Political Inclusivity and the Aspirations of Young Constituents: Identifying the Effects of a National Empowerment Policy

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Abstract: Using exogenous geographic variation in exposure to 1992 reforms that introduced seat quotas for women in local government in India, I find a sizable increase in the enrollment rate of school-age children resulting from additional exposure to women leaders. This effect was commensurate with reductions either in idle time or employment in household-based enterprises. For young women, effects are particularly concentrated among poorer households and those with less-educated proximate role models. A number of potential channels are ruled out, including school infrastructure, direct labor market effects, marriage market matching and other household-bargaining-type dynamics. Supplementary analysis highlights ways in which women leaders are able to affect the educational preferences of, or for, young women by serving as role models.

Keywords: gender gap, education, human capital, political reservations, empowerment, development.

JEL Classification: D13, H11, I21, I22, I24, I25, J16, O10, O12.

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Many countries enact policies to proactively rectify historical inequities. Mandated seat or candidate quotas in elected bodies are a common incarnation of such efforts, having been legislated in over 90 countries as of 2015.[1] Given the popularity of such policies, it is important to thoroughly understand the many dimensions of effects that a changed leadership composition may have on the well-being of constituents. That is, does policy aimed at enhancing individuals' equality of opportunity in the sociopolitical sphere can lead to changes in the economic realities for members of newly-empowered groups? In this paper I examine the introduction and implementation of a national policy in India reserving elected seats in local government for women, and the relationship between exposure to women leaders and educational enrollment.

Credibly estimating policy effects for such a situation poses a number of challenges. Beyond likely endogeneity in policy adoption, educational decisions in developing countries are made amidst a complex web of factors that may be slow to change, including long-held traditions and cultural beliefs. This means that the effect of exposure to leaders on constituents' human capital investment may be difficult to detect in the short term depending on the mechanism by which leaders affect the experience of constituents.

To estimate long-term effects on enrollment I exploit a feature of the Indian experience in seat quota implementation that provides exogenous variation in exposure to the quota regime across areas over a span of 15 years. I find a substantial increase in enrollment rates of both young women and young men. There are, however, distinct patterns in the activities in which enrollees moved by the policy would have otherwise been engaged. Those from higher social groups saw increased enrollment among individuals who would have otherwise engaged in household production or been idle; among lower social groups, shifts towards enrollment come only from those who would have been idle or engaged in non-wage household duties, despite a larger share of children engaged in household-based employment. Reminiscent of Basu and Van (1998), this provides empirical evidence that household labor is a persistent barrier to educational enrollment among more disadvantaged groups.

An investigation into parameter heterogeneity begins to highlight the potential role of leaders as role models: policy effects are concentrated among those segments of the population

[1] Author's calculations based on data from The Quota Project, available at www.quotaproject.org. Accessed September 2014.

who likely had low expected returns to education (those from minority groups, those among lower-income families, and young women in families with uneducated adult women). Corroborating this role model hypothesis is evidence that exposure to a small number of high-powered women at higher levels of government can have effects on educational enrollment. I then rule out several potential channels by which the relationship between reservations and education may be effected. Using detailed administrative data on school infrastructure, I show that there is no evidence of increases in either the extensive on intensive margins of schooling facilities related to higher levels of exposure to seat quotas. I also find no effects in the labor market or among other typical indicators of women's empowerment. Finally, a set of supplementary analyses show how the introduction of women leaders to government may communicate a changing social environment to which individuals' respond in setting their expectations and aspirations for social and economic outcomes.

I. India's 73rd Constitutional Amendment Act

Quotas for women and other historically marginalized groups in elected bodies were first implemented at the national level in India with the 73rd and 74th Amendments to the Indian Constitution. These pieces of legislation, passed in 1992, gave national support to the formalization and implementation of an historical, decentralized governance structure known as the panchayat (or, more formally, Panchayati Raj Institutions).[2] The 73rd Constitutional Amendment Act instituted a three-tiered system of local government at the village, sub-district (block), and district levels across rural areas of the country, while the 74th Constitutional Amendment Act instituted a revised local governance structure in municipalities.[3] The

[2] Prior to 1992, panchayats operated at the village level and consisted of a small number of individuals chosen by a village to oversee various local affairs. Panchayats were not standardized in their structures, organization, operations, or responsibilities, nor were they necessarily elected bodies. By the mid-20th century, the panchayat system was widely recognized to embody "concealed forms of social prejudice, oppression and exploitation that were firmly rooted in local power structures" (GOI 2008). In the latter half of the 20th century there was support for the revival of a reformed panchayat system and by 1989 there was strong support at the national level to give constitutional status to a broad panchayat system, leading to the 73rd and 74th Constitutional Amendment Acts being enacted in 1993.

[3] The governance structures provided for in the 73rd Amendment have come to be known as the "Panchayati Raj". This terminology specifically refers to rural governance, as urban bodies have a different name, function differently, and the implementation timing of the 74th Amendment was different from the 73rd Amendment. Responsibilities of the Panchayat include administration of state transfer programs, planning and implementation of schemes for economic development, establishment and administration of local public goods such as educational and medical

Amendments were intended to provide large-scale devolution and decentralization of powers to the local bodies, stipulated that members of the local governance bodies were to be elected at five-year intervals, and provided for one-third of all seats at each governance level to be filled by women and a proportionate seat share reserved for representatives from historically marginalized social groups (Scheduled Castes/Scheduled Tribes, or SC/ST). These quotas also overlapped, providing for one-third of the SC/ST seats to be held by SC/ST women.

The 73rd Amendment stipulated that states had the responsibility to adjust or amend local elections to comply with the provisions of the Amendments, and all states amended existing laws or passed new laws to be compliant within one year. Compliant elections were eventually held in nearly all states, and there is considerable variation in the timing of these first elections across states. This timing is plausibly exogenous due to state authorities waiting for the term of existing governing bodies to expire. That is, most state governments waited for the term of office of incumbent local officials to expire before conducting fresh elections in compliance with the provisions of the reform. Mathew (1995) provides a history of local elections by state, confirming (where available) that most states held their elections in the year predicted by pre-policy elections and terms of office.[4] Although comprehensive data do not exist for all states, available evidence also confirms there was only minimal involvement of women in local elected bodies prior to this reform (Mathew 2000).

Figure 1 depicts the variation in state-level differences in cumulative exposure to the policy as of 2007, where darkly-shaded states correspond to those states with longer (earlier) cumulative exposure to (implementation of) reservations. Madhya Pradesh, in the center of the

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facilities, oversight of local infrastructure (water, sewage, roads, etc.) and the monitoring of civil servants (Duflo 2005).

[4] In other cases, implementation timing varied less exogenously. Several states chose to incorporate provisions regarding political reservations for women prior to when the constitutional amendment was to come into effect. Andhra Pradesh provided for 22 to 25 per cent reservations for women in the Andhra Pradesh Gram Panchayats Act, 1964 (GOI 2008), while Karnataka introduced a similar level of reservation for women in 1985. Both Kerala and West Bengal restructured their institutions of local government in anticipation of the passing of the 73rd Act (in 1991 and 1992, respectively) although elections implementing these reservations were not held until after enforcement in 1993. Bihar was prevented from implementation due to legal issues regarding certain provisions of the Amendments (Iyer et al. 2012). States comprised of primarily tribal populations (Meghalaya, Mizoram and Nagaland) were explicitly excluded from the purview of the Amendments (GOI 2008). Jammu - Kashmir introduced reservations at a level consistent with the Amendments via state-level legislation in 1997, although the election of panchayats implementing the reservations provided for under its own Act has not yet taken place (GOI 2008). Jharkhand has similarly never held reserved elections. Iyer et al. (2012) and Duflo (2010) provide further details on the implementation, structure and other aspects of the Panchayati Raj.

country, is reflected as having longer exposure due to its early implementation whereas neighboring Chhattisgarh was one of the last states to implement the provisions of the 73rd Amendment in 2005. Once the provisions of the reform were implemented, one-third of seats were reserved for women at any level of the governance hierarchy; for single-seat leadership positions, reservations were assigned randomly across areas (whether at the village or district level) and election cycles. This feature of rotating leadership assignment has been used to assess the effects of women leaders in a number of previous studies (Beaman et al. 2012 and Iyer et al. 2012, among others).

It is well-established that decisions of a governance body can be influenced by changing their demographic composition. Using state-level variation in India over four decades, Pande (2003) identifies how the mandated reservations of legislative positions for minority groups increased the redistribution of resources towards these groups, demonstrating enhanced policy influence. Besley et al. (2004) found that reservation of a leadership position for SC/ST individuals increased access among SC/ST households to infrastructure and government services. Chattopadhyay and Duflo (2004a) use information on the location of public goods to show that when an area has leadership positions reserved for SC/ST individuals, the share of public goods going to that group is significantly higher, while Chattopadhyay and Duflo (2004b) use village-level variation in political reservations for women to predict the type of public goods provided in 265 reserved and unreserved villages in West Bengal and Rajasthan, finding that leaders invest more in infrastructure that is directly relevant to the needs of their own gender. Overall, the group identity of leaders has shown to matter in the type of public goods provided under the purview of the governing body, and this has been established in various contexts not limited to the Indian case (e.g., Powley 2007, Washington 2008). Duflo (2005) provides an assessment of the case for political reservations for women and other historically-underrepresented groups, and, using evidence from India, concludes that reservations incur a significant reallocation of public goods toward the preferred allocation of the previously politically-underrepresented groups. Pande and Ford (2011) provide a recent comprehensive review of the literature on gender quotas.

Additionally, the character and actions of governance bodies may influence those groups with which they interact. Topalova and Duflo (2004) find that women leaders in India are less

likely to take bribes than their male counterparts, while Iyer et al. (2012) find evidence that political empowerment resulted in greater reporting of crimes against women. Leaders from newly-empowered groups may also change the perceptions of their group, potentially affecting an array of social and economic outcomes. Beaman et al. (2009) show how perceptions of women improve once men are exposed to women in leadership roles, providing substantial evidence in support of the model of attitudes and bias implicit in Hoff and Stiglitz (2010).[5] Ghani et al. (2014) quantify the link between the timing of reservations and changes in women engaged in India's manufacturing sector, find strong evidence of an increase in small-scale female entrepreneurship.

The current work is also related to studies of the salience of returns to education. Jensen (2010a) shows how the provision of information on returns to education in the Dominican Republic affects enrollment, while Jensen (2010b) finds that knowledge (salience) of educational returns increases educational investment among girls. Linking these literatures, Nguyen (2008) finds that role model identity matters in the effect of information on updating perceived returns to education: role models of similar backgrounds to students have a larger impact on outcomes than role models of dissimilar backgrounds.

