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DOES AFFIRMATIVE ACTION INCENTIVIZE SCHOOLING? EVIDENCE FROM INDIA

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Abstract—Affirmative action raises the likelihood of getting into college or obtaining a government job for minority social groups in India. I find that minority group students are incentivized to stay in school longer in response to changes in future prospects. To identify causal relationships, I leverage variation in group eligibility, school age cohorts, and state-level intensity of implementation in difference-in-differences and regression discontinuity designs. These estimators consistently show that affirmative action incentivizes about 0.8 additional years of education for the average minority group student and 1.2 more years of education for a student from a marginal minority subgroup.

I. Introduction

AFFIRMATIVE action is a contentious issue for policymakers and academics across the world, including the United States, India, Sri Lanka, Malaysia, Nigeria, and Brazil. The research and subsequent policy debates involve issues of college mismatch (Arcidiacono et al., 2011), direct effects on college enrollment and test scores (Bagde, Epple, & Taylor, 2016), and the consequent effects on nonminority groups (Bertrand, Hanna, & Mullainathan, 2010). However, little is known about the impacts on human capital investment for potential future beneficiaries. In this paper, I study the causal impact of affirmative action policies on schooling decisions in India. Reservation quotas make it easier for minority groups to get into college or get a government job. As with many other affirmative action programs, a minimum level of education is required to be eligible for certain positions, changing potential future benefits, and encouraging skill acquisition. I find that by raising the future expected returns to education, such policies incentivize minority groups to stay in school longer.

Policymakers have at least three factors to consider while implementing reservations: (a) the average effects of quotas, (b) the fraction of seats that should be reserved, and (c) which groups should be eligible. I take three distinct empirical approaches to speak to each of these issues. First, I study a nationwide law change that reserved federal government jobs for Other Backward Classes (OBCs). These jobs required specific educational qualifications, raising the returns to certain levels of schooling. By comparing eligible castes to ineligible castes and student cohorts young enough to change their schooling decisions to those that were too old, I estimate that minority groups attain 0.8 more years of education. These effects are absent among ineligible minorities, ineligible candidates within the eligible minority groups, and low-income students from ineligible upper castes.

The average effects from my first approach, however, say little about how this relationship changes as we increase the fraction of reserved seats (or the intensity of reservations). A very high intensity of reservations may lead to complacency or lead employers to devalue the qualifications of minority groups, thereby disincentivizing educational attainment (Coate & Loury, 1993). For my second approach, I leverage variation in affirmative action laws for college admissions and government jobs at the state level. I create an original data set based on historical laws passed in each state by petitioning the government for archived commission reports. I then examine three sources of variation—the timing of these laws, the minority groups eligible, and the intensity of reservations—to determine how changing the intensity affects the relationship between affirmative action and educational attainment. By comparing low-intensity states to high-intensity ones, I show that the relationship between changes in education and the fraction of seats reserved is concave. This suggests that extremely intensive affirmative action programs may detrimentally lower the educational attainment of minority groups.

The third policy factor to consider is which minority groups, on the margin, should be eligible. India has numerous subcastes, and subcaste eligibility is a contentious issue. Marginal subcastes are better off than the average low-caste candidate and may find it easier to take advantage of such affirmative action policies but may have less to gain from them. In my third approach, I compare subcastes that just received the program to subcastes that just lost out. Haryana, a state in India, conducted a large socioeconomic survey and ranked subcastes on an “index of backwardness” to determine

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subcaste eligibility. Any subcaste with a score greater than half of the total value of the index was eligible. Using a regression discontinuity (RD) design, I compare subcastes on either side of this cutoff. Since schooling levels of older members should not be affected by this policy, I further use them as a control group in a difference-in-discontinuities approach. On average, a student from the marginal subcaste attains 1.2 more years of education, suggesting that there are plausibly large, positive incentive effects of expanding the coverage of these programs to marginal minority subgroups.

All estimators consistently point toward an increase in educational attainment for the targeted minority group in response to reservations. Importantly, each estimator determines a different parameter necessary for welfare analyses of such policies. While the difference-in-differences approach estimates the average impact of the implementation of new quotas, the RD determines the impact on the marginal subcaste included. State-level variation allows me to determine how these impacts vary as we increase the intensity of reservations. These three parameters are crucial for any meaningful discussion of the costs and benefits of such policies.

Public education policy in low-income countries is usually associated with lowering the costs of education (Duflo, 2001; King & Orazem, 2008). The policies I study in this paper are different in that they change the less tangible future returns rather than current schooling costs.[1] Higher returns are likely to induce more schooling (Freeman, 1976), and there is reliable evidence from India supporting such claims (Foster & Rosenzweig, 1996; Jensen, 2012; Kochar, 2004). Yet increasing the returns may have perverse effects in low-income settings, as better wage opportunities raise the opportunity cost of schooling (de Brauw & Giles, 2008), and strategic incentives within households may lead to adverse consequences (Jensen & Miller, 2015). Such evidence suggests that it is unclear what the expected response to higher returns will be in developing countries, and I seek to resolve this ambiguity in my work.

In the Indian context, the affirmative action literature has mostly focused on direct impacts on a sample of engineering colleges and shows that college reservations are well targeted, improve the performance of minority groups, and have substantial impacts on college attendance and academic achievement (Bagde et al., 2016), leading to strong, positive economic effects (Bertrand et al., 2010). Alternatively, Krishna and Robles (2015) highlight mismatch at an engineering college, as minority students end up earning less than they would have if they picked less selective majors.[2] In contrast to these approaches, I focus on the country as a whole (rather than engineering colleges) and look at educational attainment at precollegiate levels, before the benefits materialize.[3] This paper is novel in that it empirically isolates the causal impacts on incentives before the policy benefits kick in. In doing so, I compile an original data set of state laws and exploit one state's law to perform an RD analysis.

In section II, I discuss the context of caste and class in India, the underlying legal and historical foundation behind affirmative action, and recruitment in public sector jobs. Section III explores the theory, building on a rich literature on affirmative action. Section IV discusses the data, whereas section V covers the main empirical strategies and results. Section VI concludes.

II. Reservation Policy in India

Historically, each subcaste in India was linked to certain occupations, resulting in a social hierarchy that, despite political gains, persists in socioeconomic outcomes today (Gang, Sen, & Yun, 2011).[4] Affirmative action policies were defined on the basis of caste or social class, and the policy interventions were much larger and more salient than in most other countries. Today, reservations are an integral part of political platforms and election campaigns; the media cover policy extensively, and any policy change is met with protests from different factions.[5]

The Constitution identifies certain disadvantaged Scheduled Castes (SCs), and certain aboriginal Scheduled Tribes (STs) as the least-well-off groups. These groups were eligible for reservations in government jobs, university admissions, and legislatures.[6] Importantly, Article 16(4) in the Constitution left room for expanding this preferential treatment to other disadvantaged sections of society. The purpose was to provide a level playing field for communities that have suffered from historical discrimination. The Constitution states, "The State shall promote with special care the educational and economic interests of the weaker sections of society . . . and shall protect them from social injustice and all forms of exploitation." Based on this, over time, there has been an

[1]Information on returns is poor in low-income settings, and the salience of expected returns affects educational choices (Dinkelman & Martinez, 2014; Jensen, 2010).

[2]Poor college fit and mismatch are costs discussed in the United States (Arcidiacono et al., 2011, 2014).

[3]In a somewhat different vein, Rao (2019) finds that a program that required Delhi public schools to admit students from poorer backgrounds had large, positive impacts on the prosocial behavior of richer peers.

[4]Affirmative action existed in some form in pre-independence times. Early examples are reservations for non-Brahmins in 1902 Kolhapur and 1918 Mysore. These spread to other states in post-independence India.

[5]For the first few decades after independence, the Congress Party dominated politics and was strongly associated with upper-caste members of society (Jaffrelot, 2003). Lower-caste movements challenged this dominance, and over time, political concessions were promised to address these inequities. Affirmative action policies were one important aspect of these concessions.

[6]Reservations for SC-STs were proportional to the population share, and 15% was reserved for SCs and 7.5% for STs. Until the early 1990s, SCs and STs were still underrepresented in government jobs, but this picked up after the mid-1990s. Sheth (2004) and Deshpande (2011) attribute this pickup to the activism that followed reservations for OBCs. Based on responses to my right to information (RTI) requests, the Department of Personnel and Training claims that by 2004, the SC-ST quotas were fulfilled at the federal level (the department refused to share earlier data despite repeated requests). Cassan (2019) studies historical access to a bundle of preferential policies for SC-STs and finds increases in educational attainment for men only.

attempt to identify groups that are better off than SCs and STs but less well-to-do than upper-caste members of the different communities. These groups are known as Other Backward Classes (OBCs). The constitutional leeway allowed states to autonomously reserve seats for different OBC communities in state-run universities and state government jobs.[7]

A. *The Mandal Commission Report and Implementation*

The Mandal (1980) Commission was established to determine what fraction of federal seats should be reserved and which subcastes were OBCs. The report created a national list of OBC subcastes based on their socioeconomic status and recommended reserving 27% of seats in national colleges and federal jobs for them. This report was met with large protests and strikes from the urban upper-class public, who argued that they were being discriminated against and that disadvantaged groups already had a "level playing field" (Kohli, 2001). Counterprotests popularized the report nationally, and these politically contentious recommendations were tabled for awhile.

