6-2016

Essays in Labor and Development Economics

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by

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A dissertation submitted to the Graduate Faculty in Economics in partial fulfillment of the requirements for the degree of Doctor of Philosophy, The City University of New York

2016

This manuscript has been read and accepted by the Graduate Faculty in Economics in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

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This dissertation is comprised of two chapters that investigate the long-term effects of a quota policy for women in local government in India.

Chapter 1 In the first chapter, "Political Inclusion and Educational Investment," I investigate whether political empowerment can affect the human capital investment decisions of children. By using exogenous variation in the implementation of the policy, nationally-representative survey data allow me to estimate effects on educational enrollment in a geographic discontinuity design using adjacent areas on either side of a state border as counterfactuals receiving different levels of exposure to the quota policy.

In this paper, I find a sizable increase in the enrollment rates of both female and male school-age children resulting from additional exposure to local female leaders. I find that the effects are particularly concentrated among poorer households and those with less-educated proximate role models, and were commensurate with reductions in idle time and employment in household enterprises. There is no evidence for enrollment increases being facilitated by changes in school infrastructure, the labor market, or indicators related to intra-household dynamics. Overall, this paper makes a broader contribution to the literature in taking seriously the need to evaluate the external validity of experimental studies. Earlier work on this topic has focused on evaluating the same quota policy in a geographically restricted setting, with the added benefit of high internal validity but at the risk of the findings potentially being limited in their generalizability to the country as a whole. This chapter seeks

to understand whether these earlier results do generalize, and if so, where and among which populations. While I find similar effects on enrollment among young women, I also find that the increase in scope reveals increases in enrollment among young men as well, which had not previously been found. This opens the potential for future work investigating the reasons for this response among young men.

Chapter 2 The second chapter asks how the quota policy in local government affected participation and representation in higher levels of government over time. This study is motivated by the argument that quota policies have the potential to increase the participation of targeted groups even after they are no longer in place, or in environments not directly subject to quotas. I test this argument empirically using variation over a span of 15 years in the assignment of one-third of powerful leadership seats in Indian local government being randomly held by women. Quotas increase the number of female candidates who later contest seats in state and national legislatures, where such policies do not exist. The effects can be explained by repeat candidacies of career politicians (indirect evidence of a response to *changed voter preferences*) and new candidacies by politicians who gain particular experience from the quotas (a *candidate supply* effect). Effect magnitudes imply that the policy accounts for a substantial portion of the increase in female candidates in these bodies since the start of the policy. Quotas do not induce permanent change in the presence of female candidates in higher office due to the low probability of a female candidate winning an election, and effects on candidacy subsiding over time. This paper adds to the literature by evaluating the effect of quota policies on the broader occupation of political public service by taking a career-based view of the potential for quotas to have lasting effects on the individual beneficiaries and same-group members in the long term.

The two papers further elucidate the potential for quota policies to have effects beyond the immediate environments to which they are applied. Such evaluations are particularly

important to fully understand the range of unintended consequences – be they negative or positive – of social and economic policies enacted in developing countries and among marginalized populations.

Acknowledgments

I wish to thank my advisor, David Jaeger, as well as committee members and others who gave advice and support, including Ted Joyce, Jonathan Conning, Mike Grossman, Bill Kerr and Hunt Allcott. Since I was very young, my parents, Bryan and Rosemary, led me to intellectual challenge and academic pursuit; I owe my proclivity toward study to the environment they fostered and the standards they set. I wish to also thank Paloma Lopez de mesa Moyano, who has been a guiding light of support, enthusiasm and encouragement throughout the past several years. The Program in Economics at the Graduate Center (CUNY) provided resources that facilitated my research. I am thankful to the Graduate Center (CUNY) for providing material support for my graduate study. Many other friends and colleagues provided helpful input and encouragement along the way; for those I have not mentioned here, thank you.

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Chapter 1

Political Inclusion and Educational Investment

1.1 Introduction

Many countries enact policies to increase the social and economic participation of historically disadvantaged groups. In India, a one-third seat quota for women in local government has been in place since the early 1990s; however, mandated seat or candidate quotas in elected bodies are common elsewhere as well, 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. Recent work has shown substantive effects of the India quota policy for women in local government outside the particular environment to which it has been applied. These effects have been found among children's school enrollment and aspirations (Beaman et al., 2012a), crime reporting (Iyer et al., 2012a), small-scale business creation (Ghani et al., 2014), and sex selection (Kalsi, 2015), among others.

[1] Author's calculations based on data from The Quota Project (2015).

In this paper, I investigate long-term effects of the Indian seat reservation policy on school enrollment rates of young constituents. Motivated by the findings of Beaman et al. (2009), who examine the effects of exposure to women in village-level leadership positions on educational enrollment and aspirations in a sample of villages in one state of India, the current work uses nationally representative survey data and a quasi-experimental identification strategy exploiting geographic discontinuities in policy exposure to identify effects of exposure to the quota policy. This analysis provides important insight into the degree to which political quota policies can affect educational decisions of children across a highly heterogeneous country like India.

Human capital investment decisions are made amidst a complex set of factors that may be slow to change, including long-held traditions and cultural beliefs, with immediate or short-term effects being small or difficult to detect empirically. For this reason, I follow earlier studies estimating effects of local political leaders on educational enrollment using cumulative differences in policy exposure arising over a period of 15 years. This exposure measure arises from the staggered state-level implementation of the quota policy first used by Iyer et al. (2012a), and more recently by Ghani et al. (2014) and Kalsi (2015).

I first approach the estimation of policy effects on enrollment rates as an event study, allowing for an unrestricted view of differences in the evolution of enrollment rates over time relative to policy implementation. I then implement a geographic discontinuity design in which contiguous areas spanning state borders are used to remove persistent unobservables in the estimation of causal effects on enrollment. Mirroring the event study estimates, the border identification strategy finds effects of the policy on enrollment rates of both young women and young men – on the order of one to two percentage points (approximately 0.05 -0.10 standard deviations) per additional year of exposure. These effects are substantial in view of pre-policy enrollment rates and the average policy exposure of just over ten years as of the period of analysis.

Rich information on household characteristics and composition available in the survey data is used to show that effects were strongest among more economically disadvantaged individuals and those who were from families with uneducated adult women. Using additional detail on labor force participation and household responsibilities, I find that enrollment increases were effected though reductions in idle time and non-labor force household duties. There was also a shift out of household-based employment, although the magnitude of reduction is smaller and less pervasive across groups than reductions in other activities. Despite a nontrivial share of children engaged in the labor force, particularly among poorer households, this does not appear to be the primary margin from which enrollment gains were seen.

I then examine potential channels by which political seat quotas can affect educational decisions. Using detailed administrative data on school infrastructure, I show that there is no evidence of increases in either the extensive or intensive margins of schooling infrastructure resulting from longer exposure to seat quotas. I also find no effects in the labor market or among other typical indicators of women's empowerment.

While the core findings of this paper mirror those of Beaman et al. (2012a) in pattern and magnitude for young women, I provide several contributions. The first is to substantially extend the generalizability of results found in Beaman et al. (2012a) to the national level using quasi-experimental identification strategies. This is important in view of the substantial heterogeneity across regions of India and the need for continued assessment of the external validity of findings from pioneering experimental studies. Second, I shed light on the activities displaced by increased enrollment following the policy, as well as use schooling infrastructure data and labor force surveys to test alternative hypotheses regarding public goods provision and direct effects on the labor market. The lack of evidence for these channels further supports the notion that local leaders change educational decisions by serving as "role model" and potentially changing perceived returns to education. Finally, the finding of effects among young men in a slightly older age cohort highlights the potential for future

research on dynamic responses to increased educational attainment by women in this and other contexts.

The following section describes essential aspects of the reservations policy and related literature. I then discuss the data and the empirical methodology, and present results from the event study and border identification strategy. This is followed by an investigation into heterogeneous effects, displaced activities, and causal channels. The final section concludes with a discussion of how results complement the findings of earlier literature, and avenues for future research.

1.2 India's 73rd Constitutional Amendment Act

Passed in 1992, India's 73rd Constitutional Amendment formalized national support for an historical, decentralized governance structure known as the panchayat. Importantly for this study, the 73rd Amendment instituted a three-tiered system of local government at the village, sub-district (block), and district levels across rural areas of the country that included a one-third seat reservation at each governance level within local government to be filled by women, and a proportionate seat share reserved for representatives from historically marginalized social groups (Scheduled Castes and Scheduled Tribes, hereafter noted by "SC/ST"). For further detail on this history and implementation of the 73rd Amendment, I refer the reader to studies that examine this policy and its effects (Beaman et al. (2012a), Iyer et al. (2012a), Ghani et al. (2014), and Kalsi (2015), among others).

As detailed by Iyer et al. (2012a) and Ghani et al. (2014), the 73rd Amendment stipulated that states had the responsibility to adjust or amend local elections to comply with the provisions of the Amendments, and nearly all states amended existing laws or passed new laws to be compliant within one year. However, there is considerable variation in the timing of these first elections across states. As first pointed out by Iyer et al. (2012a), 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.

Once the provisions of the reform were implemented, one-third of seats were reserved for women at each level of the three-tiered local governance hierarchy. Although comprehensive data do not exist for all states, available evidence confirms there was only minimal involvement of women in local elected bodies prior to this reform (Mathew 2000). The policy thus represented a substantial and sharp change in the participation on women in the public sphere in rural India.

A number of studies have shown how decisions of a governance body adjust with the application of quotas or other changes to 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, 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. However, these findings are less strong when extended to South India (Ban and Rao 2008). 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. Iyer et al. (2012a) 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).[2] Ghani et al. (2014) quantify the link between the timing of reservations and changes in women engaged in India's manufacturing sector, finding 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 (2010b) shows how the provision of information on returns to education in the Dominican Republic affects enrollment, while Jensen (2010a) 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 relationship between political empowerment and education uses cross-

[2]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.

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. (2012a) 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). 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. With a larger geographic scope, in this paper I follow the conceptual approach of exploiting exogenous cross-sectional geographic variation in exposure to leaders to identify longer-term, cumulative effects of women leaders on educational enrollment.

1.3 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),

Mathew (2000), GOI 2008, (Iyer et al., 2012a), Kalsi (2015)). India's National Sample Survey Organisation "Socio-Economic SurveySchedule 10: Employment and Unemployment" provides data on enrollment (hereafter referred to as "NSS data"). 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 analyses use data from surveys in from 1983-84 to 2007-08, over which period there were six survey rounds (additionally in 1987-88, 1993-94, 1999-2000, and 2004-5). For simplicity, I refer to survey years only by the initial year.

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 restricting to the sample of school-age children in rural areas of the country, minimal data cleaning procedures were required aside from geographic definitions being made consistent over time to account for changes in administrative boundaries and the bifurcation of three states in 2001.

I use the 2007-8 survey year as the relevant outcome period for two reasons. First, after 2006-7 several states increased seat quotas for women in local government 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. Second, 2006-07 is the same period of study in Beaman et al. (2012a), so any findings will be as temporally comparable as possible.

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.[3]

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 1 shows separately the initial state-level enrollment rates for the four sample groups by gender and social group 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 pre-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

[3]Azam and Kingdon (2013) 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.

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 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, providing substantial evidence of the parallel trends assumption necessary for identification from this staggered policy implementation.

1.4 Event Study Estimates

The event study approach allows for an unrestricted examination of the differences in enrollment rates in the years before and after the policy change. This permits observation of effects of the policy that may be missed by standard difference-in-differences when longer-term dynamics may exist Wolfers (2006). The event study specification is typically represented by the following equation, where k indexes years relative to the policy exposure:

$$enrolled_{ist} = \delta_0 + \sum_k \Gamma_{k(st)} + \lambda_s + \gamma_t + \lambda_s * year_t + u_{ist} \tag{1.1}$$

In this equation, s indexes states, t indexes years, and $Yearssinceimplementation_{s,t}$ is defined as the number of years individual i in state s and year t is observed relative to policy implementation in state s. The specification is typical of a standard difference-in-differences approach, with the modification that the term indicating active treatment is expanded into a vector of indicators in β_k for different years relative to the policy implementation. Due to the frequency of the survey I use, I am able to observe enrollment only at intervals of four to six years, rather than annually, requiring the definition of k to correspond to a range of

years corresponding to the (average) interval frequency of observation. Standard errors are clustered by state and year corresponding to the definition of the vector of policy variables.

The year range spanning policy implementation in a given state is normalized to zero as the omitted group, and an indicator function creates the leads and lags (for different intervals) conditional on the year and state in which the respondent's status is observed. To limit concerns over an unbalanced state panel, states not observed in all five of the intervals are removed from the sample (although this does not affect the results substantively).

Using six rounds of survey data from 1983 to 2007, Figure 2 presents the coefficient vector β separately by sex for the sample of nine to 17 year-olds split by sex and social group. It is important to note that a test of whether the state and year fixed effects effectively controlling for differences across states leads the coefficient for the interval prior to implementation to fluctuate around zero, and this is clearly visible in the panels of Figure 2. There is also a clear pattern of enrollment increases among young women seen in the top two panels. These are approximately on the order of one percentage point increase in enrollment per additional year of exposure. There is also a slight upward trend among young men in the latter year ranges, suggesting that exposure to the policy may potentially affect enrollment of male enrollees in the longer term.

There are some of limitations to the event study approach. First, states have different unobserved phenomena occurring over the period of study despite parallel pre-policy trends, so there may be gains in terms of both bias reduction and increases in precision with the ability to either control for, or remove, additional unobservables. Particular to this context, the approach does not allow for the isolation of effects of women leaders for the minority group, who are subject to an additional quotas regime. In the next section, I outline an empirical approach that allows parametric separation of both additional unobserved confounders and quota regimes using the same exogenous variation in exposure.

1.5 Identifying Effects Using Border Discontinuities

I now briefly motivate an approach aimed at addressing several of the limitations of the event study discussed above. This approach will take advantage of discontinuities in the cumulative level of exposure across neighboring districts lying across state borders in order to estimate more precise long-term causal effects of the policy on enrollment rates. Consider a set of administrative districts (indexed by j) within larger geographic areas (indexed by k, not necessarily states). An equation relating policy exposure to enrollment rates in the cross-section of districts can be expressed as the following:

$$enrollment_{jk} = \delta_0 + \delta_1 exposure_{jk} + \mu_k + \epsilon_{jk} \tag{1.2}$$

Where $exposure_{jk}$ captures the cumulative years of exposure to the policy, which varies across districts j. The error term component μ_k contains unobservables common to the area, while the district-specific disturbance term ϵ_{jk} captures factors determining enrollments which may be distinct across municipalities within the area but is exogenous to the policy exposure.

The identifying assumption in equation (2) is that $exposure_{jk}$ is uncorrelated with the error term; this assumption will be violated when area-level factors such as tradition, culture, or social norms remain unobservable (i.e., when μ_k cannot be separated from ϵ_{jk}) and are correlated with policy exposure. If the variation in μ_k is comprised of unobserved determinants that do not particularly correspond to administrative boundaries, another method of separating μ_k becomes necessary. One solution is to find areas with ostensibly common characteristics but with differing levels of policy exposure such that a vector of fixed effects corresponding to μ_k would purge area-specific unobservables. In the current context, adjacent districts lying on either side of a state border fulfill such criteria.[4] The empirical

[4]Similar identification strategies have been implemented in many contexts, recently by Michalopoulos

strategy will thus exploit cross-state differences in exposure to the policy to identify causal effects using cross-sectional variation across adjacent districts lying on either side of a state border. Adjacent border district pairs then comprise the set of areas k while differences in policy exposure can be used to identify effects of the policy on educational enrollment.