Earlier work on the specific relationship between political empowerment and education uses cross-sectional variation to assess longer-term impacts of politicians on educational outcomes. Clots-Figueras (2012) uses a regression discontinuity design based on closely-won elections between female and male candidates to show that the gender of politicians affects the educational levels of individuals who grow up in the districts where the politicians are elected. The author finds that the election of women politicians did increase primary school completion and that this effect was primarily found in urban and not rural areas, but that the effect was not statistically different for men versus women. Among a sample of households in 495 villages in West Bengal, Beaman et al (2012) examine the effects of randomly-rotated exposure to women in village-level leadership positions to identify effects on aspirations and educational outcomes for girls, with differences in exposure across districts of up to 15 years (three election cycles).[6]

[5] Hoff and Stiglitz (2010) develop a conceptual framework to show how changes in power, technology and contacts with the outside world matter, especially because they can lead to changes in ideology.
[6] There are more than 40,000 villages in West Bengal, which itself is one of the 35 states and union territories of India.

Compared to villages that never had chairperson seats reserved for women, the gender gap in aspirations closed by 25% in parents and 32% in adolescents in villages assigned to a female leader for two election cycles (approximately ten years). The gender gap in adolescent educational attainment is erased in these villages, and girls spend less time on household chores. The authors find no evidence of changes in young women's labor market opportunities, using novel survey questions to attribute the impact of women leaders to a "role model" effect. Albeit either in a different time period or in a broader context, in this paper I follow the conceptual approach of these studies by exploiting exogenous cross-sectional geographic variation in exposure to leaders to identify longer-term, cumulative effects of seat quotas on educational enrollment.

II. Data: Reservations Implementation and School Enrollment

Data on the timing of exposure to seat quotas comes from several publications documenting the implementation and progress of the 73rd Constitutional Amendment (Mathew 1995, 2000; GOI 2008, Iyer et al. 2012). India's National Sample Survey Organisation "Socio-Economic Survey—Schedule 10: Employment and Unemployment" provides data on enrollment.[7] The NSS data come from a survey of a representative sample of households across all Indian states and union territories approximately every five years, with the household sampling frame drawn from the most recent population census and stratified within the rural and urban areas in each district. The primary analyses use data from survey rounds in 1987-88 (prior to the policy change) and 2007-08.[8] [9]

India's Employment/Unemployment schedule is akin to many household labor force surveys administered worldwide. Respondent households provide individual-level details on demographics, employment, income and consumption particulars for all household members. The analysis exploits variation in the "usual principal activity" field, which among other activities indicates whether the individual was currently "attending [an] educational institution". Beyond restriction to the estimation sample of school-age children in rural areas of the country,

[7] Hereafter referred to as "NSS data".
[8] Hereafter referred to only by the initial year for simplicity.
[9] After 2006-7 several states chose to increase seat quotas for women to a minimum of 50 percent. I thus restrict my analysis to the 2007-08 wave due to incomparability of the policy measure and its exogeneity across states.

minimal data cleaning procedures were required. All geographic definitions have been made forwards- and backwards-consistent over time to account for changes in administrative boundaries and the bifurcation of three states in 2001.

Table 1 shows trends in enrollment rates for various gender, age and social groups over the period of study based on the NSS data. Since 1987 there has been close to 100 percent enrollment in lower-primary school cohorts, motivating the restriction to individuals aged nine to 17 for whom enrollment may have been affected by the policy change. Among the non-SC/ST population there has been substantial progress in enrollment rates in all age cohorts between 1987 and 2007, with the 9-11 and 12-14 age brackets for young men improving from 91 and 78 percent to 98 and 93 percent enrollment, respectively. In all age groups, women lag in initial and ending levels, although increases in enrollment rates were greater. In panel C, I calculate the educational enrollment gap. In the youngest school-age cohort there is almost no gender gap in any of the years, while among older age cohorts the gender gap has decreased substantially from 14 percentage points in 1987 to almost zero by 2007 in the 9-11 age group, and from 28 percent to 10 percent in the 12-14 age group over the same period. The SC/ST group, having started at lower enrollment levels and with a wider gender gap, saw faster increases in enrollment rates and reduction in the gender gap, although by 2007 still not reaching full parity with same-age peers.[10]

III. Identifying Effects Using Border Discontinuities

The following section briefly motivates and derives an estimating equation that takes advantage of discontinuities in the level of exposure across neighboring districts lying across state borders. Consider a set of administrative districts (indexed by *j*) within larger geographic areas (indexed by *k*, not necessarily states). An equation for enrollment rates across districts can be expressed as the following linear function of some policy exposure:

$$Enrollment\ rate_{j,k} = \delta_0 + \delta_1 * Policy\ exposure_{j,k} + \mu_k + \varepsilon_{j,k} \quad (1)$$

[10] Azam and Kingdon (2012) characterize for a recent period the extent and patterns of sex bias in education in India. Notably, they find that pro-male gender bias exists in the primary school age group and increases with age. The authors also find that the extent of pro-male gender bias in educational expenditure is substantially greater in rural than in urban areas.

$Policy\ exposure_{j,k}$ captures the cumulative years of exposure to the policy, which varies across districts j. The area-specific error term μ_k contains unobservables related to factors which are common in the area, while the district-specific disturbance term $\varepsilon_{j,k}$ captures factors determining enrollments which may be distinct across municipalities within the area but is exogenous to the policy exposure.

The identifying assumption is that $\varepsilon_{j,k}$ and $Policy\ exposure_{j,k}$ are uncorrelated; it may still be the case that area-level factors such as tradition, culture, or social norms influence unobservables related to the policy across areas, potentially even via *de facto* rather than *de jure* empowerment. If the variation in μ_k is comprised of unobserved determinants that do not particularly correspond to administrative boundaries, another method of purging μ_k becomes necessary. One solution is to find areas with common unobservables but with differing levels of policy exposure, such that a vector of fixed effects corresponding to μ_k would purge area-specific unobservables.[11] In the current context, adjacent districts lying on either side of a state border fulfill such criteria. The empirical strategy will thus exploit cross-state differences in exposure to the policy to identify causal effects using cross-sectional variation across border districts, having purged the equation of area-specific unobservables.

In order to use state-level implementation as exogenous identifying variation, I first look for evidence of whether the implementation timing across states was related to pre-period enrollment rate levels or trends. Figure 2 shows separately the initial state-level enrollment rates for the four sample groups by gender and social groups in 1987 plotted against the order in which states implemented the reservations, supporting that claim that the reform implementation was exogenous to pre-period enrollment rate levels. The line in each panel is virtually flat, indicating no detectable relationship between pre-implementation enrollment rates and the subsequent timing of implementation across states. Table 2 provides empirical support for parallel time trends in enrollment rates in the ten years preceding the policy implementation by directly estimating the enrollment status of individuals using data from the earliest available survey year (1983-84) through the year of the reform passage (1993-94) with a linear trend term and an interaction of this trend with the state-level year of implementation. State fixed effects

[11] Similar identification strategies have been implemented in many contexts, recently by Michalopoulos and Papaioannou (2014), Ghani et al. (2014), Jofre-Monseny (2012), Duranton et al. (2011), Naidu (2011), Dube et al. (2010), and earlier by Holmes (1998).

absorb the main effect of differential adoption years by state, and state-clustered standard errors correct for serial correlation within states. A negative coefficient on the interaction term would indicate if states that implemented the policy earlier also had more quickly-rising enrollment rates prior to the policy change. I find no evidence of this for any of the subgroups that are the focus of the analysis.

IV. Estimates

When a focal district has multiple cross-border neighbors, I assign to the pair the neighboring district which is most similar to the focal district on a set of observables. The conceptual advantage of this process is that a given district can appear only once in the estimation sample as part of a unique pair, and each unique pair is comprised of the two most similar districts among possible combinations. Disadvantages to this approach are that, as with any matching procedure, the choice of matching variables is *ad hoc* and some border districts may never enter the sample, as shown in Appendix Figure 1. In practice, whether districts are assigned to pairs based on observables, or are randomly chosen, or districts with multiple neighbors are left to appear multiple times as members of different pairs does not make a substantive difference in the pattern or magnitudes of the estimated policy effects.

In terms of generalizability, there is strong evidence that border districts are similar to interior districts in terms of demographics and enrollment rates (see Appendix Table 1), both prior to the policy change in 1987 and as of the year of analysis in 2007. Table 3 provides summary statistics of the matched sample.[12] The identifying assumption when estimating equation (1) is given by:

$$E\left[\varepsilon_{j,k} | Policy\ exposure_{j,k}\right] = 0. \quad (2)$$

Violations of this assumption are likely to come in the form of either persistent unobservables in $\varepsilon_{j,k}$ that are not purged by area fixed effects μ_k, or concurrent phenomena

[12] There are different observation counts across measures due to some cells containing zero values in the denominator; in general, these district pairs would be jointly very small in size and thus less important in weighted regressions. In the estimations, district pairs missing values in any regressor are implicitly dropped.

affecting enrollment confounded with $Policy\ exposure_{j,k}$. Three steps are taken to address these concerns, leading to the specification variants shown in the tables. First, I estimate equation (1) directly. I then include the pre-policy 1987 district enrollment rate for the focal sample as a pre-policy measure of any persistent unobservables across districts not removed by μ_k. Finally, I directly control for important concurrent policies in terms of (a) a large-scale national education program that was implemented across districts at different times, and (b) the state's cumulative exposure to progressive-party rule. An additional specification estimates men's and women's enrollment jointly to allow for efficiency gains by accounting for cross-equation error correlation (Breusch–Pagan $p<0.01$ in all instances). None of the estimates are particularly sensitive to these controls, so for parsimony I refer to those estimates that only control for the pre-period enrollment rate as the preferred specifications. I estimate district enrollment rates as of 2007, with the main regressor being the cumulative exposure to the 73rd Amendment provisions as of 2007.

Table 4 contains the results of the estimation of equation (1) for non-minority group women (Panel A) and men (Panel B). Coefficient magnitudes are interpreted as the fractional increase in the enrollment rate per additional year of policy exposure. Based on the preferred specification (column 2) including only the pre-policy control, an additional year of exposure to the policy increases the enrollment rate of non-SC/ST women by .7 percentage points; given that pre-policy enrollment rates for this sample was less than 50 percent (Table 3) and mean exposure to the policy was 10.3 years, the policy likely played a substantial role in increasing educational enrollment of young women. Panel B shows a commensurate effect of the policy among young men in the same social group, suggesting the policy may have had effects on enrollment beyond just those on women.