In 1993, the federal government implemented the first stage of the Mandal (1980) Commission recommendations, reserving 27% of government jobs for OBCs; in 2006, reservations in colleges were implemented.[8] Despite legal challenges, the Supreme Court upheld the implementation of reservations in government jobs in a landmark case.[9] The implementation was contentious, and over time, the government disregarded certain Mandal (1980) recommendations while politicians made newer promises.[10] Different subcastes within the OBC group were treated equally, and there was no separate allocation for women.[11]

Reservations were implemented for direct recruitment,[12] and OBCs that obtained jobs without reservations would not count against the quota. By the late 1990s, caste-based reservations at the central level existed alongside state laws, which varied in intensity across states. While state-level laws provide quotas in both educational institutions and government jobs, the federal law changes studied here focus on OBC reservations in government jobs. In one empirical strategy, I focus on the state of Haryana, which loosely followed Mandal (1980) and ranked subcastes on the basis of their socioeconomic disadvantage. This ranking allows me to obtain an RD estimate of the impact of these policies. In another strategy, I use data from each state and leverage variation across the state-level intensity of reservations.

B. *Public Sector Jobs and OBC Representation*

There are four categories of government jobs, all of them eligible for quotas.[13] The highest category (groups A and B) of mostly high-level civil servants require finishing high school (upper secondary) or having a college degree. These comprise about 11.5% of all jobs. The next level (group C) needs candidates to finish either middle school or secondary school and consists of 58% of the jobs. The last category (group D) consists of the remaining 30.5% of the jobs and requires candidates to be either literate or have completed primary school. Group C and D jobs include lower-skilled jobs like revenue inspectors, assistants, clerks, and drivers. As such, the incentive effects should matter for not just graduating from high school but also in attaining these specific levels of education that make candidates eligible for each job category.

Public sector jobs are sought at all levels of the skill distribution, and the premiums are especially salient at lower levels. Given the ensured regular salary that is often above market, combined with nonwage benefits, job security, and social authority, the returns to a public sector job are high.[14] This, combined with a highly publicized change in the likelihood of getting a public sector job, may intensify human capital responses. By 2000 (when much of my analysis takes place), 2.5% of all jobs, 60% of formal sector jobs, and 14.8% of enterprise-based jobs were in the public sector (both state and central).[15] The unconditional yearly wage premium for public sector jobs relative to other enterprise jobs was \$485 for OBCs and \$465 for all.[16]

[7]The earliest instances of such reservations started outside the northern belt of states soon after independence, with other states introducing quotas in the 1960s and 1970s. These introductions were contentious and challenged in court, and the Supreme Court in 1962 ruled that not more than 50% of seats could be under a quota.

[8]I do not study the 2006 college reservations and instead focus on the 1993 reservations and state policies.

[9]The case of *Indira Sawhney v. Union of India* (1993) introduced the concept of the "creamy layer" for the more well-off members of the OBC community, who were now excluded from taking advantage of these policies.

[10]Some changes were codified in office memorandums; others (such as a campaign promise to expand the number of government jobs) were not. The Mandal (1980) Commission claimed that 13% of government jobs were already held by OBCs, so any expansion in total jobs would presumably be small. The promise to expand college seats was passed as an Act of Parliament, which stated that institutions must "increase the total number of seats in such a manner that the number of unreserved seats (general category) is not reduced due to reservation for OBCs. This was based on the number of seats available for general category before the Act was passed." Central Educational Institutions (Reservation in Admission) Amendment Bill.

[11]The government's office memorandums required that preference shall be given to poorer OBCs and unfilled vacancies then filled by other OBCs, but with no specifics on who is "poor."

[12]The Supreme Court confined reservations to direct recruitment, as the government's office memorandums were silent about promotions. This was challenged later, as many low-caste workers were stuck in group D jobs.

[13]Some documents refer to the four categories as groups I to IV instead of A to D.

[14]Additionally, if there is discrimination in the private sector, a government job is lucrative.

[15]There were 3.9 million central government jobs in 2000 and 45% in the Indian railways. Fifteen percent were in large cities, 53% were in rural areas, and the rest were in small towns. Increasing the supply of skilled work to the public sector may otherwise affect public sector wages. This is, however, unlikely as government wages are fixed, in real terms, for a decade at a time by Pay Commissions (1983, 1994, and 2006) that set wages and compensation for public sector employees.

[16]My calculations using the 2000 National Sample Survey.

The Supreme Court interpreted Article 335 of the Constitution, which instructs the government to maintain the efficiency of the public sector, by requiring that educational qualifications not be relaxed for any reserved groups.[17] After meeting minimum qualifications, different jobs have different criteria, including interviews or tests, yet all such positions are competitive.

Household survey data show a marked increase in middle-class (including public sector) employment for OBCs after reservations (Lee, 2018). In 1999–2000 (seven years after reservations), 22% of public sector (state or central) jobs were held by members of the OBC community, whereas in 2004–2005, this number was about 27.7%. Yet the Department of Personnel and Training (DoPT) claims that for a subset of a few central ministries, the OBC share in 2009 was 12.3% and steadily rose to 22% by 2016. The discrepancy between household surveys and government reports likely reflects the fraction of OBCs obtaining government jobs via the general category. Different levels of government jobs require different educational qualifications: 15.45% of OBCs with a government job had less than or a primary school education, 14.42% finished middle school, 25.13% finished secondary school, 16.63% finished higher secondary school, and 28.37% were college graduates.

I am among the first to look at reservations for OBCs in government jobs. Policies in the 1980s that reserved jobs for a different minority group, the Scheduled Castes (SCs), had substantial impacts on formal sector employment (Prakash, 2010) and intergenerational occupational mobility (Hnatkovska, Lahiri, & Paul, 2012, 2013). Because OBCs are better off than SCs to start with, we may expect that there would be even more qualified OBCs to avail themselves of these quotas.

Recent work by Lee (2018) uses data from 2012 to look at the combined impacts of preferential treatment (including direct access to educational benefits like scholarships) for OBCs and finds a marked increase in education, being employed in middle-class jobs (including government jobs), and knowing government officials.[18] This evidence suggests that by 2012 at least, reservations may have helped with upward societal mobility for OBCs.

III. The Incentive Effects of Affirmative Action: Literature and Theory

The theory on incentive effects of affirmative action highlights certain ambiguities. Coate and Loury (1993) write that under certain assumptions, affirmative action encourages effort and leads to a "benign equilibrium," where employers' negative stereotypes about minorities are eradicated. Yet under other conditions, it leads to a "patronizing equilibrium," where stereotypes persist and employers devalue minority group credentials, discouraging educational investments among minorities.

Similarly, in signaling models, affirmative action may discourage investments for low-ability minorities. In the absence of affirmative action, costs are high for all minority students: different abilities may be pooled, encouraging low-ability students to get as much education as high-ability students. If affirmative action encourages high-ability minorities to get more education, we may get separating equilibria where low-ability students drop out early (Bedard, 2001).

These theoretical results suggest that it is important to understand not only who is affected but also how intense these programs are, as I do in my empirical exercise. In the Indian context, affirmative action programs are more salient and larger in magnitude than in most other countries. Reserving a very large fraction of seats may allow low-ability, low-caste students to get into college and into public sector jobs, exacerbating employers' negative stereotypes, leading to more discrimination and less human capital accumulation. Which groups are eligible, and how expansive these policies are, are therefore closely linked with the longer-run theoretical equilibrium. As such, in my empirical exercise, I study not just the average impacts but also how increasing the intensity of reservations and enlarging the definition of minority groups may affect outcomes.

