

# Do the Effects of Temporary Ethnic Group Quotas Persist? Evidence from India

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## Abstract

Do electoral quotas for ethnic groups continue to improve their chances of winning elections after quotas are withdrawn? This is an important question since ethnic group quotas are common, and are often intended to be temporary. Using natural experiments, I find that electoral quotas for India’s “scheduled castes” (SCs) fail to boost SCs’ chances of winning office after they are discontinued. These results contrast with the significant positive effects of past women’s quotas found in similar contexts.

Electoral quotas—the requirement that contestants for office be members of a particular group—are used to improve the descriptive representation of women and ethnic groups in over 100 countries across the world. While quotas often boost the descriptive representation of target groups while they are in place ([Krook 2009](#)), do their beneficial effects persist after they are withdrawn? This is an important question, because quotas—much like affirmative action programs, and due to liberal ideal that ascriptive identities should be irrelevant to politics—are often intended to be

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temporary, used to right historical wrongs.<sup>1</sup> Estimating the effects of quotas after they are discontinued therefore allows us to understand whether electoral quotas can truly be temporary, in that they ensure the continued representation of target groups even after they are removed.<sup>2</sup>

While a literature has shown that electoral quotas for women increase the proportion of women elected to office even after they are withdrawn (Beaman et al. 2009; Bhavnani 2009; Deininger et al. 2011; Paola, Scoppa and Lombardo 2010), in contexts as diverse as Italy and India, I turn to asking whether positive effects obtain for past electoral quotas for ethnic groups. By doing so, this work joins a small literature that has explored the long term effects of hiring quotas for minorities in the United States (Miller and Segal 2012; Miller 2015). Since women and ethnic minorities differ in many ways, it is by no means obvious that electoral quotas for ethnic groups would have downstream effects. I therefore turn to investigating the degree to which past electoral quotas for ethnic groups continue to work after they lapse.

In this paper, I examine the downstream effects of a particular type of quota—reservations—for India’s erstwhile untouchables, or “scheduled castes” (SCs).<sup>3</sup> SCs are a historically disadvantaged group—arguably akin to African-Americans in the United States<sup>4</sup>—that constitute 16 percent (180 million) of India’s population, and electoral reservations for their benefit have been in place since 1937. Since only SCs are allowed to run for office in seats reserved for them, reservations guarantee that SCs are elected while they are in place. However, whether reservations for SCs continue to boost the chances of SCs winning office after reservations are withdrawn is not known. This is a particularly consequential question, since SCs suffer uniquely humiliating and entrenched discrimination (Pandey 2013; Shah et al. 2006; Thorat and Newman 2010), and reservations—in elections,

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<sup>1</sup>For example, the U.S. Supreme Court “expects that 25 years from now, the use of racial preferences will no longer be necessary” in college admissions ( 2003, 343). A notable exception to this pattern are ethnic quotas in consociational systems, which are often intended to be permanent (Lijphart 1977).

<sup>2</sup>Quotas could also be temporary if bottlenecks to the representation of target groups are alleviated for reasons other than those having to do with quotas.

<sup>3</sup>SCs are so-called because a list of them is appended to a “schedule” or appendix to the Indian constitution.

<sup>4</sup>See, for example, Berreman (1960) and Pandey (2013). For a contrasting view, see Beteille (1990).

colleges and for government jobs—are the primary means through which the state has sought to improve SCs’ position.<sup>5</sup> India’s experience with reservations—starting in 1937—is also among the world’s oldest.<sup>6</sup> Further, reservations for SCs have explicit sunset provisions, which have been extended several times, on the untested assumption—in legislation<sup>7</sup> and scholarship (Joshi 1980; Mendelsohn and Vicziany 1998)—that reservations continue to be necessary for the election of SCs. I put this assumption to the test.

The literature suggests two mechanisms by which past reservations for SCs might work. First, past reservations might create incumbents that are able to use office to perpetuate their hold on power. Bhavnani (2009) shows this to be the case with regard to reservations for women in the Indian city of Mumbai. Indeed, incumbents in India have substantial resources with which to help perpetuate their hold on power. These include direct control over discretionary grants (Keefer and Khemani 2009), and influence over the allocation of fiscal (Khemani 2003; Rodden and Wilkinson 2004) and bureaucratic resources (Iyer and Mani 2012).<sup>8</sup> A second mechanism by which reservations for SCs might work is by exposing people to SC leaders. If voters have a “taste” for discriminating against SC candidates, or discriminate against them “statistically” (Becker 1957), the reservations-induced exposure of voters to SCs could give voters an opportunity to update their opinions about SC leaders. This mechanism has been shown to work in the case of reservations for women in the Indian state of West Bengal (Beaman et al. 2009) and Italy (Paola, Scoppa and Lombardo 2010). Consistent with this possibility, Chauchard (2014) has shown that the behavioral intentions of non-

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<sup>5</sup>Certainly, reservations for SCs were instituted for a number of other reasons, including as a way of keeping the Indian independence movement together.

<sup>6</sup>Reserved seats were introduced following the Poona Pact of 1932.

<sup>7</sup>The recent 109th Constitutional Amendment Act, which extended legislative reservations for SCs until 2020, for example, notes in its “statement of objects and reasons” that “although the Scheduled Castes ... have made considerable progress in the last sixty years, the reasons which weighed with the Constituent Assembly in making provisions with regard to the aforesaid reservation of seats ... have not ceased to exist.”

<sup>8</sup>It is worth noting that India’s legislators have been shown to experience an incumbency disadvantage (Uppal 2009), insofar as incumbents are thought to be disadvantaged when matched with *equally qualified* challengers. However, most incumbents are far more qualified than their challengers. Reflecting this, incumbents have a much higher election rate in the data (49 percent) than non-incumbents (6 percent).

SC citizens towards SC citizens are improved by exposure to SC leaders in the Indian state of Rajasthan.

