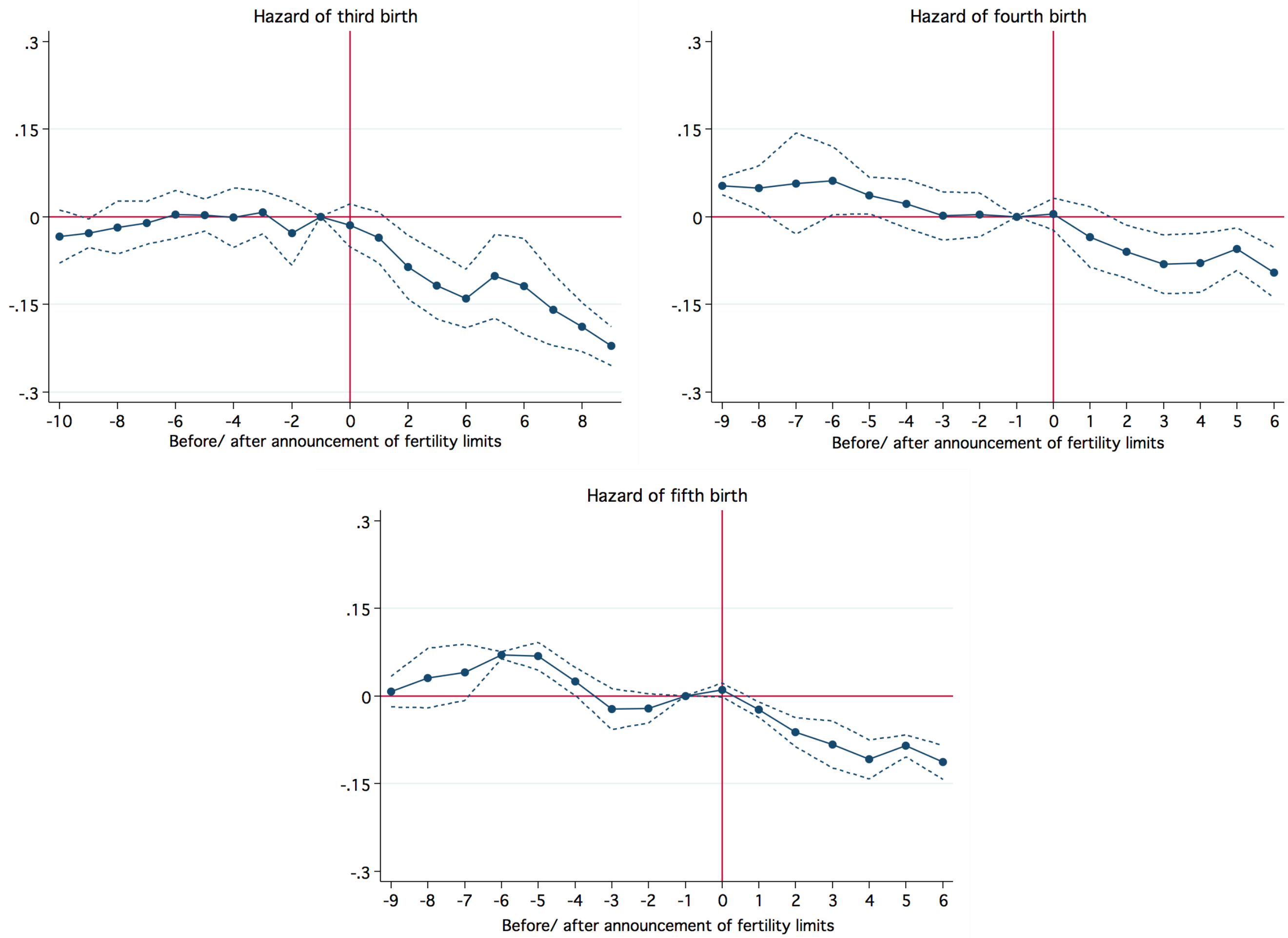
9 Figures and Tables

Figure 1: Treatment and control states



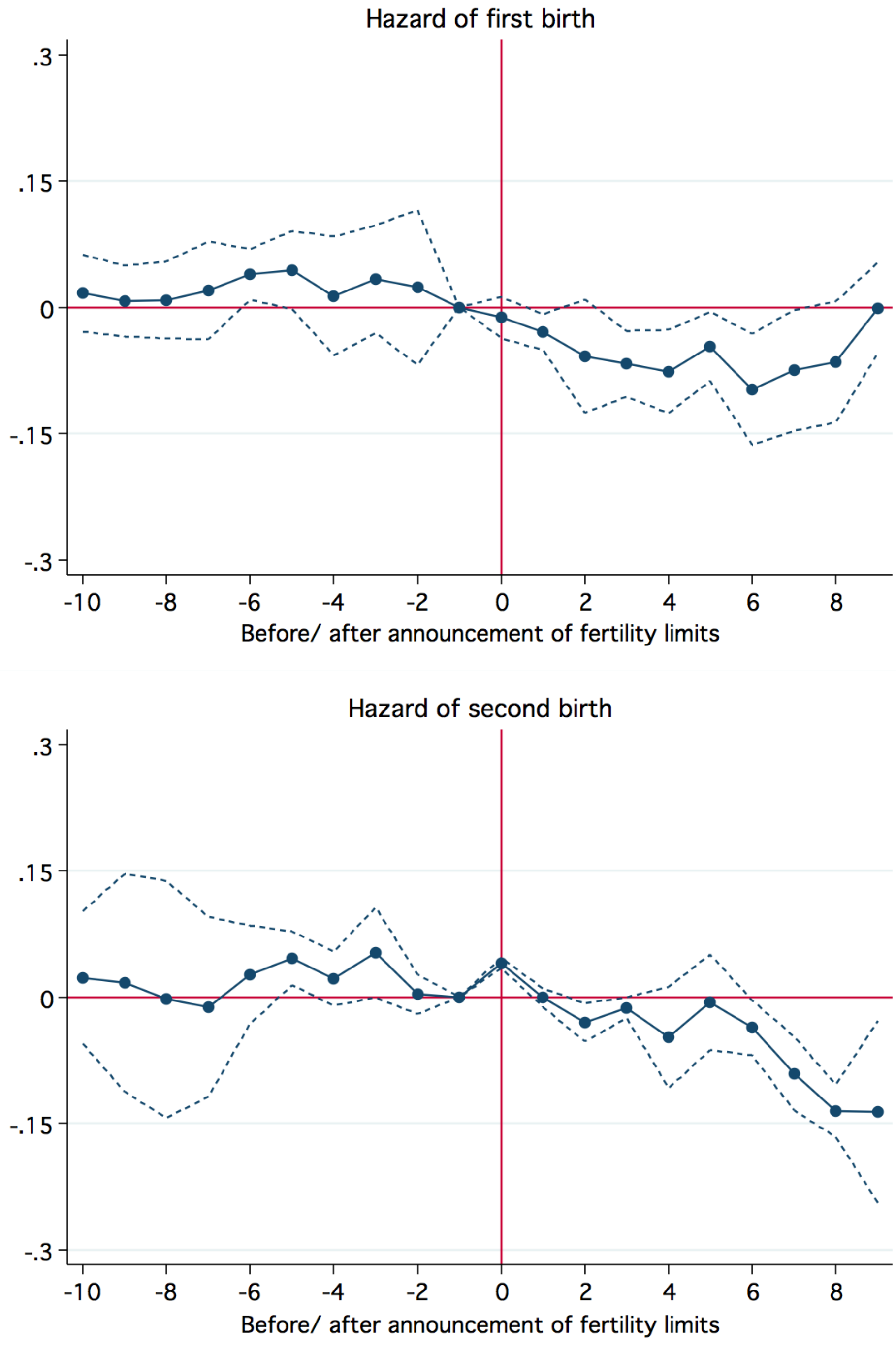
NOTES: Treatment states are those that have enacted a fertility limit at some point during our sample period. Control or never-treated states are those that have not.