Theoretical work on affirmative action shows that getting into college may motivate students to graduate from school, overcoming effort pessimism (Bodoh-Creed & Hickman, 2018; Fryer & Loury, 2005). Alternatively, there could be complacency effects: smarter sections of the minority group put in less effort knowing that it is easier to get into college (Assuncao & Ferman, 2015). The criteria for preferential treatment—race, caste, income, geography—matter and affect high school investments, test scores, and college applications (Antonovics & Backes, 2014; Card & Krueger, 2005; Domina, 2007). Importantly, simple time series evidence suggests that aspirations respond to such laws (Akhtari, Bau, & Laliberté, 2017), and if peers are seen to benefit from this policy, then a role model effect may encourage educational attainment (Dee, 2004).[19]

In the Indian context, state-level reservations in colleges may incentivize marginal students to finish high school. The effects of reservations in government jobs, however, are theoretically ambiguous and depend on the shape of the probability function. Since different government jobs (classes A–D) have different thresholds—literacy, finishing primary school,

[17] The Supreme Court's interpretation: "Where an educational qualification has been prescribed in the recruitment rules, all candidates including the SC, ST and OBC candidates shall satisfy the said qualification. Sometimes, a minimum number of marks or a minimum grade is prescribed as part of the educational qualification in the recruitment rules. In such cases, the minimum marks/grade so prescribed shall uniformly apply to all candidates including SC, ST and OBC candidates."

[18] In a similar vein, Basant and Sen (2016) look at the 2006 reservations in higher education institutions, and Gille (2013) studies stigma associated with caste.

[19] However, evidence in the American context shows little support for the role model hypothesis. It instead suggests that benefiting minority students are less popular because they are accused of "acting white" (Fryer & Torelli, 2010; Ogbu, 2003).

finishing middle school, high school, or college—the effects depend on where in the education distribution students would expect to drop out in the absence of the quota. Students just below the job qualification threshold may work hard to get at least as much education as the government job requires, but it may induce students just above a threshold to drop out early at the threshold and settle for the still lucrative lower-level government job. For instance, for jobs that require only middle school, a student may drop out just after finishing middle school. The shape of the probability function and the different returns to each level of job therefore determine how students respond to quotas.

Importantly, the probability function depends on the extent of the quotas, and reservations increase this probability at each level. Therefore, the size of the impacts is a function of how many seats are available at each level, which I examine leveraging cross-state variation in the intensities of reservations. If the intensity is very high and a lot of low-level government jobs open up, students may drop out early and settle for these still lucrative low-level jobs. If the intensity is high, the policies will have detrimental effects on ineligible groups that must now compete harder for fewer positions that are available.

Two additional features in the Indian context affect the expected impacts. First, the policies are extremely salient and discussed widely, allowing households to plan ahead for future human capital investments. Second, public colleges and government jobs are among the most sought after in the country. Such colleges rank among the highest-quality institutions while charging low tuition. Public sector jobs are attractive, especially for candidates without a college degree, as they provide a lucrative package of higher wages, social authority, job security, and other nonpecuniary benefits. In some ways, the salience of the policies and the clearly valuable expected benefits make this the ideal setting to analyze this question.

IV. Data

I compile a number of data sources specifically for this analysis, including various household surveys and government commission reports. First, I use the Indian National Sample Survey (NSS), a representative, repeated cross-section. This data set has information on educational attainment, caste, age, labor market outcomes, and a comprehensive consumption expenditure module. The nationally representative, large-sample thick rounds of the data are conducted every five years. Since I focus on affirmative action policies instituted in the early 1990s, the main data set used is the 2000 module, which was also the first thick round to ask whether the respondent is OBC and whether the person works in public sector establishments.[20] The 1995 round is too early to capture the effects of early 1990s policies, as changes in schooling decisions take time. The 2005 round is too late and suffers from both other confounding policies implemented in the early 2000s and changes in definitions of OBCs across waves of the survey.[21] In robustness checks, I use the 2005 rounds as well to show consistent results with either round.

Primary source data were compiled on affirmative action policies instituted by the federal government and the various Indian states (appendix table A.5 table notes). I did this by obtaining government reports using the Right to Information (RTI) Act for states in the country. Furthermore, I obtained information on the classification and identification of OBCs for a few states. Importantly, for the purposes of my RD design, the state of Haryana's committee reports laid out detailed methodology and underlying tables used for identifying who qualifies as an OBC.

The third source is the 1999 ARIS-REDS (Additional Rural Incomes Survey and Rural Economic and Demographic Survey) data set. The NSS is nationally representative, but it has information only on four broad caste categories. Despite having a smaller sample, the ARIS-REDS (for sixteen states) asks respondents their subcastes and thus has information at a finer level.

V. Empirical Framework and Results

I use three identification strategies to provide a comprehensive picture of the incentive effects of affirmative action. Across empirical strategies, I leverage variation along a few dimensions: age, eligibility, and the intensity of reservations. The average age for entering the last year of high school is 17 years. Figure A.1 shows a sharp drop (16 percentage points) in enrollment rates at the age of 18, when most students finish high school.[22] At the time that reservations were implemented, anyone under the age of 18 years could have changed their schooling.

In figure A.2, I plot the trends in educational attainment by birth cohort and social group. Cohorts born before 1976 would be over the age of 18 by the time the federal government policy was implemented in 1994. There is not much convergence in education among early cohorts, but for cohorts born shortly after 1975, OBC education grew faster than for other groups.[23]

[20]The NSS 55th Round was collected between July 1999 and June 2000 using a stratified two-stage sampling design. First, clusters (rural villages/urban blocks) were sampled, and then twelve households within each cluster were sampled.

[21]A policy change in the early 2000s was OBC scholarships under the Ninth Five-Year Plan, negating the usefulness of the 2005 round. This was also the period leading up to reservations in national-level colleges.

[22]The Factories Act of 1948 and Mines Act of 1952 banned employment of persons under the age of 18. Many public sector jobs are available only to people who are at least 18.

[23]For cohorts born after the 1980s, there was also a growth in SC-ST education, perhaps reflecting the direct impacts of additional affirmative programs and scholarships for this group. Importantly, they upheld the decision to provide reservations in job promotions for the SC-ST groups and established a national commission for SC-STs. As such, using SC-STs in control groups would attenuate results.

There are, nevertheless factors that confound the identification of these effects. If the quality of schooling and the number of schools increase, then costs of attending are lower, which tends to discourage students from dropping out. The government of India made large investments in schooling in the early 1990s, and although the education reforms were not caste or class specific, they may confound estimates. I compile original data and account for the reforms in robustness checks. I show falsification tests with other minority groups (e.g., Muslims and poorer sections of upper castes), for whom I find no detectable effects on educational attainment, lowering the likelihood that other coincidental policies are driving the main results.

Table A.1 shows socioeconomic status by group. On most indicators, such as education, expenditure, and urbanization, OBCs lie between SC-STs and upper-caste households. Public sector work is highest among rich upper-caste Hindus. There were changes to SC-STs policies in different states over this period, but Muslims and poorer sections of upper-caste Hindus were not beneficiaries of affirmative action and serve as good placebo groups in the empirical exercise.

The results across all three empirical methods paint a consistent picture of the impacts, yet each approach has its own set of strengths and weaknesses. The first strategy (a difference-in-differences, DID) must grapple with other coincidental educational expansions and the fact that reservations may indeed hurt upper-caste individuals who now have less access to jobs. The RD approach may have limited external validity as it is restricted to only one state in India.[24]

There are differences in the estimated parameters too. While the DID estimator identifies the average treatment effect on the treated (ATET), the RD identifies a localized effect—in the neighborhood of the cutoff—for the marginal subcaste. Political agitation from unreserved subcastes raises questions about how expanding benefits to marginal subcastes would affect incentives, a result captured by the RD. If marginal castes are better off than the average low caste, then impacts may be larger, as they have the capacity to take advantage of reservations. Alternatively, if marginal castes are already well off, there is little room for improvement, and the RD treatment effect will be smaller than the DID ATET.

State-level variation captures a third relationship: how treatment effects vary with the intensity of treatment. A very high intensity of reservation expands the pool of reserved candidates, affecting the overall equilibrium and possibly leading to detrimental effects on OBCs, as highlighted by the theoretical literature discussed in section III. High reservation intensity will also hurt the upper caste, who may now need to compete more for the fewer jobs available.

[24]Haryana had about 21 million people in 2000, the year the data were collected. On most indicators (fraction OBC, SC, per capita expenditures, urbanization, educational attainment), Haryana looks similar to the average Indian state. Yet it is different from the typical South Indian state and has a negligible fraction of STs.