That said, we have reasons to both doubt that SC incumbents will be reelected without reservations, and that exposure to SCs will reduce bias against SC candidates. For example, the fact that SCs lack family ties to largely upper caste parties<sup>9</sup> (such ties are thought to increase incumbents' chances of renomination and their vote shares—see [Bohlken and Chandra 2016](#)) might reduce their reelection rate. Also, SCs are a geographically dispersed<sup>10</sup> and largely endogamous group.<sup>11</sup> Both attributes will reduce the degree of contact between SCs and others. If the ability of past reservations to reduce bias is increasing in contact, past reservations for SCs might be ineffective. Yet another reason that SC incumbents are unlikely to be reelected is that few SCs are given charge of important ministries (while 2.7 percent of non-SCs were in charge of major ministries,<sup>12</sup> only 1 percent of SCs held these positions). The lack of executive power might lessen the degree to which SC incumbents can ensure their reelection, and might also—since it reduces voter exposure to SC leaders—reduce the degree to which reservations could reduce bias. In sum, we have reasons to both believe in and to doubt the efficacy of previous SC reservations. We therefore empirically examine the effects of past reservations here.

Estimating the downstream effects of reservations is difficult since reservations have been infrequently withdrawn, and since they are invariably introduced and withdrawn for political reasons, which might bias the estimated effects of reservations. I therefore examine the downstream effects of reservations for SCs using two natural experiments, wherein some reserved seats in India's state legislatures were quasi-randomly declared to be "open" in 1974 and 2008 due to the necessity of reserving whole rather than fractions of seats, even as reservations remained in place and were extended elsewhere. Some noteworthy aspects of the study are that it uses data from all of India's

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<sup>9</sup>Although, as [Chauchard \(2015\)](#) points out, SCs do have some dynastic ties to parties.

<sup>10</sup>SCs formed more than 50 percent of the population in less than 1 percent of the constituencies in the data.

<sup>11</sup>In fact, SCs are themselves made up of several sub-groups or *jatis*, which are endogamous.

<sup>12</sup>Major ministries are defined as those that oversee the home, agriculture, commerce, finance, industry and law departments.

largest states, and that it (specifically, the natural experiment due to the 1974 redistricting or delimitation) allows for the estimation of the long-term effects of the withdrawal of reservations, over 30 years.

The outcome I focus on is the degree to which SC candidates are elected to office after reservations are withdrawn. I focus on this outcome for three reasons. First, since electing more SCs is perhaps the most immediate goal of reservations. Second, since reservations, while they are in effect, unambiguously increase the number of SCs elected to office, even as their effects on socio-economic outcomes are less certain to non-existent ([Chin and Prakash 2011](#); [Jensenius 2015](#); [Pande 2003](#)). If reservations for SCs fail to improve socio-economic outcomes while they are in place, they are presumably even less likely to improve socio-economic outcomes after they are withdrawn. And third, since this focus follows the literature on the effects of past quotas for women ([Beaman et al. 2009](#); [Bhavnani 2009](#); [Paola, Scoppa and Lombardo 2010](#)).

Despite the long history of reservations in India, I find that reservations for SCs fail to impact SCs' electoral outcomes after their withdrawal, in the short or long terms. These results contrast with the significant effects of temporary women's reservations found elsewhere. Past reservations fail to have positive effects after they are withdrawn partially because SC incumbents do not rerun for office. This is not due to a number of observable differences between SC and non-SC incumbents or their constituencies, leaving open the possibility that SC incumbents are forced out due to their poor performance in office, or because parties and/or voters discriminate against SCs. The poor performance of SCs in office might also explain why exposure to SC leaders does not appear to reduce bias.

By examining the downstream effects of quotas for SCs, this paper advances a substantial literature on the efficacy of a common electoral institution—electoral quotas—for ethnic groups and for SCs, in particular ([Chin and Prakash 2011](#); [Dunning 2010](#); [Dunning and Nilekani 2013](#); [Jensenius 2015](#); [Pande 2003](#)). It does so by examining the effects of past reservations on the electoral prospects of ethnic groups after the quotas are withdrawn. It also contributes to a newer literature that examines differences between gender and ethnic group quotas ([Bjarnegård and Zetterberg](#)

2014; Htun 2004; Krook and O’Brien 2010; Phillips 1995). The paper provides a vivid illustration of the difficulty of changing the distribution of political power, and has policy implications. I elaborate on these aspects later.

I proceed by detailing the empirical strategy and data that I employ to estimate the downstream effects of reservations for SCs (section I). I then present the main results—that reservations for SCs have statistically hard-to-detect effects on the chances of SCs winning elections after they are withdrawn—robustness tests, and a discussion of the reasons for the non-effect of reservations (section II). I conclude with a discussion of implications (section III).

## I Empirical Strategy and Data

Two features of electoral quotas make their lasting effects hard to estimate. First, quotas have been rarely withdrawn once they are introduced, leaving us with few instances with which to assess their downstream effects.<sup>13</sup> Second, in instances where quotas are withdrawn, they are withdrawn for explicitly political reasons, which makes it difficult to recover unbiased estimates of the effects of previous quotas. In the context of India, we might expect non-SCs to remove reservations in precisely those places where they (non-SCs) would win office. This would attenuate the estimated effects of past reservations.

To circumvent these problems, I leverage two natural experiments in India, where some of the single-member seats that make up the country’s state legislatures were reserved or set aside for SCs on an apolitical, formulaic basis. The identification of the effects of past reservations is possible for two reasons. First, the periodic *de novo* implementation of the reservations assignment algorithm has occasionally led to the removal of some reservations (and the continued implementation of some, and extension of others). And second, since we are able to isolate plausibly exogenous variation in past reservations due to the need to reserve whole (rather than fractions of) seats for

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<sup>13</sup>Denmark, Egypt, India, Italy and Pakistan have, however, eliminated some electoral quotas. India, specifically, entirely eliminated quotas for women, Muslims and Christians in its state and national legislatures in 1950.

SCs.

Consider the process by which seats are reserved for SCs. Following the requirements of the constitution, the Indian parliament appointed independent delimitation commissions<sup>14</sup> in 1962, 1972 and 2002 to redraw the boundaries for the single-member territorial constituencies or seats that India's state legislatures (also known as legislative assemblies or *Vidhan Sabhas*) are composed of. The delimitation commissions were simultaneously required to set aside or reserve a subset of the newly-drawn constituencies for SCs.<sup>15</sup> The decisions of the 1972 commission were implemented from 1974 on, and the decisions of the 2002 commission were implemented from 2008 on.