Because quotas for ethnic minorities were set to be proportionate to the local population share rather than fixed, it is possible to distinguish the effects of the different spheres of reservation by interacting the SC/ST population share (divided by .33 to give interpretations comparable to the main policy variable) with the cumulative years of exposure to the policy. In this specification, the main policy regressor is interpreted as the pure effect of the quota for women, while the coefficient on the interaction term gives the effect of SC/ST quotas. Table 5 contains these estimates for the SC/ST sample, showing large effects (although less precisely

estimated) effects among young women (Panel A) as well as men (Panel B). These effects are moderated somewhat, however, by partially offsetting effects from the SC/ST reservation (not reported).

The enrollment status information used in the analysis above is only one of a set of activities respondents may claim as their principal use of time; I am able to investigate which other activities experienced a reduction among school-age children who are enrolled in school subsequent to the policy. After making alternative activities consistent across surveys, there are four possible other uses of time that respondents may claim as their principal activity. Three are traditional labor force activities (working in a household enterprise, wage work in a non-household enterprise, or "looking for work"), while the final category comprises non-wage, non-labor force domestic duties. Tables 6 and 7 present estimations of the preferred specification in the border strategy (including only the pre-policy control) across the different activities and samples. Containing estimates for non-SC/ST women and men, Table 6 shows that those of higher social groups saw increased enrollment among individuals who would have otherwise engaged in household production or been idle; Table 7 shows that this pattern is not the same among lower social groups, as shifts towards enrollment come only from those who would have been idle or engaged in non-wage household duties despite a larger share of children engaged in household-based employment. This suggests that while the reservations policy appeared to have a uniformly positive effect across all subsamples, this force may not have been strong enough to overcome the immediate opportunity cost of household production for those in lower castes. This analysis thus suggests that the availability of household labor remains is a persistent barrier to educational enrollment among more disadvantaged groups. While further investigation of these differences is left for future work, patterns across samples in shifts in time use linked to policy exposure suggest attention be paid to shifts in time-use alternatives to schooling across distinct subsamples of the population.

The main estimates are robust to several alternative approaches. First, coefficient estimates and patterns across samples are unchanged when weighting districts by the log of population, Winsorizing the dependent variable at either one or five percent, including area demographic controls directly, or adopting a differenced specification. Instrumenting for the policy exposure differential based on an "expected" differential calculated from the expected

implementation timing based on election cycles prior to the passage of the 73rd Amendment. Mathew (2000) gives state level information on local governance elections prior to the 1993 reforms used to construct this expected differential.[13] While the magnitude of the effect is somewhat moderated, the direction and pattern of effects is still consistent with the main estimates.[14] Event study estimates also show comparable patterns to the main estimates, and are available as Appendix Figure 3. Monte Carlo simulations based on placebo variation in state implementation timing indicate the magnitude and significance of the effects are unlikely to arise by chance, and additional robustness checks ensure that the estimates are not sensitive to the inclusion of any particular state. To address the concern mentioned above regarding the necessary loss of some border districts in constructing the matched sample, Appendix Figure 2 shows the empirical distribution of coefficients across 500 district pair sets with multiple-neighbor pairings chosen randomly. This approach, in aggregate, loses no information contained in border districts and confirms that the effect magnitudes and patterns across subsamples in the primary estimates are not particularly sensitive to these methodological decisions.[15]

A remaining criticism is that the policy measure based on state implementation timing necessarily conflates exposure to decentralized local governance with exposure to seat reservations for women. While full details of this approach and analysis are available in the Web Appendix, Appendix Table 2 uses within-state variation in exposure to district chairperson seat reservations to estimate the effects of longer exposure to women leaders in the highest seat in local government on enrollment rates. This policy measure does not suffer from any conflation with exposure to decentralization, and across the sample of states for which it is available, I find highly comparable effects with estimates given by the border strategy—providing evidence that women leaders do effect enrollment, and that the estimates given by the border strategy reflects

[13] While not directly available for every state, I am able to construct the expected implementation year for a majority of states based on election cycle lengths and pre-1993 election years; the majority of these expected implementations are the same or very close to the actual effective implementation years. For states for which it is not possible to construct this information I use the actual effective implementation date. This latter necessary adjustment suggests an artificially strong first stage, and possible mechanical violation of the exclusion restriction. Thus caution should be taken in the interpretation of the IV estimates, which are provided merely as a robustness check rather than as a preferred strategy.

[14] Effects of the state-level implementation in a well-identified differences-in-differences framework estimating short-run policy effects, an approach used by several studies in the literature, unsurprisingly find no discernible effects of either level of policy variation on enrollment rates.

[15] Many of these robustness checks are available in the original working paper; all are available from the author upon request.

these effects. Additionally, this analysis explicitly shows that part of the effect of women leaders is found among higher-level government positions, and is not necessarily limited to very local leaders who are in closer daily or physical proximity to their constituents.

V. Heterogeneous effects

I next use the rich cross-sectional nature of the survey data to investigate additional parameter heterogeneity. I estimate the preferred specification for samples differentiating individuals by age group, by their household's relative income level and by a proxy for initial expectations for educational attainment via the highest education level among adult women in the household. Table 8 presents these estimates, showing that for both groups of women, the quotas appeared to affect enrollment at the well-known age range of 12 and above for girls dropping out of school. There are no clear patterns in differential effects across relative income levels, although there is some tendency for a stronger effect among children from poorer families. Most importantly, however, is the pattern across young women in the effects when the sample is split by the highest education of adult female household members. Across both groups, policy effects are concentrated among young women in families that had uneducated (illiterate) older women in the household—those young women that had less-educated initial proximate female role models. This suggests a potential causal channel by which leaders may affect educational investment as one which particularly operates among those who had lower initial expected educational attainment and returns to education.

VI. Causal channels: school infrastructure and contemporaneous returns to education

Various mechanisms potentially contribute to the circuitous link between political and economic empowerment, including infrastructure and public goods provision (Chattopadhyay and Duflo (2004a, b), labor market effects, women's bargaining power (Schultz 2001) or aspirations (Beaman et al 2012). In the following section, I investigate and discuss these hypotheses as

potential channels by which the effects of a changed local leadership composition on educational enrollment may arise.

School provision

Chattopadhyay and Duflo (2004a) find that the gender of local leaders affects the public goods provided in a community, and that women leaders direct public spending to infrastructure that is more relevant to their own gender. Earlier work has shown that male and female leaders have different policy preferences, a conceptual extension would suggest differential provision of schooling infrastructure if women leaders indeed prefer educational infrastructure more so than male leaders. To investigate whether educational infrastructure provision was caused by longer exposure to the 73rd Amendment reforms, I use data from India's "District Report Cards" provided by the District Information System for Education (DISE). Since 2000-01, this agency has annually compiled district-level data on over 400 education-related indicators. For the purpose of parsimony, I constructed a small set of variables from the DISE data that allow me to test hypotheses regarding the extensive and intensive margins of schooling infrastructure, including schools per thousand persons, new schools established per thousand persons since 1995, teachers per pupil and the share of classrooms in good condition. I also capture a set of measures related to the experience of girls in schools, particularly the share of schools that have a separate girls' lavatory and the share of teachers who are female. Using the 2007-08 DISE data corresponding to the enrollment figures analyzed above, Table 9 shows the results of estimating equation (1) using these schooling indicators as the outcome. Each column in Table 9 shows the coefficient on the difference in exposure using the dependent variable indicated. For the extensive margin, the preferred measure (rural schools per thousand rural population) does not appear to be correlated to a longer exposure gap.[16] While there is some evidence that teacher-to-pupil ratios were commensurate with the enrollment increases induced by the policy (by nature of a non-negative estimated effect), this is estimated imprecisely and the hiring of teachers is likely to follow increased enrollment, rather than precede it. Finally, among gender-related factors, there is no relationship between female-friendly facilities or higher female share of

[16] Results of this analysis are not sensitive to inclusion or exclusion of the 1987 enrollment difference as a control.

teachers and longer policy exposure. Overall, minimal evidence, if any, of an increase in educational infrastructure associated with longer exposure to the reform.[17]

Labor market effects and alternative returns to education

Labor market dynamics making women's work more valuable may affect educational decisions of young women in two ways: via increased incomes of adult women changing relative intra-household bargaining power, and by increasing the returns to education for women. A long literature has looked at gender-differentiated effects of household income as well as the role of female earnings and bargaining power on educational attainment.[18] Table 10 investigates a range of labor market, marriage and household bargaining power-type indicators in the same border framework to determine whether these factors appear related to longer exposure to the quota policy. These include measures of women's labor force participation, probability of marriage, and, conditional on being married, number of children, age at first birth, husband's education, the product of husband's and wife's education, and husband's wage. This analysis purposively uses the a sample of rural women from an older cohort (25-35 years old) whose educational decisions were complete or near-complete by the time of the 73rd Amendment and its implementation, allowing me to investigate effects absent changes in educational attainment brought about by the policy. Congruent with earlier studies, there is little evidence of changed labor market, marriage or household dynamics associated with longer exposure to the quota policy.[19,20]

[17] These results appear to contrast with those of Clots-Figueras (2012). In particular, Clots-Figueras (2012) found that women leaders increased educational enrollment in urban but not rural areas, and among both boys and girls, and this effect occurred through greater provision of schooling infrastructure. In my paper, I find effects for girls only, and rule out schooling provision as a mechanism for this to occur. It may not be reasonable, however, to compare the two studies directly: Clots-Figueras (2012) studies a substantially earlier time period in India when enrollment rates were substantially lower and the electoral context was absent any seat quota system. For this reason, incentives regarding the provision of public goods, as well as constituents' perception of women leaders, are likely to be different in the two contexts.

[18] Among others, Edmonds and Pavcnik (2002) use a panel dataset of Vietnamese households to show that exogenous changes in household income though rice prices cause a reduction in child labor, and these reductions are largest for girls of secondary-school age and correspond to a commensurate increase in school attendance for the group; for female bargaining power, de Carvalho Filho (2008) finds a pronounced effect of social security benefits on female child labor when benefits go to female household members.

[19] Two recent studies provide some evidence that seat quotas in India affected labor market opportunities for adult women. Ghani et al. (2014) find short-run effects of political reservations caused an increase in women-owned establishments created in the unorganized manufacturing sector, particularly in traditional and household-based industries, relative to new male-owned establishments. In the public sector, longer cumulative exposure to women leaders increased the share of government-provided employment given to women at the introduction of a national rural employment guarantee scheme over the period of 2005-2008 (Ghani et al. 2013). The share of women

VII. Aspirations

With minimal evidence pointing to explicit cost or return factors as the channel by which seat quotas affected enrollment, this section details additional descriptive evidence that suggests the introduction of women leaders to government may communicate a changing social environment to which individuals' respond in setting their aspirations for social and economic outcomes. To this end, I find:

- the existence of at least one channel for women leaders to be salient, with their public status allowing them to enter into the "cognitive window" of constituents,
- that the selection of female candidates in unreserved parliamentary elections suggests female candidates need to have educational attainment equivalent to be competitive with male candidates, and,
- that areas with more exposure to women in leadership positions see more women candidates contest, but not win, unreserved parliamentary elections.

