Does Affirmative Action Worsen Bureaucratic Performance? Evidence from the Indian Administrative Service*

Rikhil R. Bhavnani† Alexander Lee‡

September 22, 2019

Forthcoming, *American Journal of Political Science*

Short Title: Does Affirmative Action Worsen Bureaucratic Performance?

Key Words: Bureaucracy, affirmative action, public goods, India

*We thank Francesca Jensenius, Tom Pepinsky, Daniel Rogger and Steven Wilkinson for comments on previous drafts, and participants at the Quality of Governance Conference at Stanford, the Annual American Political Science Association meetings and the Annual Conference on South Asia for discussions. We are grateful to Priyadarshi Amar, Ishwari Bhattarai and Jennifer Walky for excellent research assistance.

†Associate Professor, Department of Political Science, University of Wisconsin–Madison, 110 North Hall, 1050 Bascom Mall, Madison, WI 53706. Email: bhavnani@wisc.edu

‡Assistant Professor, Department of Political Science, University of Rochester, 333 Harkness Hall, Rochester, NY 14627. Email: alexander.mark.lee@rochester.edu

Does Affirmative Action Worsen Bureaucratic Performance?
Evidence from the Indian Administrative Service

Abstract

Although many countries recruit bureaucrats using affirmative action, the effect of affirmative action recruits on bureaucratic performance has rarely been examined. Some worry that affirmative action worsens bureaucratic performance by diminishing the quality of recruits, while others posit that it improves performance by making recruits more representative of and responsive to the population. We test for these possibilities using unusually detailed data on the recruitment, background and careers of India's elite bureaucracy. We examine the effect of affirmative action hires on district-level implementation of MGNREGA, the world's largest anti-poverty program. The data suggest that disadvantaged group members recruited via affirmative action perform no worse than others.

Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: http://dx.doi.org/XXX.

Word Count: 9,998

In many countries, some ethnic groups have lower levels of education, wealth, social connections, and political power than other groups due to discrimination or historical legacies of marginalization. To reverse these inequalities, many countries have implemented some form of affirmative action for marginalized groups, using quotas or more subtle positive discrimination mechanisms. A large literature has examined the effects of affirmative action in education (Bertrand, Hanna and Mullainathan, 2010; Arcidiacono, 2005), politics (Bhavnani, 2009, 2017; Chauchard, 2014; Dunning and Nilekani, 2013; Jensenius, 2017; Besley et al., 2004; Pande, 2003; Karekurve-Ramachandra and Lee, forthcoming) and the private sector (Griffin, 1992; Carter, Simkins and Simpson, 2003; Holzer and Neumark, 1999). However, these literatures have not examined the effects of affirmative action in government bureaucracies, despite the importance of bureaucracies in shaping welfare. Similarly, the flourishing literature on the role of bureaucrats in public service delivery in poor countries has not directly examined the effects of affirmative action policies, despite the commonness of these policies and the fierceness with which they are contested.

While affirmative action policies are intended to change the socioeconomic status of beneficiaries, they may also alter—and are frequently intended to alter—institutional performance. A prominent concern in the literature is that affirmative action might hurt bureaucratic efficacy by lowering the *quality* of personnel (Lewis, 1997; Johnson, 2015; Lott, 2000; Griffin, 1992). This concern is particularly relevant in bureaucracies with recruitment procedures that are thought to be meritocratic, since in these cases affirmative action recruits are by definition of lower formal quality than others. If correct, this would be a strong argument against affirmative action policies, showing that any gains to the target group are balanced by social losses. However, this claim has not gone uncontested, with some scholars holding that affirmative action may improve bureaucratic performance by making bureaucracies more *representative* of citizens (Meier and Nigro, 1976; Krislov, 2012). More representative bureaucracies might be more willing and able to serve underprivileged citizens, or simply more able to avoid the type of discrimination found in unrepresentative bureaucracies.

This paper will examine the effects of affirmative action in India, which has a powerful upper bureaucracy that recruits using affirmative action. India's elite bureaucracy, the Indian Administrative Service, is one of the world's most powerful, monopolizing the most important bureaucratic posts and supervising the implementation of anti-poverty programs vital to hundreds of millions. It is thus unsurprising that the personal traits and incentives of IAS officers have been shown to predict state and local policy outcomes (Bertrand et al., forthcoming; Bhavnani and Lee, 2018; Iyer and Mani, 2012). While IAS officers are selected through a fiercely competitive national exam, at least 50% of positions are reserved for members of three categories of traditionally marginalized groups whose low exam scores would otherwise disqualify them from office. Given the power and prestige of the bureaucracy, these quotas (and similar quotas for other positions in government) are one of the most electorally salient policies of the Indian state, and their effects are fiercely contested.

In considering the effects of affirmative action, scholars face two major research design challenges. The first is that the affirmative action "treatment" is a bundle of at least two things: affirmative action hires are both members of disadvantaged groups and have worse formal qualifications. Often, these effects are observed together, or are highly correlated: affirmative action increases the proportion of disadvantaged group members, but we do not know which (if any) of these individuals would have been recruited without affirmative action (Lewis, 1997). Sowell (2005, 174) goes as far as to claim that this aggregation makes most existing empirical work on affirmative action invalid, since it conflates the effects of affirmative action and minority status.

We address this problem by studying the IAS, to which disadvantaged group members are recruited both with and without affirmative action, and for which we have a rich new dataset. Our dataset, obtained using online sources and India's Right to Information (RTI) Act, includes detailed data on the origins, educational backgrounds and complete service histories of every IAS officer, as well as their caste category and exam scores. The latter two criteria determine whether and how—with or without affirmative action—candidates joined the IAS. We therefore know which candidates were recruited using affirmative action, and by how much they benefited. The context and data allow us to compare affirmative action recruits with others, and to compare affirmative action recruits with disadvantaged group members recruited without affirmative action.

The second research design problem is that of selection. Countries and institutions that adopt affirmative action differ from others, not least in their attitudes toward the marginalized. Even within a country or institution with affirmative action, quota candidates may be assigned to different tasks than others, because of personal choice or discriminatory attitudes. In the context of the IAS, this would mean that disadvantaged group members would be assigned to different, perhaps less desirable, areas than others.

To address this selection problem, we take three steps. First, all reported models contain two sets of fixed effects, one at the district level (to account for slow moving or time invariant confounds, such as institutional quality) and another at the state-year level (to account for policy changes and other political and economic shocks). Second, we include an extensive set of controls, for district-level time varying factors. Third, we employ an instrumental variables estimator, leveraging the fact that bureaucrats early in their careers are quasi-randomly assigned to districts. We show that while later in their careers the observable traits of bureaucrats are correlated with the observable traits of the districts they serve in, this is not true early in their careers. This fact allows us to instrument for the traits of officers with the traits of early-career officers, thereby yielding the local average treatment effect of swapping early-career affirmative action hires for early-career non-affirmative action hires in comparable districts.

As our main measure of bureaucratic output, we focus on the implementation of the world's largest anti-poverty program, the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA), although we also examine the effects of bureaucrats on the implementation of two other government programs. The primary purpose of the program is to reduce poverty by providing rural households with employment on public works as needed, and our main measure is the number of households that received the guaranteed 100 or more days of employment. Both our data and the existing literature show that there is considerable variation in employment provided under MGNREGA across district-years, some of it traceable to bureaucratic effort (Gulzar and Pasquale, 2017). District officers—whose influence we study—play a major role in the program's implemententation. They are tasked with "ensur[ing] wage-seekers are provided work as per their entitlements" as well as 19 other administrative responsibilities.[1]

To estimate the effects of affirmative action on bureaucratic output, we examine whether the assignment of affirmative action hires to districts changes MGNREGA outcomes in those districts. Since we estimate the marginal effect of replacing early-career affirmative action hires with non-affirmative action hires, our analysis does not speak to the question of what would happen if affirmative action in the IAS were scrapped altogether. We find that districts served by affirmative action recruits have similar levels of MGNREGA employment to other districts. The null effect of affirmative action suggests that fears about the detrimental effects of affirmative action on bureaucratic effectiveness, at least with regard to the world's largest welfare program, are unfounded. We find similar results when we estimate the effects of affirmative action hires on road construction, and time to approval of projects sponsored by legislators using their constituency development funds. This implies that the null effect of affirmative action on public goods are not specific to anti-poverty programs (which disproportionately benefit individuals from the caste categories that receive affirmative action), but also extend to the provision of goods preferred by the population as a whole and elites.

To explore the mechanisms behind the null estimated effect of affirmative action, we disaggregate the affirmative action treatment bundle into two components—disadvantaged group status and exam performance. We find a slight, statistically insignificant, negative association between MGNREGA implementation and officer exam rank, which is more than counterbalanced by a positive and statistically significant association between disadvantaged group identity and MGNREGA implementation. In other words, among officers with similar exam ranks, disadvantaged group officers perform better than others. This is consistent with Ferreira and Gyourko (2014) and Anzia and Berry (2011), who also find that seemingly "equally qualified" female politicians in the United States perform better than men. The fact that disadvantaged group IAS recruits perform

[1] http://nrega.nic.in/Circular_Archive/archive/Roles_responsibilites.pdf, accessed 4/24/2018. For documentation of the role of district officers in MGNREGA implementation, see Section A of the Supporting Information.

poorly on the interview portion of the recruitment exam, where it is relatively easy to guess caste identity, rather than the more objective written portions of the exam, points to the specific stage at which candidate quality is understated.

Our results suggest that, at least within selective bureaucracies like the IAS, improvements in diversity can be obtained without efficiency losses for some kinds of bureaucratic output. This finding allows us to reject the worst fears of affirmative action skeptics, namely that these programs inevitably worsen bureaucratic performance. While the wider social and political implications of bureaucratic affirmative action in India require further study, its institutional effects are not uniformly negative.

1 The Effects of Affirmative Action

The origins and performance of bureaucrats are widely thought have an influence on policy outcomes, particularly in developing countries (Gulzar and Pasquale, 2017). However, with a few exceptions (Lott, 2000; Deshpande and Weisskopf, 2014; Lewis, 1997), there has been little study of the effect of affirmative action in bureaucracies.

1.1 Affirmative Action Outside the Bureaucracy

The most common type of affirmative action program, and the most studied, is in admissions to educational institutions. Many studies have found that affirmative action has positive effects on beneficiaries, measured by earnings and educational outcomes (e.g. Arcidiacono, 2005; Lee, 2019*a*). Others have argued that gains for successful applicants are negated by losses to unsuccessful applicants from non-targeted groups (Bertrand, Hanna and Mullainathan, 2010).

Unlike educational quotas, quotas in elections are not primarily promoted as being beneficial for individuals, but to benefit the underrepresented group as a whole. Since some election quotas have been implemented quasi-randomly, we have a rich set of empirical findings on this issue. Some studies have found that affirmative action leads to improved provision of public goods for members of underrepresented groups (Besley et al., 2004; Pande, 2003), while others have found improvements in attitudes towards group members (Chauchard, 2014). Still others, by contrast, have found mixed or null effects, perhaps traceable to the strong incentives of politicians to serve those who voted for them, rather than members of their own group (Dunning and Nilekani, 2013; Jensenius, 2017; Bhavnani, 2017).

Perhaps the closest analog to bureaucratic affirmative action is the practice of affirmative action in corporations. A small literature examines the effects of increases in diversity among employees on firm performance (Griffin, 1992; Deshpande and Weisskopf, 2014; Holzer and Neumark, 1999). Many of these studies do not observe the effects of affirmative action independent of an increase in diversity, and therefore conflate the two. Other studies focus on between-firm differences in affirmative action policy, for example comparing firms that contract with the US government with those that do not (Griffin, 1992), though contractors may differ from other types of firms.

1.2 Negative Institutional Effects of Affirmative Action: Declines in Efficiency

Many worry that affirmative action worsens bureaucratic efficiency. The argument is straightforward. Without affirmative action, bureaucrats are recruited through a process that maximizes the quality of recruits, and recruit quality is assumed to be correlated with job performance. Affirmative action causes the overall quality of recruits to decline, since it relaxes recruitment standards in favor of disadvantaged group members. This leads to declines in institutional performance, and possibly social efficiency as well. Bolick (1996, 60), for instance, states that "Racial preferences ignore relative qualifications, leapfrogging less qualified people over better ones... Predictably, such deviations from the highest standards result in diminished efficiency and productivity." The argument that reservations hurt efficiency is widely made within the Indian media, with Shah (1991, 1732), for instance, arguing that "efficiency or merit is not a fetish of the elite, but an essential ingredient in every field of life... The policy of reservations for backward classes is a major barrier to achieving efficiency."

Evidence for the negative effects of affirmative action is mixed. Lott (2000) finds that more diverse police departments have poor performance. Marion (2009) finds that abolishing affirmative action among government contractors reduced overall costs, leading to efficiency gains. Lewis (1997) and Johnson (2015) find that minority US federal employees have poorer performance evaluations than white employees, though it is unclear if this reflects actual differences in performance. However, other studies, primarily in the private sector, find that while the formal qualifications of marginalized group hires are often lower, their performance is often just as good or better. Examples include American corporate employees (Holzer and Neumark, 1999) and Indian railway workers (Deshpande and Weisskopf, 2014). Consistent with this, Johnson (2015) finds that veterans hired into the US bureaucracy through preferential policies are promoted at a faster rate than others.

A potential reason for the mixed estimated effects of a reduction in employee "quality" due to affirmative action is that the techniques used by bureaucracies to measure quality are imperfect. Meritocratic recruitment exams, a hallmark of "Weberian" bureaucracies, may test academic prowess rather than honesty, commitment, social skills, or other factors that might be correlated with being a successful bureaucrat. Even more concerningly, scores might be correlated with the socioeconomic status of recruits (Jencks, 1998). If measured quality is weakly correlated with actual quality, there is less reason to expect that affirmative action will reduce performance.

The limited literature on bureaucracy in large organizations has focused on recruitment to entry-level positions. The subsequent promotion process might ameliorate any potential efficiency losses from affirmative action—to the extent that candidates are inefficient, they are less likely to rise to positions where their inefficiency can hurt the organization. However, organizations might also have quotas in promotion to higher positions, or use affirmative action to recruit disadvantaged group candidates directly to these positions, as on corporate boards. While we will not consider promotion or high-level hiring quotas in this study, we note that such quotas might have efficiency costs that are more severe than affirmative action in hiring.[2]

[2]That said, Carter, Simkins and Simpson (2003) finds a positive relationship between diversity on corporate boards.

1.3 Positive Institutional Effects of Affirmative Action: Ethnic Favoritism and Missmeasurment

The debate on the institutional effects of affirmative action is far from one-sided. Some scholars argue that affirmative action for administrative posts improves institutional performance, much as it has been claimed to do for elected officials. The most common form of this argument focuses on ethnic favoritism leading to changes in the *distribution* of services. Affirmative action might improve bureaucratic performance because recruits from marginalized groups might be more likely to serve members of their own groups effectively. This could lead to gains in provision for the marginalized group at the expense of the entrenched group (leading to gains in equity) or gains in provision for the marginalized group while the entrenched group's provision stays the same or improves (leading to gains in equity and efficiency). If affirmative action bureaucrats serve populations made up primarily of members of their own group, as in an anti-poverty program, this second outcome is likely to dominate—any improvement in distribution to the poor is likely to improve the overall performance of the program.