A. *Method 1: Difference-in-Differences and Central Government Jobs*

The federal government reserved 27% of government jobs for OBCs in 1993. The double-difference estimator leverages variation on two fronts: age and social group. Some cohorts were too old to be affected by changes in the reservation policy, whereas others will be young enough and still in school. Since the NSS data were collected seven years after 1993, anyone over the age of 24 is unable to change their education. Additionally, high schoolers, who may have already dropped out of school, would find it hard to change their education. We should then see the impact being larger for younger individuals. For instance, the impact on 15-year-olds will be smaller than the impact on 10-year-olds, as many 15-year-olds would have already dropped out of school:

$$edu_{iac} = \sum_{a<50} \sum_{c \neq oth} \beta_{ac} \left(\mathbb{1}_{age=a} \times \mathbb{1}_{caste=c} \right) + \alpha_a + \kappa_c + X'_{iac}\boldsymbol{\gamma} + \epsilon_{iac}. \quad (1)$$

Equation (1) is a flexible DID regression, where edu_{iac} is the education (level or years) attained by individual i in age-cohort a and caste c. I show results for both years and levels of education.[25] Here, α_a and κ_c are age-cohort and caste fixed effects, while β_{ac} is the Treatment on the Treated ($ATET_{ac}$) for caste c in cohort a. X'_{iac} is a vector of controls added incrementally, including higher-dimensional state-by-cohort and state-by-caste fixed effects.

For $a > \bar{a}$, we expect $ATET_{c,a>\bar{a}} = 0$, where $\bar{a}$ is school-leaving age (18 at the time of the policy). If this condition is violated, we may have little confidence in the parallel trends assumption. For $a \leq \bar{a}$ and for the OBC group, we would expect $ATET_{c=OBC,a\leq\bar{a}} > 0$. Additionally, younger OBCs find it easier to change their years of schooling and should be more affected than older OBCs, who may have already dropped out, suggesting $ATET_{c=OBC,a} > ATET_{c=OBC,a+1}$.

Figure 1 plots coefficients β_{ac} for the OBC group across age cohorts, with standard errors clustered across 33 states and union territories.[26] The omitted social group is the Others upper-caste members, and ages above 50 are omitted cohorts.[27] The coefficients are statistically indistinguishable from 0 for cohorts above school-leaving age, and the trend in education is mild, suggesting the absence of strong pretreatment differential trends. For cohorts below $\bar{a}$, the coefficient

[25]The NSS records levels of education. I use the median years in each level to create the years. For instance, primary school often ends in year 5, so the median between one and five years is three. This translation leads to eight distinct mass points. Certain changes in levels of education may be more relevant than the years of education. For instance, the difference between being illiterate and literate even without formal schooling will change the chances of acquiring the lowest-level government jobs.

[26]Cameron, Gelbach, and Miller's (2008) cluster bootstrap procedures produce similar confidence intervals.

[27]The NSS data set has four broad social groups: SCs, STs, OBCs, and Others. The results are similar when the omitted category does include SCs and STs.

FIGURE 1.—OBC COEFFICIENTS OF DIFFERENCE-IN-DIFFERENCES REGRESSION ACROSS AGE COHORTS

Plot of coefficients from DID regression on levels of education (as measured in 2000). Standard errors clustered at the state level. People above school-going age (vertical line) in 1993 should be unaffected by the implementation of the policy. The vertical line indicates the age of implementation. Omitted social group is general category castes. Omitted age group is those above 50.

is positive and statistically significant, and it is larger for younger cohorts as predicted.

In appendix figure A.3, a few other graphs help strengthen this result. First, I look for secular trends across age-caste groups in the data. Since the 2000 wave was the first to ask the OBC identifier question, a prepolicy analysis of this cannot be done. In the 2005 wave, the DID graph is shifted about five to six years to the right, as expected (figure A.3). This may help negate fears of age-specific caste trends that kick in at exactly age 25.[28] In the middle panel, I translate the dependent variable from levels of education to years of education, which is more widely used in this literature. In the final panel, I include a stringent set of higher-dimensional state-by-age and state-by-caste fixed effects.

In table 1, I present the main DID estimates across various specifications and control sub-samples. Panel A shows the results as I incrementally include more controls. In the first column, I show the standard DID result from a specification that simply includes indicators for being young and OBC and the interaction between the two. Here, the increase is 0.8 years of education. In subsequent columns, I include higher-dimensional fixed effects and demographic controls (like gender) sequentially. Doing so does not affect the estimated coefficient by much. Column 5 has state-by-age and state-by-caste fixed effects, along with demographic controls. In column 6 of panel A, I perform the standard pretrends test where I compare cohorts 18 to 34 years old (as young) to those above age 34 and find no evidence of differential pretrends. Table A.2 tests for pretrends by each 10-year age bin.

In panel B of table 1, I restrict the subsample and control groups in meaningful ways. In the first few columns, I include only Muslims or poorer sections of upper-caste Hindus as control groups, as they have similar baseline characteristics to OBCs but do not benefit from affirmative action. In column 9 I restrict the sample to individuals whose household heads do not work in the public sector, to check whether the results are driven by direct effects of income gains from public sector employment. Column 10 restricts the sample to states that had below-median expenditures on Operation Blackboard, a major schooling policy, to ensure that the results are not driven by schooling expansions. In column 11, I look at only high (above median) literacy states, and in column 12, I restrict the sample to students with no older siblings to check whether there may be other effects from older siblings benefiting from reservations.

Table 2 reproduces the main results using levels of education as the outcome. Across specifications and control samples, younger OBCs catch up with upper-caste individuals. The education gap that existed between older OBCs and the upper caste is reduced by 0.6 levels of education, but upper-caste individuals still get more education than OBCs on average.

Possible concerns with the difference-in-differences analysis. The differential trend in education (for OBCs relative to Others) among older cohorts in figure 1 is mild and statistically not different from 0 (table A.2). This gives some support to the parallel trends assumption as $ATET_{c=OBC;a>\bar{a}} = 0$. While one may expect convergence in education over time, in the absence of affirmative action, it is unclear why convergence should kick in at around the same time as reservations.

Yet this strategy cannot rule out effects on the behavior of upper-caste students, who may feel discouraged by the reservations. Alternatively, they may view these policies as increasing the competitiveness of getting a job, and thus work harder and attain more education in order to compete for fewer spaces, attenuating results downward. As far as the federal law change is concerned, these reactions may be mildly mitigated, as political promises were made to expand the number of jobs so as to ensure that general category applicants were unaffected.[29] Other costs include peer effects in the classroom, which affect incentives for upper-caste students in attending school. Rao (2019), however, shows that in a different context, where Delhi public schools were required to admit poorer students, there is evidence to the contrary.[30]

[28]Cross-survey comparisons should be interpreted with caution, as between 1999 and 2005, more subcastes were added to the list of OBCs, and scholarship programs were started. Presumably groups that had enough political capital lobbied to avail themselves of these reservations, undermining comparisons between the 1999 and the 2005 wave.

[29]There is little evidence documenting new jobs. Many OBCs were already in government jobs (13% in 1979, according to Mandal, 1980), so this expansion was not to be large. The act governing reservations in educational institutions "requires institutions to increase the total number of seats in such a manner that the number of unreserved seats (general category) is not reduced due to reservation for OBCs. This was based on the number of seats available for general category before the Act was passed." Such expansions could lead to additional costs.

[30]Other general equilibrium effects include states changing policies in light of the federal government policy. I drop states that introduced affirmative action policies in a five-year span around the federal government

TABLE 1.—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF RESERVATIONS ON YEARS OF EDUCATION

	(1)	(2)	(3)	(4)	(5)	(6)
			Years of Education			
			A. Controls			
OBC × Young	0.804***	0.816***	0.802***	0.753***	0.753***	0.0618
	(0.0988)	(0.102)	(0.0849)	(0.102)	(0.101)	(0.0746)
OBC	−1.211***	−1.139***				
	(0.263)	(0.193)				
Young	−0.592***		−0.425***			
	(0.0883)		(0.0882)			
Observations	540,089	540,089	540,089	540,058	539,680	273,645
R^2	0.010	0.211	0.112	0.269	0.361	0.337
State-Age FE	No	Yes	No	Yes	Yes	Yes
State-Caste FE	No	No	Yes	Yes	Yes	Yes
Controls	No	No	No	No	Yes	Yes
Sample	All	All	All	All	All	Above 18
	(7)	(8)	(9)	(10)	(11)	(12)
			B. Samples			
OBC × Young	1.111***	1.178***	0.686***	0.817***	0.564***	0.698***
	(0.116)	(0.135)	(0.0947)	(0.152)	(0.141)	(0.104)
Observations	287,190	262,492	524,381	294,091	285,478	112,988
R^2	0.324	0.334	0.361	0.363	0.366	0.360
State-Age FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Caste FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Poor, Muslim, and OBC	Poor and OBC	No public sector	Low Operation Blackboard	High literacy states	No older sibling

Note to panel B: Difference-in-differences estimates on years of education using the National Sample Survey 2000. Standard errors clustered at the state level.

Standard errors are clustered at the state level. "Young" are defined to be under the age of 18 in 1993 (except in column 6, where it is defined as being between 18 and 34). The "All" sample includes all castes.

