Incentivizing Standards or Standardizing Incentives? Affirmative Action in India

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Abstract

Affirmative action raises the likelihood of getting into college or obtaining a government job for minority groups in India. I study how this change in future prospects affects schooling incentives, and find that minority group students are incentivized to stay in school longer. This approach is unique, in that it focuses on the incentives affirmative action gives to those who are not yet eligible for the policy *per se*. I create a comprehensive primary dataset using state commission reports which allows for a regression discontinuity and difference-in-discontinuities analysis. These results are supported at the national level using a difference-in-differences approach, and utilizing variation in state-level policies. Together these estimators consistently show that affirmative action policies incentivize about 1 additional level of education for the average minority group student, and 1.6 more years of education for a student from a marginal minority sub-group. Given the debate about the effectiveness of such policies, it is particularly important for both researchers and policymakers to account for these incentive effects when evaluating affirmative action programs.

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1 Introduction

Given the lack of interventions that change the returns, the education literature has focused on reductions in the costs of schooling, rather than the extent to which increasing returns to education may raise educational attainment. In this paper, I study the causal impact of affirmative action policies on educational attainment in India, focusing on policies that 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 level is required to be eligible for certain programs, changing the returns to education and potentially leading to large indirect effects on skill acquisition. I find that by raising the future expected returns to education, such policies incentivize minority groups to stay in school for longer.

Affirmative action is a contentious issue for policymakers and academics in the developed and developing world, including the US, India, Sri Lanka, Malaysia, Nigeria and Brazil. The research and subsequent policy debates involve issues of college-mismatch (Arcidiacono et al., 2011), direct effects of affirmative action on college enrollment or test-scores of beneficiaries (Bagde et al., 2016), and the consequent effects on non-minority groups (Bertrand et al., 2010). However, there is little known about the impacts on human capital investment incentives for potential *future* beneficiaries, and my paper examines the extent to which these exist.

I take three distinct empirical approaches in my analysis. First, I study a nationwide law change that reserved federal government jobs for certain lower-caste candidates. These jobs required specific educational qualifications, thereby raising the returns to certain educational levels. By comparing eligible to non-eligible castes, and student age-cohorts that were young enough to change their schooling decisions to those who were too old, I determine the change in educational attainment for the average low-caste student. I find that, on average, such students attain 1.38 more years of education. These effects are absent among non-eligible minority groups, non-eligible candidates within the eligible minority groups, and low-income students from the ineligible upper castes.

The average effects, however, say little about how this impact would change as we increase the 'intensity of reservations,' which I define to be the fraction of seats reserved relative to the fraction of the population that is from the minority group. So for my second approach, I use affirmative action laws for college-admission and government jobs at the state level, rather than the federal level. I create an original dataset based on historical laws passed in each state in the country by petitioning the Government for archived commission reports. I then exploit three sources of variation – the timing of these laws, the minority groups eligible, and this intensity of reservations – to determine how changing the intensity of these programs affects educational attainment. Certain states reserve a larger fraction of their seats than other states. By comparing low-intensity states to high-intensity ones, I show that the relationship between the change in educational attainment and relative fraction of seats reserved is concave. This suggests that extremely intensive affirmative action programs may detrimentally lower the educational attainment of minority groups.

The above two results, however, do not address the effects of expanding these benefits to an additional marginal sub-group. India has numerous sub-castes, some of which are eligible for affirmative action benefits, and others that are not. In my third approach, I compare sub-castes that just received the programs to sub-castes that just lost out, to causally identify this parameter. One of the states in India conducted a large socio-economic survey and ranked the sub-castes on an 'index of backwardness' in order to determine which sub-castes should be eligible for these programs. Any sub-caste that had a score greater than half of the total value of the index was eligible. Using a regression discontinuity (RD) design I compare sub-castes on either side of the cutoff. Since the educational attainment of older members of these castes should not be affected by the introduction of this policy, I further use them as a control group in a difference-in-discontinuities approach. I find that, on average, a student from the marginal sub-caste in that state attains 1.58 more years of education. This suggests that there are plausibly large positive unintended effects of expanding the coverage of these programs to other marginal minority sub-groups.[1]

While the Regression Discontinuity (RD) and Difference-in-Discontinuities approach exploit certain policy features in a particular Indian state, the cross-state intensity and the national-level Difference-in-Differences approaches are representative of the entire country. All estimators consistently point towards an increase in educational attainment for the targeted minority group in response to reservations in higher education and government jobs. More importantly, the different estimators determine different parameters that would be useful for any welfare analysis of these policies. While the Difference-in-Differences approach estimates the average impact, the RD determines the impacts on the marginal sub-caste. The state-level variation allows me to determine how these impacts vary with differing intensities of the policies. All three parameters are crucial for any meaningful discussion of the costs and benefits of these policies.

This paper is unique in its approach, in that it is among the first papers to empirically study the causal impacts on incentives before the benefits of the policy actually kick-in. Unlike the other papers in the India context, I use nationally representative data to look at the impacts on the entire country, rather than on a subset of engineering colleges. While most research focuses on college-admissions I also look at labor market affirmative action policies, and study the impacts on the extensive margin of drop-outs rather than the intensive margin of test-scores. Last, I compile an original data set of state-level laws, and exploit a state's law to perform a regression discontinuity and a difference-in-discontinuities analysis to identify the causal impact of the policies.

[1] I use the term 'unintended' since to the best of my knowledge there are no official documents claiming that changing the incentives for minority group students was part of the intended outcomes.

In the rest of this section I discuss the possible effects of affirmative action programs by couching it in the relevant theoretical and empirical literature on the returns to education and affirmative action policies. In Section 2 I discuss the context of caste and class in India, and the underlying legal and historical foundation behind these policies. After which, in Section 3 I discuss the data and provide some descriptive evidence of the trends over time for different socio-economic groups. In Section 5, I set-up a dynamic optimization model with testable implications, and predictions on what may confound the empirical analysis. The main focus of this paper is Section 6. It discusses the various empirical strategies used and their corresponding results. The last section concludes, and discusses policy implications.

1.1 Returns to Education and Human Capital Investments

Government education policy in low-income countries is usually associated with lowering the costs of education rather changing the returns (King and Orazem, 2008). Numerous examples of schooling expansion programs that reduce both the monetary and non-monetary costs for students can be found across the developing world.[2] The government policies I study in this paper, are unique in that they change the future returns, rather than the current costs of schooling. Since the benefits are in the future, programs that affect returns are likely to have different impacts than an immediate tangible fall in the costs of schooling. Furthermore, while costs are easy to perceive, information on returns and future opportunities may be poor in low-income settings. This lack of information may skew the demand for schooling (Dinkelman and Martinez, 2014; Jensen, 2010).

Outside of government programs, a literature exists on human capital investments in response to changes in the returns to education. This is true not just of the US (Freeman, 1976; Griliches, 1997; Kane, 1994; Ryoo and Rosen, 2004), but also in the developing world, and especially the Indian context. On the one hand, this research finds evidence of increases in schooling with increasing returns. One of the earliest papers in the Indian context by Foster and Rosenzweig (1996) shows how the Green Revolution led to an increase in primary schooling arguably because of higher returns to education. Similarly, Kochar (2004) finds that households increase schooling in response to higher returns in the nearest urban labor market, and Jensen (2012) shows that better jobs for women in the IT sector of Delhi increases schooling for girls. At higher education levels, Khanna and Morales (2015) studies how an increase in returns to IT sector jobs in the US and India increases enrollment in engineering schools in the 1990s.

On the other hand, increasing the returns to education may have adverse effects in such low-income

[2] Some examples can be found in Indonesia (Duflo, 2001), Burkina Faso (Kazianga et al., 2013), Zimbabwe (Aguero and Bharadwaj, 2014), Nigeria (Osili and Long, 2008), Sierra Leone (Cannonier and Mocan, 2012), Uganda (Deininger, 2003), Zambia (Ashraf et al., 2015), Kenya (Bold et al., 2013), Tanzania (Sifuna, 2007), West Bank & Gaza (Angrist, 1995), and India (Afridi, 2010; Chin, 2005; Khanna, 2016).

settings. Jensen and Miller (2015) show that strategic incentives amongst rural Indian households can actually lower educational attainment in response to higher returns to education. They argue that parents that want a child to remain at home and look after them, curb their child's migration opportunities by lowering their educational investments. Access to higher wages regardless of skill level, may also raise the opportunity cost of schooling – de Brauw and Giles (2008) find that school attainment falls in rural China in response to better migration opportunities, because of higher opportunity costs. It is, therefore, unclear what the expected impacts of changes in expected wages are in the developing country context.

1.2 Affirmative Action in the US and India: Theory and Evidence

Theoretical work on affirmative action discusses different types of behavioral responses. The Coate and Loury (1993) employer-learning model shows that under certain assumptions such policies can indeed encourage effort, and over time the policies could lead to a 'benign equilibrium' where employers' negative stereotypes about the minority group are eradicated. However, under other assumptions it could lead to a 'patronizing equilibrium' where the negative stereotypes persist, potentially discouraging human capital accumulation. If employers devalue the credentials of any minority group candidate because of the affirmative action policies, it can disincentivize members of the minority group from obtaining education.

Similarly, in a signaling model, affirmative action may discourage investments for low-ability minority group students. If in the absence of affirmative action, even the high-ability low-caste students would not get into college and only finish high-school, then the low-ability low-caste students would finish high-school as well, so as to take advantage of being 'pooled' with the high-ability students. If affirmative action allows the high-ability low caste students to attend college, then we get a separating equilibria where the low ability students would drop out a lot earlier.[3]

It is, therefore, crucial to understand not only who is affected but also how intense these programs are. In the Indian context, affirmative action programs are more salient and larger in magnitude than in most other countries. Reserving a large fraction of seats may allow low-ability low caste students to get into college and into public sector jobs, exacerbating employers' negative stereotypes. If such employers discriminate against future applicants it may discourage further human capital investments. On the other hand, large-scale reservations may also lead to a higher 'pooling equilibrium' whereby both the high and low ability students from the minority group get more education – the low-ability students taking advantage of being pooled with the high-ability students. It is, therefore, important to study not just the average impacts of the entire minority group, but also how increasing the intensity of reservations affects these average impacts, and how enlarging

[3]This is a modification of a result shown in Bedard (2001).

the definition of the minority group may affect outcomes for this additional sub-group. I explore all these parameters in my analysis.

What then should be the form of affirmative action? Fryer and Loury (2005) show that 'equal opportunity' is often not enough to close educational inequalities that arise from historical discrimination, and increasing the probability of getting into college may motivate students to graduate from school, overcoming 'effort pessimism.'[4] Hickman (2013) uses an auction theory based structural model and compares various forms of potential policies to show that race quotas in the US would induce more human capital investment by minorities, but could involve a larger welfare loss than other possible policies. The literature also mentions 'complacency' effects of such policies on incentives for schooling – for instance, smarter sections of the minority group could put in less effort knowing that it is easier to get into college.[5] In the political and academic sphere these possible outcomes are the topic of contentious debate. Nonetheless, there is little empirical evidence to back up these claims.[6]

The evidence in the US context highlights large possible costs associated with such policies. Arcidiacono et al. (2014, 2011) study the impact of affirmative action policies on college fit and mismatch, and show that laws banning the use of racial preferences in California public colleges lead to better match quality and higher graduation rates in colleges. The form of preferential treatment is also important – Domina (2007) shows that the diversity programs enacted in Texas, after affirmative action was banned, boosted educational outcomes at the high-school level. Furthermore, eliminating the use of race-based affirmative action in Texas and California state universities did not seem to adversely impact the SAT-sending behavior of highly qualified minority group students (Card and Krueger, 2005). Using the reinstatement of affirmative action in Texas, a recent working paper by Akhtari and Bau (2017) show that minority student aspirations and achievement did, in fact, rise. Lastly, if peers are seen to benefit from this policy, then a 'role model' effect may also have a positive impact on educational attainment. 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 and Torelli, 2010; Ogbu, 2003).[7]

In the Indian context, the literature has focused on the direct impacts on a sample of engineering colleges. Empirical work suggests that college-reservations are well targeted, improve the performance of the minority groups in question, and have "*strong positive economic effects*" (Bertrand

[4] Anthropological studies in the American context suggest that difficulties faced by minority groups in finding employment ('job ceiling' hypothesis) discourage them from attaining education (Ogbu, 2003).

[5] Assuncao and Ferman (2015) show that affirmative action reduced test scores of minority groups in Brazil.

[6] Weisskopf (2004) provides a theoretical comparison of affirmative action policies in the US and India, and discusses the various expected effects of such policies, including the impacts on incentives to stay in school.

[7] Teacher-student pairings of the same race, however, have been found to have positive impacts, which may be evidence in support of a role-model effect (Dee, 2004).

et al., 2010). Bagde et al. (2016) also look at a sub-sample of engineering colleges in a particular Indian state, and argue that reservations have a "*significant and substantial positive effect both on college attendance and first-year academic achievement.*" On the other hand, Krishna and Robles (2015) look at a detailed data set from an engineering college in India and show that affirmative action policies lead to mismatch – minority students end up earning less than they would have if they picked less selective majors. While most papers study a group of engineering colleges, I study the country as a whole. Furthermore, the focus in the literature is on the direct outcomes at the collegiate level, whereas my work looks at educational attainment at primarily pre-collegiate levels of schooling, before the policy benefits kick in.[8]

2 Caste, Class and Reservations in India

In India, affirmative action policies are defined on the basis of caste or social class, and the policy interventions are much larger and more salient than in most other countries. Reservations are part of the platform for political parties, election campaigns center around them, the media covers it extensively, and any policy changes are met with protests from different factions. The Constitution identifies certain castes as the most disadvantaged group and codifies them as the Scheduled Castes (SCs). It also enumerates certain aboriginal tribal groups, which are referred to as Scheduled Tribes (STs). Over time there has been an 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). There is almost no literature on affirmative action for OBCs, and I focus on this group in my paper.