There are a variety of explanations for why members of marginalized groups could serve their own especially effectively. First, they may have a cognitive bias or preference towards members of their own group, a pattern well-attested in the distributional politics literature (Kramon and Posner, 2016). Second, they may lack the discriminatory attitudes possessed by members of the dominant group (Dee, 2005). Third, they may be exposed to social sanctions from members of their own group, creating an additional incentive not to shirk their responsibilities towards that group (Tsai, 2007). Fourth, they may have more information about their own group and its problems than other groups, enabling improved efficiency in administration (Kasara, 2007).

The existing literature on "representative bureaucracy," while not explicitly concerned with affirmative action, supports this hypothesis. These works find that bureaucracies that are similar to the population they serve perform better than other bureaucracies (Meier and Nigro, 1976; Krislov, 2012). The distributional argument is frequently given in the Indian context as a justification for reservations: The Mandal commission report (India, 1980, 57), for instance, argues that "Chances are that owing to [affirmative action candidates'] social and cultural handicaps they may be generally a shade less competent. But, on the other hand, they will have great advantage of possessing firsthand knowledge of the sufferings and problems of the backward sections of society. This is not a small asset for field workers and policy makers even at highest level." Note that this argument could easily stated in the opposite sense: members of disadvantaged groups could outperform members of other groups not because they favor their own group, but simply because they do not discriminate against their own.

Affirmative action programs could also improve institutional performance due to flaws in the recruitment process. In many cases, absent affirmative action, agencies will recruit bureaucrats from the powerful group who are of lower quality than some marginalized group applicants, because of discriminatory practices or because the measures used to assess quality are biased towards the powerful (Jencks, 1998). An alternative way of formulating this point is that since candidates from marginalized groups face unobserved selection effects due to discrimination, successful candidates from these groups are better qualified than candidates from other groups with similar formal qualifications (Ferreira and Gyourko, 2014; Anzia and Berry, 2011). If this is the case, affirmative action will raise the quality of recruits, and potentially lead to improved outcomes.

2 The Indian Case

2.1 Caste Quotas in India

Indian society is divided by a variety of politically relevant and frequently cross-cutting social cleavages, including religion, language, caste and class. Government policy has focused on rectifying inequalities across several of these cleavages, including caste. Hindus are divided into thousands of castes or *jatis*, which are endogamous groups, often with a common origin story and traditional occupation. Jati was traditionally a "ranked" identity, with each group being defined in part by its (usually contested) position in a religiously legitimated status ordering, with the "twice born" castes at the top and the "untouchable" castes at the bottom. Non-Hindus often belong to endogamous "communities" or tribes that are similar to caste groups, particularly insofar as membership in these communities is highly predictive of wealth and education.

For the purposes of affirmative action in the bureaucracy, people are grouped into three broad categories, with the classifications administered by national and state governments. The Scheduled Castes (SCs, dalits) are the formerly untouchable caste at the bottom of the status hierarchy, while the Scheduled Tribes (STs, adivasis) are the very poor aboriginal tribes of upland India. The Other Backward Classes (OBCs) are a heterogeneous collection of groups with a higher traditional status than SCs and STs, but with some degree of social disadvantage (Lee, 2019*b*).

Caste-based affirmative action has been a contentious topic since before independence. The post-independence constitution guaranteed SCs and STs positions proportional to their population in legislatures, the bureaucracy and public sector education. Reservations for OBCs in the bureaucracy and education were instituted at the national level in 1994, after lengthy court battles and protests that included upper caste students immolating themselves. Many aspects of India's reservations policy—including the precise groups that they cover, the proportion of positions that are set aside for disadvantaged group members, and whether reservations should cover promotions in addition to recruitment—remain controversial.

2.2 The Indian Administrative Service

The Indian Administrative Service is the most powerful group of civil servants in the country, the successor of the colonial Indian Civil Service. The IAS is an elite organization, supervising the work of "subordinate" civil services. Not only does the IAS monopolize all senior posts, but the most junior IAS officers hold positions that members of the subordinate services hold at the end of their careers. Serving as an IAS officer is widely regarded as prestigious, with many material benefits.

Recruitment to the IAS and other central (that is, federal) services is via the three-stage Central

Services Examination, administered by the Union Public Service Commission (UPSC).[3] All college graduates between the ages of 21 and 32 are eligible, although the upper age limit is higher for certain castes. Around 400,000 people a year take the multiple choice preliminary exam, of whom the top 7,500 are invited to take the main exam. This main exam is primarily a series of essay questions, drawing on a mix of mandatory questions (on history, reasoning and general knowledge of current affairs) and optional subjects. Lastly, there is a personal interview and "qualifying" questions on language proficiency. The examiners who mark the written sections do not know the name or caste of the candidates, but the committee of generally upper caste civil servants who conduct the interview are in a position to learn more personal details about the candidate. An extensive coaching industry has built up around the exam, which many students study for for years and take multiple times. Students are ranked based on the sum of their scores on the written and interview portions of the assessment, and individuals are allowed to choose their service in rank order, until all openings are filled. Almost all top recruits choose the IAS, while others opt for bureaucracies such as the Indian Foreign Service.

While the IAS is recruited and paid by the central government, its officers spend much of their careers serving in state government. At the beginning of their careers, IAS officers are assigned to the "cadre" of a particular state through a complicated process designed to ensure a mix of "local" and outside officers and an even distribution of talent across the states (Iyer and Mani, 2012). Bertrand et al. (forthcoming) shows that state assignment is orthogonal to all observable attributes of officers, including caste and exam rank.

The fundamental unit of administration in India is the district, of which there are several hundred. The head of the district administration—called the district officer, district magistrate, district collector or deputy commissioner—is usually a junior IAS official, though state civil service officers also hold these positions. The district officer has many subordinates with titles such as subdivisional magistrate and district development officer, some of whom are also IAS officers at the very beginning of their careers. However, 70% of the district-years in our data have only one

[3] A smaller group is drawn into the IAS without taking the exam, from mid-career officers of the subordinate civil services.

IAS officer. The district administration has a very broad set of responsibilities, including the implementation and coordination of virtually all government programs and the supervision of local elections. For this reason, district officers are generally well known, their relative honesty and efficiency is discussed (Bertrand et al., forthcoming), and citizens and politicians go to great lengths to influence IAS officers (Iyer and Mani, 2012). Personal traits of IAS officers, such as their origin (Bhavnani and Lee, 2018), perceived competence (Bertrand et al., forthcoming) and tenure in office (Iyer and Mani, 2012), have been shown to be correlated with policy outcomes.

Civil servants are assigned to districts by the highest ranking civil servant in each state. Early in their careers, such assignments to districts are arbitrary. In some cases, assignments to districts are verifiably quasi-random. Later in their careers, civil servants are assigned to districts by a complex and opaque process. These later assignments are driven by efficiency concerns, but are also influenced by IAS officers (since some postings are more desirable than others) and politicians (who wish to reward loyal officers and place them in strategic posts; Iyer and Mani 2012). The assignments of officers in the first years of their careers are less subject to these pressures, both because officers are less known to politicians, and because officers are sufficiently uninfluential that must go where they are sent (often to undesirable locations). We return to this issue later.

2.3 Caste in IAS Recruitment

Each year, the Ministry of Personnel announces the number of vacancies in the IAS. These vary from year to year but have grown over time, from 74 in 1995 to 176 in 2014. Each year, the allocation of positions across caste categories is in proportion to the population: 50.5% of seats are open to the highest ranked recruits regardless of background, 27% of seats are reserved or set aside for OBCs,[4] 15% for SCs and 7.5% for STs.[5]

The limited number of openings means that below an exam rank cutoff that varies by year individuals can no longer choose the IAS. Further, since open or "general" seats are filled first,

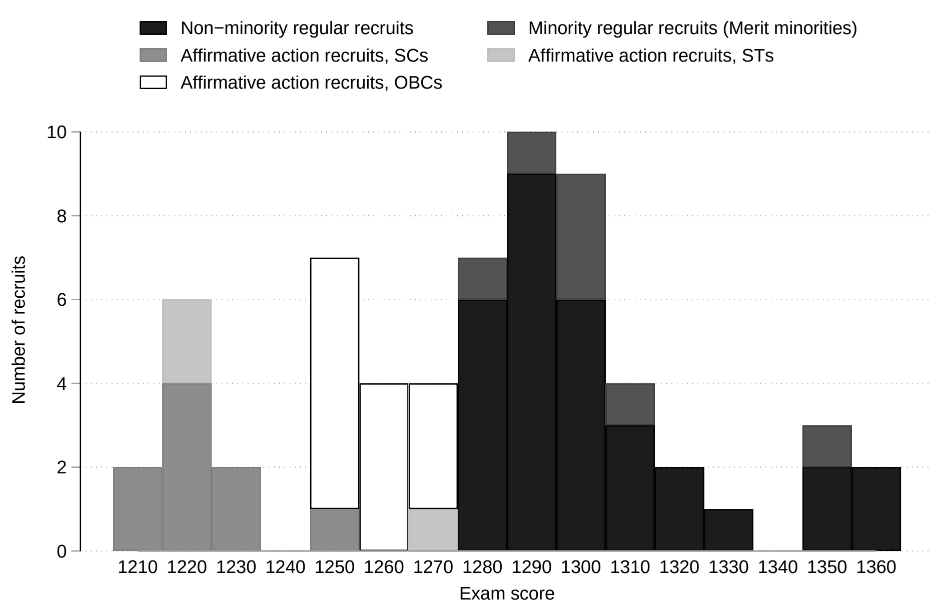
[4] In theory, children of high income households cannot take advantage of the OBC quota, though this rule is widely evaded.

[5] Within each category, 3% of seats are reserved for people with physical disabilities.

and since a disproportionate number of high scorers are not SC, ST or OBC, this cutoff varies by reservation category. In 2014, general candidates had to be ranked 95th and above to get assigned to the IAS, OBC candidates 466th and above, SC candidates 650th and above, and ST candidates 773rd and above. All the candidates who were hired with a rank below 95 were thus beneficiaries of affirmative action, since they would not have been hired had they been members of a different caste category.

Disadvantaged group members that score above the general cutoff are counted towards the general quota rather than their own caste category.[6] In the years since 1995, 22% of disadvantaged group recruits (13% of all recruits, 81% of whom were OBCs) scored above the general cutoff, and thus met the qualifications expected of non-affirmative action candidates. To help clarify these patterns, Figure 1 uses a stacked bar graph to map the distribution of exam scores of the 64 recruits to the IAS in 2005.

Figure 1: Exam scores and caste category for IAS recruits in 2005



Notes: This stacked bar graph shows the distribution of exam scores of the 64 recruits to the IAS in 2005. For example, it shows that three "regular recruits" scored between 1350 and 1359 on the entrance exam, and that one of these candidates was a "merit minority."

[6]A few of these candidates benefited from other forms of positive discrimination, including a relaxation of the maximum age to take the exam, or an increase in the number of attempts allowed. We return to this issue below.

Given the large number of people that take the IAS exams, even the "low" scorers among those selected through the UPSC exam are highly qualified people relative to the country as a whole. If the difference in quality among the top scorers only corresponds to small differences in real ability, there is little reason to think that the recruitment of low scorers through affirmative action should lead to efficiency losses. However, Bertrand et al. (forthcoming, Table 2) find a positive and statistically significant association between achievement on the exam and perceived performance in office. Note also that even if underlying quality were similar between high and low scorers, this would not effect the internal validity of the estimates we present, only our ability to generalize to other bureaucracies.

3 Research Design and Data

To assess the impact of affirmative action, it is necessary to link the biographical details of IAS officers to the districts in which they served, and then to district-level outcomes. We obtained the assignment histories of IAS officers, along with a set of officer-level controls, by scraping a Government of India website with the biodata and work histories of all IAS officers.[7] To code IAS officers' caste, exam rank, and whether they were recruited via affirmative action, we supplemented this with data from another government website,[8] Right to Information requests and repeat visits to government offices. The resulting database has the caste, exam rank and recruitment method for all IAS officers serving in districts, with the exception of a few officers recruited in the early 1990s.

3.1 Measuring Outcomes

As the senior administrators in districts, IAS officers implement a wide variety of programs. We focus on India's and the world's largest welfare program, in terms of the number of people served: the Mahatma Gandhi National Rural Guaranteed Employment Scheme (MGNREGA). First im-

[7] `https://supremo.nic.in/knowyourofficerIAs.aspx`, accessed 2/27/2017.

[8] `http://persmin.gov.in/ais1/QryCA.asp`, accessed 10/26/2016.

plemented in 2006, MGNREGA guarantees one member of each household at least 100 days of employment on small-scale public works projects, aiming to serve as an income floor for rural dwellers. Since MGNREGA is a national program funded by the central government, program goals do not differ across districts, and consistent data is centrally available. Variation in program outcomes therefore reflects the preferences of state governments,[9] who are responsible for the implementation of the program, and the effects of bureaucrats as well. As Dutta et al. (2014) note, while employment is formally guaranteed, there is substantial unmet demand for employment, and this unmet demand constitutes the main limitation of the program. While there is substantial "leakage" from the MGNREGA program, Dutta et al. (2014, 145) estimate that 80% of wage payments in the program are paid to recipients.

The bureaucracy serves as MGNREGA's central coordinating and permission giving body, and senior bureaucrats carefully monitor program implementation. Bureaucrats must issue job cards to eligible individuals, organize projects for them to work on, measure worker attendance and project completion, and arrange payment. An official list of MGNREGA responsibilities lists 20 tasks that should be performed by the District Program Coordinator (usually the district officer, occasionally another senior bureaucrat), including duties to "Ensure wage-seekers are provided work" "accord timely sanction to shelf of projects," and "ensure timely release and utilization of funds." Most importantly for our purposes, the district officer must prepare the labor budget for each year, which determines the number of people who can be employed. There is significant variation in the enthusiasm of IAS officers for this task: one evaluation report notes that "the District Magistrate, the senior-most bureaucrat of the district, has significant control over the quantity and quality of the MD [inspection] visits, so coverage is likely higher when MD is a priority for the DM." Fuller quotations from these and other sources on the role of the district officer in MGNREGA implementation are given in the Supporting Information.

MGNREGA implementation thus represents a good test of bureaucratic output, and recent studies on the Indian bureaucracy have used it for this purpose (Gulzar and Pasquale, 2017). Naturally,

[9] In the analysis that follows, we control for this variation using state-year fixed effects.

political and social factors also play a role in determining MGNREGA supply and demand. We discuss our strategy for addressing these confounding district-level effects below.

Our dependent variable is the log number of households that received 100 or more days of employment under MGNREGA, normalized to have mean 0 and standard deviation 1.[10] We use this outcome measure (rather than say the number of man-days of employment, which we use in a robustness test) due to data availability and since MGNREGA guarantees at least 100 days of employment for each household. The data are observed at the district-year level, and cover 2009–2016.[11] The raw data are from MGNREGA Public Data Portal.[12] Table A7 in the Supporting Information (SI) summarizes the data.

To see if our findings generalize beyond MGNREGA, we also examine the effects of bureaucrats on the number of villages newly connected under a road building program, and on the time to approval of projects sponsored by legislators using their constituency development funds. We refrain from analyzing subjective measures of bureaucratic performance (similar to Bertrand et al. forthcoming), since they might reflect caste stereotypes.

3.2 Estimating the Effects of Affirmative Action

To examine the effects of affirmative action on bureaucratic performance, we start by estimating the following equation:

$$Y_{it} = \alpha + \beta AA_{it} + \gamma \mathbf{X_{it}} + \delta_i + \theta_{st} + \varepsilon_{it} \tag{1}$$

This equation models our measure of bureaucratic output (Y) in district-years (districts are indexed by i; years by t) as a function of the proportion of affirmative action recruits (AA) that served in district-years. The control set, $\mathbf{X}$, is composed of measures of whether districts experienced positive or negative rainfall shocks and a set of political controls—the proportion of state legislators from the Congress, the BJP, the state's governing party, and elected constituencies reserved for

[10]We take the log transformation of our outcome since it is right-skewed. We standardize the variable to aid with interpretation.