The delimitation commissions operated as follows. First, the number of seats reserved for SCs in each state was calculated by setting the proportion of reserved seats equal to the proportion of SCs in the state population, with seat fractions rounded to the nearest integer. Population figures used for the 1962, 1972 and 2002 delimitations were from the 1961, 1971 and 2001 censuses, respectively. Second, seats were allocated across administrative districts (into which states are divided—not by the delimitation commission, but through long historical processes; during the 1962 delimitation, the median district had nine assembly constituencies) by setting the proportion of seats assigned to a district equal to the proportion of the state's population in that district, and rounding the fractional seats using the largest remainder method.<sup>16</sup> Third, the number of seats reserved for SCs in each administrative district was calculated by multiplying the number of SC seats in the state, calculated in step 1, by the proportion of the state's SC population in that district, and rounding the fractional seats using the largest remainder method. Fourth, within each district,

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<sup>14</sup>These commissions are generally acknowledged to be independent (Iyer and Shivakumar 2012; Singh 2000). See Bueno de Mesquita (1978) for a dissenting view on the 1962 commission.

<sup>15</sup>Although this delimitation process was supposed to have been conducted every decade, delimitations were suspended from 1974 and until 2002, so as to not reward regions with high population growth rates with enhanced representation.

<sup>16</sup>In this method, districts are first assigned the unrounded, whole number of seats obtained by setting the proportion of seats assigned to a district equal to the proportion of the state's population in that district. Next, districts with the largest remainders are assigned seats until all the seats have been allocated.

constituencies were drawn so as to have equal populations. Seats were then set aside or reserved for SCs until the target number of reserved constituencies, calculated in the second step, was reached. This last step introduced some discretion to the reservation process, both due to the method by which constituencies were drawn, and in the choice of constituencies to be reserved for SCs.<sup>17</sup>

The straightforward calculation of the subsequent effects of reservations—comparing electoral outcomes in seats that were and are not reserved (the control group) with constituencies that were but are not reserved (the treatment group)—is complicated by three factors. First, the delimitation process oftentimes changed constituency boundaries substantially, which makes constituencies non-comparable over time. Second, and as detailed in the last step above, delimitation commissions had some discretion in the drawing and reservation of constituencies within districts. This potentially biases any comparison of constituencies based on their reservation status. Both problems may be addressed by “going up” a level, that is, by aggregating and analyzing electoral outcomes at the district level. This is the case since although the drawing of constituency boundaries within a district could, in principle, be politicized, the delimitation commissions were mandated to take existing administrative district boundaries—which were largely historically determined—and then draw electoral constituencies within them. Further, the determination of the number of reserved seats in a district was done on a strictly apolitical basis, and as per steps 1–3, above.<sup>18</sup> An additional advantage of a district-level analysis is that, by pooling observations, it accounts for the potential spillover effects of reservations within districts. Such spillover effects would obtain, for example, if reservations in one constituency make voters more tolerant of SCs in other constituencies in the same district.<sup>19</sup>

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<sup>17</sup>There is some guidance for both these steps. Constituencies were drawn in a district starting from “North to North-West and then proceeding in a zig-zag manner to end at the Southern side,” and while keeping existing sub-district administrative boundaries intact. Constituencies in a district are to be reserved for SCs starting with the constituency with the highest concentration of SCs, but while ensuring some geographical spread. See [http://eci.nic.in/delim/Procedure/Delimitation of Constituencies.pdf](http://eci.nic.in/delim/Procedure/Delimitation%20of%20Constituencies.pdf) for details.

<sup>18</sup>A replication of these steps for the 2008 delimitation indicates that they were followed perfectly.

<sup>19</sup>In principle, an alternative strategy would be to “go down” a level. However, this would necessitate a change in the dependent variable, possibly to the proportion of panchayat or village

To statistically identify the impact of past reservations, I isolate the quasi-random portion of past reservations that is due to the application of the largest remainder rule.<sup>20</sup> This rule was applied since fractions of seats cannot be assigned to districts. To estimate the impact of present and past reservations, I estimate:

$$Y_{i,t} = \alpha + \beta R_{i,PRE} + \gamma R_{i,POST} + \phi F_{i,PRE} + \zeta F_{i,POST} + \eta X_{i,t} + \delta_{s,t} + \varepsilon_{i,t} \quad (1)$$

where  $Y_{i,t}$  is the proportion of SCs elected in district  $i$  in post-redistricting year  $t$ ,  $R_{i,POST}$  is the proportion of reserved seats after redistricting,  $R_{i,PRE}$  is the proportion of reserved seats before redistricting,  $F_{i,POST}$  is the proportion of seats that would have been reserved after redistricting if it were possible to reserve seat fractions, and  $F_{i,PRE}$  is the proportion of seats that would have been reserved before redistricting if it were possible to reserve seat fractions. The control set  $X_{i,t}$  includes current population share of SCs, since this might directly affect the chances of an SC being elected, the number of seats in the district in the present and the past, and land area.  $\delta_{s,t}$  is a vector of state-year fixed effects. Standard errors are clustered by state-year.  $\beta$ , the coefficient on  $R_{i,PRE}$ , is the estimated effect of past reservations.

It is worth underlining that the downstream effects of past reservations are estimated using instances where reservations were withdrawn even as (and this is an important caveat) reservations remained in place elsewhere, and were newly implemented in still other places. This estimate is of policy interest, since the existence of reservations is changeable in precisely the constituencies under examination. SCs are sufficiently concentrated in these areas for the implementation of reservations to be contested. A related estimate of theoretical interest—which I am unable to recover here, but I discuss later—is the effects due to the withdrawal of all reservations.<sup>21</sup>

Since reservations were implemented anew in 1974 and 2008, we have two natural experiments with which to examine the effects of past reservations. I analyze these episodes separately since

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cluster leaders that are SCs.

<sup>20</sup>See footnote 16 for a description of this rule.

<sup>21</sup>I say theoretical interest, since the complete withdrawal of reservations is not politically feasible at the present, despite an explicit sunset constitutional provision for these quotas.

some of the country's states and districts were split between 1974 and 2008, and since—as I note below—the dependent variables that I am able to use for these analyses are slightly different. In the case of the 1974 redistricting, I use the districts in 1974 as the unit of analysis, and examine the effects of reservations in 1965–1973 on election outcomes in 1974–2007.<sup>22</sup> In this period, there were 22 districts with fewer reserved seats after the delimitation than before.<sup>23</sup> Since the panel structure of the data allows me to repeatedly observe districts over time, there are a total of 158 district-years with fewer constituencies reserved for SCs than before 1974. In the case of the 2008 redistricting, I use the districts as of 2008 as the units of analysis,<sup>24</sup> and examine the effects of reservations that were in place in 1974–2007 on elections between 2008–2012. In this instance, there were 34 districts with fewer reservations than in the past.<sup>25,26</sup>

The 1974 and 2008 redistrictings provide us with sufficient statistical power ( $\geq .8$ ) with which to detect small (say 2 percentage point)<sup>27</sup> increases in the probability of SCs winning office due to past reservations.<sup>28</sup> This is because, although the treatment groups for the 1974 and 2008 experiments have 22 and 34 districts, respectively, the control groups for the experiments are much larger (290 and 245 districts, respectively), and since the 1974 analysis includes 6–8 follow-up measurements. For example, a randomized control trial (RCT) with 22 treatment districts, 290

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<sup>22</sup>Data for districts split after 1974 were assigned to their original 1974 districts.