Figure 2: Hazards of third, fourth, and fifth birth in treatment states



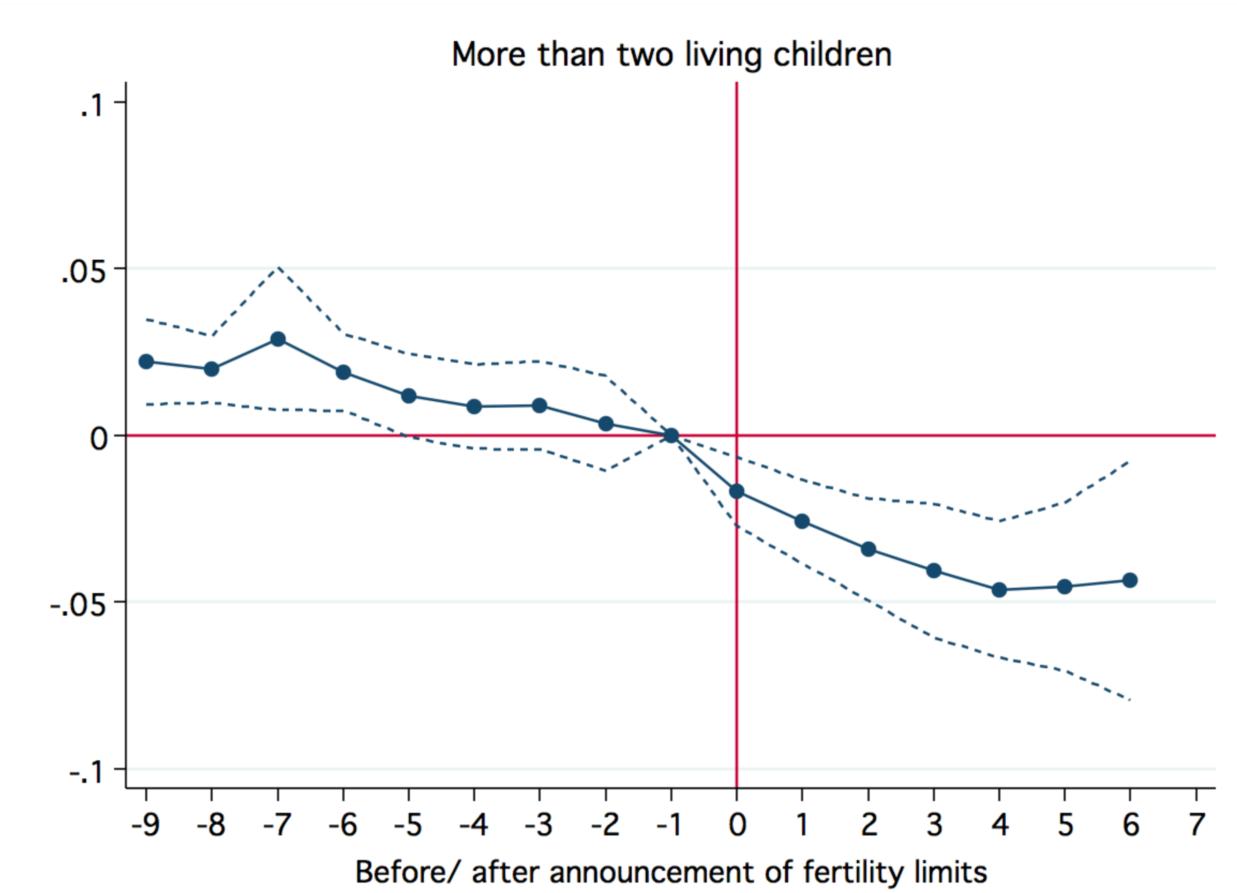
NOTES: This graph plots the estimates of α_k coefficients and their 95 percent confidence intervals (dashed lines) from estimating specification (1). Due to sample size issues, for hazards of fourth and fifth birth, the sample time period is restricted to 9 years before and 6 years after treatment. The 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 up to and including the year of birth b. The sample is restricted to only the treatment states. The vertical line denotes the year of announcement and the year before announcement is the omitted year. Standard errors are clustered by state.