[11]The data start in 2009 since this is the first year that all of India's districts were eligible for MGNREGA funds.

[12]http://mnregaweb4.nic.in/netnrega/dynamic2/dynamicreport_new4.aspx, accessed 12/14/2017.

Scheduled Castes and Tribes. These variables are intended to capture variation in the incentive of politicians to deliver resources to districts. To control for district-level unobservables, we include district fixed effects (δ)—78% of variance in MGNREGA employment is across districts, rather than within them. We control for time-varying unobservables at the state level using state-year fixed effects (θ; states are indexed by s). Since the estimation strategy employs district and state-year fixed effects, β is the estimated effect of substituting non-affirmative action IAS officers for affirmative action recruits, controlling for bureaucrat, district and state-year confounds.

A potential problem with this specification is that the treatment (AA) is likely endogenous to the outcomes. First, omitted and unobservable variables such as the time-varying attractiveness of districts could affect both bureaucrat assignment and outcomes. A second potential problem is reverse causality, as affirmative action recruits might be deliberately assigned to places with poor welfare provisioning. Although equation 1 begins to address these issues through the use of controls and a demanding set of fixed effects, potential bias in the estimated effects of affirmative action remains.

To address the possible endogeneity in the assignment of affirmative action recruits, we leverage the fact that IAS officers early in their careers are quasi-randomly assigned to districts within states (the process by which bureaucrats are assigned to states is controlled for using state-year fixed effects). Although the precise mechanism by which district assignments are made vary by state and are opaque, Bhavnani and Lee (2018) document the quasi-random assignment of bureaucrats to districts in four large states (Andhra Pradesh, Karnataka, Rajasthan and Uttar Pradesh), covering 24% of our sample. For example, IAS officers in Andhra Pradesh in 2013 were "assigned in alphabetical order of their names to districts that were ordered based on their serial number" and further that such serial numbers were "assigned based on the district's geographical position in the state proceeding clockwise" (Bhavnani and Lee, 2018, 78).[13]

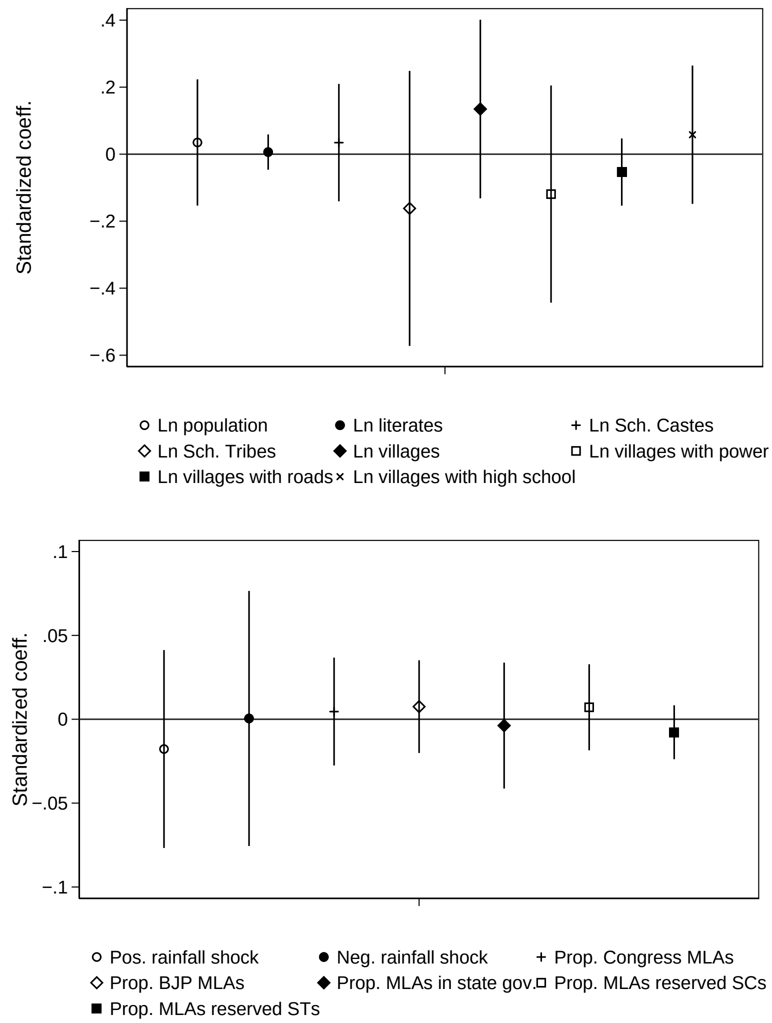
That the district assignments of early-career bureaucrats are quasi-random is consistent with our fieldwork and the logic of the assignment process. District assignments are made by the state

[13]Since most small and medium-sized states only have a handful of officers assigned each year, inspection cannot confirm whether their assignment procedures use the patterns described above.

Chief Secretary (a senior IAS officer) in consultation with the Chief Minister (a politician), frequently using rules-of-thumb such as those described above. Although district assignments might theoretically be influenced by politicians or the IAS officers themselves, the observers that we have spoken with have been skeptical that such efforts would be made or be successful. This is because early-career bureaucrats are unfamiliar to the senior bureaucrats and politicians who control their assignments, and because they have not built up the links to these figures that they will later acquire. So while a Chief Minister or Chief Secretary may wish to assign an early-career officer strategically, they do not know enough about officers to do this. During our fieldwork, officers emphasized the quickness and importance of this type of information gathering—"they judge a man's character when he joins the service. Two, three postings, and they have him marked forever" (IPS Officer F Interview, Patna, 11/16/2017). At the same time, officers strive for more desirable postings. However, early-career officers generally do not have the network to make such requests stick, and in fact there is much less variation in the desirability of posts early in officers' careers than later. Consistent with this account, an official statement of posting policies suggests that officers might have a choice in postings only after their initial assignments (Ministry of Home Affairs, 2010).

The process by which district assignments are made mean that bureaucrats' early assignments—which we define as those in the first five years of service, although our results are robust to using four years as the cutoff—are orthogonal to possible confounds. We are able to verify this claim with regard to observables in SI Tables A8 and A9. These show that the proportion of early-career affirmative action recruits are orthogonal to district characteristics (population, literacy, the presence of disadvantaged group members, the number of villages, and the number of villages with power, roads and high schools) and the time-varying characteristics of districts (whether districts experienced positive or negative rainfall shocks, and the proportion of state legislators from the Congress, the BJP, the state's governing party, and from constituencies reserved for Scheduled Castes and Tribes). The results of these 15 balance tests are summarized in Figure 2. We are unable to reject a joint test of the significance of these possible confounds. Nonetheless, to improve the precision of our estimates, we control for all these variables.

Figure 2: Balance tests for the proportion of early-career affirmative action recruits (the instrument)



Notes: The plots show the estimated "effects" of the instrument on possible confounds. All outcomes are standardized to have mean 0 and standard deviation 1. Full regression results are reported in SI Tables A8 and A9.

An examination of the average job assignment lengths of affirmative action and other hires illustrates both why our instrument is valid and why it is necessary (SI Table A14). Although affirmative action hires have longer postings (regression 1), the assignment lengths of affirmative action recruits early in their careers are the same as that of others (regression 2).

The quasi-random initial assignment of bureaucrats to districts allows us to instrument our key independent variable—the proportion of affirmative action recruits (AA)—with the proportion of early-career affirmative action recruits (Z). This first stage regression may be written as:

$$AA_{it} = \kappa + \lambda Z_{it} + \mu \mathbf{X_{it}} + \nu_i + \xi_{st} + \varepsilon_{it} \tag{2}$$

As discussed above, we have theoretical and empirical reasons to believe that initial assignments and therefore Z are quasi-random. Also, Z and AA are certain to be correlated since AA is a function of the instrument. SI Figure A3 shows that the instrument and endogenous term are indeed correlated ($\rho = 0.6$).

An alternative method of estimating the effect of affirmative action is to use a discontinuity analysis to compare general and affirmative action officers who scored very close to the exam cutoff: while these two groups have different caste identities, they should be similar in terms of whatever skill the exam is capturing.[14] This approach allows us to recover another estimate of the effects of affirmative action recruits, namely the effect of replacing a relatively highly ranked affirmative action hire with a comparably ranked non-affirmative action hire.

4 Results

To examine the effects of affirmative action recruits on the number of households that received at least 100 days of MGNREGA employment, we start by examining the simple bivariate relationship between the two variables using OLS (Table 1, regression 1; full results are in SI Table A15). Contrary to concerns that affirmative action recruits perform worse than others, the bivariate regression suggests a positive but statistically insignificant relationship between affirmative action recruits and MGNREGA provisioning.

In regression 2, we control for the potential time-varying confounds that we checked for balance on previously. In regression 3, we add fixed effects for administrative districts and state-years. These control for unobservables that vary by district (such a levels of poverty) and those that vary

[14]A standard regression discontinuity analysis is not possible since the forcing variable (relative exam rank) does not exclusively determine the treatment (that is, affirmative action). Since only disadvantaged group members with below-cutoff exam ranks can be recruited, assignment to the treatment is determined by both relative exam rank and bureaucrat identity.

by state-years (such as political support for MGNREGA). The controlled correlation between affirmative action and MGNREGA performance remains positive and statistically insignificant.

To better rule out endogeneity concerns, including the specific concern that affirmative action recruits are posted to areas where MGNREGA performance is poor, we switch to using the 2SLS estimator described previously. The first column of regression 4 displays the first stage results, and confirms that the instrument (the proportion of affirmative action recruits in the first five years of their careers) is indeed positively related to the proportion of affirmative action recruits, while the first-stage F-statistic is well above 10, which is the rule-of-thumb for a strong instrument.

The second stage estimate of the effects of affirmative action recruits on MGNREGA delivery remains positive and statistically and substantively insignificant. The point estimate suggests that increasing the proportion of affirmative action bureaucrats by a standard deviation (0.42) increases the log households that receive at least 100 days of employment under MGNREGA by 0.013 standard deviations. At the mean, this is the equivalent to increasing the number of households that received at least 100 days of employment under MGNREGA by 60 or 2%.[15] The 95% confidence interval for the estimated effect of a one standard deviation increase in the proportion of affirmative action bureaucrats is narrow $(-.04, .06)$, allowing us to rule out costs to MGNREGA implementation larger than one-twentieth of a standard deviation. In short, and contrary to the concerns of critics, affirmative action recruits perform no worse than regular recruits.

4.1 Robustness Tests

In the Supporting Information, we examine the robustness of the findings to a variety of alternative approaches, including focusing only on senior district officers, using post-treatment controls and district-specific time trends, not using district fixed effects, not using districts with multiple IAS officers, and estimating the effect of affirmative action using a discontinuity analysis. None of these alternatives produces substantially different results.

[15] The mean of the log households that received 100 or more days of MGNREGA employment is 7.94 (2,807 households), with a standard deviation of 1.66. An increase of 0.013 standard deviations (.42x.03) would raise the mean to 7.96 (2,867 households).

Table 1: The effects of affirmative action bureaucrats on MGNREGA implementation

Estimator:	OLS	OLS	OLS	2SLS	
Equation:				1st stage	2nd stage
	1	2	3	4	
Prop. affirmative action bureaucrats	0.11	0.10	0.03		0.03
	[0.08]	[0.08]	[0.05]		[0.06]
Prop. early-career officers recruited under AA				0.66***	
				[0.03]	
Controls?	N	Y	Y	Y	Y
State-year fixed effects?	N	N	Y	Y	Y
District fixed effects?	N	N	Y	Y	Y
Observations	2,047	2,047	2,047	2,047	2,047
Adjusted R-squared	0.00	0.09	0.88		0.88
F-statistic for AA bureaucrats					368

Notes: The dependent variable is the logarithm of households that received 100 days or more of employment under MGNREGA, standardized to have mean 0 and standard deviation 1. Controls are dummies for whether districts experienced positive or negative rainfall shocks, and the proportion of state legislators from the Congress, the BJP, the state's governing party, and from constituencies reserved for SCs and STs. Standard errors are clustered by district. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Importantly, we are able to confirm that the null effects of affirmative action obtain with regard to two other major government programs. One of these is the government's premier road building program, the Pradhan Mantri Gram Sadak Yojana (PMGSY). Roads arguably benefit a broader set of beneficiaries than MGNREGA. Since 2000, the government has spent over $40 billion under the PMGSY to connect isolated villages to the country's road network (Asher and Novosad, forthcoming). Road building is a complex process, and better bureaucrats are able to push road construction through the planning, contracting and construction phases. This outcome is a noisier measure of bureaucratic performance than MGNREGA since although every household is guaranteed 100 days of employment if they demand it (and state governments are guaranteed money to provide it to them), each village is not guaranteed a road. In SI Table A2, we examine the effects of affirmative action recruits on the log number of villages connected under PMGSY standardized to have mean 0 and standard deviation 1. Across all specifications (see the SI for a detailed discussion), affirmative action recruits appear to have no substantive or statistically significant effects on road building.

As an alternative, we also examine the effects of affirmative action on the time taken to approve projects proposed by national legislators using the Members of Parliament Local Area Development Scheme (MPLADS). Under MPLADS, India's MPs have a small annual budget (over $500,000) to propose and fund public works projects, which are implemented by the bureaucracy. Each project, which might be a road, a well, a school, etc., must be approved by the district officer, and while this process is supposedly automatic, there is wide variation in the time taken to authorization. The time taken to approve proposals is a relatively direct measure of bureaucratic responsiveness, and MPLADS projects arguably cater more to political elites (on this point, see Bohlken 2018) than MGNREGA and PMGSY projects. In SI Table A3, we examine the effects of affirmative action recruits on project approvals. Across a number of specifications (see the SI for a detailed discussion), affirmative action recruits fail to affect the time taken to approve projects.

4.2 Mechanisms I: Why No Effect of Affirmative Action Hires?

We next turn to a more speculative discussion of the causes of the null effect of affirmative action. Overall, affirmative action recruits do not affect MGNREGA provisioning. To help understand this result, we disaggregate the treatment variable, that is, the proportion of affirmative action recruits. We do so by adding a control for the proportion of disadvantaged group recruits (Table 2, regression 1; full results in SI Table A16). Recall that while approximately four-fifths of disadvantaged group recruits are recruited via affirmative action, the rest are not. Following our treatment of the proportion of affirmative action recruits, we instrument for the proportion of disadvantaged group recruits with the proportion of early-career disadvantaged group recruits. The regression results suggest that disadvantaged group recruits not recruited via affirmative action slightly improve MGNREGA performance. The effect is 0.1 standard deviations in size, and is positive and statistically significant at the 10% level. However, and as in the previous models, recruitment via affirmative action is associated with a small and statistically insignificant improvement in MGNREGA performance (in this specification, the effect of affirmative action recruits is given by the sum of the first two regression coefficients). To summarize, while disadvantaged group officers are associated with some improved performance, this effect is smaller and is statistically indistinguishable from 0 for those recruited using affirmative action.