Over time, the Indian government has instituted laws whereby a certain percentage of seats in colleges or government jobs are set aside for low-caste candidates. This 'reservation policy' primarily benefits SCs, STs and OBCs in various ways. The primary purpose of this law is to provide a level playing field for communities that have suffered from historical discrimination. The Constitution states that "*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.*" This law allows states to autonomously reserve seats for different communities in state-run universities and in government jobs, producing useful variation from a researcher's point of view. There is some macro-level evidence highlighting the possible effects of these laws on SC-STs – Desai and Kulkarni (2008) show that educational inequalities have been falling over time for SCs and STs, that do benefit from reservations, but have not been declining for the Muslim community who are excluded from the current reservation policy. Cassan (2017) studies historical access to a

[8] In a somewhat different vein, Rao (2016) studies a program that required Delhi public schools to admit students from poorer backgrounds, and found that this had large positive impacts on the pro-social behavior of richer peers.

combined bundle of preferential policies for SC-STs and finds increases in educational attainment for men but not women.

In 1980, a Commission was established to determine what percentage of seats should be reserved in national universities and federal government jobs for OBCs (Mandal, 1980). The report recommended reserving 27% of the seats in national colleges and federal jobs for the OBCs that they identified. This was met with large protests from the urban upper-class public who argued that they were being discriminated against, and that the disadvantaged groups already had a 'level playing field' (Kohli, 2001). In 1993, the federal government implemented the first stage of the Mandal (1980) Commission recommendations by reserving 27% of government jobs for OBCs, and then in 2006 the reservations in colleges were implemented. The Indian Supreme Court excluded the more well-off members of the OBC community (known as the 'creamy layer') from taking advantage of these policies, and this is another source of variation that is exploited.[9]

These caste-based reservations at the central level exist alongside the state laws, which vary in intensity across states. In one empirical strategy, I focus on the state of Haryana, which ranked sub-castes on the basis of their socio-economic disadvantage, and classified the worst-off sub-castes as OBCs. This ranking allows me to obtain a Regression Discontinuity estimate of the impact of these policies. I exploit variation across various dimensions: caste, age, region and eligibility in order to determine the impact of such policies. While state-level laws provide quotas in both educational institutions and government jobs, the federal law changes studied here will focus on OBC reservations in government jobs.[10]

Importantly, there are four categories of government jobs, all of which are eligible for quotas. The highest category (Groups A and B) require finishing high-school or having a college degree, and these comprise of 11.5% of all the jobs. These are mostly high-level civil servants. The next level (Group C) need candidates to finish either middle school or secondary school, and consist 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 complete primary school. Group C and D jobs include lower skilled jobs like revenue inspectors, assistants, clerks and drivers. Therefore, the incentive effects will not just be seen in graduating from high-school, but also in attaining certain levels of education that make candidates competitive for these jobs.

My paper is among the first to look at reservations in government jobs. Prakash (2010) shows that policies in the 1980s that reserved jobs for a different minority group – the Scheduled Castes (SCs) – had substantial impacts on the probabilities of formal sector employment and wages for this specific minority group. Given that OBCs are slightly better off than SCs to start with, we

[9] In 1993, the Supreme Court upheld the implementation of reservations for OBCs in government jobs in the landmark case: *Indira Sawhney v. Union of India, 1993* and introduced the concept of the creamy layer.

[10] This is because the federal level implementation of OBCs in higher educational institutions only happened recently in 2009.

may expect that there would be even more qualified OBCs to avail themselves of these quotas. My work tests whether these future prospects of better employment and wages can actually induce students to reach the educational thresholds required for these jobs.[11]

3 Data

I compiled a number of data sources specifically for this analysis, including various household surveys and governmental commission reports. First, I use the Indian National Sample Survey (NSS), which is a representative repeated cross-section. This data set has information on educational attainment, caste, age, and host of labor market outcomes along with a comprehensive consumption expenditure module. The nationally-representative large-sample 'thick' rounds of the data set are enumerated every five years. Since this paper focuses 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 questions about whether the person is OBC or not.[12] The 1995 round is too early to capture the effects of policies instituted in the early 1990s, since changes in schooling decisions take time. And the 2005 round is too late and may suffer from other confounding policies introduced in the interim years, and changes in definitions of the OBC group across waves of the survey.[13] In my robustness checks I use the 2005 rounds as well to show that my results are consistent with either round.

The data set has information on *level* of education, rather than years of education. The various levels of education are (a) illiterate, (b) literate without formal schooling, (c) literate with formal schooling, (d) primary school, (e) middle school, (f) secondary school, (g) higher secondary school, (h) college educated. Even though we may expect the level to matter for eligibility for jobs and colleges, I discuss how to translate these levels into years to be consistent with the rest of the literature, and present results for both changes in the levels of education and years of education.

Primary source data was compiled on affirmative action policies instituted by the federal government and the various Indian states. I did this by obtaining government reports via the Right to

[11]In the US, there is a literature that shows that schemes like the federal contractor program, under which the targeted groups of women and African-Americans, were given preferential treatment, increased their employment and the demand for them in such sectors (Leonard, 1984; Smith and Welch, 1989), but there is little consensus on the impacts of court-ordered affirmative action in the US (Donohue, 1991).

[12]The NSS 55th Round was collected between July 1999 and June 2000 using a stratified two stage sampling design. First, clusters (rural villages or urban blocks) were sampled, and then 12 households within each cluster were sampled.

[13]One big policy change in 2000 was the introduction of OBC level scholarships under the Ninth Five Year Plan (see Gupta(2004)). Another was the implementation of the Millennium Development Goals (MDGs) in 2000. If the 2005 data set was used, then this policy would make it impossible to disentangle the direct effects of reservations in government jobs, because of the coincidental presence of scholarships and the MDGs.

Table 1: Social Groups in India

	SC	**ST**	**OBC**	**Others**	**Total**
Sample Size	94098	66798	195579	237102	593577
Proportion of Sample (%)	15.85	11.25	32.95	39.94	100
Mean Education Level	3.62	3.90	4.26	5.55	4.63
Mean Years of Education (Approx.)	3.04	3.40	3.90	5.68	4.42
Illiterate (%)	46.20	50.93	42.13	28.02	38.34
College Educated (%)	1.96	1.63	2.79	8.42	4.76
Household Month Exp (Rs.)	1245	1444	1440	2074	1609
Per Cap Month Exp (Rs.)	398.67	427.32	446.33	519.02	465.67
Urban (%)	30.87	22.61	33.21	48.17	37.62
Work in Agriculture (%)	56.00	72.89	53.31	40.36	51.53
Wage work (not Casual) %	26.61	32.65	40.46	68.38	45.85

Source : *NSS* 2000. Summary tabs re-weighted with sampling weights.'Others' are general category individuals (i.e. not SC, ST or OBCs). 'Mean Education Level' covers 8 levels of education from illiterate to college graduates. Nominal exchange rate: approx Rs. 50 to $ 1. Household Monthly Expenditure deflated by rural-urban-region-wise CPIs.

Information (RTI) Act. This dataset is comprehensive in that it has information on reservation policies for all states in the country. Furthermore, detailed knowledge on classification and identification of OBCs was found for a few states. The states in question had Committee Reports that laid out the methodology for identifying Other Backward Classes (OBCs) and their recommendations for reservation policy. Therefore, some of the estimation procedures will allow me to look at the effect on the entire country, while more detailed analysis has been done for the states where the in-depth reports were obtained.

The third source is the ARIS-REDS (Additional Rural Incomes Survey and Rural Economic and Demographic Survey 1999) data set. Unlike the NSS data, ARIS-REDS has information on disaggregated sub-castes. While the NSS is nationally representative, it only has information on four broad caste categories. Despite having a smaller sample, the ARIS-REDS asks respondents their sub-castes, and thus has social-group information at a finer level. Neither data set has information on educational aspirations and expectations, nor test scores, which would have been useful for additional analysis.

4 Descriptive Evidence

By the end of the decade, in 2000 – when the NSS dataset was collected – there were about 3.9 million Central Government jobs, and 45% of them were in the Indian railways. Only 15% of these jobs were in large cities, 53% of them were in rural areas, and the rest were in small towns. In the

2000 NSS survey, 60% of all formal sector jobs and 14.8% of all enterprise-based jobs, and 2.5% of all jobs were in the public sector (both state and central government).

Table 1 uses NSS data to summarize the primary variables of interest by social groups. About one-third of the sample was self-reported to be OBCs. The proportion of SCs (16%) and STs (11%) are smaller. Looking at the mean education level by social group, it is clear that SCs and STs have the lowest rates of educational attainments, whereas OBCs do slightly better than them, but worse than the non-OBC/SC/ST group (known as 'Others' or 'upper-caste'). The mean education level for the upper-caste group is 5.5, indicating that not a large proportion of them are in college either.[14] The mean expenditure for schooling is \$6.85 on average, but about \$16.8 for private school goers (Das et al., 2013).

SCs, STs and OBCs have lower monthly expenditure compared to the rest of the population (Table 1). These three disadvantaged groups are predominantly rural and work in the agricultural sector. They are also more likely to be employed as casual labor rather than in formal wage employment. Kohli (2001) discusses how the Indian growth story has largely been concentrated on urban, English-speaking, educated middle-class families, and the large-scale reforms of 1991 have been unable to bridge the inequalities between these social groups.

In the 2000 NSS data, the yearly wage premium for public sector jobs relative to other jobs with any enterprise is \$485 for OBCs, and \$465 for all people. Over the next decade, government wages were still higher than private sector wages, but private sector wages were rising at a faster rate. Changes in the education distribution and increasing the supply of skilled work to the public sector may have effects on the public sector wage. This is however unlikely in this scenario since government wages are fixed, in real terms, for a decade at a time by the Pay Commissions (1983, 1994 and 2006). These Pay Commissions fix wages and compensation for all public-sector employees and tie them to the rate of inflation.

Since different levels of government jobs require different educational qualifications, I can study the educational levels for public sector employees using the NSS data. The education distribution for OBCs with government jobs displayed a skew towards higher levels of education. 15.45% of them had less than or a primary school degree, 14.42% finished middle school, 25.13% finished secondary school, whereas 16.63% finished higher secondary school and 28.37% finished college.

Calculations using the NSS data show a large increase in public-sector employment for OBCs over time. In 1999-00 (six or seven years after implementation of the law), 22% of public sector jobs were held by members of the OBC community, whereas in 2004-5 this number was about 27.7%. Representation of OBCs in government jobs has steadily increased since the implementation of the

[14] This is important, since if we expect the upper-caste to have reached the ceiling in educational attainment then the reservation policy should have no impact.

policy to match the 27% amount of seats reserved for them.[15]

In the decade before the quotas were implemented (1981-91), all public sector employment grew by 24% whereas Central government jobs grew by only 7%. Since 1997 Central government full-time employment actually slightly shrank because they began outsourcing and cut full-time jobs to part-time contract work. Between 1998 and 2002, 31% of all public sector vacancies were filled by OBCs. Using, the 2005 round of the National Sample Survey (NSS), one can also look at public sector employment by cohort. Only 23.4% of public-sector employees for cohort born in 1946-55 are OBCs, whereas this is number is 27.4% for the cohort born in 1976-85. By 2011, 6.9% of Central Government Group A workers and 17% of Group D workers were OBCs. The Ministry of Personnel, Public Grievances and Pensions, in 2012 claims that the "*representation of OBCs is still low for the reason that reservation for them started only in 1993. Moreover, those OBCs recruited before 1993 have not shown themselves as OBCs [since] they were recruited/ appointed as general candidates.*"

5 A Simple Theoretical Framework

In this section, I set up a simple dynamic optimization problem to highlight the possible effects of quotas on the incentives for students in school, and the empirical challenges (Cameron and Heckman, 2001). Every period an agent chooses to dropout or stay in school. If the returns to s years of schooling is $w(s)$, then for a discount rate β, the agent's value function is:

$$V_s = \max_{Dropout,Stay} \left\{ \sum_{t=s}^{\infty} \beta^{t-s} w(s), \;\; -c_i + \alpha_i + \beta \left(p_{si} \frac{1}{1-\beta} B(s) + (1-p_{si}) V_{s+1} \right) \right\} \tag{1}$$

In this equation, $B(s)$ is the expected net extra benefits from going to college or getting a government job.[16] For those that don't drop out and don't get a government job, they can make the choice again next period V_{s+1}. The simplest version of the model doesn't allow for on-the-job search, and once the agent drops out and gets a job at wage $w(s)$ he/she earns that wage forever.[17] The cost of an additional year of schooling for person i is c_i, which generates heterogenous levels of optimal schooling. This varies by person, and is drawn from a distribution $F(c)$. α_i

[15] For a large fraction of the OBC population, a government job may be their best option given the level of job-security, and non-pecuniary benefits in addition to the pay. Additionally, if there is discrimination in the private sector employment market, then a government job is a more lucrative option.

[16] Government jobs may not only pay a higher wage, over and above the going wage, but also provide job security. This is implicit in the model as $w(s)$ and B are in utility units.

[17] Currently the agent has perfect foresight. In alternative specifications, expectations of benefits can be made to depend on the information set of the agent. This will allow us to see the impacts of a change in the peer group that benefits from affirmative action, and incorporate the possibility of 'role model' effects.

captures the preferences for schooling, and would be affected by 'role-model' effects, if present.[18] The probability of getting into college or getting a government job p_{si} is a function of various factors:

$$p_{si} = p(schooling_i,\ caste_i,\ quotas_i,\ grades_i,\ peers_i,\ ability_i,\ Z_i)\ , \tag{2}$$

where Z_i is a vector of other individual characteristics that generate heterogenous responses to changes in the probability. The probability function can be different in various contexts. For instance, different levels of government jobs require different levels of education, which means that the probability p_{si} could discretely jump across levels of schooling. Getting into college, however, requires one to complete high-school, as $p_{si} = 0$ for any s below the last year of schooling.

For an increasing and concave wage function $w(s)$, the value function converges to an optimum under certain regularity conditions,[19] and can be solved by using backward induction. The optimal policy function has a threshold strategy, whereby a student chooses to drop out of school when his marginal value from an additional year of schooling is less than the cost he/she must bear. Let this threshold level of education be s^*.

When the probability of getting into college increases, it raises the expected value of an additional year of schooling and lowers dropouts. Thus reservations in colleges can incentivize the marginal student to stay in school for more years; $\frac{\partial s^*}{\partial quotas} > 0$.