Figure 3: Hazards of first and second birth in treatment states



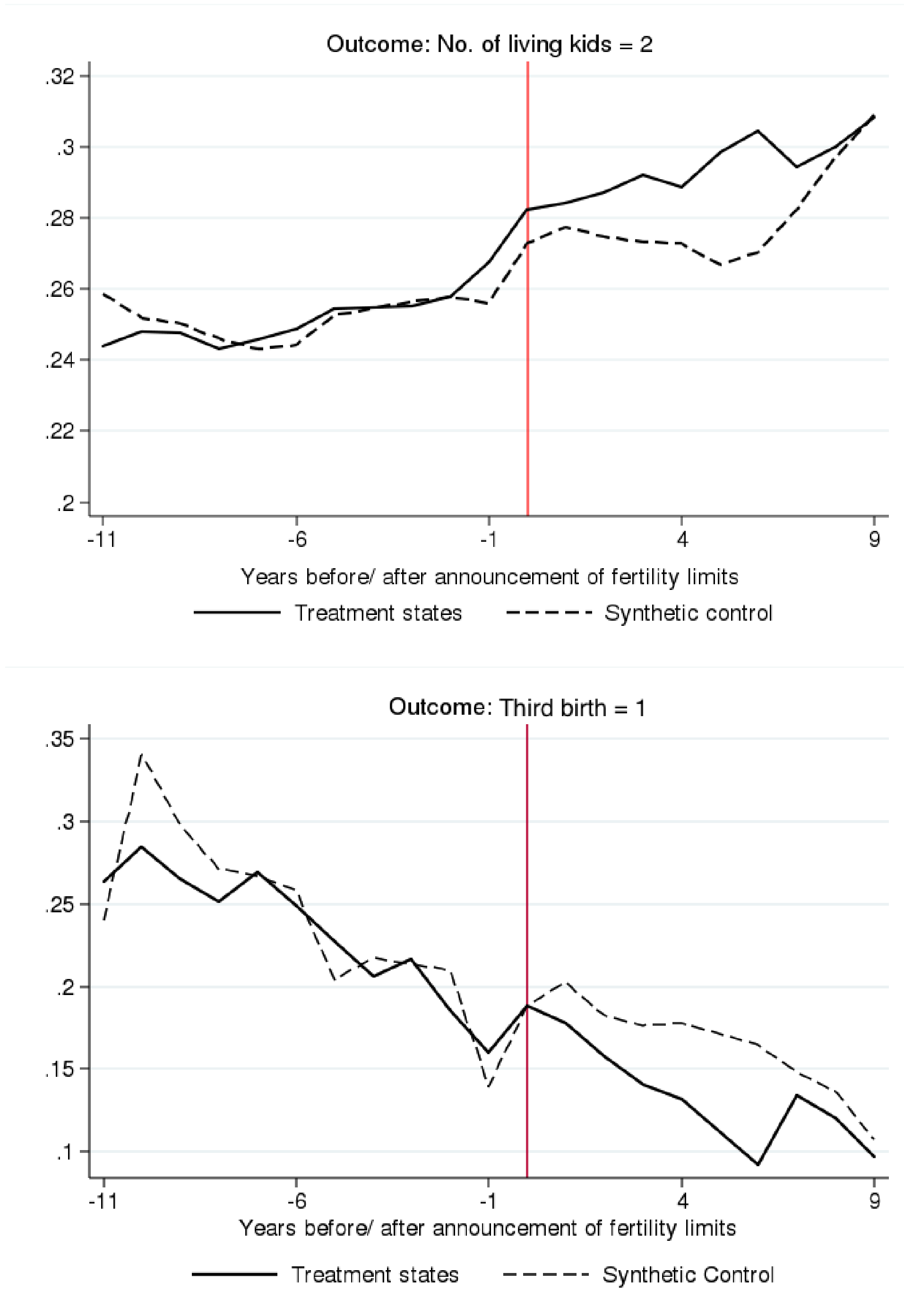
NOTES: This graph plots the estimates of α_k coefficients and their 95 percent confidence intervals (dashed lines) from estimating specification (1). The outcome variables are indicators for births of order one and two. Where birth of order two is the outcome, the sample includes years after first birth and up to and including the year of second birth. Where the first birth is the outcome, the sample includes years after the year of marriage and up to and including the year of first birth. The sample is restricted to only treatment states. The vertical line denotes the year of announcement and the year before announcement is the omitted year. Standard errors are clustered by state.

Figure 4: Probability of having more than two living children in treatment states



NOTES: This graph plots the estimates of α_k coefficients and their 95 percent confidence intervals (dashed lines) from estimating specification (1) where $k \in [-8, 7]$. The outcome variable is an indicator for having more than two living children in year t. The sample is restricted to only the treatment states. The vertical line denotes the year of announcement and the year before announcement is the omitted year. Standard errors are clustered by state.

Figure 5: Effect on fertility using the synthetic control method



NOTES: The outcome variable in the top figure is an indicator for two living children in a given year and in the bottom graph indicates third birth in a given year. In the bottom graph, the sample is restricted to years after the second birth and up to and including the third birth. 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 in Appendix Table A.13. The vertical line indicates the year of 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)[43]
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. 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

	Never treated		Treated			
			$Post_{st} = 0$		$Post_{st} = 1$	
Variable	Mean (1)	Std. Dev. (2)	Mean (3)	Std. Dev. (4)	Mean (5)	Std. Dev. (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
1-4 years	0.085	0.279	0.079	0.269	0.079	0.269
5-10 years	0.215	0.411	0.175	0.380	0.227	0.419
10 - 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
1-4 years	0.104	0.305	0.106	0.308	0.095	0.293
5-10 years	0.311	0.463	0.308	0.462	0.327	0.469
10 - 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 ≥ 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]
≥ 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]
≥ 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)
$Treat_{st}$	-0.0185	-0.0197	-0.0191
	[0.0120]	[0.0093]**	[0.0086]**
	(0.0130)	(0.0103)*	(0.0095)*
Baseline mean		0.2221	
N		202,797	
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 specification (2). The outcome variable is an indicator for third birth. The sample is restricted to years after second birth and up to and including third birth for mothers whose second child was born before the year of announcement. We also restrict years since second birth to ≤ 10. 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 $Treat_{st} = 0$. *** 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)
$Treat_{st}$	-0.0365	0.0197	-0.0209	-0.0118	-0.0237	-0.0011	-0.0168	-0.0177	-0.0145	-0.0266
	[0.0142]**	[0.0191]	[0.0106]*	[0.0136]	[0.0100]**	[0.0110]	[0.0112]	[0.0098]*	[0.0088]	[0.0123]**
	(0.0174)**	(0.0185)	(0.0120)	(0.0128)	(0.0107)**	(0.0103)	(0.0103)	(0.0100)	(0.0087)	(0.0145)**
Baseline mean	0.2555	0.2589	0.2099	0.2104	0.2546	0.1245	0.1560	0.2704	0.2011	0.2682
N	33,040	19,111	46,128	104,518	120,644	20,028	86,008	116,789	139,252	63,545