Why do disadvantaged group members recruited via affirmative action perform worse than "merit" disadvantaged group members? Could the poorer exam scores of affirmative action recruits help explain their relatively poorer performance? To get at this, we replace our measure for affirmative action recruits with the mean log exam rank of recruits (regression 2). Following our treatment of the proportion of affirmative action recruits, we instrument for the mean log exam rank of recruits with the mean log exam rank of early-career recruits. As expected, the regression suggests that although disadvantaged group recruits somewhat boost MGNREGA performance ($p = 0.06$), poor exam performance has the opposite effect, though this latter effect is not statistically significant. That said, the point estimates suggest that the positive effects of disadvantaged

Table 2: Mechanisms for the effects of affirmative action bureaucrats on MGNREGA implementation

Dependent variables:	HHs that recd. 100+ days	HHs that recd. 100+ days	Ln person-days recd. by SCs/STs	Prop. spent on materials	HHs that recd. 100+ days
	1	2	3	4	5
Prop. affirmative action bureaucrats	-0.04		0.01	0.08	
	[0.07]		[0.07]	[0.12]	
Prop. disadvantaged group bureaucrats	0.10^{*}	0.11^{*}		-0.05	
	[0.06]	[0.06]		[0.12]	
Bureaucrats' ln exam rank		-0.02			
		[0.02]			
Prop. SC/ST bureaucrats			0.09		
			[0.07]		
Prop. other disadvantaged group bureaucrats			0.08		
			[0.06]		
All affirmative action bureaucrats?					0.04
					[0.07]
Some affirmative action bureaucrats?					-0.03
					[0.19]
Controls?	Y	Y	Y	Y	Y
State-year fixed effects?	Y	Y	Y	Y	Y
District fixed effects?	Y	Y	Y	Y	Y
Observations	2,047	2,047	2,024	1,532	2,047
Adjusted R-squared	0.88	0.88	0.94	0.76	0.88
F-statistic for AA bureaucrats	186		136	121	
F-statistic for disadvantaged group bureaucrats	234	224		124	
F-statistic for exam rank		106			
F-statistic for SC/ST bureaucrats			140		
F-statistic for other disadvantaged group bureaucrats			117		
F-statistic for all AA bureaucrats?					90
F-statistic for some AA bureaucrats?					6

Notes: Controls are dummies for whether districts experienced positive or negative rainfall shocks and the proportion of state legislators from the Congress, the BJP, the state's governing party, and from constituencies reserved for Scheduled Castes and Tribes. Standard errors are clustered by district. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

group bureaucrats are neutralized by recruits with exam ranks 83 and higher. In our data, all but 37 (of 434) affirmative action recruits had exam ranks greater than or equal to 83. Our finding about the positive effects of disadvantaged group bureaucrats also extends to the MPLADS data, where disadvantaged group officers are associated with significantly higher levels of on time approval of MPLADS funds (SI Table A3).

4.3 Mechanisms II: Why Might the Merit Disadvantaged Perform Better?

Table 2 showed that holding exam performance constant, disadvantaged group officers recruited without affirmative action ("the merit disadvantaged") were associated with slightly better outcomes than others. In this section, we discuss several possible reasons for this finding.

One idea advanced in the literature is that disadvantaged group members perform better than others because they tend to channel resources to co-ethnics who would ordinarily not receive resources from the bureaucracy, leading to a higher overall levels of provisioning. To test this "representative bureaucracy" hypothesis, we specify the log person-days of MGNREGA employment received by SCs/STs as the dependent variable,[16] and examine whether SC/ST bureaucrats in particular positively influence this outcome. Regression 3 does not suggest that this is the case.

A second possibility that we are also unable to confirm is that disadvantaged group members increase MGNREGA disbursements to generate rents for themselves. To test for this, we employ our standard 2SLS set up with expenditures on materials as a proportion of MGNREGA expenses as the dependent variable. Materials expenditures are thought to proxy for corruption, since it is arguably easier to steal from expenditures on materials, such as for sand for road building, than from people's wages. Regression 4 does not support this account: the proportion of expenses on materials is unaffected by disadvantaged group officers.

A third possibility that we test for is that merit disadvantaged group members improve MGNREGA performance through diversifying the group of IAS officers. To test for this, we use binary variables capturing if all the IAS officer(s) in a district are affirmative action hires, or if some of

[16] The state does not track person-days received by OBCs.

the IAS officers in a district are affirmative action hires, implying greater diversity. Regression 5 fails to suggest that districts with more diverse leaderships perform better than others. Note that this finding is in many ways unsurprising given the small numbers of IAS officers in each district and their hierarchical organization.

Disadvantaged group members arguably overcome greater hurdles than others, and might therefore be of higher quality (Ferreira and Gyourko, 2014; Anzia and Berry, 2011). In our context, if the UPSC exam is biased against or especially difficult for disadvantaged group members, successful members of these groups might have higher unobserved abilities than others. To explore this possibility, we employ unusual detailed data on officers' scores on different parts of the UPSC exam.[17] Recall that the UPSC exam has written parts, for a maximum of 2,000 points in the period studied, and an in-person oral interview or "personality test," for a maximum of 300 points. While the written parts of the exam are relatively objective and anonymous, the in-person interview is subjective, is not anonymous, and is conducted in English by a largely upper caste board. One former chair of the commission was frank about the biases this introduces: "A candidate from a rural background and educated at a small place finds it difficult to compete in communication skills before the interview panel with those who are from cities, and have been educated in a better atmosphere."[18]

In SI Table A17, we specify the subjective interview score as the dependent variable and examine its correlates. These regressions suggest that both merit and affirmative action disadvantaged group members perform worse than others on the subjective portion of the exam, while controlling for their performance on the written portion of the test.[19] Put differently, low caste individuals, recruited with and without affirmative action, both score worse on the interview portion of the test than do than others with identical scores on the written section of the test. This implies that elim-

[17] These data are only available for exam years after 2004. We exclude exam years after 2013, when a major change in test format took place.

[18] https://indianexpress.com/article/explained/upsc-civil-services-exam-india-2880015/, accessed 5/27/2019.

[19] Regression 8 suggests that merit disadvantaged group members that received the same written score as others received 5.2 fewer points on the interview. This is a very large effect, insofar as in all the years that we have data for, 1–2 points separate candidates who make it into the IAS from those do not.

inating the interview would in general raise the ranking of lower caste candidates, and could lead to more lower caste candidates being recruited through the general quota.

5 Conclusion

The main finding of this paper, that the performance of bureaucrats hired through affirmative action is similar to those who were not, is striking within the context of the polemical debate on affirmative action. In this debate, strong claims are often made for the negative effects of affirmative action. We find that reservations have neither led to hiring of officers unable to perform their jobs nor led to a dramatic improvement in institutional output, at least for one important government program. We cannot comment on the effects of quotas in promotions (which might have quite different effects), or the effect of quotas on the honesty of bureaucrats.

An exploration of the mechanisms behind the null effect of affirmative action suggests it might mask two opposing effects. Disadvantaged group officers recruited without affirmative action are associated with somewhat higher levels of MGNREGA provision, possibly since they are of higher quality than are others. This effect is somewhat counterbalanced by lower performance among officers with lower exam ranks, though the negative effect of exam rank is not in itself statistically significant. These findings indicate that one of the major theoretical predictions in the existing literature—the positive effect of underprivileged group representation, holding quality constant—is plausible, though it might stem from differences in quality rather than ethnic favoritism. This type of advantage might be especially plausible in cases—like the interview stage of the UPSC exam—where assessment is subjective and/or assessors are able to infer the background of the candidates.

Our findings underline the fact that affirmative action is a composite intervention, one that changes several aspects of personnel recruitment. As a result, the effects of affirmative action might vary by context. When the quality difference between the affirmative action and other hires is small, affirmative action may be associated with improvements in bureaucratic effectiveness.

When the difference is large, these gains may be attenuated or negative, depending on the context. Similarly, the relevance of the assessment procedure will influence the net effects of affirmative action. If the qualification demanded is not meaningful, or simply measures cheating or test-taking skill, hiring less qualified candidates will not necessarily be costly. If the qualification is biased against members of the disadvantaged group, hiring less qualified candidates may actually have benefits.

The results presented here do not exhaust the potential effects of affirmative action recruits. They do not, for instance, speak to the socio-economic impact of affirmative action on underprivileged communities, the psychological impact of placing members of previously underprivileged groups in positions of power, and the impact of bureaucrats on more informal transfers of resources from the state to citizens. They do suggest, however, that any potential gains in these areas can be obtained, at least under some conditions, without sacrificing the ability of bureaucrats to execute their institutional responsibilities.

References

Anzia, Sarah F and Christopher R Berry. 2011. “The Jackie (and Jill) Robinson effect: why do congresswomen outperform congressmen?” *American Journal of Political Science* 55(3):478–493.

Arcidiacono, Peter. 2005. “Affirmative action in higher education: How do admission and financial aid rules affect future earnings?” *Econometrica* 73(5):1477–1524.

Asher, Sam and Paul Novosad. forthcoming. “Rural Roads and Local Economic Development.” *American Economic Review* .

Bertrand, Marianne, Rema Hanna and Sendhil Mullainathan. 2010. “Affirmative action in education: Evidence from engineering college admissions in India.” *Journal of Public Economics* 94(1):16–29.

Bertrand, Marianne, Robin Burgess, Arunish Chawla and Guo Xu. forthcoming. “The glittering prizes: Career incentives and bureaucrat performance.” *The Review of Economic Studies* .

Besley, Timothy, Rohini Pande, Lupin Rahman and Vijayendra Rao. 2004. “The politics of public good provision: Evidence from Indian local governments.” *Journal of the European Economic Association* 2(2-3):416–426.

Bhavnani, Rikhil R. 2009. “Do electoral quotas work after they are withdrawn? Evidence from a natural experiment in India.” *American Political Science Review* 103(1):23–35.

Bhavnani, Rikhil R. 2017. “Do the Effects of Temporary Ethnic Group Quotas Persist? Evidence from India.” *American Economic Journal: Applied Economics* 9(3):105–123.

Bhavnani, Rikhil R. and Alexander Lee. 2018. “Local Embeddedness and Bureaucratic Performance: Evidence from India.” *The Journal of Politics* 80(1):71–87.

Bohlken, Anjali Thomas. 2018. “Targeting Ordinary Voters or Political Elites? Why Pork Is Distributed Along Partisan Lines in India.” *American Journal of Political Science* 62(4):796–812.

Bolick, Clint. 1996. *The affirmative action fraud: can we restore the American civil rights vision?* Cato Institute.

Carter, David A, Betty J Simkins and W Gary Simpson. 2003. “Corporate governance, board diversity, and firm value.” *Financial Review* 38(1):33–53.

Chauchard, Simon. 2014. “Can descriptive representation change beliefs about a stigmatized group? Evidence from rural India.” *American Political Science Review* 108(2):403–422.

Dee, Thomas S. 2005. “A teacher like me: Does race, ethnicity, or gender matter?” *The American Economic Review* 95(2):158–165.

Deshpande, Ashwini and Thomas E Weisskopf. 2014. “Does affirmative action reduce productivity? A case study of the Indian railways.” *World Development* 64:169–180.

Dunning, Thad and Janhavi Nilekani. 2013. “Ethnic quotas and political mobilization: caste, parties, and distribution in Indian village councils.” *American Political Science Review* 107(01):35–56.

Dutta, Puja, Rinku Murgai, Martin Ravallion and Dominique Van de Walle. 2014. *Right to Work?: Assessing India’s Employment Guarantee Scheme in Bihar*. World Bank.

Ferreira, Fernando and Joseph Gyourko. 2014. “Does gender matter for political leadership? The case of US mayors.” *Journal of Public Economics* 112:24–39.

Griffin, Peter. 1992. “The impact of affirmative action on labor demand: A test of some implications of the Le Chatelier principle.” *The Review of Economics and Statistics* pp. 251–260.

Gulzar, Saad and Benjamin J Pasquale. 2017. “Politicians, bureaucrats, and development: Evidence from India.” *American Political Science Review* 111(1):162–183.

Holzer, Harry and David Neumark. 1999. "Are affirmative action hires less qualified? Evidence from employer-employee data on new hires." *Journal of Labor Economics* 17(3):534–569.

India. 1980. *Report of the Backward Classes Commission*. Controller of Publications.

Iyer, Lakshmi and Anandi Mani. 2012. "Traveling agents: political change and bureaucratic turnover in India." *Review of Economics and Statistics* 94(3):723–739.

Jencks, Christopher. 1998. "Racial bias in testing." *The Black-White test score gap* 55:84.

Jensenius, Francesca R. 2017. *Social justice through inclusion: The consequences of electoral quotas in India*. Oxford University Press.

Johnson, Tim. 2015. "Service after Serving: Does Veterans' Preference Diminish the Quality of the US Federal Service?" *Journal of Public Administration Research & Theory* 25(3).

Karekurve-Ramachandra, Varun and Alexander Lee. forthcoming. "Do gender quotas hurt less privileged groups? Evidence from India." *American Journal of Political Science* .

Kasara, Kimuli. 2007. "Tax me if you can: Ethnic geography, democracy, and the taxation of agriculture in Africa." *American Political Science Review* 101(01):159–172.

Kramon, Eric and Daniel N Posner. 2016. "Ethnic Favoritism in Education in Kenya." *Quarterly Journal of Political Science* 11:1–58.

Krislov, Samuel. 2012. *Representative bureaucracy*. Quid Pro Books.

Lee, Alexander. 2019*a*. "Does Affirmative Action Work? Evaluating India's Quota System." http://www.rochester.edu/college/faculty/alexander_lee/wp-content/uploads/2019/04/resp accessed 9/16/2019.

Lee, Alexander. 2019*b*. *From Hierarchy to Ethnicity: The Politics of Caste in Twentieth Century India*. Cambridge University Press.

Lewis, Gregory B. 1997. "Race, sex, and performance ratings in the federal service." *Public Administration Review* pp. 479–489.

Lott, JR. 2000. "Does a helping hand put others at risk?: Affirmative action, police departments, and crime." *Economic Inquiry* 38(2):239–277.

Marion, Justin. 2009. "How costly is affirmative action? Government contracting and California's Proposition 209." *The Review of Economics and Statistics* 91(3):503–522.

Meier, Kenneth John and Lloyd G Nigro. 1976. "Representative bureaucracy and policy preferences: A study in the attitudes of federal executives." *Public Administration Review* pp. 458–469.

Ministry of Home Affairs. 2010. "Guidelines of Transfer/Postings of IAS/IPS Officers of Joint AGMU Cadre.".

Pande, Rohini. 2003. "Can mandated political representation increase policy influence for disadvantaged minorities? Theory and evidence from India." *The American Economic Review* 93(4):1132–1151.

Shah, AM. 1991. "Job reservations and efficiency." *Economic and Political Weekly* pp. 1732–1734.

Sowell, Thomas. 2005. *Affirmative Action around the world: An Empirical Study*. Yale University Press.

Tsai, Lily L. 2007. "Solidary groups, informal accountability, and local public goods provision in rural China." *American Political Science Review* 101(02):355–372.

Supplemental Information for “Does Affirmative Action Worsen Bureaucratic Performance? Evidence from the Indian Administrative Service”

Rikhil R. Bhavnani
University of Wisconsin–Madison

Alexander Lee
University of Rochester

American Journal of Political Science

September 22, 2019

Table of Contents

A The Role of IAS Officers in MGNREGA Implementation

1. Ministry of rural development. "Roles and Responsibilities of Key Functionaries." 2014. http://nrega.nic.in/Circular_Archive/archive/Roles_responsibilites.pdf, accessed 5/18/2019.

