Does Affirmative Action Work?

Evaluating India's Quota System

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October 15, 2020

Abstract

This paper examines two common critiques of ethnic quota policies in government hiring and education: that they do not benefit the target group, and that any benefits are unevenly distributed within the target group. It focuses on the effects of educational and hiring quotas for Other Backward Class (OBC) castes in India, using difference-in-difference and triple difference designs that take advantage of the gradual introduction of these quotas. The results provide little support for these critiques: affirmative action is associated with small increases in educational attainment and government employment among eligible age cohorts, though the increases in government employment may be a result of other social and political trends. These benefits extend even to poorer OBCs (though not the very poorest), and increase the chances of social contact between uneducated OBCs and government officials.[1]

[1]Replication materials and code can be found at the Harvard Dataverse, https://doi.org/10.7910/DVN/YWKZHJ, Lee 2020

1 Introduction

Many countries have severe economic inequalities between ethnic groups, often the result of deep-seated social discrimination or legacies of previous discrimination (Cederman et al., 2010). One of the most common proposed solutions to such problems is the introduction of quotas or other preferential practices in educational admissions and government hiring processes. For example, in the United States African American and Latino/a applicants are granted preferential treatment in admissions to most universities and in hiring for many government or private sector jobs, while in India a proportion of places at public universities and jobs in public sector institutions are "reserved" for members of specific lower caste groups. The efficacy and justice of these policies have been, and continue to be, fiercely debated (e.g. Sowell, 2005; Kennedy, 2013).

In both the United States and India, critics of affirmative action frequently make empirical claims about the effects of these polices. First, some claim affirmative action does not fulfill its primary goal of raising the socioeconomic status of the targeted group due to limited scope or poor "matching" of recipients to opportunities, leading to high rates of failure and no lasting gain (Sander, 2004). Second, some claim that any benefits of affirmative action are disproportionately concentrated among the socio-economic elite of the targeted group (Massey et al., 2006; Galanter, 1984; Sowell, 2005). If this is correct, quotas might not change the proportion of the disadvantaged group that is disadvantaged, and might even lead to poor applicants from the advantaged group being disfavored relative to wealthier ones from the disadvantaged group.

Despite the importance of the question, the purely empirical literature on educational and hiring preferences is modest in size, particularly relative to the flourishing literature on the effects of electoral quotas (Jensenius, 2017; Chauchard, 2014; Dunning and Nilekani, 2013; Karekurve-Ramachandra and Lee, ming), and the effects of quotas on institutional efficiency (Bhavnani and Lee, 2019; Bertrand et al., 2010). In fact, only the first of these critiques has been the subject of sustained

empirical investigation, much of it challenging the "matching" hypothesis (Howard and Prakash, 2012; Arcidiacono, 2005; Loury and Garman, 1993; Hinrichs, 2012). However, there is virtually no empirical little work on the distribution of benefits within the target group, or effects outside the immediate beneficiaries. The *institutional* effects of quotas are thus somewhat better understood than their *social* effects, despite the importance of the social effects in normative debates on the topic.

This project focuses on a particularly controversial instance of affirmative action: the gradual implementation of hiring quotas for members of the Other Backward Classes (OBC) social category in India. Some Indian caste groups (jatis) are much poorer than others, whether because of different traditional occupations, colonial policies, or continuing discrimination against "low" caste groups. Two categories of groups with very low social status, the Scheduled Castes (SCs) and Scheduled Tribes (STs) have enjoyed quotas ("reservations") in university admissions and state and central government hiring throughout India's post-independence history. Reservations for OBCs (the low status groups not sufficiently disadvantaged to be included in the first two categories) were also instituted in some states during the early and mid 20th century, and in 1994, OBC reservations were instituted both at the national level and at the state level in all states that had not already adopted them. The wisdom and scope of OBC reservations were, and are still, extremely politically important in India (Jaffrelot, 2003; Suryanarayan, 2019).

One reason for the small size of the literature on non-electoral preferences (at least relative to the enormous political salience of the policy) is the difficultly of causally identifying the effects of preferential policies at the group level in the presence of severe selection effects. These policies are not imposed randomly. Groups that benefit from preferences are (almost by definition) different socio-economically before treatment from those that do not. Similarly, these policies were often adopted at times of political and social change, which might have changed the social position of disadvantaged groups even without quotas.

The gradual implementation of quotas in India mitigates some of these selection effects. The

this paper compares OBC and non-OBC Indians who came of age before and after 1994 in the states where local policy changed in that year using is a difference-in-difference (DD) design. A second set of models use a difference-in-difference-in-difference (DDD) design that assesses the effect of state policies, accounting not only for national trends but region and category specific ones, using the states where the policy did not change in 1994 as a comparison set. The "simple" DD and DDD models are supplemented by very conservative models that include a full range of fixed effects for state-birth years, caste-caste categories, and caste category-birth years, while other models add controls. Additional models use alternative measures of reservation status, account for other political changes during the period, use a repeated cross-section as a substitute for the age cohort approach, and formally test for pretrends in the outcomes.

These models show that reservations increased the education level of OBCs without decreasing the achievement levels of other groups, though these effects appear to have been modest in substantive size— an increase of approximately three quarters of a year of education in the immediate aftermath of the policy, growing slightly in subsequent years. The implementation of OBC reservation is also associated with an increase in government employment of between five and seven percentage points relative to the declining trend in government employment among non-OBCs. However, OBCs in states where reservation policies did not change saw similar or even higher relative increases, suggesting that these effects may be a result of the broader changes in the political power of OBCs that occurred in this period.

There is no evidence that these increases benefit only the rich: the effects of the policy are not larger among the tiny group of OBCs with university-educated fathers, nor consistently larger among OBCs whose fathers attended school at any time. The introduction of reservation policy is associated with an increased probability (2-12 percentage points) of a poor individual's social network including a doctor, teacher, or gazetted government officer from their caste. This is a potentially significant result given the importance of connections in Indian life and Indians' relationships with the state. These results may be underestimates, due to the fact that older non-beneficiaries of the policy may

meet quota beneficiaries almost as easily as younger non-beneficiaries.

The results indicate that these specific critiques of affirmative action policies appear to be ill-founded in the Indian context. Among the targeted groups, quota policies appear to achieve their goal of socio-economic uplift, and in fact seem to spread their benefits fairly widely. These increases are not associated with observed increases in "casteist" behavior. The results have a broad relevance for those interested in the extension, preservation and reform of affirmative action policies, and for the political debate on the value of these policies.

2   Predicting the Effects of Affirmative Action

"Affirmative action" (AA) is a policy of preferences for members of a disadvantaged category in opportunity allocation, either in the form of a formal quota or (as in the United States) a less formal set of preferences. These preferences may be implemented in any process in which opportunities are distributed among individuals, though by far the most common are in admissions to higher education and government hiring. Though these policies are theoretically separable, in practice all the most commonly discussed cases of affirmative action (India, the United States, Malaysia, the Soviet Union) implemented both at the same time, making it impossible to disentangle their group-level effects.

Arguably the most important goal of affirmative action policies is to raise the socio-economic status of the targeted group and increase its representation within the social and economic elite. Much of the intended effect is supposed to occur directly: educational opportunities or desirable jobs are given to members of the targeted group. There may also be informal spillovers from the beneficiaries to their friends and family, as money, status and contacts are transferred from the direct recipients to those around them. Finally, there is a third, institutional channel, in which members of the beneficiary group change the policies of the institutions they work for, or use their educational opportunities to change national policy. Due to the nature of the research design, this paper will focus primarily on the first two of these channels. However, there have been several high quality recent

studies of the institutional channel in India, both for political quotas (Jensenius, 2017; Chauchard, 2014; Dunning and Nilekani, 2013; Karekurve-Ramachandra and Lee, ming) and the bureaucracy (Bhavnani and Lee, 2018, 2019), which have found evidence for positive institutional spillovers in specific contexts. Some of these studies have also considered the question of whether these quotas lead to efficiency losses for the institution (Bhavnani and Lee, 2019; Bertrand et al., 2010).

2.1 Socio-Economic Effects

The narrowest test of affirmative action would thus be whether it provides economic and social benefits to the beneficiaries—the students who are admitted or the bureaucrats who are hired. While it may seem may seem obvious that affirmative action would benefit the targeted group, an influential account, using US law school data, has argued for null or negative AA effects on beneficiaries due to poor academic preparedness within these groups (Sander, 2004). For Sander, AA leads to the allocation of opportunities to those who are not prepared to handle them, leading to high rates of failure. A related concern is that a positive AA effect might not be measurable at the group level, particularly if the set of opportunities is small or the sample is small. This gives us a critique of affirmative action:

> **Critique One:** *Affirmative Action programs will not improve the socioeconomic status of beneficiaries and targeted groups in relative terms.*

This critique is, by a sizable margin, the one with the largest empirical literature addressing it. Sander's claim has been strongly challenged by others, using a broad range of data and more sophisticated causal inference strategies, who find positive effects of AA on the beneficiaries (Arcidiacono, 2005; Loury and Garman, 1993; Bertrand et al., 2010; Bagde et al., 2016) and among groups as a whole, driven by a combination of beneficiary effects and anticipatory investment (Howard and Prakash, 2012; Khanna, 2018). Not surprisingly, group-level effects are modest in substantive size, since any positive effect on beneficiaries or potential beneficiaries is averaged across the many members of the group who neither benefit from AA nor changed their behavior in expectation of doing so.

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These results are unsurprising in many ways. Indeed, the avidity with which AA policies are sought by the beneficiaries would lend credence to the view that they confer some sort of benefit. The finding that AA leads to anticipatory investment in education also highlights a theoretical problem with the preparedness critique: AA could increase educational achievement even if the beneficiaries fail at the university level, since the increase might be partly driven by expectations and occurs before beneficiaries enter university.

2.2   Concentrated Benefits

Even if affirmative action does improve the socio-economic status of underprivileged groups, it is not clear that these benefits flow to the most underprivileged members of these groups. Consider the (very common) situation where AA policies establish a quota or preference for members of the underprivileged category, but within the category distribute opportunities based on perceived merit. Frequently, merit (or ability to excel in the procedures designed to measure it) is correlated with socio-economic status. Indeed, this correlation is one reason why quotas are implemented in the first place. If this is the case, open competition on "meritocratic" criteria within the disadvantaged category will lead to a situation where the beneficiaries of affirmative action are disproportionately made up of socio-economically advantaged individuals within the disadvantaged group, or members of relatively advantaged subgroups within the disadvantaged group. This is usually considered undesirable, because if only the children of middle class individuals benefit from programs that help individuals join the middle class, the size of the underprivileged group middle class will increase little over time.

There have been numerous claims that a pattern of concentrated benefits occurs in real world affirmative action programs. Sowell (2005, 167-8) states that "Those individuals most likely to be compensated are often those with the least disadvantages, even when the groups they come from may suffer misfortunes." India's programs, which give benefits to large categories of castes, have been dogged by allegations that they disproportionally benefit richer groups within the categories

(Deshpande and Yadav, 2006). As one anti-reservation protestor complained to the *Hindustan Times*, "[reservations] promote undeserving people who are already rich."[2] These claims have led to the adoption of rules (discussed below) to subdivide caste large categories and exclude the rich as beneficiaries. In the United States, critics of reservations for African Americans have claimed that they tend to benefit relatively educated children of African immigrants, or the children of educated African Americans in general (Massey et al., 2006). However, this claim has never been tested, in part due to the difficulties (discussed below) in determining pretreatment socioeconomic status.[3]

This claim gives us a second critique of affirmative action programs:

> **Critique Two:** *Affirmative Action programs will only improve the socio-economic status of the pretreatment elite of targeted groups.*

Even if all members of the targeted group have an equal chance of receiving the benefit, only a small number will actually do so, given that the number of university seats or desirable government jobs redistributed by AA is usually small relative to the size of the category as a whole. One possible defense of AA's narrow scope is that it might have spillover effects on non-beneficiary members of the targeted category. In recruitment to political office and some bureaucratic positions, the spillovers might be institutional: the new recruits will change policy or practice to benefit coethnics (Dunning and Nilekani, 2013; Jensenius, 2017; Bhavnani and Lee, 2018; Chauchard, 2014).

Another, more broadly applicable spillover effect is the expansion of the social networks of non-beneficiaries. Knowing a government employee might be associated with an increased probability of receiving benefits from the government, either because government servants favor members of their own social network, because members of their own group feel more comfortable approaching them, or because they provide information to their network on the correct way to apply for benefits. Such

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[2] https://www.hindustantimes.com/india-news/lok-sabha-nod-to-10-quota-for-poorer-sections-will-reservatior story-qH9CabDxC88SX4CBNXq7AP.html Accessed 8/5/20.

[3] Cassan (2016) tests a gendered version of the concentration hypothesis in India, and finds that Scheduled Caste quota benefits are concentrated among men.

effects are widely noted in the extensive literature on coethnic favoritism in public goods provision (Ejdemyr et al., 2017; Kramon and Posner, 2016; Lee, 2018) and the existence of such effects has been the basis for arguments for the creation of "representative bureaucracies" (Krislov, 2012). Even if no material benefit exists, personal acquaintance with an empowered member of a disadvantaged group might change the views of non-members about the group (Chauchard, 2014).

The question of whether AA creates these useful social relationships is intimately related to Critiques One. If affirmative action does not increase the number of professionals in the target group, it will obviously not increase the pool of potential contacts. They are, however, a function of the concentration of effects among pretreatment elites: if the effects of affirmative action are concentrated within a section of the target group where most people already know an official, the overall effect of AA on building social ties between disenfranchised groups and the state will be small.

3 Affirmative Action in India

3.1 Historical Background

India contains thousands of endogamous caste groups or *jatis*, most specific to a given region and associated with a traditional occupation. In traditional conceptions of the caste system, castes are sorted in a religiously legitimated hierarchical ordering from high to low, with lower groups considered naturally fitted to a subordinate social and economic role. At the extreme, the lowest status groups were regarded at polluting upper-castes by their touch or shadow. Since the colonial period, such ideas have become less influential, with lower caste elites challenging these hierarchical conceptions of caste and claiming that their groups are equally prestigious (Lee, 2019; Jaffrelot, 2003). While the Indian government has formally banned discrimination based on caste, discrimination and violence against lower caste groups remains extremely common (Shah, 2006). In the mid-20th century, "lower" castes, were underrepresented in both government employment and higher education (Mathur, 2004; Jaffrelot, 2003).