NOTES: This table reports the coefficients from specification (2). The outcome variable is an indicator for third birth. The sample is restricted to years after second birth and up to and including third birth for mothers whose second child was born before the year of announcement. We also restrict years since second birth to ≤ 10. 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 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:			**All states**		
$Treat_{st}$	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

NOTES: This table presents the regression estimates corresponding to specification (2) 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 $Treat_{st} = 0$. *** 1%, ** 5%, * 10%.

Table 7: Heterogeneous effects on the likelihood of > 2 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 (2) using an indicator for > 2 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:			**All states**		
$Treat_{st}$	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

NOTES: This table reports the coefficients from specification (3). 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. The baseline mean is calculated for observations where $Treat_{st} = 0$. *** 1%, ** 5%, * 10%.

Table 9: Effects on mortality

	Neonatal (1)	Infant (2)	Under-5 (3)
$Treat_{st} * Male_j$	-0.0022	0.0012	0.0162
	[0.0027]	[0.0046]	[0.0079]*
	(0.0030)	(0.0044)	(0.0112)
$Treat_{st}$	-0.0007	-0.0088	-0.0248
	[0.0035]	[0.0045]*	[0.0133]*
	(0.0035)	(0.0060)*	(0.0173)*
N	273,064	257,281	190,697
Baseline mean (girls)	0.0516	0.0851	0.1203
Baseline mean (boys)	0.0593	0.0873	0.1156

NOTES: This table reports the coefficients from specification (4). The outcomes variables are indicators for death during the first month, during the first year, and during the first five years of birth in columns (1), (2), and (3), respectively. The baseline mean is calculated for observations where $Treat_{st} = 0$. 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: Fraction of women whose ideal number of children > 2

Fraction that desire > 2 children		
SC/ST	OBC	Upper caste
0.56	0.44	0.48
Hindu	Muslim	Other religions
0.49	0.65	0.33
Low SLI	Medium SLI	High SLI
0.59	0.37	0.23

NOTES: This tables uses data from NFHS-1,2,3 to show the fraction of women within each subsample whose ideal number of children reported at the time of survey is strictly greater than two. SC, ST, and OBC respectively denote Scheduled Caste, Scheduled Tribe and Other backward Class. Low, Medium, and High SLI (standard of living index) are equal to one if the household respectively belongs to the bottom-third, the middle-third, and the top-third of household wealth distribution in all of India

Table A.2: 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.3: Effects on hazards of birth in treatment states