Districts typically have multiple cross-border neighbors, requiring some discussion of process undertaken for assigning districts into pairs comprising the contiguous areas indexed by k. The primary estimates come from a sample of cross-border pairs constructed to ensure that each district appears in the sample only once, as part of one pair. When constructing a pair for a given district from among multiple neighbors, the assignment to a pair is made based on commonality on a set of observables from 1991 Population Census data (the rural population share, the SC/ST population share, and the rural female literacy rate). The two districts forming the "matched pair" are then included in the sample and removed from the pool of possible matches for other districts. Each unique pair is thus comprised of the two most similar districts among possible adjacency combinations. The empirical advantage of this process is that a given district can appear only once in the estimation sample as part of a unique pair, such that there is no mechanically-induced error term correlation resulting from inclusion of the same district multiple times.[5] Disadvantages to this approach are that, as with any matching procedure, the choice of matching variables is chosen by the researcher, and some border districts with only a single neighbor may never enter the sample (i.e., this process results in an imperfect bipartite matching). In practice, whether non-duplicative pair sets are constructed based on matching observables, or are randomly chosen among multiple neighbors (more discussion below) does not make a substantive difference in the pattern or magnitudes of the estimated policy effects, or conclusions therefrom. Below, I use

and Papaioannou (2014), Ghani et al. (2014), Jofre-Monseny (2012), Duranton et al. (2011), Naidu (2012), Dube et al. (2010), and earlier by Holmes (1998).

[5]A situation in which districts would appear multiple times would parallel matching "with replacement," where bootstrap adjustments for standard errors in the general context have been shown to be invalid for matching estimators by Abadie and Imbens (2008).

the covariate-matched sample to present preferred estimates, and then put these estimates in the context of a range of likely effect sizes from the empirical distribution of coefficients across randomly-generated matchings.

Sample weights are used to calculate sex- and group-specific district-level enrollment rates in $enrollment_{jk}$, and Table 3 provides summary statistics across districts for the covariate-matched sample. Appendix Table 1 provides evidence that border districts are similar to interior districts in demographics and enrollment rates both prior to the policy change in 1987 and as of 2007.[6] The identifying assumption when estimating equation (2) is given by:

$$E[\epsilon_{jk}|exposure_{jk}] = 0 \tag{1.3}$$

Violations of this assumption are likely to come in the form of either persistent unobservables in ϵ_{jk} that are not purged by area fixed effects μ_k, or concurrent phenomena affecting enrollment confounded with $exposure_{jk}$.

I first establish that the exposure measure is unrelated to observable district characteristics by regressing the exposure measure on 1991 values of the middle-school completion rates of women, the SC/ST population share, the sex ratio, and the area's urbanization level. I also include measures of exposure to a large-scale national education program and state progressive-party rule from 1992 to 2007. Using a specification analogous to equation (1), Table 4 presents the coefficients on each of these measures in explaining eventual exposure conditional on pair fixed effects. These are estimated separately in columns 1-6, and then jointly in column 7. None of the measures individually predict exposure levels (implementation timing) and the joint hypothesis test of the coefficients in column 7 yields an F-statistic of 1.04 (p-value: 0.44) providing further confirmation that the border districts used do not

[6]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.

exhibit any observable characteristics related to eventual exposure.

Table 5 contains the results of the estimation of equation (2) for non-minority group women and men in column (1) of panels A and B, respectively. Coefficient magnitudes are interpreted as the fractional increase in the enrollment rate per additional year of policy exposure. The base specification implies that an additional year of exposure to the policy increases the enrollment rate of non-SC/ST women by .7 percentage points. Pre-policy enrollment rates for this sample was less than 50 percent (Table 3) and mean exposure to the policy was 10.3 years, implying the policy 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, corroborating the event study estimates and suggesting the policy may have had effects on enrollment beyond just those on young women.

To further address identification concerns, I provide several specification variants after estimating equation (2) directly. In column 2, I include the 1987 district enrollment rate for the focal group to capture persistent differences not removed by the vector of area fixed effects, μ_k. Next, I include in the specification measures of exposure to (a) a large-scale national education program – the District Primary Education Program, or DPEP – that was implemented across districts at different times, and (b) the number of years of progressive-party rule in the state. Finally, I add to the prior specification the series of additional area characteristics to further test sensitivity of the results. None of the estimates are particularly sensitive to these controls, so for simplicity I refer to those estimates that only control for the pre-period enrollment rate as the preferred specifications (column 2). I estimate district enrollment rates by group as of 2007, with the main regressor being the years of exposure to the 73rd Amendment provisions as of 2007.

Because there were concurrent population-proportionate quotas for ethnic minorities in local government, it is necessary to separate the effects of the different spheres of reservation for those in ethnic minority groups by interacting the minority population share with the

cumulative years of exposure to the policy. In this specification, the main policy regressor can then be interpreted as the isolated effect of exposure to women leaders. Table 6 contains these estimates for the SC/ST sample, showing larger (although less precisely estimated) effects among young women (Panel A) and again among men (Panel B).

The effects of exposure to women leaders on enrollment are substantial for SC/ST children as well. The point estimate is substantially larger for young SC/ST women compared to the effect in Table 5, potentially reflecting the greater disparity in enrollment faced by SC/ST groups; in aggregate, this magnitude he the potential to explain the vast majority of the increase in female SC/ST enrollment rates, as well as the rapid closure of the gender enrollment gap, over the study period.

The primary estimates are robust to several alternative approaches. First, coefficient estimates and patterns across samples are unchanged when weighting districts by the log of population or Winsorizing the dependent variable at either one or five percent. Falsification tests based on placebo variation in state implementation timing indicate the magnitude and significance of the effects are unlikely to arise by chance, and the estimates are not sensitive to the inclusion of any particular state.[7]

1.5.1 Alternative sample construction and effect sizes

To address the loss of some border districts in constructing the matched sample and the choice of matching variables, I construct 500 matched-pair samples where in each sample, districts with multiple cross-border neighbors were randomly matched among the set of adjacencies (and then removed from the pool) to form unique pairs. Each sample then results in a set point estimates for the four groups comparable to those in column 2 of Tables 5 and 6. This approach reflects the range of potential effect sizes, as well as recognizes important researcher-driven choices in sample construction and the use of split samples for inference (Fafchamps

[7]These robustness checks are available from the author upon request.

and Labonne (2016).[8] In aggregate, this approach thus loses no information contained in border districts and does not rely on the researcher's choice of matching variables.

Table 7 contains a summary of the distribution of coefficient magnitudes estimates across the 500 samples and includes for comparison the point estimates and confidence intervals from the preferred specification in Tables 5 and 6. The repeat sample analysis confirms that the effect magnitudes and patterns across subsamples in the primary estimates in Tables 5 and 6 based on matched pairs are not particular outliers in the distribution of effect sizes across samples. The one group with a point estimate (column 1) that is substantially larger than the median coefficient and confidence interval form the multi-sample approach is that for non-minority group men, suggesting some moderation in the interpretation of the effect size for the group among the primary estimates.

1.5.2 Heterogeneity by age group and comparison with previous findings

The positive effects among young men require some discussion, particularly in relation to earlier studies that analyzed enrollment among a sample of villages in West Bengal and found no effects among young men (Beaman et al., 2009). Beyond a different geographic sample, it should first be clear that there are several important differences in the policy variation used to estimate effects on enrollment: Beaman et al. (2012a) use variation in leadership seats at the village level of the panchayat system, while the policy exposure used in the current work implicitly captures exposure to reserved seats in the non-leadership body of the panchayats (in addition to leaders), at all levels of the structure. Nevertheless, some of the difference in the results for men may be explained by sampling: when restricting the nationwide analysis

[8] In their work addressing the invalidity of the bootstrap in matching estimators, Abadie and Imbens (2008) point to the approach of Politis and Romano (1994), who suggest the construction of confidence regions based on subsamples as a valid inferential method.

above to only the 11-15 age group, effect magnitudes among young women persist, whereas for young men they disappear entirely. There is evidence of parameter hetoerogeneity by age group among both men and women, with the largest policy effect among young women roughly corresponding to the 11-15 age range, while for men there is no substantial effect among this group but is rather found in the upper secondary age range of 16 and 17 years old. Appendix Table 2 shows the results of the preferred specification estimated for a pooled sample across social groups. Panel A considers the overall enrollment rate for the 9-17 age range, While Panels B, C and D contain results from the estimation of enrollment rates for the 9-10, 11-15, and 16-17 age ranges, respectively. This suggests that while women leaders may have played a substantial role in closing the gender enrollment gap for ages 11-15, there is a strong and robust enrollment increase among men of upper secondary age. While the middle school educational enrollment gap may have closed as a result of the policy, this is less so for the enrollment gap in upper secondary school, or educational attainment overall. Thus a more nuanced finding emerges when expanding the sample to all pre-university-age adolescents.

1.5.3 Displaced activities and heterogeneous effects

This section presents analysis of the activities displaced by the increase in enrollment, and heterogeneous effects within the population. Using information on the other activities respondents may claim as their principal use of time available in the NSS data, I am able to investigate which other activities experienced a commensurate reduction among school-age children. 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 8 and 9 present estimations of the preferred spec-

ification (including only the pre-policy control) across the different activities and samples. Containing estimates for non-SC/ST women and men, Table 8 shows that those of higher social groups saw increased enrollment among individuals who would have otherwise engaged in household production or been idle; Table 9 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 and the availability of household labor remains is a persistent barrier to educational enrollment among more disadvantaged groups. This suggests that household labor may be a persistent barrier to educational enrollment among more disadvantaged groups, at least in terms of responsiveness to the focal policy (see Basu and Van (1998) for a theoretical discussion). 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.

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 10 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 householdthose 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.

1.6 Testing causal channels of school infrastructure, the labor market, and household bargaining

Various mechanisms potentially contribute to the 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., 2009). 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.

1.6.1 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, Table 11 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.[9] 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 may mechanically follow from increased enrollment, rather than precede it. Finally, among gender-related factors, there is no relationship between female-friendly facilities or higher female share of teachers and longer policy exposure. Overall, minimal evidence, if any, of an increase in educational infrastructure associated with longer exposure to the reform.[10]

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

[10]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.

1.6.2 Labor market effects and contemporaneous 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.[11] Table 12 investigates a range of labor market, marriage and household bargaining power indicators (available from the NSS data) 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 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

[11] 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, I.E. (2008) finds a pronounced effect of social security benefits on female child labor when benefits go to female household members.

household dynamics associated with longer exposure to the quota policy.[12][13]

1.7 Conclusion

This paper assesses the relationship between political inclusion and human capital investment of young constituents, finding that India's flagship political empowerment policy mandating representation quotas for women in local government increased post-primary school enrollment rates of adolescent girls and boys. 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 enacted and result in staggered implementation due to asynchronous policy implementation across jurisdictions.

In the same framework, I also examine mechanisms that could allow women leaders to affect the educational decisions of young constituents in the same framework using information on labor force activities and schooling infrastructure. I find no evidence of effects among schooling infrastructure, the labor market, or indicators of women's status in the household.

[12]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 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.

[13]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 Millett (2011) 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.

When focusing on a similarly-constructed sample, effects for young women are comparable in magnitude to earlier, geographically-focused work exploiting truly randomized exposure to seat quotas. This supports broader generalizability of the effects among middle-school age children to the entire country. Increasing the sample to all pre-university-age adolescents highlights the presence of an enrollment response among young men of upper secondary age groups. It is left for future work to determine the nature and cause of these potentially dynamic responses to quota exposure among non-beneficiary groups.

Overall, these findings can further guide policymakers as to whether or not seat quotas can reduce gender inequality in either school enrollment or educational attianment, and for which age groups, and may better inform the degree to which such a policy can be expected to have gender-specific, or broader, effects on human capital accumulation.

Table 1.1: Mean enrollment rates in rural areas, by social group, gender, age and year

	non-SC/ST						**SC/ST**					
Age group	1987	1993	1999	2004	2007	Change	1987	1993	1999	2004	2007	Change
Panel A: Male												
5 – 8	0.98	0.99	0.99	1	0.99	0.01	0.96	0.98	0.99	0.99	0.99	.03
9 – 11	0.91	0.94	0.97	0.99	0.98	0.07	0.83	0.89	0.94	0.98	0.97	.14
12 – 14	0.78	0.85	0.88	0.91	0.93	0.15	0.65	0.73	0.79	0.87	0.88	.23
15 – 17	0.5	0.55	0.61	0.63	0.69	0.19	0.39	0.41	0.48	0.51	0.55	.17
Panel B: Female												
5 – 8	0.96	0.98	0.99	1	0.99	0.03	0.94	0.94	0.98	0.99	0.97	.03
9 – 11	0.77	0.84	0.93	0.96	0.96	0.19	0.6	0.73	0.86	0.93	0.93	.33
12 – 14	0.5	0.62	0.73	0.79	0.83	0.33	0.32	0.43	0.6	0.71	0.78	.46
15 – 17	0.22	0.31	0.42	0.48	0.55	0.33	0.12	0.19	0.3	0.37	0.45	.33
Panel C: Enrollment gap												
5 – 8	-0.02	-0.02	0	0	0	0.02	-0.02	-0.04	-0.01	-0.01	-0.02	.01
9 – 11	-0.14	-0.11	-0.05	-0.03	-0.02	0.12	-0.23	-0.16	-0.09	-0.05	-0.04	.19
12 – 14	-0.28	-0.23	-0.15	-0.12	-0.1	0.18	-0.33	-0.3	-0.19	-0.16	-0.1	.23
15 – 17	-0.29	-0.24	-0.19	-0.15	-0.14	0.15	-0.27	-0.22	-0.19	-0.14	-0.1	.16

Note: Author's calculations using data from the National Sample Survey Organisation, Schedule 10: Employment/Unemployment (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 1.2: Testing for differential trends in pre-policy enrollment rates

Sample:	**Dependent variable: individual enrollment [0/1]**			
	women		**men**	
	non-SC/ST	SC/ST	non-SC/ST	SC/ST
	(1)	(2)	(3)	(4)
Year	0.01756***	0.01099***	0.02046***	0.00963***
	(0.00213)	(0.00174)	(0.00254)	(0.00202)
Year*73rd CAA implementation year	0.00071	0.00016	0.00009	0.00107**
	(0.00053)	(0.00028)	(0.00066)	(0.00044)
N	65961	22531	78718	27805
adj. R2	0.107	0.040	0.146	0.049

Note: 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 1.3: Summary statistics, cross-border estimation sample

Variable	Mean	Std. Dev.	Min	Max	N
Enrollment rate, 2007					
non-SC/ST, female	0.8	0.13	0.41	1	178
non-SC/ST, male	0.89	0.09	0.56	1	178
SC/ST, female	0.74	0.21	0.05	1	177
SC/ST, male	0.8	0.14	0.29	1	177
Enrollment rate, 1987					
non-SC/ST, female	0.45	0.2	0	0.96	174
non-SC/ST, male	0.72	0.15	0	1	174
SC/ST, female	0.29	0.22	0	1	172
SC/ST, male	0.61	0.21	0	1	173
Years Exposure to 73rd Amendment, 2007	10.73	2.67	2	14	178
Combined population of district pair, 1987	267212.5	177386	5652	1013896	174

Note: 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. Sample counts differ across measures due to missing values.

Table 1.4: District border sample: balance

	Dependent variable: cumulative policy exposure						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SC/ST population share	2.248						2.124
	(1.768)						(1.788)
Sex ratio		8.500					10.109
		(7.622)					(9.961)
Rural population share			1.498				-0.738
			(1.526)				(2.238)
Female middle school completion rate				-4.072			2.247
				(6.837)			(6.903)
DPEP Phase 1 district					0.817		0.882
					(0.661)		(0.731)
DPEP expansion district					0.301		0.441
					(0.919)		(0.803)
Years of Congress-party rule						-0.095	-0.095
						(0.114)	(0.112)
F-statistic, joint hypotheses test					0.76		1.04
N	178	178	178	178	178	178	178
adj. R2	0.546	0.521	0.518	0.518	0.524	0.524	0.565

Note: Table presents a falsification test predicting eventual policy exposure with pre-policy area characteristics. Estimates via OLS. Sample comprised of 178 covariate-matched border districts as described in text. All specifications include an unreported constant term and district pair fixed effects. Significance indicated by: $*p < .1, **p < .05, ***p < .01$.

