The Direct and Spillover Impacts of Gender Quotas in Public Sector Employment

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Preliminary Draft – Comments Welcome

Abstract

Expanding women's access to stable economic opportunities remains a central policy challenge in many countries around the world. Can gender quotas help? We provide new evidence on the short- and long-run impacts of gender quotas in public sector employment in India. Leveraging variation in quota exposure across cohorts and across adopting and non-adopting states, we report three main findings. First, quota exposure increases the likelihood of public employment for women in the short- and long-run, with short-run gains concentrated among those with higher pre-quota education levels. Second, quotas lead to greater educational investments in the long-run, with women more likely to become secondary school and college graduates. Third, while there is no increase in private employment in the short-run, women are more likely to be employed in the formal private sector in the long-run. Yet, in the long run, there is no effect on *overall* employment. Together, these results show that gender quotas in public employment can catalyze human capital investments and shift women towards better jobs, though increasing overall female labor force participation requires addressing additional underlying constraints.

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Women's access to good, stable jobs remains limited in many low- and middle-income countries. They are less likely than men to hold formal-sector jobs, which tend to offer higher wages, better benefits, greater job security, and stronger prospects for financial independence (World Bank, 2023). While governments have often used quota policies to redress historical disadvantages in access to jobs along ethnic, racial, or caste lines, the adoption of *gender*-based quotas in hiring has been less common. Consequently, evidence on the impacts of gender quotas in the labor market remains scarce (Fletcher et al., 2017).

In this paper, we provide new evidence on the effects of gender quotas in public sector employment, focusing both on their direct effects on women's access to these jobs as well as their broader effects on educational and labor market investments. Specifically, we study the adoption of state-level gender quotas in public employment in India over the last four decades. These policies reserve a share of new vacancies in state-level jobs—positions typically filled through competitive examinations—for women. Competitive exams often have explicit age-eligibility criteria or exhibit age-based participation patterns, generating variation in cohort-level exposure to gender quotas. We exploit this variation to estimate causal effects by comparing exposed and unexposed cohorts in states with and without quotas.

The public sector is a particularly important setting for evaluating gender quotas. These jobs are highly desirable: they offer high and stable salaries; predictable hours; job security; and benefits, such as pensions and paid leave. They are also considered prestigious and safe, making them especially attractive to women.[1] Given these features, competition for these jobs is intense, with vacancies often attracting hundreds of applicants per position; that is, obtaining a public sector job is broadly considered a visible marker of success. Expanding women's access to these jobs may therefore generate spillovers beyond the jobs themselves—by influencing women's educational attainment, labor force participation, and other life choices.

Our analysis draws on a newly compiled dataset documenting the timing and scope of state-level gender quotas implemented since 1989. We combine this data with individual-level data from the 1993–2011 rounds of the National Sample Survey (NSS), which provide information on employment, education, and demographics. As of 2024, a total of 18 (out of 28) states have adopted gender quotas; given the availability of NSS data, we are able to study 13 of these reforms.

Our empirical strategy exploits the staggered rollout of public-sector gender quotas across Indian states using a stacked difference-in-differences design. We leverage two sources of variation: across cohorts, based on age at the time of quota adoption, and across states, based on differences in timing of adoption. For each adopting state, we construct a sub-experiment that compares exposed cohorts—usually, women below age 30 at implementation—to unexposed older cohorts, benchmarking these differences against the same cohorts in never-treated states.[2] We then stack these sub-experiments and estimate a pooled treatment effect, including state-by-stack and age-by-stack fixed effects, a range of individual and baseline controls, and clustering at the state-by-birth-cohort level. Identification comes from

[1] Globally, the public sector is a larger employer of women than the private sector (Mukhtarova et al., 2021).
[2] Due to NSS data limitations, our analysis restricts to post-treatment NSS rounds only.

within-state, within-stack comparisons between exposed and unexposed cohorts, under the assumption that in the absence of quotas these groups would have followed parallel trends. By construction, treated states never serve as controls for later-treated states, avoiding the comparison problems that arise in standard TWFE models.

Using this approach, we separately estimate short-run (within 7 years) and long-run (8–15 years) effects of quota exposure on employment and education. The short-run samples include cohorts for whom education is unlikely to adjust in response to the policy, while the long-run samples allow enough time for schooling investments to materialize. We have three main findings.

First, quota-exposed cohorts of women are more likely to hold a public-sector job in the short- and long-run. In the short run, public employment among exposed cohorts increases by 0.6–0.7 percentage points—a roughly 25% rise relative to the mean among eligible women in never-treated states. These gains are concentrated among women with more than ten years of schooling prior to quota implementation, consistent with minimum educational thresholds for public-sector hiring. These patterns point to both quota compliance and active public-sector recruitment over the study period. These gains for women come partly at the expense of highly educated men, who become less likely to hold public-sector jobs; however, these men move into private-sector jobs, both formal and informal, and their overall employment actually rises, likely reflecting reduced queuing for government positions.

In the long run, the effects on women's public employment persist at similar magnitudes (0.6–0.9 percentage points), suggesting a sustained expansion in access to public-sector opportunities. These longer-run gains likely reflect both the initial compositional shift induced by the policy and subsequent educational responses that enable a broader set of women to qualify for public-sector work.

Second, we find little evidence of spillovers onto other types of employment for women in the short run, but in the long run women become more likely to work in the private formal sector. Disaggregating employment into private formal and informal categories, we detect no statistically significant short-run changes in either sector. Overall employment among women rises by 1.0–1.4 percentage points in the short run, with the public-sector gain accounting for about half of this.[3] In the long run, however, clear spillovers emerge: women are 0.2–0.3 percentage points more likely to be employed in the private formal sector—a roughly 16–20% increase—accompanied by a larger decline (1.3–1.7 percentage points) in informal employment. Yet, we find no evidence of significant changes in overall employment. These results suggest that the quota operated by shifting the composition of women's employment toward formal jobs, rather than raising overall rates of employment.

Third, we find large and significant impacts on women's educational attainment. Women in quota-exposed cohorts acquire 0.8–0.9 additional years of schooling—an 11–13% increase relative to the mean among eligible women in never-treated states. Because government positions require minimum educational qualifications, we also examine attainment of key degree

[3]The share of women in public employment is small (under 3%) and the quota-driven increase still keeps this share modest.

thresholds. Exposed women are 1.8 percentage points more likely to complete senior secondary school and 2.0 percentage points more likely to graduate from college, representing 7% and 16% increases relative to the corresponding means in never-treated states. These effects are larger for women with longer durations of exposure to the quota regime. The substantial rise in schooling likely contributes to the long-run reallocation from informal to formal employment documented above.

This paper makes three contributions. First, we provide new evidence on the impacts of gender quotas in public sector hiring. Prior work has focused primarily on political and corporate leadership quotas, showing broad gains in women's representation and influence (see Pande and Ford, 2012; Hughes et al., 2017; Clayton, 2021, for a review), as well as downstream effects on aspirations and economic activity (Beaman et al., 2009; Bose and Das, 2018; Ghani et al., 2014). Evidence on gender quotas in specific occupations is far more limited and examines productivity or output within the targeted sectors (Schaede and Mankki, 2022; Lyu, 2025). Our contribution is to show how gender quotas in public sector hiring—a particularly desirable sector for women in our context—improves women's access to stable, formal employment, both directly through increased public employment and indirectly through shifts into formal private-sector work.

Second, we contribute to the literature on how human capital investments respond to labor market opportunities. Within India, two recent papers examine how caste-based reservations affect schooling.[4] Khanna (2020) studies a nationwide extension of job quotas to the Other Backward Classes (OBCs) and finds average gains of 0.8 years of schooling for the targeted group, but does not estimate effects separately by gender. Cassan (2019) finds that Scheduled Caste (SC) reservations increase educational attainment for men but not women. Experimental evidence also shows that improving women's perceived job prospects can raise schooling (Jensen, 2012). We show that reserving desirable public sector jobs for women substantially increased their secondary schooling and college attainment, which may have contributed to the narrowing of gender gaps in education over the last two decades.