Taken together, these facts suggest: (a) women leaders are salient to constituents though news media, (b) they provide information on the pedigree required to compete with men in the social sphere, and (c) that the reservations policy is directly related to increased aspirations of women (politicians), as expressed through the contesting of unreserved parliamentary elections.

News media as a perpetual, broad-based salience channel

From an optimistic standpoint, the news media can serve as an objective fact-reporting mechanism reflecting an increasing presence of women in public life with a potentially lower propensity to express existing social norms than other media. To test whether the media plays a

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participating in either of these specific labor force activities is relatively small, however, and unlikely to appear as large changes when considering the labor force participation or activities of all women.

[20] Beyond bargaining power, expanded labor force activities available to women may change incentives for parents to invest in daughters. A substantial literature investigates the role of demand-side factors changing the underlying valuation of education for girls in LDCs. Munshi and Rosenzweig (2006) find that the expansion of the financial sector and other white collar industries caused enrollment in English language schools to increase for girls (but not boys). Similarly, Oster and Millet (2010) show that the introduction of a call center in towns in southern India generated large increases in school enrolment for both boys and girls. Shastry (2010) finds that the information technology sector grew more rapidly in areas of India where English is more widely spoken, and that in turn those areas experienced increased school enrolment. Kochar (2004) showed that urban rates of return influence rural schooling, particularly amongst households who are most likely to seek urban employment.

role in the salience of (women) leaders, I analyze the web archive of news media articles available on the Times of India from 2001 to 2007.[21] Using this single, leading national English-language daily will likely indicate coverage patterns across similar media given high correspondence of news reporting across outlets, and may be less likely to report on local rural governance than smaller news outlets—thus providing a lower bound to the responsiveness of news coverage of local politicians.

The archive in total contains 560,552 valid links to articles. Once downloaded, articles are inspected for geographical references in the initial lines of text based on a list of 1,200 of India's largest cities. Of these, 378,439 articles reference a specific geographic location. The articles are then assigned a reference state based on city information, and information regarding the timing of elections is matched into the data. I remove from the sample articles identifiable as coming from sports, business, fashion/style, or international sections of the paper, leaving over 345,000 articles. Dropping articles about areas not part of the panchayat system (largely Delhi), 244,462 articles remain.

I inspect the text of articles for strings indicating words referring to three concepts: panchayats or leaders ("panchayat", "sarpanch", "mukhiya", or "pradhan"), school or education ("educat-", "school", "class", "teach-", "college", or "university") and women or girls ("women", "woman", "female", or "girl"). While rudimentary, this approach nonetheless should capture the bulk of articles referring to any of these concepts, with perhaps the exception of the latter group being overly general and likely more noisy by construction.[22] I use the textual information to construct three separate indicators for articles containing panchayat, education, or "women" words. I then use a difference-in-differences identification strategy to assess the change in the incidence of articles containing references to these concepts in state elections years versus non-election years to associated election years with differential coverage of these topics relative to non-election years. The estimating equation is:

$$Y_{i,s,t} = \delta_0 + \delta_1 * Election_{s,t} + \theta_s + \gamma_t + \varepsilon_{i,s,t} \quad (3)$$

[21] Available at: http://timesofindia.indiatimes.com/archive.cms, accessed May-September 2014.
[22] No other words, stems or combinations have been tested to identify alternative sets of article groups referencing these concepts.

Where $Y_{i,s,t}$ is an indicator for article i, referencing state s in year t, $Election_{s,t}$ an indicator for there being panchayat elections in state s and year t, and θ and γ the vectors of coefficients on state and year indicators, respectively. $\varepsilon_{i,s,t}$ is the disturbance term, potentially correlated within states. I estimate this specification with a linear probability model for the six outcome indicators in $Y_{i,s,t}$: separately for articles referencing the three topic groups above, as well as articles referencing three combinations of the topics (women and panchayats, school and panchayats, and women and education).[23]

Table 11 contains the estimated coefficients for δ_1 for the theme indicators, scaled in magnitude to be read as "per thousand articles". Within state and controlling for national yearly shocks, Column 1 shows an increase of 10 out of every 1,000 articles that reference panchayats in elections years versus non-election years; this is not particularly surprising: media coverage of the governance system goes up in election years. Column 2 shows that the share of articles referencing schools also increases in election years by nearly the same magnitude, while column 3 shows no discernible increase in articles containing the set of words used to capture references to women (admittedly an imprecise measure). Column 4 looks at the increase in the share of articles that reference both the panchayat system and women: nearly one-fifth of the increase in the share of articles referencing panchayats is comprised of articles also referencing women, and there is a smaller and marginally significant increase in the share of articles referencing both panchayats and schools. There is no increase in articles referencing both women and schools, although this may be due to the noise in the former measure.

The magnitudes seem small until the volume of new articles analyzed is considered. Given nearly 250,000 articles over the sample period, this translates to nearly 100 articles per day. Given the magnitudes above, this implies an additional article referencing panchayats *per day* in election years, and an additional article approximately weekly referencing panchayats and women, and panchayats and schools, in election years.

To some degree, making leaders salient of an obvious and natural role of the media—the above analysis provides corroborative empirical evidence of this being the case, rather than a

[23] It is important to note that this analysis does not attempt to distinguish whether news media changes in response to the presence of women leaders, or demand from readers for articles on certain topics, or if news media definitively drives educational outcomes; rather the analysis looks for evidence of this channel as one allowing women leaders to be salient to the general public.

rigorous test of the transmission of role-model effects. Given that the framework used earlier is one that considers cumulative effects of women leaders, more instances of increased salience over time (as estimated by the difference-in-difference estimator as "each election year") can be seen as a mechanism affecting preferences *over time*. A more rigorous test of the transmission of the role-model effects lefts for future work would necessarily also assess the consumption of media to understand for whom the aspirations are operating (parents, children, or both) and to investigate a range of specific media. Nonetheless, the above analysis suggests popular media, by making women leaders and issues of education more salient, may play a role in affecting the aspirations of, and/or for, younger constituents.[24]

Competition in unreserved parliamentary elections: educational attainment of candidates

Table 12 shows the educational composition of candidates and winners in the 2009 parliamentary (Lok Sabha) elections by sex. (It is important to note that there are no quotas for women in parliamentary elections.) Overall, there is no distinct difference in the distribution of educational attainment of candidates or election winners by sex, suggesting that women candidates are selected without overcompensating for sex bias with educational attainment *beyond* that of male candidates. At the same time, the equivalence in the distribution of educational attainment among candidates does not reflect persistent gender differences in educational attainment in the population; thus the difference in the average female candidate's education relative to that of the average female constituent is likely greater than the difference between the average male candidate's education relative to the average male constituent—suggesting educational attainment as one potential margin which allows women to achieve social status equivalent to that of men.

Competition in unreserved parliamentary elections: sex composition of candidates

Using variation in cumulative years of district leadership seats reserved for women, Table 13 shows the effect of an additional year of reservations for women on various measures of electoral competition in the 2009 parliamentary elections. Column 1 shows that each additional year of

[24] The Times of India is known to be slightly "center-left" in its reporting. This may skew the results upward if such leaning increases the responsiveness to reporting on local governance and/or coverage of gender issues. However, likely dominating such an effect is that the Times of India is a national English-language daily, which may be less likely to report on local elections and issues relative to regional or local newspapers—thus likely understating the media's role in making leaders salient to constituents at the local level.

exposure to the district chairperson seat being reserved for woman results in an increase of more than four percentage points in the probability that the constituency will have at least one female candidate running for an (unreserved) parliamentary seat. However, there is not a strong effect among women candidates winning the election (column 3), nor in the vote share garnered by women candidates.

Overall, there is a plausible channel for women leaders to be salient to constituents, provide information on the pedigree required to compete with men in the social sphere, and the quota system is linked to expressions of higher aspirations of women (politicians) who become more likely to contest unreserved parliamentary elections. While not exhaustive, these investigations lend further support for the potential of a role-model effect of leaders in changing the aspirations of constituents.

VIII. Conclusion

This paper assesses the relationship between political inclusion and economic well-being, finding that India's flagship political empowerment policy mandating representation quotas for women increased post-primary school enrollment rates of both girls and boys.

I examine a battery of potential mechanisms that could allow women leaders to affect the educational decisions of young constituents. There is no evidence of effects among schooling infrastructure, the labor market, or marriage/household dynamics. What remains are margins related to factors not directly observable with large-scale surveys: perceived returns to education and/or aspirations, whereby women leaders increase educational investment of girls via a "role model" effect. In the current context, increased educational aspirations are not observationally distinct from increases in perceived returns to education among young women, which I do not rule out as a potentially overlapping mechanisms (in both definition and consequence) that can be actuated by role models.[25] I provide novel empirical evidence of ways in which leaders serve as role models and indicators of a changing social fabric, affecting the expectations and aspirations of younger constituents.

[25] For evidence that children's educational expectations and aspirations are highly covariant (with a partial correlation of .5), see Bernard et al (2014).

The empirical strategy undertaken provides a method for assessing the long-term impacts of mandated seat quotas in a general context and using administrative data, as it takes advantage of a design naturally arising from constraints likely to be faced when similar policies are or have been enacted at a national level: staggered implementation arises from asynchronous election timings across jurisdictions, and randomly subjecting leadership seats to reservation is a fair and accepted method to implement such provisions for levels of the hierarchy which are comprised of only one individual.

Ideally, future work will further investigate the existence of the relationship between political and economic empowerment in other countries that have implemented seat or candidate quotas. As a body of work, such results can guide policymakers as to whether political reservations may be a viable option to reduce gender or social inequality across dimensions that are historically difficult to affect directly via typical policy levers.