For the purposes of the Indian government, jatis and communities are grouped into four categories: Scheduled Castes (SCs), Scheduled Tribes (STs), Other Backward Classes (OBCs) and General. The SC groups, sometimes know as untouchables or dalits, were traditionally associated with "unclean" occupations such as scavenging and leatherwork, and have traditionally faced high levels of social discrimination. The ST groups, the majority of whom inhabit hilly and forested areas on the margins of the more developed parts of India, have suffered from similar problems. The OBC category is less homogenous, a grab-bag of groups who have faced some level of social discrimination, but but for whom the discrimination is thought to be less severe than that faced by SCs and ST. The category includes many small groups associated with specific traditional occupations (potters, oil pressers, weavers etc.) who historically had lower rates of landownership than other groups. It also includes many Muslim converts, some formerly nomadic tribes and (most controversially) many farming groups with low ritual status but subtantial local political power.

Despite the artificial nature of the caste categories, they are strongly correlated with socioeconomic status. According to the 2011-12 Indian Human Development Survey, among surviving adults born before independence literacy was 21% among SCs, 17% among STs, 37% among OBCs, and 53% in the general category.

3.2 The Adaptation of OBC Reservations

India's post-independence leaders, while overwhelmingly upper-caste, were keenly conscious of both these inequalities and the discrimination that gave rise to them. They also faced competition from lower caste elites who argued that social opportunities should be redistributed through quotas, and who had succeeded in implementing quotas for official hiring and university admissions in several western and southern provinces during the late colonial period, with the earliest example being Mysore in 1918 (Mathur, 2004). These early reservations programs were primarily designed to mitigate the social and educational predominance of the Brahmin caste, and thus most non-Brahmin

Hindu (over 80% of the population) were eligible for some type of benefit.

In the Indian Constitution, SCs and STs were granted quotas proportional to their share of the population in university admissions, government hiring, and political representation at both the local and state levels, which have remained in place since then. The drafters could not find a consensus, however, on whether reservation should be extended to other groups. The compromise solution was to include a non-binding "directive principal" that the government should promote the welfare of an undefined set of "Other Backward Classes." In practice, this meant that states could decide for themselves whether to impose quotas.

The period immediately after independence was one of flux in reservation policy, as changing state borders and law suits from aggrieved upper-caste plaintiffs made the alteration of colonial era policies inevitable. There was, however, a sharp regional trend. By 1964, every state in the south of the country had implemented some form of OBC reservation in higher education and government hiring, and only one northern state (Punjab) had implemented even a small quota. In the north, where caste discrimination remained more entrenched, help for OBCs took the form of modest scholarship schemes, or nothing at all. The other major trend was the narrowing in the set of targeted groups, as the supreme court in *MR Balaji v Mysore* (1962) had held that no more that 50% of places could be allocated by quota (Galanter, 1984).[4]

After the electoral defeat of the Congress Party in 1977, several other states passed reservation laws. The new Janata Party government also established a national commission, chaired by BP Mandal, to consider OBC reservations at the national level in higher education and government hiring and government hiring. The Mandal Commission's report (1980) came out strongly in favor of OBC reservations, but was not implemented by the new Congress government. However, by creating a national list of OBC castes, the report accentuated the trend toward determining OBC

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[4]Tamil Nadu managed to get itself exempted from this rule. At the time of writing, several other states are testing the constitutionality of the 50% limit.

status based solely on caste, rather than class or some interaction between caste and class.[5]

The Mandal report's recommendations were finally made law in 1990 by the next non-Congress government led by VP Singh, though court challenges and administrative difficulties delayed its implementation until 1994. All states that had not done so implemented reservations for OBCs in education and hiring (and some elected local government posts), and several other states took the opportunity to reform existing quota systems. The central government also implemented OBC reservation in its own hiring, though reservation in the small number of central educational institutions had to wait until 2006. The implementation was bitterly contested by upper-caste activists, some of who immolated themselves in public places in protest. The controversy led to a realignment of political loyalties in India, in particular the rise of "pro-Mandal" politicians dedicated to defending reservation policies (Jaffrelot, 2003; Suryanarayan, 2019).

3.3 Reservations in Practice

In India today, all admissions to government universities and hiring to government jobs or jobs in state-owned corporations are subject to quotas. Reservations in private sector hiring and public sector promotion have been proposed but not widely implemented. However, the areas where reservation is used are significant: public universities make up the majority of university seats and are considered the most prestigious, and public sector employment is considered both more secure and desirable than comparable private employment.

Both university admissions and government recruitment in India use competitive written exams. The highest scoring individuals (up to a number equivalent to 50% of the final goal) are admitted or hired as the "general" quota. Officials then continue down the list admitting the highest scoring SCs, STs and OBCs until the quotas for these groups are filled or until there are no applicants who meet a minimal quality threshold. Note that the percentage of members of underprivileged groups

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[5]The constitution is unclear on the role identity should play in determining OBC status. Class-based OBC reservations had been implemented in several states in the 1960s, and have recently been implemented nationally as well.

among the beneficiaries can be above 50% (if many applicants from these groups score above the general cutoff) or less than 50% (if most applicants from these groups score below the qualification cutoff), meaning that consistently scaling the size of the quota across states is difficult.[6]

To qualify for the OBC quota, applicants must provide caste certificates showing that they belong to a group on the central or state lists of OBCs. These certificates are issued by the subdistrict administration, and are based on inquires by government officials in the individual's neighborhood. Individuals must also provide certificates that they do not belong to the so-called "creamy layer" of individuals that are too rich to qualify for reservations. In 2012, the creamy layer included individuals with a family income above 450,000 rupees a year, as well as the children of politicians and senior civil servants. There have been reports of corruption in the provision of both caste and income certificates, the empirical consequences of which will be discussed in Appendix Section H.

In most states, including (in 2012) all of those that introduced OBC reservations in 1994, OBCs are treated as a unit for the purposes of reservation: all members of groups on the state list compete equally for vacancies. Sensitive to the criticism that this provides an advantage to wealthier OBC jatis, some states have subdivided the OBC category into various subcategories, each with their own list and quota. In most states, this has taken the form of small quotas for "Extremely Backward Castes," but some states have more complicated schemes.

Not surprisingly, there is considerable controversy about which groups belong in the OBC category, and a large number of claimants to OBC status. The initial list of OBC groups at the national level was made by the Mandal Commission, while at the state level they were also initially taken from the Mandal Commission list in the castes that added reservation in 1994, and will be the focus of the main analysis. States that implemented reservation before 1994 set up state commissions to classify groups. Both state and national commissions based their decisions on available data and testimony

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[6]For this reason, the main statistical models discussed in this paper use a binary measure of reservation status, rather than attempting to construct a continuous measure of the size of the quota.

about the socio-economic status of the group, but in practice political considerations played at role.

Since 1994, both the state and national governments have added new castes to the OBC lists. While most "late additions" are small groups neglected in the early process, a few are large groups with political pull. These late additions will be excluded from the analysis.

4   Data and Models

4.1   Measuring Social and Political Outcomes

The primary data for this study is the 2011-2012 Indian Human Development Survey, a clustered random survey of social conditions and social attitudes (Desai and Vanneman, 2015). This study was chosen because of its wide range of questions, and the fact that it includes explicit information about any individual's caste. Robustness checks use data from all four rounds of the National Family Health Survey.

All models are weighted to account for variation in sampling probability. After excluding individuals who turned 18 before 1979 or were not 18 at the time of the survey, SCs and STs, and residents of a few small states with few OBCs and no OBC reservation,[7] the sample includes 66,230 individuals. All reported standard errors are clustered at the household level. Substantively identical results with standard errors clustered at the caste level are available on request from the author.

Socioeconomic Status: The most immediate effects of affirmative action in government hiring and education would be to increase the educational attainment and occupational status of the recipients. The primary measure of educational attainment is the *number of years of education* completed by each individual, excluding individuals younger than 23 at the time of the survey. Note that, due to lack of data, we are unable to measure another factor potentially influenced by AA, the relative prestige of different subtypes of educational institutions.

To measure the effects of affirmative action in government recruitment, it uses a binary measure of

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[7]Tripura, Meghalaya, Mizoram and Arunachal Pradesh. Jammu and Kashmir has class-based reservations.

whether an employed individual over 23 and under 60 is *employed by the government*.[8] Job security, benefits and working conditions in these jobs are widely thought to be higher that in the private sector for individuals with comparable qualifications. Public sector employment was once pervasive in India—in 1985, more than 50% of paid non-agricultural employees worked for the government (Chew, 1992, 17)—but since the economic reforms of the 1990s their relative importance has been declining.

Concentrated Effects: If the concentrated benefit hypothesis claim is correct, the benefits of AA should be only apparent among those individuals with a high socio-economic status before the implementation of the policies. This raises a problem, since the cross-sectional nature of the data means that we do not observe pretreatment SES—and in fact, the beneficiaries in the sample were children at the time of the implementation of AA policies. The only measure that captures pretreatment SES is the *father's level of education* which is available for all household heads and individuals living with their parents.[9] Father's education is operationalized as two binary measures: whether one's father attended university (a qualification held by only 4% of fathers), and whether one's father attended any school at all (as 59% of fathers had). Father's education is correlated with caste category: 7.9% of general category respondents' fathers attended university), versus 3% of OBC respondents' fathers.

The necessary condition for any type of contact-driven spillover of AA benefits is that the implementation of AA will increase the proportion of members of the underprivileged group who know an individual from their own group in a position recruited through quotas. To test this claim, the paper examines the self-reported *social contacts* of IHDS respondents. Respondents were asked if anyone in their household was acquainted with individuals holding a wide variety of jobs. I have focused on four occupations which all require higher education of some sort: doctors, teachers, police officers of inspector rank or above and gazetted (middle to senior ranking) civil service officers. The main measure is a binary measure of whether someone in an individual's household knew someone

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[8]Those employed on government relief schemes are dropped, as are non-workers.

[9]Note that since the IHDS took place 17 years after the implementation of AA, virtually none of their fathers would have benefited from AA.

from their caste with any one of these four jobs, something that was true of 42% of respondents. To better capture the idea that AA should increase the social networks of those who would not be eligible to benefit from the policy itself, all these models exclude college educated individuals. Substantively identical results for other socio-economic subsets of the population are available on request.

4.2   Measuring Reservation

To study the effects of India's quota policies, it is first necessary to know who the beneficiaries were. Table B.2, based primarily on Lee (2019), notes the year reservations were implemented and the amount of the quota(s). Periods of reservation of less than two years that were subsequently overturned by the courts are ignored. As Section Three mentioned, about half the states adopted reservation in 1994, while the other half implemented them earlier in the 20th century.

Beneficiary status was defined by birth year. One must be 17 (the usual age of university examination taking) in order to benefit from reservation policies in both education and hiring in India. Since individuals older than 17 would have already be admitted to college before the advent of reservations, they cannot benefit from any change in the quota system after this date, and will find it difficult to benefit from employment reservations (since they will have already made many decisions effects their level of human capital and choice of career).

It is very possible that people who are teenagers at the time of a policy change will not get the full benefit from a change in AA policy. Since taking up these opportunities involves graduating from secondary school, individuals who have already left secondary school will be unable to do so, even if they might have chosen to attend secondary school if they had known the level of opportunities that would be available when they graduated. Put simply, policy changes might have small immediate effects because the pipeline of eligible students from disadvantaged groups is small, and it will take time for this pipeline to adjust to the new policy. This will be particularly true if secondary education and exam preparation are costly investments, as they are in modern India. To account

for this "pipeline" possibility, a second set of tests defines beneficiaries as those who turned 13 (the usual age the year before secondary school entry) in the year of a policy change, rather than 17.

The final problem is determining who is a member of the targeted group. In the Indian context, the simplest way of doing this, used in the main models, is to ask individuals which caste category they belong to. To correct for those jatis added to the OBC category since 1994, individuals whose self-reported jati matched one of these "late addition" groups on the state OBC lists, were only coded as OBC if they turned 18 after their caste was reclassified.

Using self-reported caste categories raises several problems. Firstly, members of the general category may reclassify themselves if doing so will bring them benefits. In this paper, the main models use self-reported category, while Section 5.4 discusses a set of alternative tests using measures of caste category extrapolated from self-reported jati identity. Secondly, since we do not have data on parental income, we cannot accurately determine if these members were part of the "creamy layer." This problem is also discussed in Section 5.4.

4.3   The Difference-in-Difference Estimator

Though OBC reservations have gradually been implemented through the 20th century, for reasons of analytical simplicity the analysis focuses on the largest and most recent of these changes, the extension of OBC reservations at both the state and national levels that occurred in 1994. For this reason, the main models exclude individuals who turned 18 before the last major round of reservation expansion in 1978.

Any test of the effects of the effects of quota implementation in India faces obvious problems of selection. Even at the margin, OBCs are different from members of other groups, since they were either poor, had enough political muscle to be included on the OBC list, or both. Similarly, individuals who came of age between 1979 and 1994 are systematically different from those who came of age between 1994 and 2012, when (among other things) the Indian economy was more

16

prosperous, the political position of OBC groups much stronger, and the variety of social welfare programs much larger. Finally, while the final implementation of OBC reservation at the state level in 1994 was exogenous to the states involved, the states that were forced to implement quotas at that time differed from those that voluntarilyimplemented them earlier.

The standard way of estimating this type of gradually implemented policy shift would be a difference-in-difference (DD) design focusing on the states where the policy changed in 1994 (the "Mandal states"), estimating a treatment effect conditional on time and unit effect. The simplest way to estimate this treatment for individual i of caste category c in year y in state s would be:

$$\text{Outcome}_i = \beta_0 + \beta_1 \text{OBC Caste}_c + \beta_2 \text{Post-1994}_y + \beta_3 \text{Post-1994 OBC Caste}_{cy} + \varepsilon_i \qquad (1)$$

A more conservative (though more difficult to interpret) version of this model, which accounts for variation at the level of the state-caste category and state year, would be:

$$\text{Outcome}_i = \beta_0 + \gamma_{cs} + \delta_{ys} + \beta \text{Post-1994 OBC Caste}_{cy} + \varepsilon_i \qquad (2)$$

Where γ_{cs} is a vector of fixed effects for each state-caste category and δ_{ys} is a vector of fixed effects for each state-year. In Model 1, members of castes are being compared to each other across different states and years, whereas in Model 2 the comparisons are within castes and years.