Table 1.5: Estimation of school enrollment rates: non SC/ST

	Dependent variable: district enrollment rate			
Specification:	Base	Incl. 1987 control	Incl. policy controls	Policy controls * covariates
	(1)	(2)	(3)	(4)
Panel A: non-SC/ST women				
Exposure, 2007 (years)	0.008**	0.007*	0.008**	0.007*
	(0.004)	(0.004)	(0.003)	(0.003)
Mean outcome			0.803	
Std. Dev. Outcome			0.126	
N			178	
Pre-policy control	N	Y	Y	Y
adj. R2	0.412	0.416	0.429	0.448
Panel B: non-SC/ST men				
Exposure, 2007 (years)	0.006**	0.006*	0.006**	0.006*
	(0.003)	(0.003)	(0.003)	(0.003)
Mean outcome			0.889	
Std. Dev. Outcome			0.089	
N			178	
Pre-policy control	N	Y	Y	Y
adj. R2	0.358	0.356	0.352	0.346

Note: Table presents coefficients from an unweighted linear regression of the district enrollment rate in 2007 on the 2007 cumulative policy exposure. Underlying sample is comprised of 9-17 year-olds in rural areas in districts on state borders. Enrollment rates are calculated using sample weights. Pre-policy control is defined as the enrollment rate for the focal group in the district in 1987. "Concurrent policy controls" include years exposure to DPEP program and progressive party rule. Heteroskedasticity-consistent robust standard errors clustered by state 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 1.6: Estimation of school enrollment rates: SC/ST

	Dependent variable: district enrollment rate			
Specification:	Base	Incl. 1987 control	Incl. policy controls	Policy controls * covariates
	(1)	(2)	(3)	(4)
Panel A: SC/ST women				
Exposure, 2007 (years)	0.043***	0.045***	0.046***	0.041***
	(0.010)	(0.010)	(0.010)	(0.011)
Mean outcome			0.737	
Std. Dev. Outcome			0.211	
N			178	
Pre-policy control	N	Y	Y	Y
adj. R2	0.338	0.351	0.334	0.346
Panel B: SC/ST men				
Exposure, 2007 (years)	0.022*	0.025**	0.025**	0.021**
	(0.012)	(0.011)	(0.010)	(0.008)
Mean outcome			0.801	
Std. Dev. Outcome			0.143	
N			178	
Pre-policy control	N	Y	Y	Y
adj. R2	0.313	0.307	0.363	0.369

Note: Table presents coefficients from an unweighted linear regression of the district enrollment rate in 2007 on the 2007 cumulative policy exposure. Underlying sample is comprised of 9-17 year-olds in rural areas in districts on state borders. Enrollment rates are calculated using sample weights. Pre-policy control is defined as the enrollment rate for the focal group in the district in 1987. "Concurrent policy controls" include years exposure to DPEP program and progressive party rule. Heteroskedasticity-consistent robust standard errors clustered by state 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 1.7: Effect sizes across 500 matchings

	Covariate-matched sample (Tables 5 & 6)		**Coefficient distribution 500 matched samples**	
	Point estimate	95% confidence interval	Median point estimate	95% empirical confidence interval
	(1)	(2)	(3)	(4)
non-SC/ST women	0.008**	[0.0002, 0.0151]	0.0064611	[0.0008, 0.0128]
non-SC/ST men	0.006**	[0.0005, 0.0114]	0.0023815	[-0.0032, 0.0082]
SC/ST women	0.043***	[0.0221, 0.0645]	0.0422824	[0.0073, 0.0892]
SC/ST men	0.022*	[-0.0034, 0.0474]	0.0288732	[-0.0056, 0.0520]

Note: Columns 1 and 2 above reproduce coefficients and 95% confidence intervals from column 2 of Tables 5 and 6. Columns 3 and 4 present the median point estimate from the empirical coefficient distribution across 500 border district matchings and the corresponding empirical 95% confidence interval. Significance of point estimates reproduced in column 1 indicated by: $*p < .1, **p < .05, ***p < .01$.

Table 1.8: Cumulative effect of reservation on alternative time use outcomes: non-SC/ST

	cross-state border district variation				
	Dependent variable: district mean share of 9-17 year-olds engaged in activity				
	In labor force	LF: Work in home enterprise	LF: Paid wage work	LF: Looking for work	Household duties
	(1)	(2)	(3)	(4)	(5)
Panel A: non-SC/ST women					
Exposure, 2007 (years)	-0.0027	-0.0032*	0.0006*	-0.0014*	-0.0046
	(0.0018)	(0.0016)	(0.0004)	(0.0008)	(0.0033)
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. R2	0.400	0.417	0.140	0.065	0.409
Panel B: non-SC/ST men					
					0
Exposure, 2007 (years)	-0.0063***	-0.0050**	-0.0006**	-0.0008	0.0003
	(0.0021)	(0.0021)	(0.0003)	(0.0012)	(0.0005)
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	0.055	0.469

Note: Table presents coefficients from an unweighted linear regression of the 2007 share of school-age children of the focal sample engaging in the activity indicated by column headers on the 2007 policy exposure measure among neighboring districts lying across state borders. All measures calculated using sample weights. "LF" indicates traditionally-defined labor force activities. 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 1.9: Cumulative effect of reservation on alternative time use outcomes: SC/ST

	cross-state border district variation				
	Dependent variable: district mean share of 9-17 year-olds engaged in activity				
	In labor force	LF: Work in home enterprise	LF: Paid wage work	LF: Looking for work	Household duties
	(1)	(2)	(3)	(4)	(5)
Panel A: SC/ST women					
Exposure, 2007 (years)	-0.0041	-0.0017	-0.0015	-0.0002	-0.0379***
	(0.0045)	(0.0043)	(0.0011)	(0.0019)	(0.0077)
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)	-0.0156**	-0.0060	-0.0031	-0.0114**	-0.0112***
	(0.0071)	(0.0068)	(0.0022)	(0.0046)	(0.0022)
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. R2	0.213	0.226	-0.232	0.005	0.471

Note: Table presents coefficients from an unweighted linear regression of the 2007 share of school-age children of the focal sample engaging in the activity indicated by column headers on the 2007 policy exposure measure among neighboring districts lying across state borders. All measures calculated using sample weights. "LF" indicates traditionally-defined labor force activities. 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 1.10: Parameter heterogeneity in enrollment effects

	Dependent variable: district enrollment rate							
	Age			**Income tercile**			**Highest F. educ. Level**	
	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. R2	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. R2	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. R2	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. R2	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

Note: 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 indicated 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 1.11: Differences in schooling infrastructure and schooling environment

	Dependent variable: district schooling infrastructure indicator						
	Schools per person	Schools per person (rural)	New schools per thousand	Share classrooms "good" condition	Teachers per pupil	Share schools w/ 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. R2	0.435	0.500	0.347	0.512	0.404	0.480	0.588

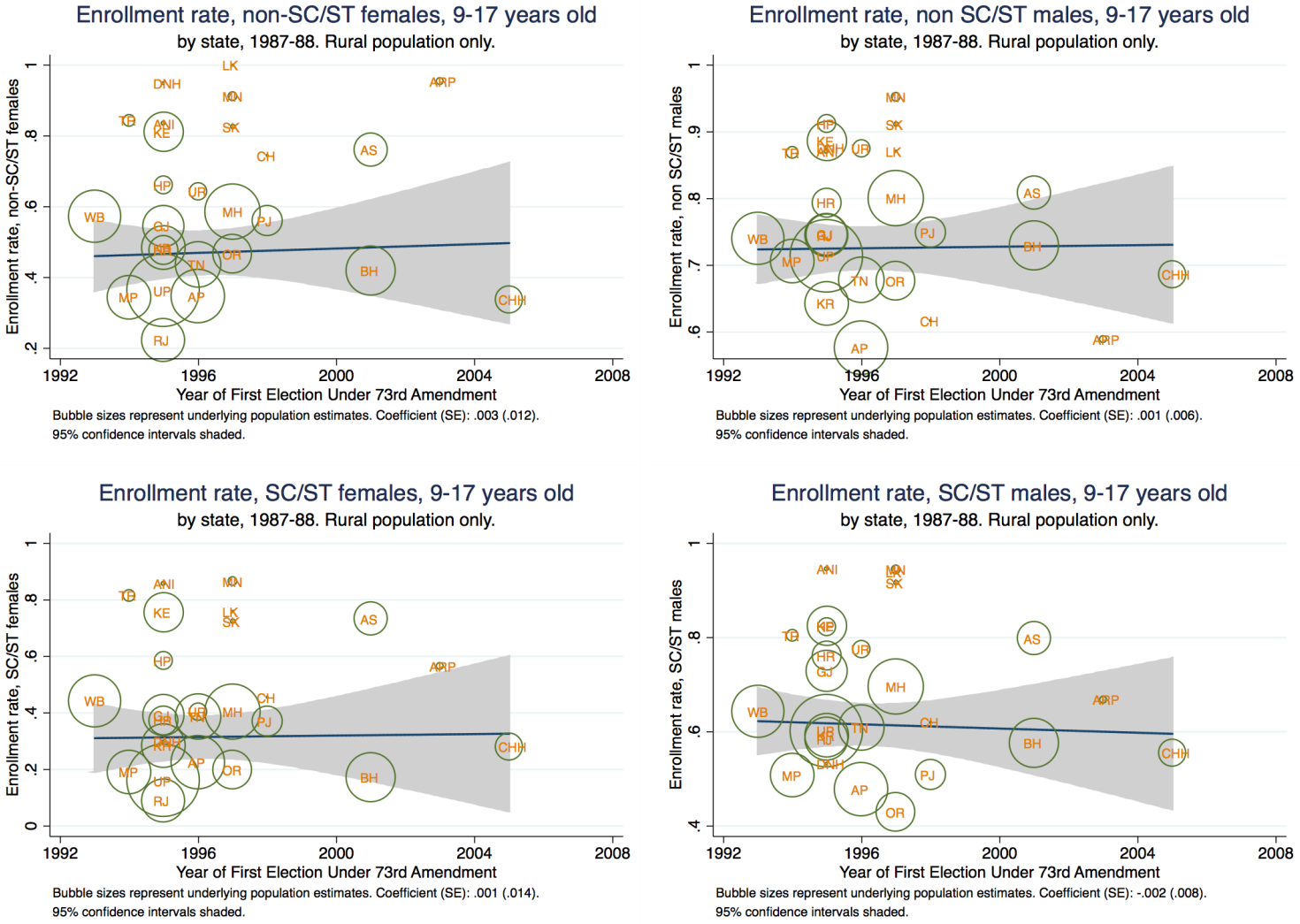
Note: Notes: Table presents coefficients from an unweighted linear regression of the district schooling infrastructure measure (indicated by column) in 2007 on the 2007 policy exposure measure among neighboring districts lying across state borders. Heteroskedasticity-consistent robust standard errors 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 1.12: Cumulative effect of quota exposure on alternative outcomes

	Dependent variable: standardized district indicator mean						
	Labor force participation	share married	number of children	age at first birth	husband's ed. (years)	husb. ed* *own ed.	husband's wage
	(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. R2	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. R2	0.118	0.147	0.331	-0.199	0.351	0.168	0.421

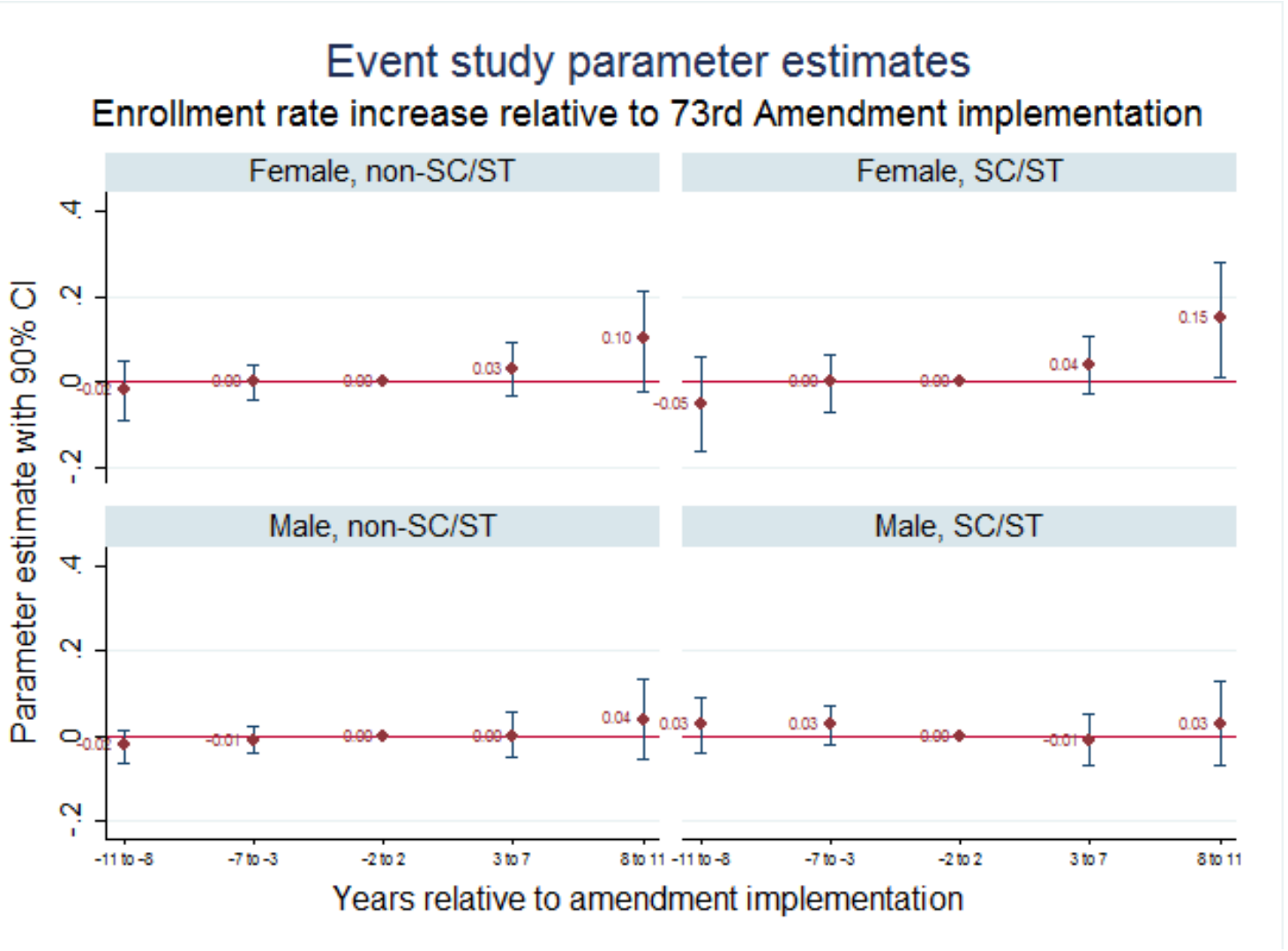
Note: Table presents coefficients from an unweighted linear regression of the unit-standardized 2007 district-level mean of the measure indicated by column headers on the 2007 policy exposure measure among neighboring districts lying across state borders. Underlying sample is comprised of 25-35 year-old women in rural areas. 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: $*p < .1, **p < .05, ***p < .01$.

Figure 1.1: Pre-policy enrollment rates, by group



Notes: Figures depict pre-policy enrollment rate for the focal group relative to eventual policy implementation year. Source: Author's calculations based on GOI (2008) and data from NSS Schedule 10, 1987-88.

Figure 1.2: Event study estimates, by group



Notes: Figure depicts coefficient estimates with 90% confidence interval from event study specification as described in Section III.