Finally, we contribute to the literature on female labor force participation in low- and middle-income countries (see Heath et al., 2024, for a review). Specifically, we shed light on the relationship between women's educational gains and their employment outcomes. While quotas increased women's educational attainment and access to formal jobs, they did not increase overall employment rates for women. This pattern aligns with recent evidence from Ghana showing that large gains in women's education increases entry into government jobs but does not translate into higher overall employment (Duflo et al., 2024), and with evidence from schooling expansions across Africa indicating that human capital improvements do not consistently raise women's labor force participation (Dinkelman, 2025). Together, these findings highlight a persistent disconnect between women's rising educational attainment and their employment opportunities in many LMICs.

[4] A broader set of studies have examined affirmative action in other countries. For instance, Akhtari et al. (2024) show that racial affirmative action in college admissions incentivized pre-college human capital. Meanwhile, Estevan et al. (2019) do not find any effect on test preparation for an affirmative action policy at highly ranked Brazilian university.

1 Context

1.1 Affirmative action policies in India

Since independence, India has implemented an extensive system of affirmative action policies across multiple domains—in politics, public employment, and educational institutions. The backbone of this system is caste-based reservations, which aim to mitigate historical disadvantages faced by Scheduled Castes (SC), Scheduled Tribes (ST), and Other Backward Classes (OBC). These caste-based reservations typically exist both at the national and at the state level.

In contrast to caste-based reservations, gender quotas have been adopted in a more limited fashion. The most prominent example of adoption is in local politics: the 73rd and 74th Constitutional amendments mandate the reservation of one-third of seats for women in Panchayati Raj institutions and in urban local bodies. Another example is the Companies Act of 2013, which mandates that certain classes of companies have at least one woman on their Board of Directors. Importantly, there is, however, no gender quota in public sector employment at the center. Instead, several states have adopted gender quotas on their own and we study this state-level adoption of gender quotas in this paper.

1.2 Timeline and coverage of state-level gender quotas

Figure 1 shows the staggered adoption of gender quotas in public employment across Indian states over the past four decades. Tamil Nadu was the first state to implement a gender quota in 1989, followed by Odisha and Maharashtra by 1994. A second wave of adoption occurred from 1995 to 1999, when many states—including Andhra Pradesh, Gujarat, Karnataka, Madhya Pradesh, Rajasthan, and Uttar Pradesh— introduced quotas.[5] The 2000s saw only two new adopters, Jharkhand and Assam, while the last wave came in the 2010s, with Sikkim, Bihar, Punjab, and Tripura implementing reservations. In total, as of 2024, 18 states have gender quotas in public employment, while 10 states do not.

These reservations only applied to *new* vacancies filled via direct recruitment—typically conducted through competitive exams— and excluded vacancies filled via promotions or political appointments. To the best of our knowledge, gender quotas typically applied to state administrative jobs; in some states, quotas were also explicitly extended to other public sector roles in educational institutions or state-owned enterprises. The educational requirement for state administrative jobs is usually a college degree for higher-ranked posts and at least a 10th-grade education for lower-ranked posts. Over time, some states have increased educational requirements for lower-ranked posts to a college degree; even where formal requirements remain lower (e.g. in Tamil Nadu), college graduates are much more likely to be successful in competitive exam-based hiring for public sector jobs (Mangal and Singh, 2025).

The size of the initial reservation adopted across states varied from 20–35%.[6] Given

[5] For the newly created states of Chhattisgarh, Uttarakhand, and Telangana, we assign the quota policy of the parent state until the time period when the new state modified its policies.

[6] An exception is Jharkhand which had a substantially lower quota of 5%.

existing caste-based quotas at the central and state levels, as per Supreme Court rulings, gender quotas can only be implemented horizontally within each caste category rather than vertically. This means that gender quotas extended economic opportunities for women across caste groups. States often also included rules specifying that if no eligible or sufficiently qualified women were available in the applicant pool, those vacancies could be filled by qualified men. Following the initial reservation, six states increased the quota size by an average of 6 p.p. As there are limited changes over time to the size of the reservation, we focus on the initial gender reservation in our analysis.

2 Data and Empirical Strategy

2.1 Data sources

Reservations: We compiled the timing and extent of reservation for gender quotas in government positions across different states by systematically reviewing publicly available official records, including government orders, legislative acts, and policy notifications issued by the state governments. These documents provided information on the year of introduction, the percentage of reserved positions, the type of positions reserved and any subsequent amendments or expansions of the quota policy. To ensure accuracy and completeness, we cross-verified these dates and provisions using newspaper reports from regional and national outlets, which often reported on the announcement, implementation, and political context of these reforms. This approach allowed us to reconcile discrepancies, fill gaps in official archives, and construct a consistent and reliable state level timeline of gender quota adoption and changes.

Employment and Education: We compiled individual-level employment data from the thick rounds of the National Sample Survey (NSS) conducted between 1993 and 2011. These rounds provide detailed labor market information, including an individual's employment status, distinguishing between work in the public sector, formal private sector, and informal employment. Alongside employment details, the data include a rich set of demographic and socioeconomic characteristics—such as caste category, religion, and place of residence (state and district)—enabling us to examine patterns in employment outcomes across different social, and geographic groups. The NSS data also contains information on the number of years of education of the respondents, which we use in our analysis.

2.2 Empirical strategy

Our primary empirical strategy exploits the staggered introduction of public-sector gender quotas across Indian states using a *stacked* difference-in-differences (DiD) design. To maximize the sample of treatment states we can examine, we rely on two sources of variation: (i) across cohorts, based on differences in quota exposure determined by age at the time of policy

implementation, and (ii) across states, based on differences in timing of policy adoption.[7]

Eligibility rules and age-based participation patterns for public employment generate variation in quota exposure. Generally, women who were under the age of 30 when a state implemented its quota policy are classified as *exposed*, while those beyond the upper age limit for entry into public employment serve as *unexposed* cohorts.[8] States that did not adopt the reform by 2011, the last year in which the NSS data are available, form the set of never-treated comparison states; there are fifteen such states, including Jharkhand and the four states that adopted quotas after 2011.[9]

To implement the stacked design, we construct a separate sub-experiment, r, for each adopting state. In each sub-experiment, we compare exposed and unexposed cohorts in the treated state with the same cohorts in a set of never-treated states, using only post-treatment NSS rounds. Observations from treated states appear only in their own sub-experiment, while those from never-treated states may be repeated across sub-experiments, depending on the time window under consideration. We then stack these sub-experiments into a single estimation sample and run the following:

$$Y_{isr} = \beta \text{Treated}_{sr} \times \text{Exposed}_{a(i),r} + \mu_{sr} + \phi_{b(i,r),r} + X_{ir} + \varepsilon_{isr} \tag{1}$$

where Y_{isr} is the outcome for individual i in state s in sub-experiment r; μ_{sr} are state-by-stack fixed effects; and $\phi_{b(i,r),r}$ are fixed effects for 3-year bins of age at quota adoption, $b(i,r)$, interacted with stack. X_{ir} includes a set of individual level controls.[10] Standard errors are clustered at the state-by-birth-cohort level, reflecting the level at which treatment exposure varies and accounting for duplication of observations across stacks.[11] In additional specifications, we also probe the robustness of our results to pre-existing differences in women's employment and schooling by controlling for historical state-specific age profiles measured in the 1987 NSS: the employment rate and the share with lower secondary education or above, computed with survey weights for each state, sex and single year of age and assigned to respondents by their state, sex and age at the time of the survey.

Given the NSS data coverage, we have up to 13 distinct sub-experiments. In each sub-experiment, identification comes from within-state and within-stack comparisons between exposed and unexposed cohorts. The fixed effects μ_{sr} absorb any common shocks to all cohorts in a state-stack cell, and $\phi_{b(i,r),r}$ absorb shocks common to individuals of a given cohort within that sub-experiment. Our identifying assumption is that, absent the quota policy, exposed and unexposed cohorts across treated and comparison states within a sub-experiment would have followed parallel trends. We use this design to estimate a single

[7] We also report a staggered difference-in-differences that uses the timing of adoption across states directly, described below. Because the panel is unbalanced, that design carries limitations the stacked design does not: for several outcomes, most notably public employment, the pre-period is too thin to assess parallel trends between treated and comparison states directly.