As we will see, several of the major outcome variables were changing rapidly over time. To account for the possibility of a linear divergence in trends between groups, most of the DD models includes linear time trends and the interaction of this time trend with caste category.[10].

The simple DD model thus accounts for the two types of selection that we are most concerned about in this case: pre-treatment differences between OBCs and others (e.g. the general category having better outcomes than other groups) and differences between the pre or post-treatment periods based on age or birth year (e.g. those who turned 18 before 1994 having lower educational

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[10]The simple linear time trend is perfectly collinear with the state-year FE in Equation Two

attainment, elder people having better jobs, or the introduction of government programs or reforms at the national level). Equation two estimates a treatment effect conditional on an even larger set of factors: Pre-treatment differences between categories (e.g. differences between SCs, STs, and general); differences between the pre or post-treatment periods; disaggregating for particular birth year/ages; pre-treatment regional differences in caste traits (e.g. OBC groups being richer relative to the general category in Rajasthan than in Uttar Pradesh) and time trends at the state level (e.g. Bihar becoming poor relative to Rajasthan during the late 20th century).

4.4   The Difference-in-Difference-in-Difference Estimator

the DD design has a weakenss in this context, even with the full set of fixed effects, since it does not account for (non-linear) time trends at the caste category level. For instance, many scholars have argued that the social conditions of OBCs have been improving relative to other caste categories during the 20th century, (Jaffrelot, 2003). If this is the case, it is possible that any improvement in their social conditions is a result of these trends rather than reservation; in fact, reservation might be a consequence rather than a cause of social change within the OBC category. Similarly, any national effect or policy that helped OBCs more than other groups would lead to an overestimate of the treatment effect.

While, as we will see below, the actual level of *category-specific* pretrends is modest, the theoretical problem is very real. Since the DD model focuses on states where AA policy did not change, it assumes that any post treatment non-linear divergence in outcomes is due to the policy: we do not observe trends among OBCs not effected by the policy change. To address this defect, I report a set of difference-in-difference-in-difference (DDD) models. Intuitively, these models use time trends among OBCs in states where AA policy did not change to separate the effects of the policy change from category-specific trends. The model estimated is:

$$\text{Outcome}_i = \beta_0 + \beta_1 \text{OBC Caste}_c + \beta_2 \text{Post-1994}_y + \beta_3 \text{Mandal State}_s +$$
$$\beta_4 \text{Post-1994 OBC Caste}_{cy} + \beta_5 \text{OBC Caste in Mandal State}_{cs} +$$
$$\beta_6 \text{Mandal State Post-1994}_{ys} + \beta_7 \text{OBC Caste in Mandal State Post-1994}_{cys} + \varepsilon_i \quad (3)$$

Where "Mandal States" are the set of states where OBC quotas were introduced in 1994.[11] Note

that β_7 corresponds to the estimated conditional effect of state-level changes to the quota system,

while the estimated effect of the national changes is subsumed within other category-specific trends

in β_4. Equation 4 includes a more comprehensive set of fixed effects.

$$\text{Outcome}_i = \beta_0 + \gamma_{cs} + \delta_{ys} + \phi_{cy} + \beta \text{OBC Caste in Mandal State Post-1994}_{cys} + \varepsilon_i \quad (4)$$

Where ϕ_{cy} is a set of caste category-year fixed effects.

It is worth emphasizing the conservatism of this model, which accounts caste-category specific

cohort effects (e.g. for STs turning 18 1997, or OBCs turning 18 in 2009) in addition to those

accounted for in the DD models. Note that the caste category-year fixed effects are perfectly collinear

with the linear time trends included in the DD models. In fact, the only possible confounding factors

are trends that are specific to a set of castes in a specific set of states, after accounting for overall

group and regional trends. Below, we will show that such trends do not appear to exist.

One additional conservative element of applying the DDD model in this context should be noted.

In the classic DD or DDD models, outcomes in units where a new policy is adopted (e.g. a higher

minimum wage) are compared to areas where the policy did not change (e.g. where the minimum

wage stayed constant). In the DDD model, we are comparing outcomes in units (caste categories

or caste category-states) where a new policy is adopted to units where the policy had *already* been

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[11]Rajasthan, Uttar Pradesh, Madhya Pradesh, Himachal Pradesh, Assam, Orissa, West Bengal, Sikkim, Manipur
and the union territories. Haryana, where quotas were introduced in 1991, is also included in this category, and
observations from Haryana in 1991-3 are dropped. States where the quota expanded in the early 1990s could also
be recategorized without affecting the results (not-reported).

adopted. If the benefits of the policy do not increase over time in these already treated units (or the increases in benefit have diminished to zero), then the DDD term is an accurate estimate of the treatment effect. However, if the already treated units see an increasing effect of the policy between t_0 and t_1, then this term will be an underestimate of the treatment effect, since some part of the treatment effect will be subsumed into the time trend.

4.5   The Parallel Trends Assumption

For their estimates to be valid, DD and DDD models require that group-specific time trends not exist: the *parallel trends assumption*. For the DD model, the assumption implies that, in the absence of reservation, any change in outcomes among OBCs after 1994 would have been equal to the change among other groups, leaving intergroup differences the same. There is nothing inherently implausible in this assumption, since the policy change began at the national level, and there was little sign that the remaining states would have imposed reservation endogenously without a national shift.[12]

The simplest way to show that this assumption holds is to visually display time trends in important outcomes. Figures A.1 through A.3 show pretrends in the raw data (averaged by year), while the figures in the text show smoothed polynomials. The most obvious features in the Mandal states are the strong upward trend in education over time, the declining trend for government employment and the flat trend for contacts. As we would expect, the gap between OBCs and non-OBCs is also perceptible for all the variables, reflecting the lower social status of OBCs in the Mandal states. However, before 1994 the gap between OBCs and non-OBCs for the various variables remains fairly constant, seeming to validate the parallel trends assumption: there is no evidence of pretreatment convergence in any outcome in the full sample.

A more formal test of the parallel trend assumption is to use leads of the treatment as a placebo. Since the distribution of OBC reservations did not change for those who turned 18 in 1996 or 1990, we should not expect positive treatment effects in those years. Tables E.1 to E.6 show the results

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[12]See Jaffrelot (2003) for a discussion of the stalemate of reservation policy in the 1980s.

for four and eight year leads of all the outcomes, using both the DD and DDD models. None of these models show positive and statistically significant "treatment" effects, supporting the idea that there were no noticeable differences in trends between OBCs and non-OBCs before 1994.

5   Results

Section Four discussed four models of varying levels of complexity and conservatism to examine the effect of the imposition of reservation on OBCs in the Mandal states. However, since full exposition of these models would be both time consuming and repetitive, this section will only report the marginal effects of the simple difference-in-difference model in the Mandal state sample (Equations One and Two). The DDD results are reported in Section C of the online appendix.

5.1   Socio-Economic Effects

Does the introduction of quotas for OBCs in education increase their educational attainment? Table 1 shows the marginal estimates for a linear regression models with individual years of education as the dependent variable. In the simple DD model, individuals who turned 17 after 1994 in the Mandal states are considerably better educated than those who do not, while OBCs are less educated that the average of General respondents (by 2.5 years). Conditional on these two effects, the estimated effect of being an OBC who turned 17 after 1994 is positive and statistically significant, though small in substantive size: .7 years of Education. The coefficient remains similar in size and statistically significant once caste-state and state year fixed effects are added. Similarly, the estimated effect increases slightly when the alternative treatment threshold age (those who were starting secondary school in 1994) is used (Models 3 and 4). The DDD models, though more complex to interpret, produce similar results, though they are only statistically significant for the 13 year old time threshold (Table C.1). Figures 1a and 1b show these patterns graphically.

Panel B shows the results of a series of linear regression models with government employment as the dependent variable. The results are substantively similar to those in Panel A. In the DD model, the in-

Table 1: Quotas and Socioeconomic Status

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Panel A: Years of Education				
OBC Caste	-2.527***		-2.206***	
	(0.236)		(0.341)	
Post 1994	-0.375		-0.618**	
	(0.236)		(0.311)	
OBC*Post 94	0.686*	0.704*	1.140**	1.118***
	(0.379)	(0.369)	(0.446)	(0.420)
Time Trend	0.132***		0.153***	
	(0.0131)		(0.0185)	
OBC*Time Trend	0.00433	-0.00328	-0.0199	-0.0269
	(0.0215)	(0.0209)	(0.0272)	(0.0254)
Observations	35,021	35,021	32,477	32,477
R-squared	0.094	0.164	0.090	0.159
Panel B: Government Job				
OBC Caste	-0.0792***		-0.0658***	
	(0.0182)		(0.0202)	
Post 1994	-0.0433**		-0.0339	
	(0.0220)		(0.0232)	
OBC*Post 94	0.0693***	0.0669**	0.0704***	0.0554**
	(0.0222)	(0.0261)	(0.0221)	(0.0241)
Time Trend	-0.00472***		-0.00415**	
	(0.00145)	(0.00173)		
OBC*Time Trend	-0.00265**	-0.00380**	-0.00270*	-0.00421**
	(0.00132)	(0.00173)	(0.00140)	(0.00174)
Observations	8,784	8,784	7,915	7,915
R-squared	0.040	0.170	0.032	0.149
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The tables show the estimates from linear regression models with individual years of education and an indicator for government employment as the dependent variables. "Post 94" refers to the year respondents turned 17 or 13, as noted in the column headings. All models are weighted for sampling probability and have standard errors clustered by household. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

troduction of OBC reservations is associated with an increase in the probability of government employment for OBCs who turn 18 after the adoption of reservations. The increase in the conditional probability of a government job in the treatment group is 7 percentage points in the DD model, a quite large substantive effect compared to the full sample average of 13%. The contrast between the small increase in government employment among OBCs and the gradual decline among non-OBCs is apparent in Figure

1c. OBCs were being welcomed into the public sector right when the public sector ceased to have the central role in the economy it previously had. The results echo the conclusions from a visual inspection of Figure 1. While there has been a trend towards convergence of OBC and non-OBC outcomes in the Mandal states, this trend only became more marked for those who came of age some years after 1994.

The effects of government employment in the DDD models (Table C.2) are quite different, being negative in sign (though not generally statistically significant). The reason for this difference is evident when we compare Figures 1c and 1d. In the Mandal states, government employment was declining in parallel before 1994, and converging afterwards. However, in the non-Mandal states, the pretrends were not parallel; general category government employment was declining much more rapidly than among OBCs before 1994, and the trend continued afterwards, with OBCs in the youngest age cohorts actually having higher levels of the state employment than members of the general category. The relative improvement of OBC position in states that already had quotas indicate that it is possible that OBC state employment might have improved for political reasons even without the introduction of quotas, though there is no sign of this in the Mandal state pretrends. It is also possible that the continuing effects of quotas in the non-Mandal states was larger than the initial effects of quotas in the Mandal state, leading to the null or negative estimates (see Section 4.4.)

One final interesting aspect of these models is that there is no evidence for the effects of AA being zero sum, with gains for OBCs counterbalanced by losses for other groups. Non-OBC achievement continues its linear decline (government employment) or its linear increase (education) indicating that any losses by Non-OBCs are relative rather than absolute.

5.2 Critique Two: Concentrated Benefits

Are these benefits concentrated in any one segment of the population? Table 2 reruns the models in Table 1, but interacts the key independent variables with an additional binary variable for whether on not the respondent's father attended school (Panel A) or university (Panel B). Results for

Figure 1: Caste SES by State and Caste Category in India 1979-2012

(a) Years of Education: Mandal States
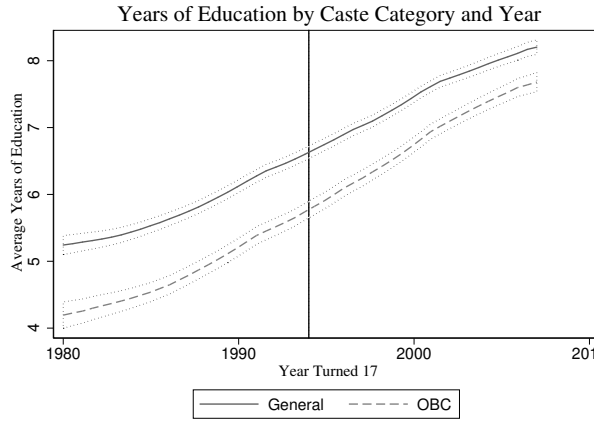
(b) Years of Education Non-Mandal States
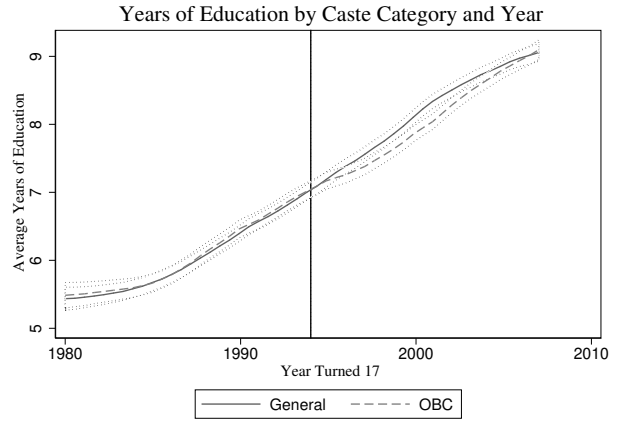
(c) Government Job: Mandal States
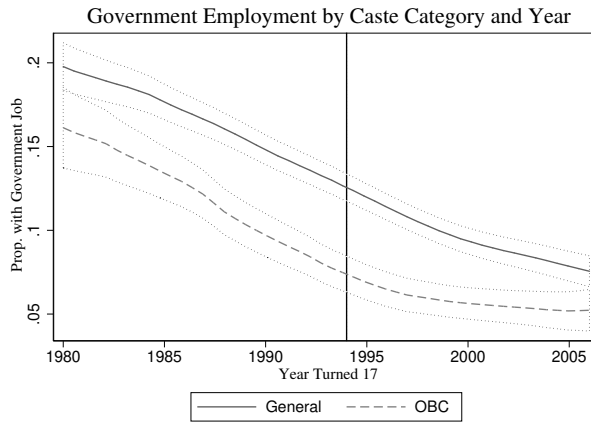
(d) Government Job: Non-Mandal States
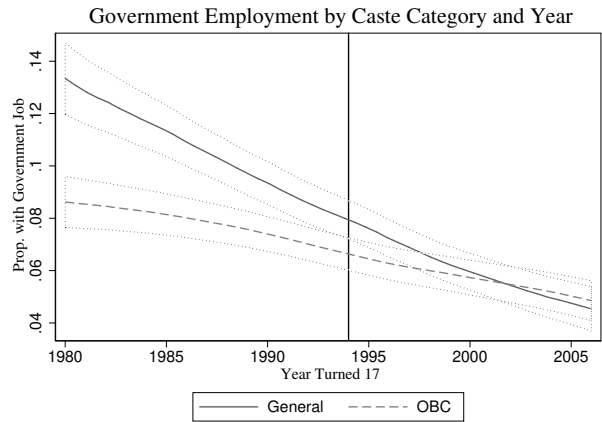
Note: The Y axis shows the proportion of employed individuals of a particular caste category who turned 18 in that year with a middle class job, and their average years of education. The lines represent caste category specific linear polynomial estimates. The solid line corresponds to 1994, the dotted line corresponds to 1998 (the year when those who were 14 in 1994 turned 18). Figures a and c cover states when OBC reservation was introduced in 1994, Figures b and d cover states when OBC reservation was in place between 1979 and 1994.

government employment are reported in Table C.4.