Appendix Table A1: Comparison of state border and interior district enrollment rates

	Interior	State border	Border - Interior	p-value	N
Panel A: 1987					
enrollment, non-SC/ST female	0.468	0.477	0.009	0.71	374
enrollment, non-SC/ST male	0.737	0.722	-0.015	0.37	374
enrollment, SC/ST female	0.335	0.333	-0.002	0.95	368
enrollment, 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					
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

Note: 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 A2: Effect sizes across age samples

Dependent variable: district enrollment rate		
	Sample:	
	Women	Men
	(1)	(2)
Panel A: Pooled sample		
Exposure, 2007 (years)	0.01390***	0.00854***
	(0.00442)	(0.00212)
Panel B: 9-10 year-olds		
Exposure, 2007 (years)	0.00107	-0.00035
	(0.00402)	(0.00127)
Panel B: 11-15 year-olds		
Exposure, 2007 (years)	0.01835***	0.00327
	(0.00613)	(0.00231)
Panel B: 16-17 year-olds		
Exposure, 2007 (years)	0.00357	0.03163***
	(0.01599)	(0.00889)

Note: Table presents pooled-sample coefficients from an unweighted linear regression of the district enrollment rate in 2007 on the 2007 cumulative policy exposure. Panel A estimates the net enrollment rate for entire 9-17 year-old age sample. Panels B, C and D estimate enrollment rates for the indicated age group. The underlying sample is comprised of children in rural areas and 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, the enrollment rate for the focal group in the district in 1987, district pair fixed effects, an indicator for SC/ST status, the rural SC/ST population share, the rural SC/ST population share interacted with SC/ST status, and this measure interacted with policy exposure. Significance indicated by: $*p < .1, **p < .05, ***p < .01$.

Chapter 2

Quota policies and upward mobility

I

2.1 Introduction

Increasing the economic and social participation of historically-underrepresented groups is a popular policy goal. Quotas are a common tool to achieve these goals, and are applied to various spheres such as education, business and politics.[1] Ideally, these policies render themselves obsolete over time by changing aspects of the environment in which they operate that originally gave rise to the targeted disparities in participation. Whether institutional change is possible has long been debated in the many contexts in which such policies have been proposed or advocated (Coate and Loury, 1993).

Typically due to institutional or political factors, quotas are most often targeted at a specific level of a broader structure, rather than uniformly in an entire system or organization. In governance, quotas are commonly applied to national legislatures, but not in sub-national governance, or vice versa. Among firms, quotas can be seen applied to various strata but

[1] In a common incarnation of such efforts, more than 90 countries have mandated seat or candidate quotas in elected bodies as of 2015 (The Quota Project, 2015).

rarely throughout the entire organization, as in the case of corporate board quotas in Norway or Germany. In education, for example, quotas are often found at the tertiary level, as in Indian and Brazilian public universities. Often implicit is the argument that the application of quotas to one environment can affect representation in related areas over time. The question motivating this paper is: *how do quotas applied to a specific strata of an organization or structure affect participation and representation in adjacent levels of the same structure that are not subject to quotas?* To answer this, I use the Indian experience with quotas for women in local politics to investigate effects on both participation and representation of women in higher levels of government, where quotas do not exist.

There are several technical challenges to answering this question which jointly motivate the use of the Indian experience with quotas in politics as an ideal environment to study longer-term effects across strata of the governance hierarchy. The first is that effects in adjacent environments are likely to appear only after some time due to the nature of occupational experience that is a first-order channel through which such policies can affect later participation; this requires the ability to observe a policy and participation over a substantial period in order to detect long-term effects. Second, and in view of the common applications of quotas to the private sector, a substantial difficulty is that dynamics of competition and upward mobility are often difficult to observe without extensive or proprietary data sources (requiring longitudinal employee-employer data, in the case of the private sector); even when such data sources are available, however, the outcomes available (e.g., promotion) may only provide a partial picture of the dynamics within the organization given that failed (promotion) contests are rarely directly observable. The public nature of election records overcomes these initial typical limitations. Finally, such records are rarely maintained in a manner that allows viable observation of individual candidates over time and at different levels of government, making individual-level panel data on repeat candidacies by politicians practically non-existent – especially for developing countries. By constructing such a dataset

and perfectly observing exogenous geographic exposure to seat quotas in local government over a period of up to 15 years, the Indian experience in politics provides an opportunity to overcome typical obstacles in approaching these questions empirically.

Using randomly-assigned variation in exposure to women leaders across constituencies, I show that longer exposure to seat quotas in local leadership roles increases female political candidacy in higher office among state and national legislatures – where quotas do not exist. State legislature constituencies that experienced ten additional years of exposure to women leaders in place through the quota policy fielded .15 more female candidates than constituencies with zero exposure; parliamentary constituencies fielded .41 more female candidates. The magnitude of these effects is substantive, as they imply that the local quota policy was responsible for around 50 percent of the increase in the number of female candidates running for office in higher levels of government since the policy began. There was no effect on female candidates *winning* elections, however. These findings are robust across various specifications and alternative estimators, and effects are not correlated with heterogeneity in constituency characteristics.

I then investigate hypotheses related to the reasons why these higher levels of government saw increased candidacy. By observing political career histories of candidates, I find the policy effect is driven by two mutually exclusive groups of candidates. Approximately half of the increase in female parliamentary candidates is due to candidates who had previously contested a state assembly election and continued contesting for higher office in areas that received longer quota exposure. Another portion of the candidacy response is attributable to politicians who gained direct experience from the quota system, having previously served in local government. Together, these findings suggest a combination of direct effects via cohorts of female politicians who contest elections at higher levels of government after gaining specific political experience provided them by the quota system, and indirect effects making certain areas more conducive to female candidacy. While prior studies, notably Beaman et al. (2009),

have established that voter attitudes change after exposure to women leaders, this paper is the first to provide evidence of a response among potential candidates to voters' exposure. Overall, this is the first paper to show specific supply-side effects of quotas on later, higher level candidacy alongside suggestive evidence of a response among potential candidates to changed voter demand.

Finally, I investigate potential reasons why there is no demonstrated effect on these female candidates winning the elections they contest. Overall, female candidates perform approximately as well as the median candidate in the elections they contest. I also find that most of the new candidates run as independents, rather then as part of major or minor parties, suggesting a potential lack of access to resources for campaigns at higher levels of government. Finally, I show no effects among voter turnout, disqualifying the hypothesis that candidacy increases arise indirectly due to increased (female) voter mobilization by local female politicians.

This paper is among the first to quantify the degree to which quotas at a lower strata of an organizational hierarchy can affect dynamics at higher levels of the same structure. In India, a one-third quota policy for women in local politics has increased the substantive representation of women in local government, and several studies have shown this quota system to have beneficial effects on constituents beyond those changes directly linked to governance.[2] The relative success of these quotas for women in local government, however, has not yet translated into an increase in representation by women in higher levels of government, where quotas do not exist: the share of female parliamentarians in the directly-elected lower house of parliament has remained around ten percent since the mid-1980s, and there is a similar stability in these figures among state legislatures. Overall, little is known about whether the quotas affect the broader dynamics of the political system in which they operate, and if so,

[2]These effects include, for example, public goods provision (Chattopadhyay and Duflo, 2004a) as well as longer-run investments attributable to changes in aspirations (Beaman et al., 2012b).

whether such changes are temporary or persistent in nature.

I contribute to the literature in a number of areas. This paper is most directly related to earlier studies on quotas in politics in developing countries, a number of which have investigated the Indian experience with seat reservations in politics and their varied effects among both government and constituents. These studies primarily assess immediate effects on policy outcomes or changes outside the political system: Chattopadhyay and Duflo (2004a) find differences in public good provision, while Beaman et al. (2012b) find investments among young women attributable to changes in aspirations. Jensenius (2015), however, finds no effect of caste-based reservations on development outcomes in India over a period of 30 years that these quotas were active. In terms of institutional change, Iyer et al. (2012b) find evidence that political empowerment resulted in greater reporting of crimes against women, and Brollo and Troiano (2015) find evidence of reduced political patronage by women mayors in Brazil which ultimately affects their reelection probabilities (negatively). On the ability of leaders to change attitudes and perceptions, 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 in Hoff and Stiglitz (2010). Conceptually similar to this body of work, I also look at effects of quotas outside the particular environment to which they are applied; in contrast, however, I investigate effects in other areas of the political system rather than outside politics.

This investigation of the effects of exposure to women leaders in politics on future candidacy is also related to earlier work that investigates legacy effects of female political participation on voters and candidates. While distinct legacy effects have been found in a number of contexts for local politics, evidence is mixed at higher levels of government.[3] Bhalotra et al. (2015) find positive effects of a woman winning an election on subsequent female candidacy

[3]Studies of the legacy effects of female politicians on future candidacies in local politics include Nagarajan et al. (2011) for India, and Gilardi (2014) for Switzerland.

in state legislatures in India (primarily driven by the same candidates contesting again), while Broockman (2014) finds no effect among U.S. state legislature elections. Perhaps most closely related is Bhavnani (2009), who uses randomly assigned municipal council elections to determine the effects of seat reservation in municipal government in India on later candidacy in the same municipal councils to find that quotas served to introduce women into politics who would later run, and to demonstrate more broadly that women could be viable candidates. In the current work, I complement this literature by investigating effects of female politicians across levels of government, rather than within the same legislative body.

The paper is also closely related to empirical investigations of the effect of affirmative action and quotas in the commercial sector, notably Coate and Loury (1993) and, more recently, Bertrand et al. (2014). Bertrand et al. (2014) study an instance of the European experience with gender quotas in Norway and find that a mandated change in the gender composition of the upper echelons of firm governance yielded no effects on women outside these few positions in the same firm. As mentioned before, the current study evaluates the effect of quotas which plausibly operate in the opposite direction through the creation of a supply of experienced female candidates. Finally, the paper is related to studies on gender differences in risk, competition, and tournament entry. A detailed and recent review of this literature can be found in Niederle (2014).

2.2 History of seat quotas in local government

Seat quotas for women in government were introduced in India at the national level with the 73rd Amendment 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. The 73rd Constitutional Amendment Act instituted a three-tiered system of local government within states, consisting of the village,

sub-district (block), and district levels across rural areas of the country. The 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. These policies and their implementation has been the focus of a number of studies to date; further details can be found in Chattopadhyay and Duflo (2004a), Chattopadhyay and Duflo (2004b), and Iyer et al. (2012b).

There are a few particularly salient aspects of the policy implementation worth discussing. The first is that the 73rd Amendment stipulated that states had the responsibility to adjust or amend local elections to comply with the provisions of the Amendments. All states amended existing laws or passed new laws to incorporate provisions of the amendment, and compliant elections were eventually held in nearly all jurisdictions. However, and as first pointed out by Iyer et al. (2012b), there is considerable variation in the timing of these first elections across states. Second, the quota policy was applicable only to local governance, importantly excluding state and national legislatures from having quotas for women. Finally, once the provisions of the reform were implemented, one third of seats were reserved for women at any level of the local governance hierarchy; for single-seat leadership positions, reservations were assigned randomly across areas in each election cycle such that in aggregate, the one-third quota would be met. This feature of rotating leadership assignment has been used to assess the effects of women leaders in previous studies, including Beaman et al. (2012b) and Iyer et al. (2012b), among others. After several election cycles with the random assignment, there is considerable variation across areas in the *cumulative* number of years exposed to a woman in the leadership position; it is this variation in cumulative exposure to quotas applied to district leadership seats – at the highest level of this governance structure – that provides exogenous cross-sectional variation in exposure to women leaders used to identify dynamic

cross-level effects in the political hierarchy.

2.3 Channels for quotas to affect candidacy

There are two channels by which exposure to quotas might increase candidacy of women. A direct, supply-side channel provides a cohort of particular women experience in the political arena in local government, and some portion of these same women continue in a political career by contesting for higher office. The single district-level leadership seats are particularly germane to focus on as potential conduits for individual politicians to translate experience into candidacy for higher office: the district constituencies served by the chairperson consisted of areas (populations) that are larger than state assembly constituencies and, on average, of a similar size to parliamentary constituencies. The power accorded these leadership positions is thus substantial, relative to those at lower levels of the local government, and has the potential to accord both experience and exposure to the public at a level necessary for politicians to consider candidacy at higher levels of government.

The other channel, evidence for which has been established by the work of Beaman et al. (2009) and Bhavnani (2009) in similar contexts, suggests a demand-side mechanism in which constituents' exposure to women leaders changes voter attitudes and thus the viability of female candidates contesting elections in those constituencies. I augment typical elections records by constructing a longitudinal candidate panel dataset that allows the observation of candidacies by the same individual over time in order to disentangle these channels. Support for the candidate supply channel would be evidenced by the net policy effect being traced to individuals who served in local government due to quotas. While direct evidence for the demand-side effect would require voter data, response to changes in voter attitudes may be evidenced by candidacy response among women who were not previously involved in local government. In the analysis to follow, I show that both are present and contribute

approximately equally to the net candidacy response to quota exposure.

2.4 Data

2.4.1 Exposure to female leaders

Measures of exposure to seat quotas over time is available from Iyer et al. (2012b), who digitized archival data from various sources for ten states' full history of district-level seat reservations since implementation of the 73rd Amendment.[4] Using these data, Figure 1 shows the variation in cumulative exposure to district chairperson reservations as of 2007 for the states where this information is available, with those districts receiving more (less) exposure to women leaders more (less) heavily shaded.

Table 1 contains summary statistics of the cross-district variation in cumulative exposure to district chairperson reservations as of 2007 by state (as shown in Figure 1). The mean level of exposure across states is relatively similar, but there is substantial within-state variation in years of exposure, reflecting the nature of the chairperson reservation assignments. Note, however, that some states (for example, Haryana and Rajasthan) have little variation in exposure across districts while other states (Gujarat, Andhra Pradesh, and West Bengal, for example) have exposure ranging from zero to 10 or 11 years.

Figure 2 depicts this same variation across states and districts. In this figure, each position on the vertical axis represents a separate district, with districts in the same states grouped together. For each district, the appearance of a filled line indicates that the district chairperson seat was reserved for a woman in that year (indicated on the horizontal axis). Districts that did not receive any period of reservation are indicated in grey within each state

[4]Comprehensive information on seat reservations is not readily accessible from any public sources or records. For their study, the authors of Iyer et al. (2012b) collected data from diverse sources, including filing Right to Information Act requests for the history of district leader reservation assignments since the implementation of the 73rd Amendment. Some states did not respond to these requests, resulting in the ten-state sample shown in Figure 1.

series. A few important patterns are worth highlighting. First, assignment of chairperson seat reservation is not perfectly rotated: at the end of each election cycle, some districts continue being reserved while others switch status. Second, a number of districts received no reservation at all; these are present in seven out of ten states. Third, Maharashtra followed a three year election/reservation cycle, while all other states followed a five year cycle. Additionally, there are two dimensions of exogenous variation arising after several election cycles: in cumulative exposure and in relative recency of exposure. In the analysis that follows, the variation in cumulative exposure will be used to identify net policy effects, while variation in recency will be used to explore secondary hypothesis related to the mechanisms by which quotas affect political participation.

2.4.2 Elections data

Digitized historical state assembly election data come from Jensenius (2013), and include vote counts and basic candidate information for candidates contesting state legislature elections through 2007. Due to elections being held in different years across states, the sample is selected to the most recent election prior to and including calendar year 2007 for each state for the state assembly analysis, and these records are then associated with cumulative policy exposure as of the election year.[5] Data from parliamentary elections comes from the Election Commission of India and contains the details of all candidates across all constituencies of the directly-elected lower house of parliament (the Lok Sabha). The candidate data for both assemblies and parliament are reported with a number of fields, most importantly candidate names, gender, vote shares, and constituency voter turnout.

The candidate lists provide the potential to observe candidacy by the same individual across elections, which is used below to distinguish candidacy effects by candidates' carrer

[5]Appendix Table 1 shows the years in which the last assembly election was held prior to and including 2007.

histories. Creating a viable candidate panel is not straightforward, however, due to the fact there is no unique identifier field associated with candidates and a given individual's name may not be spelled or registered the same way across elections for any number of reasons (including differences in transliteration, honorifics, or name abbreviations, among others). In order to match candidates across elections, I apply an approximate string matching algorithm which searches for each candidate name in a given state and election with potential name matches in the following election cycle in the same state and with the same gender. Potential matches are then selected among names that are within a given string distance and are reviewed manually and a final match decision is made. Summary statistics can be found in Appendix Table 3 and the Appendix contains a full description of this process.