	Outcome: Birth = 1				
Coefficients of	**1st**	**2nd**	**3rd**	**4th**	**5th**
$Treat_{st}$	(1)	(2)	(3)	(4)	(5)
$t-9$	0.0168	0.0233	-0.0341*		
	[0.0144]	[0.0248]	[0.0143]		
$t-8$	0.0074	0.0171	-0.0281**	0.0525***	0.0074
	[0.0132]	[0.0407]	[0.0077]	[0.0058]	[0.0102]
$t-7$	0.0087	-0.0027	-0.0186	0.0493**	0.0306
	[0.0144]	[0.0442]	[0.0142]	[0.0147]	[0.0198]
$t-6$	0.0199	-0.0116	-0.0106	0.0565	0.0405*
	[0.0183]	[0.0336]	[0.0115]	[0.0337]	[0.0188]
$t-5$	0.0391**	0.0267	0.0039	0.0618**	0.0698***
	[0.0094]	[0.0183]	[0.0129]	[0.0228]	[0.0024]
$t-4$	0.0442*	0.0461**	0.0028	0.0363**	0.0679***
	[0.0146]	[0.0101]	[0.0086]	[0.0122]	[0.0092]
$t-3$	0.0136	0.0220	-0.0017	0.0224	0.0251**
	[0.0222]	[0.0101]	[0.0160]	[0.0162]	[0.0094]
$t-2$	0.0335	0.0531*	0.0074	0.0012	-0.0224
	[0.0201]	[0.0169]	[0.0116]	[0.0161]	[0.0136]
$t-1$	0.0237	0.0034	-0.0282	0.0031	-0.0214*
	[0.0290]	[0.0074]	[0.0172]	[0.0146]	[0.0097]
t	0	0	0	0	0
$t+1$	-0.0122	0.0406***	-0.0149	0.0044	0.0101*
	[0.0077]	[0.0020]	[0.0115]	[0.0106]	[0.0046]
$t+2$	-0.0295**	-0.0006	-0.0359*	-0.0346	-0.0236***
	[0.0067]	[0.0034]	[0.0137]	[0.0203]	[0.0051]
$t+3$	-0.0583*	-0.0300**	-0.0863**	-0.0603**	-0.0621***
	[0.0211]	[0.0071]	[0.0171]	[0.0177]	[0.0096]
$t+4$	-0.0671**	-0.0126**	-0.1176***	-0.0815***	-0.0830***
	[0.0123]	[0.0038]	[0.0180]	[0.0196]	[0.0156]
$t+5$	-0.0762**	-0.0479*	-0.1404***	-0.0792**	-0.1087***
	[0.0156]	[0.0189]	[0.0158]	[0.0197]	[0.0130]
$t+6$	-0.0464**	-0.0062	-0.1019**	-0.0557**	-0.0856***
	[0.0129]	[0.0178]	[0.0226]	[0.0142]	[0.0072]
$t+7$	-0.0973**	-0.0365**	-0.1195**	-0.0956***	-0.1137***
	[0.0207]	[0.0103]	[0.0258]	[0.0169]	[0.0113]
$t+8$	-0.0750**	-0.0906***	-0.1596***		
	[0.0225]	[0.0138]	[0.0192]		
$t+9$	-0.0648*	-0.1352***	-0.1889***		
	[0.0226]	[0.0098]	[0.0131]		
$t+10$	-0.0010	-0.1362**	-0.2217***		
	[0.0169]	[0.0339]	[0.0104]		
N	58,444	42,227	44,786	48,556	28,160

NOTES: This table presents the regression estimates from 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 up to and including birth b for mothers whose $(b-1)^{th}$ child was born before the year of announcement. The sample is restricted to only the treatment states. Standard errors in brackets are clustered by state. The year before announcement is the omitted year. *** 1%, ** 5%, * 10%.

Table A.4: Effects without restrictions on timing of prior births

		Male = 1				
	3rd birth = 1	**All**	**SC**	**ST**	**OBC**	**Upper**
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_{st}$	-0.0121*	0.0066	-0.0156	-0.0061	0.0437***	-0.0027
	[0.0068]	[0.0047]	[0.0133]	[0.0113]	[0.0142]	[0.0122]
N	242,311	190,319	36,604	21,980	46,576	85,159

NOTES: This table reports the coefficients corresponding to column (3) of Table 4 and Panel B of Table 8. There is no restriction on the year of birth of the prior children. *** 1%, ** 5%, * 10%.

Table A.5: 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]
N		323,174	
B. 2nd birth = 1			
$Treat_{st}$	0.0069	-0.0016	0.0001
	[0.0092]	[0.0115]	[0.0099]
N		213,242	
C. 4th birth = 1			
$Treat_{st}$	0.0065	0.0001	-0.0003
	[0.0093]	[0.0056]	[0.0046]
N		160,970	
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 specification (2). 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 up to and including 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. *** 1%, ** 5%, * 10%.

Table A.6: Effects on the number of living children, all states

	Kids = 1	**Kids = 2**	**Kids = 3**	**Kids = 4**	**Kids = 5**
	(1)	(2)	(3)	(4)	(5)
Panel A:			**With state x mother's age FE**		
$Treat_{st}$	0.0008	0.0090	-0.0018	-0.0052*	-0.0024
	[0.0055]	[0.0068]	[0.0055]	[0.0026]	[0.0020]
N			1,143,057		
Panel B:			**Without state x mother's age FE**		
$Treat_{st}$	0.0011	0.0123**	0.0015	-0.0043	-0.0020
	[0.0052]	[0.0057]	[0.0047]	[0.0028]	[0.0018]
N			1,266,201		
Baseline mean	0.2351	0.2351	0.1711	0.0878	0.0379

NOTES: This table presents the regression estimates corresponding to specification (2) using indicators for 1/ 2/ 3/ 4/ 5 living children as the outcome variables. Both treatment and control states are included in the sample. Each column within a panel is a different regression. Standard errors in brackets are clustered by state. The baseline mean is calculated for observations where $Treat_{st} = 0$. *** 1%, ** 5%, * 10%.