[Role of] District program coordinator

a) Assist the District Panchayat (DP) in discharging its functions.

b) Receive the Block Panchayat plans and consolidate them along with project proposals received from other implementing agencies for inclusion in the District Plan for approval by the DPs.

c) Accord timely sanction to shelf of projects.

d) Ensure timely release and utilization of funds.

e) Ensure wage-seekers are provided work as per their entitlements under this Act.

f) Review, monitor and supervise the performance of the POs and all implementing agencies in relation to MGNREGA works.

g) Conduct periodic inspection of the works in progress and verification of Muster Rolls.

h) Ensure that First Information Report (FIR) is filed in every case in which there is prima facie, evidence of misappropriation or financial irregularity.

i) Appoint Project Implementation Agencies (PIAs) throughout the district, keeping in mind that for at least 50% of value of works, the PIAs need to be GPs.

j) Ensure that Rozgar Diwas is organised at every Ward and Gram Panchayat level at least once a month.

k) Carry out responsibilities related to grievance redressal.

l) Coordinate an Information Education and Communication (IEC) campaign for MGNREGA within the district.

m) Develop annual plans for training and capacity building of various stakeholders within the district.

n) Submit periodic progress and updates to the State Government.

o) Ensure that social audits are done in all GPs once in six months and ensure follow-up action on social audit reports.

p) Ensure that all transactions including issue of JCs, recording of applications for work, allocation of work, generation of wage slips and Fund Transfer Orders (FTOs), entries relating to work performed, delayed payment of wages, and unemployment allowance are made through NREGASoft only.

q) Ensure that all entries relating to works such as details of the shelf of works, GPS coordinates, status of implementation, photographs of works at three different stages are entered in NREGASoft at every required stage.

r) Ensure that all funds received by Implementing Agencies and District level authorities including Panchayats are posted in NREGASoft no later than two days of receipt of such funds.

s) Ensure that all required entries in NREGASoft are made by all concerned officials including the line departments, in the district.

t) Ensure that technical quality of the convergence project is maintained through District Resource Group.

2. International Growth Centre “Auditing the auditors: Rapid response process evaluation of MGNREGA Divas for Rural Development Department, Government of Bihar.” 2013. https://www.theigc.org/wp-content/uploads/2018/08/IDInsight-2013-final-report.pdf**, accessed 5/18/2019.**

According to RDD’s official data on MD [inspection visits], only 3 MD visits have been taking place per district per week. If RDD’s instruction to cover all blocks of the district per week had been followed, the average number of weekly MD visits per district should have been 14.

However, coverage varies greatly by district. Around 60% of the panchayats in Bihar were visited between 1 June 2012 to 1 May 2013. While only one district had achieved 100% coverage of panchayats in this period, 10 of 38 districts did not even cover 50% of total panchayats.

While it was beyond the scope of the study to collect any quantitative information on the reasons for low coverage and the high district-wise variation, anecdotal evidence suggest that there is a shortage of District Collectors and other senior officers at the district-level for such visits. Given the shortage of officers, the existing ones seem to be overloaded with other administrative work. In addition, it appears the District Magistrate (DM), the senior- most bureaucrat of the district, has significant control over the quantity and quality of the MD visits, so coverage is likely higher when MD is a priority for the DM.

3. Business standard. “Railways Start work under MGNREGA Scheme.âĂİ December 13, 2018. https://www.business-standard.com/article/news-ians/railways-start-work-under-mg **accessed 5/18/2019.**

Sharma said that in Kishanganj, the proposal of railway line embankment repair was sanctioned for 5.7 km of track at an approximate cost of Rs 13.4 lakh. "Around 30 labourers are turning up on a regular basis and all are being provided with job card against MGNREGA. Similarly, the District Magistrate of Uttar Dinajpur had also sanctioned supplementary estimate of embankment repair for 8.3 km of railway track at an approximate cost of Rs 21.5 lakh under the rural job scheme," he said.

4. Ministry of Rural Development. “Statewise details of action taken on serious complaints under MGNREGA.” 2009. https://nrega.nic.in/State_Details_19022010.pdf**, accessed 5/18/2019.**

In this case, a F.I.R was lodged in Police Station Punnuganj, district Sonbhadra against the responsible official Sh. Baliram, Assistant Development Officer, Agricultured. District Magistrate has also intimated that the HonâĂİble High Court at Allahabad had also summoned the Investigation Officer and the Officer who has lodged the F.I.R. and the above directions of HonâĂİble Court have been complied with. Besides above action under Indian Panel Code, on the recommendations of the District Magistrate, Sonabhadra the above official has been placed under suspension by the competent Authority and a disciplinary proceeding has been initiated against him. The Block Development Officer of Chatra block who is also the Programme Officer under NREGA has been awarded a mid term adverse entry by the District Magistrate.

5. Government of Rajasthan “Implementation of NREGA in Rajasthan : What has worked ?” 2010. http://rdprd.gov.in/PDF/Implementation%20of%20NREGA-23.10.08.pdf**, accessed 5/18/2019.**

*CM convened 3 conferences of Collectors for review of NREGA.
*Regular review by the Chief Secretary
*Fortnightly review note by Pr. Secretary, RD & PR
*Video Conferences with Collectors & CEO's, ZP on fixed agenda
*Mukhya Mantri Sarvjan Sambal Mahaabhiyan (May–June, 2008).
*Village Contact Drive (Jan., 2007)
*Tours by Senior Officers
*District Officer's-in-charge inspect minimum 3 NREGA works in a month
*Meeting by the Collectors with POâĂŹs on fixed agenda
*Review by Sectoral in-charge
*Review by the District in-charge & Minister in-charge

6. Sinha, Chandan. *Public sector reforms in India: New role of the District Officer.* SAGE Publications India, 2007.

Development schemes in the social sector for which the DO has direct responsibility are spread over various areas... rural development schemes, Jawahar Rozgar Yojana, Employment Assurance Scheme and the National Rural Employment Guarantee Act, 2005 assume additional emphasis.

Congress Member of Parliament Santosh Chowdhary on Thursday asked the district authorities to use funds received under Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA) scheme for works relating to public welfare only.

7. Hindustan Times "Use MGNREGA funds for public welfare works only: MP." April 19 2012. https://www.hindustantimes.com/punjab/use-mgnrega-funds-for-public-welfare-works-o **accessed 5/18/2019.**

"The authorities should ensure proper utilisation of funds received from the central government and avoid misappropriation of funds by all means," she said while addressing a meeting of district authorities as chairperson of district vigilance and monitoring committee for central funds.

"There are many public welfare works, like cleaning of village ponds, removal of cannabis plants from roadsides and strengthening pillars of small bridges. These can provide great relief to villagers as the conditions would improve," she added.

Kapurthala deputy commissioner Alaknanda Dayal said on this occasion that 14,829 job cards were issued under MGNREGA scheme in the district. Since its inception, nearly 7,000 families had been given employment.

Besides MGNREGA, she also reviewed other Centre-sponsored schemes like supplementary nutrition scheme, kishori shakti scheme, Indira Gandhi old age pension scheme, Indira awas yojna and swarn jayanti village self-employment scheme.

Sultanpur Lodhi MLA Navtej Singh Cheema and Zila Parishad chairman Sucha Singh Chouhan were also present on the occasion.

8. **Rural Development Department, Government of Himachal Pradesh. "No. SMS-1/2010-11-RDD Approval of MGNREGA Shelf of Projects for the year 2011-12 " April 19 2012.** http://www.hprural.nic.in/cir112.pdf**, accessed 5/18/2019.**

It is, therefore, requested that the shelf of projects for the year 2011–12 be forwarded to the concerned Deputy Commissioner, who is designated as district programme coordinator (MGNREGA) by November 10, 2010.

9. **Ministry of Rural Development, Government of India. "Guidelines for Planning for**

Works & preparation of Labour Budget FY 2018-19" April 19 2012. https://nrega.nic.in/netnrega/wr **accessed 5/18/2019.**

Sub section 6 of section 14 of the MGNREG Act 2005, directs that the District Programme Coordinator (DPC) under MGNREGA shall prepare, in the month of December every year, a Labour Budget (LB) for the next financial year containing the details of the anticipated demand for unskilled manual work in the district and the engagement of workers in the works covered under the programme.

10. **Video Volunteers. "Video Advocacy: MGNREGA" ND.** https://www.videovolunteers.org/refo **accessed 5/18/2019.**

In September 2015, VV in association with our partners and funders Poorest Areas of Civil Society (PACS), conducted a training during our National Meet. 132 Correspondents from 13 states attended and learnt about MGNREGA provisions and the avenues through which Correspondents can use their videos to get the authorities to respond.

As a result, the focus of Correspondents' videos has shifted from merely documenting failures to a more reform-based approach, aimed at solving the gaps in implementation and celebrating the successful outcomes of the scheme. For example, Navita Devi ? our correspondent from Katihar, Bihar ? achieved impact using a mixture of both video footage and community mobilisation to highlight the plight of 100 workers who had not been paid their wages. By getting together a group of the workers to approach the District Officer, and then showing him the video documentation (below), payment was released to all the 100 workers through reform-focused dialogue.

B Robustness Tests

To check if our null results are driven by our **choice of dependent variable**, we check for robustness using another MGNREGA-related outcome, and to using the outcomes of two other government programs. In regression 1 of SI Table A1, we switch our MGNREGA-related dependent variable to the logarithm of person-days of employment. We do not use this variable in our main analysis since we observe it for fewer years. Affirmative action hires again have no detectable effect on the dependent variable. In fact, and although these data only start in 2012, the estimated effect of affirmative action has a narrower confidence interval than in our main specification.

We next examine the effects of affirmative action recruits on the standardized log number of villages connected by road under the country's flagship road building scheme (the Pradhan Mantri Gram Sadak Yojana or PMGSY) as the outcome. This relationship is explored step-by-step in SI Table A2. Regression 1 examines the bivariate relationship between roads and affirmative action hires, controlling for the number of villages that are not connected by roads. Regression 2 adds controls for a number of possible confounds (dummies for positive and negative rainfall shocks, and the proportion of Congress MLAs, BJP MLAs, and the proportion of MLAs in the state government, reserved for Scheduled Castes and Tribes), and regression 3 controls for state-year and district fixed effects. Regression 4 instruments for the proportion of affirmative action bureaucrats with the proportion of early career bureaucrats. Across all these specifications, affirmative action recruits appear to have no substantive or statistically significant effects on road building.

In a last set of tests to ensure that our results are not driven by our choice of dependent variable, we examine the effects of affirmative action bureaucrats on the time taken to approve MPLADS projects. This relationship is explored step-by-step in SI Table A3, where proposed projects are

Table A1: Robustness tests for the effects of affirmative action bureaucrats on MGNREGA implementation, 1/5

Estimator:	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Model:	Alt. DV 1	More controls	No FE	Lg. DV	Dist. time trends	Interactions
	1	2	3	4	5	6
Prop. affirmative action bureaucrats	0.03	0.02	0.10	-0.00	-0.02	0.04
	[0.05]	[0.06]	[0.07]	[0.03]	[0.06]	[0.07]
Prop. affirmative action bureaucrats X positive rainfall shock						-0.10
						[0.08]
Prop. affirmative action bureaucrats X negative rainfall shock						0.03
						[0.08]
Lagged dependent variable				0.80***		
				[0.02]		
Positive rainfall shock dummy	-0.04*	0.03	0.01	-0.01	-0.00	0.08
	[0.02]	[0.03]	[0.05]	[0.02]	[0.02]	[0.05]
Negative rainfall shock dummy	0.02	0.04**	-0.02	0.02	0.02	0.03
	[0.02]	[0.02]	[0.04]	[0.02]	[0.02]	[0.04]
Prop. Congress MLAs	-0.05	-0.10	-0.13	-0.02	-0.07	-0.10
	[0.05]	[0.06]	[0.11]	[0.04]	[0.07]	[0.06]
Prop. BJP MLAs	-0.10**	-0.04	-0.22*	-0.00	-0.10	-0.05
	[0.04]	[0.06]	[0.11]	[0.04]	[0.07]	[0.06]
Prop. MLAs in state gov.	0.15***	0.07	0.05	0.02	0.01	0.08
	[0.05]	[0.06]	[0.09]	[0.03]	[0.06]	[0.06]
Prop. MLAs reserved for Scheduled Castes	-0.06	0.07	0.34**	0.05	0.03	0.08
	[0.06]	[0.08]	[0.16]	[0.04]	[0.08]	[0.07]
Prop. MLAs reserved for Scheduled Tribes	0.23**	-0.06	0.42***	0.03	0.02	-0.04
	[0.10]	[0.16]	[0.11]	[0.04]	[0.19]	[0.16]
Bureaucrats' age		-0.01*				
		[0.00]				
Prop. female bureaucrats		-0.02				
		[0.05]				
Bureaucrats' degree class		0.04				
		[0.03]				
Prop. local bureaucrats		-0.07				
		[0.05]				
Bureaucrats' years experience		0.01				
		[0.01]				
State-year fixed effects?	Y	Y	Y	Y	Y	Y
District fixed effects?	Y	Y	N	N	Y	Y
Observations	1,292	2,047	2,047	1,525	2,047	2,047
Adjusted R-squared	0.96	0.88	0.62	0.92	0.94	0.88
F-statistic for AA bureaucrats	136	323	914	468	252	127
F-statistic for AA bureaucrats X positive rainfall shock						83
F-statistic for AA bureaucrats X negative rainfall shock						136

Notes: The dependent variable for regression 1 is the logarithm of the person-days of employment under MGNREGA. The dependent variable for all other regressions is the logarithm of households that received 100 days or more of employment under MGNREGA. The dependent variables are standardized to have mean 0 and standard deviation 1. Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Robustness tests: The effects of affirmative action bureaucrats on road construction, 2/5

Estimator:	OLS	OLS	OLS	2SLS	
Equation:				1st stage	2nd stage
	1	2	3	4	
Prop. affirmative action bureaucrats	-0.01	0.00	-0.00		0.04
	[0.08]	[0.07]	[0.06]		[0.08]
Positive rainfall shock dummy		0.10	0.03	0.01	0.03
		[0.06]	[0.06]	[0.03]	[0.05]
Negative rainfall shock dummy		-0.11**	0.00	-0.02	0.00
		[0.05]	[0.04]	[0.02]	[0.04]
Prop. Congress MLAs		0.07	0.16	0.02	0.16
		[0.12]	[0.12]	[0.07]	[0.10]
Prop. BJP MLAs		0.42***	0.04	0.10	0.04
		[0.11]	[0.13]	[0.06]	[0.11]
Prop. MLAs in state gov.		-0.29***	-0.08	-0.01	-0.08
		[0.10]	[0.10]	[0.05]	[0.08]
Prop. MLAs reserved for Scheduled Castes		0.18	-0.12	0.08	-0.12
		[0.21]	[0.17]	[0.08]	[0.14]
Prop. MLAs reserved for Scheduled Tribes		0.33**	-0.40	0.10	-0.40
		[0.13]	[0.30]	[0.08]	[0.25]
Ln unconnected villages	0.00***	0.00***	0.00**	-0.00	0.00***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Prop. early-career officers recruited under AA				0.63***	
				[0.04]	
State-year fixed effects?	N	N	Y	Y	Y
District fixed effects?	N	N	Y	Y	Y
Observations	1,641	1,641	1,641	1,641	1,641
Adjusted R-squared	0.08	0.11	0.78		0.78
F-statistic for AA bureaucrats					231

Notes: The dependent variable is the logarithm of the number of villages connected by road under PMGSY, standardized to have mean 0 and standard deviation 1. Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Robustness tests: The effects of affirmative action bureaucrats on MPLADS project approvals, 3/5

Dependent variable:	Sanctioned within bureaucrats' term						Within 75 days	Log days to sanction
Estimator:	OLS	OLS	OLS	2SLS		2SLS	2SLS	2SLS
Equation:				1st stage	2nd stage			
	1	2	3	4		5	6	7
Prop. officers recruited under AA	-0.02	0.03	-0.00		0.07	-0.09	-0.01	0.24
	[0.03]	[0.03]	[0.03]		[0.06]	[0.06]	[0.08]	[0.20]
Log cost of proposed projects		0.00	-0.00	0.01	-0.00	0.00	0.00	-0.03
		[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.03]
Prop. early-career officers recruited under AA				1.06***				
				[0.17]				
Prop. officers OBC/SC/ST						0.23***		
						[0.07]		
State-year fixed effects?	N	N	Y	Y	Y	Y	Y	Y
District fixed effects?	N	Y	Y	Y	Y	Y	Y	Y
Observations	82,776	82,776	82,776	82,776	82,776	82,776	82,776	82,776
Adjusted R-squared	0.00	0.19	0.26		0.26	0.24	0.20	0.30
First stage F-statistic for AA bureaucrat					39	21	39	39

Notes: The dependent variable for the first four regressions is a dummy for whether MPLADS projects were approved within the bureaucrats' term; for regression 6 it is a dummy for whether proposed projects were approved within 75 days; for regression 7 it is the logarithm of the days to approval. Standard errors are clustered by district. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

the unit of analysis. The data are from Bohlken (2018). Regression 1 examines the bivariate relationship between a dummy for whether projects are approved during a bureaucrat's term and affirmative action hires. In regression 2, we control for the cost of the proposed project, and district fixed effects; regression 3 also controls for state-year fixed effects. In regression 4, we employ our 2SLS strategy. In regression 5, we disaggregate the effects of identity and affirmative action status, echoing the findings in Table 2: "merit minorities" perform better than others, but affirmative action recruits do not. In regressions 6 and 7, we switch the dependent variables to dummies for whether the proposed projects were approved within 75 days (this is the legal mandate) and the number of days to approval. Across all these specifications, affirmative action recruits appear to have no substantive or statistically significant effects on the time to approve projects.