2.4.3 Matching districts and political constituencies

Parliamentary and assembly constituencies comprise the focal units of analysis given that elections are conducted according to political boundaries; district boundaries, which determine exposure to chairperson seat reservations, do not overlap with parliamentary or assembly constituencies (see Appendix Figure 1). To calculate an accurate measure of a constituency's exposure to female district leaders, I use GIS software to identify the intersection of the two sets of geographic areas in order to assign the respective portions of each district to its parliamentary constituency; this process then creates a set of unique geographic areas defined by the area pertaining to a unique combination of a district and constituency; summary statistics are presented in Appendix Tables 4 and 5.[6] Table 2 provides summary statistics for these parliamentary and state assembly geographies.

[6]It is important to note that this intersection process results in the basic geographic units that are the foundation of the empirical analysis. If districts are indexed by d and constituencies (either parliamentary or assembly) by c, any given unit thus has a particular definition as the area that comprises the unique intersection of district d and constituency c; the area defined by d, c, in isolation, does not correspond to any existing administrative or political unit.

2.5 Analysis

2.5.1 Identification

Causal identification hinges on the exogeneity of the reservations assignment such that for the estimating equation

$$Y_{s,d,c} = \delta_0 + \delta_1[Exposure_d] + \gamma_s + \mu_{s,d,c} \tag{2.1}$$

where $Y_{s,d,c}$ is the outcome for state s in the unique area comprised of the overlapping area of district d and constituency c, $Exposure_d$ measures the cumulative number of years that the district leadership seat was reserved for a woman as of 2007, and γ represents a vector of state fixed effects, the residual is orthogonal to exposure, conditional on γ, i.e., $cov(\mu_{s,d,c}, Exposure_d|\gamma_s) = 0$.

Identifying variation arises from two aspects of the policy. First, some areas were reserved multiple times over the span of study; second, some areas had not yet completed their reserved term of office as of the focal assembly or parliamentary election simply because they were assigned reservation in a later election cycle than other areas. The following section provides evidence that this variation is not correlated with observable characteristics of constituencies; that is, there is substantial evidence supporting the proper implementation of the random assignment of the leadership seat reservations across constituencies and election cycles.

2.5.2 Falsification tests for selection on observables

Figure 3 provides visual evidence of exogeneity in cumulative exposure to female leaders in district chairperson seats by plotting an area's eventual exposure level against its pre-policy degree of female political participation (measured as the share of female candidates in

the election prior to the policy implementation). In the top panel, I show this relationship for state assembly constituencies; the left-hand graph shows the unconditional relationship, and the right-hand graph shows the relationship after removing state fixed effects (i.e., the within-state variation shown in Figure 1 that will be used to identify effects in the empirical strategy). The bottom panel follows a similar pattern for parliamentary elections; in both cases, the flat line suggests no strong relationship between ultimate exposure and preexisting differences in the local propensity for women to be involved in politics.

For the analysis that follows, I focus on the relationship between policy exposure and constituency-level measures of female and male candidacy. To formalize the above falsification test, I collect a set of pre-policy measures of district socioeconomic characteristics from various sources and use them to predict the election outcomes that are the focus of analysis; this predicted outcome is then regressed on the policy measure to test whether this aggregate of pre-policy local characteristics encapsulated in the predicted outcome can be explained by later policy variation. These characteristics are taken from the 1991 population census, India's standard household labor force survey carried out by the National Sample Survey Organization in 1987-88, and elections records for the 1991 general election. The indicators include demographic characteristics (average household size, sex ratios, ethnic mix), school enrollment rates, female literacy rates, household consumption per capita, and the same pre-period measures of female political participation.

Table 3 presents these estimations. For both state and national legislatures, the policy measure has trivial explanatory power for the predicted outcomes. Another test lending support to exogeneity in the policy measure across area observables is a direct test of the degree to which pre-policy area characteristics can predict the policy measure; in Appendix Table 2, I show that an F-test of the vector of coefficients on the pre-policy area characteristics fails to reject the null hypothesis at any traditional level of significance. Taken together, these tests suggest strong within-state balance in pre-policy demographic, social,

and political characteristics across districts receiving different eventual exposure intensities.

2.5.3 Estimation

Because seat reservation is assigned randomly in each election cycle, cumulative exposure is uncorrelated with confounding unobservables as shown in the previous section. The empirical strategy is thus straightforward: I regress a set of focal metrics of political competition across constituencies for the focal elections on the contemporary cumulative exposure to female district leaders. Regressions are weighted by the population share in the constituency based on data from the Gridded Population of the World (CIESIN, 2005). By construction, the error term is correlated by district (due to the policy variation) and constituency (the units for which elections outcomes are observable), so standard errors are thus two-way clustered by district and constituency.

Table 4 shows point estimates for the number of female and male candidates when separately regressed on cumulative years of chairperson reservations for the set of most recent state assembly elections across states prior to 2009 using both OLS and count data (Poisson) estimators. The first column of Table 4 shows that an additional year of women in leadership positions increases the number of female candidates by a small, but statistically distinguishable .015. That is, for a constituency that experienced two election cycles (ten years) of reservations relative to a constituency receiving no exposure, the former would expect to see .15 (or approximately .22 standard deviations) more female candidates. This is not seen alongside an increase in the number of male candidates (column 2). The pattern and significance of the OLS estimates are mirrored in count data models in columns 3 and 4 using a Poisson estimator.

Table 5 shows coefficients for the same measures based on parliamentary elections in 2009. Coefficient patterns are largely similar to effects among assembly elections, although the candidacy effect is substantially larger: for an additional ten years of exposure to female

district leaders there is an additional .41 female candidates (or approximately .4 standard deviations). At the mean level of exposure, almost one in six districts fielded an additional female candidate (0.041*3.4 = .14 additional female candidates per constituency). There is again no evidence of a response among male candidates in either direction. In both tables, Panel B shows that results are similar in pattern and magnitude when controlling for pre-policy factors, including female candidate shares and local demographic characteristics.

The directional correspondence in effects of exposure to women leaders across two levels of government confirms that district leadership seats are a potentially pivotal role in according experience that is sufficient to enhance the (possibly self-assessed) viability of a candidate in other elected bodies. That the effect is stronger at the parliamentary level also suggests that the state legislature is not a necessary intermediate step for career politicians from reserved areas – potentially explained by comparability in constituency sizes across districts and parliamentary constituencies relative to state assembly constituencies.

The estimated magnitudes of the policy effect are substantial in view of the total increase in the number of female candidates for office in these elections. In the 1991 parliamentary elections there were 157 female candidates, compared to 265 in the 2009 elections, for the states in the sample. With an average of 3.4 years of exposure to the policy across 278 constituencies, the point estimate of .041 implies an additional 38.75 female candidates as a result of the policy, comprising approximately 35 percent of the increase the female candidacy in these states seen over this period. In state legislatures, the policy effect is smaller in magnitude, but is similarly responsible for a large share of the increase in female candidates: with an average exposure of 2.8 years across 1,615 constituencies, the point estimate of .015 implies an additional 67.8 female candidates running for state legislatures – the majority of the increase from 705 to 802 female assembly candidates in these states from before the policy to the most recent election.

2.6 Robustness and effect heterogeneity

2.6.1 Non-linearities and moderators

Table 6 interacts the exposure measure with various local socioeconomic characteristics to explore whether the overall effects on candidacy are concentrated in areas with any particular characteristics. Column 1 repeats the unconditional OLS specifications from Tables 4 and 5 in Panels A and B, respectively. Column 2 reports results from a specification including the square of the exposure measure, while columns 3-6 interact the exposure measure with rural female literacy rates and educational attainment, the population sex ratio, and the population share of lower-caste groups. Overall the estimates of the main effect of exposure are remarkably robust across these specifications, with the exception of the polynomial specification (column 2) in which effects are imprecisely estimated for parliamentary elections, and the squared term becoming more prominent among the state assembly elections.

2.6.2 Sample construction and composition

An alternative to the current sample construction method would be to calculate the weighted average policy exposure for each constituency based on the various district components comprising the constituency. This also obviates the need for multi-way clustering of standard errors. Appendix Table 7 shows these estimates for both the state assemby and parliamentary elections, with results mirroring (or stronger) than those in Tables 4 and 5. Results are also robust to weighting observations by constituency area share instead of population, as seen in Appendix Table 8.

Two concerns remain regarding the sample construction. The first is technical, and arises from the GIS intersection process used to find overlapping areas between districts and constituencies. In this process, even small deviations in shapefile polygon borders from a "true"

common border between a district and a constituency will result in a large number of miniscule areas in the resulting intersected dataset; some of the areas will represent a true overlap, while some will represent erroneous overlaps into adjacent areas. These latter cases naturally introduce classical measurement error, although in weighted regressions these areas are less likely to be influential and their removal should have minimal or slighly positive effects on coefficient magnitude. The second remaining concern is the use of multi-district constituencies at all. Appendix Figure 2 addresses both of these concerns by showing coefficient magnitudes and t-statistics for area samples that begin by removing the small areas from the sample comprising less then one percent of the overall constituency area and progressively increasing that cutoff up to areas that comprise 80 percent or greater of total constituency area, leaving a sample comprised of constituencies that cleanly overlap with a district that comprises 80 percent or more of its area. This exercise makes two points: first, removal of the smallest areas has the expected effect on coefficient magnitudes (a slight increase, if any), and second, even when restricting to more cleanly overlapping district-constituency pairs, coefficient magnitudes and significance are similar (if not greater) than in the full sample.

2.7 Causal channels for the candidacy effect

2.7.1 Prior exposure to politics

The results above suggest that exposure to female leaders via quotas at a lower level of the political hierarchy increases the political candidacy decisions of women running for higher office. To better understand the ways in which the policy affects the candidacy margin, I investigate personal characteristics of the candidates who are revealed to be moved by the policy. To do this, I first use the panel nature of the candidate dataset to incorporate historical information on candidacy and public service to determine if the effects in Table 5

are particularly concentrated among either repeat candidates or incumbents.

I segment candidates by various measures of prior political experience and exposure, with the focal outcomes now defined as the number of female candidates who are also (a) candidates with prior exposure in local government, (b) repeat (parliamentary) candidates, (c) previous candidates for lower office, and (d) candidates with known family involvement in politics (Labonne et al., 2015). Table 7 presents point estimates when estimating the same crosssectional specification as above.

By observing candidates' career histories, I show that the policy effect among female candidates for parliament is effected equally by two groups of women: those who had previously served in local government (column 1), and those who had previously contested (but not won) state assembly elections and continued in politics to contest parliamentary elections (column 3). These two groups are almost entirely distinct (shown in Appendix Table 6). This result is fundamental for a number of reasons. First, the effects found at higher levels of the political organization come, at least partially, from individuals with repeat candidacies. Short-run effects arising through this channel are thus likely to be small, and this highlights the need to examine these dynamics from a cumulative, longer-term perspective. Second, these results show that the "bottom-up" quota policy affects other levels of the organizational hierarchy at least in part thorough internal candidates, some of whom were originally introduced into politics through quotas. This stands in particular contrast to the lack of effects outside the upper echelons of corporate structures from the "top-down" quota policies for women in several European companies (Bertrand et al., 2014). Finally, this is evidence for both the candidate-supply mechanism (being the exact same individuals who gained experience in local government now contesting) and candidate response to changes in voter demand, where prior higher-office candidates continued contesting elections in areas that had historically been exposed to women politicians in positions of particular power. While changes in attitudes after exposure to women leaders has been noted previously (Bea-

man et al., 2009), Table 7 provides novel evidence suggesting a response among potential candidates – who were not direct beneficiaries of the quota policy themselves – to likely changes in voter attitudes in constituencies with greater past exposure to women politicians.

2.8 Representation Effects

2.8.1 Winning, election finishes, and effects on electoral competition

Table 8 estimates changes in the share of votes garnered by female candidates and in the (unconditional) probability of a woman winning the election or finishing either in the top five finishers or in the top 30 percent of finishers. Column 1 includes the change in the share of candidates that are female for comparison. While column 2 shows female candidates capture at least a proportionate share of votes (although imprecisely estimated), column 3 shows that there is no distinguishable increase in the probability of electing a female candidate in either state or national legislatures. This is important to note in the context of the findings of Bhalotra et al. (2015), who show that political experience causes an increase in female candidacies through repeat candidates. In the present situation, the ultimate effects of the quotas policy could be compounded with positive incumbency effects were the female candidates who contested as a result of quotas also likely to win elections. This is not the case, however, and provides one reason for the impermanence of cross-level effects of local quotas.

Focusing only on whether women win elections may understate changes in the viability of female candidates. To address this, I construct alternative measures of whether a female candidate finished in either the top five candidates or in the top 30 percent of candidates (to adjust for candidacy pool size). Columns 4 and 5 contain the results of these estimations

and indicate there are not substantial effects on women candidates finishing in the top of the distribution of finishers in the elections in which they compete – suggesting the additional female candidates induced by the policy receive relatively low vote shares in the elections they contest, or, at least, the additional candidates finish no better or worse than the average candidate.

This investigation into individual finishes does not allow for indirect effects of additional candidates on the election contest overall, given that marginal candidates may still cause changes in voting behavior of particular blocs to which they are particularly connected. In Table 9, I establish several facts about the complier population responding to policy exposure via parliamentary candidacy. To do this, I segment candidates into whether they ran for office as independents or as part of a major or minor party, and then look at the effect of policy exposure on the share of votes won by major party candidates. This establishes important points about the candidates that run for parliament in constituencies longer-exposed to quotas: (a) they largely run as independents, rather than party candidates (columns 1-3), and (b) policy exposure reduces the vote share going to major party candidates (column 4).[7] These marginal candidates may indirectly affect political outcomes through independent candidacies that disproportionately reduce the vote share going to major party candidates.

I next estimate the effect of quota exposure on the probability of a female candidate winning conditional on the presence of different types of female candidates. That is, does quota exposure have a stronger effect on the probability of a female candidate winning when the candidate has been incorporated into a major party, for example? Table 10 presents the estimation of the specifications in columns 3, 4, and 5 of Table 8 conditional on the presence of a female candidate (Panel A), the presence of a major-party female candidate (Panel B), the presence of a minor-party female candidate (Panel C), and the presence of

[7]There were four major party candidates in a constituency, on average, suggesting the expected vote share loss for major party candidates would be approximately ten percent of the magnitude in column 4.

an independent female candidate (Panel D). Abstracting from selection concerns, a clear pattern emerges across panels: conditional on candidacy, women who are incorporated into major parties increase their chances of election when their constituency has previously had greater exposure to to quotas, whereas this is the opposite for independent candidates. These results parallel the findings among municipal elections of Bhavnani (2009) by showing that the most likely conduit to achieve follow-on representation effects for women after a quota regime is through the concurrent incorporation of female candidates into major parties.

Finally, I interact the exposure measure with an indicator for the presence of an incumbent running in the election. If the lack of overall effects on representation was driven by the presence of strong incumbents, we might expect to see female candidates winning at higher rates in areas without a contesting incumbent compared to areas in which an incumbent is contesting. The presence of an incumbent, however, may drive selection of different types of female candidates in constitutencies in which an incumbent is running compared to those for which this is not the case. To check for this selection effect, the first two columns of Table 11 estimate effects on the number of female candidates, with the incumbency indicator used defined as any incumbent present (Column 1) or a major party incumbent present (Column 2). The negative effect on the main incumbency indicator in Columns 1 and 2 suggests substantive negative effects of the presence of an incumbent on the presence of female candidates. The effects in Columns 3 and 4 on whether a female candidate wins an election may then be driven by selection: incumbents generally discourage female candidacy, and those women who do contest an election with an incumbent running may be particularly strong candidates. The presence of an incumbent is not meaningfully related to policy exposure, so it is likely the selection mainly occurs among female candidates limiting the conclusions that can be drawn about whether incumbents themselves are the cause of a lack of representation effects.