<sup>23</sup>Reservations increased in a few districts in 1976–80, due to the removal of “area restrictions” on the counting of SCs. Prior to this, some SCs were only counted as SCs if they resided in particular regions. The removal of area restrictions increased the SC population count, which increased the SC seat assignments. Since India's states have elections on a rolling basis, these changes were implemented as a part of the 1974 delimitation in most states.

<sup>24</sup>Data for districts split after 2008 were assigned to their original 2008 districts.

<sup>25</sup>The 2008 delimitation changed the allocation of reservations to a greater degree than the 1974 delimitation, since while the latter had occurred after 10 years (the delimitation prior to 1974 had used the 1961 census results), the 2008 delimitation occurred after 30 years.

<sup>26</sup>The 1974 dataset contains data for 14 states, encompassing over 90 percent of the population in 1974. The 2008 dataset is smaller—covering 10 states and 61 percent of the population—since, as of writing, some states have not had their first post-delimitation elections.

<sup>27</sup>I use 2 percentage points for this exercise since this is the point estimate for the effects of past reservations on the proportion of SC winners after 2008.

<sup>28</sup>Since there are an average of 7.4 constituencies in each administrative district, a two percentage point increase in the probability of SCs winning office due to past reservations is equivalent to two additional SCs winning office from 100 constituencies in 13.5 average-sized districts.

control districts, and 6 follow-up measurements—akin to the natural experiment due to the 1974 redistricting—would be able to discern treatment effects of 2 percentage points, assuming that constituencies that had never been reserved for SCs would not elect SCs (i.e., the value of the outcome for the control group is 0), with power well above 0.8.<sup>29</sup> A RCT with 34 treatment districts, 245 control districts, and one follow-up observation—akin to the natural experiment due to the 2008 redistricting—would also be able to discern treatment effects of 2 percentage points with power greater than 0.8.

I draw on two datasets for the analysis—a new dataset on SC population counts by constituency, as used by the 1962, 1972 and 2002 delimitation commissions and collected through archival research at the Election Commission of India, and a comprehensive database of state elections. Recall that the key dependent variable is the proportion of SC winners in each district. This is known for the natural experiment due to the 2008 redistricting, but, since candidate caste data are only available from 2004 on, has to be estimated for the 1974 redistricting. For the latter natural experiment, I therefore estimate winners’ castes using candidate names. I turn to describing this method next.

## **A Identifying Whether Candidates’ are SC**

For the natural experiment due to the 1974 redistricting, I impute candidates’ caste using their names (the main natural experiment analyzed here, due to the 2008 redistricting, uses actual caste data). Variants of this method have been previously used to estimate people’s ethnicity, race and gender (Figlio 2007; Fryer and Levitt 2004; Kerr 2008). In a typical application of this method, for example, Fryer and Levitt (2004) note that DeShawn tends to be a black name, in that 99 percent of the people in their database with this name are black. Although scholars often use 1 percent census samples for such exercises, India’s census databases do not include people’s names and therefore cannot be employed for this purpose.

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<sup>29</sup>STATA’s `sampsi` command was used for these calculations. The power of the tests remain above 0.8 assuming that outcomes are autocorrelated between 0.1, 0.5, 0.9, 0.99. See Online Appendix Table 1 for further assumptions.

To estimate the caste of candidates, I create state-specific lists of *all* candidates that ran for India’s state and national lower house elections since 1964–2012, along with their full names and the reservation status of the constituency from where each candidate ran. The names databases are state-specific since the list of SCs in India is state-specific. I rely on full rather than last names for consistency, since although naming conventions vary across the country, following these would cause us to rely on ad hoc adjustments. I collapse these name lists to yield the probability that a person with a particular name has run for office from a constituency reserved for SCs.<sup>30</sup> This probability is also an estimate of the probability that a person with a particular name is SC— $\Pr_{name,state}(SC) = \text{Candidates in SC seats}_{name,state} / (\text{Candidates in SC seats}_{name,state} + \text{Candidates in open seats}_{name,state})$ —since all candidates in seats reserved for SCs are SCs, and since most candidates who run for office in open constituencies are not SCs. To estimate a candidate’s caste, I look up the name of the candidate in the names database and assign the “rounded up” value of  $\Pr(SC)$  for that name to the candidate. Rounding up the measure of estimated caste maximizes the classification of candidates as SC, thereby minimizing false negatives (i.e., SCs classified as non-SCs) at the cost of increasing false positives (i.e., non-SCs classified as SCs).

To examine how well the caste estimation strategy performs, I compare candidates’ estimated caste with their known caste for the 2004–2012 period. The caste estimation method classifies a high percentage (84 percent) of candidates correctly.<sup>31</sup> Although imperfect, recall that we only estimate caste for the natural experiment due to the 1974 redistricting. For the main analysis, of the natural experiment due to the 2008 redistricting, I am able to employ data on candidates’ actual caste.

A last point to note is that the caste estimation strategy employed here inflates estimates of the effects of past reservations for two reasons. Recall that the effects of past reservations may be written as  $= \Pr(SC\ winner)_{treatment} - \Pr(SC\ winner)_{control}$ , where treatment units had reservations

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<sup>30</sup>An advantage of this method is that the underlying data are more representative of India’s political class than a 1 percent census sample would be.

<sup>31</sup>Rounding  $\Pr(SC)$  to the nearest integer classifies only 50 percent of SCs as SCs. Rounding down  $\Pr(SC)$  classifies 40 percent of SCs as SCs.

in the past but not in the present, and control areas were without reservations in the past and present. First, I set the probability that a candidate is SC to 1 if a candidate had run for office when their constituency was reserved for SCs. Since no such adjustment could take place for control group candidates, the first term is likely to be inflated relative to the second. Second, the general underestimation of  $\Pr(SC \text{ winner})$  due to the assumption that all candidates who run for office in open constituencies are not SC is likely to attenuate the second term more, since these constituencies were never reserved for SCs.