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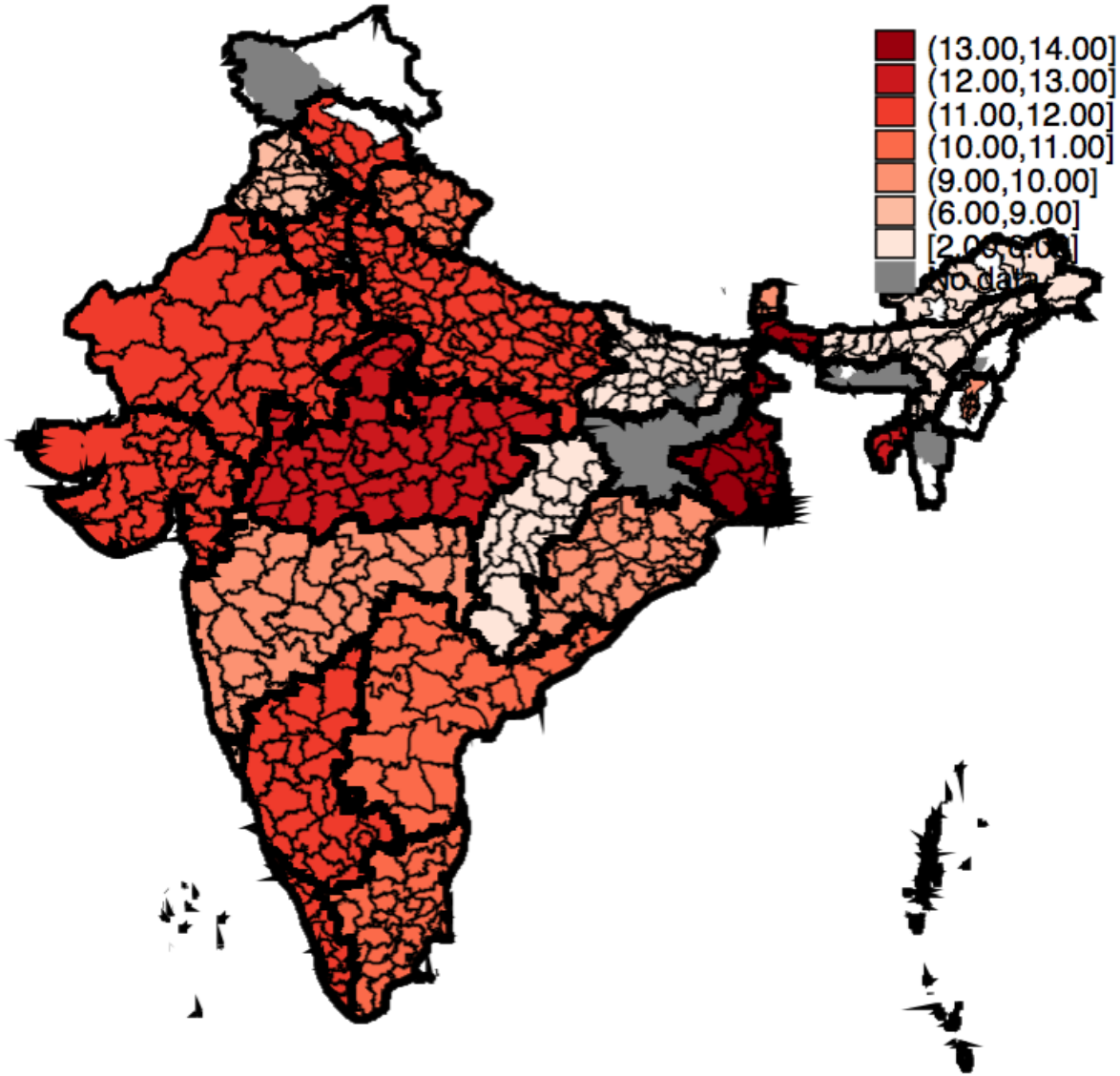
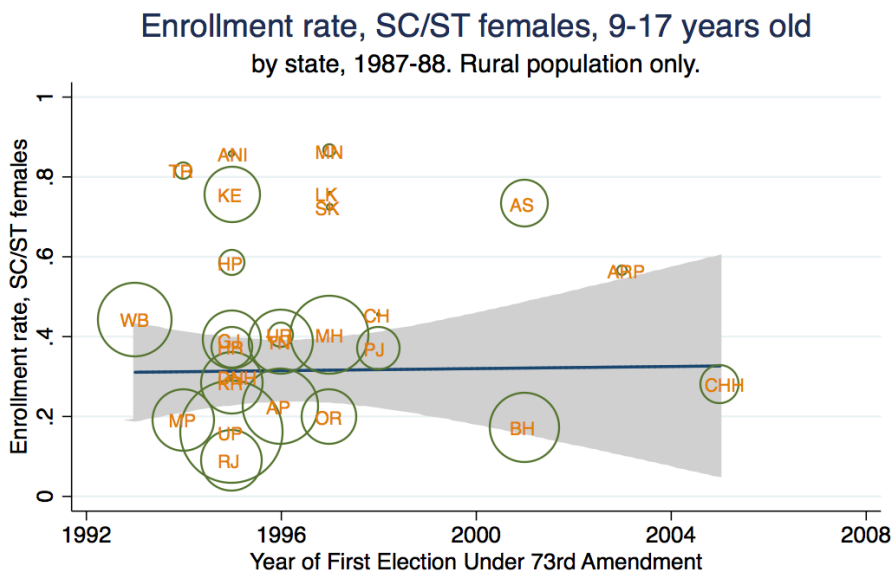
Figure 1

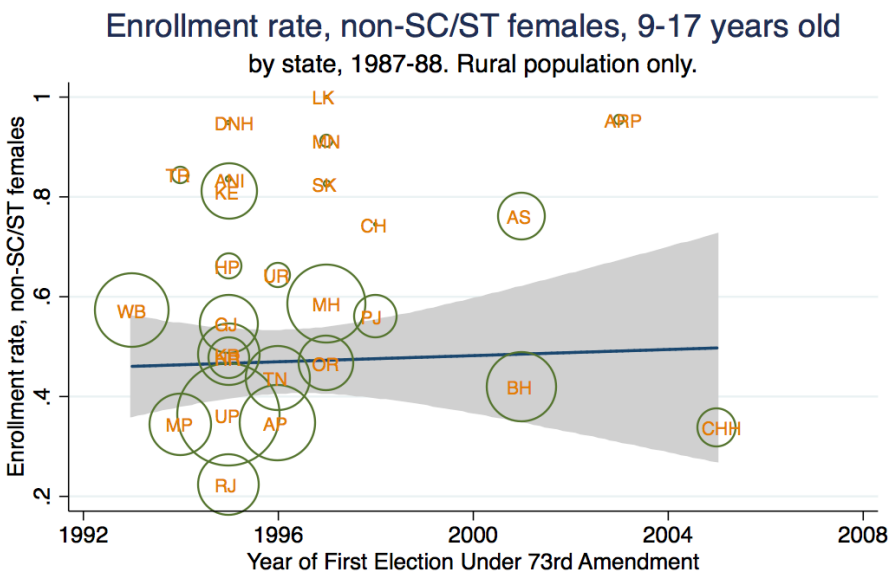
Cumulative years of 73rd Amendment reservations, 2007

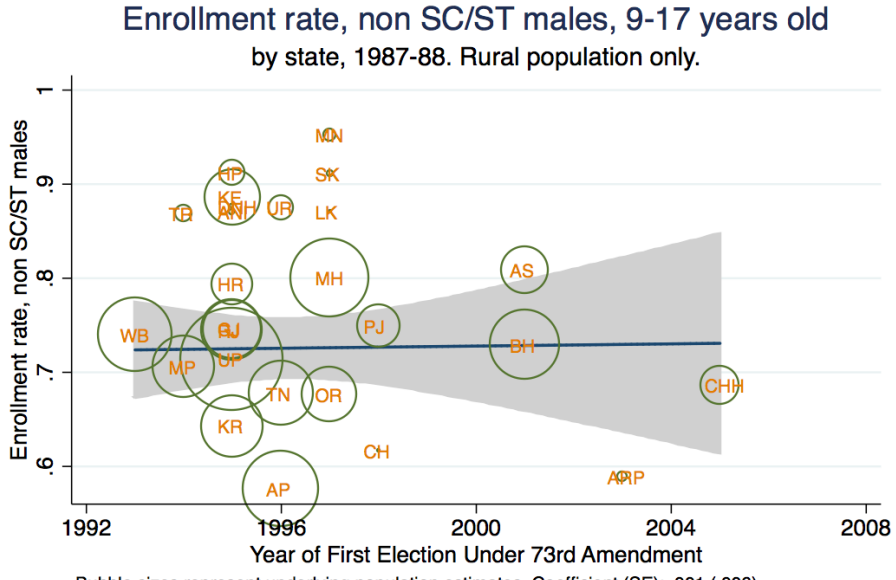


Source: Author's calculations.

Figure 2


Enrollment rate, SC/ST females, 9-17 years old
by state, 1987-88. Rural population only.
Enrollment rate, SC/ST females
0
.2
.4
.6
.8
1
1992
1996
2000
2004
2008
Year of First Election Under 73rd Amendment
Bubble sizes represent underlying population estimates. Coefficient (SE): .001 (.014).
95% confidence intervals shaded.


Enrollment rate, non-SC/ST females, 9-17 years old
by state, 1987-88. Rural population only.
Enrollment rate, non-SC/ST females
.2
.4
.6
.8
1
1992
1996
2000
2004
2008
Year of First Election Under 73rd Amendment
Bubble sizes represent underlying population estimates. Coefficient (SE): .003 (.012).
95% confidence intervals shaded.


Enrollment rate, non SC/ST males, 9-17 years old
by state, 1987-88. Rural population only.
Enrollment rate, non SC/ST males
.6
.7
.8
.9
1
1992
1996
2000
2004
2008
Year of First Election Under 73rd Amendment
Bubble sizes represent underlying population estimates. Coefficient (SE): .001 (.006).
95% confidence intervals shaded.

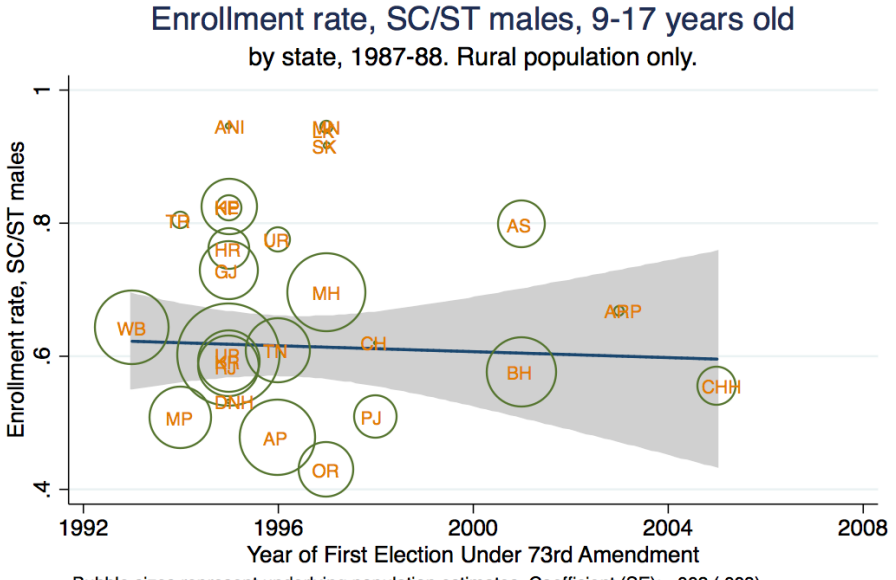

Enrollment rate, SC/ST males, 9-17 years old
by state, 1987-88. Rural population only.
Enrollment rate, SC/ST males
.4
.6
.8
1
1992
1996
2000
2004
2008
Year of First Election Under 73rd Amendment
Bubble sizes represent underlying population estimates. Coefficient (SE): -.002 (.008).
95% confidence intervals shaded.

