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

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- Representative democracy is good for welfare:
  - Promotes stable economic growth (Rodrik (2000), Mobarak (2005))
  - Narrows income inequalities (Acemoglu and Robinson (2000), Gradstein (2007))
  - Prevents elite capture (Foster and Rosenzweig (2004), Brown and Mobarak (2009))
- Wider candidate pool $\rightarrow$ better quality leaders in democratic systems

  (Besley and Coate (1997), Besley and Reynal-Querol (2011), Osborne and Slivinski (1996))
- Typically, minimal legal restrictions on who can become an elected democratic leader

- In practice, democratic leaders may be of “poor” quality:
  - Substantial entry barriers to the candidate pool

    – political networks, campaign costs, and other socioeconomic inequities
  - Voters may have imperfect information on candidates’ characteristics
  - Voters may prefer to elect leaders who can provide patronage at the expense of other constituents

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- Some countries have sought to improve candidate quality by imposing “desirable” characteristics on candidates, such as minimum education levels and no criminal convictions
- Limited evidence on the effects of these “quality controls” on policy outcomes and citizens’ behavior

This paper

- We focus on a unique policy experiment in India
- Since 1992, several Indian states bar individuals (male or female) with more than 2 children from contesting village council (*Panchayat*) elections
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- Can restricting elected leadership positions to candidates with "desirable" attributes lead citizens to adopt those attributes?

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- Can restricting elected leadership positions to candidates with “desirable” attributes lead citizens to adopt those attributes?
  Yes, but...

- Starting in 1992, 11 states have enacted fertility limits for Panchayats and/ or municipal elections
- 4 states revoked them in 2005, but they remain in effect in 7 states
- One year grace-period: births during this year were exempt
- If ≥ 2 children when the law was announced:
  - Additional births after the grace-period $\implies$ disqualification
- If < 2 children when the law was announced:
  - Third birth after the grace-period $\implies$ disqualification

Disqualifications

Background: Panchayats

- India is a stable democracy
- Panchayats are the lowest unit of governance in India
- Granted constitutional status in 1992
- 3 tiers: village councils, block councils, and district councils
- Regular elections every 5 years
- No term limits on Panchayat members
- Minimum age to contest elections is 21 years
- Average population per village Panchayat $\approx$ 3,100
- Voter turnout in Panchayat elections generally $>$ 70%
- Gender and caste quotas

- India is world's second most populous country and houses one-third of its poorest
- This manipulation of the candidate pool aims to curb population growth, and is not intended to directly improve leaders' performance
- Seek to improve economic outcomes by precipitating fertility decline

Background: Fertility limits

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- This manipulation of the candidate pool aims to curb population growth, and is not intended to directly improve leaders' performance
- Seek to improve economic outcomes by precipitating fertility decline
- Stated mechanism: **role-model channel** and by conveying policymakers' seriousness about curbing population growth
- The limits, however, also **incentivize** individuals who intend to contest elections to plan smaller families
- May lead to **fear or anticipation** of stricter fertility limits in other dimensions, such as for government jobs

- The limits impact a large share of potential candidates of childbearing age
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Policy Relevance

- The limits impact a large share of potential candidates of childbearing age
  – In Rajasthan, 47% of Panchayat members in 2012 under 36 years of age and 41% in the 36-50 year age-group
- Official salaries of Panchayat members are not substantial
  – typical monthly salary of a village council head is about USD 50 - USD 60
- However, Panchayats have considerable power at the local level and members have discretion over a large share of local funds
  - Receive substantial funds from the national and state governments and are authorized to implement development schemes, e.g., NREGA
  - Can collect taxes, license fees, and fines, and receive seignorage from the auction of local mineral and forestry resources
  - Responsible for provision of public goods, such as roads and wells
- High potential private returns from political rents and corrupt practices

- Pool 3 repeated cross-sections of National Family Health Survey-1,2,3
- Years of survey: 1992-93, 1998-99, 2005-06
- Each round is representative at the state-level
- **Complete birth history** for each woman
    - e.g., month and year of child's birth, birth order, mother's age at birth
- Construct a large, retrospective, unbalanced woman-year panel
- Entry in the year of marriage and exit in the year of survey
- Sample period: 1973-2006
  – We cannot credibly examine the effect of revocations that took place in 2005
- 99,804 women and 256,267 births from 18 states