## II Reservations Fail to Have Downstream Effects

Table 1 investigates the effects of reservations using data from before and after the 1974 and 2008 delimitations and a linear probability model. For this analysis, the unit of observation is the administrative district, the outcome is the proportion of constituencies in a district with an SC winner,<sup>32</sup> and the independent variable of interest is the proportion of constituencies that had reservations in the past ( $R_{i,PRE}$ ).

[Table 1 Goes Here]

Regressions 1 and 2 use the natural experiment due to the 2008 redistricting to examine the effects of past reservations. Regression 1, which controls for the proportion of constituencies currently reserved for SCs ( $R_{i,POST}$ ) and state-year fixed effects, suggests that past reservations ( $R_{i,PRE}$ ) have a statistically insignificant effect on the proportion of SC winners. Regression 2 improves upon the previous regression by estimating the effects of past reservations ( $R_{i,PRE}$ ) while controlling for the proportion of seats that would have been reserved for SCs in the past and present if fractional seats could be reserved for SCs ( $F_{i,PRE}$  and  $F_{i,POST}$ ). This implements equation 1, is the preferred specification, and isolates the quasi-random variation in reservations due to the need to reserve whole rather than fractional seats.

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<sup>32</sup>Despite the use of OLS, all but two of the predicted probabilities fall in the 0–1 range.

Regression 2 suggests that past reservations had a small and statistically insignificant effect on the proportion of SCs elected. The point estimate suggests that a district that was entirely reserved for SCs in the past experienced a 2.3 percentage point increase in the percent of SC winners after reservations were withdrawn. Recall that the natural experiment has statistical power close to 1 to detect an effect of this size. The 95 percent confidence interval for this effect is  $-1.4$  to  $6.0$  percentage points. Since there are an average of 7.4 constituencies in each administrative district, a 2.3 percentage point increase in the probability of SCs winning office due to past reservations is equivalent to a little over two additional SCs winning office from 100 constituencies in 13.5 average-sized districts. This effect is substantially less than the equivalent effects of reservations for women in Mumbai's municipal corporation (Bhavnani 2009 estimates this to be 17.9 percentage points, with a 95 percent confidence interval of 7.0 to 28.8 percentage points).<sup>33</sup> It is also somewhat less than the effects of past reservations for female legislators in Birbhum's panchayats or village councils (Beaman et al. 2009 estimate the effects of past reservations for women in Birbhum to be 5.7 percentage points, with a 95 percent confidence interval of  $-0.2$  to 11.6 percentage points).<sup>34</sup> In short, the effects of past reservations for SCs in India's state legislatures is substantively small, statistically indistinguishable from zero, and is also somewhat less than the downstream effects of women's reservations estimated in other contexts.

The next two columns replicate the results of regressions 1 and 2, using the natural experiment due to the 1974 delimitation. This episode allows us to examine the longer-term effects of the withdrawal of reservations, until 2004. Per the discussion in the previous section, however, winners' actual caste is not known for this period, and is therefore estimated, using candidate names. Recall also that the caste estimation method results in inflated estimates of the effects of past reservations. The results for this natural experiment are consistent with the effects of the 2008 redistricting insofar as they are statistically indistinguishable from zero. The point estimate suggests that past reservations increase the chance of SCs winning office by 5.2 percentage points, with a 95 percent confidence interval of  $-5.6$  to 16 percentage points. The point estimate is likely inflated due to the

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<sup>33</sup>See row 1 of Table 3 of Bhavnani 2009.

<sup>34</sup>See column 9 of Table 3 of Beaman et al. 2009.

method used to estimate caste. Confirming this, re-estimating regressions 1 and 2, which analyzed the effects of the 2008 redistricting, using estimated rather than actual caste as the outcome yields much larger and statistically significant effects of past reservations (see Online Appendix Table 3). We know these inflated estimates to be incorrect.

In the last regression, I interact the variable for the proportion of seats reserved for SCs pre-delimitation with the number of elections that have lapsed since delimitation. The beneficial effects of past reservations might decay or amplify over time.<sup>35</sup> The coefficient on the interaction term is statistically insignificant, however, thereby suggesting that there is no statistically detectable trend in the evolution of the effects of past reservations.

It is worth reiterating that the causal estimate recovered here is the effect of the withdrawal of reservations for SCs in some constituencies while reservations are in place elsewhere. This estimate is of interest since reservations are changeable as per the rules in precisely these constituencies. This estimate is probably different from the effect of the withdrawal of all reservations, however, which is not estimable in this context, since reservations were not entirely withdrawn. Indeed, the complete elimination of reservations does not appear to be politically feasible in India at the moment, despite explicit sunset provisions. That said, we might expect greater positive downstream effects if reservations are entirely eliminated, as particularly strong SC candidates (who might tend to be retained when few reservations are removed) rerun for office, as parties will be pressured to accommodate at least some SC incumbents in open seats<sup>36</sup> (to descriptively represent SCs, or because of ties to SC incumbents) and since people might be less hostile to SCs if all reservations withdrawn. The estimated effect of the withdrawal of *some* reservations are therefore likely to be a lower bound for the effects of the withdrawal of *all* reservations.

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<sup>35</sup>Treatment effects might decay over time, as incumbents retire. If, however, reservations alter voter attitudes, we might expect a more lasting, and even increasing, effect of past reservations.

<sup>36</sup>After reservations were withdrawn, SC incumbents from de-reserved seats were either denied party tickets altogether, or were—in a few instances—accommodated elsewhere. So, for example, a former SC minister—elected under reservations—from Himachal Pradesh, Kuldeep Kumar, was not renominated from his newly de-reserved constituency, but was shifted to another reserved seat, that of Chintpurni (Sharma 2012). Parties' ability to accommodate SC incumbents in reserved seats will be limited as more reservations are withdrawn.

## A Robustness tests

There are two possible threats to the identification of the effects of past reservations. The first is that the estimated impacts of reservations are due to disproportionate flows of SC migrants, which jointly determine both reservations and the degree to which SCs win office. This is unlikely to be the case, given the low and similar migration rates for SCs and non-SCs (Munshi and Rosenzweig 2016; Sebastian 1992). To allow for this possibility, however, I control for the proportion of SCs in other districts in a state, since migrants usually remain within their state.<sup>37</sup> Regressions 1 and 2 in Table 2, for 2008 and 1974 delimitations, respectively, indicate that the main results are robust to controlling for migration pressures from neighboring districts.