2.8.2 Separating effects of recent versus past exposure

I next separate variation in the main exposure measure into periods of recency relative to the focal election. If the effect of women leaders on candidacy were through some measure of salience or inspiration (either directly, or through expectations of immediately-enhanced voter support or turnout), we might expect to see effect sizes increasing in recency; if the primary channel by which candidacy is affected is through providing opportunities for political experience, effect sizes may be less tied to particularly recent experience.

For this analysis, the policy measure is separated into three periods of exposure recency relative to the focal election: exposure in the preceding four years, exposure five to nine years prior, and early exposure in the first four years of the policy. These cutoffs are created in order to capture the fact that terms of office are five years long; this ensures, for example, that an area experiencing reservation five to nine years prior would necessarily have had a completed term of office for those in reserved seats as of the time of the focal election. Estimates for state assembly elections are in columns 1 and 2 of Table 12; effects in parliamentary elections can be found in columns 3 and 4. Across both levels of government, it is clear that the main results from Tables 4 and 5 are largely driven by exposure in the moderate past, in which the particular leaders who would have been in the chairperson seat would have had time to complete their full term of office and used that experience to establish a foundation for, or begin, their next political campaign. For parliamentary elections, it is clear that short-term effects are small and the most meaningful exposure comes from a moderately recent exposure period. This lack of effects among the most recent exposure category also refutes the hypothesis that female candidates run because there will be better cross-level support from those currently in office in local government via the quota system. In the parliament, it is clear also that effects of exposure decrease after moderate recency, suggesting an impermanence in the effects – further supporting the hypothesis that the effect

of quotas on later candidacies is concentrated specifically among those who gain political experience as a result of the quotas that can be drawn upon in soon-upcoming political campaigns.

2.8.3 Voter motivations and turnout

I use available measures of voter turnout in parliamentary and assembly elections to investigate whether there is evidence that female candidacy is increased indirectly by expectations of enhanced voter turnout by groups potentially more likely to vote for female candidates. For parliamentary constituencies, voter turnout measures are available separately by sex, while available state assembly data contains only total voter turnout; Table 12 reports point estimates from the same empirical framework as above.[8] The precisely-estimated zero effect of female leaders on measures of voter turnout in Table 13 provides evidence against the existence of effects on the extensive margin of voting behavior.

2.9 Discussion

This paper is the first to provide evidence of effects of a quota policy in politics outside the level of government in which the policy operates. Using a natural experiment in India in which one third of leadership seats in local government were randomly reserved for women across election cycles, I identify a causal response to these seat quotas among women contesting seats in later elections for state legislative bodies and the national parliament. This suggests additional, longer-term effects of quotas on political dynamics, and effects outside the particular bodies in which the quotas were active. Estimate magnitudes imply these quotas were responsible for a majority of the increase in female candidates in state legislature

[8]India has a long history of reporting voter turnout measures separately by sex, originally made easily available due to separate polling stations; although this practice is currently being phased out, the Elections Commission continues to report turnout numbers separately.

and parliamentary elections since the policy went into effect.

The cross-level effects of quotas in politics were linked to distinct groups of individuals who either gained specific experience from the quotas or were not direct beneficiaries of the policy, but continued contesting for office in areas that had had prior exposure to women leaders. There was no overall increase in the probability of electing a woman despite the increase in female candidates, so these additional candidates are precluded from accessing positive incumbency benefits because they do not win the elections they contest. However, there is some evidence of representation effects appearing when female candidates are selected to run on major party tickets.

Effects on the extensive margin of voting are limited. The impermanence of the effects is also potentially attributable to the political experience accorded by the policy to specific individuals becoming less valuable over time, as the cross-level quota effects have an inverted U-shape in recency of exposure. The additional female candidates disproportionately reduce the vote share going to major party candidates, however, suggesting some possibility for the candidates to affect the political platform. This, however, is left for future research. If a policy goal is to increase substantive representation throughout politics, quotas in local government have distinct, but limited, effects on representation in higher levels of the political structure.

The findings have implications for the understanding of how quotas affect later candidacy for higher positions in the occupation in which quotas operate. It is clear that follow-on representation effects are not automatic, and may only be accessible in environments where candidates can avail sufficient resources needed to compete for higher level positions.

Appendix Table 2.1: Average years of exposure district chairperson quotas

State	Mean	St. Dev.	Min.	Max.	*N*
Andhra Pradesh	4.1	2.6	0	11	22
Bihar	1.0	0.9	0	2	27
Gujarat	3.7	2.6	0	10	17
Haryana	4.3	0.9	3	5	12
Kerala	4.3	1.8	0	8	14
Maharashtra	4.1	2.2	0	8	27
Orissa	4.1	1.7	1	7	13
Punjab	3.2	2.8	0	8	12
Rajasthan	4.3	1.0	3	5	26
West Bengal	3.6	2.9	0	10	16
Overall sample	3.6	2.3	0	11	186

Note: Source: Author's calculations using data from Iyer et al (2012).

Appendix Table 2.2: Summary statistics, intersected constituency returns datasets

Variable	Mean	Std. Dev.	Min.	Max.
Panel A: Assembly constituencies				
Candidates	8.184	3.977	2	33
Number of female candidates	0.427	0.677	0	4
Whether area had any female candidate	0.337	0.473	0	1
Whether female candidate won election	0.063	0.243	0	1
Vote share for female candidates	0.069	0.161	0	.973
Years exposure to chairperson reservation	2.759	2.458	0	10
Number of constituencies		1,615		
Number of districts		186		
N		2,995		
Panel B: Parliamentary constituencies				
Candidates	13.495	5.911	4	38
Number of female candidates	0.857	1.087	0	5
Whether area had any female candidate	0.520	0.500	0	1
Whether female candidate won election	0.109	0.312	0	1
Vote share for female candidates	0.085	0.175	0	.904
Years exposure to chairperson reservation	3.396	2.315	0	11
Number of constituencies		278		
Number of districts		186		
N		1,375		

Note: Summary statistics reflect unconditional means across parliamentary and state assembly constituencies intersected with district boundaries in the ten-state sample shown in Table 1.

Appendix Table 2.3: Testing policy variation in explaining pre-policy predicted outcomes.

	State legislatures		Parliament	
	Female cand.	Male cand.	Female cand.	Male cand.
	(1)	(2)	(3)	(4)
Cum. years exposure	-0.000	-0.005	0.007	0.007
	(0.002)	(0.016)	(0.005)	(0.032)
State fixed effects	Yes	Yes	Yes	Yes
Mean of outcome	0.42	7.86	0.90	12.5
St. dev. of outcome	0.20	2.92	0.28	3.52
N	2,995		1,375	

Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on measures of candidacy as predicted by pre-policy observables. Coefficients are from the estimation of equation (1) in the text. Estimated with OLS. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table 2.4: Effect of exposure to women leaders on state legislature candidacy.

	OLS		Poisson	
	Female cand.	Male cand.	Female cand.	Male cand.
	(1)	(2)	(3)	(4)
Panel A: Unconditional				
Cum. years exposure	0.015*	-0.054	0.028*	-0.008
	(0.009)	(0.058)	(0.016)	(0.009)
State fixed effects	Yes	Yes	Yes	Yes
Panel B: Including covariates				
Cum. years exposure	0.014*	-0.051	0.025*	-0.008
	(0.008)	(0.057)	(0.015)	(0.009)
State fixed effects	Yes	Yes	Yes	Yes
Mean of outcome	0.43	7.22	0.43	7.72
St. dev. of outcome	0.68	4.16	0.68	4.16
N	2,995			

Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on constituency-level metrics of candidacy. Coefficients are from the estimation of equation (1) in the text. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table 2.5: Effect of exposure to women leaders on parliamentary candidacy

	OLS		Poisson	
	Female cand.	Male cand.	Female cand.	Male cand.
	(1)	(2)	(3)	(4)
Panel A: Unconditional				
Cum. years exposure	0.041*	0.113	0.049**	0.010
	(0.021)	(0.109)	(0.025)	(0.009)
State fixed effects	Yes	Yes	Yes	Yes
Panel B: Including covariates				
Cum. years exposure	0.035*	0.109	0.044*	0.009
	(0.022)	(0.103)	(0.025)	(0.008)
State fixed effects	Yes	Yes	Yes	Yes
Mean of outcome	0.89	12.61	0.89	12.61
St. dev. of outcome	1.04	5.64	1.04	5.64
N	1,375			

Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on constituency-level metrics of candidacy. Coefficients are from the estimation of equation (1) in the text. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table 2.6: Non-linearities in policy effects.

Outcome:	Count of female candidates					
Interaction:		Exposure	Literacy	Educational attainment	Sex Ratio	Low-caste population share
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: State assemblies						
Cum. years exposure	0.015*	-0.026	0.016*	0.015*	0.016*	0.014*
	(0.009)	(0.019)	(0.009)	(0.009)	(0.009)	(0.008)
Interaction		0.056**	0.053	0.026	0.051	0.117
		(0.026)	(0.066)	(0.060)	(0.055)	(0.082)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N				2,995		
Panel B: Parliament						
Cum. years exposure	0.041*	0.049	0.043**	0.043**	0.041*	0.041*
	(0.021)	(0.056)	(0.020)	(0.020)	(0.021)	(0.021)
Interaction		-0.009	-0.090	-0.253	-0.045	0.035
		(0.055)	(0.206)	(0.243)	(0.183)	(0.245)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N				1,375		

Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on the number of female candidates by constituency. Coefficients are from the estimation of equation (1) in the text, including an additional interaction term with the measure indicated in column headers. Measures of local characteristics used in columns (3)-(6) are unit standardized and main effects of the interaction terms are unreported. Estimated with OLS. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table 2.7: Effect of exposure to women leaders on parliamentary candidacy via career politicians.

Effect channel:	*Candidate supply*	*Response to voter demand*		*Other*
Measure:	Prior local politician	Female prev. Parl. candidate	Female prev. state leg. candidate	Known political family
	(1)	(2)	(3)	(4)
Cum. years exposure	0.015*	0.003	0.026***	0.016*
	(0.007)	(0.002)	(0.009)	(0.009)
State fixed effects	Yes	Yes	Yes	Yes
Mean of outcome	0.09	0.02	0.19	0.14
St. dev. of outcome	0.32	0.15	0.39	0.39
N		1,375		

Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on constituency-level metrics of within-level and cross-level repeat candidacies. Coefficients are from the estimation of equation (1) in the text. Estimated with OLS. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table 2.8: Effects of policy exposure on women's election finishes.

Outcome:	Female share of candidates (1)	Female vote share (2)	Female winner (3)	Female finish in: Top 5 (4)	Top 30% (5)
Panel A: State assemblies					
Cum. years exposure	0.00307**	0.00200	0.00110	0.01096	0.00239
	(0.00154)	(0.00218)	(0.00268)	(0.00793)	(0.00577)
State fixed effects	Yes	Yes	Yes	Yes	Yes
N			2,995		
Panel B: Parliament					
Cum. years exposure	0.00274	0.00546	0.01147	0.01541	0.01705
	(0.00204)	(0.00509)	(0.01010)	(0.01221)	(0.01161)
State fixed effects	Yes	Yes	Yes	Yes	Yes
N			1,375		

Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on segments of elections outcome finishes. Coefficients are from the estimation of equation (1) in the text. Estimated with OLS. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table 2.9: Effect of exposure to women leaders on parliamentary party competition.

	Female candidates, major party (1)	Female candidates, minor party (2)	Female candidates, independent (3)	Vote share, major party cand. (4)
Cum. years exposure	0.014	0.005	0.022**	-0.030**
	(0.017)	(0.012)	(0.011)	(0.015)
State fixed effects	Yes	Yes	Yes	Yes
Mean of outcome	0.30	0.24	0.31	0.47
St. dev. of outcome	0.53	0.50	0.62	0.60
N		1,375		

Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on constituency-level metrics of female candidates by party type. Coefficients are from the estimation of equation (1) in the text. Estimated with OLS. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table 2.10: Effect of exposure to women leaders on the election of female candidates conditional on party type.

Outcome:	Female winner	Female finish in: Top 5	Top 30%
	(1)	(2)	(3)
Panel A: Female candidates>0			
Cum. years exposure	0.016	0.011	0.013
	(0.015)	(0.012)	(0.013)
Panel B: Major party female candidates>0			
Cum. years exposure	0.034*	0.023*	0.023
	(0.021)	(0.012)	(0.017)
Panel C: Minor party female candidates>0			
Cum. years exposure	0.022	0.021	0.046**
	(0.022)	(0.023)	(0.023)
Panel D: Independent female candidates>0			
Cum. years exposure	-0.032*	-0.012	-0.032*
	(0.017)	(0.016)	(0.019)

Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on an indicator for a female candidate winning the election. Coefficients are from the estimation of equation (1) in the text. Estimated with OLS. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table 2.11: Effect of exposure to women leaders on the election of female candidates conditional on presence of incumbent.

	Female candidates		Female winner	
	Any incumbent (0/1)	Major party incumbent (0/1)	Any incumbent (0/1)	Major party incumbent (0/1)
	(1)	(2)	(3)	(4)
Cum. years exposure	0.022	0.017	-0.001	-0.002
	(0.033)	(0.034)	(0.012)	(0.012)
Cum. years reserved * incumbent present	0.038	0.052	0.024*	0.027**
	(0.047)	(0.048)	(0.012)	(0.012)
Incumbent present	-0.231	-0.306*	-0.096	-0.110*
	(0.177)	(0.185)	(0.061)	(0.059)
N	1375	1375	1375	1375
Mean of outcome	0.86	0.86	0.11	0.11
St. dev. of outcome				

Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on the number of female candidates and whether a female candidate wins the election. Coefficients are from the estimation of equation (1) in the text, including an additional interaction term with the incumbency indicator indicated in column headers. Estimated with OLS. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table 2.12: Separating effects of recent and past exposure.