Table 1
Mean enrollment rates in rural areas
by social group, gender, age and year

	non-SC/ST						SC/ST					
Age	**1987**	**1993**	**1999**	**2004**	**2007**	**Change**	**1987**	**1993**	**1999**	**2004**	**2007**	**Change**
Panel A: Male												
5 -- 8	.98	.99	.99	1.00	.99	**.01**	.96	.98	.99	.99	.99	**.03**
9 -- 11	.91	.94	.97	.99	.98	**.07**	.83	.89	.94	.98	.97	**.14**
12 -- 14	.78	.85	.88	.91	.93	**.15**	.65	.73	.79	.87	.88	**.23**
15 -- 17	.50	.55	.61	.63	.69	**.19**	.39	.41	.48	.51	.55	**.17**
Panel B: Female												
5 -- 8	.96	.98	.99	1.00	.99	**.03**	.94	.94	.98	.99	.97	**.03**
9 -- 11	.77	.84	.93	.96	.96	**.19**	.60	.73	.86	.93	.93	**.33**
12 -- 14	.50	.62	.73	.79	.83	**.33**	.32	.43	.60	.71	.78	**.46**
15 -- 17	.22	.31	.42	.48	.55	**.33**	.12	.19	.30	.37	.45	**.33**
Panel C: Female - Male enrollment gap												
5 -- 8	-.02	-.02	.00	.00	.00	**.02**	-.02	-.04	-.01	-.01	-.02	**.01**
9 -- 11	-.14	-.11	-.05	-.03	-.02	**.12**	-.23	-.16	-.09	-.05	-.04	**.19**
12 -- 14	-.28	-.23	-.15	-.12	-.10	**.18**	-.33	-.30	-.19	-.16	-.10	**.23**
15 -- 17	-.29	-.24	-.19	-.15	-.14	**.15**	-.27	-.22	-.19	-.14	-.10	**.16**

Notes: Author's calculations using National Sample Survey data (various rounds). Enrollment rates are calculated as the mean across individuals of an indicator for usual principal activity reported as "attending an educational institution" for the age groups specified. Population estimates are constructed by weighting by the inverse sampling probability (sample weights) provided with the data.

Table 2
Testing for differential trends in pre-policy enrollment rates

	Dependent variable: individual enrollment [0/1]			
Sample:	***women***		***men***	
	non-SC/ST	**SC/ST**	**non-SC/ST**	**SC/ST**
	(1)	(2)	(3)	(4)
Year	0.018	0.020	0.011	0.010
	(0.00213)	(0.00254)	(0.00174)	(0.00202)
Year*73rd CAA implementation year	0.00071	0.00009	0.00016	0.00107
	(0.00053)	(0.00066)	(0.00028)	(0.00044)
N	65,961	22,531	78,718	27,805
adj. R^2	0.289	0.306	0.186	0.202

Notes: Table presents a test of differential pre-period trends estimated via a linear probability model regressing individual-level enrollment status of 9 to 17 year-olds in rural areas across three pre-intervention survey waves on a linear time trend and the time trend interacted with the state-level policy implementation year. Heteroskedasticity-consistent robust standard errors clustered by state reported in parentheses. All specifications include an unreported constant term and vectors of state fixed effects. Estimations are weighted by the provided sampling weight. Significance indicated by: + p<.1, ++ p<.05, +++ p<.01.

Table 3
Summary statistics, cross-border estimation sample

Variable	**Mean**	**Std. Dev.**	**Min**	**Max**	***N***
Enrollment rate - 2007					
non-SC/ST, female	0.80	0.13	0.41	1.00	178
non-SC/ST, male	0.89	0.09	0.56	1.00	178
SC/ST, female	0.74	0.21	0.05	1.00	177
SC/ST, male	0.80	0.14	0.29	1.00	177
Enrollment rate - 1987					
non-SC/ST, female	0.45	0.20	0.00	0.96	174
non-SC/ST, male	0.72	0.15	0.00	1.00	174
SC/ST, female	0.29	0.22	0.00	1.00	172
SC/ST, male	0.61	0.21	0.00	1.00	173
Years Exposure to 73rd Amendment, 2007	10.7	2.7	2.0	14.0	178
Combined population of district pair, 1987	267,213	177,386	5,652	1,013,896	174

Notes: Table shows summary statistics for districts lying on state borders. District-level enrollment rates calculated as the count of individuals reporting usual principal activity as "attending an educational institution" divided by the total population for the sample of 9 to 17 year-olds in rural areas using sample weights.

Table 4
Estimation of school enrollment rates
cross-state border district variation

	Dependent variable: district enrollment rate			
Specification:	**Base**	**Incl. 1987 control**	**SUR**	**Incl. policy controls**
	(1)	(2)	(3)	(4)
Panel A: non-SC/ST women				
Exposure, 2007 (years)	0.007+	0.007+	0.007+++	0.008++
	(0.004)	(0.004)	(0.002)	(0.003)
Mean outcome	0.803	0.803	0.803	0.803
Std. Dev. Outcome	0.126	0.126	0.126	0.126
Pre-policy control	N	Y	Y	Y
N	178	178	178	178
adj. R^2	0.406	0.416	0.416	0.429
Panel B: non-SC/ST men				
Exposure, 2007 (years)	0.006++	0.006+	0.006+++	0.006++
	(0.003)	(0.003)	(0.002)	(0.003)
Mean outcome	0.889	0.889	0.889	0.889
Std. Dev. Outcome	0.089	0.089	0.089	0.089
Pre-policy control	N	Y	Y	Y
N	178	178	178	178
adj. R^2	0.350	0.356	0.356	0.352

Notes: Table presents coefficients from an unweighted linear regression of the enrollment rate in 2007 on the 2007 cumulative policy exposure among neighboring districts lying across state borders. Underlying sample is comprised of 9-17 year-olds in rural areas. Enrollment rates are calculated using sample weights. Heteroskedasticity-consistent robust standard errors two-way clustered by state and district reported in parentheses. All specifications include an unreported constant term and district pair fixed effects. Significance indicated by: + p<.1, ++ p<.05, +++ p<.01.

Table 5
Estimation of school enrollment rates
cross-state border district variation

	Dependent variable: district enrollment rate			
Specification:	**Base**	**Incl. 1987 control**	**SUR**	**Incl. policy controls**
	(1)	(2)	(3)	(4)
Panel A: SC/ST women				
Exposure, 2007 (years)	0.048+++	0.045+++	0.045+++	0.046+++
	(0.011)	(0.010)	(0.006)	(0.010)
Mean outcome	0.737	0.737	0.737	0.737
Std. Dev. Outcome	0.211	0.211	0.211	0.211
Pre-policy control	N	Y	Y	Y
N	178	178	178	178
adj. R^2	0.350	0.351	0.351	0.334
Panel B: SC/ST men				
Exposure, 2007 (years)	0.025++	0.025++	0.025+++	0.025++
	(0.011)	(0.011)	(0.007)	(0.010)
Mean outcome	0.801	0.801	0.801	0.801
Std. Dev. Outcome	0.143	0.143	0.143	0.143
Pre-policy control	N	Y	Y	Y
N	178	178	178	178
adj. R^2	0.315	0.307	0.307	0.363

Notes: Table presents coefficients from an unweighted linear regression of the enrollment rate in 2007 on the 2007 cumulative policy exposure among neighboring districts lying across state borders. Underlying sample is comprised of 9-17 year-olds in rural areas. Enrollment rates are calculated using sample weights. Heteroskedasticity-consistent robust standard errors two-way clustered by state and district reported in parentheses. All specifications include an unreported constant term and district pair fixed effects. Significance indicated by: + p<.1, ++ p<.05, +++ p<.01.

Table 6
Cumulative effect of reservation on alternative time use outcomes
cross-state border district variation

	Dependent variable: district mean share of 9-17 year-olds engaged in activity				
Outcome:	**In labor force**	**LF: Work in hh enterprise**	**LF: Work in formal sector**	**LF: Looking for work**	**Household duties**
	(1)	(2)	(3)	(4)	(5)
Panel A: non-SC/ST women					
Exposure, 2007 (years)	-0.003	-0.003++	0.001	-0.002++	-0.005
	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)
Mean outcome	0.051	0.046	0.002	0.016	0.146
Std. Dev. Outcome	0.073	0.070	0.012	0.035	0.119
Pre-policy control	Y	Y	Y	Y	Y
N	178	178	178	178	178
adj. R^2	0.400	0.417	0.140	0.065	0.409
Panel B: non-SC/ST men					
Exposure, 2007 (years)	-0.006+++	-0.005++	-0.000+	-0.001	0.000
	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)
Mean outcome	0.102	0.084	0.007	0.026	0.009
Std. Dev. Outcome	0.085	0.080	0.017	0.040	0.022
Pre-policy control	Y	Y	Y	Y	Y
N	178	178	178	178	178
adj. R2	0.265	0.359	0.117	0	0.469

Notes: Table presents coefficients from an unweighted linear regression of the district enrollment rate in 2007 on the 2007 policy exposure measure among neighboring districts lying across state borders. Underlying sample is comprised of 25-35 year-old women in rural areas. All measures calculated using sample weights. Heteroskedasticity-consistent robust standard errors clustered by state reported in parentheses. All specifications include an unreported constant term, district pair fixed effects, and a district-level pre-policy control measure. Significance indicated by: + p<.1, ++ p<.05, +++ p<.01.