The effects of reservations in government jobs is less apparent, and depends on the shape of the probability function. Since different government jobs have different thresholds – literacy, finishing primary school, finishing middle school, high school or college – it depends on the distribution of where students would expect to drop out in the absence of the quota. On the one hand, it could incentivize students just below the job-qualification threshold to get at least as much education as the government job requires. On the other hand, it may induce students just above a threshold to drop out at the threshold. For jobs that require only primary schooling, a student may be encouraged to drop out as soon as they finish their primary-school rather than stick around for longer. The shape of the probability function, and the different wages at each level of government job, therefore, determines how students respond to quotas.

There are, however, factors that may confound the identification of these effects. If the quality of schooling and the number of schools increase, then costs of attending are lower, which also tends to discourage the agent from choosing to drop out of school $\frac{\partial s*}{\partial c_i} < 0$. This is an important result as it will be the primary confounding factor in the empirical specification. The government of India made large investments in schooling at around the same time that affirmative action policies were expanded. These investments were made under the National Policy of Education (NPE)

[18]It is also easy to add an on-the-job ability that raises wages, and is correlated with costs of schooling.

[19]We need the slope of the wage function to be steeper than the probability function: $\frac{1}{1-\beta} w_s(s) \geq Bp_{si}$

program in 1986, under two schemes – Operation Blackboard and the District Primary Education Program (DPEP). Though the education reforms were not caste/class specific, I compile original data and control for both schemes in robustness checks. Furthermore, placebo tests with other minority-groups will be shown to provide evidence that no other coincidental policies will produce these results.[20]

Another implication is the result on test scores. The factors that determine the probability of getting into college p can be seen as substitutes. Marginal students who had a low probability of entering college may now seek to improve their test scores when it is easier to get into college due to reservations, whereas the marginal student who has a high probability of getting into college may actually lower their effort input when it is easier to enter college.[21] This may lower the variance of the distribution of test scores for the minority group in question (Assuncao and Ferman, 2015). However, due to a lack of reliable data, I will be unable to examine how test scores change in response to these policies.[22]

There are few things that jump out from the model. First, the shape of the probability function p_s determines whether students are incentivized or disincentivized to get more education. Since different government jobs have different educational requirements, students may be encouraged to drop out earlier or later depending on which margin they lie on. Second, the probability p_s depends on the extent of the quotas, and more reservations will increase this probability even more. Therefore, the size of the impacts are a function not only of whether seats are reserved or not, but also how many seats are reserved. To get at this, the paper will also exploit variation in the different intensities of reservations across states. Crucially, at the time the government implemented the quotas in 1993, it expanded the total number of jobs so as to keep the number of jobs in the general category unchanged.

6 Identification and Results

I use three different identification strategies to provide a complete and comprehensive of what the incentive effects of affirmative action policies are. The results across strategies paint a consistent picture of the impacts. While the Difference-in-Differences estimator will identify the Average Treatment Effect on the Treated (ATET), the RD will identify a localized effect – in the neighborhood of the cutoff – for the marginal sub-caste. Political agitation from unreserved sub-castes raise the question about how expanding the benefits would affect incentives; a result captured by

[20]One aspect, that may also affect the probability function, is the peer group. If changing the composition of classrooms has peer effects, then that affects the interpretation of the results. There is little evidence of these negative peer effects, however (Rao, 2016).

[21]In the model this can be seen if 'quotas' and 'test scores' are substitutes: $\frac{\partial testscores}{\partial quota} = \frac{\partial testscores}{\partial p}\frac{\partial p}{\partial quota} < 0$

[22]In the empirical section, I also discuss the possible effects on the test scores of non reserved category students.

the RD. State-level variation captures a third relationship: how the treatment effect varies with the intensity of treatment. An extremely high intensity of reservation may expand the pool of reserved candidates affecting the overall equilibrium, as in signaling models.

6.1 Difference-in-Differences

The double difference estimator will exploit variation on two fronts: (a) age and (b) social group. Some cohorts were too old to be affected by changes in the reservation policy. Others will be young enough (and still in school) and can change their level of educational attainment. Additionally, only certain social-groups were eligible, providing variation in policy implementation on the social-group front. As discussed above, the federal government implemented reservations for OBCs in government jobs in 1993, whereby 27% of all public sector jobs were reserved for this group.

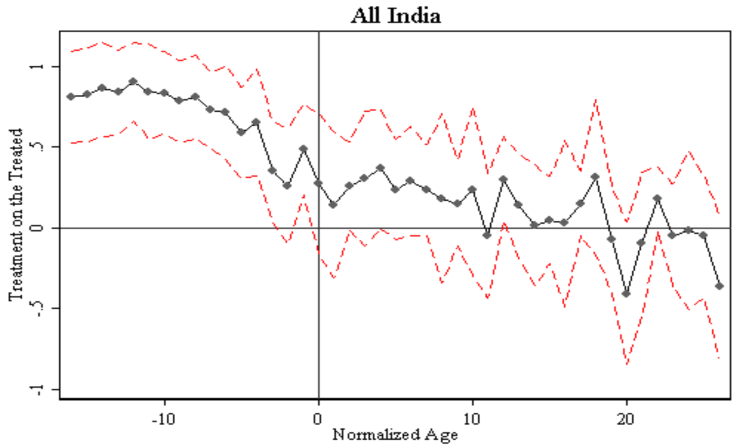
If such measures incentivize attaining a higher level of education, we should expect to see this for the OBC group. The average age for entering the last year of high-school lies between 17 and 18 years. In Figure A.1 one can look at the enrollment rates by ages and see a sharp drop off at the age of 18, which is when most students finish high-school. This represents a 16 percentage point drop. The Factories Act of 1948 and Mines Act of 1952 were the first laws to ban employment of persons under the age of 18. Many public sector jobs are therefore only available to people who are at least 18. At the time that reservations were implemented, anyone under the age of 17 or 18 years could have changed their educational attainment. Since the data was collected 6 to 7 years after that, by that time anyone who is above the age of 24 would not have been able to change their level of education.

Furthermore, there would be many high-schoolers who have already dropped out of school and will thus find it hard to change their educational attainment. We should then see the impact of this policy being larger for younger individuals. For instance, the impact on 15 year olds will be smaller than the impact on 10 year olds, since many 15 year olds would have already dropped out of school.

Equation (3) is the Differences-in-Differences regression, where $\boldsymbol{\alpha_a}$ and $\boldsymbol{\kappa_c}$ are vectors of dummies for age cohort and caste respectively. Here $\boldsymbol{\beta_{0ac}}$ is a vector containing the relevant coefficients, and is allowed to vary by age cohort a and caste c. The coefficient identifies the Treatment on the Treated ($\boldsymbol{\beta_{0ac}} = ATET_{ac}$) for caste c in cohort a. For $a > 24$, we expect $ATET_{c,a>24} = 0$. If this condition is violated, then we may not be satisfying the parallel trends assumption, which would then bias our coefficient of interest. For $a \leq 24$ and for the OBC group, we would expect $ATET_{c=OBC,a\leq 24} > 0$.

Additionally, younger OBCs find it easier to change their years of schooling, and should be more af-

Figure 1: OBC Coefficients of Diff-in-Diff Regression Across Age Cohorts



Plot of coefficients from Difference-in-Differences regression on 'levels of education'. Standard errors clustered at state-level. People above school-going age (vertical line) should be unaffected by the implementation of the policy. Vertical line indicates the year of implementation.

fected than older OBCs that may have already dropped out (i.e. $ATET_{c=OBC,a} > ATET_{c=OBC,a+1}$).

$$edu_{ac} = \boldsymbol{\alpha_a} + \boldsymbol{\kappa_c} + \boldsymbol{\beta_{0ac}\alpha_a} * \boldsymbol{\kappa_c} + \epsilon_{ac} \tag{3}$$

Since the NSS data only asks for education levels and not years of education, the results are produced for both measures.[23] The NSS data set has four broad social groups: (a) SCs, (b) STs, (c) OBCs and (d) Others. The 'Others' category includes the upper-caste section of the population ineligible for any reservations (i.e. not OBCs, SCs or STs). They comprise of 33% of the sample, have a higher monthly per capita expenditure than OBCs, SCs and STs, and are more likely to be urban and salaried (Table 1). More than 67% of Muslims fall into the 'Others' category, and almost 70% of the 'Others' are Hindu. The above regression specification was run where the omitted social group was SC-STs and upper-caste members, and ages above 50 were the omitted cohort category.[24] Figure 1 plots these coefficients $\boldsymbol{\beta_{0ac}}$ for the OBC group across the various age cohorts, and confidence intervals based on standard errors at the state-level.

The coefficients are close to zero and statistically indistinguishable from 0 for age cohorts above the age of 24. This confirms the absence of pre-treatment differential trends. For those below 24, however, the coefficient is positive and statistically significant. Furthermore, it is larger for younger cohorts as the model predicted. At its highest point, the coefficient is below 1, indicating that the

[23] A rough translation from the level of education to the years of education maintains the results. In the Indian context, 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 a low-level government job.

[24] The results are similar, but slightly larger when the omitted category does not include SCs and STs, and only includes them as a separate non-omitted category instead (see Figure A.2).

Table 2: Difference-in-Differences – Years of Education

Education Years	**OBC vs. Others**		
	Younger Cohort	**Older Cohort**	**Difference**
OBC	4.439	4.129	-0.310
	(0.013)	(0.016)	(0.020)
Others	5.579	6.654	1.076
	(0.013)	(0.016)	(0.020)
Difference	-1.140	-2.526	**-1.386**
	(0.018)	(0.022)	**(0.029)**

Using NSS 1999-2000 data. Standard errors in parentheses. Difference-in-Differences value in bold. 'Others' are general category individuals (i.e. not SC, ST or OBCs).

reservation policy caused an increase of at most one level of education for the OBC group; this could be even something informal from a transition away from illiteracy to basic literacy.[25]

Since this is a difference-in-differences coefficient, it captures both the changes over cohorts and across groups relative to older age cohorts. Young OBCs still receive less education than young upper caste students, but the gap is smaller than in the older cohorts by 1 level of education.[26] In the Appendix, there are a few other graphs. One of them reproduces the Difference-in-Differences figure but uses the omitted category to be only upper-caste individuals (Figure A.2) while controlling for the SC-ST categories. This merely makes the impacts slightly larger. Another figure translates the dependent variable from levels of education to years of education (Figure A.4).

The Difference-in-Differences tables can be made by dividing the sample into younger and older cohorts; and OBCs and upper-caste. Looking at the Difference-in-Differences results in Table 2 we see that being an OBC in an age category eligible for education corresponds with a statistically and economically significant increase in educational attainment of about 1.07 educational levels on average (Table 3), and 1.38 years of education (Table 2). The education gap that existed between older cohorts of OBCs and upper-caste individuals is merely reduced by one level of education in the younger cohorts, but upper-caste individuals still continue to get more education than OBCs on average. The gap is bridged by half – an economically significant amount. The ATET in the tables is the weighted average of all the $ATET_{ca}$s seen in the figures, where the weights are proportional to the cohort sizes.

[25] While the effects seem to be plateauing, the data also artificially truncates any larger effects for younger cohorts since those cohorts had not yet reached schooling age at the time of the survey.

[26] At the same time that the government implemented OBC reservations, they also upheld the decision to provide reservations in job *promotions* for the SC-ST groups, and established various National Commissions for the SC and ST groups. In the early 1990s, new policies were initiated to ensure that vacant quota seats were being filled by SC-STs and that upper-caste members were not appropriating the seats for themselves. These policies may attenuate any impacts I find on OBCs.

6.1.1 Addressing Possible Concerns with Differences-in-Differences

One possible concern with the Difference-in-Differences strategy is that of violating the parallel trends assumption. By looking at the figures we can see that older unaffected cohorts do not have a trending education gap with respect to the omitted categories ($ATET_{c=OBC;a>24} = 0$), suggesting that the parallel trends assumption holds in this context. There may also be the concern of mean reversion. While seemingly unlikely, a theory of mean reversion would predict that over time this gap will fall. It is hard to see why this mean reversion should kick-in at exactly the same time as the reservation policy is implemented.[27] Nonetheless, I present evidence to show that mean reversion did not affect other disadvantaged social groups, and the other estimation strategies discussed in this paper will be unaffected by this issue of mean reversion.

Another concern arises if the omitted group is simultaneously 'treated.' Despite the fact that reservations are only applicable to OBCs, we may see a change in behavior of upper-caste members of society for various reasons. One possibility is akin to the John Henry effects discussed in the experimental literature, where the control group may react adversely because they were denied treatment. Upper-caste members of society may feel discouraged by the reservations and lower their educational attainment.[28] Another possible reaction by upper-caste members is to view these policies as increasing the competitiveness of getting a job, and thus working harder and attaining more education in order to compete for these spaces – these would attenuate the results downwards. As far as the federal law change is concerned, these reactions are unlikely since the number of government jobs were expanded to ensure that general category applicants were unaffected. Many states also expanded seats in colleges and jobs in order to accommodate the quotas and ensure that general category applicants had the same number of seats as before. However, there still existed a few states where quotas were implemented without the expansion of seats and thus this may be a concern when interpreting some of the state-wise graphs that I will present.[29]

The general equilibrium effects of such policies may also affect the interpretation of the coefficients found. Increasing the number of seats could lower the wages paid in the government jobs, which may then attenuate the ATETs found. Since the government wages are fixed (tied to inflation) for significant periods of time by the Pay Commissions, and for large sections of society this is the best possible job, the changes in wages may be of little concern. On the other hand, there may be peer effects in the classroom, which may affect the incentives for upper-caste students in attending school. Rao (2016), however, shows that in a different context, where Delhi public schools were required to admit poorer students, there is evidence to the contrary.[30]

[27] Furthermore, models of intergenerational transfers of human capital would predict the opposite trend.

[28] In the US context, there is no evidence of this happening (Ogbu, 2003).

[29] Furthermore, it is unclear how well the expansion of seats was handled at the federal level. Such expansions could lead to additional costs, leading to cut-backs along other margins.

[30] Other general equilibrium effects include states changing policies in light of the federal government policy change. In order to tackle this I drop the states that introduced affirmative action policies in a 5-year span around

One possible remaining concern is that of simultaneous policy changes. As discussed above, in 1986 the Indian government revamped the National Policy of Education (NPE) program and started spending on the improvement of schooling infrastructure, by building new schools and recruiting more teachers across the country. They also expanded scholarships, provided access to adult education, and provided incentives for poor families to send their children to school regularly. This program was not OBC specific. However, as the program will lower the costs of attending school, it may matter the most for communities that have a higher cost of schooling. The results could merely be picking up this declining gap because of this additional spending.