To check the robustness of our results to **specification changes**, we start by controlling for potentially post-treatment bureaucrat characteristics, including bureaucrats' mean age, the proportion of female bureaucrats, bureaucrats' mean bachelor's degree class, the proportion of bureaucrats serving in the state from which they are from and bureaucrats' mean years of experience. The null result is somewhat strengthened by this change, insofar as the confidence interval is narrower (regression 2 of SI Table A1).

Given the dataset's relatively short time span (2009–2016), we might be concerned with Nickell bias. We address this issue by dropping district fixed effects (regression 3), although this fails to control for time-invariant district level confounds such as levels of discrimination against lower castes, which might both impede MGNEGA implementation and influence the assignment of affirmative action bureaucrats to districts. As an alternative, we add the lag dependent variable (regression 4). The null result remains with both changes. It also survives the inclusion of district-specific time trends (regression 5).

Lastly, to examine if affirmative action officers are particularly responsive to rainfall shocks, we interact the affirmative action variable with positive and rainfall shocks (regression 6). Our results are robust to these modifications.

Recall that our theory is agnostic about the precise **functional form** underlying the relationship between affirmative action hires and bureaucratic output. Since this is the case, we next consider a number of alternative functional forms to model this relationship. First, affirmative action hires might have non-linear effects on MGNREGA implementation. To check whether this is the case, we control for the proportion of affirmative action bureaucrats and its square, instrumenting these terms with our standard instrument and its square (regression 1 of SI Table A4). Our results are robust to this modification.

Second, recall further that IAS officers can serve in junior and senior positions in the district bureaucracy. Might we find a negative effect of affirmative action recruits if we separate the effects of more powerful, senior officers (that is, the district collector, commissioner or magistrate) from others? Regression 2 suggests that this is not that case. Note that this is unsurprising, since in a large majority of cases the only IAS officers in the district are senior.

Third, just one affirmative action bureaucrat might have a negative effect in a district. To check whether this is the case, we round up the proportion of affirmative action bureaucrats and its instrument (regression 3). Our results are robust to this change as well.

Fourth, since districts typically have just one IAS officer in a year this variable takes on a value of 0 or 1 in 70% of district-years. The estimated effects of affirmative action recruits is robust to rounding this variable (regression 4).

Fifth, recall that some disadvantaged group members who scored above the general cutoff received preferential treatment at earlier stages of the recruitment process, on the preliminary exam and/or in a relaxation of the age and exam repetition limits. A strict definition of affirmative action would thus include these individuals as beneficiaries. Regression 5 shows the results of a model that uses this definition. The estimated effect of affirmative action is practically unchanged.

We next examine the robustness of our **identification strategy**. To do so, we start by estimating the effects of affirmative action recruits using the standard 2SLS specification while restricting the sample to states where we are able to document the quasi-exogenous rules by which officers are assigned to districts (regression 1 of SI Table A5)[1] and to the remaining states (regression 2). In regression 3, we pool these observations and while still estimating the effects of affirmative action recruits in the two sets of states separately. A *t*-test for the difference in coefficients is unable to reject the possibility that they are equal (p=0.19). For completeness, we also estimate the effects of affirmative action in each of India's major states separately (regression 4). Interpreting the variation in the estimated effects of affirmative action recruits is beyond the scope of the paper.

To further interrogate our identification strategy, we also estimate the reduced-form effect of the instrument on the dependent variable (regression 6 of SI Table A4). The estimated effect of affirmative action remains positive, substantively small and statistically insignificant. The null effect of affirmative action also obtains if we change the definition of "early career bureaucrats" from those serving in the first five years after recruitment to those serving up to four years after recruitment (regression 7).

In a last robustness test of the identification strategy, we use a discontinuity analysis to examine

[1] This restricts the sample to Andhra Pradesh, Karnataka, Rajasthan and Uttar Pradesh. These rules are discussed in the research design section.

Table A4: Robustness tests for the effects of affirmative action bureaucrats on MGNREGA implementation, 4/5

Estimator:	2SLS	2SLS	2SLS	2SLS	2SLS	OLS	2SLS
Model:	AA squared	Juniors, seniors	Any AA	Rounded	Strict AA	Reduced form	Four year
	1	2	3	4	5	6	7
Prop. affirmative action bureaucrats	-0.31						0.03
	[1.15]						[0.06]
Prop. affirmative action bureaucrats squared	0.36						
	[1.19]						
Prop. affirmative action senior bureaucrats		0.06					
		[0.07]					
Prop. affirmative action junior bureaucrats		0.02					
		[0.08]					
Dummy for any affirmative action bureaucrat			0.02				
			[0.07]				
Prop. affirmative action bureaucrats, rounded				0.03			
				[0.06]			
Prop. affirmative action bureaucrats, strict defn.					0.04		
					[0.06]		
Prop. early-career officers recruited under AA						0.02	
						[0.05]	
Positive rainfall shock dummy	0.04	0.04	0.04	0.04	0.09***	0.04	0.04
	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]
Negative rainfall shock dummy	0.05*	0.04*	0.04*	0.04*	0.06***	0.04	0.04*
	[0.03]	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]	[0.02]
Prop. Congress MLAs	-0.10	-0.10	-0.10	-0.10	-0.17**	-0.10	-0.10
	[0.07]	[0.06]	[0.06]	[0.06]	[0.08]	[0.08]	[0.06]
Prop. BJP MLAs	-0.05	-0.05	-0.05	-0.05	-0.01	-0.05	-0.05
	[0.06]	[0.06]	[0.06]	[0.06]	[0.10]	[0.07]	[0.06]
Prop. MLAs in state gov.	0.08	0.08	0.08	0.08	0.00	0.08	0.08
	[0.06]	[0.06]	[0.06]	[0.06]	[0.09]	[0.07]	[0.06]
Prop. MLAs reserved for Scheduled Castes	0.08	0.08	0.08	0.08	0.14	0.08	0.08
	[0.08]	[0.07]	[0.07]	[0.07]	[0.13]	[0.09]	[0.07]
Prop. MLAs reserved for Scheduled Tribes	-0.05	-0.04	-0.04	-0.03	-0.42	-0.04	-0.04
	[0.16]	[0.16]	[0.16]	[0.16]	[0.30]	[0.19]	[0.16]
State-year fixed effects?	Y	Y	Y	Y	Y	Y	Y
District fixed effects?	Y	Y	Y	Y	Y	Y	Y
Observations	2,047	2,047	2,047	2,047	1,084	2,047	2,047
Adjusted R-squared	0.88	0.88	0.88	0.88	0.91	0.88	0.88
F-statistic for AA bureaucrats	186		121	160	514		278
F-statistic for AA bureaucrats squared	152						
F-statistic for senior AA bureaucrats		121					
F-statistic for junior AA bureaucrats		169					

Notes: The dependent variable is the logarithm of households that received 100 days or more of employment under MGNREGA, standardized to have mean 0 and standard deviation 1. Standard errors are clustered by district. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A5: Robustness tests for the effects of affirmative action bureaucrats on MGNREGA implementation, 5/5

Estimator:	2SLS	2SLS	2SLS	2SLS
Sample:	Random assign.	Others	All states	All states
	1	2	3	4
Prop. affirmative action bureaucrats	-0.12	0.06		
	[0.11]	[0.07]		
Prop. aff. action bureaucrats X states with verified quasi-random assign.			-0.11	
			[0.11]	
Prop. aff. action bureaucrats X states without verified quasi-random assign.			0.06	
			[0.07]	
Prop. aff. action bureaucrats X Andhra Pradesh				0.24***
				[0.08]
Prop. aff. action bureaucrats X Bihar				-0.10
				[0.44]
Prop. aff. action bureaucrats X Chhattisgarh				-0.14**
				[0.07]
Prop. aff. action bureaucrats X Gujarat				0.08
				[0.28]
Prop. aff. action bureaucrats X Haryana				0.11
				[0.09]
Prop. aff. action bureaucrats X Himachal Pradesh				0.04
				[0.12]
Prop. aff. action bureaucrats X Jharkhand				-0.46
				[0.32]
Prop. aff. action bureaucrats X Karnataka				-0.09
				[0.10]
Prop. aff. action bureaucrats X Kerala				-0.04
				[0.13]
Prop. aff. action bureaucrats X Madhya Pradesh				0.37**
				[0.16]
Prop. aff. action bureaucrats X Maharashtra				1.92***
				[0.59]
Prop. aff. action bureaucrats X Orissa				-1.29***
				[0.24]
Prop. aff. action bureaucrats X Punjab				0.36
				[0.31]
Prop. aff. action bureaucrats X Rajasthan				0.13
				[0.26]
Prop. aff. action bureaucrats X Tamil Nadu				-0.25*
				[0.13]
Prop. aff. action bureaucrats X Uttar Pradesh				-0.87*
				[0.52]
Prop. aff. action bureaucrats X Uttarakhand				0.29
				[0.43]
Prop. aff. action bureaucrats X West Bengal				0.16
				[0.20]
Prop. aff. action bureaucrats X other states				-1.13
				[0.79]
Positive rainfall shock dummy	-0.01	0.05	0.04	0.02
	[0.03]	[0.03]	[0.03]	[0.03]
Negative rainfall shock dummy	0.06*	0.05*	0.04**	0.05*
	[0.03]	[0.03]	[0.02]	[0.03]
Prop. Congress MLAs	-0.03	-0.13*	-0.10	-0.13**
	[0.13]	[0.08]	[0.06]	[0.06]
Prop. BJP MLAs	0.06	-0.07	-0.05	-0.07
	[0.09]	[0.08]	[0.06]	[0.07]
Prop. MLAs in state gov.	0.24***	0.04	0.09	0.08
	[0.08]	[0.08]	[0.06]	[0.07]
Prop. MLAs reserved for Scheduled Castes	-0.18	0.19**	0.08	0.09
	[0.15]	[0.09]	[0.07]	[0.09]
Prop. MLAs reserved for Scheduled Tribes	-0.46**	0.06	-0.05	0.02
	[0.19]	[0.18]	[0.16]	[0.17]
State-year fixed effects?	Y	Y	Y	Y
District fixed effects?	Y	Y	Y	Y
Observations	484	1,563	2,047	2,047
Adjusted R-squared	0.94	0.87	0.88	0.88
F-statistic for AA bureaucrats	63	316		
F-statistic for AA bureaucrats, verified states			31	
F-statistic for AA bureaucrats, other states			162	

Notes: The dependent variable is the logarithm of households that received 100 days or more of employment under MGNREGA, standardized to have mean 0 and standard deviation 1. Standard errors are clustered by district. *$p <$ 0.10, **$p <$ 0.05, ***$p <$0.01.

the effects of affirmative action conditional on exam rank. Recall that recruits ranked below a (year-varying) cutoff are affirmative action recruits. For example, recruits that were ranked 94 and below in 2001 were recruited via affirmative action. Comparing the performance of bureaucrats on either side of this threshold therefore yields an estimate of the "cost" of affirmative action, one that is particularly focused on holding candidate quality constant. Note that this estimate is *not* the average effect of being assigned an affirmative action officer (since many affirmative action candidates are well below the cutoff), but is rather the effect of being assigned an affirmative action officer relative to being assigned a general category officer with a similar exam score.[2]

Recall that the discontinuity analysis assumes that the treatment, that is, affirmative action status, has no effect on predetermined covariates. SI Figure A1 presents graphical tests to check whether this is indeed the case. The running variable in this analysis is IAS officers' exam rank, normalized such that those with exam ranks greater than 0 are affirmative action recruits (district-years with multiple IAS officers were excluded), and the treatment is the assignment of an affirmative action recruit to a district. The plots suggests that affirmative action recruits do not affect a number of predetermined confounds, specifically dummies for whether districts experienced positive or negative rainfall shocks and the proportion of state legislators from the Congress, the BJP, the state's governing party, and from constituencies reserved for Scheduled Castes and Tribes.

SI Figure A2 graphically presents the results of the discontinuity analysis. The running variable is IAS officers' exam rank, normalized such that those with exam ranks greater than 0 are affirmative action recruits (district-years with multiple IAS officers were excluded), and the treatment is the assignment of an affirmative action recruit to a district. The bandwidth is calculated using the standard CCT optimization procedure. The plot suggests that affirmative action recruits are associated with marginally higher levels of MGNREGA employment at the discontinuity.

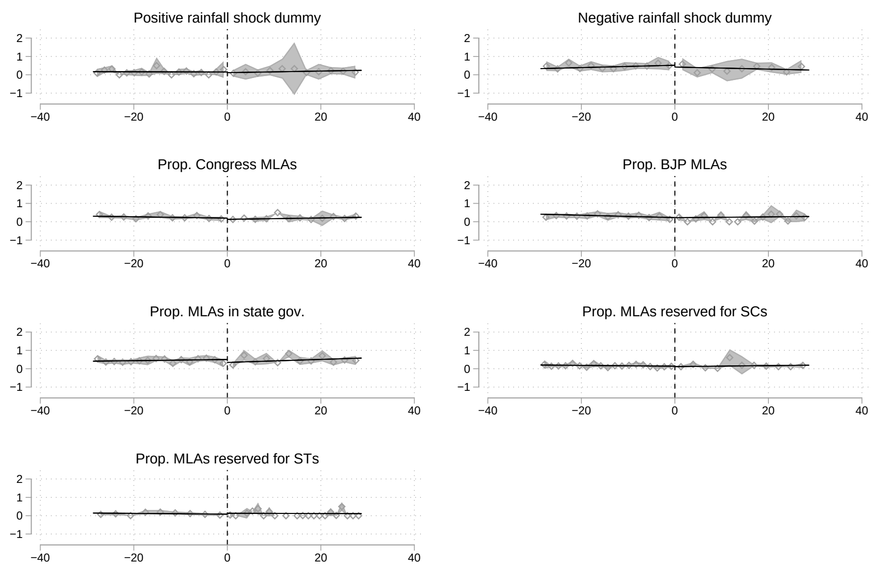
Detailed results presented in SI Table A6 show that the estimated positive effect of affirmative action is somewhat attenuated with the additional of controls, and is further attenuated when the sample is restricted to early-career officers. Since early-career officers are arguably quasi-randomly assigned to districts, these are our preferred results. In this analysis, the positive effect of affirmative action is statistically indistinguishable from 0.