Table 7
Cumulative effect of reservation on alternative time use outcomes
cross-state border district variation

	Dependent variable: district mean share of 9-17 year-olds engaged in activity				
Outcome:	**In labor force**	**LF: Work in hh enterprise**	**LF: Work in formal sector**	**LF: Looking for work**	**Household duties**
	(1)	(2)	(3)	(4)	(5)
Panel A: SC/ST women					
Exposure, 2007 (years)	-0.003	-0.002	-0.001	-0.000	-0.042+++
	(0.005)	(0.005)	(0.001)	(0.002)	(0.006)
Mean outcome	0.073	0.070	0.002	0.020	0.190
Std. Dev. Outcome	0.101	0.100	0.016	0.045	0.183
Pre-policy control	Y	Y	Y	Y	Y
N	177	177	177	177	177
adj. R2	0.237	0.230	0.169	0.151	0.244
Panel B: SC/ST men					
Exposure, 2007 (years)	-0.016++	-0.005	-0.004	-0.012++	-0.009+++
	(0.007)	(0.008)	(0.002)	(0.005)	(0.002)
Mean outcome	0.172	0.144	0.010	0.047	0.027
Std. Dev. Outcome	0.132	0.132	0.037	0.062	0.064
Pre-policy control	Y	Y	Y	Y	Y
N	177	177	177	177	177
adj. R^2	0.213	0.226	-0.232	0.005	0.471

Notes: Table presents coefficients from an unweighted linear regression of the district enrollment rate in 2007 on the 2007 policy exposure measure among neighboring districts lying across state borders. Underlying sample is comprised of 25-35 year-old women in rural areas. All measures calculated using sample weights. Heteroskedasticity-consistent robust standard errors clustered by state reported in parentheses. All specifications include an unreported constant term, district pair fixed effects, and a district-level pre-policy control measure. Significance indicated by: + p<.1, ++ p<.05, +++ p<.01.

Table 8
Parameter heterogeneity in enrollment effects
cross-state border district variation

	Dependent variable: district enrollment rate							
		Age			**Income tercile**		**Highest F. educ. Level**	
Sample group:	**9 to 11**	**12 to 14**	**15 to 17**	**1st**	**2nd**	**3rd**	**Illiteracy**	**Literacy**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: non-SC/ST women								
Exposure, 2007 (years)	-0.002	0.010	0.003	0.013++	0.016	0.009	0.007++	-0.002
	(0.003)	(0.007)	(0.008)	(0.005)	(0.010)	(0.007)	(0.003)	(0.004)
adj. R^2	0.173	0.329	0.350	0.377	0.119	0.225	0.259	0.035
N	177	176	174	150	170	173	174	172
Panel B: non-SC/ST men								
Exposure, 2007 (years)	-0.001	-0.004	0.018+++	0.007+	0.008	-0.002	0.005	-0.001
	(0.001)	(0.004)	(0.006)	(0.003)	(0.007)	(0.009)	(0.003)	(0.003)
adj. R^2	0.101	0.240	0.325	0.095	0.072	0.050	0.212	0.120
N	177	176	174	150	170	173	174	172
Panel C: SC/ST women								
Exposure, 2007 (years)	0.021	0.037++	0.101+++	0.058++	0.019	0.074	0.042+++	-0.028
	(0.013)	(0.014)	(0.016)	(0.023)	(0.032)	(0.046)	(0.011)	(0.024)
adj. R^2	0.027	0.314	0.389	0.096	0.077	0.159	0.310	0.417
N	174	167	167	141	157	142	173	133
Panel D: SC/ST men								
Exposure, 2007 (years)	0.007++	0.031++	0.027	0.018	0.007	0.138+++	0.019+	0.043
	(0.003)	(0.014)	(0.030)	(0.019)	(0.024)	(0.028)	(0.011)	(0.034)
adj. R^2	0.151	0.172	0.170	0.104	0.058	0.408	0.307	0.022
N	174	171	167	142	164	159	173	137

Notes: Table presents coefficients from an unweighted linear regression of the district enrollment rate in 2007 on the 2007 policy exposure measure among neighboring districts lying across state borders. Underlying sample indicacted by panel and column titles. Enrollment rates are calculated using sample weights. Heteroskedasticity-consistent robust standard errors clustered by state reported in parentheses. All specifications include an unreported constant term, district pair fixed effects, and a district-level pre-policy control measure. Significance indicated by: + p<.1, ++ p<.05, +++ p<.01.

Table 9
Differences in schooling infrastructure and schooling environment
cross-state border district variation

	Dependent variable: district schooling infrastructure indicator						
	Schools per total population	**Schools per population (rural)**	**New schools established per 000 population**	**Share of classrooms in "good" condition**	**Teachers per pupil**	**Share of schools with girls' lavatory**	**Share teachers female**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exposure, 2007 (years)	-0.00001	-0.00003	-0.00001	0.00728	0.01960	-0.00017	-0.00756
	(0.00004)	(0.00004)	(0.00003)	(0.00806)	(0.01539)	(0.00031)	(0.00734)
N	176	176	176	177	177	177	177
adj. R^2	0.435	0.500	0.347	0.512	0.404	0.480	0.588

Notes: Table presents coefficients from an unweighted linear regression of the district schooling infrastructure measure (indicated by column) in 2007 on the 2007 policy exposure measure among neighboring districts lying across state borders. Heteroskedasticity-consistent robust standard errors clustered by state reported in parentheses. All specifications include an unreported constant term, district pair fixed effects, and a district-level pre-policy control measure. Significance indicated by: + p<.1, ++ p<.05, +++ p<.01.

Table 10
Cumulative effect of reservation on alternative outcomes
cross-state border district variation

	Dependent variable: standardized district indicator mean						
Outcome:	**Labor force participation**	**share married**	**number of children | married=1**	**age at first birth | married=1**	**husband's education (years) | married = 1**	**husb. ed*own ed. | married = 1**	**husband's wage | married = 1**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: non-SC/ST women							
Exposure, 2007 (years)	0.024	-0.045	0.015	-0.114++	0.033	0.004	0.002
	(0.033)	(0.042)	(0.034)	(0.041)	(0.045)	(0.039)	(0.030)
N	178	163	163	163	163	163	162
adj. R^2	0.352	0.038	0.084	-0.058	0.289	0.378	0.217
Panel B: SC/ST women							
Exposure, 2007 (years)	0.004	-0.056	0.010	-0.116+++	-0.064	-0.051	0.030++
	(0.045)	(0.055)	(0.032)	(0.033)	(0.039)	(0.059)	(0.011)
N	177	174	174	173	174	174	173
adj. R^2	0.118	0.147	0.331	-0.199	0.351	0.168	0.421

Notes: Table presents coefficients from an unweighted linear regression of the district enrollment rate in 2007 on the 2007 policy exposure measure among neighboring districts lying across state borders. Underlying sample is comprised of 25-35 year-old women in rural areas. Enrollment rates are calculated using sample weights. Heteroskedasticity-consistent robust standard errors clustered by state reported in parentheses. All specifications include an unreported constant term, district pair fixed effects, and a district-level pre-policy control measure. Significance indicated by: + p<.1, ++ p<.05, +++ p<.01.

Table 11
Salience of leaders via news media:
Difference-in-differences estimates using the Times of India web archive (2001-2007)

	Dependent variable: indicator for at least one word in topic set(s) found in article text					
	Panchayats/ leaders	**School/ education**	**Women/ girls**	**Women/girls and Panchayats/ leaders**	**School/ education and Panchayats/ leaders**	**Women/ girls and school/ education**
	(1)	(2)	(3)	(4)	(5)	(6)
Election year indicator	10.33+++	9.708+++	-2.297	1.711++	1.033+	-0.630
	(2.960)	(4.197)	(4.071)	(0.861)	(0.635)	(2.728)
N	244,462	244,462	244,462	244,462	244,462	244,462
adj. R^2	0.009	0.011	0.003	0.002	0.003	0.002

Notes: Table presents coefficients from an unweighted linear probability model regressing an indicator for appearance of word related to the topics in column headers on an indicator for the state and year the article references holding Panchayat elections. Heteroskedasticity-consistent robust standard errors clustered by state reported in parentheses. All specifications include separate vectors of state and year fixed effects and an unreported constant term. Significance indicated by: + p<.1,

Table 12
Education of Lok Sabha Candidates and Members, 2009 elections

	Candidates		Winners	
Education level	**Women**	**Men**	**Women**	**Men**
Illiterate	4%	3%	0%	1%
Some School	27%	27%	7%	12%
Undergraduate	10%	10%	18%	8%
Graduate	37%	40%	67%	73%
Unknown	22%	20%	9%	6%
Total	*100%*	*100%*	*100%*	*100%*

Source: Author's calculations based on data from empoweringindia.org. Chi-square tests fail to reject distributional equality across either the candidate (X^2=4.53, p>.30) or the winner (X^2=6.69, p>.15) samples.

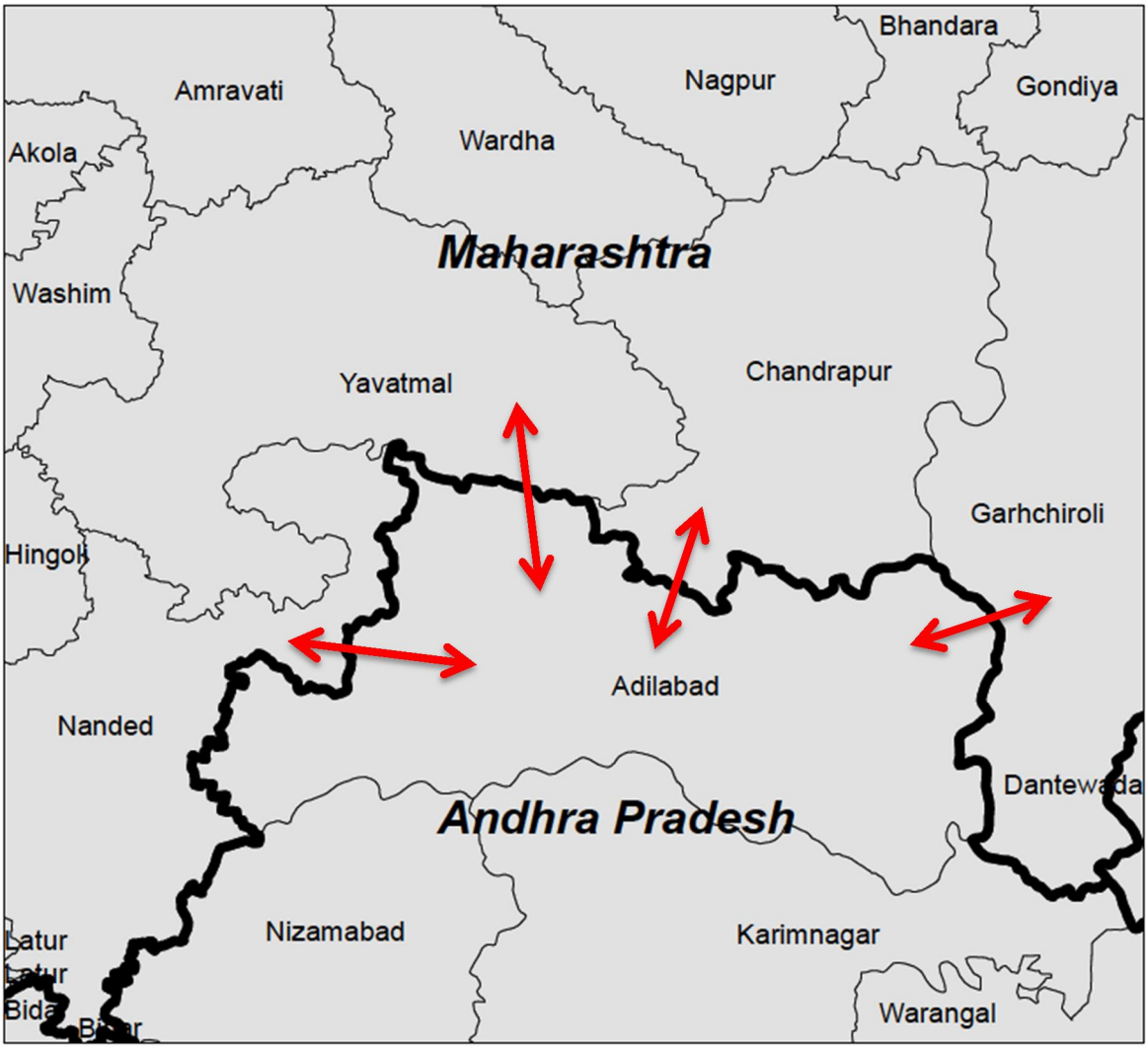
Table 13
Cumulative effect of district leadership on contesting Lok Sabha elections
within-state constituency variation