One of the largest expenditures was in hiring more primary school teachers under Operation Blackboard. Chin (2005) shows that despite hiring new teachers, teachers-per-school didn't increase and class sizes didn't decrease. There was merely a redistribution of teachers from larger to smaller schools. And for girls, she finds that this may have impacted the primary school completion rate in states that had a higher 'intensity' of redistribution. I re-construct the measures for the intensity of Operation Blackboard, and control for flexible forms of it in my analysis, and it does not affect my results (Figure A.6a). 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.

The other large policy at that time was the District Primary Education Program (DPEP), originally piloted in 1994, but expanded over the next decade to other districts. Khanna (2016) uses a regression discontinuity design to show the program increased education at the cutoff, but this result did not differ by caste. Figure A.6c controls for a flexible polynomial of the DPEP intensity and an indicator for whether a state received any DPEP funds, and the results are identical to the graphs without controls.

Lastly, unlike affirmative action, these school-building policies should also affect other disadvantaged groups like the low-income upper-caste population, and the Muslim population.[31] From Table A.2 in the Appendix, we can look at the educational attainment and per capita expenditure for the Muslims and poorer members of the upper-caste category (non-OBC/SC/ST). Both categories have mean per-capita expenditures and land assets that are *lower* than those of OBCs, and should thus be a relevant comparison group. While the poorest-fifth of the upper-caste category have very slightly more years of education than the average OBC; Muslims have less years of

the federal government policy. The results remain identical. Some states that had affirmative action policies for more than 20 years prior to the federal government change made minor changes to the amount of quotas, thus the parameter identified here may include that – giving us the policy relevant parameter that includes the inducement of minor state-level law changes. However, as I show, controlling for state-level laws does not in any way affect the impacts of the federal level law change.

[31] Desai and Kulkarni (2008) propose a similar test – when looking at the trends in indicators for SC-ST they compare them to the trends for Muslims., as both minority groups have similar levels of socio-economic indicators, and also similar geographic dispersion.

education, which would imply the possibility of a larger impact on Muslims.

Table 3: Difference-in-Differences Table - Levels of Education

	Panel A:	**OBC vs. Others**	
	Younger Cohort	**Older Cohort**	**Difference**
OBC	4.279	3.673	-0.605
	(0.010)	(0.013)	(0.016)
Others	5.185	5.652	0.467
	(0.009)	(0.012)	(0.016)
Difference	-0.906	-1.978	**-1.072**
	(0.014)	(0.018)	**(0.023)**
	Panel B:	**Hindus vs. Muslims**	
	Younger Cohort	**Older Cohort**	**Difference**
Muslim	3.965	3.565	-0.400
	(0.015)	(0.021)	(0.025)
Hindu	4.527	4.293	-0.234
	(0.007)	(0.009)	(0.011)
Difference	-0.562	-0.728	**-0.166**
	(0.017)	(0.024)	**(0.029)**
	Panel C:	**Rich vs. Poorer Others**	
	Younger Cohort	**Older Cohort**	**Difference**
Others-Poorest 20%	4.059	4.420	0.361
	(0.020)	(0.030)	(0.035)
Others-Richest 80%	5.500	5.909	0.409
	(0.010)	(0.013)	(0.017)
Difference	-1.441	-1.489	**-0.048**
	(0.022)	(0.032)	**(0.039)**

Dependent variable is levels of education. Using NSS 1999-2000 data. Standard errors in parentheses. Difference-in-Differences value in bold. 'Others' are general category individuals (i.e. not SC, ST or OBCs). Levels of education determined by NSS.

Table 3 compares the Difference-in-Differences impacts on OBCs (Panel A) to the analogous impacts on Muslims (Panel B), or the poorer upper-castes (Panel C). While the impact on Muslims is statistically significant, it is economically small, being less than one-sixth of the effect on OBCs.[32] The impact on the poorest 20% of the upper-caste category is both economically and statistically insignificant. The Difference-in-Differences result therefore indicates that the policies incentivized a rise in education by 1.07 levels of education on average.

[32] The slight impact on Muslims can also be explained by the fact that some Muslim groups are also categorized as OBC and can benefit from affirmative action policies.

Comparing the graphs in Appendix Figure A.7, we can see that the largest impact is on the OBCs. Muslims seem to experience little or no impact, but there is a slight change in trend in the poorest-fifth of the 'Others' category many years after the policy was implemented. Nonetheless, the effect on this population is much smaller than the total impact on the OBCs. There still remains a large impact on OBC schooling that can only be explained by the affirmative action policies. In Figure A.8 I restrict the sample to be only OBCs, Muslims and poorest upper-caste, and plot the differential impact on OBCs where the control group is Muslims and the poorest upper-caste. We can see that OBCs still have a substantial differential effect. Furthermore, the other identification strategies used in this paper are unaffected by other educational interventions.

These tables and pictures can also be produced by excluding college-goers. Artificially truncating the sample by dropping all people who have college education allows us to focus on human capital accumulation at the pre-collegiate level.[33] Looking at Figure A.5, in the Appendix, the impacts on the OBC group are still starkly significant. It is natural that the impacts of these reservations lead to an increase in educational attainment even at the pre-collegiate level, since many lower-level government jobs do not require collegiate education.

One can also look for secular trends across age groups and castes in the data. Since the 1999-2000 wave was the first to ask the OBC identifier question a pre-policy analysis of this cannot be done. In the 2005 wave, one can see that the Difference-in-Differences graph is shifted about 5 to 6 years to the right, as expected (Figure A.3). This may help negate fears of age-specific caste trends that kick in at exactly the age of 24.

6.1.2 The Creamy Layer

The *Indira Swahney v. Union of India, 1993* case prompted the Supreme Court to exclude the relatively wealthier members of the OBC group from being eligible for these reservations. This excluded group was referred to as the 'creamy layer,' and consists of sons and daughters of people with high-ranking Constitutional Posts (the President, Supreme Court Judges, etc.), high-ranking civil service posts, and large landowners. It also excludes sons and daughters of richer members of certain occupations (doctors, lawyers, dentists, film professionals, authors, sportsmen, etc.). The members of these occupations are subject to an income test, where their annual household income must be below Rs. 100,000 (approx. $2000) in order to be eligible for reservations.[34]

Using the NSS Labor Force Survey data, I can identify certain occupational groups and industrial sectors, allowing me to classify persons as 'creamy layer' or not. Then using the income information in the Labor Force Survey, I construct the total household income for adults. However, this

[33] Since we are look at reservations in jobs and not colleges (in this section) there is no *a priori* reason to drop college goers, other than to focus on pre-collegiate education.

[34] Since then this threshold has been raised and now stands at Rs. 600,000 (approx. $12000).

constructed measure will be far from perfect as (a) the labor force survey only identifies broad occupational groups and not the 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.[35] Therefore, the creamy layer indicator will be at best, a close approximation of whether the persons took advantage of these policies or not.

Table 4 produces the Difference-in-Differences tables for the creamy layer and non-creamy layer groups separately. While there is some impact on the creamy layer group – which could be a result of income-reporting manipulation, or other ways of getting around the eligibility criteria – the impact on the non-creamy layer group is more than double the size of that in the creamy layer group. The triple-difference estimator is the difference between the two double difference estimates in the table, and is a statistically and economically significant 0.614 years of education. One can also run the triple-difference regression (results shows in Appendix table A.5), which produces the same estimate. The tables therefore show that the bulk of the impact was on the non-creamy-layer households.

6.1.3 Transition Between Education Levels and Effects Across the Distribution

While the Difference-in-Differences estimates show that on average, there was an increase of about 1 level of education for OBCs, it says little about the transition between the different levels of education. In being eligible for government jobs, these levels of education are important milestones in the qualification criteria. The policy allows for reservations at any of the four classes of public sector jobs. While on average students are incentivized to get another level of 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, one can make Difference-in-Differences tables for each level of education (using the highest attained grade as a 1/0 indicator). For instance, looking at Secondary School grade attainment in Table A.3, it can be seen that only 8.7% of older OBCs had secondary school as their highest grade attained, whereas this number is 14.6% of the older individuals in the upper-caste category. The difference-in-difference coefficient (0.028) shows that there is a relative (to the upper-caste group) transition of the OBCs into having secondary school as their highest grade attained.

These tables can be produced for every level of education to look at the relative transition of OBCs in and out of certain grade levels. The difference-in-differences coefficients for each grade level are reproduced in Table A.4. These tables were also made for the sample excluding college goers, by artificially truncating the data and dropping all college-goers, in order to focus on transitions in

[35]The law stipulates that the income criteria will be applicable to 'household' income, where the definition of 'household' is also subject to manipulation.

Table 4: Years of Education: Creamy Layer vs. Non-Creamy Layer

Panel A:	**Creamy Layer**		
	Old	**Young**	**Difference**
OBC	10.674 (0.13)	7.263 (0.16)	-3.412 (0.21)
Others	11.734 (0.06)	7.711 (0.08)	-4.023 (0.10)
Difference	-1.060 (0.14)	-0.448 (0.18)	0.612 (0.22)
Panel B:	**Non-Creamy Layer**		
	Old	**Young**	**Difference**
OBC	4.046 (0.02)	4.415 (0.01)	0.370 (0.02)
Others	6.406 (0.02)	5.523 (0.01)	-0.883 (0.02)
Difference	-2.360 (0.02)	-1.108 (0.02)	1.253 (0.03)
Triple Difference			**0.641** **(0.274)**

Dependent variable is years of education. Standard errors in parentheses. Panel A has 9133 observations and Panel B has 370500 observations. Households with no income or occupational information are excluded. The Triple Difference estimate is the difference between the two double difference estimates: 0.614 years of education. The triple difference estimator has standard errors clustered at the state-level.

the pre-collegiate level. Similarly, Figures A.9 and A.10 show the CDF and the treatment effect at the CDF.

The table and figures indicate that the relative transition of OBCs before and after the policy, has been away from illiteracy (and away from below primary and primary levels of education) and into secondary school and college.[36]

[36]Between 2001 and 2002, the government tried to implement non-caste based policies to universalize elementary education and the Millennium Development Goals, but the effects of these policies are not being captured here since the dataset was collected before the MDG projects were implemented.

6.2 The Intensity of Treatment and State Level Variation

Since different states have, over time, passed different laws reserving state-level seats and jobs for OBCs, this kind of analysis can be done for each state separately. In the graphs in Appendix Figure A.11, I perform an analysis of all the state-law changes, where the vertical line represents a marked change in reservation policy for the OBC group in that state. By restricting the sample to the corresponding state and plotting the coefficients, we can see that the state-wise changes in reservation policy have impacts similar to the federal law change.

This cross-state analysis can also be used to study how the difference-in-difference treatment effect varies by the intensity of reservations, and can address the issues of mean reversion and simultaneous policy changes mentioned above. While variation in social group and age were exploited in the difference-in-differences section, it is possible to investigate another dimension of variation: 'the intensity of the reservation policy.' Since each state has its own reservation policy, there is variation in terms of which states are more pro-reservations and which are less so. Let us define the 'intensity of reservation' as the ratio between the percentage of quotas and the population percentage for each group: $\frac{quotas\%}{population\%}$. For instance, in the state of Karnataka, this ratio is $\frac{53}{36} = 1.47$, whereas in the state of Madhya Pradesh it is only $\frac{13}{40.5} = 0.32$, thus making the intensity higher in Karnataka than in Madhya Pradesh.

In the following regression specification, edu_{ics} is the education level obtained by a person i belonging to caste c and residing in state s. OBC is an indicator for whether the person is OBC or not. Most states made significant changes to reservation policies in the early 1990s.[37] The variable $young$ equals 1 for cohorts that were still in school or will attend school after the changes in reservation policy have been implemented. $\boldsymbol{Z}$ is a vector of controls.[38] The parameter γ captures how the ATET varies with intensity of treatment – if more intensity allows OBCs to catch up faster, then this parameter should be positive.

$$edu_{ics} = \beta_0 young_i + \beta_{1cs} intensity_{cs} + \beta_{2c} OBC_c + \beta_{3c} OBC_c * young_i + \beta_{4cs} intensity_{cs} * young_i + \beta_{5cs} OBC_c * intensity_{cs} + \gamma OBC_c * intensity_{cs} * young_i + \boldsymbol{Z\beta} + \epsilon_{cs} \quad (4)$$

This specification is akin to a continuous form of the triple-difference estimator, where the three dimensions of variation are age, caste and intensity of reservations. As Gruber (1994) explains,

[37] The reason that the law changes from the early 1990s are used (as opposed to previous changes) is because the federal law also changed at that time. The federal law change should not differentially impact residents of different states because people are competing for federal seats with people all over the country. If state-law changes from periods both before and after the 1990s were studied, then they would be confounded by other changes like the federal law change.