[8] The exact age band for exposed cohorts varies depending on the outcomes and whether we estimate short- and long-run effects. See section 2.3 for details.

[9] Given the very low reservation percentage (5%) in Jharkhand relative to the other treated states, we assign Jharkhand to be a never-treated comparison state.

[10] These include: religion, caste, marital status, household size, and urban or rural residence

[11] Clustering at the state level yields fewer clusters and is less precise.

pooled coefficient, β, which should be interpreted as a weighted average of stack–specific treatment effects.

2.3 Sample construction

We examine both short- and long-term consequences of these reforms using two complementary samples that differ in the time elapsed between quota adoption and measurement of outcomes.

Short-run effects: We use all available observations that occur within seven years of quota implementation in treated states and restrict comparison states to the same time window in each sub-experiment. Given that the NSS data is available only through 2011, the treated states in this analysis are: Andhra Pradesh, Chhattisgarh, Gujarat, Karnataka, Madhya Pradesh, Maharashtra, Odisha, Rajasthan, Tamil Nadu, Uttar Pradesh, Uttarakhand, Assam, and Telangana.

For employment outcomes, we define *exposed* cohorts as individuals aged 22–30 at the time of quota implementation. The lower bound captures individuals who have completed schooling and are in the early stages of, or just entering, the labor market, ensuring that their human capital choices are largely predetermined. This minimizes concerns that the quota affected schooling or skill investment decisions and instead isolates exposure through changes in labor market opportunities. The upper bound reflects age-based participation patterns in public-sector jobs, which decline sharply after the early thirties and often involve formal age caps around 30–35, substantially limiting opportunities for older women. *Unexposed* cohorts are defined as individuals aged 38–50 at the time of implementation—an age group well past typical schooling and early-career stages and therefore unlikely to be directly affected by the reforms.

Long-run effects: For this analysis, we only use NSS rounds where the sample of observations are from 8 to 15 years after quota implementation. Relative to the short-run sample, this excludes Assam, which implemented the quota in 2005 and for which outcomes cannot be observed beyond seven years given that the NSS data end in 2011.

For employment outcomes, we define *exposed* cohorts as individuals who are at least 18 years old in the NSS round up to those who were 25 at the time of quota implementation. The lower bound captures individuals who are just entering the labor market after having spent much of their schooling years under the quota regime, allowing us to examine long-run effects operating through both human capital accumulation and labor market opportunities. The upper bound reflects ages at which individuals would still plausibly be preparing for or pursuing public-sector careers at the time of treatment, while trimming the upper range relative to the short-run sample to create a wider separation between exposed and unexposed cohorts. *Unexposed* cohorts consist of individuals who were 35 years old at the time of quota implementation up to those who are 55 years old in the NSS round—a group whose education

and early-career trajectories were fully determined before the policy.

3 Results: Labor market dynamics and human capital acquisition

In this section, we present both the short-run and long-run effects of quota adoption. The short-run results capture the immediate impacts of expanding women's access to public sector jobs. In contrast, the long-run results shed light on the more persistent impacts of the policy, including potential spillovers to other labor market outcomes and human capital acquisition.

3.1 Short-run Impacts

3.1.1 Public employment

Our analysis shows that the quota reforms led to a meaningful increase in women's likelihood of securing public-sector jobs in the short run. Table 1 presents the estimates from equation 1. Eligibility under the quota policy raises the probability of public employment for women by about 0.6–0.7 percentage points (Panel A), against a low mean of 2.66 percent among eligible women in never-treated states. Although the absolute increase is modest, it represents a sizable percentage gain (24–25%) given the very limited baseline representation of women in state-level public employment. These results suggest both administrative compliance with the quota rule and the creation and filling of new reserved vacancies.

Because most public sector jobs require at least a secondary education and often a college degree, we also consider effects separately by pre-quota education levels. Among high-education women, defined as having more than ten years of education, the probability of holding a public sector job increases by 3.2–3.7 percentage points, reflecting a 26–30% increase. Effects for low-education women are similar in percentage terms, though smaller in absolute terms given their lower baseline rates of public employment. These patterns suggest that the reform initially benefits those with the qualifications and exam readiness needed to compete for newly reserved positions.

In Figure 2, we estimate a dynamic version of equation 1 using the following specification:

$$Y_{isr} = \sum_{b \neq b_0} \beta_b \, \mathbf{1}\{b(i,r) = b \wedge Treated_{sr} = 1\} + X_{ir} + \mu_{sr} + \phi_{b(i,r),r} + \varepsilon_{isr}. \tag{2}$$

where $b(i,r)$ is the 3-year bin of age at quota adoption. We omit the 32–34 bin, b_0 (23–25 for years of education), and all coefficients are shown with respect to that.

Treatment effects on public employment are largest and statistically significant for women aged 11–19 at adoption, smaller and imprecise for those aged 20–25, and close to zero from the late 20s onward. Coefficients for older, ineligible cohorts hover near zero and are statistically insignificant. The absence of effects among older cohorts supports the validity of our

identification strategy.

3.1.2 Private and overall employment

We now investigate the short-run effects on private and overall employment for quota-eligible cohorts. The expansion in public sector opportunities could lead women to shift from private formal or informal jobs into newly available public-sector positions. This would have little net impact on overall employment outside the public sector. Alternatively, women who are out-of-labor force may choose to enter with the expansion in good job opportunities, increasing overall employment.

Table 2 examines the short-run spillover effects of the quota policy on: (i) employment outside public-sector jobs, disaggregated into private formal and informal, and (ii) overall employment, defined as the sum of public-sector, private formal, and informal employment.

For all women, estimates for private formal and informal employment are close to 0 and not significant, while overall employment rises by 1.0–1.4 percentage points, significant at the 10 percent level or better. Among high-education women, there is a positive and statistically significant effect on overall employment, with no change in private formal or informal employment. The magnitude of the overall employment effect is very similar to the increases in public-sector employment. For low-education women, overall employment rises by 1.1–1.5 percentage points, significant at the 10 percent level or better, while private formal employment does not change and informal employment rises only in the specification without 1987 controls (significant at the 10 percent level). In sum, we find no short-run spillovers into private formal employment; the remaining increase in overall employment beyond public-sector jobs comes through informal work and is imprecisely estimated.

3.1.3 Effects on Men

To assess whether women's short-run gains came at the expense of men, Tables 3 and 4 report the short-run effects of quota eligibility on men's employment outcomes. We find no effect on public-sector employment for all men combined or for low-education men. In contrast, high-education men—those most directly competing for the types of public-sector jobs affected by the quota—experience a significant decline of about 2.9–3.1 percentage points relative to a mean of 19.7 percent among eligible high-education men in never-treated states (Panel B of Table 3). However, this reduction is offset by sizable increases in private-sector employment: high-education men become 1.6–2.3 percentage points more likely to work in private formal jobs, and also experience gains in informal and overall employment. All men, and even low-education men, show positive increases in private formal employment as well. These results suggest that improved prospects for high-education women in public employment makes queuing for public-sector work less attractive for men and accelerates their reallocation toward private-sector jobs.

3.2 Long-run Impacts

3.2.1 Education

We now directly examine how quota policies affected human capital acquisition for women. Our definition of exposed cohorts for educational outcomes varies by measure. For years of education, exposed cohorts are aged 18 or younger at quota adoption and at least 12 when surveyed. For senior-secondary completion, they are aged 16 or younger at adoption and at least 17 when surveyed; for college completion, they are aged 19 or younger at adoption and at least 22 when surveyed. Comparison cohorts are aged 20–37 at adoption and satisfy the corresponding minimum survey-age requirement.

Table 5 presents the long-term effects of the quota reform on women's educational attainment. Panel A shows that the reform is associated with large and statistically significant increases in years of education by 0.8–0.9 years, relative to a mean of 6.9 years among eligible women in never-treated states. Panel B shows clear gains in senior secondary completion, with the probability increasing by 1.8 percentage points, statistically significant at the 1 percent level. Panel C shows that the probability of being a college graduate rises by 2.0 percentage points. Figure 3 shows the age-specific effects on education and gains are largest among younger cohorts and approach zero for the older cohorts.