The results differ depending on which definition of an "educated father" is used. In the DDD models, there is some evidence that OBCs with fathers who received some education see their education increase faster in the post-1994 period than one would expect based on the experiences of non-OBCs with educated fathers or OBCs with uneducated fathers, though this result is only inconsistently close to conventional levels of statistical significance. Education levels increased by

24

approximately 2.5 years among OBCs with educated fathers after the introduction of reservation, compared to approximately 2 years among other OBCs. In the general category, education increased in both groups by virtually the same amount (1.8 years).

There is even less evidence for concentration in Table 2, which defines educated fathers as those with some university education. In all models, the post-1994 gains in education are larger among those whose fathers did not attend university than those whose fathers did, though this difference is not usually statistically significant. While individuals from slightly educated families may gain more access to education after the implementation of reservation than those from uneducated families, those from highly educated families appear to benefit less than others.

Note that levels of educational attainment are still very high among the children of college educated OBCs, much higher than among other OBCs. This may explain the disjunction between these results and the folk consensus that most educated OBCs come from advantaged backgrounds. While most education among OBCs is concentrated among the already educated, the *increase* in education associated with quota policies is not. It is also possible that educated OBCs benefit disproportionately by attending more prestigious institutions than they would otherwise (Bertrand et al., 2010; Bagde et al., 2016), a hypothesis that cannot be tested here.

Another explanation for the lack of divergent effects is equally probable, and speaks to the limitations of the data. Given the historical poverty of most of the Mandal states, the proportion of OBCs with university educated fathers in 1994 was very small (2.2%) and many of these probably held degrees that brought little prestige in a national context. Given this fact, it is possible that there were simply not enough "elite" OBCs in the Mandal states to monopolize the benefits of reservation.

This relative lack of concentration might also be a product of the fact that many states have introduced complex systems of subquotas, and "creamy layer" carveouts designed to eliminate precisely this type of concentration, especially separate quotas for "extremely backward castes." However, this is not the case: most states with subquotas implemented reservations before 1994 (and are hence not

Table 2: Quotas, Education and Paternal Education

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Panel A: Fathers with Any Education				
Post 94	1.027***		0.930***	
	(0.192)		(0.199)	
Ed. Father	4.616***		4.586***	
	(0.197)		(0.175)	
Post 94*Ed. Father	0.408		0.558**	
	(0.249)		(0.252)	
OBC	-0.892***		-0.887***	
	(0.179)		(0.165)	
Post 94*OBC	0.459*	0.308	0.676***	0.637**
	(0.247)	(0.293)	(0.261)	(0.307)
Ed. Father*OBC	-1.392***		-1.357***	
	(0.275)		(0.240)	
Post 94*Ed. Father*OBC	0.391	0.763*	0.208	0.421
	(0.344)	(0.390)	(0.338)	(0.385)
Observations	27,044	27,044	27,044	27,044
R-squared	0.247	0.326	0.252	0.326
Panel B: University Educated Fathers				
Post 94	1.890***		1.945***	
	(0.143)		(0.144)	
Ed. Father	6.803***		6.691***	
	(0.275)		(0.236)	
Post 94*Ed. Father	-1.683***		-1.830***	
	(0.308)		(0.284)	
OBC	-1.980***		-1.987***	
	(0.150)		(0.134)	
Post 94*OBC	0.648***	0.587***	0.829***	0.860***
	(0.185)	(0.201)	(0.184)	(0.192)
Ed. Father*OBC	1.072*		1.162**	
	(0.593)		(0.514)	
Post 94*Ed. Father*OBC	-0.883	-0.343	-1.199**	-0.939
	(0.643)	(0.662)	(0.591)	(0.592)
Observations	27,044	27,044	27,044	27,044
R-squared	0.141	0.238	0.141	0.239
State-Caste Category*Ed Father FE	NO	YES	NO	YES
State-Birth Year*Ed Father FE	NO	YES	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The tables show the estimates from linear regression models with individual years of education as the dependent variable. "Post 94" refers to the year respondents turned 17 or 13, as noted in the column headings. All models are weighted for sampling probability and have standard errors clustered by household. Educated fathers are those with more than a secondary education (Panel A), or any education (Panel B). $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

in the sample), and the exclusion of the few state-years with these policies does not effect the results

(available on request). Section 5.4 shows that the results are similar once the creamy layer is excluded.

Do non-beneficiaries at least gain social contact with some of the beneficiaries? Table 3 reports the marginal effects of a model with a measure of contact with teachers, doctors, police inspectors and gazetted civil servants among non-university graduates. The results fit with the idea that at least social networks are being enriched in relative terms, if not absolute ones: The estimated conditional effect of reservation on social contacts is positive. These results are much stronger, and statistically significant, using the alternative (age 13) treatment cutoff. Conditional on time trends, the gap between OBCs and others in contact possession was reduced from 13.2 percentage points among those who turned 13 before 1994 to 1.3 percentage points among those who turned 13 afterwards.

Table 3: Quotas and Social Contacts

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-0.144***		-0.132***	
	(0.0304)		(0.0418)	
Post 1994	-0.0262		-0.0729**	
	(0.0361)		(0.0362)	
OBC*Post 94	0.0276	0.0196	0.117**	0.119**
	(0.0580)	(0.0596)	(0.0563)	(0.0596)
Time Trend	-0.000451		0.00132	
	(0.00190)		(0.00197)	
OBC*Time Trend	0.000619	0.00107	-0.00252	-0.00197
	(0.00326)	(0.00331)	(0.00344)	(0.00354)
Observations	30,966	30,966	28,695	28,695
R-squared	0.014	0.062	0.015	0.061
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Notes: The table shows the estimates from linear regression models with an indicator for whether the respondent lives in a household where a member knows a doctor, teacher police inspector or gazetted government officer as the dependent variable. "Post 94" refers to the year respondents turned 17 or 13, as noted in the column headings. All models are weighted for sampling probability and have standard errors clustered by household.

Figure 2a shows that while post-treatment convergence in contacts between caste categories is a very marked trend, it is centered in the Mandal states and only becomes noticeable around 1994. While trends were roughly parallel (and, like government employment overall, declining, before 1994 and in the non-Mandal states, after 1994 in the Mandal the level of contacts among OBCs rose even

Figure 2: Social Contacts by Caste Category in India 1979-2012
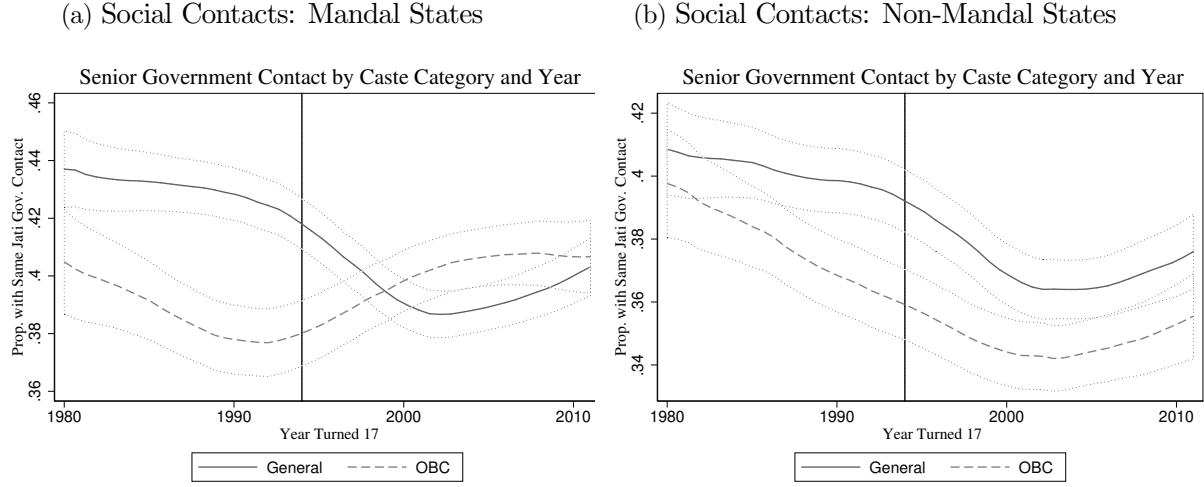
(a) Social Contacts: Mandal States
(b) Social Contacts: Non-Mandal States

Note: The Y axis shows the proportion of individuals of a particular caste category who turned 18 in that year who live in a household where a member knows a doctor, teacher police inspector or gazetted government officer . The lines represent caste category specific linear polynomial estimates. The solid line corresponds to 1994, the dotted line corresponds to 1998. Figure b plots the results of the effect of OBC status by year from a set of bivariant regression models within Mandal states, with middle class job and years of education as the dependent variables, and SEs weighted for sampling probability and have standard errors clustered by household. Figure a covers states when OBC reservation was introduced in 1994, Figure b covers states when OBC reservation was in place between 1979 and 1994.

as that of non-OBCs continued to decline, a trend much more marked than the trends in goverment employment itself shown in Figure 1c.

Note that these results may represent underestimates of the effects of reservations on contacts. Since individuals can gain contacts at any time (much more easily than they can change jobs or go back to school) individuals who came of age before 1994 may know individuals who were educated under AA afterwards.

5.3   Repeated Cross-Sectional Survey Analysis

There are several potential problems with the use of age cohort membership as an index of exposure of quotas. Individuals might find some way to take advantage of benefits (particularly in hiring) even though they turned 18 before their introduction. Differential death or migration rates across groups might bias estimates, as might retrospective reclassification of group membership. The obvious way to address these concerns would be to use a panel or a repeated cross-section, that

directly measures outcomes in the treated and untreated group before and after treatment.

There are several obstacles, however, to implementing this strategy in the Indian context. The most direct is that before the national implementation of the Mandal report, OBC status was not socially salient enough to be included on survey questionnaires: The National Sample Survey did not measure OBCs until the 55th round (1999-2000) and the National Family Health Survey did not include it until the 2nd round (1998-9). However, for all NFHS rounds (unlike the NSS) data on the raw responses to the jati question is available after an application process. I was able to obtain this data, manually standardized the spellings, and then matched the jatis to the state OBC lists.[13] Given the highly variable terminology Indians use to describe their castes, not to mention the many transliteration and transcription difficulties in a large national survey, this method excludes many OBCs.

The first round of the NFHS only measured individual education among our outcomes.[14] Table 4 reports the results of a series of DD and DDD models with years of educations as the dependent variable. Quota introduction has a positive and statistically significant relationship with with educational attainment. The magnitude of the estimates (between .43 and .73 years) is substantial when we consider that these effects are averages over the entire population between 18 and 40.

5.4 Movement between Categories and the Creamy Layer

The main analysis defined the treatment group (OBCs) as those who self-defined as OBCs in 2012, less those who self-identified as members of jatis that were added to the OBC category after the respondent turned 17. However, individuals may move between categories, particularly if they have an incentive to do so (Rao and Ban, 2007), and affirmative action can provide just such an incentive (Francis et al., 2012). This might be particularly true because caste categories, unlike jatis, are not primary social identities. Individuals might either be unaware that their jati is classified as an OBC, or may decide to classify themselves as members of a category that they feel will bring benefits.

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[13]This is the same method used for the DHS data in Appendix Section G.

[14] Over time trends are show graphically in Figure A.4.

Table 4: Quotas and Education: NFHS Data

VARIABLES	(1) Edu.	(2) Edu.	(3) Edu.	(4) Edu.
OBC Caste	-1.542***		-0.832***	
	(0.0899)		(0.0844)	
Post 1994	2.426***		2.751***	
	(0.0432)		(0.0463)	
Mandal State			-0.448***	
			(0.0586)	
OBC Caste*Mandal State			-0.710***	
			(0.123)	
OBC*Post 94	0.727***	0.835***	0.0896	
	(0.0960)	(0.103)	(0.0929)	
Post 94*Mandal State			-0.325***	
			(0.0633)	
OBC*Post 94*Mandal State			0.637***	0.431***
			(0.134)	(0.144)
Observations	540,343	540,343	893,896	893,896
R-squared	0.029	0.073	0.037	0.109
State-Caste Category FE	NO	YES	NO	YES
Year-State FE	NO	YES	NO	YES
Caste Category-Year FE	NO	NO	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The tables show the estimates from linear regression models with individual years of education as the dependent variable. "Post 94" refers to the year of the survey, as noted in the column headings. The sample excluded STs, SCs, and those under 18 or over 40. Observations are weighted based on sampling probability, and standard errors clustered by household. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

To test whether the results are a product of this type of self-reclassification, Appendix Section G shows the results of a set of models in which caste category is catagorized based on self-reported jati. Individuals are coded as being members of a caste category if they are members members of a jati listed in the OBC lists for that state. Due to variation in terminology, using this method, only 11% of those who turned 17 before 1994 were OBC, and 12 of those who turned 17 afterwards. The results in these tables broadly echo those in Section Five.