[38] I present results with and without controls, where the controls include the intensity of SC and ST reservations, and the interactions with the young indicator and SC ST indicators – a fully saturated model.

such an approach allows us to control for caste-specific trends ($\beta_{0c} OBC_c * young$), and state specific trends in laws ($\beta_{1cs} intensity_{cs} * young$). Controlling for these trends allows us to tackle the issue of simultaneous timings of policy; there is no reason to believe that the timing of the state-wise intensity of reservations should be correlated with timing of investments in schooling infrastructure. Furthermore, this method also allows us to control for state-specific caste preferences $\beta_{2cs} OBC_c * intensity_{cs}$, since certain states may care more about certain castes, and the intensity variable would then be picking up these preferences. Last, this approach also solves the automatic mean-reversion problem, since there is no reason to believe that non-policy driven mean-reversion should be higher in states that have more favorable reservation policies than others.[39]

Table 5: The Intensity of Treatment and State Level Variation

Education Level	Full Sample	Intensity> 0	SC-ST Controls
OBC*Intensity* Young	0.182	0.178	0.168
Standard errors:			
State Level	(0.0985)*	(0.0983)*	(0.0957)*
Region Level	(0.0788)**	(0.0788)**	(0.0780)**
SC-ST Controls	N	N	Y
Observations	508,410	503,981	508,410
R-squared	0.026	0.027	0.087

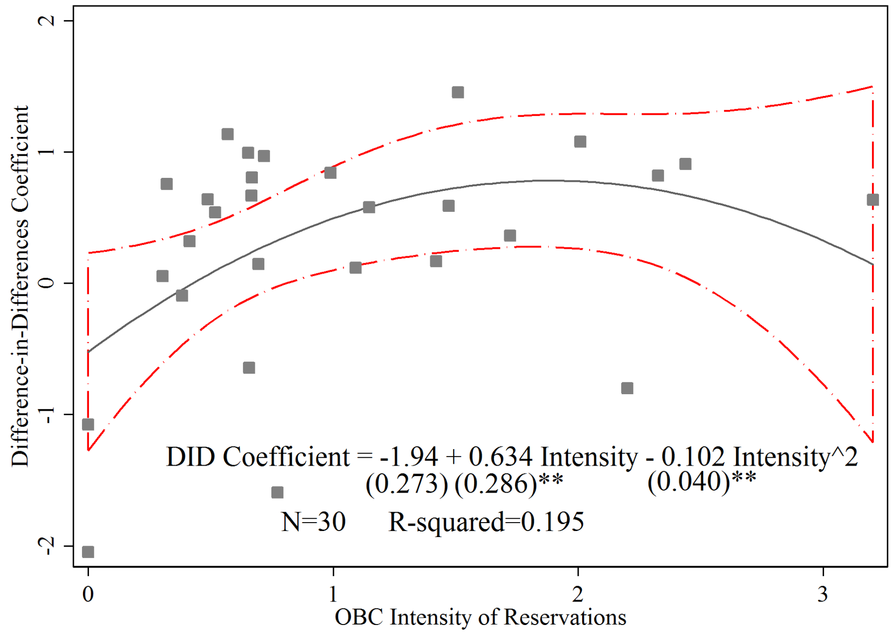
Dependent variable is level of education. Standard errors in parentheses clustered at the state-level (30 states) or NSS-defined region level (77 regions). Specification 'Intensity>0' drops the 2 states that have no reservations for OBCs. Controls in all specifications include an OBC indicator, a young indicator, the state-level intensity of reservations, and all double interactions between these variables. The final column that has SC-ST Controls also includes 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.

In Table 5 we can 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 one unit increases the treatment effect of these policies by between 0.17 and 0.18 levels of education for OBCs. This coefficient is stable across specifications with and without additional controls.

This regression specification, however, imposes a linear functional form. In order to explore non-linearities in the relationship between intensity and the effect of reservations, I use a method also

[39] It may be interesting to see if the 'intensity' variable is correlated with the minority group's situation in society. If greater socio-economic disadvantage in the state is positively correlated with more intensity, then the treatment effect will probably be larger (since there is potentially a larger gap to bridge). If however, more advanced minority groups, can (say via political power) influence greater 'intensity', then the treatment effect will potentially be smaller. I find no evidence of such heterogeneity and no statistically nor economically significant correlation between intensity and the baseline socio-economic characteristics of the minority groups.

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.

used by Donald and Lang (2007). This method also tackles the issue of having a small number of clusters. 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 treatment effect in each state, I do a simple difference-in-differences using only the sub-sample of each state. I then plot the difference-in-differences coefficient across the intensity of reservation by each state in order to find the relationship. This is a meta-analysis of each state's policy. In Figure 2, I plot the relationship and display the auxiliary regression that captures this relationship – which is increasing at a decreasing rate.[40]

6.3 Regression Discontinuity and Difference-in-Discontinuities

For my final empirical strategy, I exploit a state-determined methodology of identifying/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 biggest advantage of the RD estimate in this context, is that it is not encumbered by issues such as mean reversion and simultaneity of government policy. Government spending on school infrastructure should have uniform impacts on castes just below and above the

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

cutoffs determined by the eligibility methodology, and there should be no confounding effects of the government's investment in the schooling program. There is also the benefit of identifying a different and interesting parameter – the effect of such policies on a student from the *marginal* sub-caste.

Classification and identification of OBCs for state-level reservation policies is the prerogative of the state government. States appoint committees to determine who the OBCs are and what reservations they should be eligible for. Some committees conduct a socio-economic survey and collect data. They use this data to rank the different sub-castes on the basis of socio-economic indicators. Castes above a certain cutoff of 'backwardness' are eligible for reservations. This set-up allows us to estimate the impacts of the reservation policy using a regression discontinuity design. If we have information on the index of 'backwardness,' we can compare sub-castes just above to those just below the cutoff to see what the causal impacts of reservations are. The analysis in this section will focus on the state of Haryana, which had one such methodology for classifying the OBCs.

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 then perform a difference-in-discontinuities analysis, using the sub-caste index to identify the discontinuity, and the age cohorts to identify the difference in the discontinuities for each cohort.

In the state of Haryana, an 'index of backwardness' for each sub-caste is published, making it possible for me to conduct a sharp RD. The Singh (1990) Haryana Backward Classes Commission Report was the first ever committee in the state. Being the first is an added bonus, since it prevents any lingering policies from contaminating the before-after analysis.[41] The Committee 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 half-way mark is an intuitive cut-off point and it is thus unlikely that the cut-off 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 survey data where the respondents were probably unaware of the utilization purpose of this data. I observe no bunching of castes just above the cutoff.[42] Manipulation of the methodology from the government's side is also unlikely, since they use the same methodology formulated by the Mandal (1980) Federal Commission. Lastly, I test that the treatment is discontinuous at the cutoff and that all other baseline characteristics vary continuously.[43]

[41] The handful of other states that used similar methodologies had lingering policies; furthermore, the other states do not publish the tables used to formulate the index.

[42] This would not be a valid way of testing manipulability if there were certain groups that wanted to move in opposite directions. But since it is reasonable to believe that the marginal caste wants to be eligible for reservations, in the presence of manipulation we should see bunching just above the cutoff.

[43] In 1995, the Ramji Lal Committee – the second backward classes commission in Haryana – added 4 more castes to the list. In the dataset used, this adds two castes below the cutoff to be eligible for reservations. However, since these castes will have only felt the benefits for less than 3 years (the data was collected in 1999), they have been

Singh (1990) identifies the OBCs by creating an index of 'backwardness' based on (a) social, (b) educational, and (c) economic disadvantage. The social disadvantage criterion looks at 10 indicators, including employment in manual labor, the unorganized sector, and lack of access to proper sanitation and other civic amenities. The educational criterion studies 10 other indicators related to drop-out rates, female literacy, test scores and vocational education. And the economic index looks at 15 indicators such as family assets, consumption expenditure, maternal mortality rates, unemployment rates, etc. 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 can reconstruct the index and it matches the final index produced. While the RD analysis is done for only one state, it confirms a powerful causal finding on affirmative action.

The data set used for the RD analysis is the ARIS-REDS 1999 data set. The nationally representative NSS data cannot be used since it doesn't have disaggregated sub-caste categories, which we require for the RD analysis. Unlike the NSS data set, ARIS-REDS collects information on years of education rather than levels. Therefore, the results in this section analyze the impact of affirmative action policies on the years of education. In the RD results in Figure 3, the dependent variable is the difference in mean years of education between the older and younger members of that caste.[44] Once again 'older' is defined as being too old to enjoy the benefits of this reservation policy. There are 27 sub-castes for which the ARIS-REDS and the Haryana Committee Report have matching caste names.[45]

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 4). We can see a slight downward trend, since a higher index indicates a larger socio-economic disadvantage. Looking at regression Table A.11 in the Appendix, we can see that the cutoff is not statistically nor economically significant for the educational attainment of the older population.

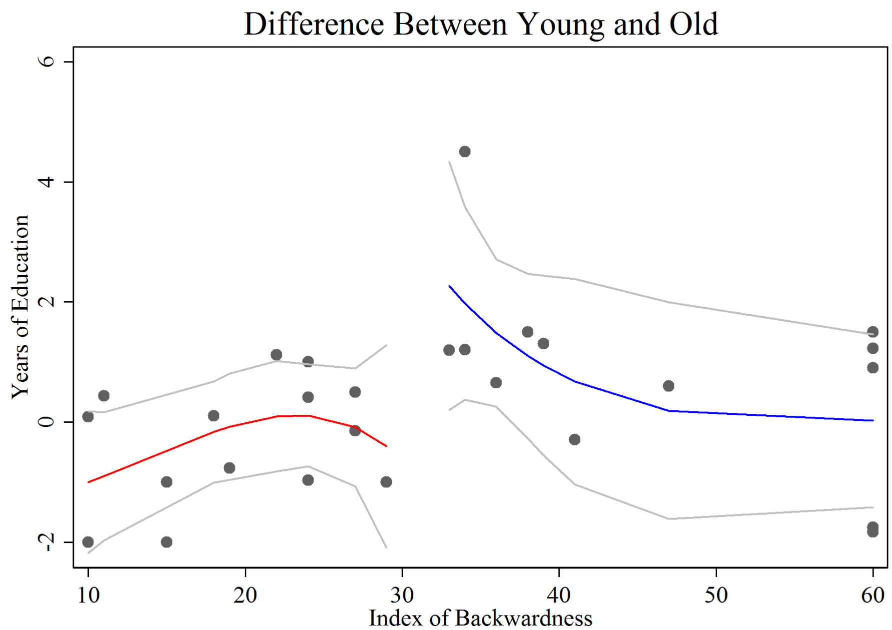
Based on the RD literature, I explore various regression specifications. Imbens and Lemieux (2008) suggest changing the bandwidth and seeing if the results are robust to restricting the sample to a small area just around the cut-off, whereas van der Klaauw (2008) suggests using a semi-parametric approach of local linear regressions with higher order polynomials. I use higher order polynomials of the index value, in the spirit of the Heckman and Robb (1985) 'Control Function Approach,' and sub-samples restricted around the cutoff, in the specifications below. Column headings have the index's degree of polynomial order, and if 'Restricted' is mentioned, then the sample only includes half the index-span around the cutoff (index values of 15 to 44). 'Flex Slope' indicates that the

coded as ineligible. Doing so does not change the results.

[44] Dependent variable = mean education of young in subcaste c − mean education of old in subcaste c.

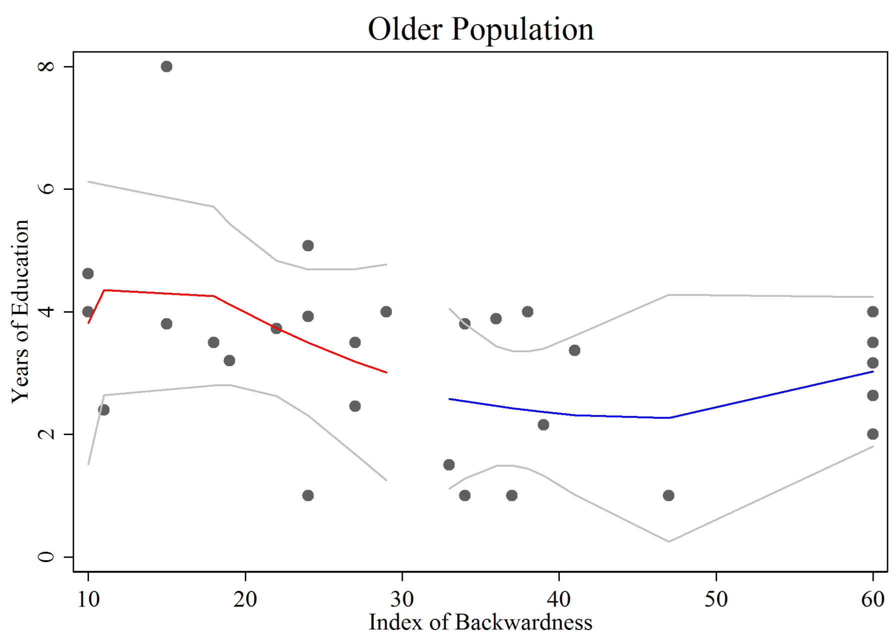
[45] While 34 of the ARIS-REDS castes could be matched to the names in the Committee Report, there was no data on 7 of these castes in the data-set as the data set is much smaller than the NSS. Furthermore, the Scheduled Castes were assigned the highest possible index values as they were already eligible for these policies (and are therefore not castes near the threshold); this shows up in the bunching at the highest possible index value of 60.

Figure 3: RD: Average change in years of education by caste



Regression Discontinuity on d_{ed}, defined as: mean years of education young$_c$ $-$ mean years of education for old$_c$.

Figure 4: RD: Mean Education by Caste of Older Population



Sample restricted to population above the age of 27 (too old to benefit from the reservation policy). Regression Discontinuity on years of education.

slope is allowed to vary on either side of the cutoff.[46]

The RD framework identifies the localized treatment effect in the neighborhood around the cutoff. Students from sub-castes that are close to the cutoff may behave differently from the average student. The coefficient of interest is the Neighborhood Average Treatment Effect (NATE) since it is the average impact of the policies in the neighborhood around the cutoff of eligibility. In this context, there is a large mass of students who get no schooling whatsoever. These students presumably belong to sub-castes further away from the cutoff, having the highest levels of 'backwardness.' This NATE therefore may be larger than the ATET found via the Difference-in-Differences methodology since the ATET is pulled down by students who have the highest costs of schooling. The students who are far from the RD cutoff with high values of 'backwardness,' would presumably only respond to extremely large changes in the returns to education to first budge them on the extensive margin of attending school. Since a large number of high-cost students will not increase their schooling despite these policies, the ATET may be lower than the NATE.

I use three distinct regression specifications. The first – a Discontinuity-in-Differences – is what Figure 3 plots, where the dependent variable is the mean difference in education between the younger and older members within a caste – this regression is at the sub-caste level. The second restricts the sample to only the young, and estimates the discontinuity for the young sample. The third – a Difference-in-Discontinuities – combines a difference-in-differences approach with the RD approach to estimate the differential discontinuity for the young.

In my first, approach – a Discontinuity-in-Differences – I control for a flexible polynomial of the index $f(index)$ and estimate the following regression:

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

In Appendix Table A.6, it is clear that despite having a small number of observations, the coefficient of interest is both economically and statistically significant. Looking at the third order polynomial column, the coefficient shows that the causal effect of reserving seats for backward classes is to increase their high-school education by about 2.6 years. In Table A.7 I perform a specification check by dropping three years above and below the 27-year cutoff, so as to account for impreciseness in the school-leaving age criteria.