Taken together, these long-run results indicate that the quota reform had substantial and broad-based effects on women's educational attainment. The emergence of significant gains in both secondary and college completion, along with the larger increase in overall years of schooling, suggests that the policy altered educational trajectories for younger cohorts in a sustained way. These findings are consistent with a human capital expansion channel, where improved prospects for public-sector employment incentivize greater investment in education over time, with benefits extending across multiple stages of the educational ladder.

3.2.2 Employment

Table 6 presents the long-term impacts of the quota reform on public-sector employment. Consistent with the short-run findings, we continue to observe positive and statistically significant effects on women's public employment, with magnitudes in the range of 0.6–0.9 percentage points. These estimates closely mirror the short-run effects, indicating that the initial gains in women's access to public-sector jobs persist over time. Figure 4 shows that these long-run gains are concentrated among women aged 13 or younger at adoption, with small and statistically insignificant coefficients for older cohorts.

Table 7 reports the long-run effects on women's employment across private formal, informal, and overall sectors. Unlike the short-run results, we now observe spillovers: quota eligibility leads to statistically significant increases in private formal employment of 0.23–0.29 percentage points, accompanied by larger significant declines in informal employment (1.3–1.7 percentage points). Yet, despite these shifts toward higher-quality jobs, the effect on overall employment remains small and statistically insignificant.

Figure 5 plots the age-specific long-term effects on private formal employment. The coefficients are generally positive and mostly significant for women aged 16 or younger at adoption—cohorts who spent much of their schooling under the quota and so had time to respond to the policy through educational investments—while effects are smaller and statistically insignificant for older cohorts. Together, these results suggest that in the long run, the quota reform's effects extend modestly into private formal employment, potentially through a human-capital expansion channel operating over time.

3.3 Who benefits? Heterogeneity by caste

An important question is whether gender quotas primarily expand opportunities for the most advantaged women or whether they also benefit women from historically marginalized caste groups. This concern is potentially mitigated by the fact that a distinctive feature of India's gender-quota system is that it is implemented *horizontally*: the female reservation applies within reservations for each caste category—OBC, SC, and ST—rather than creating a single pool of reserved seats for all women. This design should ensure that women from disadvantaged castes compete for reserved positions primarily against other women within their own caste category, which has the potential to generate meaningful gains for lower-caste women who might otherwise face steep competition from more privileged groups.

Figures 6 and 7 report long-run effects separately for ST, SC, and General/OBC (GOBC) women. ST women experience the largest gains in both public employment and years of schooling. SC women's education gains are at least as large as those of General/OBC women, but their gain in public employment is small and not statistically significant. These patterns suggest that the horizontal design of the quota can broaden access across the caste hierarchy, rather than simply reinforcing existing inequalities.

4 Conclusion

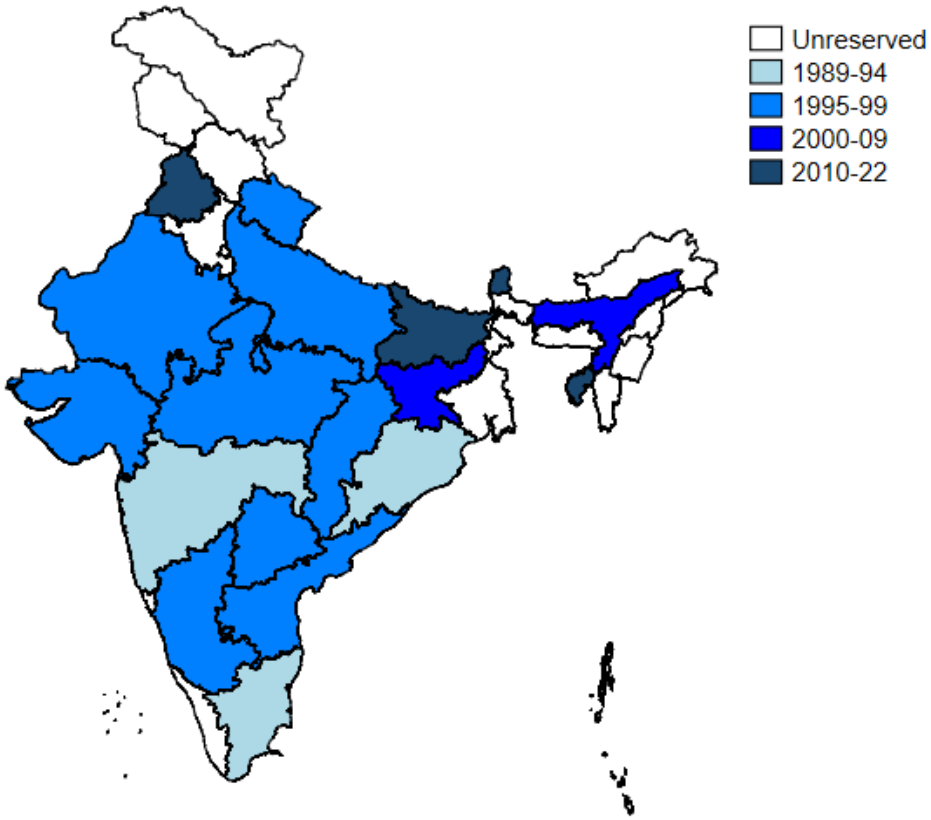
In this paper, we provide new evidence on the consequences of gender quotas in public-sector hiring. Our findings show that gender quotas durably expand women's access to high-quality public employment and stimulate substantial human-capital investments. We also document spillovers into private-sector labor markets: in the long run, women exposed to the quotas are more likely to hold formal private-sector jobs and less likely to work in informal employment. Although these spillover effects are modest, they indicate that quotas can shift women onto more stable employment trajectories. At the same time, we find no long-run impact on overall employment rates, highlighting that expanded access to better jobs does not necessarily translate into higher aggregate female labor force participation.

Several questions remain. First, which industries absorb the increase in more educated women entering the private sector, and what types of firms are able to substitute toward newly qualified female workers? Second, do these reforms reshape household and marriage-market dynamics—through changes in women's bargaining power, marriage timing, or part-

ner selection? Third, under what conditions might gender quotas generate aggregate employment gains, and what complementary policies are needed for improved job quality to translate into higher overall participation? We aim to address these questions in the future.