	State legislatures		Parliament	
	Female cand.	Male cand.	Female cand.	Male cand.
	(1)	(2)	(3)	(4)
Years reserved, 10-15 years prior	0.022	0.070	-0.031	-0.003
	(0.027)	(0.139)	(0.038)	(0.198)
Years reserved, 5-9 years prior	0.021	-0.125	0.091**	0.168
	(0.017)	(0.105)	(0.036)	(0.172)
Years reserved, 0-4 years prior	0.003	-0.028	0.012	0.150
	(0.016)	(0.104)	(0.045)	(0.234)
State fixed effects	Yes	Yes	Yes	Yes
Mean of outcome	0.43	7.22	0.89	12.61
St. dev. of outcome	0.68	4.16	1.04	5.64
N	2,995		1,375	

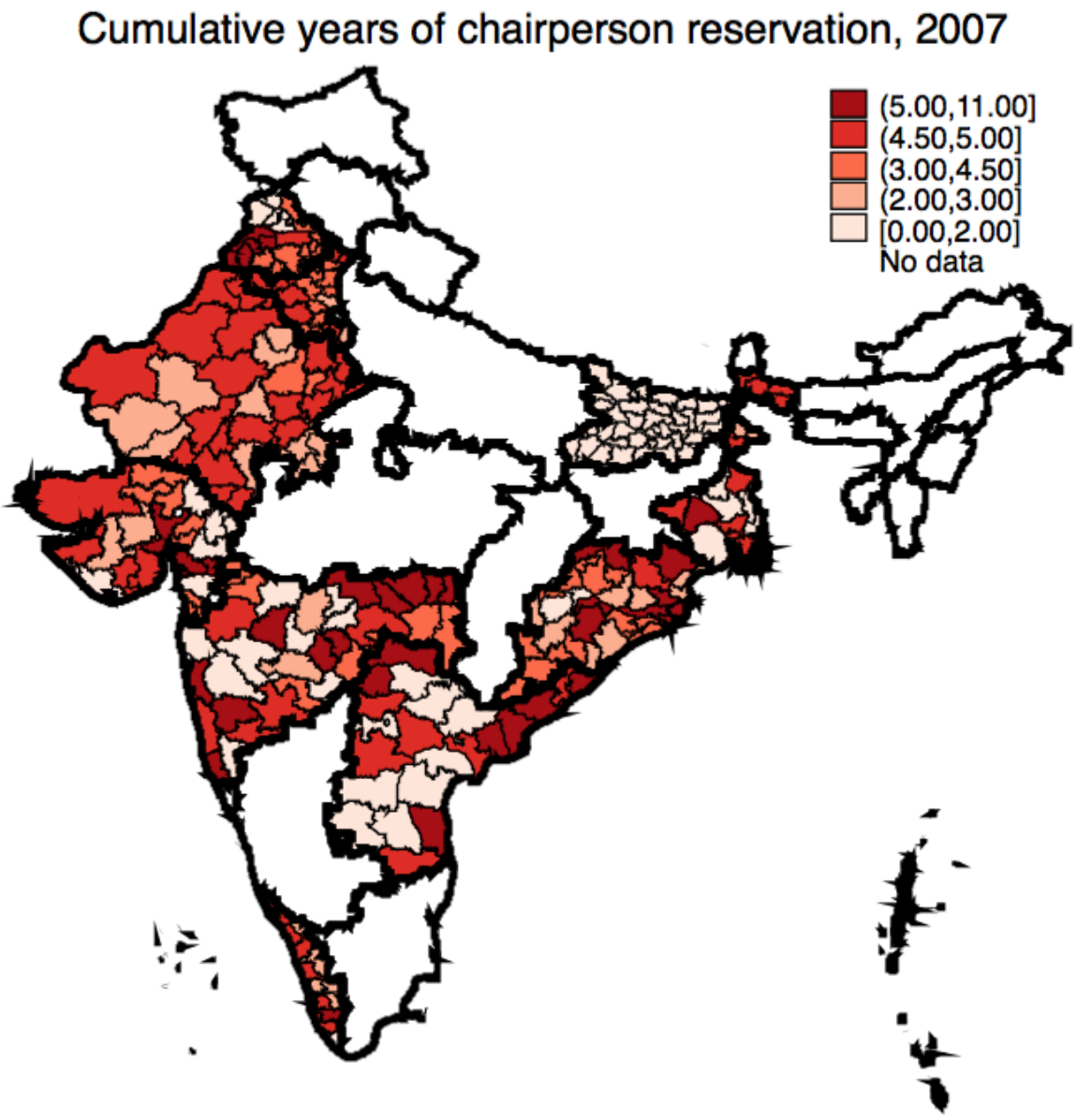
Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders in different periods of recency on constituency-level metrics of candidacy and election outcomes. Coefficients are from the estimation of equation (1) in the text. Estimated with OLS. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table 2.13: Effect of exposure to women leaders on voter motivation and turnout.

Level:	Voter turnout Parliament	Female share voters Parliament	Female voter turnout Parliament	Voter turnout Assemblies
	(1)	(2)	(3)	(4)
Cum. years exposure	0.00073	0.00037	0.00082	0.00302*
	(0.00158)	(0.00052)	(0.00183)	(0.00170)
State fixed effects	Yes	Yes	Yes	Yes
Mean of outcome	0.62	0.46	0.60	0.67
St. dev. of outcome	0.15	0.04	0.16	0.13
N		1,375		2,995

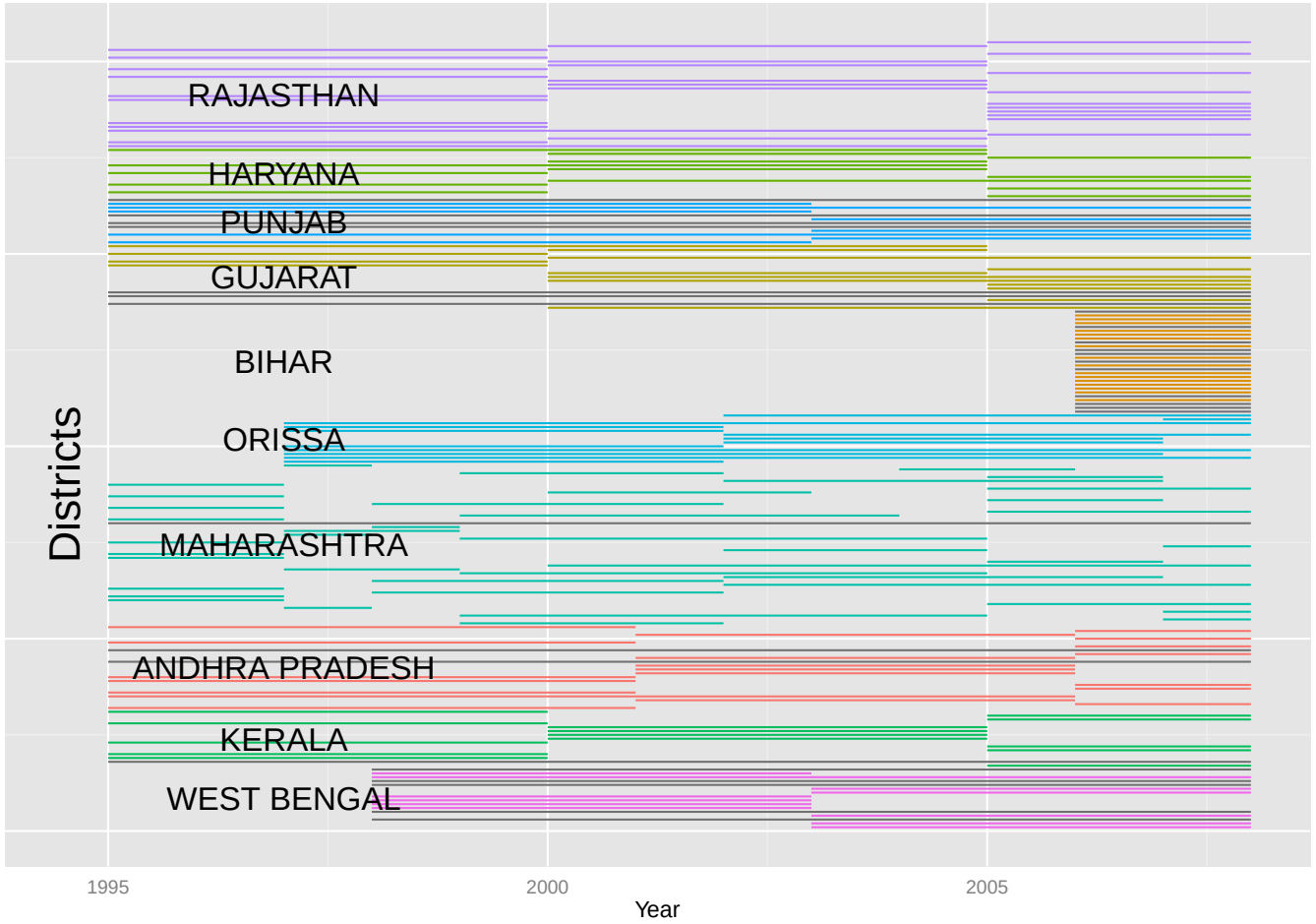
Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on constituency-level metrics of voter turnout outcomes. Coefficients are from the estimation of equation (1) in the text. Estimated with OLS. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Figure 2.1: Policy variation in district chairperson exposure



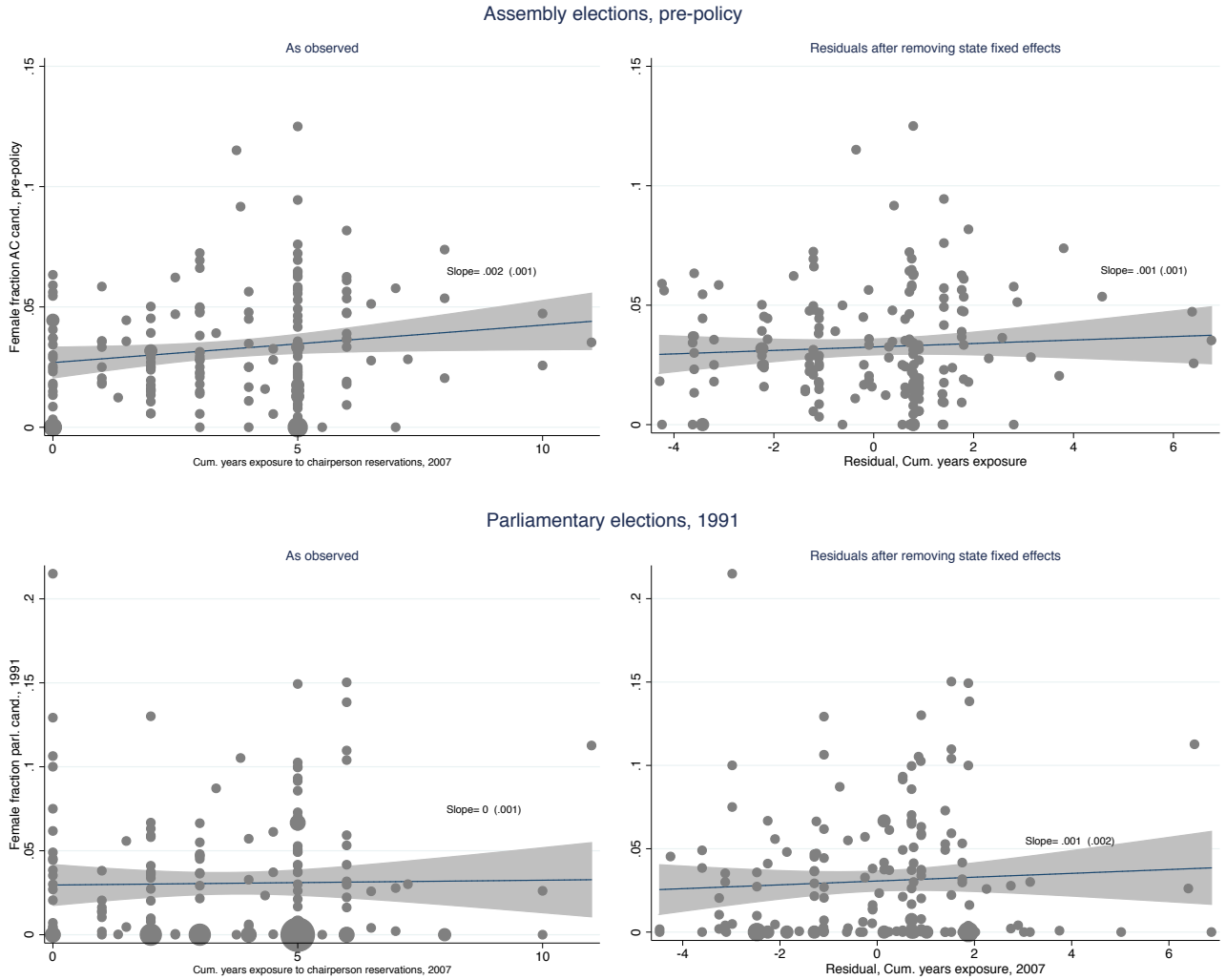
Source: Author's calculations using data from Iyer et al. (2012).

Appendix Figure 2.2: Variation in district exposure to seat reservations: timelines.



Notes: Author's calculations using data from Iyer et al. (2012). In this figure, each position on the vertical axis represents a separate district, with districts in the same states grouped together. For each district, the appearance of a filled line indicates that the district chairperson seat was reserved for a woman in that year (indicated on the horizontal axis). Districts that did not receive any period of reservation are indicated in grey within each state series. States are ordered based on relative geographical position.

Appendix Figure 2.3: Falsification: predicting pre-policy female political participation with cumulative exposure.



Note: These figures show the unconditional (left) and conditional (right) relationship between pre-policy female political participation as measured through state and national legislature candidacy rates and years of exposure to women leaders via seat quotas from 1995 to 2007. A flat line indicates no relationship between pre-policy measures and eventual exposure.

A1 Candidate Panel dataset

Similar to transparency efforts in many other countries in recent decades, the Indian government has undertaken a large-scale process to digitize a great deal of its official records and make them easily accessible to the public; elections data now available from various sources (all ultimately sourced from the Elections Commission of India) are a prime example of this. The Elections Commission (eci.nic.in) publishes numerous internet and PDF documents, as well as digitized databases, containing detail on elections outcomes for directly-elected bodies including the Lok Sabha and state legislative assemblies. Currently, digitized candidate lists and outcomes by constituency are available for the entire history of the Lok Sabha; digitized data for state assembly elections have been made available as part of Jensenius (2013).

Candidate linking was performed for all Lok Sabha elections from 1991 to 2014. Candidates were linked by taking all candidates in the focal year and assessing the closest name match in the same state and constituency in the target year, up to a .2 Levenshtein distance. This results in one closest name match (or multiples, in the case of an exact tie) in the target year for each candidate in the focal year. Each match is then reviewed manually for sufficient similarity across the two candidate name entries and a final match decision is made. Many candidates had exact name matches. There are important aspects of this process to note:

1. The use of the closest match only induces the propensity for undercounting repeat candidates, whose second or third match in the target year may have been proper (but ignored by the current algorithm).

2. Some names were difficult to fully verify either way, particularly in the cases of candidates only having a single (usually given) name listed, in the case of common given-surname pairs, and/or in the case of election "fraud" or the running of several candidates with the same name. In the absence of other information, exact or near matches of this nature were recorded as separate individuals.

Sufficient criteria for assessing a positive match largely proceeded according to the following rules:

1. reordering of surname, given name, and/or

2. reordering or removal of an alias or honorifics, and/or

3. spelling differences via differences arising from name transliteration or spelling/typos.

In the case of the use of a single initial for a candidates' given name in official records, records were given closer scrutiny and required exact matching of the abbreviated name in both records. Appendix Table 3 contains a summary of the matching process as it applied to the state and national elections used in the analysis.

Table 3 highlights a number of facts about politics and politicians in India. First, repeat candidacies in both levels of government are substantial, and a substantial share of these repeat candidacies are by individuals who previously *lost* an election. (See for example, 6,870 repeat candidacies in state assembly elections contesting 4,094 seats; similarly in the parliament, 1,004 repeat candidacies contesting 543 seats). Second, there is also substantial movement of candidates across levels, with more then 1,000 candidates for the 2009 parliamentary elections having previously contested a state assembly seat. Third, the probability of a woman contesting for parliament after a state legislature candidacy is three to four times higher than for a previous male candidate, and this difference exists both for those who won and lost their state legislature contest. Because of this, the parliamentary candidacies by former assembly candidates are dominated by men and women who previously *lost* their contest for state legislature seats. There is a non-trivial subset of candidates who contest higher levels of political office over time, and these trajectories are not necessarily impeded by a prior loss, either at the same or a lower level of government.

Any matching process of this nature is subject to some degree of error. False negatives are most likely to arise in the cases of candidates using either a single (or a very common)

name which cannot be definitively assigned across elections, or in the case of a candidate's name being represented or recorded substantially differently across elections so as to appear a different candidate. False positives are likely to occur only in the case of very common names, or the extreme case of multiple individuals running in the same state with very similar (or exactly-matching) names. The above is unlikely to induce bias in the analysis for two reasons: these occurrences are far less common for female candidates simply due to the number of female candidates, who are more readily identifiable and less likely to present false positives, and there is no reason for such cases to be related to variation in exposure to women leaders.

A2 Assigning constituency treatment levels and population weights

As mentioned in the text, district boundaries, which determine exposure to chairperson seat reservations, do not overlap with parliamentary constituencies (shown in Appendix Figure 1 above) or state assembly constituencies.

I begin with an administrative shapefile of Indian districts as of 2001. (Note that the district chairperson infomation has been adjusted to account for changes in district definitions relative to 2001 definitions.) I then overlay boundary files for the 2009 Lok Sabha constituencies and pre-delimitation (2007) state assembly constituencies.

I then find the intersection of district and constituency boundaries to create distinct units which correspond to polygons defined by unique overlaps of district-constituency pairs. This process creates a set of unique geographic areas defined by the area pertaining to a unique combination of a district and constituency.