In my second approach, I restrict the sample to young cohorts who would be able to change their schooling decision, and control for the mean education level of the older population in that caste. I cluster the standard errors at the caste level, and also show the p-values with a Wild-t small-cluster bootstrapping procedure (Cameron et al., 2008). The added advantage of just performing the regression for the younger population is that we can make sure that the discontinuity isn't

[46]Data-driven bandwidth selection procedures, like the one discussed in Calonico et al. (2014) cannot be used in this context as there are only twenty-seven mass points of the index.

merely arising out of the older populations education, and reconfirm the results in Figures 3 and 4. In some specifications, I restrict the sample to index values around the cutoff. The regression of interest is:

$$edu_{ic} = \beta \ \mathbb{1}_{index>0} + f(index) + \overline{ed\ old}_c + \epsilon_{ic} \quad \text{for } \{-v_1 < index < v_2\} \tag{6}$$

Appendix Tables A.8 through A.10 shows similar coefficients as before across different sample restrictions: I vary the age cutoff that defines 'young', and show results for sub-samples with or without college education. Consistently, the causal impact of reservations is to increase years of high-school education by about 3 years for the average student in the neighborhood of the cutoff.

My preferred specification, however, is a Differences-in-Discontinuities approach, which is relatively new to the literature (Grembi et al., 2016). This incorporates the discontinuity along the caste index, and differences it across the older and younger age cohorts. In Equation (7), I interact the cutoff with the variable $Young$. This interaction term should have a positive sign since the younger group will benefit from the reservation policy. When $Young = 0$, the discontinuity should be close to zero (as seen in Figure 4 and Table A.11), but when $Young = 1$ those above the 'backwardness' cutoff should increase their educational attainment. The model predicts that the coefficient β_1 will be positive:

$$edu_{ic} = \beta_0 \ \mathbb{1}_{index>0} + \beta_1 \ \mathbb{1}_{index>0} * Young_i + \beta_2 \ Young_i + f(index, Young_i) + \epsilon_{ic} \text{ for } index \in \{-v_1, v_1\} \tag{7}$$

Table 6: Difference in Discontinuities

Polynomial	β_1	Clustered SE	CGM pval	Flex Slope	Small BWidth	R-squared
3rd	2.128***	(0.650)	(0.022)			0.104
3rd	2.806**	(1.020)	(0.040)		X	0.103
4th	1.923***	(0.607)	(0.018)			0.105
4th	2.313*	(1.317)	(0.006)		X	0.108
5th	2.890***	(0.929)	(0.00)			0.105
5th	3.722**	(1.623)	(0.054)		X	0.111
1st	2.020***	(0.652)	(0.020)	X		0.106
1st	1.509**	(0.588)	(0.064)	X	X	0.103
2nd	1.031***	(0.152)	(0.00)	X		0.105
2nd	1.797***	(0.378)	(0.00)	X	X	0.113

Subsample of people with no people 3 years before and after school going age at time of survey (27 years in 2000). This excludes 24 to 30 year olds. Dependent variable is years of education. Level of significance (based on clustered standard errors): *** 0.01; ** 0.05; * 0.1. 'Small BWidth' 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 on either side of the cut-off. Standard errors clustered at the caste-level, and Cameron et al. (2008) Wild Bootstrap p-values also presented.

Table 6 shows an increase in education for the OBCs. Once again, the results are economically and statistically significant and of a similar magnitude, giving us a consistent story across all the different specifications. The average value for the coefficients across specifications is about 2.22 years of education. Gelman and Imbens (2014) recommend first or second order polynomials when restricting the sample to smaller bandwidths – among these results, the average change in years of education is 1.58 years. In variants of this specification in the Appendix, I use a larger sample in terms of age cohorts (Table A.12), and drop anybody that attended college (Table A.13). The results are consistent across specifications. This Difference-in-Discontinuities specification is the preferred specification, since it utilizes the entire data set and maximizes power.

I conduct numerous robustness checks to validate these results. It is important to check that other factors are not discontinuous at the same cutoff, since the RD design requires all other factors (income, assets, etc.) to vary smoothly at the cutoff. Looking at Table A.15 in the Appendix, we see that there are no other discontinuities at the same threshold (an index value of 30). The table presents results on total expenditure, medical expenditure and work in casual labor, but robustness checks were done on other variables as well. We can also look for educational discontinuities at any other value of the index. When studying the impact on the average change in education between the younger and older cohorts, no other values of the index have statistically nor economically significant discontinuities. The only value for which the discontinuity is visible is at the true cutoff (a value of 30 on the index). Figure A.12 shows cutoffs at 20, 25, 35 and 40 – none of which have detectable discontinuities.

6.4 Discussion of Empirical Results

So far this paper has used three different approaches to answer the primary question of interest: does affirmative action incentivize students to stay in school? The double-difference strategy uses a nationally representative dataset to identify the average treatment effect on the treated ($ATET_{ca}$) for each age cohort a and caste c. Exploiting variation in caste and age, the estimator finds that the students eligible for reservations increased educational attainment. The concern of confounding policies – like large expenditures on school building – was tackled by looking at placebo groups that should have been affected by school building but not by reservations. There wasn't an impact on educational attainment for the Muslim population and the low-income high-caste population, suggesting that the bulk of the impact on lower-caste members was due to the affirmative action policies. I also compare the differential impact between the excluded creamy layer and included non-creamy-layer populations – the non-creamy-layer population was seen to have more than twice the impact than that of the creamy layer group.

The state-wise analysis exploits variation on a third front: the intensity of reservation policy. The paper finds that the impact is larger in states that have a more generous reservation policy. Using a 2-stage estimation procedure, I show the treatment effect of quotas is increasing, at a decreasing rate, with the intensity of reservations.

The regression discontinuity approach looks at the introduction of OBC reservations in the state of Haryana. OBCs were classified on the basis of an index of 'backwardness,' which provides the running variable for the RD. Across specifications, I consistently find an increase in educational attainment for the population eligible for reservations, and no discontinuities in other dimensions. While the RD is the most 'internally valid' of all the identification strategies used here, it could be less externally valid than the other strategies that use the nationally-representative data since it focuses on the state of Haryana. Haryana is similar to most North Indian states, but may be quite different from some South Indian states. In Appendix Table A.14 the means of the major variables are studied comparing Haryana with the rest of India. The one stark difference is that Haryana has virtually no Scheduled Tribes (STs). The other differences are economically insignificant: the average Haryanvi is richer than the average Indian by about $1.1 a month, is half a year older, and has about one-tenth more levels of education. This differences are small, and not statistically significant, suggesting that it is a representative North Indian state.[47]

The Difference-in-Differences estimator identifies the Treatment on the Treated (ATET) of about 1.38 years of education. Whereas the NATE from the RD strategy is somewhere around 1.58 and 3.1 years of education, the average impact is only about 1.38 years of education.[48] The NATE may be the relevant estimate of interest if the government is considering adding another sub-caste to the list of OBCs; whereas the ATET may be the relevant estimate if the government wants to know the overall impact of changing the amount of quotas for all OBCs. The state-level intensity variation tells us how increasing the intensity of quotas may affect the magnitude of the treatment effect. As theoretical models of signaling predict, a state that reserves a very high fraction of seats for OBCs may end up in a low-level equilibrium.

7 Conclusion

Using three different empirical strategies, I find that policies that raise the returns to education for certain groups encourage students to increase their education by one more schooling-level. An increase of one level of education bridges half the skill gap between OBC and non-OBC populations. This has major implications as it indicates the possibility of encouraging students just below the threshold of a certain level of education to cross that threshold and get to the next highest level. Similarly, Shreshta (2016) finds that the Gurkha community in Nepal attains one more year of

[47]Nonetheless, South Indian states are culturally different and have a history of reservations unlike Haryana.

[48]It is important to remember that the RD and Difference-in-Differences are looking at the impacts of different policies: the difference-in-differences looks at the impact of reservations only in governmental jobs and not colleges.

education, on average, in response to a change in the mandatory employment eligibility law of the British army. However, it is hard to translate the results in most of the other literature that focuses on cost reductions rather than increases in returns. Kazianga et al. (2013) show that enrollment rises by 20% when schools are made more girl-friendly, and Dinkelman and Martinez (2014) show that absenteeism falls by 14% when information is provided about financial aid. Using the booming IT sector near Delhi as a sign of an increase in returns to education, Oster and Steinberg (2013) find that school enrollment rises by 4% to 7% when a new IT center is opened in the area. On the other hand, 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 actually have some positive incentive effects. There is, however, a non-linearity – very high levels of reservations may lead to a 'patronizing equilibrium' (Loury, 1992). Furthermore, a patronizing equilibrium may get strong with time as information updating takes place. While the model may be generalizable to other contexts, like the US, Sri Lanka, Malaysia, the empirical results would probably drastically differ by context. For instance, in the US, affirmative action lacks the backing of certain salient features – like explicitly reserving seats for certain groups – found in the Indian context. Furthermore, interventions in the US are minor in size, compared to a 27% reservation of seats in all government jobs. This lack of salience, and difference in policy-size may lead to different results.

The policies may also come with certain costs if there is a crowding out of educational attainment for upper-caste members. The government and Supreme Court have tried to mitigate this concern by increasing the seats in colleges and government jobs so that the absolute number of seats available to the upper-castes does not change, but it is not clear what the possible costs of increasing seats are. Such a large policy may have other general equilibrium effects in terms of the work-force composition and composition of classrooms, and peer-effects may play a role.

In terms of the benefits for the minority group however, an increase in education can translate into high wage gains since the estimated returns to education in developing countries vary between 7% and 14% (Behrman, 1999; Duflo, 2001; Khanna, 2016; Psacharopoulos and Patrinos, 2004; Strauss and Thomas, 1995). There are also various non-pecuniary benefits of education, like greater participation in the political process and better health. Lastly, lowering educational inequalities – and possibly wealth inequalities – may be something intrinsically valuable to policymakers. In light of these results, therefore, policymakers should keep in mind the externalities of such affirmative action policies.

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A Additional Tables and Figures

Table A.1: Difference-in-Differences Table – Years of Education

OBC vs. Others	Younger Cohort	Older Cohort	Difference
OBC	4.439	4.129	-0.310
	(0.013)	(0.016)	(0.020)
Others	5.579	6.654	1.076
	(0.013)	(0.016)	(0.020)
Difference	-1.140	-2.526	**-1.386**
	(0.018)	(0.022)	**(0.029)**
Hindus vs. Muslims	**Younger Cohort**	**Older Cohort**	**Difference**
Muslim	4.031	3.961	-0.071
	(0.018)	(0.025)	(0.030)
Hindu	4.762	4.952	0.190
	(0.009)	(0.011)	(0.014)
Difference	-0.731	-0.992	**-0.261**
	(0.021)	(0.030)	**(0.036)**
Rich vs. Poorer Others	**Younger Cohort**	**Older Cohort**	**Difference**
Others-Poorest 20%	4.111	5.083	0.972
	(0.025)	(0.037)	(0.044)
Others-Richest 80%	5.989	6.983	0.993
	(0.014)	(0.017)	(0.023)
Difference	-1.879	-1.900	**-0.022**
	(0.030)	(0.041)	**(0.051)**

Using NSS 1999-2000 data. Standard errors in parentheses. Difference-in-Differences value in bold. 'Others' are general category individuals (i.e. not SC, ST or OBCs).

Table A.2: Social Groups and Religions

	Years of Education	Land owned (acres)	Per Cap Month Exp (Rs.)
ST	3.40	1.17	427.32
SC	3.04	0.39	398.67
OBC	3.90	0.99	446.33
Others	5.68	1.10	519.02
Richest 80%	6.07	1.14	578.00
Poorest 20%	4.10	0.96	283.17
Hindu	4.46	1.01	465.57
Muslim	3.61	0.49	424.17

'Others' are general category individuals (not SC, ST or OBCs). Nominal exchange rate: approx Rs. 50 to $ 1. Household Monthly Expenditure deflated by rural-urban-region-wise CPI.

Table A.3: Proportion of Students with Secondary School

Secondary School	**Old**	**Young**	**Difference**
OBC	0.0877	0.0736	-0.0141
	(0.001)	(0.001)	(0.001)
Others	0.1463	0.1041	-0.0422
	(0.001)	(0.001)	(0.001)
Difference	-0.0586	-0.0305	**0.0281**
	(0.001)	(0.001)	**(0.002)**

Standard errors in parentheses. Difference-in-Differences value in bold. 'Others' include that section of the population ineligible for reservations (i.e. not SCs, STs, or OBCs)

Table A.4: Relative Transition of OBCs between grades

	Difference-in-Differences Coefficient	
Level of Education	**Including College**	**Excluding College**
Illiterate	-0.0862	-0.0661
	(0.003)	(0.003)
Below Primary Education	-0.0132	-0.0083
	(0.002)	(0.003)
Primary Education	-0.0149	-0.0065
	(0.002)	(0.002)
Middle School	0.0077	0.0217
	(0.002)	(0.002)
Secondary School	0.0281	0.0455
	(0.002)	(0.002)
Higher Secondary School	0.0047	0.0139
	(0.002)	(0.002)
College Graduate	0.0741	
	(0.002)	

Standard errors in parentheses. Levels of education determined by NSS 1999-2000. Sample of 'excluding college' drops all people with at least some college education.

Table A.5: Creamy Layer v Non Creamy Layer

VARIABLES	Years of Education
OBC	-2.360***
	(0.297)
SC-ST	-3.392***
	(0.274)
Young	-0.883***
	(0.107)
Creamy Layer	5.328***
	(0.155)
OBC*Young	1.253***
	(0.0944)
SC-ST * Young	1.682***
	(0.125)
OBC* Creamy layer	1.301***
	(0.305)
SC-ST * Creamy Layer	1.647***
	(0.364)
Young* Creamy Layer	-3.140***
	(0.170)
OBC*Young* Creamy Layer	**-0.641****
	(0.274)
SC-ST*Young* Creamy Layer	-0.943***
	(0.297)
Constant	6.406***
	(0.150)
Observations	521,063
R-squared	0.093

Dependent variable is years of education.
Standard errors in Parenthesis
Level of significance: *** 0.01; ** 0.05; * 0.1

Table A.6: Discontinuity in Differences (full sample)

Polynomial	3rd	4th	5th	1st	2nd
Cutoff	2.604*	2.342	4.105**	5.158**	18.07
SE	(1.347)	(1.387)	(1.959)	(2.031)	(12.83)
Nonparametric bootstrap p	0.0605	0.198	0.0505	0.0252	0.359
Wild-t p-value	(0.0560)	(0.148)	(0.154)	(0.0280)	(0.196)
Flex Slope				X	X
R-sq	0.400	0.427	0.439	0.381	0.417

Dependent variable is years of education. Standard errors in Parenthesis. Level of significance : *** 0.01; ** 0.05; * 0.1. Nonparameteric bootstrapped p-values, and Wild-t p-values presented. Regressions consist of 27 sub-castes in the state of Haryana. Flex-slope specifications allow the slopes to vary across the cutoff.