Table A.7: Effects on the sex ratio of first births

Male = 1	**All**	**SC**	**ST**	**OBC**	**Upper**
	(1)	(2)	(3)	(4)	(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]
N	34,018	5,818	5,783	7,511	14,906
Baseline mean	0.5152	0.5062	0.5103	0.5162	0.5198
Panel B:			**All states**		
$Treat_{st}$	-0.0007	0.0325	-0.0063	-0.0304	0.0093
	[0.0076]	[0.0295]	[0.0339]	[0.0228]	[0.0162]
N	86,023	15,245	9,265	19,345	42,168
Baseline mean	0.5150	0.5128	0.5096	0.5173	0.5158

NOTES: This table reports the coefficients from specification (2). 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. The baseline mean is calculated for observations where $Treat_{st} = 0$. *** 1%, ** 5%, * 10%.

Table A.8: 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 (2), except that now the time subscript refers to the year of interview. The coefficients estimate the difference in contraceptive use for women interviewed before and after the fertility limits were enacted in their respective states. Unlike prior regressions, these specifications use repeated cross-sections of data. 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)-(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 and in parentheses are wild-cluster bootstrapped by state. The baseline mean is calculated for observations where $Treat_{st} = 0$. *** 1%, ** 5%, * 10%.

Table A.9: Robustness check: Drop one treatment state at a time

	State dropped:						
	Haryana	HP	MP	AP	Orissa	Maharashtra	Rajasthan
3rd birth = 1	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Treat_{st}$	-0.016*	-0.025***	-0.018*	-0.015*	-0.021**	-0.018*	-0.023**
	[0.008]	[0.008]	[0.0010]	[0.008]	[0.010]	[0.009]	[0.009]
N	197,322	193,760	187,645	194,904	193,506	193,483	191,396

NOTES: This table reports the coefficients from the strictest version of specification (2). The outcome variable is an indicator for third birth. Each column drops one treatment state at a time. The sample is restricted to years after second birth and up to and including third birth for mothers whose second child was born before the year of announcement. We also restrict years since second birth to ≤ 10. Standard errors in brackets are clustered by state. *** 1%, ** 5%, * 10%.

Table A.10: Robustness check: Assign placebo treatment years to treatment states

3rd birth = 1	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year of first law	1976	1977	1978	1979	1980	1981	1982	1983
$Treat_{st}$	-0.059	0.017	-0.066	-0.059	-0.015	-0.005	0.012	-0.012
	[0.128]	[0.069]	[0.041]	[0.039]	[0.020]	[0.017]	[0.019]	[0.029]
N	477	1,959	4,486	9,055	14,089	21,547	28,557	37,659

NOTES: This table reports the coefficients from the strictest version of specification (2). The outcome variable is an indicator for third birth. Each coefficient is from a separate regression with a different placebo treatment year for the first state to enact the limits. For the remaining treatment states, the placebo treatment year is automatically determined as we maintain the same gap between law enactment years across states as in reality. The sample is restricted to years before the actual treatment took place. The sample is restricted to years after second birth and up to and including third birth for mothers whose second child was born before the year of announcement. We also restrict years since second birth to ≤ 10. Standard errors in brackets are clustered by state. *** 1%, ** 5%, * 10%.

Table A.11: Robustness check: Assign fictitious treatment years to control states

	3rd birth = 1	**Kids = 1**	**Kids = 2**	**Kids = 3**	**Kids = 4**	**Male birth = 1**			
						SC	ST	OBC	Upper
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$T_s * Post_{st}$	-0.028**	0.000	0.014	-0.001	-0.008**	-0.016	-0.015	0.053***	-0.003
	[0.011]	[0.006]	[0.010]	[0.007]	[0.004]	[0.020]	[0.024]	[0.016]	[0.015]
$Post_{st}$	0.008	0.001	-0.007	-0.001	0.004	-0.005	0.034	0.000	0.005
	[0.010]	[0.004]	[0.006]	[0.004]	[0.002]	[0.014]	[0.026]	[0.010]	[0.012]
N	171,357	1,143,057	1,143,057	1,143,057	1,143,057	31,270	18,778	35,945	79,311

NOTES: This table reports the coefficients from specification (5). The outcome variable is an indicator for third birth in column (1), indicators for specific number of living children in columns (2)-(5), and an indicator for a birth being male in columns (6)-(9). In column (1), the sample is restricted to years after second birth and up to and including third birth for mothers whose second child was born before the year of announcement; we also restrict years since second birth to ≤ 10. In columns (6)-(9), 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. SC, ST, OBC, and Upper indicate Scheduled Caste, Scheduled Tribe, Other Backward Class, and upper caste households, respectively. Standard errors in brackets are clustered by state. *** 1%, ** 5%, * 10%.