Panel A varies the controls used in columns 1 to 5, and checks for pretrends in column 6. Column 1 shows estimates with no controls. Column 2 includes state-by-age fixed effects. Column 3 includes state-by-caste fixed effects, where caste has four categories: SC, ST, OBC, and general. Column 4 includes by state-by-age and state-by-caste fixed effects. Column 5 includes these fixed effects and the following controls: relationship to household head, marital status, continuous age, land owned, land possessed, land cultivated, gender, household monthly per capita expenditure. Column 6 checks for pretrends by restricting the sample to only those above the age of 18 in 1993 and redefines "Young" as being between ages 18 and 34 (compare those between 18 and 34 to those 35 and above in 1993 as a test of pretrends.)

All columns in panel B (columns 7–12) have the same set of fixed effects and controls as in column 5. Column 7 changes the control group to be only Muslim or upper-caste Hindus from the bottom two deciles of the expenditure distribution. Column 8 changes the control group to be only the bottom two deciles of the expenditure distribution for upper-caste Hindus. Column 9 drops anyone who worked in a public sector enterprise (state or central). Column 10 restricts the sample to states that had below-median expenditures on Operation Blackboard. Column 11 restricts the sample to high female literacy states as measured by the 1991 Census; these states had below-median intensity of implementation under the District Primary Education Program (DPEP). Column 12 restricts the sample to those without older siblings.

TABLE 2.—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF RESERVATIONS ON LEVELS OF EDUCATION

	(1)	(2)	(3)	(4)	(5)
			Level of Education		
OBC×Young	0.581***	0.563***	0.562***	0.863***	0.601***
	(0.0888)	(0.0843)	(0.0830)	(0.0957)	(0.124)
Observations	540,662	540,631	540,239	287,460	294,369
R^2	0.009	0.260	0.352	0.316	0.354
State-age fixed effects	No	Yes	Yes	Yes	Yes
State-caste fixed effects	No	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes
Sample	All	All	All	Poor, Muslim	Low Operation Blackboard

Difference-in-differences estimates on levels of education using the National Sample Survey 2000. Standard errors clustered at the state level. "Young" are defined to be under the age of 18 in 1993. The different levels include illiterate, literate but below primary school, finished primary school, finished middle school, finished secondary school, finished higher secondary school, and college graduate. Column 1 is the raw difference-in-difference where the controls include an indicator for OBC and for being young. Column 2 includes state-by-age and state-by-caste fixed effects. Column 3 also includes the following controls: relationship to household head, marital status, continuous age, land owned, land possessed, land cultivated, gender, and household monthly per capita expenditure. Column 4 includes all controls and fixed effects and further restricts the sample to low-income upper castes (bottom two deciles) and Muslims. Column 5 restricts the sample to states that had below-median expenditure on Operation Blackboard.

My calculations do not incorporate the costs to upper-caste members of society or the costs to any changes in the public sector. These costs are clearly important from a social welfare point of view.

Remaining concerns include simultaneous policy changes.[31] In 1986, the National Policy of Education (NPE) built new schools and recruited more teachers across the country. This program was not OBC specific, but may lower the costs of schooling for disadvantaged communities. The largest expenditures were in hiring primary school teachers under Operation Blackboard and building schools

policy. The results remain similar, and as I show later, controlling for state policies does not negate the main result.

[31] Other concurrent changes were the liberalization reforms of 1991. Kohli (2001) discusses how the growth from the large-scale reforms of the 1991 story has largely been concentrated on urban, English-speaking, educated middle-class families and has been unable to bridge the inequalities between these social groups. Around 2001–2002, the government tried to implement non-caste-based policies to universalize elementary education and the Millennium Development Goals (MDG), but the effects of these policies are not being captured here since the data set was collected before the MDG projects were implemented.

TABLE 3.—PLACEBOS: LOW-INCOME UPPER-CASTE, AND MUSLIMS

	(1)	(2)	(3)	(4)
	Years of Education			
Placebo × Young	0.0436	−0.0407	−0.0105	0.170
	(0.316)	(0.172)	(0.161)	(0.140)
Young	−1.126***	−1.081***	−1.104***	−0.299**
	(0.117)	(0.122)	(0.148)	(0.109)
Placebo	−1.906***	−1.697***	−1.661***	−0.848**
	(0.447)	(0.291)	(0.302)	(0.323)
Observations	217,042	217,042	217,042	485,896
R^2	0.039	0.046	0.044	0.004
Sample	Hindu Upper Caste			Hindu, Muslim
Treated placebo	Poorest 10%	Poorest 20%	Poorest 30%	Muslim

Difference-in-differences estimates on years of education using the National Sample Survey 2000. Standard errors clustered at the state level. "Young" are defined to be under the age of 18 in 1993. The table shows the effects on alternative placebo samples. Column 1 compares the increase in education for the poorest decile of upper-caste Hindus to all other deciles. Columns 2 and 3 do a similar exercise with the bottom two and three deciles. Column 4 compares the increase in education for Muslims over Hindus.

under the District Primary Education Program (DPEP).[32] I reconstruct measures for the intensity of both programs and control for flexible polynomials of their intensity. Doing so does not affect my results (figure A.4).[33] Additionally, state-by-caste or state-by-age fixed effects in table 1 account for most policy changes that do not vary at the caste-by-age level.

Unlike affirmative action, school-building programs and GDP growth should also affect other disadvantaged groups like the low-income, upper-caste population and the Muslim population (Desai & Kulkarni, 2008).[34] Yet, in panel B of table 1, I show that when Muslims or poorer upper caste are used as the control group, the effects on OBCs are as strong. Table 3 looks at the direct impacts on Muslims and poorer upper castes and finds them to be both economically and statistically insignificant. These falsification tests support the main results, and the other identification strategies used in this paper are unaffected by the simultaneous educational interventions. Given that the timing of education gains lines up with the implementation of reservations, these results are suggestive. As such, the DID results show that after the Mandal (1980) recommendations were implemented, there was a marked increase in OBCs' educational attainment. Yet the DID results cannot be conclusive on the mechanisms underlying the rise in education for OBCs.

The creamy layer. The case of *Indira Sawhney v. Union of India* (1993) prompted the Supreme Court to exclude relatively wealthy members of the OBC community from reservations. This excluded group was referred to as the "creamy layer" and consists of children of people with high-ranking constitutional and civil service posts, large landowners, and richer members of certain professional occupations. The members of these occupations are subject to an income test, where their annual household income must be below 100,000 rupees (approximately $2,000) to be eligible for reservations.[35]

Using the NSS Labor Force Survey data, I identify occupational groups, industrial sectors, and income levels, allowing me to approximately classify persons as "creamy layer" or not.[36] Table A.3 produces a triple-difference table that interacts the main effect with an indicator for likely being creamy layer. While there is some impact on the creamy layer group—probably a result of income-reporting manipulation or other ways of getting around the eligibility criteria—the impact on the noncreamy layer group is more than double the size of the creamy layer group. The triple-difference estimator is a statistically and economically significant 0.64 years of education.

Transition between education levels and effects across the distribution. In being eligible for government jobs, levels of education are important milestones in the qualification criteria. While on average students are incentivized to get more education, there may be parts of the distribution where students get less education and drop out early to get a lower-level public sector job. In order to see how the transition takes place, I estimate a DID effect for each level of education (using the highest attained grade as a 1/0 indicator).

In table A.4, there is a clear transition away from illiteracy (and away from below-primary and primary levels of education) and into secondary school and college. Panel B excludes college goers by artificially truncating the data in order to focus on transitions at the precollegiate level. Figures A.5 and A.6 display these results in terms of a transitional cumulative density function (CDF), and the results are statistically and economically meaningful. Given that the largest fraction of the government jobs (58%) were group C jobs that required a secondary school level of education, the largest impacts are at the secondary level.

B. *Method 2: The Intensity of Treatment across States*

Since different states have, over time, passed laws reserving state-level seats and state public sector jobs for OBCs, this kind of analysis can be done for each state separately. About

[32] Chin (2005) shows that for Operation Blackboard, despite hiring new teachers, teachers per school did not increase and class sizes did not decrease. There was merely a redistribution of teachers from larger to smaller schools. Khanna (2016) uses a regression discontinuity design to show DPEP increased education at the cutoff, but this result did not differ by caste.

[33] The figures also show little or no immediate differential impact of launching the National Policy of Education in 1986 since persons between the ages of 24 and 30 at the time of the survey should be affected by the National Policy of Education but not by reservations.

[34] In table A.1 it is clear that these social groups are as disadvantaged as OBCs are.

[35] This threshold has since been raised and now stands at 600,000 rupees (approximately $12,000).