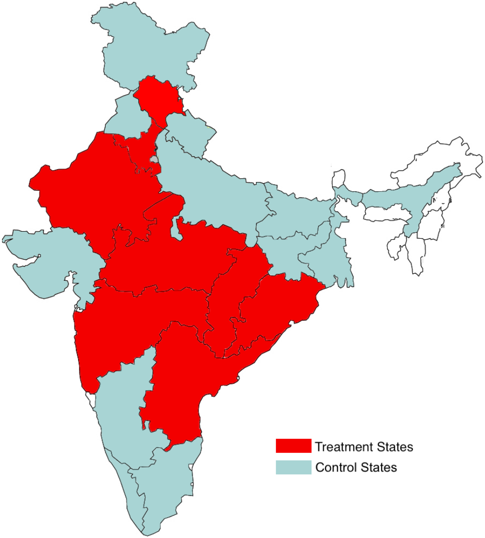
Timeline

State	Announced	Grace Period	End
Rajasthan	1992	Apr 23, 1994 - Nov 27, 1995	
Haryana	1994	Apr 21, 1994 - Apr 24, 1995	Jul 21, 2006
Andhra Pradesh	1994	May 30, 1994 - May 30, 1995	
Orissa	1993/1994*	Apr 1994 - Apr 21, 1995	
Himachal Pradesh	2000	Apr 18, 2000 - Apr 18, 2001	Apr 5, 2005
Madhya Pradesh	2000**	Mar 29, 2000 - Jan 26, 2001	Nov 20, 2005
Chhattisgarh	2000	2000 - Jan 2001	2005
Maharashtra	2003***	Sep 21, 2002 - Sep 20, 2003	
Uttarakhand (municipal)	2002		
Gujarat	2005	Aug 2005 - Aug 11, 2006	
Bihar (municipal)	Jan 2007	Feb 1, 2007 - Feb 1, 2008	

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

**Notified on May 31, 2000. This created problems since people whose third child was born in Jan 2001 contested their disqualification for birth within 8 months of the new law.
***In retrospective effect from Sep 21, 2002.

- Goal: to estimate the causal effect of the two-child limits on candidates in village council elections in a state on fertility-related outcomes among residents in the same state
- Utilize the quasi-experimental geographical and temporal variation in announcement of these laws across Indian states
- We can estimate the impact for only 7 (8) "treated" states
  - Rajasthan, Haryana, AP, Orissa, HP, MP (including Chhattisgarh), and Maharashtra
  - The limits came into effect in Bihar and Gujarat after 2006
- 9 control states
- Treatment year is based on the **year of announcement** of the law


Treatment States
Control States

- Evolution of the hazard of birth before and after the laws were announced
- For a woman i of age a in state s and year t:

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

- T_s indicates treatment states
- For treatment states, $Post_{s,t+k}$ indicates years during which the law is in place in state s
  - The year before the year of announcement is the omitted year
- For never-treated states, we use fictitious announcement years
  - Assign the same announcement year to a control state as its neighboring treatment state
  - If multiple bordering T states, we randomly choose the T year of one of the neighbors
  - Results robust to alternate assignments of placebo announcement years

- Outcomes are indicators for 1st/ 2nd/ 3rd/ 4th/ 5th birth
- For hazard of birth b, sample restricted to years after birth $(b-1)$, until b, and to women whose previous $(b-1)$ children were born before announcement of the law in their respective states
- X_i: woman's and her husband's years of schooling, indicators for religion, caste, standard of living, urban residence, year of interview
- If there is no noticeable pre-trend in the differential hazard of birth across treatment and control states, we can interpret the α_k coefficients during the post-announcement years as the causal effect of the limits
- $k \leq 5$ to equalize the number of post-treatment years across states

Net effect on birth hazards

We pool the event study coefficients in (1) and estimate:

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

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

- (2a) corresponds to (1)
- In (2b), we define treatment as zero for all control states:
  - $Treat_{st} = 1$ for women in treated states if $t \geq$ the year of announcement, and zero o.w.
- Use all available pre- and post-announcement years for each state
- Also control for years since last birth (marriage) flexibly
- (1)-(2b) capture the effects on *marginal fertility* of affected households

- Re-estimate (2a) and (2b) using indicators for whether a woman i of age a in state s and year t reports having 1/ 2/ 3/ 4/ 5 living children in year t as the outcome variables
- Unlike the hazard analysis, no restrictions in terms of the prior number of children
- Use all available years for each woman
- If the two-child limits are effective, we expect the likelihood of having 2 children to increase in the treatment states after the laws have been announced
- Capture the marginal effects on couples who had begun childbearing before the laws were announced + the behavioral response of new parents who began childbearing post-announcement