An additional "categorization problem" is the presence of the creamy layer policy, which means that some OBCs did not benefit from reservation because their families had too high an income. It is unfortunately not possible to cleanly exclude these individuals, since the IHDS does not include questions on the respondent's household income at age 17 (the relevant variable), and even if it were available the

widespread faking of income certificates would make such a measure an unreliable guide to who actually took advantage of the policy. As a rough proxy, Appendix Section H reports results that exclude all OBCs whose fathers had both a professional occupation[15] and had completed secondary school. These tables produce results that are similar in substantive size and significance to those in the main tables.

5.5 Additional Controls

There are any number of potential factors that might influence individual education, social contacts, and associational membership. Most however, fall into two categories: They are either perfectly collinear with the state, birth year and caste category fixed effects included in the main models, or are potentially causally associated with quotas, and thus post treatment. However, Appendix Section F includes five additional individual control variables that are arguably assigned independently of quotas: gender, religion FEs, and father's years of education. Including these controls in the models has minimal effects relative to the models in Section Five.

Tables I.1 through I.5 examine two additional hypotheses that might explain changes in the relative social status of OBCs. The mid-1990s were a period when OBC politicians rose to political prominence in much of northern India, and also a period of increasing migration to urban centers. Either trend might explain advances among OBCs in this period, at least if we believe that OBC politicians favor their coethnics over others and that migration might favor the traditionally marginalized. Tables I.1 through I.5 includes controls for OBC chief ministers and the presence of migrants in the household, and their interaction with OBC. These tables produce results quite similar to the models in the main text.

6 Conclusion

The imposition of hiring and educational quotas in India appears to have neither ignited a social revolution nor been an abject failure. While the effects of the programs took some time to be fully realized, they have been associated with modest increases in individual education, government

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[15] "Managerial," scientists, government officials, doctors, lawyers and accountants.

employment, and levels of social contacts with educated professionals among the uneducated. These effects do not appear to be concentrated among the children of the very highly educated, though this many be a product of the specific design of Indian affirmative action policies and the relative poverty of Indian OBCs. These results do not appear a product of pretrends (which, in the the led treatment models, appear minimal), political changes, or problems in the classification of individuals.

These results paint a more nuanced, and somewhat more positive, portrayal of the effects of OBC reservation than that common in many accounts of the issue. It shows that affirmative action tends neither to redistribute opportunities to those who are too poor a "match" to make use of them, or are so wealthy that they would always take advantage them: There are real effects throughout the beneficiary group, though perhaps somewhat smaller at the very bottom. These changes have the secondary effect of increasing the proportion of individuals with bureaucratic and professional contacts in their own caste, creating both a potential pool of role models and conferring a real advantage in a political economy as dependent on personal contacts as India.

The results suggest that affirmative action policies in hiring and education may have positive effects similar to the better-examined ones in the political sphere. More research is necessary on whether these results can be extended to other regions or regions with differently designed policies—i.e. "soft" quotas, no exclusion for the wealthy. Similarly, more research is needed on the interaction between the social effects of policies on targeted groups (discussed here) and their effect on social and institutional efficiency. Given the importance of these types of policies in many countries, this is research well worth pursuing.

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Online Appendix

A   Additional Figures

Figure A.1: Education by State and Caste Category in India 1979-2012

(a) Non-Mandal State   (b) Mandal State

Note: The Y axis shows the average years of education for those over 23 for individuals of a particular caste category born in that year. The solid line corresponds to 1994, the dotted line corresponds to 1998 (the year when those who were 14 in 1994 turned 18).

Figure A.2: Middle Class Employment by State and Caste Category in India 1979-2007
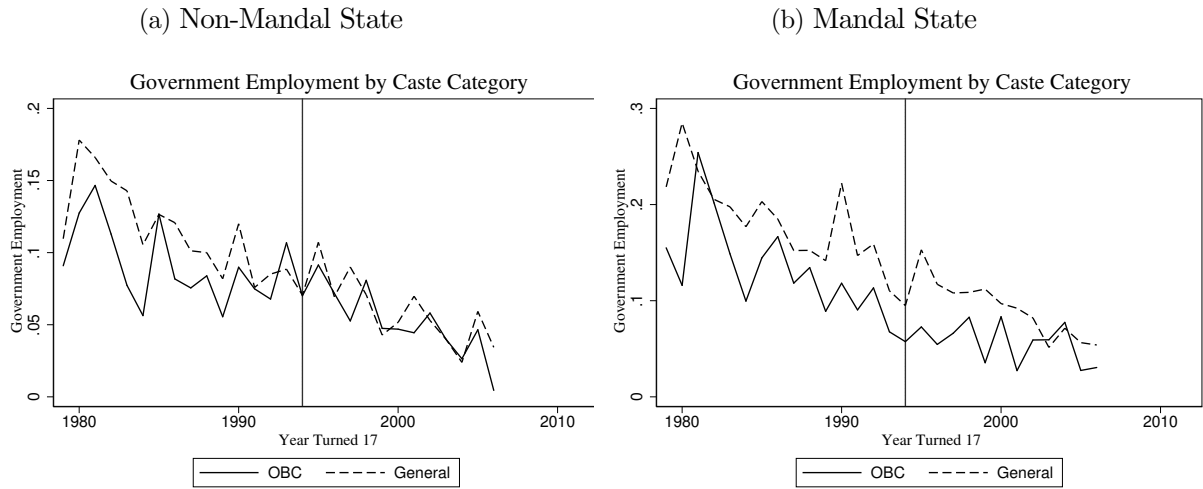
(a) Non-Mandal State   (b) Mandal State

Note: The Y axis shows the proportion of employed individuals of a particular caste category who turned 18 in that year who have a middle class job. The solid line corresponds to 1994, the dotted line corresponds to 1998 (the year when those who were 14 in 1994 turned 18).

Figure A.3: Social Contacts by State and Caste Category in India 1979-2012
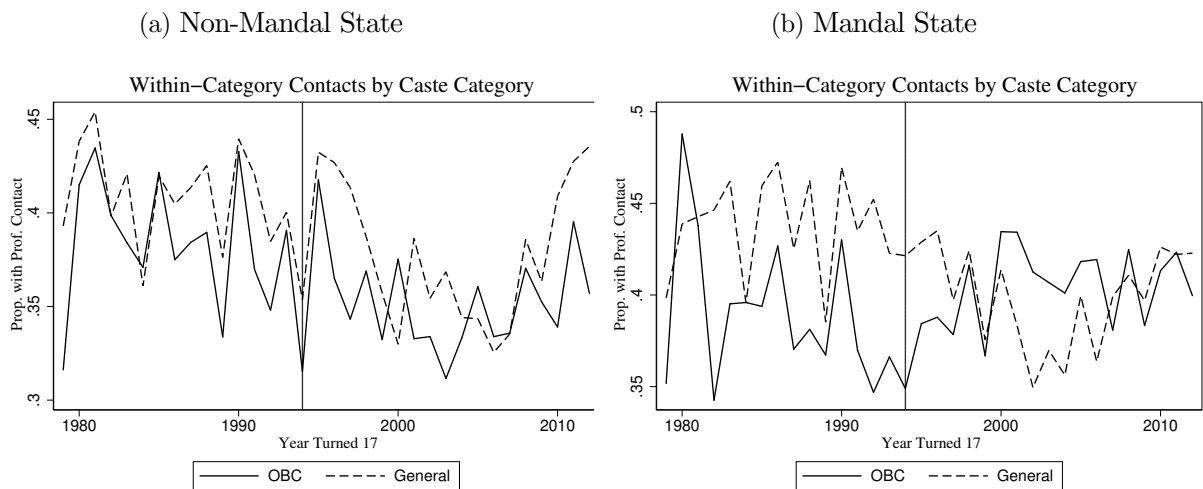
(a) Non-Mandal State   (b) Mandal State

Note: The Y axis shows the proportion of individuals of a particular caste category who turned 18 in that year who live in a household where a member knows a doctor, teacher police inspector or gazetted government officer. The solid line corresponds to 1994, the dotted line corresponds to 1998 (the year when those who were 14 in 1994 turned 18).

Figure A.4: Education by Caste Category in India with NFHS Data

(a) Years of Education: Mandal States   (b) Years of Education: Non-Mandal States
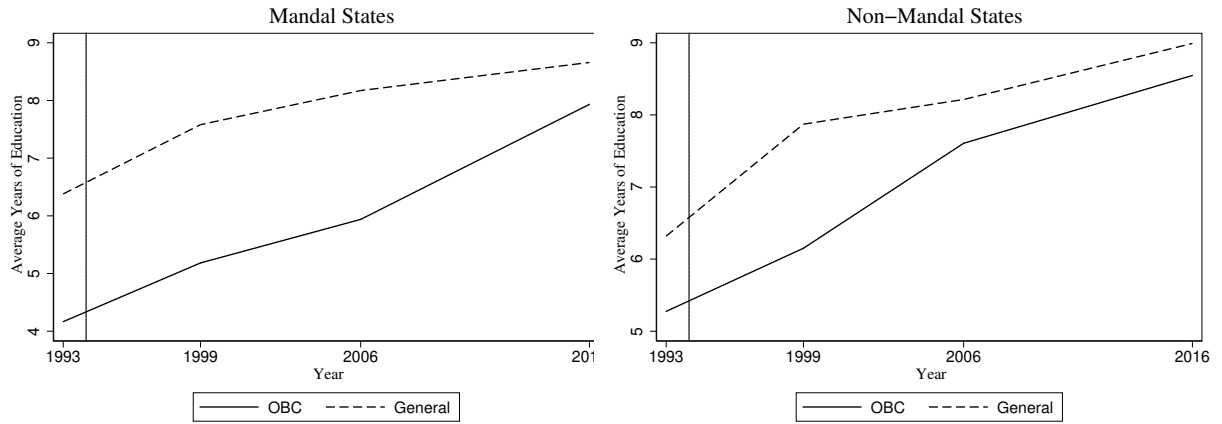
Note: The Y axis shows the average years of education for individuals 18-60 at each of the four NFHS survey years. The solid line corresponds to 1994. Figure a covers states when OBC reservation was introduced in 1994, Figure b covers states when OBC reservation was in place between 1979 and 1994.

B   Data Description and Summary Statistics

Table B.1: Summary statistics

Variable	Mean	Std. Dev.	N
Education: Completed Years	7.838	4.928	66142
Government Job	0.108	0.31	18315
In-Caste Professional contace	0.431	0.495	58533
University Educated Father	0.053	0.225	50974
Age	31.3	9.356	66230
Married	0.703	0.457	66230
OBC Caste	0.578	0.494	66230
Post 1994	0.651	0.477	66230
Mandal State	0.529	0.499	66230

Table B.2: OBC Reservations in India: A Summary

State	Some Colonial Res.	Post 1947 Res. Began	2011 OBC Quota	Subquota Year	Notes
Andhra Pradesh	Yes	1947	23%	1970	System reformed on state reorganization 1964
Tamil Nadu	Yes	1947	68%	1989	Quota expanded from 25% before 1971 and 31% before 1980
Maharashtra	Yes	1947	32%	1964	System reformed on state reorganization 1964
Kerala	Yes	1964	40%		
Punjab	No	1964	12%		Quota 5% before 1993
Karnataka	Yes	1977	32%	1977	50% quota pre-1956, changed to class-based 1961, 35% 1977, 55'% quota 1986, 32% quota 1994
Bihar	No	1978	33%	1978	Quota increased from 20% in 1992
Gujarat	Yes	1978	27%		Some quotas pre-1964, before states reorganization
Jharkhand	No	1978	14%	1978	Quota shrunk from 33% in 2000 on state reorganization
Haryana	No	1991	27%	1995	Class-based res. from 1969
Assam	No	1994	27%		
Chhattisgarh	No	1994	14%		
Delhi	No	1994	27%		
Goa	No	1994	19.5%		
Himachal Pradesh	No	1994	20%		Class-based res. from 1970
Madhya Pradesh	No	1994	14%		
Manipur	No	1994	17%		
Orissa	No	1994	12%		
Rajasthan	No	1994	21%		
Sikkim	No	1994	23%		
Uttar Pradesh	No	1994	27%		
Uttarakhand	No	1994	14%		Quota shrunk from 27% in 2000 on state reorganization
West Bengal	No	1994	7%		

Tripura, Meghalaya, Mizoram and Arunachal Pradesh do not have OBC reservations, Jammu and Kashmir has class-based reservations. Union territories are not included for reasons of space. Sources: See .

C DDD Models

Table C.1: Quotas and Education

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-1.993***		-1.854***	
	(0.157)		(0.137)	
Post 1994	2.188***		2.375***	
	(0.137)		(0.125)	
Mandal State	-0.653***		-0.555***	
	(0.165)		(0.143)	
OBC*Post 94	0.486***		0.325**	
	(0.177)		(0.164)	
OBC Caste*Mandal State	-0.496**		-0.591***	
	(0.212)		(0.186)	
Post 94*Mandal State	-0.381**		-0.642***	
	(0.182)		(0.171)	
OBC*Post 94*Mandal State	0.332	0.219	0.586***	0.527**
	(0.241)	(0.240)	(0.226)	(0.221)
Observations	66,142	66,142	66,142	66,142
R-squared	0.085	0.208	0.092	0.208
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	YES	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models are weighted for sampling probability and have standard errors clustered by household. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table C.2: Quotas and Government Employment

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-0.0856***		-0.0831***	
	(0.0139)		(0.0118)	
Post 1994	-0.0798***		-0.0929***	
	(0.0158)		(0.0141)	
Mandal State	0.0517***		0.0399***	
	(0.0172)		(0.0149)	
OBC*Post 94	0.0488***		0.0640***	
	(0.0176)		(0.0162)	
OBC Caste*Mandal State	-0.0141		-0.0139	
	(0.0204)		(0.0175)	
Post 94*Mandal State	-0.0279		-0.00664	
	(0.0214)		(0.0202)	
OBC*Post 94*Mandal State	-0.00842	-0.0343	-0.0191	-0.0500**
	(0.0250)	(0.0260)	(0.0238)	(0.0247)
Observations	18,315	18,315	18,315	18,315
R-squared	0.030	0.130	0.028	0.130
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	YES	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with an indicator for government employment as the dependent variable. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $***p<0.01$, $**p<0.05$, $*p<0.1$.