[36] This constructed measure is imperfect as (a) the labor force survey identifies only broad occupational groups, not specific occupations, and (b) persons close to the income cutoff may find it easy to manipulate their bank statements and income tax returns in order to qualify for reservations. The law stipulates that the income criteria will be based on household income, where the definition of *household* is subject to manipulation.

sixteen states had meaningful reservations for OBCs before the central government law was passed, and the rest followed soon after. In figure A.7, I perform an analysis on a sample of state law changes, where the vertical line represents a marked change in reservation policy for the OBC group in that state.[37] By restricting the sample to the corresponding state and plotting the coefficients, the state-wise changes in reservations often have impacts similar to the federal law change.

Each state, however, chooses a different intensity of reservation, and so what fraction of seats is reserved is clearly an important policy question. Even federal commissions like Mandal (1980) are essentially formed to decide the intensity of such reservations. Indeed, the theoretical results in section III suggest that there may be detrimental effects of a very high intensity of reservations if it leads employers to devalue the educational qualifications of minorities.

While variations in social group and age were leveraged in the DID section, here I empirically investigate a third dimension of variation: the intensity of the reservation policy that varies across states. I define *intensity of reservation* as the ratio between the percentage of quotas and the population percentage for each group: $\frac{quotas\%}{population\%}$ in 1995. For instance, in the state of Karnataka, this ratio for OBCs is $\frac{53}{36} = 1.47$, whereas in the state of Madhya Pradesh, it is only $\frac{13}{40.5} = 0.32$.[38] Table A.5 lists the fractions reserved and population by state.

In such an exercise, the primary independent variable of interest is an interaction term between the three dimensions of variation: being OBC, being young, and the intensity of reservations. Yet it is important to also control for each of these dimensions so as to isolate meaningful variation in the triple-interaction term. In table A.6, I introduce these controls and fixed effects one by one, starting with fixed effects for each of these categories alone and, finally, with the caste-by-age and state-by-age fixed effects. Across specifications, the triple interaction term is positive, suggesting that more intensive reservations may allow OBCs to catch up faster with the upper caste.

The more conservative approach, however, is akin to a continuous form of the triple-difference estimator (Gruber, 1994), where the three dimensions of variation are age, caste, and intensity of reservations. In equation (2), edu_{ics} is the education level obtained by a person i belonging to caste c and residing in state s. $\mathbb{1}_c$ is an indicator for each caste group, and *young* equals 1 for cohorts that were still in school or will attend school after state-level changes in reservation policy have been implemented. $\boldsymbol{Z}$ is a vector of controls.[39]

Table 4.—The Intensity of Treatment and State-Level Variation

	(1)	(2)	(3)	(4)
	Level of Education			
OBC × Intensity × Young	0.199	0.199	0.191	0.187
Standard errors				
State	(0.108)*	(0.108)*	(0.105)*	(0.107)*
Region	(0.085)**	(0.085)**	(0.084)**	(0.085)**
SC-ST controls	No	No	Yes	Yes
Demographic controls	No	No	No	Yes
Sample	All	Intensity > 0	All	All
Observations	532,688	528,082	532,688	532,657
R^2	0.014	0.015	0.072	0.120

Dependent variable is level of education. Standard errors in parentheses clustered at the state-level (thirty states) or NSS-defined region level (seventy seven regions). The omitted caste category is the general category. Specification in column 2, Intensity > 0 drops the two states that have no reservations for OBCs. Controls in all specifications include an OBC indicator, a young indicator, the state-level intensity of reservations, and all double interactions between these variables. Column 3 has SC-ST controls: an SC indicator, an ST indicator, state-level intensities of SC reservations, state-level intensity of ST reservations, a young indicator, and all double interactions between these variables. Column 4 has additional controls: relationship to household head, marital status, continuous age, land owned, land possessed, land cultivated, and gender.

Such an approach allows us to control for caste-specific trends ($\mathbb{1}_c \times young$), state-specific trends ($intensity_{cs} \times young$), and state-specific caste preferences ($\mathbb{1}_c \times intensity_{cs}$). The remaining variation comes from how the impacts of OBC reservations vary with intensity. Specifically, the parameter $\gamma_{c=OBC}$ captures how the ATET varies with intensity of treatment: if more intensity allows OBCs to catch up faster, then this parameter should be positive:

$$\begin{aligned} edu_{ics} = {} & \sum_c \beta_{1c}\left(\mathbb{1}_c \times young_i\right) + \sum_c \beta_{2c}\left(intensity_{cs} \right. \\ & \left. \times\ young_i\right) + \sum_c \beta_{3c}\left(\mathbb{1}_c \times intensity_{cs}\right) \\ & + \sum_c \gamma_c\left(\mathbb{1}_c \times intensity_{cs} \times young_i\right) + \mathbf{Z}'\boldsymbol{\beta} + \epsilon_{cs}. \end{aligned} \tag{2}$$

The first two columns of table 4 group SC-STs with the general category, whereas the last two columns include controls for each caste group separately (the general category is the omitted group). The final column also includes a set of demographic controls. Across specifications, we see that the effect of affirmative action policies is larger in states that have a higher intensity of reservations. An increase in the intensity by 1 unit increases the treatment effect of these policies by 0.19 levels of education for OBCs.

This regression specification, however, imposes a linear functional form. In order to explore nonlinearities in the relationship between intensity and the effect of reservations, I follow a method used by Donald and Lang (2007).[40] I use a two-stage estimation procedure by first computing the treatment effect for each state and then regressing that treatment effect on the intensity of reservations. In order to find the

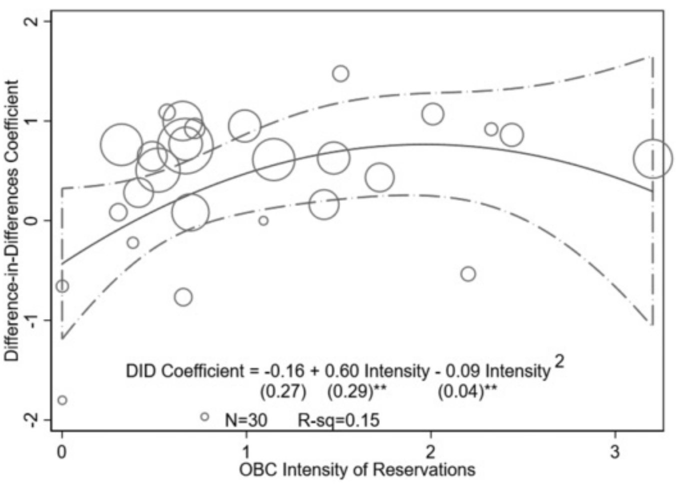
[37] Many of the changes consisted of implementing OBC reservations for the first time, whereas others increased the fraction of seats reserved.

[38] The federal law change should not differentially affect residents of different states because people are competing for federal seats with people all over the country.

[39] I present results with and without controls, where the controls include relationship to household head, marital status, continuous age, land owned, land possessed, land cultivated, and gender. In addition, some models control for the intensity of SC and ST reservations and the interactions with the young indicator and SC ST indicators (a fully saturated model).

[40] This method simultaneously tackles issues of few clusters and nonlinearities in treatment.

FIGURE 2.—RELATIONSHIP BETWEEN TREATMENT EFFECT IN A STATE AND STATE-LEVEL INTENSITY OF QUOTAS



Auxiliary regression of relationship between the ATET and intensity of OBC reservations by state, in the vein of Donald and Lang (2007). The first step estimates the DID relationship for each state separately, from a regression of education on OBC, young indicator, and the interaction between the two. The second step plots these coefficients (*y*-axis) across intensity of reservation (*x*-axis). The bubble size indicates the population of the state.

treatment effect in each state, I use a DID strategy with only the subsample of each state. I then plot the DID coefficient across the intensity of reservation by each state. In figure 2, I plot the auxiliary regression that captures this relationship, which is increasing at a decreasing rate.[41]

The concavity in results may be consistent with many of the theoretical predictions discussed in section III. A very high intensity of reservations may be detrimental to the minority group, as employers devalue any educational attainment by them (Coate & Loury, 1993), which in turn lowers the incentives for minority groups to invest in education in the first place. Additionally, there may be complacency effects, and as such, in signaling models, if intensive reservations imply enough high-ability students can separate themselves from low-ability OBCs by getting more education, then low-ability students may drop out early (Bedard, 2001). Finally, a very high intensity may also mean that there are many slots available at lower positions that require less education, which may incentivize students to drop out early and settle for the lower-level government job.[42]

C. *Method 3: RD and Difference-in-Discontinuities*

For my final empirical strategy, I exploit a state-determined methodology of identifying and classifying OBCs and obtain a regression discontinuity (RD) estimate of the impacts of reservations. Such an analysis is new to the affirmative action literature and provides a causal impact of affirmative action policies. The RD estimate here is unencumbered by issues of convergence and other concurrent government policies. For instance, government spending on schooling should have uniform impacts on castes just below and above the cutoffs determined by the eligibility methodology. There is also the benefit of identifying a different and interesting parameter: the effect of such policies on a student from the marginal subcaste.