[Table 2 Goes Here]

A second possible threat to the identification of the effects of reservations is that the treatment—changes in reservation—was bundled with a second treatment: namely, the redrawing of constituency boundaries within districts. It is therefore possible that the null downstream effect of reservations is driven by extensive changes in constituency boundaries. In order to address this possibility, I proceed in three fronts. First, I conduct placebo tests, to see if the degree to which constituencies remained the same after the 2008 delimitation (calculated by overlaying pre- and post-delimitation GIS maps, and estimating—assuming that people are evenly distributed throughout constituencies—the likelihood that two randomly drawn people from a new constituency were from the same old constituency,<sup>38</sup> and then averaging this measure over all new constituencies in the district) is statistically significantly related to a number of pre- and post-treatment observables. As Online Appendix Table 2 suggests, it is not. Second, I confirm that the null impact of pre-2008 reservations is robust to controlling for the degree to which constituency boundaries remained the same (regression 3 of Table 2). The efficacy of past reservations does not appear to vary with

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<sup>37</sup>According to the 2001 census, 85 percent of migrants were from the state of enumeration.

<sup>38</sup>This measure is the Herfindahl–Hirschman Index (HHI), calculated as  $\sum p_k^2$ , where  $p$  is the proportion of the land area of the new constituency in each overlapping old constituency, indexed by  $k$ .

boundary changes.<sup>39</sup>

The last check I conduct is to restrict the data to constituencies whose boundaries were largely unaltered by the 2008 delimitation and that are not reserved for SCs, and to then simply compare constituencies that had reservations immediately before 2008 with those that did not. Comparing the mean proportion of SC winners in constituencies that used to be reserved pre-delimitation and are no longer reserved (“treatment” constituencies) with those that were not reserved before and after redistricting (“control” constituencies), suggests that past reservations fail to increase the chances of an SC winner to a statistically significant degree. Online Appendix Table 4 presents the result of this analysis, showing that the null effects of past reservations on the proportion of SC winners obtains using three estimators (difference-of-means, OLS regression controlling for SC population shares and state fixed effects, and nearest-neighbor matching using SC population shares) and three definitions of constituencies that are comparable before and after delimitation (post-delimitation constituencies were considered comparable to predelimitation constituencies if  $> 50$  percent,  $\geq 90$  percent or 100 percent of their land area overlapped with a pre-delimitation constituency.)

## **B Mechanisms**

I next build on the discussion in the introduction to explore some possible reasons that reservations for SCs fail to have downstream effects after they are withdrawn. This discussion is speculative, since the paper focuses on whether rather than how reservations work after they are withdrawn.

To understand the non-effect of past reservations, I use equation 1 to examine the effects of past reservations on intermediate outcomes that precede the election of SCs (results presented in Table 3). Regressions 1–3 suggest that past reservations did not increase the number and proportion

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<sup>39</sup>A specific version of this concern is that the delimitation “packed” SC voters into the reserved seats remaining in districts where reservations were withdrawn. In fact, in districts where reservations were withdrawn in 2008, delimitation or redistricting increased the percent of SC voters in reserved seats by 2.3 percent, while the percent of SC voters in open seats increased by 1.6 percent. This difference is not statistically significant, which suggests that the delimitation did not lead to packing.

of SC candidates, or the proportion of constituencies with any SC candidates. These results are striking insofar as they suggest that reservations that had been in place for over 30 years—between 1974 and 2007—did not spur SCs, incumbents or others, to run for office after reservations were withdrawn. Similar results obtain when we restrict the data to constituencies whose boundaries were largely unchanged (see the right panel of Online Appendix Table 4). This is unfortunate insofar as one of the main ways in which past reservations could have worked was by creating strong SC incumbents. Indeed, this is precisely the effect that drove most of the downstream effects of women’s reservations in Mumbai (Bhavnani 2009).

[Table 3 Goes Here]

Although the low rate at which SC incumbents rerun for office could be due to their choice, this seems unlikely, for at least two reasons. First, SC incumbents are on average two years younger than non-SCs. And second, newspaper reports note SC candidates’ disappointment at not being renominated for office (Sharma 2012; Singh 2009), at which point a few of them have been known to run as independents (Singh 2009). Since securing a party ticket is essential for being elected—only 3 percent of winners in the period examined were independents—the denial of party tickets is fatal for the election prospects of SCs. But why might parties fail to nominate SCs (including incumbents) for office? Since India’s parties generally lack primaries, and candidate nominations are usually decided upon by party leaders,<sup>40</sup> the question is why party leaders fail to renominate SC incumbents.

Party leaders could fail to renominate SC incumbents due to several observable and unobservable reasons, including candidate characteristics, constituency characteristics, and SCs’ performance. For example, although SC and non-SC incumbents are similar in terms of age, education and gender, SC incumbents are substantially poorer than non-SCs, and SCs are less likely to have criminal charges leveled against them.<sup>41</sup> Since wealth and criminality are associated with electoral

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<sup>40</sup>The characterization of the candidate nomination process obviously masks some variation over parties and time (Ziegfeld 2015).

<sup>41</sup>See Online Appendix Table 5 for details. SCs’ median assets are also smaller than non-SCs’ median assets.

success in India (Aidt, Golden and Tiwari 2011; Vaishnav Forthcoming), parties might rationally prefer to nominate non-SCs over SCs.<sup>42</sup> The caste penalty in renomination rates could also be driven by constituency characteristics, such as caste demography (under conditions of ethnic voting, SC candidates from constituencies with lower concentrations of SCs should be less likely to be renominated),<sup>43</sup> the degree to which constituencies are redrawn by the delimitation commission (redistricting should attenuate candidate incentives to rerun for office), and discriminatory attitudes of party elites and/or voters.