As reported in SI Table A6, this RD-style analysis is robust to a number of additional changes, including the use of second and third order polynomials to model the forcing variable and the use of bandwidths that are half and double the preferred bandwidth chosen by the optimization procedure referenced above.

The main 2SLS estimate of the effects of affirmative action recruits, and a series of robustness tests, all suggest that affirmative action recruits do not worsen MGNREGA implementation. This null is the focus of the paper and is precisely estimated.

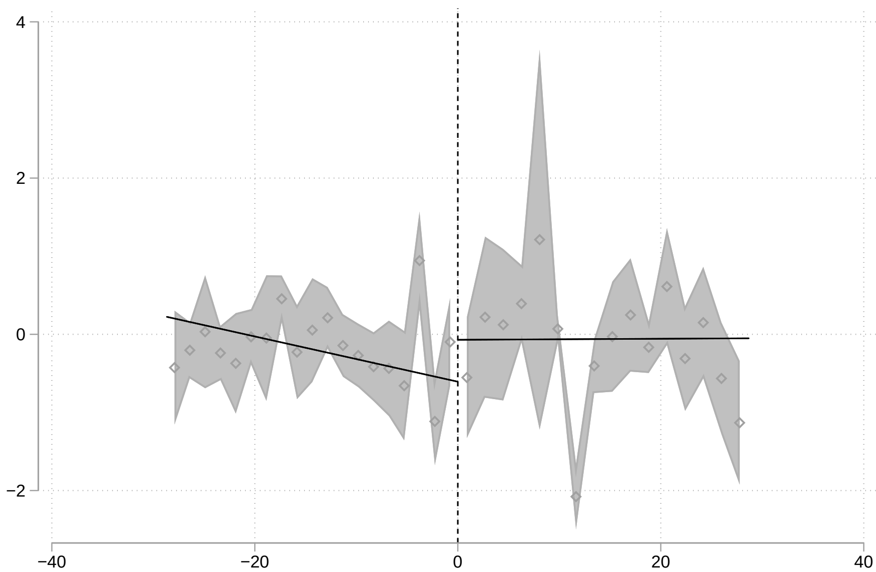
[2]As noted previously, a standard regression discontinuity analysis is not possible since the forcing variable (relative exam rank) does not exclusively determine the treatment (that is, affirmative action). Since only disadvantaged group members with below-cutoff exam ranks can be recruited, assignment to the treatment is determined by both relative exam rank and bureaucrat identity.

Figure A1: Discontinuity estimate of the effects of affirmative action bureaucrats on possible confounds



Notes: These graphs check for the "effects" of the running variable on possible confounds (that is, for balance on possible confounds). The running variable is IAS officers' exam rank, normalized such that those with exam ranks greater than 0 are affirmative action recruits. District-years with more that 1 officer are excluded. The outcomes are the positive and negative rainfall shock dummies, the proportion of Congress and BJP MLAs, the proportion of MLAs in the state government, and the proportion of MLAs reserved for Scheduled Castes and Scheduled Tribes. The solid lines plot predicted values of local linear regressions using a triangular kernel. The dots are binned sample means of the underlying data, with shaded 95% confidence intervals.

Figure A2: A discontinuity estimate of the effects of affirmative action bureaucrats on MGN-REGA implementation



Notes: This graph is a representation of the first model of SI Table A6. The running variable is IAS officers' exam rank, normalized such that those with exam ranks greater than 0 are affirmative action recruits. District years with more that 1 officer are excluded. The outcome is the logarithm of households that received 100 days or more of employment under MGNREGA, standardized to have mean 0 and standard deviation 1. The solid lines plot predicted values of local linear regressions using a triangular kernel. The dots are binned sample means of the underlying data, with shaded 95% confidence intervals.

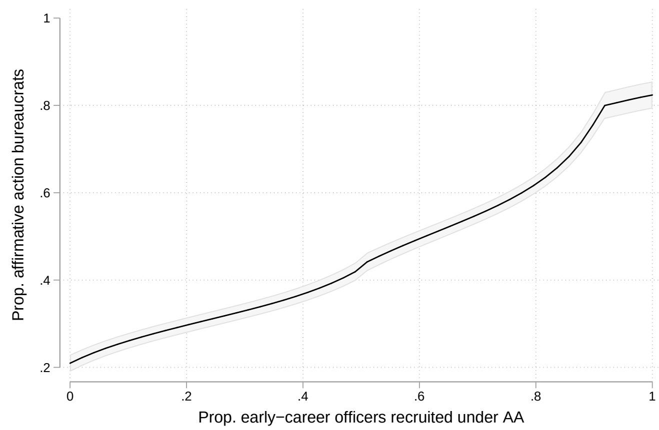
Table A6: Discontinuity estimates of the effects of affirmative action bureaucrats on MGNREGA implementation

Sample	Estimate	Std. Err.	*p*-value	Bndwdth.	*N*
Full sample	0.54	0.22	0.02	28.7	1,422
Full sample with controls	0.36	0.22	0.11	26.1	1,422
Early-career officers with controls	0.10	0.26	0.69	38.2	628
Robustness tests					
Quadratic model ($p = 2$)	0.23	0.32	0.47	47.5	628
Cubic model ($p = 3$)	0.17	0.36	0.63	44.6	628
Double bandwidth	0.16	0.19	0.42	76.5	628
Half bandwidth	0.20	0.29	0.69	19.1	628

Notes: The running variable is IAS officers' exam rank, normalized such that those with exam ranks greater than 0 are affirmative action recruits. District years with more that 1 officer are excluded. The outcome is the logarithm of households that received 100 days or more of employment under MGNREGA, standardized to have mean 0 and standard deviation 1. The estimate is the average treatment effect with locally linear regression with triangular kernel. Controls are dummies for whether districts experienced positive or negative rainfall shocks and the proportion of state legislators from the Congress, the BJP, the state's governing party, and from constituencies reserved for Scheduled Castes and Tribes. Early-career officers are defined as those in the first five years of service.

C Supplementary Tables and Figures

Figure A3: First stage relationship between the proportion of affirmative action recruits and its instrument



Notes: The solid line is an Epanechnikov kernel-weighted local polynomial plot. The shaded region displays the 95% confidence interval. See text for details.

Table A7: Summary statistics

	Obs.	Mean	Std. Dev.	Min.	Max.
Households received 100+ days of NREGA employment	2,047	8445.42	13867.77	1.00	126579.25
Ln households received 100+ days of NREGA employment	2,047	7.94	1.66	0.69	11.75
Ln households received 100+ days of NREGA employment, standardized	2,047	0.00	1.00	-4.37	2.29
Person-days of NREGA employment	1,292	4095217.17	5068122.52	47457.75	34741996.00
Ln person-days of NREGA employment	1,292	14.63	1.13	10.77	17.36
Ln person-days of NREGA employment, standardized	1,292	0.03	0.97	-3.30	2.39
Villages newly connected by roads	2,024	24.52	74.31	0.00	1082.25
Ln villages newly connected by roads	2,024	1.27	1.80	0.00	6.99
Ln villages newly connected by roads, standardized	2,024	0.02	1.02	-0.70	3.26
Person-days of NREGA employment for SCs/STs	2,024	1842076.15	2326452.43	7914.25	15875000.00
Ln person-days of NREGA employment for SCs/STs	2,024	13.70	1.34	8.98	16.58
Ln person-days of NREGA employment for SCs/STs, standardized	2,024	0.00	0.98	-3.46	2.11
Prop. of NREGA expenditures on materials	1,532	28.83	10.84	0.15	68.28
Prop. of NREGA expenditures on materials, standardized	1,532	-0.00	1.00	-2.64	3.63
Prop. affirmative action bureaucrats	2,047	0.42	0.42	0.00	1.00
Prop. affirmative action senior bureaucrats	2,047	0.18	0.35	0.00	1.00
Bureaucrats' ln exam rank	2,047	4.17	1.08	0.00	6.82
Prop. disadvantaged group bureaucrats	2,047	0.57	0.43	0.00	1.00
Prop. early-career officers recruited under AA	2,047	0.41	0.34	0.00	1.00
Prop. affirmative action early-career senior bureaucrats	2,047	0.10	0.22	0.00	1.00
Early-career bureaucrats' ln exam rank	2,047	4.20	0.87	0.00	6.82
Prop. early-career disadvantaged group officers	2,047	0.57	0.35	0.00	1.00
Positive rainfall shock dummy	2,047	0.19	0.40	0.00	1.00
Negative rainfall shock dummy	2,047	0.39	0.49	0.00	1.00
Prop. Congress MLAs	2,047	0.26	0.29	0.00	1.00
Prop. BJP MLAs	2,047	0.29	0.34	0.00	1.00
Prop. MLAs in state gov.	2,047	0.43	0.34	0.00	1.00
Prop. MLAs reserved for Scheduled Castes	2,047	0.16	0.18	0.00	1.00
Prop. MLAs reserved for Scheduled Tribes	2,047	0.12	0.27	0.00	1.00

Notes: See text for details.

Table A8: Balance tests for the instrument for the proportion of affirmative action bureaucrats, 1/2

Dependent variables:	Ln population	Ln literates	Ln Scheduled Castes	Ln Scheduled Tribes	Ln villages	Ln vill. with power	Ln vill. with roads	Ln vill. with high school
	1	2	3	4	5	6	7	8
Prop. early-career officers	0.03	0.01	0.03	-0.16	0.13	-0.12	-0.05	0.06
recruited under AA	[0.09]	[0.03]	[0.08]	[0.20]	[0.13]	[0.15]	[0.05]	[0.10]
Positive rainfall shock dummy	-0.32*	-0.07	0.16*	-0.17	-0.39**	-0.25	0.05	-0.06
	[0.15]	[0.05]	[0.09]	[0.39]	[0.17]	[0.22]	[0.04]	[0.13]
Negative rainfall shock dummy	-0.18	-0.01	-0.03	0.22	-0.25***	-0.14	0.01	-0.16
	[0.14]	[0.03]	[0.08]	[0.40]	[0.07]	[0.20]	[0.02]	[0.14]
Prop. Congress MLAs	-0.06	-0.04	0.05	-0.07	-0.43**	0.04	0.05	0.13
	[0.12]	[0.04]	[0.16]	[0.32]	[0.16]	[0.22]	[0.04]	[0.13]
Prop. BJP MLAs	0.05	0.08***	0.12	-0.30	-0.29	0.12	0.04	-0.02
	[0.14]	[0.02]	[0.12]	[0.34]	[0.25]	[0.27]	[0.03]	[0.09]
Prop. MLAs in state gov.	-0.07	0.04	-0.07	0.10	0.13*	-0.27	-0.05**	0.00
	[0.12]	[0.03]	[0.11]	[0.37]	[0.07]	[0.17]	[0.02]	[0.10]
Prop. MLAs reserved for	-0.12	0.02	0.86***	-0.08	-0.06	-0.19	-0.12	-0.22
Scheduled Castes	[0.20]	[0.10]	[0.21]	[0.32]	[0.21]	[0.17]	[0.07]	[0.15]
Prop. MLAs reserved for	-0.48***	-0.24***	-0.69***	2.49***	0.06	0.07	-0.16*	-0.54**
Scheduled Tribes	[0.12]	[0.07]	[0.20]	[0.33]	[0.09]	[0.24]	[0.09]	[0.22]
Ln population		1.03***	1.14***	0.65***				
		[0.01]	[0.11]	[0.19]				
Ln villages						0.81***	1.00***	0.60***
						[0.15]	[0.03]	[0.05]
State fixed effects?	Y	Y	Y	Y	Y	Y	Y	Y
Observations	406	406	406	406	404	404	404	404
Adjusted R-squared	0.47	0.96	0.84	0.90	0.52	0.89	0.96	0.64

Notes: Standard errors are clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See text for details.

Table A9: Balance tests for the instrument for the proportion of affirmative action bureaucrats, 2/2

Dependent variables:	Pos. rainfall shock	Neg. rainfall shock	Prop. Congress MLAs	Prop. BJP MLAs	Prop. MLAs in state gov.	Prop. MLAs reserved SCs	Prop. MLAs reserved STs
	1	2	3	4	5	6	7
Prop. early-career officers	-0.02	0.00	0.00	0.01	-0.00	0.01	-0.01
recruited under AA	[0.03]	[0.04]	[0.02]	[0.01]	[0.02]	[0.01]	[0.01]
Positive rainfall shock dummy		-0.23***	0.01	0.00	-0.00	0.01	-0.00
		[0.03]	[0.01]	[0.01]	[0.02]	[0.01]	[0.01]
Negative rainfall shock dummy	-0.15***		-0.00	-0.00	0.00	0.00	0.00
	[0.02]		[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Prop. Congress MLAs	0.05	-0.03		-0.48***	0.01	0.14*	0.11***
	[0.07]	[0.09]		[0.06]	[0.09]	[0.08]	[0.04]
Prop. BJP MLAs	0.02	-0.02	-0.52***		0.56***	0.04	0.12***
	[0.06]	[0.09]	[0.07]		[0.09]	[0.08]	[0.04]
Prop. MLAs in state gov.	-0.00	0.02	0.01	0.37***		0.05	-0.00
	[0.06]	[0.06]	[0.07]	[0.06]		[0.06]	[0.03]
Prop. MLAs reserved for	0.06	0.02	0.26**	0.08	0.13		-0.19**
Scheduled Castes	[0.07]	[0.13]	[0.13]	[0.13]	[0.15]		[0.08]
Prop. MLAs reserved for	-0.01	0.14	0.44***	0.47***	-0.01	-0.42***	
Scheduled Tribes	[0.14]	[0.15]	[0.10]	[0.11]	[0.16]	[0.12]	
State-year fixed effects?	Y	Y	Y	Y	Y	Y	Y
District fixed effects?	Y	Y	Y	Y	Y	Y	Y
Observations	2,047	2,047	2,047	2,047	2,047	2,047	2,047
Adjusted R-squared	0.44	0.45	0.77	0.85	0.77	0.69	0.94

Notes: Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See text for details.