Classification and identification of OBCs for state-level reservations are the prerogative of state governments. States appoint committees to determine who OBCs are and what reservations they should be eligible for. Some committees conduct a socioeconomic survey and use these data to rank different subcastes on the basis of socioeconomic indicators. Castes above a certain cutoff of backwardness are eligible for reservations. This setup allows us to estimate the impacts of the reservation policy by comparing subcastes just above and below the cutoff.

The RD can then be aided by an additional source of variation. Once again, certain cohorts were too old at the time the policies were implemented to be affected by these reservations. I perform a difference-in-discontinuities analysis, using the subcaste index to identify the discontinuity and the age cohorts to identify the difference in the discontinuities for each cohort.

The analysis in this section focuses on the state of Haryana, which had one such methodology for classifying OBCs. The Singh (1990) *Haryana Backward Classes Commission Report* was the first ever committee in the state, which has the added advantage of preventing any lingering policies from contaminating the before-and-after analysis. The commission conducted a survey and created a score out of a total score of 60. Any caste that had more than half the total score was considered an OBC. A halfway mark is an intuitive cutoff point, and it is thus unlikely that the cutoff itself was manipulated to include certain castes. It is also unlikely for people of different castes to manipulate their score as the index is based on large-scale survey data where the respondents were probably unaware of the utilization purpose of this data. Indeed, I observe no bunching of castes just above the cutoff (figure A.8).[43] Manipulation of the methodology from the government's side is also unlikely, since they use the same methodology (and halfway cutoff) formulated by the Mandal (1980) Federal Commission. Finally, I test that the treatment is discontinuous at the cutoff and that other baseline characteristics vary continuously.[44]

Singh (1990) creates an index of backwardness based on social, educational, and economic disadvantage. The social disadvantage criterion looks at ten indicators, including

[41] Figure 2 drops two outlier states that have very large intensity values because of negligible OBC populations. These states are among the smallest in the country (Goa and Mizoram).

[42] There are costs of intense reservations on general category candidates who have fewer seats.

[43] Scheduled Castes were assigned the highest possible index value as they were already eligible for reservations (and are therefore not near the threshold). This shows up in the bunching at the highest possible index value of 60.

[44] In 1995, the Ramji Lal Committee—the second backward classes commission in Haryana—added four castes to the list. In the data, this adds two castes below the cutoff. However, since these castes will have felt the benefits for less than three years (the data were collected in 1999), they have been coded as ineligible. This does not change the results.

employment in manual labor and the unorganized sector and access to proper sanitation or other amenities. The educational criterion uses ten indicators related to dropout rates, female literacy, test scores, and vocational education. The economic index looks at fifteen indicators such as family assets, consumption expenditure, maternal mortality, and unemployment. The survey was done in 53 villages and 4 towns, and the report produces caste-wise tables on each of the 35 indicators used in the final index. From the raw data tables, I reconstruct the index, which matches the final index produced. The major difference with the federal policy of 1993 is that the federal policy was only for government jobs, whereas here seats were reserved in colleges as well.

For the RD analysis, I used the ARIS-REDS 1999 data set. The nationally representative NSS data cannot be used since they leck disaggregated subcaste categories, which we require for the RD analysis. In figure 3a, the dependent variable is the difference in mean years of education between the older and younger members of that caste.[45] Once again "older" is defined as being too old to change education (over age 18) at the time of implementation of the reservation policy. There are 27 subcastes for which the ARIS-REDS and the Haryana Committee Report have matching caste names.[46]

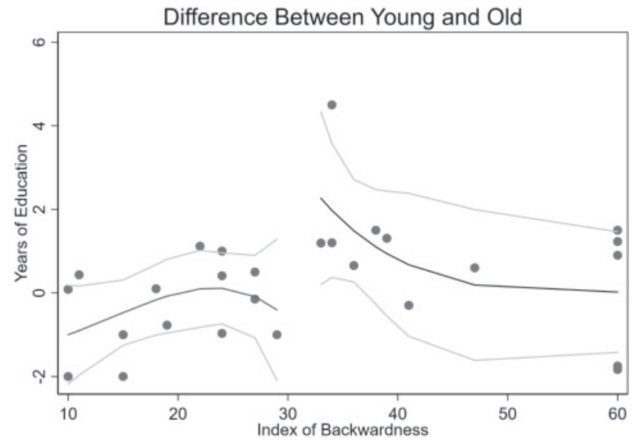
If, however, we look at the mean education level of the population that is too old to be affected by the reservation policy, we see no discontinuity at the cutoff (figure 3b). This figure shows a slight downward trend, since a higher index indicates a larger socioeconomic disadvantage. In panel a of Table A.10, there is no statistically or economically significant effect on the education of the older population. Panels B to D show balance on other characteristics.

Relying on the RD literature, I explore various specifications, including smaller bandwidths around the cutoff (Imbens & Lemieux, 2008) and higher-order polynomials (van der Klaauw, 2008). Regression tables mention the index's polynomial order, and "Restricted BW" indicates that the sample includes only half the bandwidth around the cutoff (index values of 15 to 44). "Flexible Slope" indicates that the slope varies on either side of the cutoff.[47]

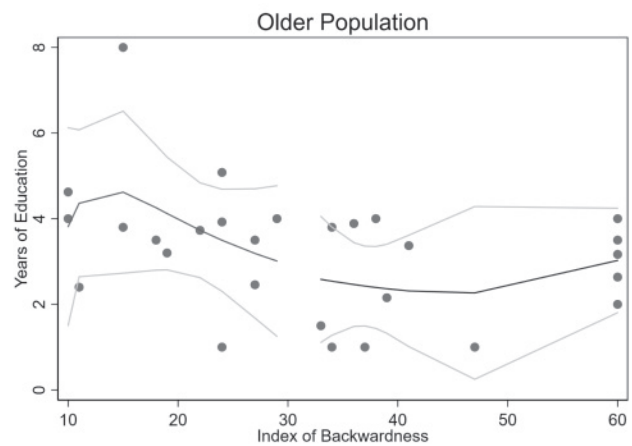
The RD framework identifies the localized treatment effect in the neighborhood around the cutoff, or the neighborhood average treatment effect (NATE).[48] There is a large mass of students who get no schooling whatsoever. These students presumably belong to subcastes further away from the cutoff, having the highest levels of "backwardness." This NATE therefore may be larger than the ATET found via the DID methodology since the ATET is pulled down by students who have the highest costs of schooling. Students who are far from the RD cutoff with high values of "backwardness" would presumably respond only to extremely large changes in the returns to education to budge them on the extensive margin of attending school. Alternatively, students in the marginal subcaste may already be well off and have little to gain from affirmative action. In this case, the NATE would be smaller than the ATET.

I use three distinct regression specifications. The first approach, a difference-in-discontinuities, combines a DID with the RD to estimate the differential discontinuity for the young. The second restricts the sample to only the young and estimates the discontinuity for the young sample. The third approach, a discontinuity-in-differences, is what figure 3a plots, where the dependent variable is the mean difference in

FIGURE 3.—REGRESSION DISCONTINUITY: EDUCATION BY OBC STATUS



(a) Average Change in Years of Education by Caste



(b) Mean Education by Caste of Older Population

Data from ARIS-REDS 1999. Panel a displays regression discontinuity on mean years of education $young_c$ − mean years of education for old_c, where "young" is under the age of 18 at the time of the policy (in 1991) and "old" is over the age of 18 at the time of the policy. See table A.9 for an analogous regression. In figure 3b, the sample is restricted to those over the age of 18 (too old to benefit from the reservation policy) at the time of the policy. Regression discontinuity on years of education. See table A.10 for regression result.

[45] The outcome is the mean years of education for young in subcaste c minus the mean years of education for old in subcaste c.

[46] While 34 of the ARIS-REDS castes could be matched to the names in the committee report, there were no data on seven of these castes in the Haryana ARIS-REDS subsample.

[47] Data-driven bandwidth selection procedures, like the one discussed in Calonico, Cattaneo, and Titiunik (2014) cannot be used in this context as there are only 27 mass points of the index.

[48] Sometimes referred to as the local average treatment effect (LATE).