- The two-child laws may also affect the sex ratio of births
- We examine the effect on sex ratio of second and higher order births
- Despite the availability of prenatal sex-determination technology, sex of the first birth is random in India

  (Bhalotra and Cochrane (2010), Dasgupta and Bhat (1997), Visaria (2005))
- Parents more likely to practice sex-selection at higher parities if they do not have a son (Portner (2010), Rosenblum (2013), Anukriti et al. (2016))
- Restrict sample to women whose 1st child was born before the announcement

  –Sex ratio at 1st birth is "normal" in control and treatment states (both pre and post)

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

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

- Outcome variable: child is male
- $Girl_i$: mother *i*'s first child is a girl

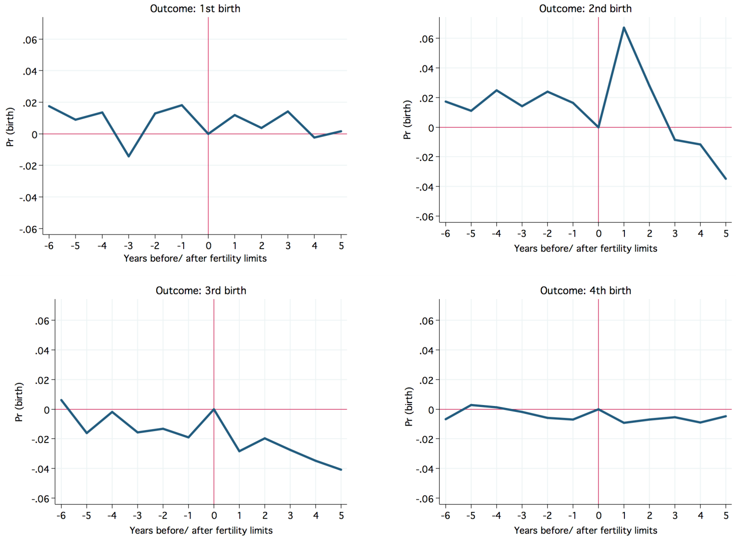
- The state-year variation in the timing of law announcement is uncorrelated with other time-varying determinants of the outcomes of interest.
- Test for correlations between law announcements and HH characteristics:

$$SES_{ist} = \alpha + \beta Treat_{st} + \gamma_s + \theta_t + \nu_s * t + \epsilon_{ist}$$

- To the best of our knowledge, during the time-period we examine, there were no other state-specific programs in the treatment states that promoted smaller families and whose timing coincided with the fertility limits

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

Differences in birth hazards for T and C states (α_k)



Net effect on the hazard of third birth

Declines by 10-11%

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

NOTES: This table reports the coefficients from specifications (2a) and (2b). Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. *** 1%, ** 5%, * 10%.

Heterogeneity in the net effect on the hazard of third birth

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

NOTES: This table reports the coefficients from specifications (2a) and (2b). Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. The baseline mean is calculated for observations where $Treat_{st} = 0$ in panel A and for observations where $Post_{st} = 0$ in panel B. *** 1%, ** 5%, * 10%.

Effects on the number of living children

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

NOTES: This table reports the coefficients from specifications (2a) and (2b). Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. *** 1%, ** 5%, * 10%. Pr(2 kids) ↑ by 3.41%.

Heterogeneous effects on the likelihood of > 2 living children

Pr(>2 kids) declines by 4.42% in treatment states

	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 reports the coefficients from specifications (2a) and (2b). Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. *** 1%, ** 5%, * 10%.

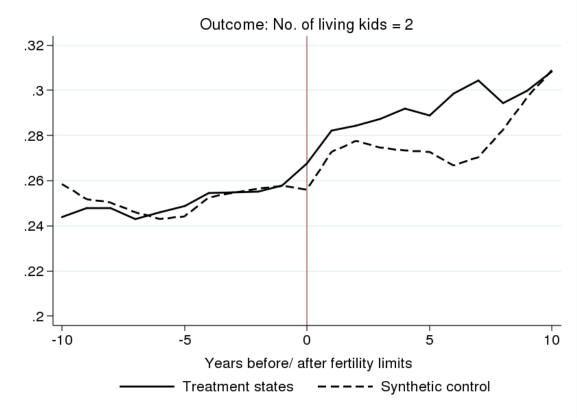
Sex ratio of second and higher parity births

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

NOTES: This table reports the coefficients from specifications (3a) and (3b). Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. *** 1%, ** 5%, * 10%. 10.32% ↑ for OBCs.