Table C.3: Quotas and Within-Caste Professional Contacts

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-0.132***		-0.144***	
	(0.0166)		(0.0151)	
Post 1994	0.0160		0.00539	
	(0.0135)		(0.0135)	
Mandal State	0.0217		0.0193	
	(0.0178)		(0.0167)	
OBC*Post 94	-0.0327*		-0.0167	
	(0.0171)		(0.0168)	
OBC Caste*Mandal State	-0.00734		-0.00966	
	(0.0236)		(0.0215)	
Post 94*Mandal State	-0.0497***		-0.0561***	
	(0.0186)		(0.0190)	
OBC*Post 94*Mandal State	0.0706***	0.0289	0.0892***	0.0563**
	(0.0243)	(0.0259)	(0.0242)	(0.0263)
Observations	58,533	58,533	58,533	58,533
R-squared	0.018	0.081	0.019	0.081
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	YES	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with a binary measure for whether someone in the household knows a doctor, teacher police inspector or gazetted government officer as the dependent variable. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table C.4: Quotas, Government Jobs and Paternal Education

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Panel A: University Educated Fathers				
Post 94	-0.110***		-0.0924***	
	(0.0160)		(0.0168)	
Ed. Father	0.278***		0.292***	
	(0.0578)		(0.0502)	
Post 94*Ed. Father	-0.0757		-0.141**	
	(0.0718)		(0.0691)	
OBC	-0.0895***		-0.0815***	
	(0.0148)		(0.0126)	
Post 94*OBC	0.0520***	0.0339	0.0447**	0.0187
	(0.0191)	(0.0214)	(0.132)	(0.0206)
Ed. Father*OBC	0.0587		-0.0219	
	(0.122)		(0.0970)	
Post 94*Ed. Father*OBC	-0.0479	0.187	0.105	0.142
	(0.145)	(0.126)	(0.145)	(0.135)
Observations	7,043	7,043	7,043	7,043
R-squared	0.059	0.267	0.059	0.267
Panel B: Fathers with Any Education				
VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Post 94	-0.0521***		-0.0568***	
	(0.0153)		(0.0144)	
Ed. Father	0.218***		0.187***	
	(0.0223)		(0.0203)	
Post 94*Ed. Father	-0.126***		-0.0932***	
	(0.0287)		(0.0293)	
OBC	-0.0424***		-0.0360***	
	(0.0135)		(0.0125)	
Post 94*OBC	0.0342*	0.0259	0.0272	0.0289
	(0.0190)	(0.0220)	(0.0181)	(0.0210)
Ed. Father*OBC	-0.0488		-0.0711***	
	(0.0315)		(0.0264)	
Post 94*Ed. Father*OBC	-0.0107	-0.00782	0.0263	-0.0174
	(0.0383)	(0.0426)	(0.0363)	(0.0388)
Observations	7,043	7,043	7,043	7,043
R-squared	0.084	0.285	0.070	0.285
Observations	27,044	27,044	27,044	27,044
R-squared	0.247	0.326	0.252	0.326
State-Caste Category*Ed Father FE	NO	YES	NO	YES
State-Birth Year*Ed Father FE	NO	YES	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The tables show the estimates from linear regression models with government employment as the dependent variable. "Post 94" refers to the year respondents turned 17 or 13, as noted in the column headings. All models are weighted for sampling probability and have standard errors clustered by household. Educated fathers are those with more than a secondary education (Panel A), or any education (Panel B). ***$p<0.01$, **$p<0.05$, *$p<0.1$.

D Lags

Table D.1: Education: Eight Year Lagged Treatment

VARIABLES	(1) yearsofeducation	(2) yearsofeducation	(3) yearsofeducation	(4) yearsofeducation
OBC Caste	-2.309***		-1.764***	
	(0.147)		(0.146)	
Post 1994	1.388***		1.991***	
	(0.143)		(0.136)	
Mandal State			-0.668***	
			(0.165)	
OBC*Post 94	0.943***	0.844***	0.398**	
	(0.183)	(0.196)	(0.176)	
OBC Caste*Mandal State			-0.545***	
			(0.207)	
Post 94*Mandal State			-0.603***	
			(0.197)	
OBC*Post 94*Mandal State			0.545**	0.607**
			(0.254)	(0.258)
Observations	28,628	28,628	53,474	53,474
R-squared	0.070	0.145	0.082	0.190
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. "Post 94" refers to the year respondents turned 10. Only individuals with fathers who graduated from secondary school are included.All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table D.2: Government Employment: Eight Year Lagged Treatment

VARIABLES	(1) govjob	(2) govjob	(3) govjob	(4) govjob
OBC Caste	-0.0850***		-0.0526***	
	(0.0133)		(0.0112)	
Post 1994	-0.0670***		-0.0483***	
	(0.0172)		(0.0152)	
Mandal State			0.0396**	
			(0.0154)	
OBC*Post 94	0.0441**	0.0409**	0.0310*	
	(0.0202)	(0.0207)	(0.0181)	
OBC Caste*Mandal State			-0.0324*	
			(0.0174)	
Post 94*Mandal State			-0.0187	
			(0.0230)	
OBC*Post 94*Mandal State			0.0131	0.0225
			(0.0271)	(0.0263)
Observations	6,624	6,624	13,708	13,708
R-squared	0.021	0.128	0.017	0.100
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with an indicator for government employment as the dependent variable. "Post 94" refers to the year respondents turned 10. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table D.3: Contacts: Eight Year Lagged Treatment

VARIABLES	(1) Caste Contact	(2) Caste Contact	(3) Caste Contact	(4) Caste Contact
OBC Caste	-0.143***		-0.153***	
	(0.0171)		(0.0171)	
Post 1994	-0.0387**		0.00558	
	(0.0173)		(0.0168)	
Mandal State			0.00468	
			(0.0197)	
OBC*Post 94	0.0641***	0.0693***	-0.00832	
	(0.0213)	(0.0249)	(0.0208)	
OBC Caste*Mandal State			0.0102	
			(0.0242)	
Post 94*Mandal State			-0.0443*	
			(0.0242)	
OBC*Post 94*Mandal State			0.0724**	0.0647**
			(0.0298)	(0.0327)
Observations	25,214	25,214	47,034	47,034
R-squared	0.013	0.057	0.018	0.080
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with a binary measure for whether someone in the household knows a doctor, teacher police inspector or gazetted government officer as the dependent variable. "Post 94" refers to the year respondents turned 10. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

D Leads

Table E.1: Education: Four Year Treatment Lead

VARIABLES	(1) yearsofeducation	(2) yearsofeducation	(3) yearsofeducation	(4) yearsofeducation
OBC Caste	-2.664***		-2.205***	
	(0.144)		(0.150)	
Post 1994	0.647***		0.853***	
	(0.184)		(0.213)	
Mandal State			-0.574***	
			(0.157)	
OBC*Post 94	0.241	0.328	0.453*	
	(0.247)	(0.262)	(0.269)	
OBC Caste*Mandal State			-0.459**	
			(0.208)	
Post 94*Mandal State			-0.206	
			(0.281)	
OBC*Post 94*Mandal State			-0.212	-0.0975
			(0.365)	(0.374)
Observations	15,115	15,115	29,631	29,631
R-squared	0.066	0.144	0.061	0.164
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. "Post 94" refers to the year respondents turned 21. Only individuals with fathers who graduated from secondary school are included. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $***p<0.01$, $**p<0.05$, $*p<0.1$.

Table E.2: Education: Eight Year Treatment Lead

VARIABLES	(1) yearsofeducation	(2) yearsofeducation	(3) yearsofeducation	(4) yearsofeducation
OBC Caste	-2.825***		-2.347***	
	(0.141)		(0.148)	
Post 1994	0.874***		0.928***	
	(0.162)		(0.174)	
Mandal State			-0.597***	
			(0.160)	
OBC*Post 94	0.466**	0.336	0.559**	
	(0.217)	(0.238)	(0.222)	
OBC Caste*Mandal State			-0.478**	
			(0.205)	
Post 94*Mandal State			-0.0538	
			(0.238)	
OBC*Post 94*Mandal State			-0.0937	-0.159
			(0.310)	(0.327)
Observations	17,670	17,670	34,710	34,710
R-squared	0.076	0.154	0.070	0.170
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. "Post 94" refers to the year respondents turned 25. Only individuals with fathers who graduated from secondary school are included.All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table E.3: Government Employment: Four Year Treatment Lead

VARIABLES	(1) govjob	(2) govjob	(3) govjob	(4) govjob
OBC Caste	-0.104***		-0.0899***	
	(0.0182)		(0.0157)	
Post 1994	-0.0897***		-0.0633***	
	(0.0229)		(0.0219)	
Mandal State			0.0747***	
			(0.0191)	
OBC*Post 94	0.00477	0.0437	0.0204	
	(0.0277)	(0.0311)	(0.0242)	
OBC Caste*Mandal State			-0.0143	
			(0.0240)	
Post 94*Mandal State			-0.0265	
			(0.0316)	
OBC*Post 94*Mandal State			-0.0156	0.0211
			(0.0368)	(0.0383)
Observations	5,109	5,109	10,836	10,836
R-squared	0.029	0.202	0.034	0.158
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with an indicator for government employment as the dependent variable. "Post 94" refers to the year respondents turned 21. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table E.4: Government Employment: Eight Year Treatment Lead

VARIABLES	(1) govjob	(2) govjob	(3) govjob	(4) govjob
OBC Caste	-0.143***		-0.109***	
	(0.0202)		(0.0175)	
Post 1994	-0.132***		-0.0997***	
	(0.0208)		(0.0208)	
Mandal State			0.0907***	
			(0.0218)	
OBC*Post 94	0.0521*	0.0699**	0.0478**	
	(0.0267)	(0.0296)	(0.0232)	
OBC Caste*Mandal State			-0.0338	
			(0.0268)	
Post 94*Mandal State			-0.0323	
			(0.0294)	
OBC*Post 94*Mandal State			0.00431	0.0340
			(0.0353)	(0.0371)
Observations	5,790	5,790	12,295	12,295
R-squared	0.040	0.214	0.043	0.167
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with an indicator for government employment as the dependent variable. "Post 94" refers to the year respondents turned 25. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table E.5: Contacts: Four Year Treatment Lead

VARIABLES	(1) Caste Contact	(2) Caste Contact	(3) Caste Contact	(4) Caste Contact
OBC Caste	-0.130***		-0.139***	
	(0.0186)		(0.0168)	
Post 1994	-0.0152		-0.0284	
	(0.0200)		(0.0213)	
Mandal State			0.00785	
			(0.0188)	
OBC*Post 94	-0.0196	-0.0288	0.0101	
	(0.0272)	(0.0299)	(0.0263)	
OBC Caste*Mandal State			0.00879	
			(0.0250)	
Post 94*Mandal State			0.0132	
			(0.0293)	
OBC*Post 94*Mandal State			-0.0297	-0.0544
			(0.0378)	(0.0398)
Observations	13,522	13,522	26,765	26,765
R-squared	0.019	0.081	0.019	0.090
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with a binary measure for whether someone in the household knows a doctor, teacher police inspector or gazetted government officer as the dependent variable. "Post 94" refers to the year respondents turned 21. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $***p<0.01$, $**p<0.05$, $*p<0.1$.

Table E.6: Contacts: Eight Year Treatment Lead

VARIABLES	(1) Caste Contact	(2) Caste Contact	(3) Caste Contact	(4) Caste Contact
OBC Caste	-0.124***		-0.155***	
	(0.0195)		(0.0167)	
Post 1994	-0.00314		-0.0317*	
	(0.0186)		(0.0181)	
Mandal State			-0.00478	
			(0.0198)	
OBC*Post 94	-0.0207	-0.0356	0.0273	
	(0.0252)	(0.0279)	(0.0227)	
OBC Caste*Mandal State			0.0307	
			(0.0257)	
Post 94*Mandal State			0.0285	
			(0.0260)	
OBC*Post 94*Mandal State			-0.0479	-0.0664*
			(0.0339)	(0.0358)
Observations	15,895	15,895	31,503	31,503
R-squared	0.018	0.082	0.020	0.091
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with a binary measure for whether someone in the household knows a doctor, teacher police inspector or gazetted government officer as the dependent variable. "Post 94" refers to the year respondents turned 25. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

F   Controls

Table F.1: Education: Controls

VARIABLES	(1) Age 17 Treat.	(2) Age 17 Treat.	(3) Age 13 Treat.	(4) Age 13 Treat.
OBC Caste	-1.388***		-1.329***	
	(0.128)		(0.125)	
Post 1994	1.070***		1.134***	
	(0.105)		(0.109)	
OBC*Post 94	0.558***	0.603***	0.582***	0.653***
	(0.150)	(0.168)	(0.153)	(0.169)
Observations	27,044	27,044	24,778	24,778
R-squared	0.384	0.428	0.381	0.423
Controls	YES	YES	YES	YES
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	YES	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models include controls for gender, urban residence, size of household, number on men and women in the household, and religion dummies. Only individuals with fathers who graduated from secondary school are included. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4.