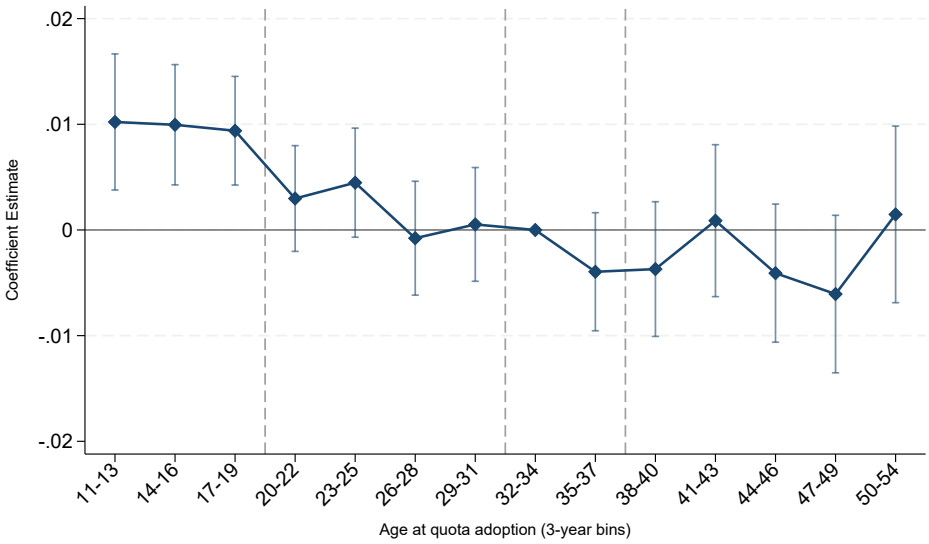
5 Figures

Figure 1: Adoption of gender quotas in public employment over time



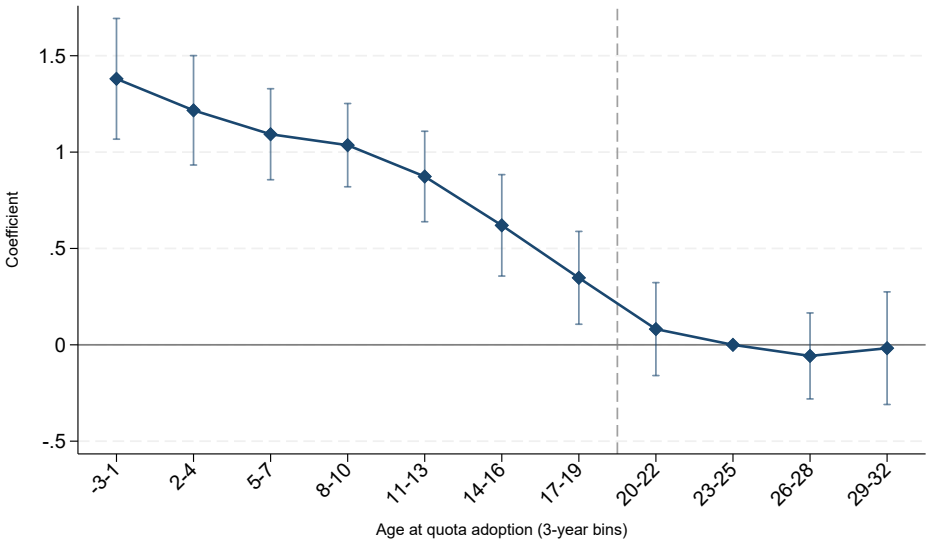
Notes: This figure shows the staggered adoption of gender quotas in public employment across Indian states. These quotas only apply state-level public employment and not those in the central government. Newly created states of Chhattisgarh, Uttarakhand, and Telangana are assigned the reservation year of their parent state. The timeline of reservations by states is as follows—1989-94: Tamil Nadu, Odisha, Maharashtra; 1995-99: Andhra Pradesh, Telangana, Gujarat, Karnataka, Madhya Pradesh, Rajasthan, Chhattisgarh, Uttar Pradesh, Uttarakhand; 2000-09: Jharkhand, Assam; 2010-22: Sikkim, Bihar, Punjab, Tripura.

Figure 2: Short Term Effect on Public Employment by Age



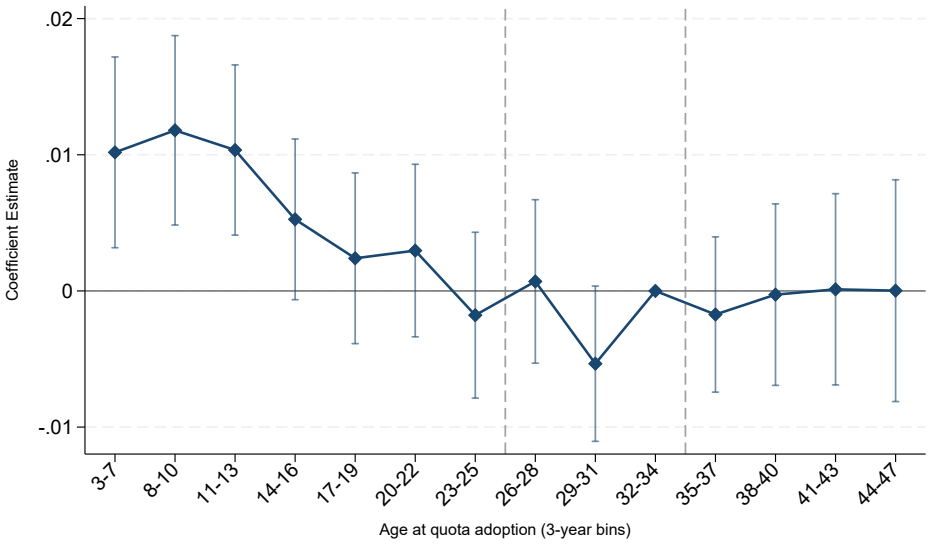
Notes: This figure presents the effects of the quota policy on women's public-sector employment in the short run, by age at quota adoption, estimated using equation 2. Age at quota adoption is grouped into 3-year bands spanning ages 11–54; for each band we estimate the difference in public-sector employment between women in currently treated states and women in the same band in never-treated states. The analysis uses data from the thick rounds of the NSS and includes fixed effects for state × stack and 3-year age-at-adoption band × stack, together with caste and religion fixed effects. Controls include effective household size, marriage status, region (rural or urban); and the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. The 32–34 band is the omitted category and is plotted at zero, so all coefficients are interpreted relative to it. Standard errors are clustered at the state × birth-cohort level, which is invariant across stacks so that control observations repeated in several stacks stay within a single cluster. These estimates come from the stacked cohort design; the staggered difference-in-differences in equation 3 instead traces effects by years since adoption.

Figure 3: Long Term Effect on Years of Education by Age



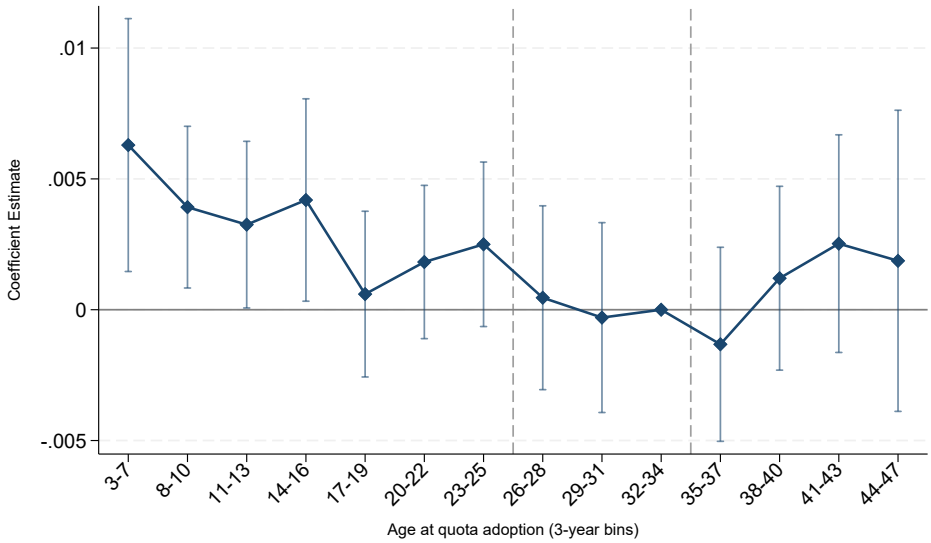
Notes: This figure presents the effects of the quota policy on women's completed years of education in the long run, by age at quota adoption, estimated using equation 2. Age at quota adoption is grouped into 3-year bands spanning −3 to 32; a negative value denotes a woman born after the law passed. For each band we estimate the difference in years of education between women in currently treated states and women in the same band in never-treated states (states that had not adopted a quota by 2011). The analysis uses data from the thick rounds of the NSS and includes fixed effects for state × stack and 3-year age-at-adoption band × stack, together with caste and religion fixed effects. Controls include effective household size, marriage status, region (rural or urban); and the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. The 23–25 band is the omitted category and is plotted at zero. It differs from the 32–34 band used in the employment figures only because this figure's range of age at adoption stops at 32. Standard errors are clustered at the state × birth-cohort level, which is invariant across stacks so that control observations repeated in several stacks stay within a single cluster. These estimates come from the stacked cohort design; the staggered difference-in-differences in equation 3 instead traces effects by years since adoption.