TABLE 5.—DIFFERENCE IN DISCONTINUITIES

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of Education					
OBC × Young	0.948***	1.418**	0.988***	1.603***	1.059***	1.439**
SE	(0.220)	(0.647)	(0.211)	(0.467)	(0.223)	(0.660)
Wild bootstrap p	[0.000]	[0.118]	[0.000]	[0.002]	[0.000]	[0.118]
Observations	2,262	1,914	2,262	1,914	2,262	1,914
Flexible slope	X	X	X	X		
Polynomial	1st	1st	2nd	2nd	1st	1st
Restricted BW		X		X		X
R^2	0.100	0.103	0.105	0.113	0.096	0.102

Dependent variable is years of education. Level of significance (based on clustered standard errors): ***0.01; **0.05; and *0.1. Columns vary the control-function polynomial. "Restricted BW" consists of half the index span around the cutoff (index values 15 to 44). "Flexible slope" allows the slope of the regression specification to vary by old-young and on either side of the cutoff. Standard errors are clustered at the caste level, and wild bootstrap p-values are presented. For robustness to alternative specifications, higher-order polynomials, and different subsamples, see table A.7.

education between the younger and older members within a caste (this regression is at the subcaste level).[49]

The first approach is relatively new to the literature (Grembi, Nannicini, & Troiano, 2016). This is my preferred specification since it uses the entire data set and maximizes power. This incorporates both the discontinuity along the caste index and differences across older and younger age cohorts. In equation (3), I interact the cutoff with the variable *young*. This interaction term should have a positive sign since those under age 18 can change their education in response to the reservation policy. When $young = 0$, the discontinuity should be close to 0 (as seen in figure 3b and table A.10), but when $young = 1$, those above the "backwardness" cutoff should increase their educational attainment, and coefficient β_1 will be positive:

$$edu_{ic} = \beta_0 \mathbb{1}_{index>0} + \beta_1 \left(\mathbb{1}_{index>0} \times young_i\right) + \beta_2 \, young_i + f(index, young_i) + \epsilon_{ic} \text{ for } index \in \{-\upsilon_1, \upsilon_1\} \tag{3}$$

Table 5 shows an increase in education for OBCs. The results are economically and statistically meaningful and of a similar magnitude across specifications, providing a consistent story across the different polynomial orders and bandwidths. The average value for the coefficients across specifications is about 1.2 years of education. Table A.7 investigates a few variations: I restrict the sample to fewer age cohorts to account for nondiscreteness in school-leaving ages, drop anybody who attended college, and control for higher-order polynomials of the index. The results are consistent across specifications.

In my second approach, I use conventional RD methods after I restrict the sample to younger cohorts who would be able to change their schooling and control for the mean education level of the older population in that caste. I cluster errors at the caste level and again show the wild small-cluster bootstrap p-values (Cameron et al., 2008). Again, in some specifications, I restrict the sample to a tighter bandwidth around the cutoff. The regression of interest is

$$edu_{ic} = \beta \, \mathbb{1}_{index>0} + f(index) + \overline{ed\ old}_c + \epsilon_{ic} \quad \text{for } \{-\upsilon_1 < index < \upsilon_2\}. \tag{4}$$

Table A.8 shows similar coefficients as before across different sample restrictions: First, I show the main result for the young subsample. Then I vary the age cutoff that defines "young" to allow for nondiscreteness in school-leaving age. Finally, I show results for subsamples with or without college education. Consistently, the causal impact of reservations is to increase years of education for the average OBC student near the cutoff. In my final approach (a discontinuity-in-differences), the regression corresponds to figure 3. I control for a flexible polynomial of the index $f(index)$:

$$\overline{ed\ young}_c - \overline{ed\ old}_c = \beta_0 \, \mathbb{1}_{index>0} + f(index) + \epsilon_c. \tag{5}$$

The coefficients of interest in appendix table A.9 are positive and statistically different from 0. In the second-order polynomial column, the coefficient shows that the causal effect of reserving seats for backward classes is to increase their education by about 1.6 years. I also perform a specification check by dropping three years above and below the age cutoff so as to account for impreciseness in the school-leaving age criteria.

I conduct numerous robustness checks to validate these results. The RD design requires all other factors (e.g., demographics, assets) to vary smoothly at the cutoff. In panels B to D of table A.10, there are no other discontinuities at the same threshold. The table presents results on farm stock, medical expenditure, and household size. Table A.11 reproduces the main result without an outlier caste that was near the RD cutoff. We can also look for educational discontinuities at any other value of the index. Figure A.9 shows cutoffs at 20, 25, 35, and 40: no other values of the index have statistically or economically significant discontinuities.

VI. Discussion

I use three distinct approaches to answer the primary question of interest: Does affirmative action incentivize students to

[49]The RD results are robust to dropping outliers, and each subcaste one by one.

stay in school? The DID estimated a treatment on the treated (ATET) of about 0.8 years of education, whereas the NATE from the RD strategy is somewhere around 1.2 years of education.[50] The NATE may be the relevant estimate of interest if the government is considering adding another subcaste to the list of OBCs, while the ATET may be important if the government wants to know the overall impact of changing the amount of quotas for all OBCs. Finally, the state-level intensity variation tells us how increasing the intensity of quotas may affect the magnitude of these treatment effects. As some theoretical models predict (Coate & Loury, 1993), a state that reserves a very high fraction of seats for OBCs may end up in a low-level equilibrium. While each of the three approaches has strengths and limitations, all point to a similar set of results.

These results raise the obvious question: Is this educational response from the minority group rational in terms of the expected costs and benefits? While the salience of the policy and widespread attention it got in the media and political spheres suggests that there may be behavioral responses as well, here I perform a simple calculation to see whether the response is consistent with the changes in expected earnings as predicted by the theoretical setup. In both the 2000 and 2005 waves of the NSS data, the naive, unconditional wage premium for working in a public sector enterprise is approximately $485 a year.[51] Between the 2000 and 2005 NSS waves, the fraction of OBCs in public sector jobs rose from 22% to 27%, and the likelihood of being in a public sector job conditional on being an OBC enterprise worker rose from 10.5% to 11.4%. This small rise of 0.93 percentage points implies that the yearly expected wage gain is only about $4.49. Assuming a thirty-year tenure, a 6% real interest rate, and a stable wage premium (as it seems to be over the early 2000s), this implies a present value expected gain of only $61.80. The mean expenditure for schooling in private schools is about $16.80 a year around that period (Das et al., 2013), suggesting that the response for the OBC group is rational in terms of the expected costs and benefits.[52]

I find that policies that raise the returns to education for OBCs encourage them to increase their education by half a schooling level. This has strong implications as it highlights the possibility of encouraging students to cross relevant thresholds of education. It is challenging to compare results to other work, as much of the other literature focuses on schooling cost reductions rather than increases in returns.[53] Yet there is some good evidence on how returns affect education. Using the booming IT sector as a sign of increasing returns to education, Oster and Steinberg (2013) find that school enrollment rises by 7% when a new IT center is opened. Similarly, Shreshta (2016) finds that the Gurkha community in Nepal attained one more year of education in response to a change in army eligibility laws. However, Jensen and Miller (2015) and de Brauw and Giles (2008) show that schooling investments may actually fall in response to higher returns.

Contrary to the expectation of complacency effects, I find that lowering standards may have positive incentive effects. There is, however, a nonlinearity, as very high levels of reservations may lead to a "patronizing equilibrium" (Loury, 1992). These results probably differ by context. In the United States, affirmative action lacks the backing of salient features, like explicitly reserving seats for certain groups, and the interventions are a lot smaller in size.

The policies also come with certain costs, most notably on upper-caste members. These costs include fewer seats to compete for, possible disruptions in public sector efficiency, and peer effects in schools. My analysis does not study these costs, which are clearly important from a social welfare perspective. Yet even for members of the minority group who do not get government jobs, an increase in education may translate into benefits (like better health) and high wages as the estimated returns to education in developing countries are between 6% and 13% (Psacharopoulos & Patrinos, 2004). Indeed, lowering educational inequalities—and possibly wealth inequalities—may be intrinsically valuable to policymakers. In light of these results, policymakers should consider the externalities of affirmative action policies when designing them.

[50] The RD and DID are looking at different policies, as the DID looks at reservations only in governmental jobs, not colleges.

[51] This may be an overestimate if there is sorting on ability or an underestimate given the additional benefits and job security associated with public sector jobs.

[52] This analysis is crude in that it ignores a few factors that shift this result in opposing directions: the opportunity cost of going to school raises the costs of schooling. But there are also many other benefits to education not included: the standard Mincerian return, nonpecuniary benefits to health, and, importantly, the nonpecuniary benefits of government jobs (including job security).

[53] Kazianga et al. (2013) show that enrollment rises by 20% in girl-friendly schools, and in Dinkelman and Martinez (2014), absenteeism falls by 14% when financial aid information is provided.

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