Table F.2: Government Employment: Controls

VARIABLES	(1) Age 17 Treat.	(2) Age 17 Treat.	(3) Age 13 Treat.	(4) Age 13 Treat.
OBC Caste	-0.0746***		-0.0708***	
	(0.0142)		(0.0128)	
Post 1994	-0.131***		-0.0953***	
	(0.0158)		(0.0172)	
OBC*Post 94	0.0547***	0.0429**	0.0505***	0.0204
	(0.0188)	(0.0203)	(0.0196)	(0.0198)
Observations	7,043	7,043	6,260	6,260
R-squared	0.115	0.256	0.095	0.226
Controls	YES	YES	YES	YES
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	YES	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with an indicator for government employment as the dependent variable. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models include controls for gender, urban residence, size of household, number on men and women in the household, and religion dummies. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table F.3: Contacts: Controls

VARIABLES	(1) Age 17 Treat.	(2) Age 17 Treat.	(3) Age 13 Treat.	(4) Age 13 Treat.
OBC Caste	-0.104***		-0.117***	
	(0.0173)		(0.0169)	
Post 1994	-0.0833***		-0.0991***	
	(0.0142)		(0.0169)	
OBC*Post 94	0.0564***	0.0523**	0.0919***	0.102***
	(0.0189)	(0.0213)	(0.0213)	(0.0245)
Observations	24,319	24,319	22,281	22,281
R-squared	0.058	0.107	0.062	0.108
Controls	YES	YES	YES	YES
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	YES	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with a binary measure for whether someone in the household knows a doctor, teacher police inspector or gazetted government officer as the dependent variable. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models include controls for gender, urban residence, size of household, number on men and women in the household, and religion dummies. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table F.4: Differential Effects on Education: Controls

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Panel A: Fathers with University Education				
Post 1994	1.116***		1.205***	
	(0.111)		(0.110)	
Ed. Father	0.441		0.507**	
	(0.279)		(0.247)	
Post 94*Ed. Father	-0.787***		-0.907***	
	(0.285)		(0.259)	
OBC Caste	-1.368***		-1.355***	
	(0.131)		(0.116)	
Post 94*OBC Caste	0.534***		0.609***	
	(0.156)		(0.152)	
Ed. Father*OBC Caste	0.441		0.542	
	(0.551)		(0.488)	
Post 94*Ed. Father*OBC	-0.709		-0.959*	
	(0.599)		(0.564)	
Observations	27,044	27,044	27,044	27,044
R-squared	0.384	0.435	0.389	0.436
Panel B: Fathers with Any Education				
Post 1994 = 1	1.346***		1.271***	
	(0.183)		(0.196)	
Ed. Father	0.530**		0.382**	
	(0.212)		(0.195)	
1.temp_int_post94#Ed. Father	-0.465**		-0.231	
	(0.232)		(0.239)	
OBC Caste = 1	-1.201***		-1.198***	
	(0.182)		(0.167)	
1.temp_int_post94#1.temp_int*OBC	0.350	0.732***	0.537**	0.593**
	(0.238)	(0.207)	(0.257)	(0.282)
Ed. Father*1.temp_int*OBC	-0.321		-0.328	
	(0.259)		(0.226)	
1.temp_int_post94#Ed. Father*1.temp_int*OBC	0.327	-0.258	0.198	0.306
	(0.323)	(0.233)	(0.322)	(0.352)
Observations	27,044	27,044	27,044	27,044
R-squared	0.384	0.438	0.388	0.445
State-Caste Category*Ed Father FE	NO	YES	NO	YES
State-Birth Year*Ed Father FE	NO	YES	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. OBCs are defined as those belonging to a state-jati where over 90% of jati members identify as OBC. "Post 94" refers to the year respondents turned 17 or 13, as noted in the column headings. All models include controls for gender, urban residence, size of household, number on men and women in the household, and religion dummies. All models are weighted for sampling probability and have standard errors clustered by household. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table F.5: Differential Effects on Government Employment: Controls

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Panel A: Fathers with University Education				
Post 1994	-0.108***		-0.108***	
	(0.0175)		(0.0175)	
Ed.Father	0.0766		0.0766	
	(0.0540)		(0.0540)	
Post 94*Ed.Father	-0.134**		-0.134**	
	(0.0684)		(0.0684)	
OBC Caste	-0.0668***		-0.0668***	
	(0.0125)		(0.0125)	
Post 94*OBC Caste	0.0430**		0.0430**	
	(0.0198)		(0.0198)	
Ed.Father *OBC Caste	-0.0429		-0.0429	
	(0.0964)		(0.0964)	
Post 94*Ed.Father*OBC	0.124		0.124	
	(0.131)		(0.131)	
Observations	7,043	7,043	7,043	7,043
R-squared	0.109	0.308	0.109	0.308
Panel B: Fathers with Any Education				
Post 1994 = 1	-0.0512***		-0.0573***	
	(0.0150)		(0.0147)	
Ed. Father	0.0327		0.00366	
	(0.0268)		(0.0248)	
1.temp_int_post94#Ed. Father	-0.145***		-0.107***	
	(0.0281)		(0.0296)	
OBC Caste = 1	-0.0569***		-0.0519***	
	(0.0143)		(0.0129)	
1.temp_int_post94#1.temp_int*OBC	0.0365*	0.0880***	0.0335*	0.0361*
	(0.0188)	(0.0208)	(0.0182)	(0.0216)
Ed. Father*1.temp_int*OBC	-0.00445		-0.0255	
	(0.0307)		(0.0260)	
1.temp_int_post94#Ed. Father*1.temp_int*OBC	-0.0105	-0.118***	0.0181	-0.0264
	(0.0372)	(0.0245)	(0.0359)	(0.0387)
Observations	7,043	7,043	7,043	7,043
R-squared	0.131	0.316	0.115	0.316
Observations	27,044	27,044	27,044	27,044
R-squared	0.384	0.438	0.388	0.445
Observations	7,043	7,043	7,043	7,043
R-squared	0.131	0.316	0.115	0.316
State-Caste Category*Ed Father FE	NO	YES	NO	YES
State-Birth Year*Ed Father FE	NO	YES	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with government employment as the dependent variable. OBCs are defined as those belonging to a state-jati where over 90% of jati members identify as OBC. "Post 94" refers to the year respondents turned 17 or 13, as noted in the column headings. All models include controls for gender, urban residence, size of household, number on men and women in the household, and religion dummies. All models are weighted for sampling probability and have standard errors clustered by household.
***p<0.01, **p<0.05, *p<0.1.

G   Individual Movement between Categories

Table G.1: Education: Caste Category Defined by Jati

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-1.466***		-1.178***	
	(0.155)		(0.151)	
Post 1994	-0.408***		-0.376**	
	(0.156)		(0.169)	
OBC*Post 94	1.667***	1.374***	1.569***	1.327***
	(0.185)	(0.187)	(0.183)	(0.183)
Time Trend	0.174***	-0.309	0.183***	-0.317
	(0.00755)	(0.863)	(0.00929)	(0.857)
OBC*Time Trend	-0.0744***	-0.0814***	-0.0738***	-0.0806***
	(0.00488)	(0.00535)	(0.00488)	(0.00534)
Observations	35,021	35,021	32,477	32,477
R-squared	0.083	0.146	0.082	0.144
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. OBCs are defined as those belonging to a state-jati where over 90% of jati members identify as OBC. "Post 94" refers to the year respondents turned 17 or 13. All models are weighted for sampling probability and have standard errors clustered by household. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table G.2: Government Employment: Caste Category Defined by Jati

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-0.0388**		-0.0125	
	(0.0175)		(0.0166)	
Post 1994	-0.0254		-0.00818	
	(0.0172)		(0.0183)	
OBC*Post 94	0.0851***	0.0675***	0.0664***	0.0363
	(0.0212)	(0.0208)	(0.0241)	(0.0232)
Time Trend	-0.00371***	0.00453***	-0.00335**	0.00438***
	(0.00122)	(0.000740)	(0.00145)	(0.000743)
OBC*Time Trend	-0.00440***	-0.00453***	-0.00435***	-0.00438***
	(0.000600)	(0.000739)	(0.000605)	(0.000741)
Observations	8,784	8,784	7,915	7,915
R-squared	0.037	0.166	0.028	0.146
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with an indicator for government employment as the dependent variable. OBCs are defined as those belonging to a state-jati where over 90% of jati members identify as OBC. "Post 94" refers to the year respondents turned 17 or 13. All models are weighted for sampling probability and have standard errors clustered by household. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table G.3: Differential Effects on Government Jobs: Caste Category Defined by Jati

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Panel A: Fathers with University Education				
Post 94	-0.0977***		-0.0798***	
	(0.0102)		(0.00970)	
Ed. Father	0.300***		0.301***	
	(0.0532)		(0.0460)	
Post 94*Ed. Father	-0.104		-0.138**	
	(0.0644)		(0.0611)	
OBC	-0.0555***		-0.0427***	
	(0.0155)		(0.0128)	
Post 94*OBC	0.0617***		0.0556**	0.031
	(0.0216)		(0.0235)	(0.024)
Ed. Father*OBC	0.150	0.044**	-0.0219	
	(0.159)	(0.022)	(0.0970)	
Post 94*Ed. Father*OBC	-0.0306	-0.0436	0.0666	0.103
	(0.191)	(0.155)	(0.188)	(0.107)
Observations	7,043	7,043	7,043	7,043
R-squared	0.051	0.256	0.044	0.256
Panel B: Fathers with Any Education				
Post 94	-0.0353***		-0.0383***	
	(0.0111)		(0.0109)	
Ed. Father	0.209***		0.167***	
	(0.0171)		(0.0148)	
Post 94*Ed. Father	-0.153***		-0.109***	
	(0.0210)		(0.0199)	
OBC	-0.0199		-0.0122	
	(0.0121)		(0.0120)	
Post 94*OBC	0.0147	0.00955	-0.00723	0.0174
	(0.0187)	(0.0162)	(0.0155)	(0.0135)
Ed. Father*OBC	-0.0318		-0.0377	
	(0.0398)		(0.0312)	
Post 94*Ed. Father*OBC	0.0680	0.0507	0.108**	0.0257
	(0.0491)	(0.0316)	(0.0462)	(0.0336)
Observations	7,043	7,043	7,043	7,043
R-squared	0.077	0.286	0.061	0.288
State-Caste Category*Ed Father FE	NO	YES	NO	YES
State-Birth Year*Ed Father FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with government employment as the dependent variable. The definition of educated fathers is listed in the column headings. OBCs are defined based on the state list. "Post 94" refers to the year respondents turned 17 or 13. All models are weighted for sampling probability and have standard errors clustered by household. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table G.4: Differential Effects on Education: Caste Category Defined by Jati

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Panel A: Fathers with University Education				
Post 94	1.929***		2.101***	
	(0.108)		(0.107)	
Ed. Father	7.517***		7.405***	
	(0.247)		(0.216)	
Post 94*Ed. Father	-1.996***		-2.224***	
	(0.279)		(0.263)	
OBC	-1.323***		-1.213***	
	(0.159)		(0.143)	
Post 94*OBC	0.967***	0.776***	1.020***	0.856***
	(0.206)	(0.205)	(0.200)	(0.198)
Ed. Father*OBC	-0.624		1.162**	
	(0.900)		(0.514)	
Post 94*Ed. Father*OBC	0.500	1.247	-0.366	-0.0826
	(0.955)	(0.796)	(0.769)	(0.723)
Observations	27,044	27,044	27,044	27,044
R-squared	0.121	0.212	0.132	0.212
Panel B: Fathers with Any Education				
Post 94	1.169***		1.208***	
	(0.142)		(0.149)	
Ed. Father	4.458***		4.419***	
	(0.151)		(0.136)	
Post 94*Ed. Father	0.298		0.353*	
	(0.196)		(0.196)	
inlist = 1	-0.0893		-0.0577	
	(0.179)		(0.171)	
Post 94*OBC	0.417	0.565**	0.543*	0.605***
	(0.272)	(0.220)	(0.293)	(0.231)
Ed. Father*OBC	-1.915***		-1.725***	
	(0.330)		(0.282)	
Post 94*Ed. Father*OBC	0.962**	-0.442	0.694*	-0.332
	(0.416)	(0.277)	(0.395)	(0.273)
Observations	27,044	27,044	27,044	27,044
R-squared	0.233	0.325	0.237	0.343
State-Caste Category*Ed Father FE	NO	YES	NO	YES
State-Birth Year*Ed Father FE	NO	YES	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. The definition of educated fathers is listed in the column headings. OBCs are defined based on the state list. "Post 94" refers to the year respondents turned 17 or 13. All models are weighted for sampling probability and have standard errors clustered by household. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table G.5: Contacts: Caste Category Defined by Jati

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-0.0654***		-0.0579***	
	(0.0207)		(0.0198)	
Post 1994	-0.0254		-0.0225	
	(0.0195)		(0.0203)	
OBC*Post 94	0.0538**	0.0348	0.0588***	0.0399*
	(0.0210)	(0.0214)	(0.0224)	(0.0230)
Time Trend	0.00207**	0.00260	0.00214**	-0.0660
	(0.000946)	(556.1)	(0.00103)	(0.337)
OBC*Time Trend	-0.00401***	-0.00324***	-0.00402***	-0.00324***
	(0.000715)	(0.000767)	(0.000708)	(0.000762)
Observations	30,966	30,966	28,695	28,695
R-squared	0.010	0.059	0.010	0.057
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with a binary measure for whether someone in the household knows a doctor, teacher police inspector or gazetted government officer as the dependent variable. OBCs are defined as those belonging to a state-jati where over 90% of jati members identify as OBC. "Post 94" refers to the year respondents turned 17 or 13. All models are weighted for sampling probability and have standard errors clustered by household. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

H   No "Creamy Layer"

Table H.1: Education: No "Creamy Layer"

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-2.558***		-2.239***	
	(0.236)		(0.341)	
Post 1994	-0.372		-0.622**	
	(0.236)		(0.311)	
OBC*Post 94	0.700*	0.713*	1.135**	1.109***
	(0.379)	(0.369)	(0.446)	(0.421)
Time Trend	0.132***		0.153***	
	(0.0131)		(0.0185)	
OBC*Time Trend	0.00395	-0.00359	-0.0196	-0.0267
	(0.0215)	(0.0209)	(0.0272)	(0.0254)
Observations	34,903	34,903	32,365	32,365
R-squared	0.095	0.165	0.091	0.160
Controls	YES	YES	YES	YES
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table H.2: Differential Effects on Education: No "Creamy Layer"