Table A.7: Discontinuity in Differences (No 24 to 30 year olds)

Polynomial	3rd	4th	5th	1st	2nd
Cutoff	3.274**	2.908**	3.862*	5.716**	21.25
SE	(1.256)	(1.285)	(1.947)	(2.183)	(12.57)
Nonparametric bootstrap p	0.0203	0.0860	0.533	0.009	0.294
Wild-t p-value	(0.0200)	(0.0540)	(0.140)	(0.0220)	(0.136)
Flex Slope				X	X
R-sq	0.400	0.427	0.439	0.381	0.417

Those above the age of 27 should be unaffected (post-school leaving age at time of implementation). To allow for variation in school-leaving age, this specification drops the sample of persons 3 years above and below this cutoff (24 to 30 year olds). Dependent variable is years of education. Standard errors in Parenthesis. Level of significance : *** 0.01; ** 0.05; * 0.1. Nonparameteric bootstrapped p-values, and Wild-t p-values presented. Regressions consist of 27 sub-castes in the state of Haryana. Flex-slope specifications allow the slopes to vary across the cutoff.

Table A.8: Young Sample (Less than 24 years)

Polynomial	β_1	Clustered SE	CGM pval	Flex Slope	Small BWidth	R-squared
3rd	3.105***	(0.746)	(0.000)			0.108
3rd	3.513**	(1.231)	(0.050)		X	0.120
4th	2.959***	(0.854)	(0.014)			0.109
4th	2.378	(1.490)	(0.126)		X	0.121
5th	3.448***	(1.222)	(0.034)			0.109
5th	3.951**	(1.572)	(0.038)		X	0.122
1st	0.473	(0.650)	(0.580)	X		0.106
1st	5.985*	(3.173)	(0.106)	X	X	0.119
2nd	19.11**	(8.051)	(0.122)	X		0.109

Subsample of people less than 24 years. Dependent variable is years of education. Level of significance (based on clustered standard errors): *** 0.01; ** 0.05; * 0.1. 'Small BWidth' 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 on either side of the cut-off. Standard errors clustered at the caste-level, and Cameron et al. (2008) Wild Bootstrap p-values also presented.

Table A.9: Young Sample (Less than 27 years)

Polynomial	β_1	Clustered SE	CGM pval	Flex Slope	Small BWidth	R-squared
3rd	2.640***	(0.747)	(0.050)			0.108
3rd	2.900**	(1.192)	(0.108)		X	0.120
4th	2.393***	(0.738)	(0.022)			0.109
4th	1.727	(1.261)	(0.266)		X	0.121
5th	3.004***	(1.103)	(0.078)			0.109
5th	2.728*	(1.570)	(0.206)		X	0.122
1st	0.520	(0.593)	(0.434)	X		0.106
1st	6.952***	(2.578)	(0.066)	X	X	0.119
2nd	17.66**	(7.477)	(0.194)	X		0.109

Subsample of people less than 27 years. Dependent variable is years of education. Level of significance (based on clustered standard errors): *** 0.01; ** 0.05; * 0.1. 'Small BWidth' 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 on either side of the cut-off. Standard errors clustered at the caste-level, and Cameron et al. (2008) Wild Bootstrap p-values also presented.

Table A.10: Young Sample (Below 27 and never attended college)

Polynomial	β_1	Clustered SE	CGM pval	Flex Slope	Small BWidth	R-squared
3rd	3.113***	(0.743)	(0.002)			0.108
3rd	3.113***	(0.743)	(0.002)		X	0.120
4th	2.882***	(0.840)	(0.024)			0.109
4th	2.882***	(0.840)	(0.024)		X	0.121
5th	4.290***	(1.129)	(0.004)			0.109
5th	4.290***	(1.129)	(0.004)		X	0.122
1st	1.365***	(0.301)	(0.000)	X		0.106
2nd	3.645***	(1.016)	(0.034)	X		0.109

Subsample of people (below 27) never attended college. Dependent variable is years of education. Level of significance (based on clustered standard errors): *** 0.01; ** 0.05; * 0.1. 'Small BWidth' 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 on either side of the cut-off. Standard errors clustered at the caste-level, and Cameron et al. (2008) Wild Bootstrap p-values also presented.

Table A.11: Education for the Older Population – Not eligible for reservations

Polynomial	Flex Slope	Small BWidth	β_1	Std Err	R-sq
3rd			-0.165	(0.109)	0.189
3rd		X	-0.0615	(0.152)	0.175
4th			-0.112	(0.106)	0.189
4th		X	-0.0361	(0.129)	0.175
5th			-0.153	(0.109)	0.189
5th		X	-0.465*	(0.240)	0.175
1st	X		0.0545	(0.0641)	0.189
1st	X	X	-0.648	(0.427)	0.175
2nd	X		-1.876**	(0.869)	0.189
2nd	X	X	7.224	(5.678)	0.175

Dependent variable is years of education. Sample restricted to older population. Level of significance: *** 0.01; ** 0.05; * 0.1. 'Small BWidth' 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 on either side of the cut-off.

Table A.12: Difference in Discontinuities (Full Sample)

Polynomial	β_1	Clustered SE	CGM pval	Flex Slope	Small BWidth	R-squared
3rd	1.892***	(0.725)	(0.100)			0.104
3rd	1.986*	(1.056)	(0.150)		X	0.103
4th	1.681***	(0.627)	(0.0300)			0.105
4th	1.156	(1.267)	(0.358)		X	0.108
5th	2.307**	(0.999)	(0.0360)			0.105
5th	1.850	(1.892)	(0.470)		X	0.111
1st	1.832**	(0.724)	(0.104)	X		0.106
1st	1.418**	(0.647)	(0.138)	X	X	0.103
2nd	0.988***	(0.211)	(0.000)	X		0.105
2nd	1.603***	(0.467)	(0.004)	X	X	0.113

Dependent variable is years of education. Level of significance (based on clustered standard errors): *** 0.01; ** 0.05; * 0.1. 'Small BWidth' 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 on either side of the cut-off. Standard errors clustered at the caste-level, and Cameron et al. (2008) small-cluster Wild Bootstrap p-values also presented.

Table A.13: Difference in Discontinuities (Never attended college)

Polynomial	β_1	Clustered SE	CGM pval	Flex Slope	Small BWidth	R-squared
3rd	2.424***	(0.623)	(0.00)			0.104
3rd	2.424***	(0.623)	(0.00)		X	0.103
4th	2.194***	(0.572)	(0.01)			0.105
4th	2.194***	(0.572)	(0.01)		X	0.108
5th	3.826***	(0.892)	(0.00)			0.105
5th	3.826***	(0.892)	(0.00)		X	0.111
1st	2.354***	(0.623)	(0.02)	X		0.106
2nd	1.127***	(0.134)	(0.00)	X		0.105

Subsample of people never attended college. Dependent variable is years of education. Level of significance (based on clustered standard errors): *** 0.01; ** 0.05; * 0.1. 'Small BWidth' 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 on either side of the cut-off. Standard errors clustered at the caste-level, and Cameron et al. (2008) Wild Bootstrap p-values also presented.

Table A.14: Comparing Haryana to the Rest of India

	Rest of India	**Haryana**	**Difference**
Sample Size	583422	10155	
Mean Education Level	4.629	4.763	-0.134
Monthly per cap Expenditure (Rs.)	464.612	526.433	-61.821
Male (%)	0.515	0.529	-0.014
Age	26.133	25.551	0.582
Urban (%)	0.376	0.363	0.013
Agricultural sector (%)	0.516	0.503	0.013
OBC (%)	0.33	0.278	0.052
SC (%)	0.158	0.165	-0.007
ST (%)	0.114	0.007	0.107

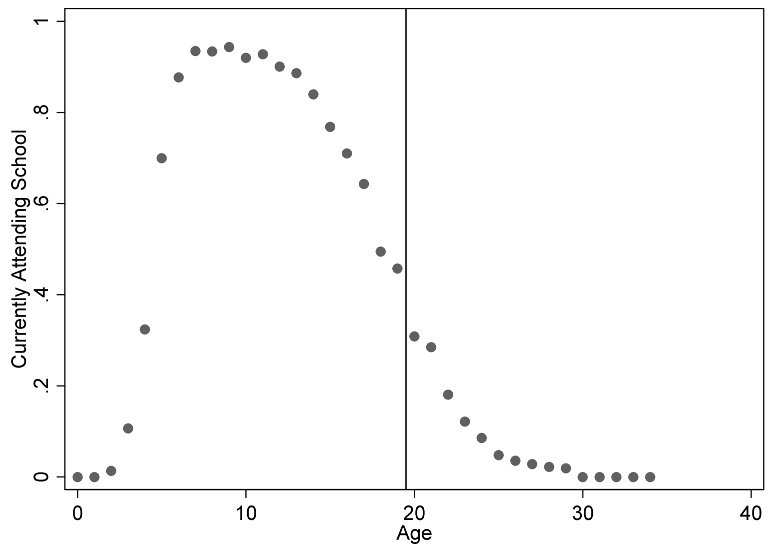
Using NSS 1999-2000 data. 'Mean Education Level' covers 8 levels of education from illiterate to college graduates. Nominal exchange rate: approx Rs. 50 to 1 dollar. Household Monthly Expenditure deflated by rural-urban-region-wise CPI.

Table A.15: RD: Robustness Checks: Other Discontinuities?

Polynomial	Small BWidth	Variable	β_1	Std Err	R-sq
3rd		Edu Expenditure	-0.0587	(0.0502)	0.016
3rd	X	Edu Expenditure	-0.0772	(0.0809)	0.013
4th		Edu Expenditure	-0.0834*	(0.0471)	0.017
4th	X	Edu Expenditure	0.0315	(0.0798)	0.014
5th		Edu Expenditure	0.0230	(0.0588)	0.019
3rd		Med Expenditure	0.0273*	(0.0156)	0.008
3rd	X	Med Expenditure	0.00234	(0.0166)	0.005
4th		Med Expenditure	0.0223	(0.0142)	0.008
4th	X	Med Expenditure	-0.0309*	(0.0172)	0.006
5th		Med Expenditure	0.00726	(0.0187)	0.009
3rd		Total Expenditure	30.73	(30.97)	0.072
3rd	X	Total Expenditure	-29.21	(25.01)	0.094
4th		Total Expenditure	9.403	(20.71)	0.094
4th	X	Total Expenditure	-59.37*	(32.23)	0.097
5th		Total Expenditure	10.06	(35.61)	0.094
3rd		Casual Labor	-0.686*	(0.390)	0.151
3rd	X	Casual Labor	0.289	(0.226)	0.189
4th		Casual Labor	-0.381	(0.259)	0.194
4th	X	Casual Labor	0.253	(0.303)	0.189
5th		Casual Labor	-0.360	(0.372)	0.194

Variable 'Edu Expenditure' is expenditure on education-related goods. 'Med Expenditure' is medical expenditure. 'Total Expenditure' is expenditure on all items. 'Casual Labor' is 1/0 indicator of occupation. Level of significance: *** 0.01; ** 0.05; * 0.1. 'Small BWidth' consists of half the index span around the cutoff (values 15 to 44).

Figure A.1: Enrollment Rates by Age



National Sample Survey 1999. The largest drop in enrollment occurs at the age of 18 - representing a 16% point fall.

Figure A.2: OBC Coefficients of Diff-in-Diff Regression across age cohorts (Controlling for SC-STs)

Unlike the figure in the main text, the omitted group in this picture is only upper-caste individuals. Whereas SC-ST indicators are used as controls. Plot of coefficients from Difference-in-Differences regression on 'levels of education'. Standard errors clustered at state-level. People above school-going age should be unaffected by implementation of policy. Vertical lines indicate school-leaving age at year of implementation of policy.

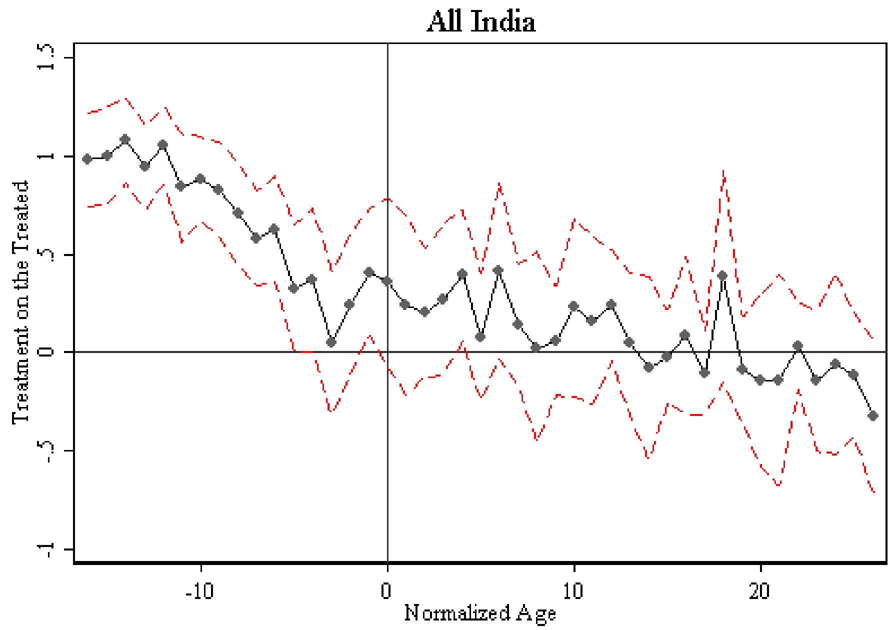
Figure A.3: Five years later - OBC Coefficients of Diff-in-Diff Regression across age cohorts (2005)

Source: 2004-5 NSS data. Standard errors clustered at state-level. Vertical lines indicate school-leaving age at year of implementation.