Table A.12: Robustness check: Using alternate fictitious treatment years for control states

	Fictitious treatment year assigned to control states:						
	1993	1994	1995	1996	1997	1998	1999
3rd birth = 1	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$T_s * Post_{st}$	-0.0239*	-0.0234**	-0.0241*	-0.0265*	-0.0254*	-0.0242	-0.0376**
	[0.0116]	[0.0105]	[0.0131]	[0.0128]	[0.0140]	[0.0177]	[0.0172]
$Post_{st}$	0.0019	0.0000	-0.0013	-0.0016	-0.0040	-0.0057	0.0182
	[0.0102]	[0.0102]	[0.0130]	[0.0123]	[0.0123]	[0.0172]	[0.0174]
N	154,349	161,020	167,326	173,338	178,658	183,616	187,687

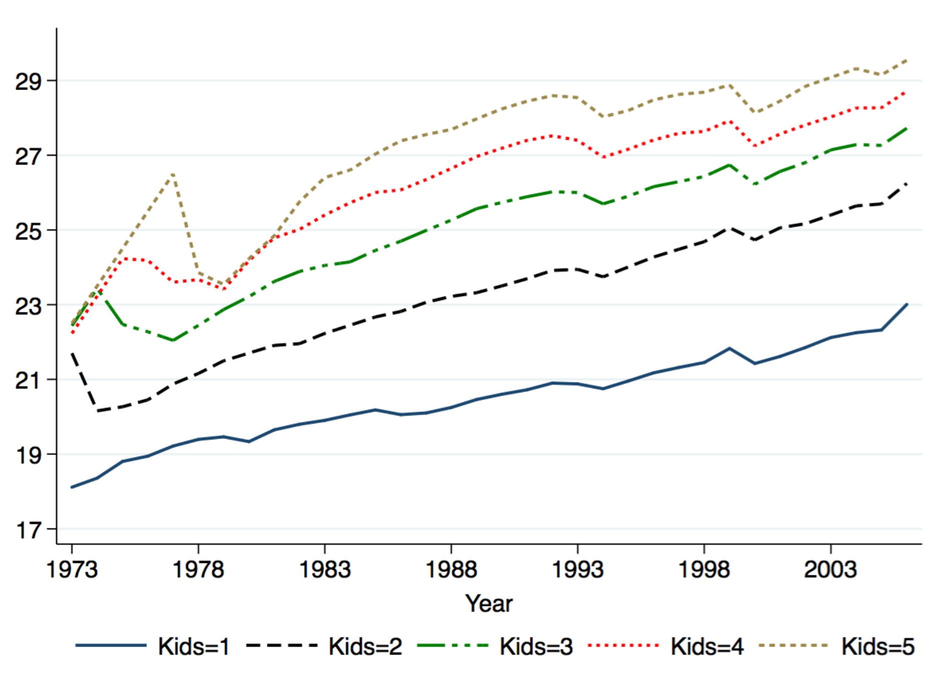
NOTES: This table reports the coefficients from specifications (5). All never-treated or control states are assigned the same "fake" or placebo treatment year that varies across columns. The outcome variable is an indicator for third birth. The sample is restricted to years after second birth and up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

Table A.13: Weights for states in the donor pool for the synthetic control method

State	Kids = 2 (1)	3rd birth = 1 (2)
Assam	0.307	0.094
Delhi	0	0
Gujarat	0.693	0.848
Kerala	0	0
Punjab	0	0
Tamilnadu	0	0.058
West Bengal	0	0

NOTES: This table presents the weights received by each control state in the donor pool for the synthetic control analysis.

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 ≤ age 33 in a given year.

B State-wise Regulations

1. Rajasthan:[44]

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[45], "...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:[46] "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."

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

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

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

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).[47]

3. Andhra Pradesh:[48]

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."[49]

4. Orissa:[50]

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:[51]

"...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

[47]Source: http://hindu.com/2006/07/22/stories/2006072207150500.htm
[48]Source: `http://www.ielrc.org/content/e9412.pdf`
[49]Further explanation at: `http://www.apsec.gov.in/RLBS_GPs/CLARIFICATIONS%202013/877%20-%20Qualification.pdf`.
[50]Source: http://secorissa.org/download/FAQ2.pdf
[51]Source: `http://www.indiankanoon.org/doc/1285129/`

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:[52]

"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...."

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