▸ Sex ratio of first births

- Upper castes are “less treatable” because of low fertility at baseline
- Consistent with lack of significant fertility effects for upper castes
- OBCs constitute significant fractions of the populations in our treatment states, such as Haryana (28.1%), Rajasthan (47.5%), Madhya Pradesh (41.2%), and Maharashtra (27.1%) that have highly adverse sex ratios



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- Abadie and Gardeazabal (2003) and Abadie et al. (2010)

Robustness 2: Alternate placebo years for control states

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

NOTES: Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. *** 1%, ** 5%, * 10%.

Robustness 3: Use all neighbors as control states

Match each T state with all its neighboring C states and create a new dataset in which C states that border multiple T states appear multiple times

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

NOTES: Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. *** 1%, ** 5%, * 10%. We weight each observation by the square root of the inverse of the number of times an observation appears in the sample.

- Average baseline terminal fertility in the treatment states is 2.8
  - 30% have $>$ 2 children
- We find that 615,390 (1.33% of) rural couples $\downarrow$ fertility due to the limits
- Hazard of 3rd birth estimates $\implies$ 2.65% of rural couples who had two children in T states $\downarrow$ marginal fertility due to the limits
- Comparison with other program impacts:
  - Matlab FP interventions $\downarrow$ fertility by 17-23% (Canning and Schultz (2012))
  - China's OCP $\downarrow$ fertility by 2% (Almond et al (2013))
  - Devi Rupak in India $\downarrow$ fertility by 1% (Anukriti (2014))
  - Our estimated effects fall in between

- Our estimated impacts are consistent with the high participation of voters and candidates in local politics, making both the aspirations and role-model channels plausible
    - 2014 World Values Survey for India: 53% say that politics is "very important" or "rather important" in their life (69% if "lower class")
- A role-model effect, however, is unlikely to be immediate
  – No "effect" on neighboring control states
- The incentive effect for individuals aspiring to run for office in the future is likely a strong explanation
    - Assuming that the effects are entirely driven by political aspirations, 28 to 43% of potential contestants per seat adjusted fertility due to the limits
- Cannot rule out fear or anticipation of stricter fertility limits in other dimensions, such as for government jobs

- Local leadership ambitions in India appear to be strong
- Individuals are willing to adjust fertility for a chance to hold political office in the future
- The limits, however, incentivize couples to deviate from their preferred fertility path and shrink the candidate pool
- Unintended sex ratio effects
- The overall effectiveness of the two-child laws thus depends on the magnitude of welfare gains from fertility decline relative to these costs

- Similar fertility limits have been proposed for members of state legislative assemblies and the national parliament in India
- Rajasthan and Haryana have enacted education requirements for Panchayat candidates
- Implications for the efficacy of democratic institutions in protecting the welfare of the socially disadvantaged, who may have higher fertility than elites due to lower access to contraception and higher risk of child mortality, and depend more on political representation to obtain resources prone to elite capture
- Potentially counteract the beneficial impacts of mandated caste and gender quotas
- Broadly, our results suggest otherwise

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

*Data available for 15 out of 23 districts

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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]
	34,018	5,818	5,783	7,511	14,906
Baseline mean	0.5152	0.5062	0.5103	0.5162	0.5198
Panel B:					
$Treat_{st}$	-0.0007	0.0325	-0.0063	-0.0304	0.0093
	[0.0076]	[0.0295]	[0.0339]	[0.0228]	[0.0162]
	86,023	15,245	9,265	19,345	42,168
Baseline mean	0.5150	0.5128	0.5096	0.5173	0.5158
Panel C:					
$T_s * Post_{st}$	-0.0042	0.0179	-0.0367	-0.0263	0.0092
	[0.0122]	[0.0379]	[0.0486]	[0.0219]	[0.0199]
$Post_{st}$	0.0056	0.0226	0.0435	-0.0053	0.0003
	[0.0126]	[0.0275]	[0.0491]	[0.0175]	[0.0185]
N	86,023	15,245	9,265	19,345	42,168
Baseline mean	0.5144	0.5224	0.5056	0.5151	0.5128

NOTES: This table reports the coefficients from specifications (3a) and (3b). Standard errors in brackets are clustered by state and in parentheses are wild-cluster bootstrapped by state. *** 1%, ** 5%, * 10%.

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