Figure A.4: OBC Coefficients of Diff-in-Diff Regression across age cohorts (Years of Education)

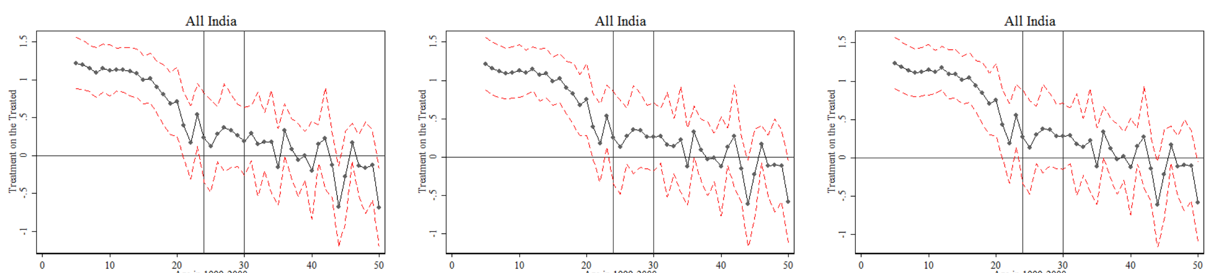
Dependent variable is years of education. Standard errors clustered at state-level. Vertical lines indicate school-leaving age at year of implementation.

Figure A.5: Pre-Collegiate Sub-sample



Standard errors clustered at state-level. Vertical line indicates end of school-going age at year of implementation. Sub-sample of those without a college education in 2000.

Figure A.6: Operation Blackboard and District Primary Education Program (DPEP)



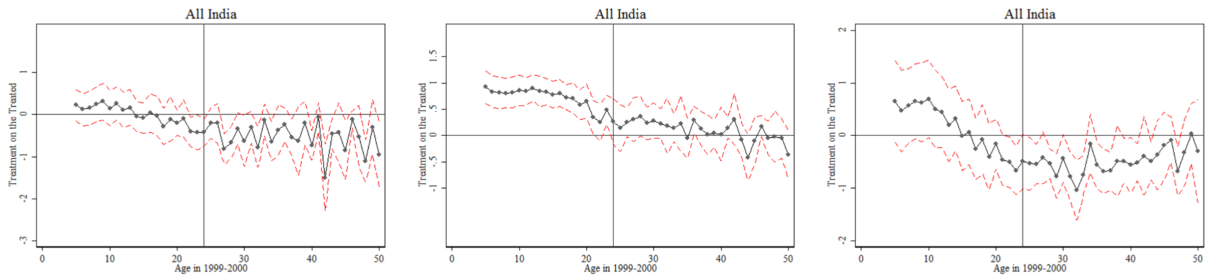
(a) Controlling for Operation Blackboard

(b) No controls

(c) Controlling for DPEP

Years of Education. Standard errors clustered at state-level. Persons above 24 unaffected by Reservations, persons above 30 unaffected by National Policy of Education. 'Others' defined as non-(OBC, SC or ST). Controls include a quadratic in the 'intensity of the policy,' an indicator for being above median intensity, and an indicator for whether the state received any funds under the policy. Intensity of Operation Blackboard is taken from Chin (2005). Intensity of DPEP is defined as the proportion of districts within a state that received funds under DPEP.

Figure A.7: Comparing Impacts on OBCs with Muslims and the Poorest 20% of Others



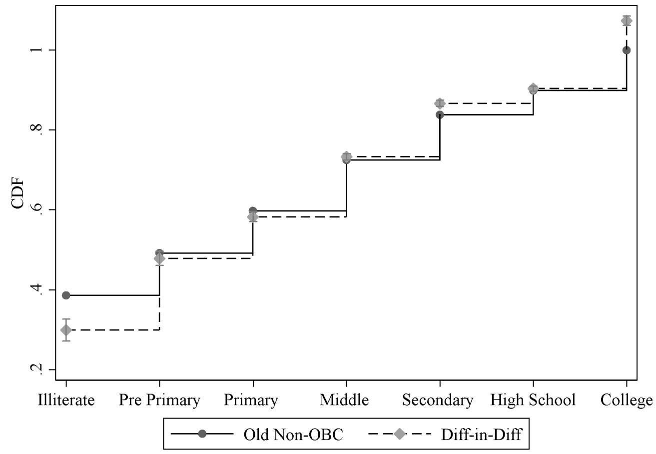
(a) Muslims (b) OBCs (c) Others - poorest 20%

Axes scaled for consistency across graphs. Dependent Variable: Level of Education. Standard errors clustered at state-level. Persons above 24 unaffected by Reservations; Persons above 30 unaffected by National Policy of Education. 'Others' defined as non-(OBC, SC or ST). In the regression with the Muslims, the omitted category is Hindus. In the regression with the poorest 20% of upper-caste members, the omitted category is the richest 80% of the upper-caste population.

Figure A.8: Impacts on OBC: Control Group – only Muslims or Poorest Upper Caste

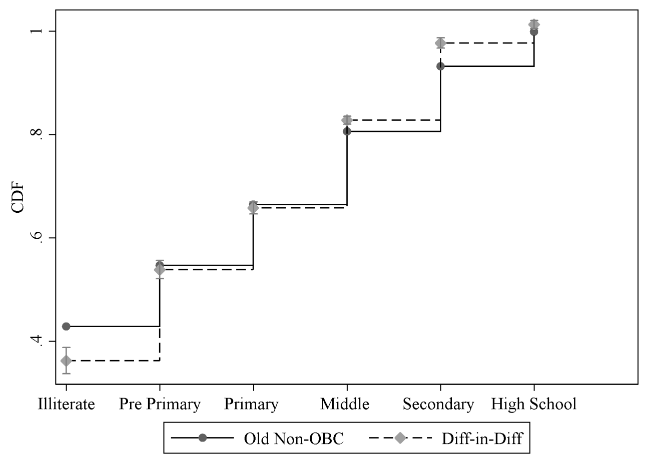
Standard errors clustered at state-level. Vertical line indicates year of implementation. Sub-sample of OBCs, Muslims, and bottom two income deciles of non-OBC/SC/ST population.

Figure A.9: CDF of Education and Treatment Effects



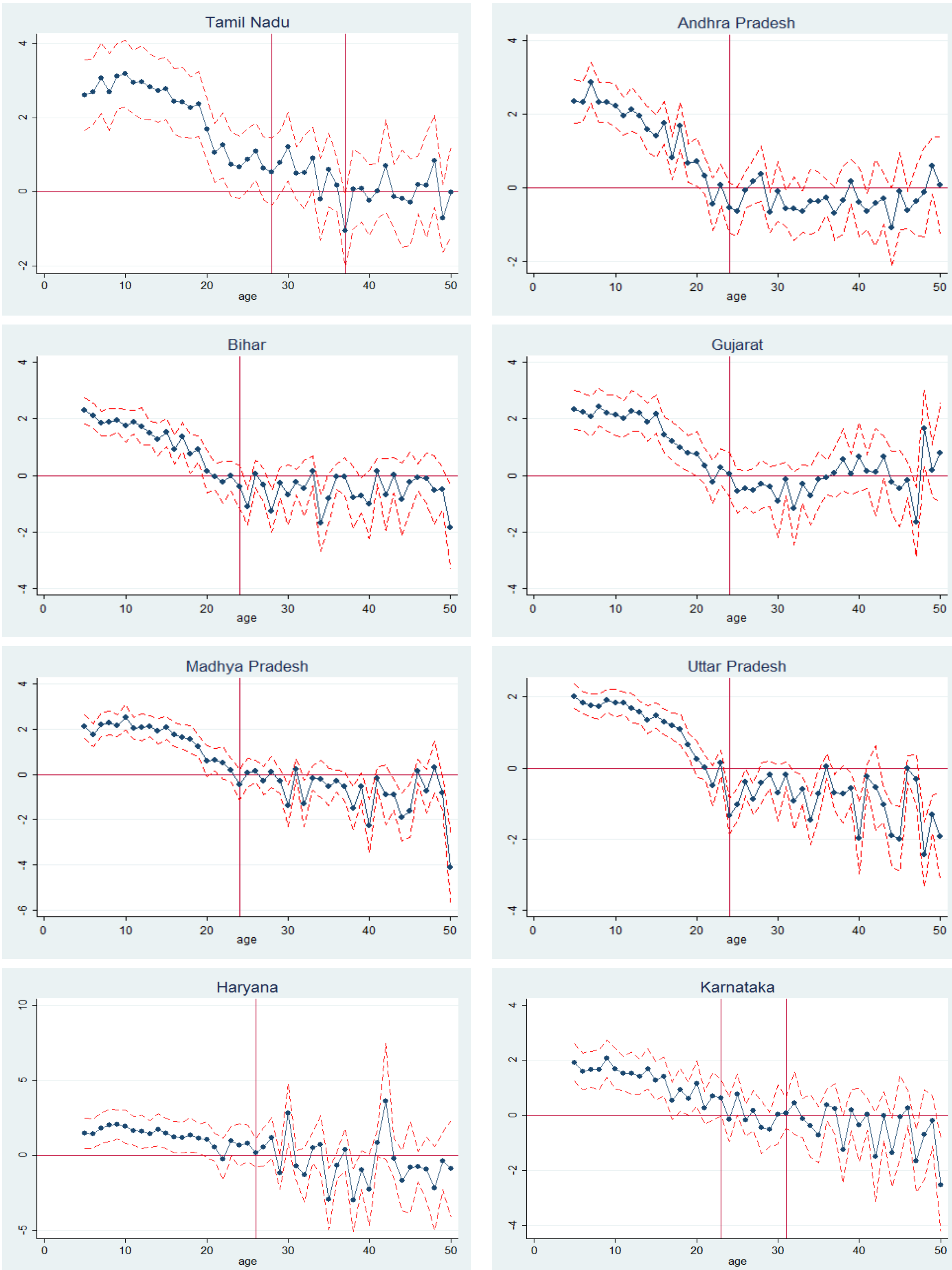
Standard errors clustered at state-level. Cumulative Density Function of education for the older non-OBC group and the difference-in-differences impact at each level.

Figure A.10: CDF of Education and Treatment Effects: Pre-Collegiate Level



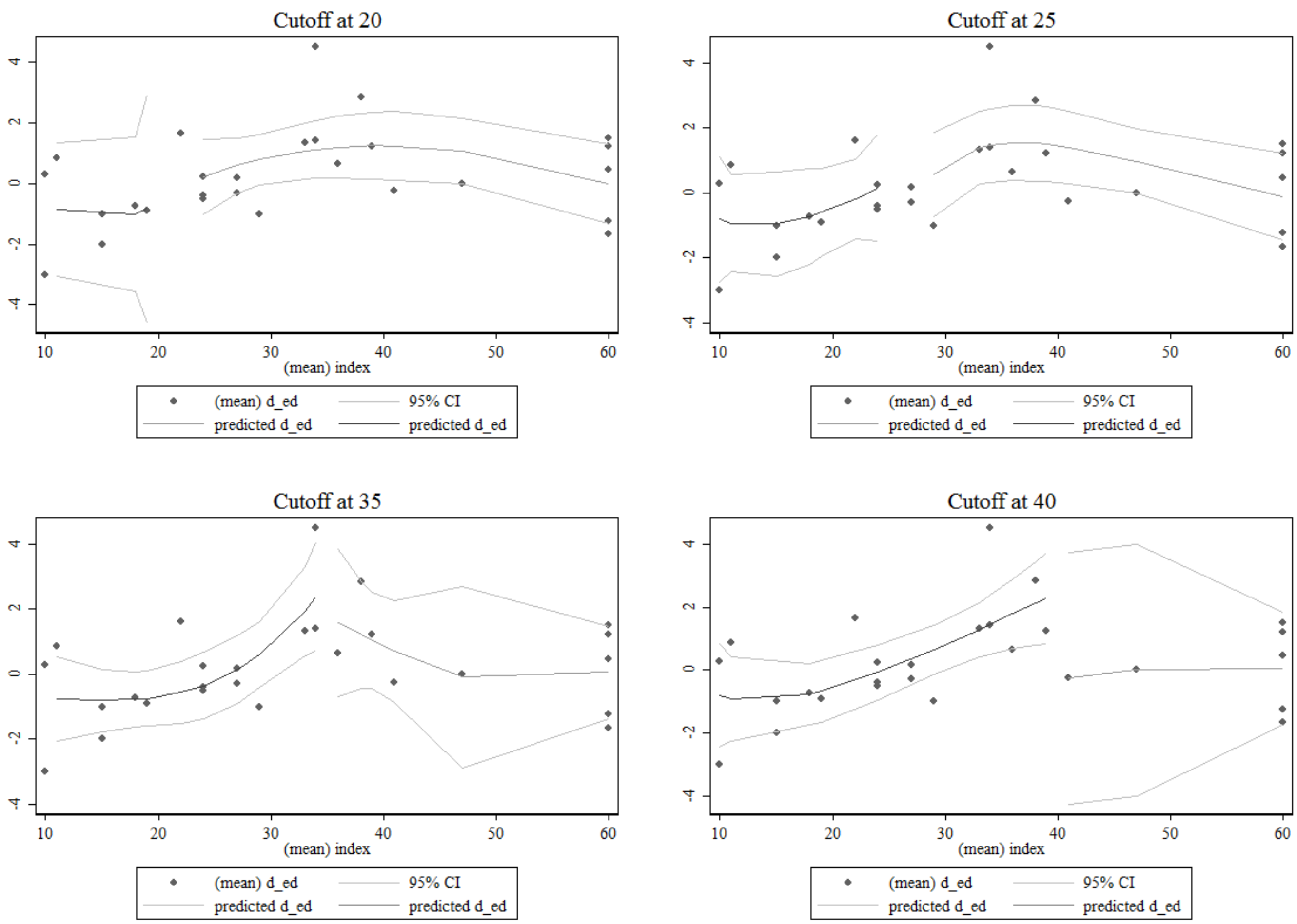
Standard errors clustered at state-level. Cumulative Density Function of education for the older non-OBC group and the difference-in-differences impact at each level.

Figure A.11: State-Wise Changes in Reservation Policy for OBCs



Vertical lines indicate year of implementation of significant changes in state-wise policies. Primary source data on reservation policy changes collected via Right to Information (RTI) Act petitions.

Figure A.12: Looking for other Discontinuities at index values: 20, 25, 35 and 40



Placebo test: looking for discontinuities at other values of the index. True cutoff value is 30.