Table A10: Balance tests for the instrument for the proportion of affirmative action bureaucrats (states where quasi-random inital assignments were verified), 1/2

Dependent variables:	Ln population	Ln literates	Ln Scheduled Castes	Ln Scheduled Tribes	Ln villages	Ln vill. with power	Ln vill. with roads	Ln vill. with high school
	1	2	3	4	5	6	7	8
Prop. early-career officers	-0.09	0.04	-0.09	-0.35	0.13	-0.09	-0.05	-0.04
recruited under AA	[0.08]	[0.05]	[0.06]	[0.26]	[0.17]	[0.11]	[0.02]	[0.11]
Positive rainfall shock dummy	-0.31	0.01	0.28	-1.05	-0.28	-0.25	-0.04	0.05
	[0.21]	[0.06]	[0.18]	[0.51]	[0.31]	[0.21]	[0.05]	[0.07]
Negative rainfall shock dummy	-0.10	0.07**	0.12	-0.09	-0.31	-0.45**	-0.06	0.04
	[0.28]	[0.02]	[0.11]	[0.49]	[0.14]	[0.09]	[0.06]	[0.11]
Prop. Congress MLAs	-0.01	-0.05	0.08	-0.60*	-0.27*	-0.23***	-0.01	0.01
	[0.18]	[0.04]	[0.12]	[0.21]	[0.10]	[0.02]	[0.01]	[0.05]
Prop. BJP MLAs	-0.09	0.06	-0.08	-0.46	-0.57	0.16	0.00	0.03
	[0.22]	[0.09]	[0.14]	[0.36]	[0.71]	[0.25]	[0.01]	[0.08]
Prop. MLAs in state gov.	0.15	-0.02	-0.16**	0.86*	0.14	0.13	-0.04	0.16*
	[0.08]	[0.08]	[0.04]	[0.27]	[0.35]	[0.20]	[0.03]	[0.07]
Prop. MLAs reserved for	-0.22	-0.15	0.63*	-0.59	-0.04	-0.27	-0.09	-0.18
Scheduled Castes	[0.47]	[0.11]	[0.24]	[0.64]	[0.64]	[0.23]	[0.13]	[0.16]
Prop. MLAs reserved for	0.04	-0.29**	-0.58	2.09	0.27	0.30	0.03	-0.24
Scheduled Tribes	[0.10]	[0.06]	[0.51]	[0.94]	[0.19]	[0.17]	[0.05]	[0.17]
Ln population		1.07***	1.05***	0.81				
		[0.03]	[0.05]	[0.42]				
Ln villages						0.58	0.95***	0.59***
						[0.28]	[0.03]	[0.06]
State fixed effects?	Y	Y	Y	Y	Y	Y	Y	Y
Observations	115	115	115	115	115	115	115	115
Adjusted *R*-squared	0.14	0.93	0.76	0.87	0.09	0.98	0.97	0.73

Notes: Standard errors are clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See text for details.

Table A11: Balance tests for the instrument for the proportion of affirmative action bureaucrats (states where quasi-random inital assignments were verified), 2/2

Dependent variables:	Pos. rainfall shock	Neg. rainfall shock	Prop. Congress MLAs	Prop. BJP MLAs	Prop. MLAs in state gov.	Prop. MLAs reserved SCs	Prop. MLAs reserved STs
	1	2	3	4	5	6	7
Prop. early-career officers	-0.03	-0.07	0.03	0.05	0.06	0.02	0.00
recruited under AA	[0.08]	[0.10]	[0.04]	[0.04]	[0.06]	[0.03]	[0.01]
Positive rainfall shock dummy		-0.18***	0.02	-0.01	0.01	-0.01	0.01
		[0.06]	[0.03]	[0.02]	[0.03]	[0.02]	[0.00]
Negative rainfall shock dummy	-0.14***		0.00	-0.03	-0.00	0.04**	0.01
	[0.05]		[0.02]	[0.02]	[0.03]	[0.02]	[0.01]
Prop. Congress MLAs	0.11	0.01		-0.38**	0.29	0.28	0.04
	[0.14]	[0.12]		[0.16]	[0.21]	[0.19]	[0.04]
Prop. BJP MLAs	-0.04	-0.14	-0.30**		0.08	-0.01	0.03
	[0.08]	[0.09]	[0.13]		[0.13]	[0.13]	[0.04]
Prop. MLAs in state gov.	0.03	-0.00	0.17	0.06		0.00	-0.02
	[0.08]	[0.11]	[0.15]	[0.11]		[0.15]	[0.02]
Prop. MLAs reserved for	-0.06	0.39***	0.45	-0.03	0.01		-0.06
Scheduled Castes	[0.13]	[0.13]	[0.30]	[0.26]	[0.41]		[0.06]
Prop. MLAs reserved for	0.25	0.73	0.38**	0.34	-0.29	-0.34***	
Scheduled Tribes	[0.16]	[0.44]	[0.15]	[0.38]	[0.21]	[0.10]	
State-year fixed effects?	Y	Y	Y	Y	Y	Y	Y
District fixed effects?	Y	Y	Y	Y	Y	Y	Y
Observations	484	484	484	484	484	484	484
Adjusted R-squared	0.42	0.50	0.77	0.77	0.71	0.63	0.94

Notes: Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See text for details.

Table A12: Balance tests for the instrument for the proportion of affirmative action bureaucrats (states where quasi-random inital assignments were not verified), 1/2

Dependent variables:	Ln population	Ln literates	Ln Scheduled Castes	Ln Scheduled Tribes	Ln villages	Ln vill. with power	Ln vill. with roads	Ln vill. with high school
	1	2	3	4	5	6	7	8
Prop. early-career officers	0.14	-0.03	0.08	0.07	0.14	-0.16	-0.05	0.10
recruited under AA	[0.10]	[0.04]	[0.12]	[0.21]	[0.17]	[0.20]	[0.07]	[0.13]
Positive rainfall shock dummy	-0.25	-0.12*	0.12	0.33	-0.43*	-0.30	0.10*	-0.09
	[0.20]	[0.06]	[0.12]	[0.39]	[0.22]	[0.31]	[0.06]	[0.18]
Negative rainfall shock dummy	-0.20	-0.06**	-0.11	0.37	-0.24**	-0.00	0.03	-0.22
	[0.17]	[0.03]	[0.10]	[0.49]	[0.10]	[0.24]	[0.02]	[0.18]
Prop. Congress MLAs	-0.08	-0.02	0.05	0.38	-0.43*	0.22	0.06	0.18
	[0.20]	[0.05]	[0.25]	[0.38]	[0.25]	[0.36]	[0.06]	[0.23]
Prop. BJP MLAs	0.37	0.08*	0.27	0.63	0.02	0.18	0.06	0.07
	[0.23]	[0.04]	[0.27]	[0.64]	[0.24]	[0.41]	[0.05]	[0.26]
Prop. MLAs in state gov.	-0.34*	0.08	-0.10	-0.75	-0.06	-0.45*	-0.06*	-0.09
	[0.17]	[0.05]	[0.21]	[0.48]	[0.12]	[0.24]	[0.03]	[0.20]
Prop. MLAs reserved for	-0.10	0.13	0.97***	0.06	-0.06	-0.16	-0.15*	-0.24
Scheduled Castes	[0.18]	[0.11]	[0.29]	[0.35]	[0.24]	[0.25]	[0.07]	[0.20]
Prop. MLAs reserved for	-0.60***	-0.22**	-0.72***	2.57***	0.01	0.08	-0.21*	-0.60**
Scheduled Tribes	[0.14]	[0.09]	[0.24]	[0.36]	[0.12]	[0.30]	[0.11]	[0.27]
Ln population		1.03***	1.15***	0.55**				
		[0.02]	[0.13]	[0.22]				
Ln villages						0.90***	1.01***	0.59***
						[0.16]	[0.04]	[0.07]
State fixed effects?	Y	Y	Y	Y	Y	Y	Y	Y
Observations	291	291	291	291	289	289	289	289
Adjusted R-squared	0.49	0.96	0.84	0.90	0.58	0.80	0.95	0.61

Notes: Standard errors are clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See text for details.

Table A13: Balance tests for the instrument for the proportion of affirmative action bureaucrats (states where quasi-random inital assignments were not verified), 2/2

Dependent variables:	Pos. rainfall shock	Neg. rainfall shock	Prop. Congress MLAs	Prop. BJP MLAs	Prop. MLAs in state gov.	Prop. MLAs reserved SCs	Prop. MLAs reserved STs
	1	2	3	4	5	6	7
Prop. early-career officers	-0.02	0.01	-0.01	0.00	-0.01	0.00	-0.01
recruited under AA	[0.03]	[0.04]	[0.02]	[0.01]	[0.02]	[0.01]	[0.01]
Positive rainfall shock dummy		-0.24***	0.01	0.01	-0.01	0.01	-0.00
		[0.04]	[0.02]	[0.01]	[0.02]	[0.01]	[0.01]
Negative rainfall shock dummy	-0.15***		-0.01	0.01	-0.00	-0.01	-0.00
	[0.03]		[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Prop. Congress MLAs	0.04	-0.04		-0.43***	-0.02	0.10	0.13***
	[0.07]	[0.11]		[0.07]	[0.09]	[0.08]	[0.05]
Prop. BJP MLAs	0.06	0.07	-0.56***		0.69***	0.07	0.17***
	[0.09]	[0.13]	[0.09]		[0.10]	[0.10]	[0.06]
Prop. MLAs in state gov.	-0.04	-0.03	-0.02	0.47***		0.03	-0.02
	[0.08]	[0.09]	[0.08]	[0.07]		[0.06]	[0.04]
Prop. MLAs reserved for	0.08	-0.17	0.20	0.10	0.06		-0.23**
Scheduled Castes	[0.08]	[0.14]	[0.15]	[0.14]	[0.13]		[0.10]
Prop. MLAs reserved for	-0.05	-0.01	0.45***	0.45***	-0.07	-0.43***	
Scheduled Tribes	[0.16]	[0.15]	[0.11]	[0.13]	[0.15]	[0.14]	
State-year fixed effects?	Y	Y	Y	Y	Y	Y	Y
District fixed effects?	Y	Y	Y	Y	Y	Y	Y
Observations	1,563	1,563	1,563	1,563	1,563	1,563	1,563
Adjusted R-squared	0.45	0.43	0.79	0.88	0.82	0.72	0.94

Notes: Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See text for details.

Table A14: Differences between assignment length in years of all and early-career assignments

Assignments:	All	Early-career
	1	2
Dummy for affirmative action	0.21^{**}	0.04
bureaucrat	(0.09)	(0.04)
Exam year fixed effects?	Y	Y
Observations	1,298	1,124
Adjusted R-squared	0.18	0.20

Notes: The unit of analysis is the individual officer. The dependent variable is the average length of officers' assignments in years, calculated using data on all assignments (regression 1) and calculated using data from the first five years of officer's careers (regression 2). Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: The effects of affirmative action bureaucrats on MGNREGA implementation

Estimator:	OLS	OLS	OLS	2SLS	
Equation:				1st stage	2nd stage
	1	2	3	4	
Prop. affirmative action bureaucrats	0.11	0.10	0.03		0.03
	[0.08]	[0.08]	[0.05]		[0.06]
Positive rainfall shock dummy		0.10	0.04	0.02	0.04
		[0.07]	[0.03]	[0.02]	[0.03]
Negative rainfall shock dummy		-0.26***	0.04	-0.01	0.04*
		[0.06]	[0.03]	[0.02]	[0.02]
Prop. Congress MLAs		-0.62***	-0.10	0.03	-0.10
		[0.13]	[0.08]	[0.06]	[0.06]
Prop. BJP MLAs		-0.36***	-0.05	0.06	-0.05
		[0.13]	[0.07]	[0.06]	[0.06]
Prop. MLAs in state gov.		-0.20*	0.08	0.04	0.08
		[0.12]	[0.07]	[0.05]	[0.06]
Prop. MLAs reserved for Scheduled Castes		0.30	0.08	0.09	0.08
		[0.21]	[0.09]	[0.08]	[0.07]
Prop. MLAs reserved for Scheduled Tribes		0.74***	-0.04	0.10	-0.04
		[0.14]	[0.19]	[0.08]	[0.16]
Prop. early-career officers recruited under AA				0.66***	
				[0.03]	
State-year fixed effects?	N	N	Y	Y	Y
District fixed effects?	N	N	Y	Y	Y
Observations	2,047	2,047	2,047	2,047	2,047
Adjusted R-squared	0.00	0.09	0.88		0.88
F-statistic for AA bureaucrats					368

Notes: The dependent variable is the logarithm of households that received 100 days or more of employment under MGNREGA, standardized to have mean 0 and standard deviation 1. Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Mechanisms for the effects of affirmative action bureaucrats on MGNREGA implementation

Dependent variables:	HHs that recd. 100+ days	HHs that recd. 100+ days	Ln person-days recd. by SCs/STs	Prop. spent on materials	HHs that recd. 100+ days
	1	2	3	4	5
Prop. affirmative action bureaucrats	-0.04		0.01	0.08	
	[0.07]		[0.07]	[0.12]	
Prop. disadvantaged group bureaucrats	0.10*	0.11*		-0.05	
	[0.06]	[0.06]		[0.12]	
Bureaucrats' ln exam rank		-0.02			
		[0.02]			
Prop. SC/ST bureaucrats			0.09		
			[0.07]		
Prop. other disadvantaged group bureaucrats			0.08		
			[0.06]		
Positive rainfall shock dummy	0.04	0.04	0.03	-0.02	0.04
	[0.03]	[0.03]	[0.02]	[0.05]	[0.03]
Negative rainfall shock dummy	0.04*	0.04**	0.02	-0.08**	0.05*
	[0.02]	[0.02]	[0.02]	[0.04]	[0.03]
Prop. Congress MLAs	-0.10	-0.10	-0.07	-0.03	-0.10
	[0.06]	[0.07]	[0.05]	[0.11]	[0.07]
Prop. BJP MLAs	-0.05	-0.05	-0.08*	0.23*	-0.05
	[0.06]	[0.06]	[0.05]	[0.12]	[0.06]
Prop. MLAs in state gov.	0.07	0.07	0.06	-0.07	0.08
	[0.06]	[0.06]	[0.04]	[0.10]	[0.06]
Prop. MLAs reserved for Scheduled Castes	0.07	0.08	0.01	0.18	0.08
	[0.07]	[0.07]	[0.05]	[0.15]	[0.07]
Prop. MLAs reserved for Scheduled Tribes	-0.03	-0.04	-0.01	0.19	-0.04
	[0.15]	[0.15]	[0.12]	[0.34]	[0.16]
All affirmative action bureaucrats?					0.04
					[0.07]
Some affirmative action bureaucrats?					-0.03
					[0.19]
State-year fixed effects?	Y	Y	Y	Y	Y
District fixed effects?	Y	Y	Y	Y	Y
Observations	2,047	2,047	2,024	1,532	2,047
Adjusted R-squared	0.88	0.88	0.94	0.76	0.88
F-statistic for AA bureaucrats	186		136	121	
F-statistic for disadvantaged group bureaucrats	234	224		124	
F-statistic for exam rank		106			
F-statistic for SC/ST bureaucrats			140		
F-statistic for other disadvantaged group bureaucrats			117		
F-statistic for all AA bureaucrats?					90
F-statistic for some AA bureaucrats?					6

Notes: All dependent variables are standardized to have mean 0 and standard deviation 1. Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A17: Scores on the subjective oral interview or personality test component of the UPSC exam

	1	2	3	4	5	6	7	8
Dummy for minority bureaucrat	-12.51***		-3.39*	-2.78	-24.98***		-5.65***	-5.15***
	(1.09)		(1.58)	(1.58)	(1.45)		(1.69)	(1.59)
Dummy for affirmative action bureaucrat		-14.71***	-12.20***	-12.19***		-36.92***	-32.88***	-32.80***
		(1.45)	(1.97)	(1.99)		(2.27)	(2.56)	(2.58)
Dummy for female bureaucrat				5.11**				4.23***
				(2.04)				(1.15)
Score for written components of UPSC exam					-0.29***	-0.41***	-0.41***	-0.41***
					(0.03)	(0.03)	(0.03)	(0.03)
Exam year fixed effects?	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,339	1,339	1,339	1,339	1,336	1,336	1,336	1,336
Adjusted R-squared	0.11	0.13	0.13	0.14	0.28	0.42	0.42	0.42

Notes: The unit of analysis is the individual officer. The dependent variable is the interview score. Standard errors are clustered by exam year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.