Outcome:	Any female candidate	Share of candidates female	Whether female candidate won	Vote share to female candidates	Voter turnout
	(1)	(2)	(3)	(4)	(5)
Culumative chairperson reservations	0.042+	0.005	0.009	0.559	0.083
as of 2007 (years)	(0.022)	(0.004)	(0.016)	(0.824)	(0.242)
Mean outcome	0.603	0.069	0.121	9.339	58.982
Std. Dev. Outcome	0.491	0.079	0.327	18.145	13.340
State fixed effects	Y	Y	Y	Y	Y
N	141	141	141	141	141
adj. R^2	0.038	-0.003	0.059	0.033	0.807

Notes: Table presents coefficients from an unweighted, cross-sectional linear regression of 2009 measures of parliamentary election competition indicated in column headers on cumulative years of reservation for women in district chairperson seats in 2007. All specifications include an unreported constant term and state fixed effects. Significance indicated by: + p<.1, ++ p<.05, +++ p<.01.

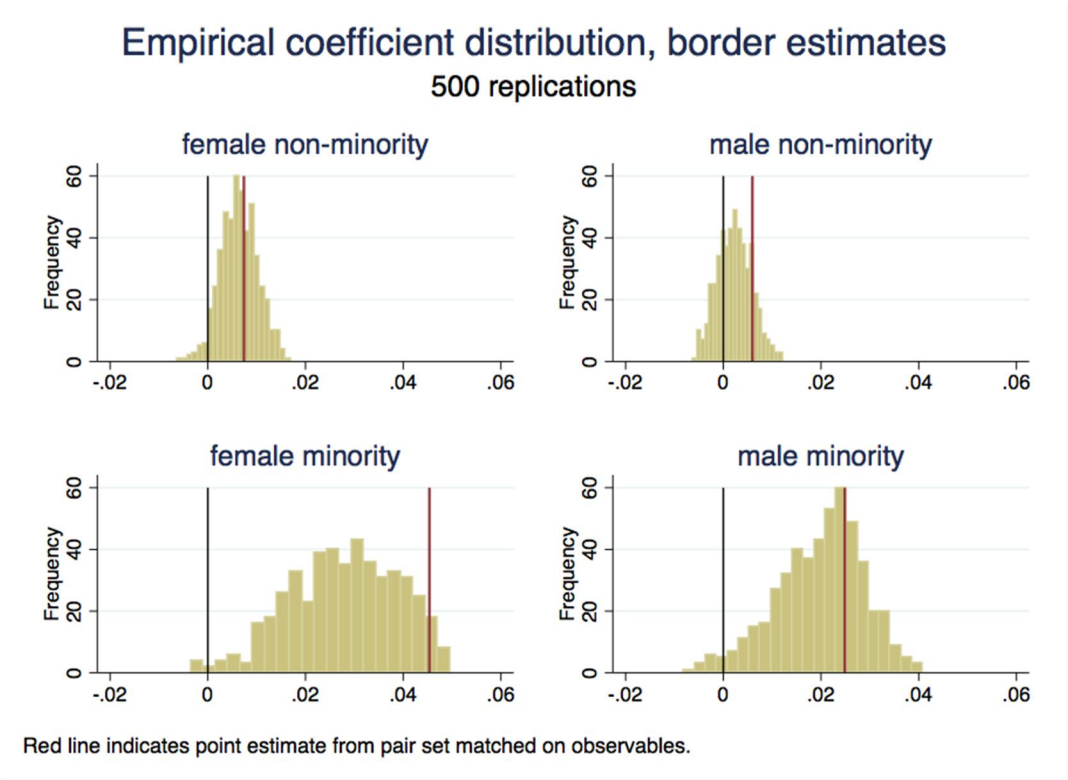
Appendix

Appendix Figure 1

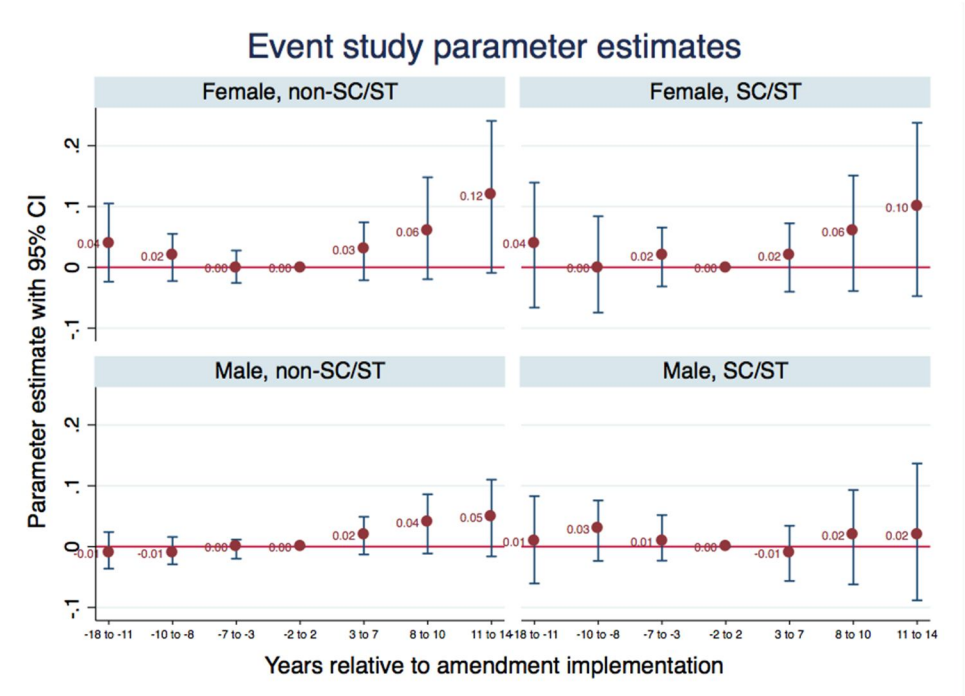


Note: Map of Maharashtra-Andhra Pradesh border at district Adilabad. Map depicts example of multiple district neighbor pairs.

Appendix Figure 2



Appendix Figure 3



Appendix Table 1
Comparison of state border and interior district enrollment rates

		Interior	State border	Border - Interior	p-value	*N*
	Panel A: 1987					
Enrollment rates	non-SC/ST, female	0.468	0.477	0.009	0.71	374
	non-SC/ST, male	0.737	0.722	-0.015	0.37	374
	SC/ST, female	0.335	0.333	-0.002	0.95	368
	SC/ST, male	0.647	0.625	-0.022	0.36	368
	Female population share	0.445	0.438	-0.007	0.30	374
	SC/ST population share	0.220	0.319	0.099+++	0.00	374
	Panel B: 2007					
Enrollment rates	non-SC/ST, female	0.787	0.815	0.028+	0.06	384
	non-SC/ST, male	0.876	0.882	0.006	0.63	384
	SC/ST, female	0.768	0.760	-0.008	0.71	376
	SC/ST, male	0.813	0.824	0.011	0.50	376
	Female population share	0.466	0.465	-0.000	0.95	384
	SC/ST population share	0.272	0.370	0.099+++	0.00	384

Notes: Table shows test of observable differences across border and interior districts in 1987 and 2007. Statistical significance tested using two-sample *t* -tests assuming equal variances. Significance levels are indicated by + <.10, ++ <.05, +++ <.01.

Appendix Table 2
Decentralization versus quotas: using chairperson reservations to estimate enrollment rates
within state, cross-district variation

	Dependent variable: Enrollment rate			
Sample:	***non-SC/ST***		***SC/ST***	
	women	**men**	**women**	**men**
	(1)	(2)	(3)	(4)
Border estimates via cumulative exposure				
Exposure to chairperson reservations (years)	0.009	0.005	0.012++	0.007+
	(0.009)	(0.005)	(0.056)	(0.003)
Mean outcome	0.794	0.886	0.707	0.791
Std. Dev. Outcome	0.122	0.093	0.214	0.131
Pre-policy control	Y	Y	Y	Y
District-pair fixed effects	Y	Y	Y	Y
N	100	100	100	100
adj. R^2	0.230	0.327	0.210	0.208
Within-state chairperson reservations				
Exposure to chairperson reservations (years)	0.000	-0.004	0.017+++	0.008+
	(0.003)	(0.003)	(0.005)	(0.005)
Mean outcome	0.771	0.862	0.702	0.788
Std. Dev. Outcome	0.420	0.345	0.457	0.409
Pre-policy control	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y
N	10170	11399	4275	4994
adj. R^2	0.037	0.021	0.030	0.007

estimates from a linear probability model of individual-level enrollment status indicator regressed on the years of chairperson reservations as of 2007. Panels contain coefficients from separate regressions estimated for the subsample indicated in the column header. Heteroskedasticity-consistent robust standard errors clustered by state (toppanel) or district (bottom panel) reported in parentheses. Sample is comprised of individuals 9-17 years old in rural areas. All specifications contain an unreported constant term. Significance indicated by: + p<.1, ++ p<.05, +++ p<.01.