Using data from the Gridded Population of the World for India (adjusted population counts by 2.5 arc-minute grid), I then assign population counts to the unique area produced

by the above process for both the parliamentary-constituency-district and state assembly constituency-district intersected areas. A GIS-format workspace containing the original boundaries and constructed intersections and joins is available in the supplementary materials posted on my website.

It is important to note that boundary segments that are coterminous in reality may not be defined precisely so in mapping files. Deviations in the path of overlapping boundaries will cause this process to generate small areas which incorrectly indicate an erroneous district-constituency overlap. (Such occurrences are most obviously seen when an overlapping area is defined by a constituency-district pair in different states; neither districts nor constituencies comprise areas in multiple states.) For both state assembly and parliamentary constituencies, I partially correct for this process by dropping any overlapping areas from district and constituencies indicated to be in from different states (such occurrences are not possible). Summary statistics of these areas are presented in Appendix Tables 4 and 5.

A3 Web research on 2009 Parliamentary candidates

Because there is no single resource detailing the background of political candidates, I relied on web research to capture basic fields about 2009 female parliamentary candidates. These included information on whether the candidate was previously involved in local government, in other (state or national) government, and/or whether the candidate was related to any known politicians.

Searches for this information was undertaken by:

1. Searching the [full/part name] of the candidate in the [state] Election Commission website

2. Searching the [full/part name] of the candidate with "panchayat election" added

3. Searching the [full/part name] of the candidate with "zilla parishad" added
4. Searching the [full/part name] of the candidate with "municipal election" added
5. Searching the [full/part name] of the candidate with [party name] added
6. Searching the [full/part name] of the candidate with [history/background] and [political party name] added
7. Searching the [full/part name] of the candidate in the past (before 2009) [panchayat/zilla parishad/municipal] election results available from the [state] election commission website

There were several additional considerations made when identifing the candidate matched to information found. Whenever there was confusion about whether the person found is the focal candidate, any of a combination of the following validation steps were undertaken:

1. Category is the same - General/SC/ST/OBC
2. The place of residence/earlier political activity of the candidate falls under the Lok Sabha constituency from which the candidate stood for the 2009 Lok Sabha election
3. Party is the same. (Note that this may not be true always as sometimes the candidates leave their party.)
4. Age - if the candidate age is 25 at the time of the 2009 Lok Sabha election then when sourcing the results of earlier elections, say from 2004 or before, if the same name is encountered and even the political party name matches still that candidate would be not assigned that particular political history since it is unlikely that a candidate at the age of 20 or less would be contesting in an election.

Appendix Table 6 contains a summary of the information found; further detail (by candidate) on this research can be found in the supplementary materials posted on the author's website.

A4 Appendix Tables and Figures

Appendix Table A1: Timing of state assembly elections

State	2003	2004	2005	2006	2007
Andhra Pradesh		X			
Bihar			X		
Gujarat					X
Haryana			X		
Kerala				X	
Maharashtra		X			
Orissa		X			
Punjab					X
Rajasthan	X				
West Bengal				X	

Source: Table presents indicators for the calendar year in which state assembly elections are observed.

Appendix Table A2: Predicting cumulative exposure with pre-policy district characteristics.

	(1)
School enrollment rate, 1987	-1.899
	(1.559)
Women's literacy rate, 1991	2.090
	(2.831)
Mean household cons. per cap., 1987	-0.007
	(0.005)
Average household size, 1987	-0.282
	(0.220)
Sex ratio, 1987	3.332
	(3.808)
Share SC/ST, 1991	-0.104
	(1.147)
Female fraction parl. cand., 1991	3.488
	(4.853)
Female fraction AC cand., pre-policy	-0.535
	(11.742)
N	179
F-statistic on test of joint significance	.792

Note: This table reports the test of joint significance of measures of pre-policy area conditions on eventual policy exposure as of 2007. Estimated with OLS. Heteroskedasticity-consistent standard errors in parentheses. Significance levels are indicated by $*$ $< .1$, ** $< .05$, *** $< .01$.

Appendix Table A3: Summary statistics, state assembly and parliamentary candidate panel dataset

	State Assemblies (2003-2009)	Parliament (2009)
Panel A: Aggregates		
Num. candidates, men	34,129	7,257
Num. candidates, women	2,220	543
Frac. candidates, women	0.061	0.070
Men elected	3,838	485
Women elected	256	58
Frac. elected, women	0.063	0.107
Repeat contest, men	6,493	1,062
Repeat contest, women	287	42
Prior AC candidate, men	-	1,009
of which was elected in prior AC	-	101
Prior AC candidate, women	-	138
of which was elected in prior AC	-	22
Panel B: Constituency-level measures		
Number of constituencies	4,078	543
Avg. number candidates	9	15
Avg. number female candidates	0.538	1
Frac. const. with any female candidate	0.392	0.600
Frac. const. electing a female candidate	0.063	0.107
Frac. const. electing a female candidate, conditional	0.159	0.178
Frac. of votes for female candidates (net)	0.068	0.086

Source: Author's calculations from candidate panel dataset constructed using data from Jensenius (2015) and the Indian Elections Commission. Table shows aggregate candidacy measures across state and national legislatures by gender (top panel) and constituency-area summary statistics (bottom panel).

Appendix Table A4: District - assembly constituencies intersected polygons: summary statistics

Variable	**Mean**	**(Std. Dev.)**	**Min.**	**Max.**	***N***
Polygon area	0.02868	(0.06614)	0	3.09819	9,579
Polygon area, same state	0.03278	(0.07190)	0	3.09819	7,829
Polygon area, different state	0.01036	(0.02026)	0	0.32990	1,750

Appendix Table A5: District - parliamentary constituencies intersected polygons: summary statistics

Variable	Mean	(Std. Dev.)	Min.	Max.	*N*
Polygon area, full sample	0.07879	(0.19471)	0	3.75577	3,527
Polygon area, same state	0.09930	(0.21477)	0	3.75577	2,779
Polygon area, different state	0.00256	(0.00478)	0	0.06240	748
Polygon area, small polygons	0.00336	(0.00588)	0	0.03995	2,588

Appendix Table A6: Candidate histories summary information

N female candidates	265
Background information found	102

Background	Candidates	Share
Prior higher office candidacy	60	58.8
Prior local government service	27	26.5
Higher office candidacy and local government experience	4	3.9
Prior higher office candidacy and political family	21	20.6
Prior local government and political family	2	2.0
Only family ties	19	18.6

Appendix Table A7: Effect of exposure to women leaders on candidacy: aggregated sample.

	OLS		Poisson	
	Female cand.	Male cand.	Female cand.	Male cand.
	(1)	(2)	(3)	(4)
Panel A: State Legislatures				
Cum. years exposure	0.013	-0.032	0.026*	-0.005
	(0.008)	(0.031)	(0.016)	(0.005)
State fixed effects	Yes	Yes	Yes	Yes
Mean of outcome	0.44	7.72	0.44	7.72
St. dev. of outcome	0.68	4.11	0.68	4.11
N	1,615			
Panel B: Parliament				
Cum. years exposure	0.051*	0.147	0.060*	0.012
	(0.029)	(0.132)	(0.032)	(0.011)
State fixed effects	Yes	Yes	Yes	Yes
Mean of outcome	0.89	12.7	0.89	12.7
St. dev. of outcome	1.04	5.74	1.04	5.74
N	279			

Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on constituency-level metrics of candidacy. Coefficients are from the estimation of equation (1) in the text. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table A8: Effect of exposure to women leaders on candidacy: size-weighted estimations.

	State Assemblies		Parliament	
	Female cand.	Male cand.	Female cand.	Male cand.
	(1)	(2)	(3)	(4)
Cum. years exposure	0.014*	-0.030	0.045**	0.090
	(0.008)	(0.060)	(0.021)	(0.107)
State fixed effects	Yes	Yes	Yes	Yes
Mean of outcome	0.43	7.22	0.89	12.61
St. dev. of outcome	0.68	4.16	1.04	5.64
N	2,995		1,375	

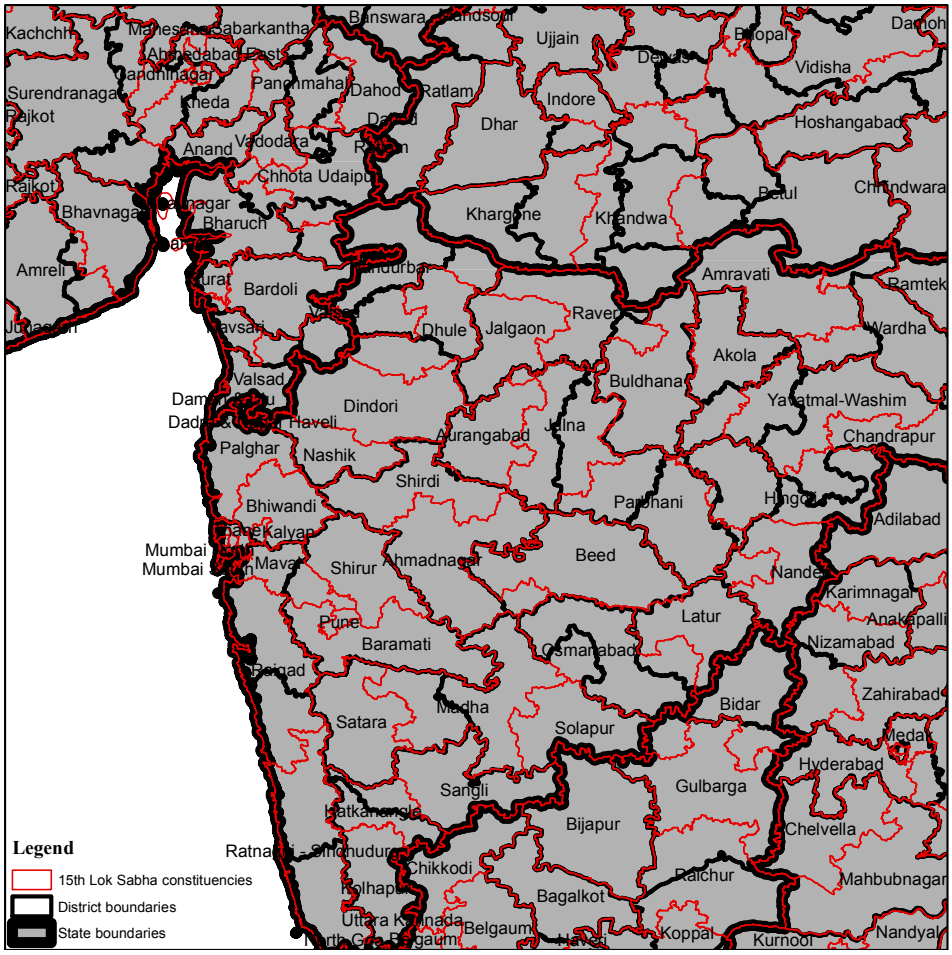
Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on constituency-level metrics of candidacy. Coefficients are from the estimation of equation (1) in the text. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Table A9: Effect of exposure to women leaders on candidacy and election outcomes: interactions with incumbent presence.

	Female candidates		Female winner	
	Any incumbent running	Major party incumbent running	Any incumbent running	Major party incumbent running
	(1)	(2)	(3)	(4)
Cum. years exposure	0.022	0.017	-0.001	-0.002
	(0.033)	(0.034)	(0.012)	(0.012)
Cum. years reserved * incumbent present	0.038	0.052	0.024*	0.027**
	(0.047)	(0.048)	(0.012)	(0.012)
Incumbent present	-0.231	-0.306*	-0.096	-0.110*
	(0.177)	(0.185)	(0.061)	(0.059)
N	1375	1375	1375	1375
Mean of outcome	0.86	0.86	0.11	0.11
St. dev. of outcome	1.09	1.09	0.31	0.31

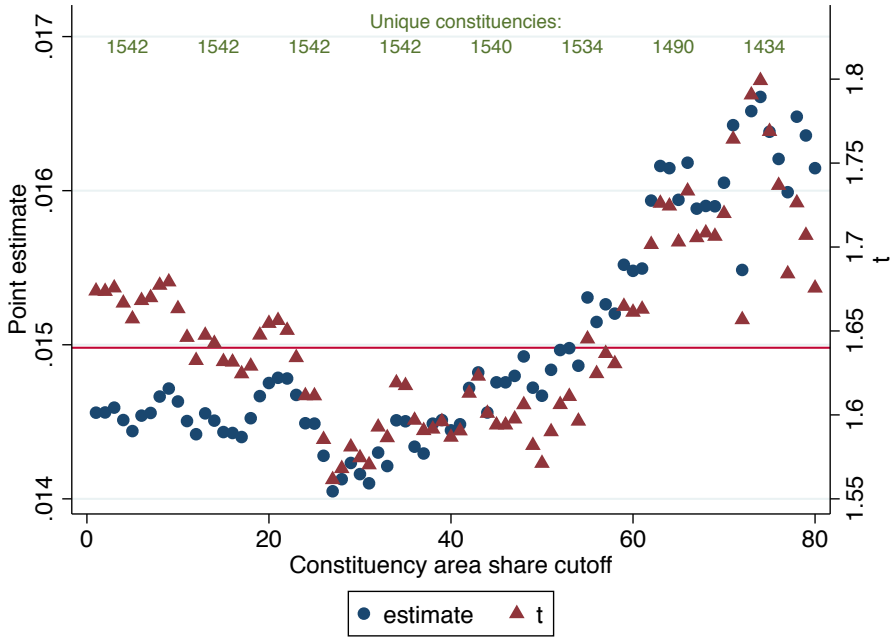
Note: This table reports coefficient estimates of the effect of an additional year of exposure to women leaders on the number of female candidates and whether a female candidate wins the election. Coefficients are from the estimation of equation (1) in the text, including an additional interaction term with the incumbency indicator indicated in column headers. Estimated with OLS. Heteroskedasticity-consistent standard errors clustered by district and constituency in parentheses. Significance levels are indicated by $* < .1$, $** < .05$, $*** < .01$.

Appendix Figure A1: District and parliamentary constituency boundaries in Maharashtra.

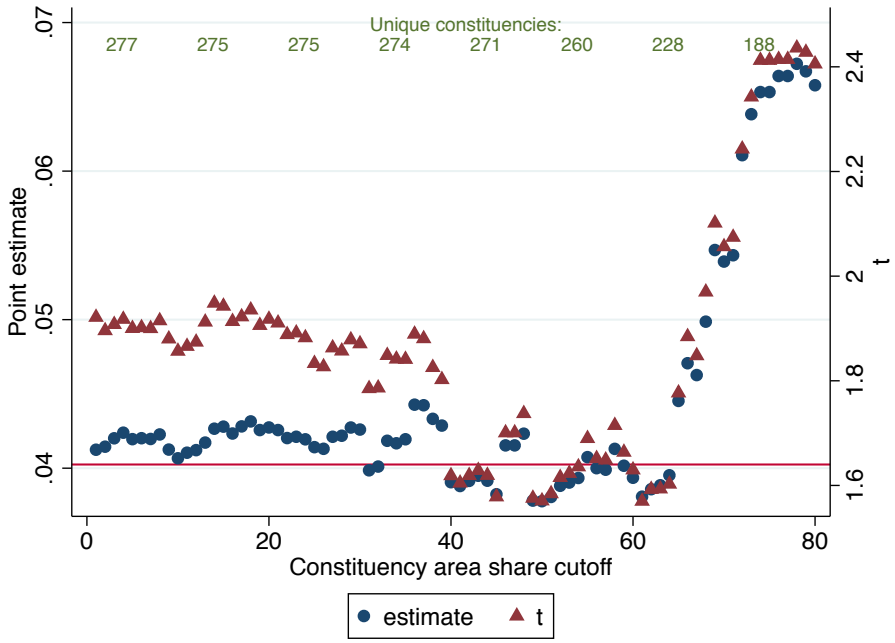


Appendix Figure A2: Coefficient magnitude and significance under varying sample restrictions.

Panel A: State assemblies



Panel B: Parliament



Note: Figures depict coefficient magnitudes and t-statistics across samples that progressively drop larger component areas based on constituency population share, ranging from zero to up to 80 percent of constituency population share.

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