Figure 4: Long Term Effect on Public employment by Age



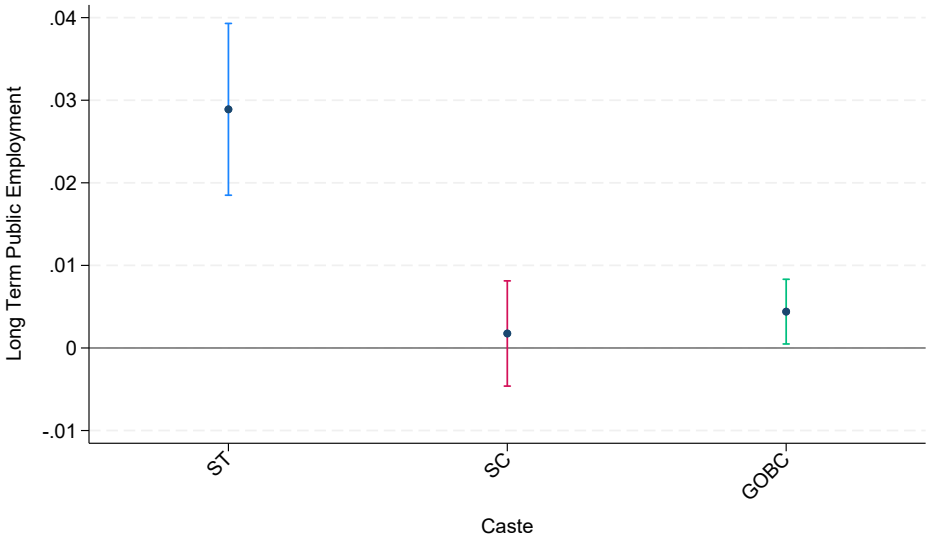
Notes: This figure presents the effects of the quota policy on women's public-sector employment in the long run, by age at quota adoption, estimated using equation 2. Age at quota adoption is grouped into 3-year bands spanning ages 3–47; for each band we estimate the difference in public-sector employment between women in currently treated states and women in the same band in never-treated states. The analysis uses data from the thick rounds of the NSS and includes fixed effects for state × stack and 3-year age-at-adoption band × stack, together with caste and religion fixed effects. Controls include effective household size, marriage status, region (rural or urban); and the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. The 32–34 band is the omitted category and is plotted at zero, so all coefficients are interpreted relative to it. Standard errors are clustered at the state × birth-cohort level, which is invariant across stacks so that control observations repeated in several stacks stay within a single cluster. These estimates come from the stacked cohort design; the staggered difference-in-differences in equation 3 instead traces effects by years since adoption.

Figure 5: Long Term Effect on Private Formal employment by Age



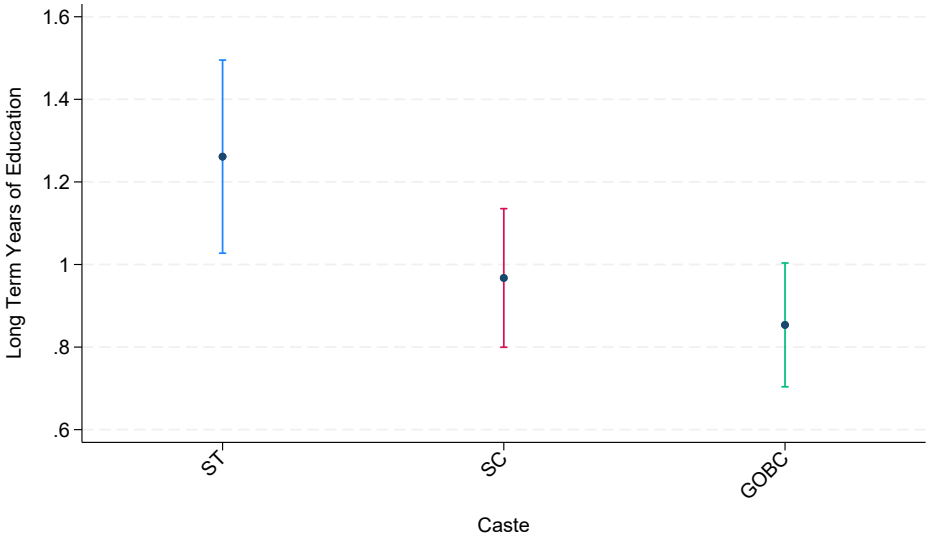
Notes: This figure presents the effects of the quota policy on women's private formal employment in the long run, by age at quota adoption, estimated using equation 2. Age at quota adoption is grouped into 3-year bands spanning ages 3–47; for each band we estimate the difference in private formal employment between women in currently treated states and women in the same band in never-treated states. The analysis uses data from the thick rounds of the NSS and includes fixed effects for state × stack and 3-year age-at-adoption band × stack, together with caste and religion fixed effects. Controls include effective household size, marriage status, region (rural or urban); and the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. The 32–34 band is the omitted category and is plotted at zero, so all coefficients are interpreted relative to it. Standard errors are clustered at the state × birth-cohort level, which is invariant across stacks so that control observations repeated in several stacks stay within a single cluster. These estimates come from the stacked cohort design; the staggered difference-in-differences in equation 3 instead traces effects by years since adoption.

Figure 6: Long Term Effect on Public Employment by Caste



Notes: This figure presents the effects of quota reform on women's long term public employment estimated using equation 1 separately for each caste. GOBC denotes General and OBC women combined. Exposed cohorts consist of individuals who were aged at least 18 in the NSS round and up to 25 at the time of policy implementation, while unexposed cohorts consist of individuals aged 35 at the time of policy implementation and up to 55 in the NSS round. The analysis uses data from thick samples of NSS between 1993 and 2011 and restricts data to 8-15 years post treatment for treated states. Controls include effective household size, marriage status, and region (rural or urban). We also include state-by-stack fixed effects, fixed effects for 3-year bins of age at quota adoption by stack, and religion fixed effects. All estimates control for the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. Standard errors are clustered at state X birth cohort level.

Figure 7: Long Term Effect on Education by Caste



Notes: This figure presents the effects of quota reform on women's years of education estimated using equation 1 separately for each caste. Exposed cohorts consist of individuals aged 18 or younger at the time of policy implementation and at least 12 in the NSS round. These definitions apply to both treated and control states. The unexposed cohort, common to both groups, comprises individuals aged 20 to 37 at the time of policy implementation. The analysis uses data from thick samples of NSS between 1993 and 2011 and restricts data to 8-15 years post treatment for treated states. Controls include effective household size, marriage status, and region (rural or urban). We also include state-by-stack fixed effects, fixed effects for 3-year bins of age at quota adoption by stack, and religion fixed effects. All estimates control for the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. Standard errors are clustered at state X birth cohort.

6 Tables

Table 1: Short-run Effects on Public Employment

	Public Employment	
	(1)	(2)
Panel A: All Women		
Eligible X Treated	0.0067***	0.0064***
	(0.0019)	(0.0019)
Observations	595,839	595,839
Never-Treated Eligible Mean	0.0266	0.0266
Control	Yes	Yes
Pre-period Controls	No	Yes
Panel B: High Education Women		
Eligible X Treated	0.0369**	0.0322**
	(0.0144)	(0.0144)
Observations	65,741	65,741
Never-Treated Eligible Mean	0.1222	0.1222
Control	Yes	Yes
Pre-period Controls	No	Yes
Panel C: Low Education Women		
Eligible X Treated	0.0034***	0.0032**
	(0.0013)	(0.0013)
Observations	529,655	529,655
Never-Treated Eligible Mean	0.0110	0.0110
Control	Yes	Yes
Pre-period Controls	No	Yes

Notes: This table presents the effects of quota reform on women's public employment estimated using specification 1. Exposed cohorts in treatment states consist of women aged 22 to 30 at time of quota adoption, while unexposed cohorts consist of women aged 38 to 50. The treated sample is restricted till 7 years after treatment. Control states include never treated states. The analysis uses data from thick samples of NSS between 1993 and 2011. Controls include effective household size, marriage status, and region (rural or urban). We also include state-by-stack fixed effects, fixed effects for 3-year bins of age at quota adoption by stack, and religion and caste fixed effects. In column 2, we control for the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. All specifications exclude respondents with no 1987 observations in their state, gender and age cell. Standard errors are displayed in parentheses and clustered at state *X* birth cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Short-run Effects on Private and Overall Employment

	Private Formal		Informal		Overall	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Women						
Eligible X Treated	0.0009	0.0013	0.0068	0.0023	0.0139**	0.0096*
	(0.0013)	(0.0013)	(0.0056)	(0.0055)	(0.0060)	(0.0058)
Observations	595,839	595,839	595,839	595,839	595,839	595,839
Never-Treated Eligible Mean	0.0163	0.0163	0.2763	0.2763	0.3178	0.3178
Control	Yes	Yes	Yes	Yes	Yes	Yes
Pre-period Controls	No	Yes	No	Yes	No	Yes
Panel B: High-Education Women						
Eligible X Treated	0.0007	-0.0017	0.0085	0.0062	0.0427**	0.0332*
	(0.0104)	(0.0105)	(0.0096)	(0.0098)	(0.0177)	(0.0177)
Observations	65,741	65,741	65,741	65,741	65,741	65,741
Never-Treated Eligible Mean	0.0930	0.0930	0.1032	0.1032	0.3161	0.3161
Control	Yes	Yes	Yes	Yes	Yes	Yes
Pre-period Controls	No	Yes	No	Yes	No	Yes
Panel C: Low-Education Women						
Eligible X Treated	0.0008	0.0009	0.0114*	0.0072	0.0153**	0.0112*
	(0.0008)	(0.0008)	(0.0060)	(0.0059)	(0.0061)	(0.0060)
Observations	529,655	529,655	529,655	529,655	529,655	529,655
Never-Treated Eligible Mean	0.0037	0.0037	0.3046	0.3046	0.3182	0.3182
Control	Yes	Yes	Yes	Yes	Yes	Yes
Pre-period Controls	No	Yes	No	Yes	No	Yes

Notes: This table presents the effects of quota reform on women's private and overall employment estimated using specification 1. Overall employment includes all types of employment: public, private formal, and informal. Exposed cohorts in treatment states consist of women aged 22 to 30 at time of quota adoption, while unexposed cohorts consist of women aged 38 to 50. The treated sample is restricted till 7 years after treatment. Control states include never treated states. The analysis uses data from thick samples of NSS between 1993 and 2011. Controls include effective household size, marriage status, and region (rural or urban). We also include state-by-stack fixed effects, fixed effects for 3-year bins of age at quota adoption by stack, and religion and caste fixed effects. In column 2, 4 and 6 we control for the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. All specifications exclude respondents with no 1987 observations in their state, gender and age cell. Standard errors are displayed in parentheses and clustered at state *X* birth cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Short-run Effects on Public Employment for Men

	Public Employment	
	(1)	(2)
Panel A: All Men		
Eligible X Treated	-0.0030	-0.0042
	(0.0046)	(0.0045)
Observations	604,364	604,364
Never-Treated Eligible Mean	0.0842	0.0842
Control	Yes	Yes
Pre-period Controls	No	Yes
Panel B: High Education Men		
Eligible X Treated	-0.0289***	-0.0311***
	(0.0109)	(0.0110)
Observations	128,325	128,325
Never-Treated Eligible Mean	0.1971	0.1971
Control	Yes	Yes
Pre-period Controls	No	Yes
Panel C: Low Education Men		
Eligible X Treated	-0.0038	-0.0035
	(0.0034)	(0.0035)
Observations	475,542	475,542
Never-Treated Eligible Mean	0.0503	0.0503
Control	Yes	Yes
Pre-period Controls	No	Yes

Notes: This table presents the effects of quota reform on men's public employment estimated using specification 1. Exposed cohorts in treatment states consist of men aged 22 to 30 at time of quota adoption, while unexposed cohorts consist of men aged 38 to 50. The treated sample is restricted till 7 years after treatment. Control states include never treated states. The analysis uses data from thick samples of NSS between 1993 and 2011. Controls include effective household size, marriage status, and region (rural or urban). We also include state-by-stack fixed effects, fixed effects for 3-year bins of age at quota adoption by stack, and religion and caste fixed effects. In column 2, we control for the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. All specifications exclude respondents with no 1987 observations in their state, gender and age cell. Standard errors are displayed in parentheses and clustered at state *X* birth cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Short-run Effects on Private and Overall Employment for Men

	Private Formal		Informal		Overall	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Men						
Eligible X Treated	0.0195***	0.0159***	-0.0073	-0.0078	0.0099***	0.0051
	(0.0041)	(0.0038)	(0.0067)	(0.0064)	(0.0032)	(0.0032)
Observations	604,364	604,364	604,364	604,364	604,364	604,364
Never-Treated Eligible Mean	0.0946	0.0946	0.7429	0.7429	0.9146	0.9146
Control	Yes	Yes	Yes	Yes	Yes	Yes
Pre-period Controls	No	Yes	No	Yes	No	Yes
Panel B: High-Education Men						
Eligible X Treated	0.0232**	0.0163	0.0321***	0.0305***	0.0279***	0.0173***
	(0.0105)	(0.0105)	(0.0098)	(0.0098)	(0.0064)	(0.0062)
Observations	128,325	128,325	128,325	128,325	128,325	128,325
Never-Treated Eligible Mean	0.3069	0.3069	0.3375	0.3375	0.8358	0.8358
Control	Yes	Yes	Yes	Yes	Yes	Yes
Pre-period Controls	No	Yes	No	Yes	No	Yes
Panel C: Low-Education Men						
Eligible X Treated	0.0097***	0.0089***	-0.0021	-0.0045	0.0047	0.0023
	(0.0026)	(0.0025)	(0.0050)	(0.0052)	(0.0030)	(0.0030)
Observations	475,542	475,542	475,542	475,542	475,542	475,542
Never-Treated Eligible Mean	0.0309	0.0309	0.8647	0.8647	0.9384	0.9384
Control	Yes	Yes	Yes	Yes	Yes	Yes
Pre-period Controls	No	Yes	No	Yes	No	Yes

Notes: This table presents the effects of quota reform on men's private and overall employment estimated using specification 1. Overall employment includes all types of employment: public, private formal, and informal. Exposed cohorts in treatment states consist of men aged 22 to 30 at time of quota adoption, while unexposed cohorts consist of men aged 38 to 50. The treated sample is restricted till 7 years after treatment. Control states include never treated states. The analysis uses data from thick samples of NSS between 1993 and 2011. Controls include effective household size, marriage status, and region (rural or urban). We also include state-by-stack fixed effects, fixed effects for 3-year bins of age at quota adoption by stack, and religion and caste fixed effects. In column 2, 4 and 6 we control for the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. All specifications exclude respondents with no 1987 observations in their state, gender and age cell. Standard errors are displayed in parentheses and clustered at state *X* birth cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Long-run Effects on Education

Panel A:	Years of Education	
Eligible X Treated	0.7609***	0.9064***
	(0.0684)	(0.0670)
Observations	2,156,795	2,156,795
Never-Treated Eligible Mean	6.8619	6.8619
Control	Yes	Yes
Pre-period Controls	No	Yes
Panel B:	Secondary Education	
Eligible Sr Sec X Treated	0.0184***	0.0184***
	(0.0061)	(0.0062)
Observations	1,650,210	1,650,210
Never-Treated Eligible Mean	0.2556	0.2556
Control	Yes	Yes
Pre-period Controls	No	Yes
Panel C:	College Graduates	
Eligible College X Treated	0.0196***	0.0196***
	(0.0034)	(0.0034)
Observations	1,489,406	1,489,406
Never-Treated Eligible Mean	0.1244	0.1244
Control	Yes	Yes
Pre-period Controls	No	Yes

Notes: This table presents the effects of quota reform on women's education estimated using specification in 1. Exposed cohorts for years of education includes individuals aged 18 or younger at the time of policy implementation and at least 12 in the NSS round; for secondary school completion, those aged 16 or younger at implementation and at least 17 in the NSS round; and for college completion, those aged 19 or younger at implementation and at least 22 in the NSS round. These definitions apply to both treated and control states. The unexposed cohort, common to both groups, comprises individuals aged 20 to 37 at the time of policy implementation. The analysis uses data from thick samples of NSS between 1993 and 2011 and restricts data to 8-15 years post treatment for treated states. Controls include effective household size, marriage status, and region (rural or urban). We also include state-by-stack fixed effects, fixed effects for 3-year bins of age at quota adoption by stack, and religion and caste fixed effects. In column 2 we control for the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. All specifications exclude respondents with no 1987 observations in their state, gender and age cell. Standard errors are displayed in parentheses and clustered at state *X* birth cohort. * p $<$ 0.10, ** p $<$0.05, *** p$<$0.01.