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Panel A: Fathers with University Education				
Post 94	1.890***		1.945***	
	(0.143)		(0.144)	
Ed. Father	6.803***		6.691***	
	(0.275)		(0.236)	
Post 94*Ed. Father	-1.683***		-1.830***	
	(0.308)		(0.284)	
OBC	-1.998***		-2.002***	
	(0.150)		(0.134)	
Post 94*OBC	0.643***	0.581***	0.820***	0.851***
	(0.185)	(0.201)	(0.184)	(0.192)
Ed. Father*OBC	0.833		0.971*	
	(0.681)		(0.568)	
Post 94*Ed. Father*OBC	-0.649	-0.00605	-0.992	-0.676
	(0.724)	(0.736)	(0.642)	(0.636)
Observations	26,926	26,926	26,926	26,926
R-squared	0.140	0.237	0.151	0.238
Panel B: Fathers with Any Education				
Post 94	1.027***		0.930***	
	(0.192)		(0.199)	
Ed. Father	4.616***		4.586***	
	(0.197)		(0.175)	
Post 94*Ed. Father	0.408		0.558**	
	(0.249)		(0.252)	
OBC	-0.892***		-0.887***	
	(0.179)		(0.165)	
Post 94*OBC	0.459*	0.308	0.676***	0.637**
	(0.247)		(0.261)	
Ed. Father*OBC	-1.477***		-1.427***	
	(0.276)	(0.293)	(0.240)	(0.307)
Post 94*Ed. Father*OBC	0.430	0.793**	0.232	0.435
	(0.344)	(0.390)	(0.338)	(0.385)
Observations	26,926	26,926	26,926	26,926
R-squared	0.247	0.326	0.252	0.326
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. The definition of educated fathers is listed in the column headings. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table H.3: Differential Effects on Government Employment: No "Creamy Layer"

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Post 94	-0.110***		-0.0924***	
	(0.0160)		(0.0168)	
hEd. Father	0.278***		0.292***	
	(0.0578)		(0.0502)	
Post 94*Ed. Father	-0.0757		-0.141**	
	(0.0718)		(0.0691)	
OBC	-0.0904***		-0.0824***	
	(0.0148)		(0.0126)	
Post 94*OBC	0.0527***	0.0333	0.0457**	0.0187
	(0.0191)	(0.0214)	(0.0193)	(0.0206)
Ed. Father*OBC	-0.00427		-0.105	
	(0.117)		(0.0861)	
Post 94*Ed. Father*OBC	0.0132	0.256*	0.206	0.168
	(0.143)	(0.134)	(0.126)	(0.136)
Observations	7,012	7,012	7,012	7,012
R-squared	0.058	0.262	0.052	0.262
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with government employment as the dependent variable. The definition of educated fathers is listed in the column headings. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table H.4: Government Employment: No "Creamy Layer"

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-0.0818***		-0.0684***	
	(0.0182)		(0.0201)	
Post 1994	-0.0432*		-0.0340	
	(0.0220)		(0.0232)	
OBC*Post 94	0.0715***	0.0671**	0.0731***	0.0572**
	(0.0221)	(0.0261)	(0.0221)	(0.0241)
Time Trend	-0.00474***		-0.00415**	
	(0.00145)		(0.00171)	
OBC*Time Trend	-0.00266**	-0.00383**	-0.00270*	-0.00424**
	(0.00132)	(0.00173)	(0.00140)	(0.00174)
Observations	8,753	8,753	7,888	7,888
R-squared	0.041	0.168	0.032	0.146
Controls	YES	YES	YES	YES
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with an indicator for government employment as the dependent variable. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table H.5: Contacts: No "Creamy Layer"

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-0.144***		-0.132***	
	(0.0304)		(0.0418)	
Post 1994	-0.0255		-0.0724**	
	(0.0361)		(0.0362)	
OBC*Post 94	0.0284	0.0199	0.117**	0.118**
	(0.0579)	(0.0594)	(0.0563)	(0.0595)
Time Trend	-0.000493		0.00129	
	(0.00190)		(0.00197)	
OBC*Time Trend	0.000539	0.000992	-0.00256	-0.00201
	(0.00326)	(0.00331)	(0.00344)	(0.00353)
Observations	30,896	30,896	28,630	28,630
R-squared	0.014	0.062	0.015	0.061
Controls	YES	YES	YES	YES
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with a binary measure for whether someone in the household knows a doctor, teacher police inspector or gazetted government officer as the dependent variable. "Post 94" refers to the year respondents turned 17 or turned 13, depending on the column heading. All models are weighted for sampling probability and have standard errors clustered by household. Models 1-4 correspond to Equations 1-4. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

I   OBC Governments and Migration

Table I.1: Education: OBC Governments

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-2.411***		-2.204***	
	(0.238)		(0.341)	
Post 1994	-0.391*		-0.635**	
	(0.235)		(0.307)	
OBC*Post 94	0.726*	0.698*	1.119**	1.133***
	(0.379)	(0.364)	(0.448)	(0.421)
OBC Gov*OBC	-0.737***	0.248	-0.198	0.0796
	(0.112)	(0.266)	(0.131)	(0.253)
Migrant*OBC	1.335***	1.103***	1.285***	1.083***
	(0.434)	(0.416)	(0.436)	(0.415)
Migrant	-2.051***	-1.627***	-2.036***	-1.627***
	(0.368)	(0.346)	(0.366)	(0.341)
Time Trend	0.134***		0.155***	
	(0.0131)		(0.0182)	
OBC*Time Trend	0.00738	-0.00487	-0.0175	-0.0284
	(0.0218)	(0.0213)	(0.0276)	(0.0256)
Observations	35,021	35,021	32,477	32,477
R-squared	0.098	0.165	0.092	0.160
Controls	YES	YES	YES	YES
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. "Post 94" refers to the year respondents turned 17 or 13. All models are weighted for sampling probability and have standard errors clustered by household. $***p<0.01$, $**p<0.05$, $*p<0.1$.

Table I.2: Differential Effects on Education: OBC Governments

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Panel A: Fathers with University Education				
Post 94	1.883***		1.861***	
	(0.144)		(0.138)	
Ed. Father	6.794***		6.642***	
	(0.270)		(0.232)	
Post 94*Ed. Father	-1.696***		-1.846***	
	(0.306)		(0.281)	
OBC	-1.853***		-1.884***	
	(0.152)		(0.136)	
Post 94*OBC	0.722***	0.541***	0.890***	0.883***
	(0.186)	(0.204)	(0.182)	(0.196)
Ed. Father*OBC	1.061*		1.179**	
	(0.590)		(0.512)	
Post 94*Ed. Father*OBC	-0.857	-0.341	-1.182**	-0.956
	(0.640)	(0.665)	(0.589)	(0.592)
OBC CM	0.246		0.978***	
	(0.222)		(0.196)	
OBC CM*OBC	-0.705***	0.360	-0.828***	-0.196
	(0.253)	(0.298)	(0.238)	(0.317)
Migrant*OBC	0.886*	0.743	0.884*	0.757*
	(0.458)	(0.455)	(0.457)	(0.456)
Migrant	-1.524***	-1.023***	-1.479***	-1.030***
	(0.384)	(0.371)	(0.381)	(0.372)
Observations	27,044	27,044	27,044	27,044
R-squared	0.143	0.238	0.156	0.239
Panel B: Fathers with Any Education				
Post 94	1.021***		0.898***	
	(0.191)		(0.199)	
Ed. Father	4.608***		4.555***	
	(0.196)		(0.174)	
Post 94*Ed. Father	0.398		0.516**	
	(0.249)		(0.252)	
OBC	-0.817***		-0.858***	
	(0.177)		(0.164)	
Post 94*OBC	0.499**	-1.369***	0.678**	0.630**
	(0.248)	(0.225)	(0.263)	(0.315)
Ed. Father*OBC	-1.398***		-1.321***	
	(0.274)		(0.239)	
Post 94*Ed. Father*OBC	0.401	3.268***	0.237	0.416
	(0.344)	(0.150)	(0.337)	(0.385)
OBC CM	0.263		0.741***	
	(0.176)		(0.157)	
OBC CM*OBC	-0.474**	0.396	-0.489**	0.0677
	(0.213)	(0.294)	(0.204)	(0.273)
Migrant*OBC	0.739*	1.017**	0.765*	0.695*
	(0.407)	(0.449)	(0.406)	(0.408)
Migrant	-0.913***	-1.042***	-0.903***	-0.600*
	(0.334)	(0.369)	(0.334)	(0.324)
Observations	27,044	27,044	27,044	27,044
R-squared	0.247	0.272	0.254	0.326
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with individual years of education as the dependent variable. The definition of educated fathers is listed in the column headings. "Post 94" refers to the year respondents turned 18. All models are weighted for sampling probability and have standard errors clustered by household. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table I.3: Differential Effects on Government Employment: OBC Governments

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
Panel A: Fathers with University Education				
Post 94	-0.107***		-0.0924***	
	(0.0162)		(0.0170)	
hEd. Father	0.277***		0.291***	
	(0.0569)		(0.0496)	
Post 94*Ed. Father	-0.0777		-0.141**	
	(0.0710)		(0.0687)	
OBC	-0.0858***		-0.0794***	
	(0.0153)		(0.0131)	
Post 94*OBC	0.0548***	0.0301	0.0460**	0.0198
	(0.0192)	(0.0216)	(0.0196)	(0.0206)
Ed. Father*OBC	0.0532		-0.0255	
	(0.121)		(0.0965)	
Post 94*Ed. Father*OBC	-0.0407	0.189	0.111	0.132
	(0.144)	(0.122)	(0.132)	(0.132)
OBC CM	-0.0236		0.00541	
	(0.0231)		(0.0262)	
OBC CM*OBC	-0.0149	0.0270	-0.0205	0.0341
	(0.0253)	(0.0321)	(0.0308)	(0.0365)
Migrant*OBC	0.0820***	0.0715***	0.0832***	0.0714***
	(0.0149)	(0.0189)	(0.0145)	(0.0188)
Migrant	-0.148***	-0.123***	-0.152***	-0.123***
	(0.0130)	(0.0169)	(0.0127)	(0.0168)
Observations	7,043	7,043	7,043	7,043
R-squared	0.067	0.271	0.060	0.270
Panel B: Fathers with Any Education				
Post 94	-0.0519***		-0.0614***	
	(0.0156)		(0.0146)	
Ed. Father	0.213***		0.182***	
	(0.0223)		(0.0201)	
Post 94*Ed. Father	-0.121***		-0.0850***	
	(0.0289)		(0.0297)	
OBC	-0.0413***		-0.0373***	
	(0.0143)		(0.0128)	
Post 94*OBC	0.0398**	0.0222	0.0345*	0.0348*
	(0.0193)	(0.0123)	(0.0183)	(0.0212)
Ed. Father*OBC	-0.0473		-0.0669**	
	(0.0314)		(0.0262)	
Post 94*Ed. Father*OBC	-0.0165	0.0228*	0.0155	-0.0270
	(0.0384)	(0.0223)	(0.0366)	(0.0390)
OBC CM	-0.0225		0.00175	
	(0.0207)		(0.0316)	
OBC CM*OBC	-0.00638	0.0267	-0.00860	0.0192
	(0.0230)	(0.0321)	(0.0355)	(0.0363)
Migrant*OBC	0.0576***	0.0732***	0.0614***	0.0542***
	(0.0138)	(0.0190)	(0.0137)	(0.0187)
Migrant	-0.120***	-0.124***	-0.127***	-0.0970***
	(0.0116)	(0.0169)	(0.0117)	(0.0167)
Observations	7,043	7,043	7,043	7,043
R-squared	0.090	0.271	0.075	0.287
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with government employment as the dependent variable. The definition of educated fathers is listed in the column headings. "Post 94" refers to the year respondents turned 18. All models are weighted for sampling probability and have standard errors clustered by household. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table I.4: Government Employment: OBC Governments

VARIABLES	(1) Middle Class	(2) Middle Class	(3) Middle Class	(4) Middle Class
OBC Caste	-0.0531***		-0.0568***	
	(0.0118)		(0.0103)	
Post-1994	-0.00394		0.0510***	
	(0.00927)		(0.0107)	
Mandal State			0.0226**	
			(0.00985)	
OBC*Post 94	0.0387***	0.0332**	0.0192	
	(0.0150)	(0.0151)	(0.0152)	
OBC Caste*Mandal State			-0.00530	
			(0.0149)	
Post 94*Mandal State			-0.0525***	
			(0.0141)	
OBC*Post 94*Mandal State			0.0132	0.00945
			(0.0212)	(0.0210)
OBC CM	-0.00163		-0.0377***	
	(0.0150)		(0.00987)	
OBC CM*OBC	-0.0429**	-0.0420*	0.0205	-0.0288*
	(0.0203)	(0.0238)	(0.0134)	(0.0171)
Migrant*OBC		-0.000857	0.0355***	0.00176
		(0.0202)	(0.0137)	(0.0147)
Migrant		-0.115***	-0.197***	-0.125***
		(0.0135)	(0.00930)	(0.0101)
Observations	22,334	22,334	44,455	44,455
R-squared	0.014	0.151	0.016	0.118
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with an indicator for government employment as the dependent variable. "Post 94" refers to the year respondents turned 17 or 13. All models are weighted for sampling probability and have standard errors clustered by household. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table I.5: Contacts: OBC Governments

VARIABLES	(1) 17	(2) 17	(3) 13	(4) 13
OBC Caste	-0.0730*** (0.0189)		-0.0646*** (0.0206)	
Post 1994	-0.0474** (0.0221)		-0.0336 (0.0232)	
OBC*Post 94	0.0718*** (0.0226)	0.0664** (0.0265)	0.0724*** (0.0222)	0.0599** (0.0235)
OBC Gov*OBC	-0.0335*** (0.00991)	0.0139 (0.0294)	0.00629 (0.0140)	0.0239 (0.0348)
Migrant*OBC	0.0739*** (0.0157)	0.0715*** (0.0192)	0.0716*** (0.0155)	0.0674*** (0.0191)
Migrant	-0.147*** (0.0135)	-0.140*** (0.0159)	-0.137*** (0.0133)	-0.133*** (0.0161)
Time Trend	-0.00426*** (0.00146)		-0.00406** (0.00171)	
OBC*Time Trend	-0.00257* (0.00138)	-0.00401** (0.00171)	-0.00293** (0.00141)	-0.00463*** (0.00171)
Observations	8,784	8,784	7,915	7,915
R-squared	0.047	0.174	0.037	0.154
Controls	YES	YES	YES	YES
State-Caste Category FE	NO	YES	NO	YES
State-Birth Year FE	NO	YES	NO	YES
Caste Category-Birth Year FE	NO	NO	NO	NO

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows the results of a series of linear regression models with a binary measure for whether someone in the household knows a doctor, teacher police inspector or gazetted government officer as the dependent variable. "Post 94" refers to the year respondents turned 17 or 13. All models are weighted for sampling probability and have standard errors clustered by household. $***p<0.01$, $**p<0.05$, $*p<0.1$.