Multivariate analysis of the probability that candidates are renominated after 2008, presented in Table 4,<sup>44</sup> allows us to rule out a number of possible reasons for the SC renomination penalty. The regressions suggest that controlling for a number of observables—candidate attributes (gender, age, education,<sup>45</sup> and whether they were charged with crimes and were registered with the tax authorities), constituency characteristics (SC population percent, in addition to state and district fixed effects), and electoral performance (vote margins<sup>46</sup> and a dummy for whether they were chosen as ministers)—only strengthens the estimated SC renomination penalty. Although each of these mechanisms could be operationalized in different ways (controlling for the effects of caste demography is particularly difficult; in the absence of detailed caste breakdowns by electoral constituency, I employ state and district fixed effects and the SC population percent to test for this mechanism), and although the analysis does not control for unobservables (in particular, incumbent performance in office or discriminatory attitudes on the part of party leaders and/or voters<sup>47</sup>

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<sup>42</sup>Criminal candidates are thought to be electorally successful due to their superior ability to finance elections (Vaishnav Forthcoming), and due to their willingness to intimidate voters (Aidt, Golden and Tiwari 2011).

<sup>43</sup>Although some Indians do vote along ethnic lines, substantial numbers of Indians vote across caste and religious lines (Jaffrelot and Verniers 2011; Thachil 2011).

<sup>44</sup>The dependent variable for this analysis is a dummy for whether incumbents are renominated. The sample is all incumbents from immediately before the 2008 redistricting.

<sup>45</sup>Operationalized as dummies for whether the candidate had some college education, and some post-graduate education.

<sup>46</sup>Defined as the candidate's vote share in the last election minus the runner up's vote share in the same election.

<sup>47</sup>Numerous studies suggest that SCs experience discrimination, in both the economic (Pandey 2013; Thorat and Newman 2010) and political (Bhavnani 2016) spheres.

towards SCs), this reinforces the puzzle of why parties penalize SCs.

[Table 4 Goes Here]

The last regression in Table 3 suggests that past reservations increase the total vote share of SCs by 2.7 percentage points. This effect is statistically significant, raising the possibility that one of the mechanisms by which past reservations for women were shown to work—exposure reduces bias (Beaman et al. 2009)—might operate in the case of reservations for SCs.<sup>48</sup> I hasten to underline that I am unable to directly observe whether indeed anti-SC bias exists, or whether it is alleviated by exposure to SC leaders. Also, learning, even if it occurs, is not prevalent enough to change the proportion of SC winners. Voters might be unlikely to “learn” about the efficacy of SC leaders because the literature suggests that although SC legislators are effective in redirecting spending towards goods preferred by SCs (Pande 2003), they are not effective in improving development outcomes in their geographical constituencies (Jensenius 2015).

### III Discussion

I have leveraged changes in reservations for SCs in India’s state legislatures in 1974 and 2008 to assess the efficacy of one of the world’s largest scale and longest experiences with electoral quotas. Although these electoral quotas could, in principle, have downstream effects, two natural experiments due to the need to reserve whole rather than fractional seats indicate that past reservations for SCs fail to increase the proportion of SC winners, in the short and long terms. These null effects contrast with the substantial positive downstream effects of women’s reservations found in Italy and in India’s local legislatures. Past reservations fail to have positive effects after they are withdrawn partially because SC incumbents do not rerun for office. This is not due to observable differences between SC and non-SC incumbents or their constituencies, leaving open to the possibility that SC incumbents are forced out due to their poor performance while in office, or that

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<sup>48</sup>Consistent with this possibility, Chauchard (2014) has shown that the behavioral intentions of non-SC citizens towards SC citizens are improved by exposure to SC leaders in the Indian state of Rajasthan.

parties and/or voters discriminate against SCs. The poor performance of SCs in office might also explain why exposure to SC leaders does not appear to reduce bias.

The null downstream effects of reservations estimated here are likely to be a lower bound on the efficacy of reservations, for two reasons. First, and as discussed earlier, the downstream effects of the elimination of *all* reservations are likely to be larger than the estimated effects of the elimination of *some* reservations, as parties and voters will feel a need to accommodate at least some SCs in the total absence of reservations. The effects of reservations are also likely to be greater in proportional representational systems, such as those with multi-member districts. In these systems, parties will likely choose candidates based not just on their winnability, which is what they have strong incentives to do in single member district systems, but also based on the degree to which candidates descriptively represent the population. This will increase the likelihood that ethnic group quotas will have downstream effects.

The absence of downstream effects of reservations vividly illustrates the durability of de facto political power, despite radical de jure attempts to reshape it. Reservations for SCs were first instituted in 1937, in response to SCs' threats to splinter the Indian independence movement. To use the terminology in [Acemoglu and Robinson \(2008\)](#), SCs were turning their contemporaneous de facto power to de jure power. This de facto power was crystallized in an electoral institution—reservations—which was intended to both reflect SCs' power at the time, and to further empower SCs. However, reservations have failed to have downstream effects.

In terms of policy, the common presumption regarding reservations for SCs appears to be correct: reservations for SCs are still needed, insofar as their elimination would decrease the number of SC legislators. The fact that past reservations for women boost their representation even as reservations for SCs fail to do so is ironic, since the literature suggests that the reverse should hold. For example, [Htun \(2004\)](#) argues that reservations are “logically appropriate” for ethnic groups whose boundaries coincide with political cleavages, even as they are inappropriate for women whose interests cross partisan lines.<sup>49</sup>

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<sup>49</sup>It remains possible that some sort of party quota for women would be even more effective than reservations.

A second policy implication, which follows from the null effects presented here, and the fact that India has had these “temporary” quotas since 1937, is that it may be time to try other means to improve the welfare of SCs. Contrasting the poor efficacy of quotas with the remarkable success of a SC party—the Bahujan Samaj Party (BSP)—is revealing. The BSP has come to rule India’s most populous state—Uttar Pradesh—four times in the last 20 years, through winning seats and cross-caste alliances. Could SCs’ path to power be simply through old-fashioned politics?<sup>50</sup>

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<sup>50</sup>These two mechanisms could be related, of course. Low caste parties could build themselves up by initially winning reserved constituencies, and those that were reserved for SCs in the past. Evidence from the BSP’s experience in Uttar Pradesh is inconsistent with this possibility, however. The BSP won only 5 percent of reserved seats in 1989, none in 1991, 26 percent of reserved seats in 1993, 21 percent in 1996, 27 percent in 2002. It did particularly well in reserved seats—winning 69 percent of them—only in 2007, well after it had established itself. See [Chandra \(2004\)](#) for details.