Table 6: Long-run Public Employment

	Public Employment	
	(1)	(2)
Eligible X Treated	0.0088***	0.0066***
	(0.0019)	(0.0018)
Observations	1,562,622	1,562,622
Never-Treated Eligible Mean	0.0273	0.0273
Control	Yes	Yes
Pre-period Controls	No	Yes

Notes: This table presents the effects of quota reform on women's long term public employment estimated using specification 1. Exposed cohorts consist of individuals who were aged at least 18 in the NSS round and up to 25 at the time of policy implementation, while unexposed cohorts consist of individuals aged 35 at the time of policy implementation and up to 55 in the NSS round. The analysis uses data from thick samples of NSS between 1993 and 2011 and restricts data to 8-15 years post treatment for treated states. Controls include effective household size, marriage status, and region (rural or urban). We also include state-by-stack fixed effects, fixed effects for 3-year bins of age at quota adoption by stack, and religion and caste fixed effects. In column 2, we control for the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. All specifications exclude respondents with no 1987 observations in their state, gender and age cell. Standard errors are displayed in parentheses and clustered at state X birth cohort level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Long-run Private and Overall Employment

	Private Formal		Informal		Overall	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible X Treated	0.0029**	0.0023*	-0.0126**	-0.0172***	-0.0008	-0.0080
	(0.0012)	(0.0012)	(0.0061)	(0.0059)	(0.0065)	(0.0062)
Observations	1,562,622	1,562,622	1,562,622	1,562,622	1,562,622	1,562,622
Never-Treated Eligible Mean	0.0145	0.0145	0.2185	0.2185	0.2579	0.2579
Control	Yes	Yes	Yes	Yes	Yes	Yes
Pre-period Controls	No	Yes	No	Yes	No	Yes

Notes: This table presents the effects of quota reform on women's long term employment estimated using specification 1. Exposed cohorts consist of individuals who were aged at least 18 in the NSS round and up to 25 at the time of policy implementation, while unexposed cohorts consist of individuals aged 35 at the time of policy implementation and up to 55 in the NSS round. The analysis uses data from thick samples of NSS between 1993 and 2011 and restricts data to 8-15 years post treatment for treated states. Controls include effective household size, marriage status, and region (rural or urban). We also include state-by-stack fixed effects, fixed effects for 3-year bins of age at quota adoption by stack, and religion and caste fixed effects. In column 2, 4 and 6 we control for the 1987 (pre-period) employment rate and fraction with lower secondary education or above, computed for each state, gender and single year of age and assigned to respondents by their state, gender and age at the time of the survey. All specifications exclude respondents with no 1987 observations in their state, gender and age cell. Standard errors are displayed in parentheses and clustered at state *X* birth cohort level. * p < 0.10, ** p <0.05, *** p<0.01.

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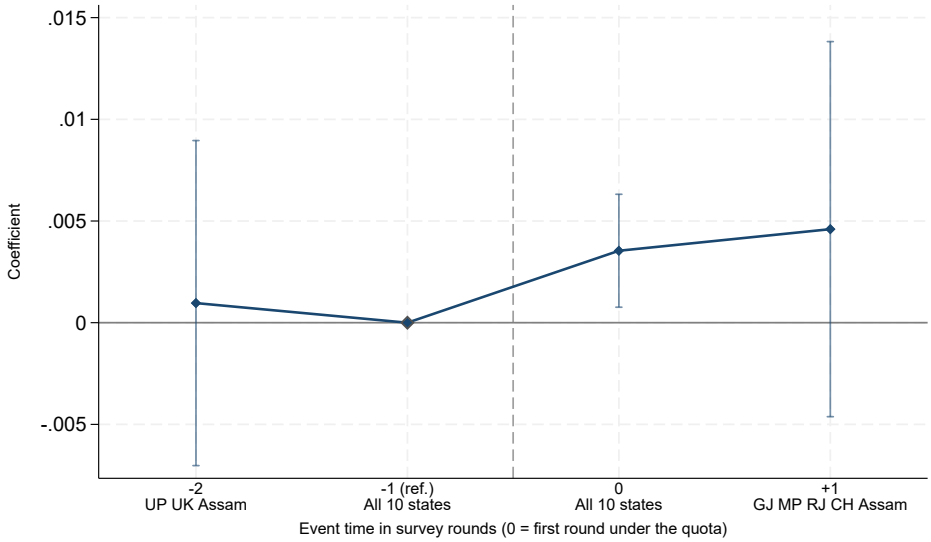
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ONLINE APPENDIX

Figure A1: Staggered Difference-in-Differences: Public Employment by Event Time



Notes: This figure plots the event-study coefficients β_k from equation 3 for women's public-sector employment, using the staggered timing of quota adoption across states rather than variation across cohorts. Event time is measured in survey rounds, where round 0 is the first round a state is observed under the quota and round -1, the last round before adoption, is the omitted category plotted at zero. The states contributing to each coefficient are listed beneath the corresponding point. The specification absorbs state-by-event and age-by-round-by-event fixed effects, together with caste and religion fixed effects, and controls for region (rural or urban), marital status, and household size. The sample is women aged 22 and over, observed at the same ages before and after adoption: post-adoption rounds keep women aged 30 or under at adoption, and pre-adoption rounds keep the matching range of ages. Bars show 95 percent confidence intervals and standard errors are clustered by state. Public-sector status is not collected in the 1987 or 2005 rounds, so Tamil Nadu and Odisha, which then have no pre-adoption round, are excluded.

Staggered difference-in-differences: As a complement to the cohort design, we exploit the timing of adoption across states in calendar time. We construct one event e for each adopting state, consisting of that state together with all fifteen never-treated states, over the NSS rounds falling between six years before and seven years after the state's adoption year L_e.[1] Because the rounds are unevenly spaced, we measure event time in survey rounds rather than calendar years: round -1 is the last round before adoption, round 0 the first round observed under the quota, and round $+1$ the second. Round 0 falls two to five calendar years after adoption and round $+1$ four to seven, depending on the event. We estimate

$$Y_{iset} = \sum_{k \in \{-2, 0, +1\}} \beta_k \, Treated_{se} \times \mathbf{1}\{Round_{et} = k\} + \mu_{se} + \phi_{aet} + X_i + \varepsilon_{iset}, \tag{3}$$

where $Treated_{se}$ indicates the adopting state in event e; μ_{se} are state-by-event fixed effects; and ϕ_{aet} are age-by-round-by-event fixed effects, which within an event-round identify a single birth cohort and hence a single age at treatment, defined for control states that have no law year of their own. X_i contains region (rural or urban), marital status, and household size, along with caste and religion fixed effects. Round -1 is the omitted category, so each β_k is read relative to the last round before the quota. Standard errors are clustered by state, so a comparison state that appears in several events remains a single cluster.

We restrict the sample to women aged 22 and over. In post-adoption rounds we keep

[1] States that adopted a quota of less than 20 percent are assigned to the never-treated group.

those aged 30 or under at the time of adoption; in pre-adoption rounds we keep the age range that matches the event's post-adoption span, so the same ages are compared before and after the law. Outcome availability determines how many events each estimate uses. Public-sector status is not collected in the 1987 or 2005 rounds, which leaves Tamil Nadu and Odisha without a pre-adoption round and excludes them, giving ten events.

This design carries a limitation the stacked cohort design does not. The panel is unbalanced: the rounds available around adoption differ across states, so the set of states contributing to each β_k changes across the event window. All states enter at rounds -1 and 0, but only Uttar Pradesh, Uttarakhand, and Assam are observed two rounds before adoption, and only a subset is observed a second round after. Coefficients away from adoption therefore rest on fewer states than those adjacent to it, and are not estimated on a common sample. We report these estimates as a complement to the cohort design rather than as our primary specification.