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Table 1: The effect of reservations on the proportion of SC winners

Sample: Dependent variable:	Pre-, post-2008 redistricting Prop. SC winners		Pre-, post-1974 redistricting Est. prop. SC winners		
	1	2	3	4	5
$R_{i,t-1}$	0.021 (0.020)	0.023 (0.019)	0.060 (0.045)	0.052 (0.055)	-0.037 (0.088)
$R_{i,t}$	0.983*** (0.014)	0.982*** (0.015)	0.829*** (0.049)	0.825*** (0.079)	0.826*** (0.079)
$C_{i,t-1}$		0.028 (0.039)		0.011 (0.021)	0.010 (0.020)
$C_{i,t}$		-0.002* (0.001)		-0.010 (0.020)	-0.009 (0.020)
Elections since delimitation					-0.035** (0.015)
$R_{i,t-1}$ X Elections since delim.					0.021 (0.016)
Controls?	Y	Y	Y	Y	Y
State fixed effects?	Y	Y	Y	Y	Y
Observations	279	279	2,208	2,208	2,208
Adjusted $R$ -squared	0.95	0.95	0.66	0.66	0.66

Notes:  $R_{i,PRE}$  is the proportion of seats reserved for SCs before redistricting,  $R_{i,POST}$  is the proportion of seats reserved for SCs after redistricting,  $F_{i,PRE}$  is the proportion of seats that would have been reserved for SCs before redistricting had it been possible to reserve seat fractions, and  $F_{i,POST}$  is the proportion of seats that would have been reserved for SCs after redistricting had it been possible to reserve seat fractions. The control set includes the current population share of SCs, the current and past number of seats in the district, and land area. Robust standard errors, clustered by state-year, in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 2: Robustness tests for the effect of reservations on the proportion of SC winners

Sample: Pre-, post- Dependent variable:	2008 redist. Prop. SC winners 1	1974 redist. Est. prop. SC winners 2	2008 redist. Prop. SC winners 3
$R_{i,t-1}$	0.022 (0.019)	0.055 (0.055)	0.023 (0.020)
$R_{i,t}$	0.982*** (0.015)	0.828*** (0.079)	0.984*** (0.014)
$C_{i,t-1}$	0.028 (0.040)	0.012 (0.021)	0.035 (0.050)
$C_{i,t}$	-0.002* (0.001)	-0.010 (0.020)	-0.002* (0.001)
SC population share in other districts in state	0.511 (0.775)	-2.023* (1.179)	
Degree to which boundaries remained the same			-0.001 (0.006)
Controls?	Y	Y	Y
State fixed effects?	Y	Y	Y
Observations	279	2,208	267
Adjusted $R$ -squared	0.95	0.66	0.95

*Notes:*  $R_{i,PRE}$  is the proportion of seats reserved for SCs before redistricting,  $R_{i,POST}$  is the proportion of seats reserved for SCs after redistricting,  $F_{i,PRE}$  is the proportion of seats that would have been reserved for SCs before redistricting had it been possible to reserve seat fractions, and  $F_{i,POST}$  is the proportion of seats that would have been reserved for SCs after redistricting had it been possible to reserve seat fractions. The control set includes the current population share of SCs, the current and past number of seats in the district, and land area. The degree to which boundaries remained the same was calculated by overlaying pre- and post-delimitation GIS maps, calculating the likelihood that two randomly drawn people from a new constituency were from the same old constituency using a Herfindahl–Hirschman Index, and then averaging this measure over all new constituencies in the district. See text for details. Robust standard errors, clustered by state-year, in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 3: The effect of reservations on auxiliary outcomes

Sample: Dependent variable:	Pre-, post-2008 redistricting			
	# SC cand. 1	Prop. SC cand. 2	Prop. cons. $\geq 1$ SC ran 3	% vote recd. by SCs 4
$R_{i,t-1}$	-0.829 (0.461)	-0.018 (0.021)	-0.146 (0.109)	2.695** (1.083)
$R_{i,t}$	9.597*** (0.600)	0.853*** (0.064)	0.116 (0.196)	96.232*** (1.894)
$C_{i,t-1}$	2.343 (2.404)	0.215*** (0.062)	0.360 (0.204)	0.270 (3.475)
$C_{i,t}$	0.077 (0.058)	0.003 (0.003)	0.002 (0.011)	0.071 (0.046)
Controls?	Y	Y	Y	Y
State fixed effects?	Y	Y	Y	Y
Observations	279	279	279	279
Adjusted $R$ -squared	0.70	0.84	0.34	0.98

*Notes:*  $R_{i,PRE}$  is the proportion of seats reserved for SCs before redistricting,  $R_{i,POST}$  is the proportion of seats reserved for SCs after redistricting,  $F_{i,PRE}$  is the proportion of seats that would have been reserved for SCs before redistricting had it been possible to reserve seat fractions, and  $F_{i,POST}$  is the proportion of seats that would have been reserved for SCs after redistricting had it been possible to reserve seat fractions. The control set includes the current population share of SCs, the current and past number of seats in the district, and land area. Robust standard errors, clustered by state-year, in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 4: Logit estimates of the effect of caste on the probability that pre-2008 incumbents are renominated (coefficients are marginal effects)

	1	2	3
SC dummy	-0.138*** (0.029)	-0.136*** (0.032)	-0.144*** (0.030)
Age		-0.004*** (0.001)	-0.005*** (0.001)
Some college education dummy		-0.012 (0.027)	-0.032 (0.028)
Some post-grad education dummy		0.061* (0.036)	0.061* (0.036)
Female dummy		0.009 (0.042)	-0.001 (0.040)
Charged with crimes dummy		-0.015 (0.026)	-0.034 (0.028)
Registered with tax authorities dummy		0.073*** (0.024)	0.082*** (0.025)
Log assets, in USD		-0.000 (0.000)	-0.000 (0.000)
Two-time incumbent dummy		0.031 (0.025)	0.042* (0.025)
Winning vote margin %		-0.002 (0.001)	-0.001 (0.001)
Minister dummy		0.135*** (0.036)	0.143*** (0.036)
SC population %		-0.024 (0.200)	-0.561 (0.613)
Degree to which boundaries remained the same		-0.008 (0.049)	0.036 (0.050)
Party fixed effects?	N	Y	Y
State fixed effects?	N	Y	N
District fixed effects?	N	N	Y
Observations	2,058	1,829	1,829
Pseudo <i>R</i> -squared	.01	.07	.15

*Notes:* The degree to which boundaries remained the same was calculated by overlaying pre- and post-delimitation GIS maps, calculating the likelihood that two randomly drawn people from an old constituency were from the same new constituency using a Herfindahl–Hirschman Index. Robust standard errors in parentheses. In regression 2, standard errors are clustered by state. In regression 3, standard errors are clustered by district. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .